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Disastrous Voting

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DISASTROUS VOTING

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Disastrous Voting

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Abstract

DISASTROUS VOTING

by

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The central question of this study is 'Is there an observable general trend of electorates punishing incumbents for natural disasters across countries and elections?' Many scholars have argued for the existence of such behavior, yet the empirical evidence rests mostly on single-country and even single-election studies. I look for a generalizable trend in two original data sets with country-election and country-constituency-election as the unit of analysis respectively. I test the punishment hypothesis by correlating the occurrence of natural disasters to the performance of incumbent parties in national lower house elections. Furthermore, I propose that the effect of natural disasters on incumbents' electoral performance varies depending on patronage expectations of the electorate and influx of international humanitarian aid. The analysis uses linear mixed-effects models with lagged variables estimated via restricted maximum likelihood estimation. The data does not support the rejection of the null hypothesis of no generalizable trend. Also, the results for the conditioning effects of patronage and international humanitarian aid are too volatile to draw inference. This

study finds itself in the company of a few studies which also used cross-sectional time-series approaches, yet on a smaller scale, and which also were unable to find a generalizable trend. Together, their results are of importance for a research agenda with renewed interest as they caution scholars to attribute too much external validity to existing studies with research designs which focus on single countries and elections. I find no evidence for a generalizable trend once effects specific to countries and elections are washed out. In return, one could interpret this as evidence of electorates not blindly punishing incumbents for random external shocks. Instead, an electorate's reaction to a natural disaster may depend upon intervening factors which allow the electorate to make a rational punishment/reward decision.

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Notwithstanding all this support, I claim all errors in this dissertation to be my own.

The nature of some research questions and the respective analyses can almost seem like they turn a blind eye to the suffering caused by the studied phenomena.

This is for all of those in need,
those who provide relief,
and those for whom help was too late.

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Chapter 1

Introduction

The central question of this study is 'Do electorates punish political incumbents at the polls for natural disasters which occurred during the incumbent's term?'. Incumbents in various settings are exposed to the political impact of natural disasters, however not all incumbents experience the same aftermath. Some incumbents seem to gain from external shocks such as natural disasters while others loose. Is there an observable general trend across elections and political units of incumbents either gaining or losing from natural disasters? If there are general effects, do they vary natural disaster type? And does the effect differ depending on the constituency's relationship to and expectations from the incumbent?

This study engages these questions in the context of national lower house elections since 1960. The sampled political units conform in (somewhat) free and fair elections as well as in the expectation that well-performing elected officials will be held accountable to and only to matters in their area of influence by rational voters. Yet we observe different performances of incumbents in the aftermath of natural disasters. Two decades of renewed interest in this paradox did not yield any tangible generalized insights as to what drives the different observations.

The following pages introduce the research program of the political impact of natural disasters, the argument of the study, its implications, and the study's organization. After

reviewing the relevant political science literature to introduce the research program, I offer the argument that the impact of natural disasters may differ depending on patronage expectations. I argue that rational voters with patronage expectations may ultimately reward incumbents for natural disasters because the rational incumbent will send help to disaster struck regions. On the flip side, my expectation for voters without patronage expectations is that they will tend to generally punish incumbents.

This study demonstrates that the data overall does *not* support any generalizable trends of electoral rewarding or punishment of incumbents in the aftermath of natural disasters. This conclusion holds regardless of patronage expectations. Nevertheless, the implication of this finding is important because it confirms suspicions that the findings of single-country studies cannot be generalized to form an expectation of stupid, blind, myopic, or irrational voters who punish incumbents for random acts of god. In other words, the data underlying this study suggests that voters do *not* punish incumbents blindly for random shocks. The absence of a generalizable trend in either direction suggests that the effects of conditioning and intervening factors may lead rational electorates to cast rational votes. To my knowledge, this study incorporates more countries and elections than any other research project on the same topic. In a science which relies on test and retest, this study serves to validate (or rebuke) the general conclusions some researchers have drawn from various single-country studies.

The Political Impact of Natural Disasters

Do voters punish their governments for events which are outside of the governments' control, or random acts of god, or natural disasters more specifically? How do voters perceive and reward government efforts for relief and mitigation? What sort of incentive system is created by such dynamics, and how do we thus expect government officials to react to (potential) natural disaster damages? Also, how does the international community react and what kind of incentive system is their response creating? Finally, how do voters perceive the interaction of the different actors in the natural disaster management system, and how can this perception be influenced by these actors or third parties, such as the media? These and similar questions are addressed by a research field which studies the politics and the political impact of natural disasters. The studies by Barnhart (1925) and Abney and Hill (1966) are generally accepted as the pioneers in this research program.

The natural disasters and elections nexus was recently picked up again to illustrate the practical shortcomings of retrospective economic voting based on individuals' subjective welfare. Achen and Bartels (2013) famously posit with the title of their study that "shark attacks are bad for democracy" because President of the USA Woodrow Wilson lost vote share to shark attacks in the 1916 election in multiple counties of New Jersey as compared to the 1912 election. Further, the authors argued that the U.S. voter has repeatedly punished incumbents for low or heavy rainfalls. Achen and Bartels (2013, 2002) used this analysis to show that voters cannot objectively evaluate their own economic well-being in order to discern incumbent performance. Instead, voters mix acts of the incumbent with acts of god, a habit which the authors term '*blind* retrospection'. Achen and Bartels (2013, 2002) thus

conclude that the results cast doubt on the theory of rational voters engaging in retrospective economic voting and the democratic accountability it is supposed to produce.

The political science literature has taken this hypothesis of voters punishing incumbents for natural disasters and reaffirmed it in the context of many case studies and even in cross-country research designs and over time. In cross-sectional studies, Quiroz Flores et al. (2013) and Chang and Berdiev (2015) linked natural disasters to the removal of political leaders and Brückner and Ciccone (2011) find that negative rainfall shocks cause advancement of democratic institutions in sub-Saharan African countries. Brückner and Ciccone (2011) put forward the concept of a window-of-opportunity in the aftermath of natural disasters which Ahlerup (2013a, 2013b) argue can lead to democratization or civil turmoil.

However, the mechanism on how natural disasters impact electoral outcomes has not been extensively theorized because initial studies treated natural disasters as instrumental variables to engage the retrospective economic voting literature. Consider recent, popular studies such as Healy (2008) and Achen and Bartels (2002, 2013). The authors consider their studies tests of the assumption of rational voters using their own economic well-being as a heuristic to judge incumbent performance during their retrospective voting decision. The authors' theoretical expectation is straight forward: a rational voter would not punish for 'bad luck' or 'shark attacks'. A separate theory for the electoral impact of natural disasters was not needed because the theoretical foundation is the retrospective economic voting literature which was tested here with a new, instrumental variable and according to which one would not have expected any statistically significant influence of natural disasters on incumbent vote share or survival chance.

The interesting thing about natural disasters was not *why* they had an electoral impact but that they had one and what it meant for democratic accountability. In other words, many works in this particular field of research are extensions of some aspects of the debate between Campbell et al. (1960) on one side and Downs (1957), Key (1966), Kramer (1971), and Fiorina (1981) on the other side which argued for a much more capable, evaluative electorate. Here, a voter punishing incumbents for random external shocks seems like irrational behavior. The negative effect to democratic accountability is exacerbated by voters' cognitive and emotional biases which cloud their judgment when attributing blame (Forgette, King, and Dettrey 2008; Cole, Healy, and Werker 2012; Healy and Malhotra 2012). If one was to accept that punishing incumbents for natural disasters is not random or irrational behavior, then one can ask how this happens, whether this behavior is conditioned on other, intervening factors, and how this knowledge can be used to one's advantage. This could be the modern research program on the political impact of random external shocks, including natural disasters.

Argument of the Study

This study presents an analytical framework on the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions. The central argument is that electorates consider natural disasters in their performance-based voting decisions. Thus, natural disasters are hypothesized to influence incumbents' performance in elections. I provide possible pathways of a mechanism which had not been formally theorized in previous work. What is innovative about my claim is that I offer a potential link to connect the various studies on the subject matter which have drawn ambiguous or even

contradicting conclusions.

I propose that natural disasters have an electoral impact through three possible pathways: (i) natural disasters are direct determinants of incumbents' electoral fate, (ii) natural disasters act as a catalyst, pushing more unsatisfied citizens towards political action by revealing or exacerbating existing conditions, and (iii) natural disasters impact elections only if an intervening variable creates the right environment. I argue that the degree of neopatrimonialism is one such intervening variable which can explain why some societies seem to punish incumbents for natural disasters while others reward incumbents. There is ample evidence for all three pathways. However, this evidence is based mostly on single-country and even single-event studies. Of those studies, most draw their conclusions from an analysis of the electorate of the United States of America. Studies with research designs aimed at deriving tendencies which can be generalized across countries and across time are rare and ambiguous in their results.

I further contribute to the scholarly discussion by providing a more formalized way of thinking about the connection of natural disasters to incumbent electoral performance and survival in leadership positions than is common in related publications. Most studies in this field rely on a theory born out of data and empirics and their literature reviews thus focus only on the results of preceding studies. Other studies use natural disasters as instrumental variables to examine perceived problems in the retrospective voting literature and its consequences for democratic accountability. This particular field can be argued to expect *no* correlation between natural disasters and a rational voter's evaluation of an incumbent: In this context, a theory of why natural disasters should be expected to have any influence on

incumbent vote share and survival in leadership positions is not needed because the ideal result of these studies is to show no correlation.

I posit three hypotheses: (i) Natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election, (ii) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies, and (iii) incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. The theoretical goal of this study is to provide (i) a possible intervening variable which could link those studies which found negative electoral impacts of natural disasters with those which found positive or ambiguous results, and (ii) an expectation for a general tendency on what reaction incumbent governments can expect from their electorate for bringing in international disaster aid. The empirical goal is to provide a cross-country cross-time empirical test of whether the finding of electorates punishing incumbent governments for natural disasters can be generalized beyond the United States of America.

Implications of the Study

My purpose is to advance our understanding of the political impact of random external shocks, in particular the impact of natural disasters on incumbents' electoral performance in national lower house elections. Despite renewed interest in this field of study, the last two decades yielded conflicting conclusions about generalizable effects, the mechanism and pathways of how natural disasters may impact incumbents' electoral performance have not

been formally theorized, and there is a significant gap of cross-sectional cross-time studies in this field. This study attempts to further our understanding of the electoral impact of natural disasters by a step in all of these three realms.

This study adds to a growing literature by theorizing about the electoral impact of natural disasters, providing a possible missing link between the conflicting results of various studies on this topic, and by questioning the existence of a generalizable trend of electorates punishing incumbents for natural disasters which seems to be accepted by several colleagues citing this field of research. I argue in the following chapters that the electoral impact of natural disasters is rooted in the literatures of democratic accountability and retrospective performance-based voting. I posit that electorates might use natural disasters as heuristics for incumbent's performance and that thus, the consideration of a random external shock, such as a natural disaster, may be compatible with the rational voter theorem. Further, I argue that patronage expectations in exchange for support on election day may be one potential missing link to explain the conflicting results of previous studies.

Overall, the data does not suggest a generalizable trend of the electoral impact of natural disasters, yet I find this result even more fascinating than its alternative. I found replicating the substantive results from similar studies to be difficult: the statistical significance of the measures is highly dependent upon the sample and generally there is little evidence for a direct impact of natural disaster on incumbents' electoral fate. The analysis of patronage expectations yielded results which were almost as ambiguous. Possible conclusions of this are (1) my data is wrong and that previous studies are right about electorates punishing incumbents for natural disasters, or (2) that the scope of my two data samples effectively

filtered out country as well as election specific variance as noise so for the analysis to side with the few studies which also could not find a generalizable trend (see e.g. Remmer (2014)).

The following chapters present the corresponding literature, underlying theories and assumptions, as well as my methodology, analysis, and interpretation. I have intentionally separated the presentation of the results of the analysis from my interpretation. I leave it up to the reader and future studies to judge and retest whether there is a general trend of electorates punishing incumbents for natural disasters or whether we might have attributed too much external validity to a significant and valuable set of single-country and/or single-election studies.

Organization of the Study

An analytical framework on the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions is developed in subsequent chapters. I pay close attention to the theoretical assumptions which one has to make when considering natural disasters as drivers of incumbents' electoral performance. I trace these assumptions through the literatures on democratic accountability, retrospective (economic) performance-based voting, and political impacts of natural disasters. I then discuss my methodological choices as well as the data gathering process and related assumptions and choices. The analysis chapter is divided into three parts: replication of previous studies, further empirical testing with additional data and variables, and the interpretation of the statistical results. I employ a cross-sectional time-series setting on two original data sets with two different units of analysis. The chapters are supposed to be read sequentially but each chapter has

an introduction, conclusion, and summary which facilitates reading individual chapters.

Chapter 2 presents theory and testable hypotheses of the electoral impact of natural disasters on incumbent governments and the chance of survival in leadership positions. Based on the literatures of democratic accountability, (retrospective) economic voting, and blame attribution, we can conclude that voters may use natural disasters as heuristics for government failure. I propose three pathways of a mechanism which suggests that natural disasters affect electoral outcomes: natural disasters as direct factors, natural disasters as catalysts for change, and natural disasters as factors of electoral fate only if an intervening variable makes the event politically important.

I argue that natural disasters can be used as heuristics for government failure and that voters may use these events in their reward-punishment mechanism as part of their rational retrospective economic voting behavior. I derive three testable hypotheses: (i) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election, (ii) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies, and (iii) incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies.

Chapter 3 describes the data gathering process and explores the relationship of the individual explanatory variables with the dependent variable. Close attention is paid to the nuances of each data source, coding decisions, assumptions, and data corrections. The goal of the preliminary analyses is to explore bivariate relations and conditioning effects of the

hypothesized interactive variables. While mixed, the results of the preliminary analysis do point towards the hypothesized relationships.

Chapter 4 reviews modeling considerations which are drawn from expectations about the true data generating process (DGP). A research design using data sets with observations clustered by countries and constituencies and collected over time has to yield a discussion of various violations of the assumptions of independence of observations and error terms. Spatio-temporal dependencies can bias estimation results when not modeled in the process. In praxis, however, data quality and estimation feasibility often do not meet the requirements to model everything which should be accounted. Therefore, the researcher has to strike a balance between data quality and resource constraints. This study's analysis uses linear mixed-effects models with lagged dependent variables. This approach splits the variance into multiple components and lags the dependent and explaining variables for practical as well as theoretical reasons. While modeling some dynamics which can be reasonably expected from the true DGP, this approach still imposes assumptions which may be impossible to hold. Nevertheless, such linear mixed-effects models are within current good academic practices.

Chapter 5 examines the (multivariate) statistical evidence for the relationship between natural disasters and incumbent party vote share in national lower house elections. Following the theory, I argue that an incumbent's vote share is dependent upon the occurrence of natural disasters, the state of the economy, the respective society's experience with natural hazards and its expectation of patronage, and the incumbent's vote share in the previous election. While most related studies focus on one single country (see table 2.1), this analysis uses two original data sets with elections observed in multiple countries over time in

order to deduce generalizable tendencies of natural disasters impacting incumbents' electoral fate. One data set uses the country-election year as the unit of analysis and the second uses country-constituency-election year. The natural disaster variables are populated with two different sources to allow disaggregation to the constituency-level in the second data set. Using two different samples allows to not only test the respective null hypotheses in multiple settings but also to check the robustness of the results across model specifications and samples.

The first sections of chapter 5 test the different hypotheses, establish comparability to previous studies, and present the empirical results. I found replicating the substantive results from similar studies to be difficult: the statistical significance of the measures is highly dependent upon the sample and generally there is little evidence for a direct impact of natural disaster on incumbents' electoral fate as hypothesized in hypothesis 1. Subsequently, I argue that patronage expectations act within the black box between natural disasters and incumbent electoral performance, determining whether a specific natural disaster is going to have an electoral impact in the respective country's next election (see hypothesis 2). Again, the sample cannot reject the null hypothesis of no base effect of natural disasters. In addition, if at all, patronage expectations seem to generally point to the opposite direction than hypothesized. That being said, the coefficients and standard errors are so volatile that I do not suggest drawing any inference from them. Finally, I test the effect of OECD humanitarian aid on incumbent party vote share in the next national lower house election (see hypothesis 3). The effect differs between clientalistic and non-clientalistic societies: the effect in non-clientalistic societies is sizable and positive while patronage expectations within

the electorate seem to decrease the effect into practical unimportance. Once more, this result is also counterintuitive given the theoretical reasoning.

The last section of chapter 5 is an important addition to the interpretation of the results. The preceding sections rely on a dichotomization into statistically significant and not significant results. However, this method of interpreting statistical results can be problematic as (a) p-values do not actually prove any hypotheses, (b) p-values are based on arbitrary significance thresholds, (c) it distracts from substantive (or practical) importance of coefficients, and (d) the difference between a statistically significant and non-significant coefficient may not itself be significant. The coefficient plots presented in this section serve to diversify the method of interpretation. Comparing coefficients and confidence intervals, I make two observations which are especially true for the natural disaster and patronage expectation variables: (i) the statistical significance of the variables of interest is volatile and depends on model specification and the sample, and (ii) the coefficients generally are either too small to be of practical importance (even if they are statistically significant) or the respective confidence intervals are so big that they carry the potential of making the effect practically interesting yet they include zero so that one cannot say what direction the effect will take (in addition to making the coefficient statistically insignificant).

Chapter 6 concludes by discussing the findings and their implications in the light of the broader literature on natural disasters and incumbents' electoral performance. Given the acceptance or rejection of the respective null hypotheses, this part moves beyond the discussion of the statistical results in chapter 5 to the substantive discussion of the results and their implications for what we think to know about the link between natural disasters

and incumbent electoral performance. I advise caution when concluding about electorates for presumably punishing incumbents for random negative external shocks. This study should be evaluated in the light of other comparable studies which have also found *no* generalizable negative correlation between natural disasters and incumbents' electoral performance when moving away from single-country and/or single-election research designs.

Chapter 2

A Theory of the Electoral Impact of Natural Disasters

I present in this chapter an analytical framework on the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions. The central argument is that electorates consider natural disasters in their performance-based voting decisions. Thus, natural disasters are hypothesized to influence incumbents' performance in elections. I provide possible pathways of a mechanism which had not been formally theorized in previous work. What is innovative about my claim is that I offer a potential link to connect the various studies on the subject matter which have drawn ambiguous or even contradicting conclusions.

I propose that natural disasters have an electoral impact through three possible pathways: (i) natural disasters are direct determinants of incumbents' electoral fate, (ii) natural disasters act as a catalyst, pushing more unsatisfied citizens towards political action by revealing or exacerbating existing conditions, and (iii) natural disasters impact elections only if an intervening variable creates the right environment. I argue that the degree of neopatrimonialism is one such intervening variable which can explain why some societies seem to punish incumbents for natural disasters while others reward incumbents.

My argument presented here complements existing research on incumbents' electoral performance after natural disasters which suggests that electorates punish incumbents for

natural disasters. For example, Achen and Bartels (2013, 2002) conclude that voters engage in blind retrospection, seemingly punishing incumbents randomly for acts of God. Quiroz Flores et al. (2013) and Chang and Berdiev (2015) linked natural disasters to the removal of political leaders and Brückner and Ciccone (2011) put forward the concept of a window-of-opportunity in the aftermath of natural disasters which can lead to either democratic advancement or civil turmoil (Ahlerup 2013a, 2013b).

Because the prevalent studies on natural disasters and incumbents' electoral performance treated natural disasters as instrumental variables in a quest to show practical problems of assuming that voters can make objective rational decisions about how incumbents affected their economic well-being (see e.g. Healy (2008) and Achen and Bartels (2002, 2013)), scholars have overlooked the potential role of natural disasters in supplying rational retrospective electorates with information about government failure. On the one hand, one can argue that natural disasters present negative external shocks which are outside of incumbents' sphere of influence. Thus, to find electorates punishing incumbents for such events violates the assumption of a rational electorate which considers incumbents' past performance and rewards or punishes appropriately (cf. Fiorina (1981)). On the other hand however, I reinterpret this observation and claim that such punishment is part of rational retrospective voting behavior because the occurrence of natural disasters can indicate government failure. The logic to this reinterpretation is tied to the definition of natural disasters as situations when humans increased their exposure to natural hazards beyond their coping capabilities.

The principles of economic voting theory continue to apply broadly to performance-based voting, including retrospective evaluations of natural disasters. Economic voting theory as-

serts a reward-punishment mechanism according to which elections are used as referenda to approve or disapprove of incumbent performance. Further, we know from that same literature that voters use heuristics to form (dis)approving opinions of their incumbents. Given what we know about retrospective (economic) voting and my reinterpretation of natural disasters as a heuristic of government failures, I expect rational, utility maximizing voters to punish incumbent governments for natural disasters. Put in different words, the ties established between natural disasters and retrospective economic voting theory remain pivotal to the theoretical foundation of my central argument. Natural disasters impact the economic well-being of voters and, given the importance of the state of the economy in retrospective voting literature, natural disasters may therefore indirectly impact voting decisions. Furthermore, government policies can greatly increase or decrease a society's vulnerability to natural processes. Thus, electorates may attribute natural disasters to government failure. This implies a direct effect of natural disasters in the retrospective, performance voting framework.

More specifically, to ask how a voter casting a vote during an election considers past events, such as natural disasters, in his or her decision, inherently makes assumptions about voter behavior and engages various overarching literatures. First, the research question assumes elections are used by voters as referenda to keep high-performing representatives and sort out lemons. Voting for the incumbent then is like rewarding the incumbent for good policy making with another term, while voting against the incumbent is considered punishment for bad performance. Democratic accountability theory (i) treats the tie between voters and their elected representatives as a principal-agent relationship and (ii) allows elections to be used as a reward-punishment mechanism in performance-based voting behavior. Second, the

research question inherently makes the two-tiered assumption that (i) calculating, rational voters consider events and performance indicators during an incumbent's term to form a decision on whether to reward the incumbent with their vote in the reelection cycle, and (ii) voters' decisions can be approximated with an additive model which allows to include multiple factors and to determine each factor's individual contribution to the vote outcome variable. The economic voting literature provides the theoretical foundation for voters deciding on the electoral faith of incumbents based on their perceptions of past events and heuristics of incumbent performance metrics. There is discussion about what set of issues enters the reward or punishment mechanism, but the empirical evidence suggests that at least ideological preference, the state of the economy, and random shocks (e.g. international price fluctuations, or disastrous events) are among the issues which a voter considers. The assumption of the additive model is born out of empirics rather than theory. Third, and finally, voters' evaluations of events and performance indicators are assumed to be sophisticated enough for performance-based voting. Gathering information on all relevant incumbent performance indicators can be cost intensive. Thus, scholars discuss the competence of voters to make informed voting decisions. The middle ground suggests that voters make use of heuristics to form decisions and together, the whole electorate makes informed decisions in the aggregate.

Since most theory of economic voting and the electoral impact of natural disaster was derived from observing the 'American voter', this study provides a much needed cross-country cross-time evaluation of the electoral impact of natural disasters. A common characteristic between the fields of economic voting and the electoral impacts of natural disasters is that

both rely heavily on the deduction from empirics, especially from studies of the electorate of the United States of America (hereafter referred to as the U.S. electorate). This does not make any of the respective studies less important, yet raises the question of whether one can generalize their findings beyond the U.S. electorate. Indeed, many authors have confirmed various aspects of economic voting in other regions of the world, albeit at varying degrees [see e.g. Lewis-Beck (1986) and Lewis-Beck and Stegmaier (2000, 2008) for some (reviews of) international studies]. As we shall see below, however, the international evidence for the electoral impact of natural disasters is scarce and the respective studies often find ambiguous and even contradicting results. This study's international comparison of the electoral impact of natural disasters attempts to fill this gaping hole in the literature.

I proceed in this chapter by briefly reviewing the theoretical and empirical literature from which I derive the intuition for the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions. The structure of the first sections of this chapter broadly follows the three assumptions laid out in the previous paragraph: I review theories on democratic accountability, (retrospective) economic voting, and the sophistication of the (retrospective) economic vote. I then define natural disasters and develop the logic for the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions and derive three testable pathways of how natural disasters may have such influences. Using this framework, I introduce the hypotheses to be tested in this study. A conclusion and summary reiterate the main points of the discussion.

Democratic Accountability: Performance-Based Voting

This section establishes elections as referenda which voters use to keep high-performing representatives and sort out lemons. The first assumption made by this study's research question (Do voters punish incumbents for natural disasters?) is that electorates use elections to cast their votes in approval or disapproval of the incumbent government's performance. Voting for the incumbent then is like rewarding the incumbent for good policy making with another term, while voting against the incumbent is considered punishment for bad performance.

Democratic accountability refers to the process of holding elected representatives accountable in election based political systems. Following this logic, one should expect that wherever the requirements for the electoral mechanism are fulfilled (e.g. free and fair elections), voters will kick out politicians who make bad policy choices. In the terms of Hirschman (1970), the principals are hypothesized to rely on voicing their opinion by confirming or firing an incumbent because the exit option (direct democracy or emigration) is hardly an option. Accountability is generally understood as one of democracy's main principles, because "governance without accountability is tyranny" (Borowiak 2011, 1). In essence, democratic accountability refers to elected officials being held accountable to their respective constituencies through elections. Similarly, a non-elected bureaucrat is accountable to its systemic (hiring or appointing) supervisor.

Election or reappointment cycles in particular are seen as the main mechanism with which elected representatives are kept in line because they incentivize good behavior. Assuming that elected officials are professionals, seeking reelection or reappointment to make a living

(cf. Downs (1957)), these cycles are supposed to create an incentive system in which agents [(non)elected officials] inherently follow the best interest of the principal (voters or supervisors). In other words, accountability is increased while opportunistic behavior of the agents is decreased (Barro 1973).

On the flip side, the incentive system created by election or reappointment cycles is argued to also encourage agents to manipulate outcomes and portray themselves in a better light in their last year of tenure before reelection or reappointment. This is possible when there is information asymmetry between agents and principals. Principals usually set a goal, for example a growing economy, but the means to get there are ambiguous to both the agents and principals. In general, policy outcomes are often difficult to predict and the comparison of different policies before implementation is uncertain at best as well. Thus, agents could use their insight into the system to slow the economy in their first years and then speed it up in the last year in order to look good to a, mostly, unsuspecting public, or principal, when it's time for reelection (see e.g. Alesina and Roubini (1992) and Harrington (1993)).

A further problem of this incentive system is that agents may be encouraged to focus on positive short term policies which could be negative in the long run. Besley and Case (2016, 779-80) find that agents who are up for reelection differ in their policy choices compared to agents who hit a term limit. When facing term limits, the authors find that the incumbent agent, in this case U.S. governors, might resist special interests more which manifested itself empirically as higher taxes and expenditures as compared with governors who could run again and thus have to build a reputation with the public or campaign contributors. And Healy and Malhotra (2009) and Gasper and Reeves (2011) find that disaster relief spending

is more popular than disaster preparedness spending and thus, incumbents prefer the former over the latter.

Overall then, the relation between voters and elected representatives (or supervisors and non-elected officials) is assumed to be one of principals (voters) and agents (representatives) which comes with the typical problems of such relationships (see e.g. Borowiak (2011, chapter 2)). In addition to the two perversions of the democratic accountability mechanism described in the previous two paragraphs, trying to fix that very same incentive system with rules and procedures presents an additional dilemma. Agents are put into positions with a mandate. The existence of a mandate is important because otherwise there would be no standards, or expectations, to which the principals could hold the agent accountable. Further, rules and procedures might be needed to limit the agents' discretion on how to achieve set goals. Rules may ensure that the goal is reached in a certain way, e.g. by minimizing negative external effects or agent corruption. On the other hand, the presence of rules and procedures can conflict with the overall expectation of agents' efficiency: checks and balances decrease agents' efficiency by slowing them down. This has been described as the accountability dilemma (see e.g. Self (1972) and Wills (2002) cited in Behn (2001, 11)).

Following the expected effects of democratic accountability for weeding out corrupt government officials, it may seem surprising that we still observe many regions in which corruption and policy choices hold back the potential of local economies for decades (and not just to provide a boost before elections). Motivated by such observations from Latin America, Lyne (2008) explores this paradox and argues that voters and politicians are stuck in what she calls *the voter's dilemma*. Punishing politicians at the polls is a collective action, i.e. a single

voter can express his or her feelings towards the incumbent, but only the majority determines whether the incumbent is punished. Thus, a voter might not be heard when not voting with the majority. This situation is exacerbated in countries where policy relies mainly on the provision of clientalistic (as opposed to public, or collective) goods which are characterized by excludability. Lyne argues that a voter in a clientalistic society would not only waste his or her vote by voting for a politician who stands for policies towards the collective well being, but the voter would also be excluded from any clientalistic goods the winner would provide. Therefore, the dominant strategy becomes voting for the clientalistic politician even though the overall payoff would be higher if everyone voted for the collective good politician. Lyne thus correctly identifies a prisoners' dilemma in Latin American politics and beyond.

In sum, democratic accountability refers to the process of holding elected representatives accountable in election based political systems. Election or reappointment cycles keep elected representatives in line because they incentivize good behavior. The relation between voters and elected representatives (or supervisors and non-elected officials) is assumed to be one of principals (voters) and agents (representatives) which comes with the typical problems of such relationships. Democratic accountability theory then lays the foundation for electorates using elections as a performance-based reward-punishment mechanism. Further, the studies cited above suggest that this basic assumption does not change even if information asymmetry, principal-agent problems, and collective action dilemmas are introduced into the system. However, the efficiency of outcomes and agent behavior may change according to the individual incentive system the respective institutional context prescribes.

Economic Voting: The Influence of the Past

Having established that (i) voters and their elected representatives live in a principal-agent relationship and (ii) democratic accountability theory allows elections to be used as a reward-punishment mechanism in performance-based voting behavior, this section reviews the economic voting literature which (i) suggests that voters actually engage in performance-based voting and (ii) formalizes various pathways of how this accountability mechanism might work in detail. In other words, this section provides the theoretical foundation for making the second assumption of this study's research question: the two-tiered assumption that (i) voters consider events and performance indicators during an incumbent's term to form a decision on whether to reward the incumbent with their vote in the reelection cycle, and (ii) voters' decisions can be approximated with an additive model which allows for the inclusion of multiple factors and determine each factor's individual contribution to the vote outcome variable.

Economic voting theory assumes voters make a decision based on a cost-benefit analysis; if the past election cycle yielded a net gain, voters are expected to confirm the incumbent. The expectation of economic voting for incumbent survival is that incumbents are retained when they are expected to yield average or above-average gain in the next term, and ousted when they are expected to yield less than average gain. Incumbent replacements are expected to show at least average performance which is a step up from the below-average expectation which resulted in incumbent replacement.

An empirical test of this proposed cost-benefit analysis to maximize net gains could be to compare economic growth rates after retaining incumbents to growth rates after incumbent

replacement. Again, the theory suggests that electorates retain only well performing incumbents who show (above) average performance and otherwise replace incumbents with new representatives who may perform below, at, or above average. One would then expect the aggregate average economic growth of retained incumbents to be higher than the aggregated average growth of incumbent replacements. However, Achen and Bartels (2004, 11-14) find no difference and conclude that economic performance metrics might drive vote decisions yet do not actually influence future performance. It shall be noted though, that this easy yet sobering exercise may depend heavily on the small sample and inherent difficulty of investigating counterfactuals. “The key idea here is that the rational electorate may punish the incumbent party when times are bad, despite the fact that the past is past, and regardless of *why* times are bad, simply in order to discipline future incumbents” (15).

The impact of economic factors on voting decisions was recognized early, yet the exact mechanisms and their parameterization are still being debated. Refer to Kramer (1971) and Monroe (1979) for reviews of the earlier literature which considered (real) income, price levels, unemployment, and inflation. While the importance of economics is generally accepted, the proxies and parameterization of the statistical models are not (Norpoth (1985), as cited in Lewis-Beck and Stegmaier (2000, 186)). Furthermore, there is disagreement about the underlying mechanism which leads voters to consider the economy. On the one hand, voters could look at the past to predict performance of candidates in the future (prospective economic voting). On the other hand, voters could “treat elections ... as referenda on the incumbent administration’s handling of the economy” (Fiorina (1981, 26), as cited in Lewis-Beck and Stegmaier (2000, 191); retrospective voting). Furthermore, voters could consider

not only the general state of the economy (sociotropic economic voting) but focus on their individual economic situation (pocketbook or egotropic economic voting). The added caveat here is that the latter might be perceived badly even when the former is perceived to be doing well, or vice versa. Finally, it is up for debate whether voters consider the entirety of the legislation period when making their decisions, or just the last 6 to 12 months. The next paragraphs pick up these controversies and provide some more details.

The first mechanical distinction in economic voting theory is between prospective and retrospective voting. *Retrospective* economic voting theory considers elections referenda of past events, originated with Key (1966), and was formalized by Fiorina (1981, 1978). The theory posits that voters hold representatives accountable on election day. High-performing representatives are reelected while the lemons are removed.¹ Assuming that representatives wish to be reelected, a system is created which incentivizes representatives to listen to the wishes of their constituencies, keep campaign promises, and work towards their voters' economic well-being.²

Prospective economic voting refers to voters using representatives' past performance to decide which representative will yield the highest net gain for the voter in the upcoming period. The candidate who is expected to yield the highest net gain in the next period for the most voters is then expected to win the election. Similar to Key (1966), Downs (1957) also argued that voters base their decisions on past performance of the agents but with a slightly different endgame: Key's voters evaluate policy outcomes to either confirm

1. Healy and Malhotra (2012, 289) provide a useful four-step rephrase of the retrospective voting mechanism.

2. Refer to Kiewiet and Rivers (1984), Lewis-Beck and Stegmaier (2000), and Healy and Malhotra (2012) for more comprehensive reviews of this literature.

or fire the incumbent (i.e. pure retrospective voting), while Downs' voters evaluate policy outcomes during the last electoral cycle to make predictions about the candidates in the upcoming election. That is, past incumbent performance is used to make predictions about future outcomes and policy choices. One could argue that the results may differ between these retro- and prospective approaches.

Different authors have drawn different, sometimes contradictory, conclusions about prospective and retrospective voting (Lewis-Beck and Stegmaier 2000, 186-188), yet the presence of the economic vote is generally accepted and theoretically useful. Despite studying the same (U.S.) electorate with essentially identical data (usually Gallup polls, survey data from the American National Election Studies (ANES), and national GDP and unemployment data), some scholars have argued that their statistical results support one over the other and others have argued for a middle ground in which both forms of economic voting are important (186-188). Lewis-Beck and Stegmaier (2000, 188) argue that some of the differences could be explained by variations in the parameterization and estimation techniques. Nevertheless, the existence of the economic vote, retrospective as well as prospective voting behavior, is generally accepted by scholars to occur in elections and may even outweigh other factors such as partisanship as well as "social and religious cleavages" (Lewis-Beck and Stegmaier (2007, 530) as cited in Lewis-Beck and Stegmaier (2008, 304)). The economic vote remains theoretically important because the studies have consistently found economic factors to be essential to predict election outcomes [see e.g. Abramowitz (1988), Erikson (1989), and Lewis-Beck and Stegmaier (2000)].

The economic vote can further be divided into the egotropic (pocketbook) and the so-

ciotropic vote. Egotropic voters evaluate their own personal finances, while sociotropic voters evaluate the status of the national economy (Kinder and Kiewiet 1981). Formally, egotropic voting is based on Downs' (1957) rational voter who elects representatives on individual net-gain expectations. Sociotropic voting does not necessarily refer to altruistic voting, rather the assessment of a candidate's ability to promote general economic well-being (and thus, one's own well-being down the road) may just be a better indicator than pure pocketbook concerns yet just as selfishly oriented (Kinder and Kiewiet 1981, 132).

At face value, egotropic voting theory may seem more appealing than its sociotropic counterpart because it requires much less information gathering. One's own financial situation is easily accessible and understood by anyone. The cost of information gathering to appropriately assess the nation's economic situation (changes in real income, inflation and interest rates, unemployment, and the stock markets) is considerably higher. Considering any public's rather little, or superficial, attention to politics, the egotropic voting theory becomes even more appealing (130). Nevertheless, Kinder and Kiewiet (1981, 132) argue that voters merely need to form "rough evaluations" of the state of the national economy which should be rather easy, i.e. without significant information gathering costs. A review of various statistical evaluations of the economic vote suggests that sociotropic voting is more pronounced than egotropic voting (in U.S. presidential elections) (Lewis-Beck and Stegmaier 2000, 194).

Though the theoretical foundation for the economic voting literature was built upon observations from the U.S. electorate, they helped to refine economic voting theory and evidence for the economic vote, sometimes conditioned upon context, has since then been

found throughout the world. For example, Gélinau and Singer (2015) find evidence for economic voting in Latin America, Lewis-Beck (1986) finds economic voting in Europe, and Lewis-Beck and Stegmaier (2008) review a wealth of studies on transitional democracies. Duch and Stevenson (2006) use survey data from 19 countries to study the magnitude of the economic vote per country and over time. They find evidence of economic voting for most of the countries in the study which, of course, results in the average cross-national and cross-time economic voting coefficients to point in that direction as well. Lewis-Beck and Stegmaier (2000, 208-211) compare multiple cross-national studies of economic voting highlighting an interesting intervening factor which could explain why economic voting is more pronounced in some countries than in others: the political context. Coalition governments, party system fragmentation, and degree of economic development (developing vs. developed countries) condition the importance of the economy when casting a vote and affect blame attribution. Overall, the link between the economy and voting decisions seems undeniable with evidence from both the micro and macro level (211).

So far we can summarize that economic voting theory expects voters to engage in a cost-benefit analysis and retain or fire incumbents according to a function which maximizes net gain. In other words, voters use the reward-punishment mechanism of elections to reward well performing incumbents with another term and replace those incumbents who disappointed. The literature distinguishes between retro- and prospective voting, and further between socio- and egotropic voting. Empirical evidence is ambiguous as towards which mechanism, or which combination of mechanisms, is most appropriate to characterize electorate behavior, yet it is clear that some form of economic voting drives outcomes in elections throughout

the world. That means, electorates do consider events and performance indicators during an incumbent's term to form a decision on whether to reward the incumbent with their vote in the reelection cycle.

There is a myriad of potential vote influencing factors and there is, at least historically, disagreement on whether voters actually respond to such factors or whether observed voting behavior is just a manifestation of partisanship voting that uses economic voting as an excuse. Using his funnel analogy, Campbell et al. (1960) argued that voters develop partisanship which is usually inherited from their guardians, and which shapes the kind of news one consumes and thus, what kind of political positions and ideas one develops. Thus, partisanship becomes the main predictor of voting behavior and endogenous to all other potential factors. Issues and ideologies (as opposed to partisanship) are found to not matter. Berelson, Lazarsfeld, and McPhee (1954) extends this argument of inheritance and introduces peer pressure into the mechanism: the voter is pressured by his peers (school, church, family, etc.) into a specific voting pattern. Together, such hypothesized disregard of issues essentially characterizes the voter as a fool, to which Key (1966) famously disagreed. Key found a responsible electorate which votes according to issues and policy outcomes. In other words, Key expects voters to make reasonable, calculated decisions given the information and transparency on current problems and alternatives.

Multiple studies since have found electorates that are responsive to various issues and policies. Lewis-Beck et al. (2008) revisit Campbell et al. (1960) and start their discussion with the funnel analogy. Looking for what factors might play into voters' consideration, the authors claim that such a factor needs to be relevant, personal, and political (as opposed

to irrelevant, external, and non-political). In praxis, however, issues are difficult to classify with this scheme: some relevant yet external conditions, such as the economy, can become relevant and personal when candidates or the media make them the issue du jour. Overall, they find that candidate attributes, partisanship, and the socio-economic environment are all predictors of the vote. Economic factors such as the gross domestic or national product, unemployment, inflation, and economic outlook have all been found to impact vote decisions (also see Lewis-Beck and Stegmaier (2000) and Duch and Stevenson (2006)).

It is impossible to determine what specific factor in the ocean of possibilities weighs most in a single voter's decision, but the literature has uncovered two important general tendencies within any electorate: partisanship and the state of the economy. Partisanship is a complicated matter because it is inherited and biases a voter's evaluations. For example, a conservative incumbent usually will be favored by conservative voters and disliked by liberal voters. Also, a voter's perception of the state of the economy may be dependent upon the partisanship of the incumbent, thus, introducing the above mentioned problem of endogeneity. In more general terms, however, partisanship is a helpful indicator for voters because knowing the party affiliation of a representative allows voters to make general predictions on what kind of policy decisions they can expect from that representative (Downs 1957). Thus, voting on partisanship is like voting on a the resolution of a set of issues in a general, yet predictable way.

All of these empirical studies assume that voters' decisions can be approximated with an additive model which allows to include multiple factors and determine each factor's individual contribution to the vote outcome variable. As far as I am aware, the modeling decision

was made initially due to practical ease and has been grandfathered in. The studies cited throughout this review of the economic vote operationalize voters' decisions with individuals' actual or intended vote choice for or against the incumbent from survey data, or by aggregating to the electorate's decision with incumbent vote share or a binary incumbent reelection indicator. The studies then use additive model structures to determine and compare statistically significant effects and effect sizes of events and performance indicators on the dependent variable (cf. Kramer (1971), Monroe (1979), Lewis-Beck and Stegmaier (2000), and Lewis-Beck et al. (2008)).

Objective Performance-Based Evaluations?

The third, and last, assumption to be justified is that voters' evaluations of events and performance indicators are sophisticated enough for performance-based voting. Closely linked to the discussion on whether voters consider past economic performance is the sophistication of the voters' look in the past. Can voters objectively evaluate their representatives' performance throughout the entire term or do they rely only on the last couple of months as this time is still fresh in the voters' memory? Can voters evaluate at all? Further, is the evaluation objective or do voters suffer from cognitive biases?

Followers of Campbell et al. (1960) and Converse (1964) might characterize voters as outright 'stupid' because they lack ideological cohesion and knowledge about political issues and representatives. Others have labeled voters as 'myopic' (Healy and Malhotra 2009) or 'irrational' (Wolfers 2007) because they find electorates to punish incumbents for random external shocks (e.g. natural disasters or world price shocks) which the incumbent could not

have caused or prevented. As famously illustrated by MacKuen, Erikson, and Stimson (1992) who titled their study “Peasants or Bankers?”, retrospective voters are usually characterized as ‘naive’ because they use elections only to reward or punish incumbents, while prospective voters engage in ‘sophisticated’ net-gain as well as future-oriented behavior.

Key (1966), however, famously concluded that “voters are not fools”. An informed voting decision does not require cost intensive detailed research but the effective, goal-driven use of heuristics, or shortcuts, such as partisanship (Downs 1957), performance metrics such as rough estimates of the economy (Fiorina 1978; Kinder and Kiewiet 1981), and third-party informers as well as personal networks (Lupia and McCubbins 1998; Lupia 1994). According to this latter argument, the rational, utility maximizing voter may reasonably use overall economic well-being instead of individual policy net-gain analyses in order to reward/punish incumbents and form expectations about the future. In their seminal book *The Rational Public*, Page and Shapiro (1992) argue that on average voters learn just enough to, on the aggregate, make rational decisions and show a cohesive set of opinions, values, and ideologies.

Healy and Malhotra (2012, 285-289) briefly summarize both sides of this discussion and conclude that voters do indeed react to government performance while (a) relying on heuristics when making decisions, and (b) being subject to emotional and cognitive biases which then adversely affect elections being used to keep high-performing representatives and vote out the lemons. “Therefore, the relevant question may not be ‘Do voters react to government performance or not?’ but rather ‘Are voters reacting in the right way?’ ” (287).³

3. The interested reader is left to consult Healy and Malhotra (2012) for a brief overview of emotional and cognitive biases that influence voting behavior. The aggregate nature of this work’s cross-sectional time-series study makes it impossible to control for voters’ mood, partisanship biases, or losses of favorite sports teams. Thus, a review of that literature here would be moot.

Part of reacting the 'right way' is considering performance metrics throughout the entirety of the last term. This way incumbents are judged on all of their actions (either for retro- or prospective voting). However, scholarship has identified a potentially dangerous use of the aforementioned heuristics: if voters consider merely the last year's economic performance and use this as a shortcut to assess incumbent performance over the entire last term, then they create an incentive system in which elected officials could manipulate the economy to perform below full potential in the first years and create relatively higher growth in the 6 to 12 months prior to elections (Tufte 1978; Achen and Bartels 2004; Abrams 2006). This would overstate incumbent performance and increase incumbents' reelection chances beyond normal while possibly damaging the economy in the long-run. So, can voters be expected to be 'competent' retrospective evaluators?

In theory, competent retrospective voters would evaluate performance metrics throughout an incumbent's term, however this ideal might be impossible to achieve. Achen and Bartels (2004) engage this issue of 'competent' retrospective evaluation and begin by assessing the likelihood of anyone making an informed estimate of the incumbent's influence on economic growth. Given short-term fluctuations, seasonal adjustments, and external shocks "voters may have a good deal of difficulty discerning systematic changes in growth rates" (10). Further, the assessment of systematic change over 4 years may thus, be just as difficult as the assessment of the last 6 to 12 months. "In that case, *any* inference about the differential competence of specific administrations to produce future economic growth can be little more than a roll of the dice" (10). Thus, the competent retrospective voter may be an interesting discussion in theory, yet may matter little in praxis.

In fact, scholarship in this field seems to have (silently) agreed on the importance of recent economic performance metrics and the *unimportance* of long-term metrics. Achen and Bartels (2004, 15-21) briefly review some studies and provide some own estimations. They conclude that voters consider short-term performance measures while long-term indicators will only approach statistical significance when earlier parts of the term are increasingly discounted. Furthermore, we do see some evidence that elected officials might be taking advantage of business cycle dynamics. Achen and Bartels (2004, 21-26) find some evidence that economic growth rates are on average higher in election years than in non-election years, presumably underlining Tufte's concerns about incumbents manipulating economic growth to improve their reelection chances. Further, Kayser (2006, 2005) finds that European incumbents show opportunistic behavior when timing elections; booming (international) economy and trade are correlated with incumbents calling elections a little early. As national economies become more interwoven, Kayser finds that the countries experience boom periods at the same time and one can observe groups of incumbents scheduling elections accordingly. On top of the potential long-term damages that an artificially stifled economy could cause, research indicates that incumbents, when given options, will pass policies which will generate the most voter support instead of long-term benefits. For example, and as mentioned earlier, Healy and Malhotra (2009) and Gasper and Reeves (2011) find that disaster relief spending is more popular than disaster preparedness spending and thus, incumbents prefer the former over the latter.

In sum, theory and empirics suggest that electorates' evaluations of events and performance indicators are sophisticated enough to engage in performance-based voting. The

competent retrospective voter might not be achievable because it is difficult to determine what the incumbent caused from what happened due to external influences (shocks, trends, seasonal adjustments, etc.). Thus, voters rely on short-term heuristics, usually from 6 to 12 months before an election, when forming opinions about incumbent performance. Heuristics are general in nature which suggests that voters rely on big picture measures such as changes in the (world) economy and the occurrence of natural disasters rather than on detailed net-gain or damage indicators. The theoretic implication of the discussion above is that individual voters may be terrible evaluators but electorates on average make reasonable decisions. Voters are thus not completely stupid, myopic, or irrational but follow a somewhat rational dynamic. Theory and empirics suggest that the basic assumption of the retrospective voter who is competent enough holds despite the dynamics that information asymmetry can create in a principal-agent relationship. In practical terms for the following analysis the discussion above yields that the inclusion of economic variables for the national economy is the most efficient and effective way to control for other factors that might drive voting decisions. Further, we expect short-term heuristics to show statistical significance while measures over the entire term are expected to show no statistically relevant correlation with voting decisions.

External Shocks as Government Failures

This section argues that regarding external shocks as indicators of government failure fits in the rational retrospective voter paradigm. The previous sections established that voters use elections as referenda on incumbent performance. Voters use past events and

performance indicators as heuristics to judge incumbents. Voters are competent enough to make reasonable, cohesive decisions in the aggregate of the electorate. I argue that external shocks can be used as a heuristic to assess incumbent government performance because governments have shown (or suggested) to be able to influence external processes.

The previous section reviewed literature which suggests that voters can use domestic, or internal, economic performance indicators as heuristics to assess incumbent government performance and this section adds that international, or external, shocks can be used in the same way. Recall the concept of blind retrospection; Is punishing incumbents for shark attacks (Achen and Bartels 2013, 2002), losses of one's favorite sport team (Healy and Malhotra 2010; Healy, Malhotra, and Mo 2010), or changes in the international economy (in particular prices (Wolfers 2007)) rational voter behavior? Some argue that it is not: in these cases the voter would be punishing the incumbent for events that are truly external to the incumbent's sphere of influence. These studies use shark attacks and the performance of one's favorite sport game as instrumental variables in retrospective voting contexts in order to show tendencies of erroneous blame attribution. I argue that these instruments are quite fitting to show degrees of irrational voter behavior while effects of the international community on national elections, and of the national economy on local elections may be signs of both naive and competent retrospective voting behavior. On the one hand, blaming a local representative for bad national performance indicators may seem like naive blame attribution. Even if that local representative had pull with national representatives, blaming one local agent for failures of many national agents seems like bad blame attribution. Similarly, blaming your national government for a poorly performing international economy to which,

at the moment, 195 countries are contributing seems equally naive.

On the other hand, voters punishing national incumbents for changes in the world economy might not be as irrational as portrayed by some. Local officials usually are part of national parties and have some pull with their colleagues one step up. A U.S. governor can influence national politics via party ties or relationships with their respective state representatives. State representatives could then be expected to take care that their constituency profits from global economic developments. Similarly, national agents are part of a government which is involved in international politics. Regular meetings of, for example, the United Nations, Oil Producing Countries (OPEC), the European Union, and the Group of Eight (G8) as well as the G77 suggest that a voter's government is part of an international effort to influence international politics and the world economy. This involvement to control peace, trade, and prices, I argue, gives voters some reason to reward (or punish) national agents for positive (or negative) external shocks. Observations from monopsony boards in Africa (Bates 1981) to the (in)famous agricultural subsidies and import taxes in the United States of America and the European Union yield the reasonable expectation that the common voter has an understanding of how much national governments can affect the international as well as national economy no matter the degree of institutionalization of their democracy.

Additionally, I argue that punishing incumbents for natural disasters is also not necessarily irrational because natural disasters may indicate failures of the government. As defined further in the following section, natural disasters occur when a natural hazard coincides with a human population, overwhelming local authorities and infrastructure. Humans naturally seek out living spaces in exposed locations for their resources, fertile lands, or beauty: river

banks, coast lines, and mountainous terrain. This increases the baseline chance of a natural disaster because humans moved into relatively more dangerous regions. Just to give some examples: Rivers experience floods, coasts have storms and flooding, and mountains raise over fault zones. Politics, however, can significantly alter the inherent risk of these regions. City planning, building codes, and insurance requirements are some instruments that (local) politicians may use to reduce the risk of accidental injuries and deaths. Therefore, since natural disasters happen when societies increase their vulnerability to natural hazards beyond their coping capabilities, and since government policies are a major part of where its population settles and how it prepares for the impact of natural hazards, I posit that electorates may act rationally punishing incumbent governments for natural disasters. Natural disasters may act as heuristics of insufficient or sub-par policy making. Thus, Ahlerup (2013b, 1-2) had the right intuition: “it is plausible to assume that the fact that a natural disaster takes place at all is a signal of government incompetence in the eyes of the electorate”.

In sum, I posit that external shocks to the world economy and price system, or by natural disasters may be used as heuristics of incumbent government performance. In other words, voters may act somewhat rationally when punishing their incumbents for natural disasters, an external shock on which the incumbent had no influence only at a first glance. The element of human behavior in the definition of natural disasters (which is further developed in the upcoming paragraph) creates a connection between incumbent government behavior and the occurrence of natural disasters.

Definition of Natural Disasters

This section defines natural disaster and its subcategories while distinguishing related terms. After establishing that electorates use past events and performance indicators when engaging the reward-punishment mechanism of elections, we have to define natural disasters before we can discuss how they enter said mechanism.

To define a natural disaster, we must separate natural *process* from natural *disaster*. Events such as earthquakes, flooding, storms, droughts, or extreme rainfall are natural processes, or hazards, which do not lead to disasters unless they occupy the same time and space elements as humans. Only when humans clash with natural processes and the impact of the natural process overwhelms local authorities and their coping capacities, we speak of natural disasters (Wisner et al. 2014). In other words, in order to become a disaster, the natural process has to meet enough humans who are in a vulnerable space, are unprepared, and do not have the required coping capacity. The concept of the progression of vulnerability in the crunch model (Wisner et al. 2014) suggests that human behavior is key to determining when natural disasters occur and how severe they are going to be: a well-prepared society needs to clash with a strong natural process for this clash to produce a natural disaster. On the other hand, a society which is ill-prepared due to bad governance, lack of resources, diseases, or conflict, will have to meet only a relatively small natural process to experience a devastating disaster.

This study refers to a natural disaster as ‘a situation when a natural process has overwhelmed local authorities’ until the operationalization of natural disasters requires a measurable concept in the statistical chapters. Exactly when are authorities or societies over-

whelmed with the impact of natural processes? Following the prevalent natural disaster data providers, the DesInventar Project and the Emergency Events Database (EM-DAT) by the Centre for Research on the Epidemiology of Disasters (CRED), this answer is approximated by expanding the working definition of natural disasters by measurable concepts of damage dealt and humans affected, injured, or killed by the natural hazard.

The literature distinguishes between multiple kinds, or families, of natural disasters. There are six families of natural disasters: geophysical, hydrological, meteorological, climatological, biological, and extraterrestrial, where each family has a variety of main events. For example, meteorological events are convective storms, extratropical storms, extreme temperatures, fog, and tropical cyclones. Further, the literature distinguishes slow onset natural disasters (extreme temperatures, droughts, epidemics, and insect infestation) from rapid onset natural disasters (all others, such as earthquakes, floods, storms, etc.). Intuitively, slow onset natural disasters creep up on the human population, can take years to fully manifest, and can last years. Rapid onset natural disasters hit populated areas without any warnings. While earthquakes and tornadoes are intuitive examples of rapid onset disasters, hurricanes usually also fall in this category. Hurricanes may be announced a few days before they hit, but when compared to the slow onset and long duration of a famine or drought the choice of rapid onset for hurricanes seems more reasonable.

Mind the difference between natural and technological disasters. Natural disasters require the natural process affecting human life. Technological disasters such as chemical spills or structure collapses are the result of human behavior such as negligence. Broadly defined, there are three families of technological disasters: industrial accidents, transport accidents,

and miscellaneous accidents. For example, the Deepwater Horizon incident on April 20, 2010, was a technological disaster. Judge Barbier ruled that the incident occurred because of negligence of the builders, owners, and operators of the oil platform (Robertson and Krauss 2014; New Orleans Sun 2014; Case 2:10-md-02179-CJB-SS 2014): The parties ruled responsible should and could have been prepared.

I will not use the term of *man-made* disasters because the term can be confusing. Some may use man-made and technological disasters interchangeably. However, man-made disasters could also refer to wars or genocide. Others may argue that every disaster is man-made: humans choose to increase their vulnerability by for example, (a) moving into natural flood plains, earthquake prone zones, and coastal regions, (b) not committing resources to mitigation and the enforcement of building codes, and (c) starting wars.

The link between hazard strength and social vulnerability creates an endogeneity problem which leads to more disasters being observed in vulnerable countries. Better institutions and stronger economic development reduce vulnerability to disasters, yet with diminishing returns (Raschky 2008). This means we expect less natural disasters in developed societies than in struggling societies, given equal exposure to natural hazards. It may then even be reasonable to expect differing impacts of natural disasters in developed and developing countries. Barone and Mocetti (2014), for example, suggests that developed regions in Italy recuperate after earthquakes while regions with lesser quality institutions seem to further deteriorate.

Natural Disasters and Elections

The political impact of natural disasters was first examined by studies such as Barnhart (1925), Abney and Hill (1966), and Miller (1925). Barnhart (1925, 527) notes the importance of farmers' interests, the "farm bloc", to elections in the United States of America (USA) and provides a good review of the, then contemporary, literature on elections and political movements and how climatic and geographic factors entered the considerations of scholars. Barnhart (1925, 539-40) argues that the Farmers' Alliance decided to become politically active before the effects of the drought were felt and thus, the rise of populism in Nebraska was due to the already badly performing economy. However, the drought exacerbated the situation and supposedly led to more voters punishing the Republican Party for the bad economy. Thus, natural disasters can act as amplifiers, or intervening variables. Miller (1925, 476,487) makes short mention of rainfall patterns influencing first the boom in Kansas until the mid 1880s, and then the burst of the economic bubble after 1885. In addition to a drought starting around 1886, the population was already struck by high inflation, significantly decreased agricultural prices, and many mortgages. Miller argues that these developments gave rise to populism in Kansas. Abney and Hill (1966) examine electoral results and interviews after hurricane Betsy hit New Orleans in 1965 and conclude that Betsy had no clear impact on electoral results. Even though affected citizens claimed that the local government was ill prepared and did not issue appropriate warning, voters did not punish the incumbent mayor. Abney and Hill (1966, 980) argue that this was so because the incumbent was capable of handling the situation as well as his public portrayal. This account suggests that the effect of natural disasters on incumbents' electoral performance

is itself conditioned on intervening variables such as the effectiveness of the government's disaster response and its portrayal in the media.⁴

The natural disasters and elections nexus was recently picked up again to illustrate the practical shortcomings of retrospective economic voting based on individuals' subjective welfare. Achen and Bartels (2013) famously posit with the title of their study that "shark attacks are bad for democracy" because President of the USA Woodrow Wilson lost vote share to shark attacks in the 1916 election in multiple counties of New Jersey as compared to the 1912 election. Further, the authors argued that the U.S. voter has repeatedly punished incumbents for low or heavy rainfalls. Achen and Bartels (2013, 2002) used this analysis to show that voters cannot objectively evaluate their own economic well-being in order to discern incumbent performance. Instead, voters mix acts of the incumbent with acts of God, a habit which the authors term '*blind* retrospection'. Achen and Bartels (2013, 2002) thus conclude that the results cast doubt on the theory of rational voters engaging in retrospective economic voting and the democratic accountability it is supposed to produce.

The political science literature has taken this hypothesis of voters punishing incumbents for natural disasters and reaffirmed it in the context of many case studies and even in cross-country research designs and over time. In cross-sectional studies, Quiroz Flores et al. (2013) and Chang and Berdiev (2015) linked natural disasters to the removal of political leaders and Brückner and Ciccone (2011) find that negative rainfall shocks cause advancement of democratic institutions in sub-Saharan African countries. Brückner and Ciccone (2011) put forward the concept of a window-of-opportunity in the aftermath of natural disasters which

4. Achen and Bartels (2013, 3-5) provide further references to anecdotal evidence from the pharaohs in Egypt to 19th century politics in the United States of America.

Ahlerup (2013a, 2013b) argue can lead to democratization or civil turmoil.

However, the mechanism on how natural disasters impact electoral outcomes has not been extensively theorized because initial studies treated natural disasters as instrumental variables to engage the retrospective economic voting literature. Consider recent, popular studies such as Healy (2008) and Achen and Bartels (2002, 2013). The authors consider their studies tests of the assumption of rational voters using their own economic well-being as a heuristic to judge incumbent performance during their retrospective voting decision. The authors' theoretical expectation is straight forward: a rational voter would not punish for 'bad luck' or 'shark attacks'. A separate theory for the electoral impact of natural disasters was not needed because the theoretical foundation is the retrospective economic voting literature which was tested here with a new, instrumental variable and according to which one would not have expected any statistically significant influence of natural disasters on incumbent vote share or survival chance.

The interesting thing about natural disasters was not *why* they had an electoral impact but that they had one and what it meant for democratic accountability. In other words, many works in this particular field of research are extensions of some aspects of the debate between Campbell et al. (1960) on one side and Downs (1957), Key (1966), Kramer (1971), and Fiorina (1981) on the other side which argued for a much more capable, evaluative electorate. Here, a voter punishing incumbents for random external shocks seems like irrational behavior. The negative effect to democratic accountability is exacerbated by voters' cognitive and emotional biases which cloud their judgment when attributing blame (Forgette, King, and Dettrey 2008; Cole, Healy, and Werker 2012; Healy and Malhotra 2012).

If one was to accept that punishing incumbents for natural disasters is not random or irrational behavior as I lay out in a section above, then one can ask how this happens, whether this behavior is conditioned on other, intervening factors, and how this knowledge can be used to one's advantage. I argue that punishing incumbents for natural disasters is not necessarily irrational because natural disasters may indicate failures of the government. Natural disasters occur by definition when a natural hazard coincides with a human population, overwhelming local authorities and infrastructure. Ergo, natural disasters happen when societies increase their vulnerability to natural hazards beyond their coping capabilities. And since government policies are a major part of where its population settles and how it prepares for the impact of natural hazards, I posit that electorates may act rationally when punishing incumbent governments for natural disasters as they may act as heuristics for government failure. This realization then elevates a research question about natural disasters impacting voting decisions from a fun exercise of little interest to incumbents to a valid part of the rational voter paradigm with value to academia and political incumbents.

Several studies have illustrated how knowledge of the electoral impact of natural disasters can be used to the incumbent's advantage. For example, Garrett and Sobel (2003) and Reeves (2011) find that presidents are more likely to grant presidential disaster declarations and increased funding in competitive states. Presidents are trying to buy votes. Further, the results in Chen (2008) suggest that FEMA⁵ disaster aid was distributed disproportionately to core Republican neighborhoods after the summer 2004 Florida hurricane season in order to maximize votes in the upcoming Presidential election. In a follow up study, Chen (2013)

5. FEMA refers to the United States of America's Federal Emergency Management Agency.

finds that the 2002 and 2004 FEMA disaster aid after hurricanes in Florida increased voter turnout for the incumbent while decreasing turnout for the opposing party.

Moreover, incumbents face an altered incentive system when disaster relief is more electorally beneficial than preparation and mitigation. Healy and Malhotra (2009) conclude that the U.S. electorate rewards incumbent presidents for disaster relief spending but not for disaster preparedness spending. Similarly, Shughart (2006) finds that new public works bring more political capital than maintaining old ones. Thus, incumbents would be expected to have little interest to prepare and mobilize prior to natural disasters. Furthermore, a history and continued promises of (international) disaster relief creates a moral hazard problem where individuals do not prepare for natural disasters but wait for the national government to step up (Shughart 2006), and where the national government waits for international help because international help organizations continue to feel compelled to help while knowing about the moral hazard problem (Weiss Fagen 2008, 5). Finally, Depoorter (2006) adds that when multiple government layers and agencies are involved in disaster management, one agent's effort to prepare has positive externalities for all others thus creating a free-rider problem. In the end, no one prepares and when disaster strikes finger-pointing becomes the dominant strategy.

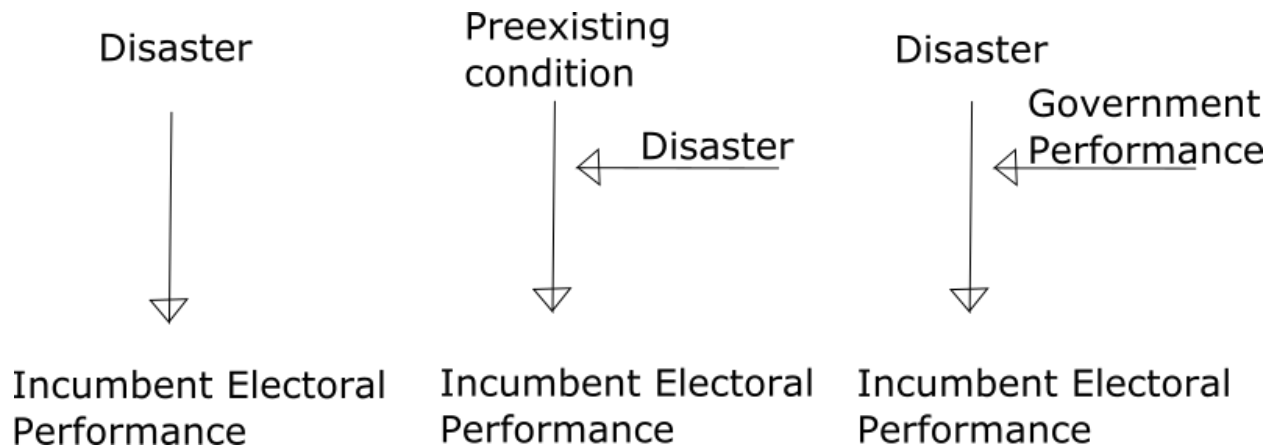
Based on anecdotal evidence, Pelling and Dill (2006) offer seven hypotheses connecting natural disasters and national socio-political conditions, while they maintain that overall natural disasters are catalysts, not triggers, of change. Pelling and Dill (2006, 3-5) posit that (i) "disasters often hit politically peripheral regions hardest [, thus] catalysing regional political tensions", (ii) "disasters are a product of development policies and can open to

scrutiny dominant political and institutional systems”, (iii) “existing inequalities can be exacerbated by post-disaster governmental manipulation”, (iv) “the way in which the state and other sectors act in response and recovery is largely predicated on the kind of political relationships that existed between sectors before the crisis”, (v) “regimes are likely to interpret spontaneous actions by non-government sectors in the aftermath of a disaster as a threat and respond with repression”, (vi) “in the aftermath of disaster, political leaders may regain or even enhance their popular legitimacy”, and (vii) “the repositioning of political actors in the aftermath of a disaster unfolds at multiple scales”. The variety of these hypotheses illustrates that the connection between natural disasters and socio-political conditions has a lot of insight to offer; much more than the view of random punishment for external shocks would.

There are three possible pathways of the mechanism according to which natural disasters would have electoral impacts. After surveying the respective literature, I conclude that there are three potential dynamics on how and why natural disasters would influence electorates’ voting decisions and thus, the incumbent vote share and survival in leadership positions. Figure 2.1 provides some visual cues towards the workings of these three possible pathways which are laid out in the following paragraphs.

First, natural disasters could be a direct determinant of the electoral fate of candidates when voters punish incumbents for random external shocks to the political or economic system. As discussed above, Achen and Bartels (2002, 2013) call this phenomenon blind retrospection while I posit that natural disasters could reasonably be interpreted as signs of governmental failures. As argued above, natural hazards turn into natural disasters when

Figure 2.1: Three Proposed Pathways of the Electoral Impact of Natural Disasters.



they coincide with humans and overwhelm local authorities. And since policy making is part of increasing the constituency's exposure and vulnerability to hazards, the occurrence of natural disasters may be used as a heuristic for government failure. Whichever reason it may be, it is necessary and sufficient for this mechanism that the electorate *perceives* the incumbent government at fault for either causing the event or not doing enough to prevent it. Other studies such as Healy (2008), Quiroz Flores et al. (2013), Chang and Berdiev (2015), and Ahlerup (2013b) would also fit this pathway because their research designs do not offer other ways on how the explaining variable would impact the dependent variable.

Second, natural disasters may act as a catalyst, pushing more unsatisfied citizens towards political action by exacerbating existing conditions or revealing them. The accounts by Barnhart (1925), Miller (1925), and Pelling and Dill (2006) certainly fit into this category: natural disasters make pre-existing conditions worse and bring weaknesses to the open. Increased suffering and revealed failures of the government will undoubtedly provoke more scrutiny and subsequent blame attribution. As a result, voters are expected to punish

incumbents by replacing them with agents who bring reform. Indeed, several studies suggest that natural disasters increase political actions. Brückner and Ciccone (2011) and Ahlerup (2013a) argue that natural disasters create a window of opportunity which can be used for democratization. On the other hand, Nel and Righarts (2008) - and a growing literature inspired by them - argue that natural disasters can also lead to violent intrastate conflict, especially so in societies with high inequalities and slow economic growth. Taken together, I would argue that these studies indicate that natural disasters may indeed increase scrutiny and expectations of functional institutions so that incumbents can choose to either offer reform to appease the electorate or resist reform and face the potential for conflict.⁶

More examples of natural disasters acting as catalysts for action are provided by Sinclair, Hall, and Alvarez (2011), Baccini and Leemann (2013), and Barone and Mocetti (2014). Sinclair, Hall, and Alvarez (2011) find that affected voters turn out to vote after natural disasters, possibly even more so than non-affected voters. This finding is nice for democratic accountability because one might otherwise think that resource-limited voters (affected citizens) might have different priorities or limited capability to go vote. Baccini and Leemann (2013) find that Swiss voters are more likely to vote for environmental policies after natural disasters.

The third pathway hypothesizes that natural disasters impact elections only if an intervening variable creates the right environment. In other words, natural disasters may have positive, negative, or no electoral impact depending on what intervening factors are present.

6. Another alternative is that governments use natural disasters to reduce political opposition by not helping after the disasters when the initial impact did not already kill of the opposition. Pelling and Dill (2006, 3)'s hypothesis of post-disaster government action increasing inequalities fits very well here.

In the words of Lewis-Beck et al. (2008), an issue needs to be relevant, personal, and political (as opposed to irrelevant, external, and non-political) and a visit of the government official to the disaster site, the promise of government aid, or media coverage can make a natural disaster relevant, personal, and political.

The third pathway, an intervening variable making natural disasters politically relevant, was already implied in various studies. Pelling and Dill (2006)'s 6th and 7th hypotheses definitely fit into this category. Also, Sen (1999, 7-8) famously concluded that countries with a free press have not seen a famine yet. The logic of this argument is that a free press provides enough scrutiny to pressure governments into good policy making. Indeed the importance of scrutiny, framing, and priming to public opinion and blame attribution after natural disasters has been made obvious in the context of both the United States of America (Abney and Hill 1966; Littlefield and Quenette 2007; Brox 2009; Bodet, Thomas, and Tessier 2016) and internationally (Bechtel and Hainmueller 2011; Lazarev et al. 2014). Littlefield and Quenette (2007, 26), in particular, observed "that the media stepped outside their role of objective observer and assumed a privileged position to point blame toward those with legitimate authority".

Another factor which intervenes on the electoral impact of natural disasters is the quality of government response (Bechtel and Hainmueller 2011; Cole, Healy, and Werker 2012; Barone and Mocetti 2014; Lazarev et al. 2014). Quite intuitively, good response should be expected to be rewarded by the electorate while bad response should be punished. Following the logic of blind retrospection, Lazarev et al. (2014) start out by expecting wild fires in rural Russia in 2010 to damage public opinion of the incumbent local and national government.

However, the authors find that good government response yielded a long-term electoral benefit for the incumbents. Even more, locals showed especially high approval of incumbents when good government response was paired with meeting high government representatives visiting the area (Lazarev et al. 2014, 642). Finally, stressing the importance of institutional quality in a study of earthquakes in two Italian regions, Barone and Mocetti (2014) find that preparation, relief, and reconstruction are better in regions with higher institutional quality. Regions with lower quality institutions, especially with corruption, are likely to further deteriorate institutional quality sending the region in a downward spiral.

The second and third pathway may seem identical, especially in the practical application: both pathways suggest the natural disaster indicator to be interacted with another variable. Theoretically however, I argue that the distinction is worth making. Pathway two suggests that natural disasters are conditioning the effect of another variable on the electoral outcome. Alone then, natural disasters should have no impact. The third pathway posits that the effect of natural disasters is conditioned by another variable. This pathway allows for a direct effect of natural disasters on electoral outcomes that can take on different directions and magnitudes according to the intervening variables.

Despite ample evidence of the electoral impact of disasters through all three pathways, it remains to be seen whether these mechanisms can be aggregated to generalized tendencies across electorates and over time. I was able to find only very few studies examining the impact of natural disasters to incumbents' electoral performance and survival in leadership positions which employed a research design that aims at producing generalizable expectations. While very sophisticated, most studies in this field focus on single events and countries, mostly

events in the United States of America. This does not make the studies any less important, yet raises the question of whether we can accept their evidence as generally valid and not limited to the U.S. electorate.

Table 2.1 presents an overview of the literature on the electoral impacts of natural disasters sorted by three families (columns): single-country studies of the USA, single-country studies of countries other than the USA, and cross-sectional studies. Those who have read Remmer (2014) and Ahlerup (2013a, 2013b) will note quickly that the authors did not find consistent results in their statistical analyses. In fact, Remmer (2014) rejects a generalizable tendency of blind retrospection in the Caribbean and Ahlerup (2013a, 2013b) finds statistically significant results only for transitional democracies. Therefore, in addition to more formal theorizing, this particular research field can profit a lot from cross-sectional time-series analyses as done in this particular study.

Study Goals And Hypotheses

The central research question of this study is ‘Do electorates punish incumbents for natural disasters?’. Assuming (a) the validity of the punishment-reward mechanism for elections in the economic voting as well as democratic accountability literatures and (b) that electorates include natural disasters as part of their incumbent performance evaluation, the first hypothesis posits that electorates punish incumbents for natural disasters. Hypothesis 1 expects an electoral impact of natural disasters void of pre-existing conditions and intervening

a. Healey (2002) describes the opportunity the 1944 San Juan, Argentina, earthquake provided for Colonel Juan Domingo Peron to gather influence and reputation.

b. Their survey data suggests that anti-regime sentiment increases after natural disasters.

Table 2.1: Studies on the Electoral Impact of Natural Disasters

Single-Country Studies (US only)	Single-Country (non-US)	Studies	Multi-Country Studies
qualitative studies: Crosby (1989), Barnhart (1925), and Miller (1925)	Argentina: Healey (2002) ^a		Caribbean: Remmer (2014)
mayoral election: Abney and Hill (1966), Arceneaux and Stein (2006), Lay (2008), Brox (2009), and Sinclair, Hall, and Alvarez (2011)	Australia: Bodet, Thomas, and Tessier (2016) Chile: Carlin, Love, and Zechmeister (2014) ^b Colombia: Gallego (2012)		Quiroz Flores et al. (2013) Chang and Berdiev (2015) Brückner and Ciccone (2011), Ahlerup (2013a, 2013b)
state legislatures: Twigg (2008) and Healy (2008)	Czech Republic: Potluka and Slavikova (2010)		
gubernatorial and presidential elections: Gasper and Reeves (2011)	Germany: Bechtel and Hainmueller (2011)		
presidential elections: Healy and Malhotra (2010, 2009), Achen and Bartels (2002, 2013), Chen (2008, 2013), and Velez and Martin (2013)	India: Besley and Burgess (2001) and Cole, Healy, and Werker (2012) Russia: Lazarev et al. (2014) and Szakonyi (2012)		
Congressional elections: Dodlova and Zudenkova (2016)	Switzerland: Baccini and Lee-mann (2013)		

variables. Following the literature cited above, the initial expectation of the relationship is hypothesized to be negative: more disasters should lead to smaller incumbent vote shares in the following elections. To reiterate,

H 1. *Natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election.*

The proposed mechanism for hypothesis 1 is that voters regard natural disasters as failures of the government. Natural disasters are defined above as natural hazards overwhelming local authorities. Ergo, natural disasters happen when societies increase their vulnerability to natural hazards beyond their coping capabilities. Since government policies are a major part of where its population settles and how it prepares for the impact of natural hazards, I assume that electorates punish incumbent governments for natural disasters as they manifest due to insufficient or sub-par policy making. Thus, natural disasters may be regarded as failures of the government.

Hypothesis 2 posits that the electoral impact of natural disasters is negative in low-corruption societies and positive in high-corruption societies. As described above, studies within the United States of America usually find that electorates punish incumbents for natural disasters. The few studies that have examined this link in a cross-country research design have either found ambiguity or effects conditional on regime type. I propose that the population's experience with everyday corruption is the intervening variable which provides the missing link between samples of countries with differing regime types or at different stages of democratization. Safe the exception to the rule, I expect that in general societies with little experience of everyday corruption will regard natural disasters as governmental failures

which deserve to be punished, while societies with much experience of everyday corruption appreciate heightened governmental involvement in relief efforts which are rewarded at the polls. To reiterate,

H 2. *Natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies.*

The mechanism for hypothesis 2 is that governments in high-corruption societies have the general chance to increase their popularity through the distribution of extra, unexpected pork. I assume that voters in neo-patrimonial societies expect their representatives to bring home pork in exchange for past and continued votes. Electorates in high-corruption societies are not used to government involvement without prior bribes or other tangible benefits for the involved government officials. Thus, governments in high-corruption societies are more likely to be perceived as the knight in shining armor who goes beyond and above what was previously expected when providing disaster relief. In low-corruption societies, however, I assume that natural disasters are regarded as government failures and extraordinary expenses towards governmental disaster relief should best be avoided with appropriate policies before disaster strikes. In other words, this assumes an electorate in low-corruption societies that may certainly appreciate relief when in need but still blames the government for allowing the disaster to happen in the first place.

Hypothesis 3 posits that incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. This hypothesis assumes an interacted relationship between corruption and international disaster aid

flows. To reiterate,

H 3. *Incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies.*

The mechanism for hypothesis 3 is that well-performing governments in high-corruption societies are expected to bring in international pork, while well-performing governments in low-corruption societies are expected to help themselves. I assume electorates in high-corruption societies reward their governments for pork and especially for government efforts to secure international pork. On the other hand, I assume that electorates in low-corruption governments expect resources and institutions to be in place to prepare for and provide relief during natural disasters. Needing international help would then be regarded as government failure. In other words, the degree of experience to everyday corruption in a society is assumed to be linked to the electorate's wish to receive pork and its evaluation of government performance.

The theoretical goal of this study is to provide (i) a possible intervening variable which could link those studies which found negative electoral impacts of natural disasters with those which found positive or ambiguous results, and (ii) an expectation for a general tendency on what reaction incumbent governments can expect from their electorate for bringing in international disaster aid. Regarding the former, if a link was found which produces replicable general results of the electoral impact of natural disasters without requiring splitting samples, I could help explain why some studies on the electoral impact of natural disasters produce ambiguous results and seem to reject the common wisdom. Moreover, future studies could profit from overall increased sample sizes when determining the effects of pre-existing

conditions (such as economic crises) or conditioning variables (such as media framing or the quality of government preparation and relief efforts). Regarding the latter, understanding more about the electoral consequences of international disaster aid for incumbents could help explain why some governments do not request or refuse to accept international disaster aid.

The empirical goal is to provide a cross-country cross-time empirical test of whether the finding of electorates punishing incumbent governments for natural disasters can be generalized beyond the United States of America. The prevalent studies claiming that electorates punish incumbents for natural disasters are widely cited and usually without providing the context that the cited results are conditioned on samples from the U.S. electorate. Given the small number of cross-country studies and their ambiguous results, I consider it very useful to supplement the single-country studies with an aggregate cross-country cross-time research design which can control for the proverbial ‘all else’ to support (or undermine) the general tendency in which existing studies are being cited.

Conclusion

This chapter has presented theory and testable hypotheses of the electoral impact of natural disasters on incumbent governments and the chance of survival in leadership positions. Based on the literatures of democratic accountability, (retrospective) economic voting, and blame attribution, we can conclude that voters may use natural disasters as heuristics for government failure. I propose three pathways of a mechanism which suggests that natural disasters affect electoral outcomes: natural disasters as direct factors, natural disasters as catalysts for change, and natural disasters as factors of electoral fate only if an intervening

variable makes the event politically important.

I argue that natural disasters can be used as heuristics for government failure and that voters may use these events in their reward-punishment mechanism as part of their rational retrospective economic voting behavior. I derive three testable hypotheses: (i) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election, (ii) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies, and (iii) incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies.

Summary

The section on democratic accountability lays the foundation for electorates using elections as a performance-based reward-punishment mechanism. Election or reappointment cycles keep elected representatives in line because they incentivize good behavior. Even though the relation between voters and elected representatives (or supervisors and non-elected officials) is assumed to be one of principals (voters) and agents (representatives) which comes with the typical problems of such relationships, the basic assumption of electorates using elections as a performance-based reward-punishment mechanism stands.

The section on economic voting theory suggests that voters engage in a cost-benefit analysis and retain or fire incumbents according to a function which maximizes net gain. In other words, voters use the reward-punishment mechanism of elections to reward well

performing incumbents with another term and replace those incumbents who disappointed. Electorates consider events and performance indicators during an incumbent's term to form a decision on whether to reward the incumbent with their vote in the reelection cycle. The literature distinguishes between retro- and prospective voting, and further between socio- and egotropic voting. Empirical evidence is ambiguous as towards which mechanism, or which combination of mechanisms, is most appropriate to characterize electorate behavior, yet it is clear that some form of economic voting drives outcomes in elections throughout the world. Finally, scholars have (silently) accepted that voters' decisions can be modeled with additive models which allow to include multiple factors and determine each factor's individual contribution to the vote outcome variable.

The section on the sophistication of the retrospective economic vote suggests that voters' evaluations of events and performance indicators are sophisticated enough to engage in performance-based voting. Voters rely on short-term heuristics, usually from 6 to 12 months before an election, when forming opinions about incumbent performance. Heuristics are general in nature which suggests that voters rely on big picture measures such as changes in the (world) economy and the occurrence of natural disasters rather than on detailed net-gain or damage indicators. Individual voters may be terrible evaluators but electorates on average make reasonable decisions. Theory and empirics suggest that the basic assumption of the retrospective voter who is competent enough holds despite the dynamics that information asymmetry can create in a principal-agent relationship. In practical terms for the following analysis the discussion yields that the inclusion of economic variables for the national economy is the most efficient and effective way to control for other factors that might drive

voting decisions. Further, we expect short-term heuristics to show statistical significance while measures over the entire term are expected to show no statistically relevant correlation with voting decisions.

The section on external shocks as signs of government failure argues that external shocks to the world economy and price system, or by natural disasters may be used as heuristics of incumbent government performance. Voters may act somewhat rationally when punishing their incumbents for natural disasters, an external shock on which the incumbent had no influence only at a first glance. The element of human behavior in the definition of natural disasters creates a connection between incumbent government behavior and the occurrence of natural disasters.

The section on the definition and families of natural disasters defines a natural disaster as a situation when a natural process coincides with human population and has overwhelmed local authorities. Examples of natural processes, or hazards, are earthquakes, floods, and unusually heavy or low precipitation. The literature distinguishes between families of natural disasters (geophysical, hydrological, meteorological, climatological, biological, and extraterrestrial natural disasters) with respective subcategories. Further, the literature distinguishes slow onset natural disasters (extreme temperatures, droughts, epidemics, and insect infestation) from rapid onset natural disasters (all others, such as earthquakes, floods, storms, etc.).

The definition of natural disasters creates an inherent endogeneity problem. Better institutions and stronger economic development reduce vulnerability to natural hazards. Thus, given equal exposure to natural processes a strong society will need to meet a strong natural

hazard to experience a natural disaster. On the other hand, a weak society only requires a relatively weak natural hazard to experience a major natural disaster.

Natural disasters are different from technological disasters because the latter is purely the result of human behavior (i.e. there is no natural process involved, only human negligence). Technological disasters are not part of this study. Further, the term ‘man-made disaster’ will be avoided due to its confusing meanings.

The section on the electoral impact of natural disasters provided a more formalized way of thinking about the connection of natural disasters to incumbent electoral performance and survival in leadership positions than is common in related publications. Most studies in this field rely on a theory born out of data and empirics and their literature reviews thus focus only on the results of preceding studies. Other studies use natural disasters as instrumental variables to examine perceived problems in the retrospective voting literature and its consequences for democratic accountability. This particular field can be argued to expect *no* correlation between natural disasters and a rational voter’s evaluation of an incumbent: In this context, a theory of why natural disasters should be expected to have any influence on incumbent vote share and survival in leadership positions is not needed because the ideal result of these studies is to show no correlation.

Natural disasters are hypothesized to affect electoral outcomes via three pathways: (i) natural disasters are direct determinants of incumbents’ electoral fate, (ii) natural disasters may act as a catalyst, pushing more unsatisfied citizens towards political action by exacerbating existing conditions or revealing them, and (iii) natural disasters impact elections only if an intervening variable creates the right environment. There is ample evidence for all three

pathways. However, this evidence is based mostly on single-country and even single-event studies. Of those studies, most draw their conclusions from an analysis of the electorate of the United States of America. Studies with research designs aimed at deriving tendencies which can be generalized across countries and across time are rare and ambiguous in their results.

The section on research goals and hypotheses posits three hypotheses: (i) Natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election, (ii) natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies, and (iii) incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. The theoretical goal of this study is to provide (i) a possible intervening variable which could link those studies which found negative electoral impacts of natural disasters with those which found positive or ambiguous results, and (ii) an expectation for a general tendency on what reaction incumbent governments can expect from their electorate for bringing in international disaster aid. The empirical goal is to provide a cross-country cross-time empirical test of whether the finding of electorates punishing incumbent governments for natural disasters can be generalized beyond the United States of America.

Chapter 3

Concepts and Measurements

This chapter describes the data gathering process and explores the relationship of the individual explanatory variables with the dependent variable. Close attention is paid to the nuances of each data source, coding decisions, assumptions, and data corrections. Goal of the preliminary analyses is to explore bivariate relations and conditioning effects of hypothesized interactive variables.

The analyses in the following chapter are based on two data sets, a national-level data set with national aggregates for all variables and a constituency-level data set which includes national aggregates for the economic and institutional controls while the natural disaster data is disaggregated to the constituency level. The national-level data set includes 793 observations, covering 793 national lower house elections in 111 countries. The panel is highly unbalanced with elections as early as 1962 and as late as 2015, while the minimum count of observed elections per country with available lagged vote share is 1 and the maximum is 27. The constituency-level data set includes 1176 observations, covering 74 national lower house elections in 333 first-order sub-national administrative regions (hereinafter referred to as constituencies) of 17 countries. This panel is also highly unbalanced with elections as early as 1980 and as late as 2014, while the minimum count of observed elections per country with available lagged vote share is 1 and the maximum is 12.

The data search limited observations to the post 1945 period for three reasons. First, the stability of the international state system and the status of the respective country's democracy need to be considered when studying elections. The World Wars represented a significant fall-back after the first wave of democratization. Thus, focusing on the post World War II period allows for uninterrupted election time-series in all European countries. Further, most African, Latin American, and Asian countries do not enter the group of democratic elections until after the 1960s and 70s. Second, most control variables, such as economic growth variables, usually are not available for periods before 1945, if even before 1950. Third, the nuances of each data source still require quite a bit of consideration before the data sets can be merged. Hence, in order to make this project feasible, the time frame had to be adjusted.

Table 3.1 yields a brief overview of the variables in the upcoming analyses. It links the factors which were hypothesized to influence the dependent variable (DV) to their proxy variables in the data sets, and lists the respective unit of measurement, sources, and the direction of the hypothesized relationships.

The preliminary analysis finds (a) some support for influence of natural disasters on incumbent electoral performance, (b) otherwise unexpectedly small correlations between the individual explaining variables and the dependent variable, (c) some support for a conditioning effect of patronage expectations, and (d) little support for electorates' previous experience with natural disasters conditioning the effect of new natural disasters on incumbent electoral performance. The preliminary analysis uses bivariate Pearson correlations to explore bivariate relations, Welch t-tests to explore difference in means between observations which

Table 3.1: Overview of Proxies, Expected Effects, and Data Sources

	Hypothesized Influencing Factor	Unit of Measurement	Source	Hypothesized Directional Relationship to DV
<i>Dependent Variable:</i>				
Incumbent party electoral success	National incumbent party vote share in general lower house elections	interval variable, percent (range 0.00 to 100.00)	CLEA	
<i>Independent Variables:</i>				
impact of natural disasters	occurrences of natural disasters	count variables	EM-DAT, DesInventar	+ in patronage societies, - in non-patronage societies
level of economic development	real GDP per capita	interval variable, in 2011USD at PPPs, log transformed	PWT 9.0	+
current economic performance	change in GDP per capita	interval variable, percent (range -100 to +100)	PWT 9.0	+
	unemployment rate	interval variable, percent (range 0 to 100)	WDI	-
international humanitarian aid	gross disbursements of OECD ODA humanitarian aid	interval variable, million real 2011USD	OECD Credit Reporting System	+ in patronage societies, - in non-patronage societies
electorates' expectation of patronage	most common form of linkage between party and constituents	ordinal variable, range programmatic (1) to clientalistic (5)	VDEM	intervening variable conditioning the effect of natural disaster occurrence and international humanitarian aid
electorates' experience with natural hazards/disasters	natural disaster risk index	interval variable, range 0 to 10 (maximum risk)	INFORM	intervening variable conditioning the effect of natural disaster occurrence

experienced the treatment effect (natural disasters) and those who did not (control group), and graphs as well as incremental (type II) F-tests to assess model fit with and without the proposed interaction terms.

This chapter starts by discussing the data, its sources, and any assumptions, corrections, or coding decisions that had to be made during the process. The discussion starts with the dependent variable, incumbent electoral performance, before moving on to the main explanatory variable, natural disaster occurrences, and ending with the institutional and economic control variables. The narrative then proceeds by exploring the bivariate relationships between individual explaining variables and the dependent variable. The final section explores the hypothesized intervening influence of electorates' expectation of patronage and experience with natural hazards on the effect of natural disasters on incumbent electoral performance. A conclusion and summary reiterate the main points of the discussion.

The Dependent Variable

This section defines the terms *incumbent government* and *incumbent electoral performance* as measurable concepts. Recall, the central research question of this study is 'Do electorates punish incumbents for natural disasters?'. The goal of a statistical analysis then presents the necessity to define *incumbent* and *electoral performance* as measurable concepts and to find proxies, or data, for this dependent variable.

By *incumbent* this study understands the political party which at the time of an election is in charge of the national lower house. A political party is in charge of the national lower house when it has the portfolio of the head of government (HoG). In most cases, the party which

fills the position of HoG also has the most seats in the national lower house. However, having the most seats is neither a necessary nor sufficient condition to be in charge. Consequently, the party in question must have won the previous election, i.e. entered government filling the HoG position, in order to be the incumbent in the subsequent election.

The decision to assert control of the lower house to the party with the HoG position instead of the party with the most seats rests on two practical coding implications. First, in case of (nearly) identical seat distribution either within coalitions or between the ruling and opposition party or coalition, the HoG position is a good indicator of which party has control over the most influential government positions and resources. Second, tracking the party with the HoG position makes coding coalitions easier. A coalition of parties cannot be considered an incumbent for the simple reason that countries see coalitions change from election to election not just in name but also in composition with former allies now clearly distancing themselves from each other. With the breakup of the governing coalition before each election, the data set would have several countries which never have their incumbents up for reelection. Instead, it makes more sense to track the influential person or party around which governing coalitions form and who usually get the HoG portfolio. For example, Mauritius saw about nine different governing coalitions between 1967 and 2014 within the respective 10 elections. The Labor Party was the main coalition partner and holder of the HoG portfolio in seven out of the nine governing coalitions. Thus, the Labor Party was coded as the winner and incumbent in the respective elections. Other examples of tracking the main actor across coalitions and elections, are the Lakas party in the Philippines and the Peronist parties in Argentina.

A party's incumbent status in the national lower house shall not be altered when the party fills the HoG position with a different individual. It is advantageous to focus on parties and not individuals because individuals can be removed for a variety of reasons. Term limits, for example, could introduce variance to a dependent variable which focuses on individuals. However, it would be nonsensical to make inference about incumbent executive heads being punished when they have to leave office due to term limits and the successor is from the same party.

This study focuses on lower house elections because it allows to mix both presidential and parliamentary systems in one data set while throwing together presidential and parliamentary executive elections could be considered problematic. Concentrating on the lower house has the practical advantage of more observations due to mixing both presidential and parliamentary systems, yet does not shift the attention away from those who play major roles in the governments' disaster response. Despite the HoG or appropriate minister rushing in and promising relief, the legislature usually has to approve the respective budget. This, for example, is the case in Germany. The incumbent Gerhard Schroeder promised relief for a flood in 2002, and the push back by the opposition in the Bundestag during the respective budget discussion is widely believed to have damaged the reputation of Schroeder's opponent Edmund Stoiber.

Further, this study focuses on political parties in charge of the lower house instead of individuals holding the respective portfolio which puts them in charge of response and relief efforts because blame can be expected to be attributed to the HoG at least as much as to the respective minister in charge. At first glance, it would seem reasonable to focus on the official

who is in charge of disaster preparedness and relief when determining any political impact of natural disasters. Some might argue that approval for the minister in charge, usually the minister of interior, would be best. This would presuppose, however, that the electorate is informed and able to correctly identify the official in charge and evaluate his/her crisis management. Studies on miss-attribution, some of which were referred to in the literature review above, suggest however, that voters are not sufficiently sophisticated to correctly identify their respective minister of interior, or others for that matter. Instead, blame or reward is usually attributed to the HoG. In the end, the distinction between minister in charge and head of the executive is only important in coalition governments anyways when the two portfolios can be occupied by differing parties. Otherwise, the members of the executive all belong to the same party.

Electoral performance of the incumbent refers to the vote share the incumbent political party received in the first round of a national lower house election. Whereas incumbent status is determined by considering seat share and the party affiliation of the HoG, electoral performance is operationalized as party vote share in the first round of national lower house elections.⁷ Why use party vote share in the first round instead of seat share after the final round? A party's vote share in the first round of an election is much closer to the electorate's true feeling about that party. On the one hand, seat shares can be distorted by seat allocation rules. Take for example the seat share of the USA's Democrat Party in 2012 and 2016: the

7. Party vote share was chosen over absolute number of votes to prevent problems of heteroscedasticity in the estimation process due to differing sizes of the voting population across countries and constituencies. Furthermore, I decided against following the example of studies using a dummy variable approach to measure incumbent removal. The loss of information is tremendous: an incumbent could be punished for bad luck or incompetence and be reelected at the same time. Unless the dependent variable is vote share, vote difference, or something similar, the model would not be able to pick up this variation.

seat share overstates the punishment of the Democrat Party when compared to the popular vote. Another example for distortion through seat allocation rules is Mexico where 200 out of 500 seats in the Chamber of Deputies are allocated via rules. On the other hand, higher election rounds are distorted because more voters engage in strategic voting, i.e. they vote for parties that they might not have voted for otherwise. Thus, election rounds other than the first might understate the punishment of the electorate.

After defining incumbent and electoral performance, the data for the dependent variable and its sources can be discussed. An incumbent is the political party in the national lower house which emerged as the main governing party holding the HoG portfolio in the last election and which is standing for reelection in this election. Electoral performance of the incumbent refers to the vote share the incumbent political party received in the first round of a national lower house election.

Incumbent Electoral Performance Data

Data for the dependent variable, defined above as the vote share of the incumbent party in the first round of national lower house elections, comes from the Constituency Level Electoral Archive (CLEA) (Kollman et al. 2017).⁸ Supplemental information was drawn

8. The Global Election Database (GED) was considered as an additional source of national election results to fill potential missing values, yet had to be discarded due to various coding errors. For example, in the GED data, there are three parties (Vanua'aku Party, Union of Moderate Parties, and Land and Justice Party) with 6 seats each winning the Vanuatu 2016 election. The GED data thus does not include the court ruling that the Land and Justice Party was awarded a seat that originally was counted towards the Melanesian Progressive Party. Further, the GED coding mixes results of lower and upper house elections in some instances. For example, in 1979 the Icelandic Independence Party is correctly attributed 14 seats for the lower house, while the Progressive Party is coded as winning 17 seats. In reality however, the Progressive Party got 11 seats in the lower house and 6 in the upper one which would sum to 17. In 1983, the Progressive Party is coded 14 seats in lower house what should have been 10 in the lower and 4 in the upper house, and the Independence party is coded 21 which should have been 15 in the lower and 8 in the upper house. Further complicating the matter is the fact that each election data base uses a non-standardized coding

from the Varieties of Democracy (VDEM) data set (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Bernhard, Fish, Glynn, Hicken, Knutsen, Krusell, et al. 2017; Pemstein et al. 2015) to calculate all necessary election result variables. Only at least somewhat free and fair elections were included in this study, i.e. elections for which VDEM’s variable *v2elfrfair_ord* > 1.⁹ The preparation of the data occurred in four steps.

First, the CLEA data had to be cleaned. I dropped all observations of Afghanistan, Belgium, Bosnia and Herzegovina, Italy, Kuwait, Marshall Islands, and Sri Lanka (before 1989) either because of data gaps or because the respective political context made tracking incumbents difficult. Furthermore, if there was another election in the same country-year, I only coded the last election in that year (c.f. Greece 1989, UK 1974, and Ireland 1982) and dropped all previous ones. The last election in a given country-year was always the one which produced a (somewhat) stable and lasting government. On the practical side, dropping these cases ensured that country-year and country-constituency-year were unique identifiers of cases in the national-level and constituency-level data sets.

The second step was to identify the number of consecutive elections and breaks in the democratic process. The calculation of any lagged variables, for example the incumbent party’s vote share in the last election, needs to account for gaps in the time series so not to calculate the lagged share based on values of election in $t-2$ instead of $t-1$. VDEM’s variable *v2ellocons* reports the number of consecutive elections which I updated for 44 countries.

scheme for political parties. Thus, using multiple election result data sets would have required the creation of a party name or code translation mechanism. With thousands of political parties in each data set, this task is outside of this study’s scope.

9. This corresponds to an expert’s opinion of at least “Ambiguous. There was substantial competition and freedom of participation but there were also significant irregularities. It is hard to determine whether the irregularities affected the outcome or not.” (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Andersson, et al. 2017, 103).

With this variable, the calculation of any measure relying on results of the last election, or changes within one electoral period becomes trivial.

Gaps in the time series may result from parties not participating in each election, when the data set does not report results for some elections, or when the respective country experiences a break in its democratic processes. For example, in the post WWII era, Suriname held regularly scheduled elections from 1949 to 1969 and from 1987 to 2015. It would make little sense to take the winner from 1969 and see what vote or seat share that party received in 1987. The corruption and military dictatorship between the two elections alter the context too much. Thus, these two time periods will have to be considered separately. The convention in studies of electoral volatility which deal with this issue a lot, especially in Africa (c.f. Kuenzi et al. (2017)), is to either treat the two time periods as separate countries (e.g., Suriname I and Suriname II) or to accept the break in the time-series. I opt for the second variant for practical reasons: It creates no inflation to the data set, the already high number of countries and low average number of observed elections per country is not exacerbated further, and it conserves the data clustering by country as it is.

The third step was to determine the winner of each national lower house election in order to derive the incumbent parties and their (lagged) vote shares. With thousands of observations in the data set, manually coding the winners was beyond the scope of this analysis. Instead, I have decided to follow the following coding rule to determine incumbents: in the first parse through the data, the party with the highest seat share in the lower house election is to be considered the incumbent. This rule should lead to no false incumbent coding in majoritarian electoral systems. In non-majoritarian electoral systems, i.e. proportional

or mixed systems, the largest party in terms of seats could end up not being in charge of the executive if the party fails to form a coalition and another group of parties steps up. In praxis, however, it is unusual for the largest party not to enter the ruling coalition. And if the negotiations fail, there often is a reelection, as in Greece in 2012. Therefore, the expected margin of error was small. If there were multiple winners, i.e. more than one party with the highest seat share, I determined the winner according to the party affiliation of the new (or confirmed) HoG. This is in line with the operationalization above.¹⁰

The coding algorithm in step three was cross-validated using VDEM's ordinal variable *v2eltvrig* which codes turnover in the lower chamber with no turnover coded as zero. After updating it for some 40 countries, comparing VDEM's turnover variable to my own revealed only a few errors of step three's algorithm, mostly caused by party identification problems due to changing alliances and names of political parties. In the end, I manually coded the election time-series of 58 countries tracking the main actors in coalitions and alliances over time.

Fourth, and finally, the CLEA data set had to yield one national-level data set with country-year as the unit of analysis and one observation per incumbent and election, and one constituency-level data set with a country-constituency-year unit of analysis and one row per national incumbent party, constituency, and election year. For the analysis of national

10. There were a few instances where such coding was necessary: Trinidad and Tobago in 1995 when UNC leader Basdeo Panday became Prime Minister, and in 2001 when PNM leader Patrick Manning became Prime Minister; the Netherlands in 1952 when William Drees of the Labour Party remained the HOG; Belgium 2003 when Guy Verhofstadt of the VLD remained the HOG; Switzerland in 1959 and 1979 when the Social Democratic Party remained to have the HOG; New Zealand in 2005 when Helen Clark of the Labour Party remained Prime Minister; Greenland in 2014 when Kim Kielsen of the Siumut party was elected Prime Minister; Curacao in 2012 when Daniel Hodge (PS) became Prime Minister; and Faroe Islands in 2011 when Kaj Leo Johannesen (Union) became Prime Minister. Additionally, I coded the party Res Publica the winner in Estonia in 2003 because it gathered enough support to form a coalition.

values, I collapsed the election results by country, election year, party, and votes won in the first round, dropping all uncontested or suspended districts. The resulting data set allows the calculation of vote share for each party before all non-incumbent observations can be dropped to create a data set with a country-year unit of analysis. To derive the constituency-level data set, I collapsed the CLEA data by country, constituency, election year, party, and votes won in the first round. By constituency I mean the first-order sub-national administrative region of each country. That would, for example, correspond to the Länder in Germany. However, the indicator of the constituencies in the CLEA data had a myriad of errors for each country. In anticipation of availability issues of the sub-national natural disaster data, I fixed CLEA’s electoral district indicator only for the countries for which I had sub-national natural disaster data.

Table 3.2 presents the descriptive statistics of the dependent variable, incumbent party vote share, for the national-level data set. The data set includes 793 observations, covering 793 national lower house elections in 111 countries. The panel is highly unbalanced with elections as early as 1962 and as late as 2015, while the minimum count of observed elections per country with available lagged vote share is 1 and the maximum is 27. Figure 3.1a presents a histogram of the observed election count per country in the national-level data in order to illustrate the highly unbalanced panel.

Table 3.2: Descriptive Statistics of the National-Level DV

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
incumbent vote share	793	39.278	15.597	0.292	29.387	49.110	94.425
incumbent vote share, lag	793	43.588	13.784	11.000	34.593	51.566	97.064

Table 3.3 presents the descriptive statistics of the dependent variable, incumbent party vote share, for the subnational-level data set. The data set includes 1176 observations, covering 74 national lower house elections in 333 first-order sub-national administrative regions of 17 countries. This panel is also highly unbalanced with elections as early as 1980 and as late as 2014, while the minimum count of observed elections per country with available lagged vote share is 1 and the maximum is 12. Figure 3.1*b* presents a histogram of the observed election count per country in the sub-national-level data in order to illustrate the highly unbalanced panel.

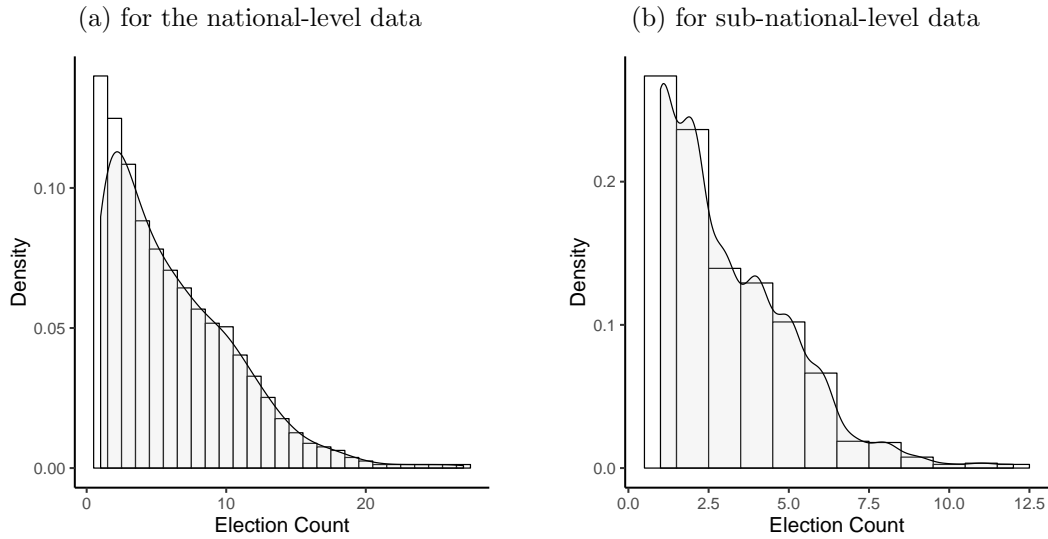
Table 3.3: Descriptive Statistics of the Sub-National-Level DV

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
incumbent vote share	1,176	35.866	16.475	0.274	24.508	46.758	98.905
incumbent vote share, lag	1,176	41.460	15.882	0.000	31.008	51.905	98.905

Natural Disaster Data

The number of natural disasters per legislative period and per cross-sectional unit is the main explanatory variable of this study. Previous work suggests that different families of disasters elicit different reactions from the electorate. Thus, I will test individual families of disasters (meteorological, hydrological, geophysical, and climatological) as well as the difference between slow and rapid onset disasters. Finally, as discussed in the literature review, the economic voting literature suggests that the electorate heavily weighs economic development in the 6 to 12 months before the election. Similarly, it could be that natural

Figure 3.1: Histograms of Election Counts per Country



disasters only have an effect on elections when they occur within a year of the election. To test this, I will introduce two counts per natural disaster family: one of natural disasters within the year before the election, and one count for all natural disasters since the last election.

As noted in the literature review, events such as earthquakes, flooding, storms, or droughts are natural processes which do not have to lead to disasters unless they occupy the same time and space elements as humans. Only when humans clash with natural processes and the impact of the natural process overwhelms local authorities and their coping capacities, we speak of natural disasters (Wisner et al. 2014). In other words, in order to become a disaster, the natural process has to meet enough humans who are in a vulnerable space, are unprepared, and do not have the required coping capacity.

When specifically is a local authority overwhelmed and how can it be measured? The

Center for Research on the Epidemiology of Disasters (CRED) maintains the Emergency Events Database (EM-DAT) (Guha-Sapir, Below, and Hoyois 2017)) which is commonly used for empirical studies covering multiple countries over time. Their working definition of a disaster requires the event to meet at least one of the following criteria: (a) kill 10 or more humans, (b) affect 100 or more humans, (c) lead to the declaration of a state of emergency, and (d) lead to a request for international help. This is a very inclusive definition because it is easy for a natural process to affect 100 people. An alternative data source is the DesInventar Project which is committed to collecting disaggregated, geo-referenced disaster data. Furthermore, the project also wants to include more small and medium sized events. Their DesInventar Project defines a disaster “as the set of adverse effects caused by social-natural and natural phenomena on human life, properties and infrastructure (an ‘Event’) within a specific geographic unit during a given period of time” (DesInventar 2018). This definition is very vague but allows the project to reach both of its goals. The project’s Disaster Inventory System does include more disasters per country than EM-DAT but covers fewer countries. For each disaster, the project also collects a variety of loss indicators. The big advantage of DesInventar is that the disasters are geo-referenced to the country, province/state, and even municipality level. Thus, researchers can choose from three levels of aggregation when designing studies.

EM-DAT and DesInventar both classify natural disasters in the same way. There are six families of natural disasters: geophysical, hydrological, meteorological, climatological, biological, and extraterrestrial. Each family has a variety of main events. For example, meteorological events are convective storms, extratropical storms, extreme temperatures,

fog, and tropical cyclones.

Following previous studies and the available data sources, I define a natural disaster as either killing 10 or more people, or affecting more than 100 people. The operational definition of disaster employed in this study has to meet two goals. First, it needs to generate a data set which is comparable to previous studies in this field. Second, the definition needs to exclude events which likely did not overwhelm local authorities. If the second condition is not given, then the first is not given either. Also, if the second condition is not given, this study will not fit into the broader literature on natural disasters. Moreover, including even the smallest events could lead to a data set in which natural disasters happen in every period. In this case, the statistical analysis would have no control group. DesInventar does not include the declaration of states of emergency or the call for international assistance. Hence, these two latter parts of the popular definition cited above need to be excluded in this particular operationalization.

The following statistical analyses will draw on EM-DAT for the national-level analysis and on DesInventar for the sub-national-level analysis. EM-DAT covers more countries and is thus the best choice for a comprehensive cross-country cross-time analysis. Since EM-DAT does not include geo-referencing, I have to use DesInventar as the source for natural disaster data in the disaggregated statistical analyses. Lagged count variables for natural disaster occurrences for the groups of geophysical, hydrological, meteorological, climatological, and slow as well as rapid onset natural disasters were derived from own calculations for each data source.

Table 3.4 presents the descriptive statistics of the main explanatory variable, the occur-

rence of natural disasters, for the national-level data set. The disaster count variables are lagged and grouped into six categories (slow and rapid onset disasters, climatological, geophysical, hydrological, and meteorological). As motivated by the literature review, the lags are available both as disaster occurrences in the last year (denoted as $t - 1$) and within the last electoral period (denoted as $EP - 1$). The data set includes 670 observations for the one-year lags and 704 observations for the lags by electoral period. The difference in observations for the two lag variants is due to incomplete disaster time series for some electoral periods.

Table 3.4: Descriptive Statistics of the National-Level Disaster Variables

Statistic	N	Mean	St. Dev.	Min	Median	Max
no. slow onset disasters, t-1	670	0.331	0.690	0	0	5
no. rapid onset disasters, t-1	727	1.981	4.080	0	1	34
no. Climatological disasters, t-1	670	0.184	0.577	0	0	5
no. Geophysical disasters, t-1	682	0.183	0.555	0	0	4
no. Hydrological disasters, t-1	690	0.848	1.737	0	0	14
no. Meteorological disasters, t-1	705	1.068	2.605	0	0	23
no. slow onset disasters, EP-1	704	1.010	2.221	0	0	34
no. rapid onset disasters, EP-1	704	6.155	13.670	0	2	243
no. Climatological disasters, EP-1	704	0.523	1.229	0	0	13
no. Geophysical disasters, EP-1	704	0.645	2.177	0	0	28
no. Hydrological disasters, EP-1	704	2.540	5.852	0	1	75
no. Meteorological disasters, EP-1	704	3.048	7.738	0	1	143

Table 3.4 includes the median statistic for each variable in order to provide a first glimpse at the excessive zero counts in all natural disaster occurrence variables. Closer examination reveals that 44.6% of observations of the one-year lag of rapid onset natural disaster occurrences are coded zero. Similarly, 27.1% of observations for the count variable of rapid onset natural disasters in the previous electoral period are zero. Respectively, this is the

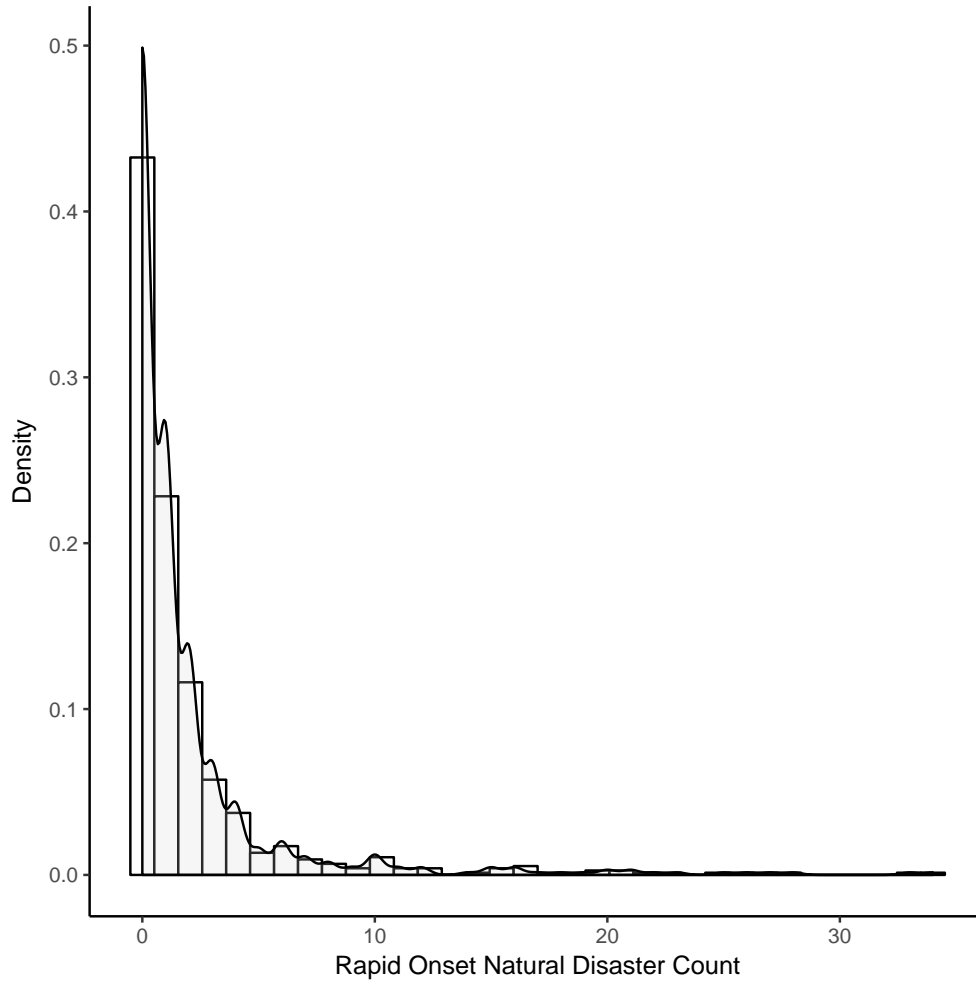
percentage of observations in the data set which have not experienced the treatment effect of rapid onset natural disasters. Figures 3.2 and 3.3 present histograms for the two variants of the rapid onset natural disaster count variables, graphically illustrating the zero-inflation and skewness of the respective variables. Note, the outlier in the rapid onset natural disaster count in the previous electoral period with 243 counts is the Philippines in 2013. The second highest count is 81 for India in 2009. As illustrated by the summary statistics in table 3.4, the other natural disaster count variables are similarly zero-inflated and skewed to the left.

Table 3.5 presents the descriptive statistics of the main explanatory variable, the occurrence of natural disasters per first-order sub-national administrative region, for the constituency-level data set. The disaster count variables are lagged and grouped into six categories (slow and rapid onset disasters, climatological, geophysical, hydrological, and meteorological). As motivated by the literature review, the lags are available both as disaster occurrences in the last year (denoted as $t - 1$) and within the last electoral period (denoted as $EP - 1$). The data set includes 1176 observations for the one-year lags and 1176 observations for the lags by electoral period.

Table 3.5: Descriptive Statistics of the Sub-National-Level Disaster Variables

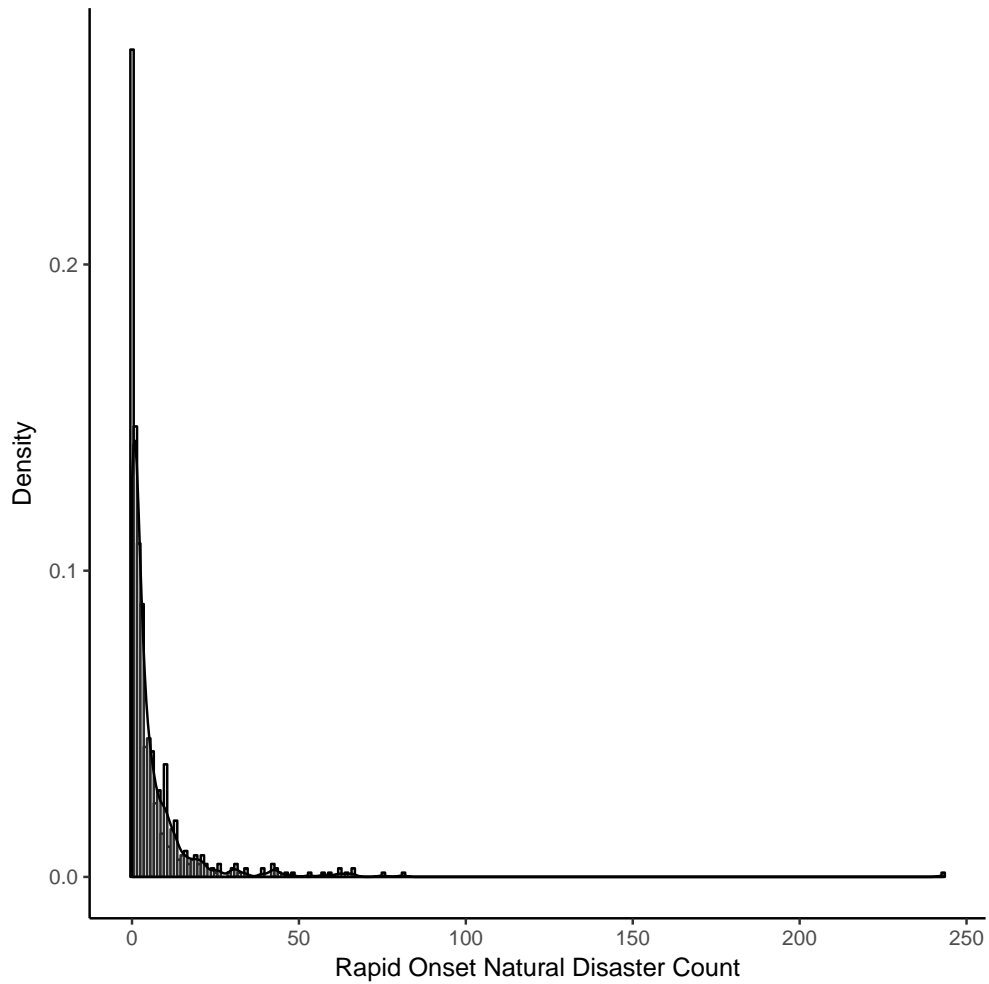
Statistic	N	Mean	St. Dev.	Min	Median	Max
no. slow onset disasters, t-1	1,176	0.052	0.280	0	0	3
no. rapid onset disasters, t-1	1,176	0.300	0.992	0	0	14
no. Climatological disasters, t-1	1,176	0.042	0.262	0	0	3
no. Geophysical disasters, t-1	1,176	0.003	0.058	0	0	1
no. Hydrological disasters, t-1	1,176	0.219	0.864	0	0	13
no. Meteorological disasters, t-1	1,176	0.083	0.371	0	0	4
no. slow onset disasters, EP-1	1,176	0.270	1.413	0	0	21
no. rapid onset disasters, EP-1	1,176	1.009	2.530	0	0	25
no. Climatological disasters, EP-1	1,176	0.253	1.397	0	0	21
no. Geophysical disasters, EP-1	1,176	0.081	0.628	0	0	12
no. Hydrological disasters, EP-1	1,176	0.632	2.161	0	0	25
no. Meteorological disasters, EP-1	1,176	0.303	0.959	0	0	11

Figure 3.2: Histogram of Rapid Onset Natural Disaster Counts in $t - 1$ per Country



Just as above, table 3.5 includes the median statistic for each variable in order to provide a first glimpse at the excessive zero counts in all natural disaster occurrence variables. Closer examination reveals that 85.4% of observations of the one-year lag of rapid onset natural disaster occurrences are coded zero. Similarly, 68.7% of observations for the count variable of rapid onset natural disasters in the previous electoral period are zero. Respectively, this is the percentage of observations in the constituency-level data set which have not experienced

Figure 3.3: Histogram of Rapid Onset Natural Disaster Counts in $EP - 1$ per Country



the treatment effect of rapid onset natural disasters. These percentages should be higher in the constituency-level data set than in the national-level one because the unit of analysis is disaggregated and any natural disaster may affect only one of the country's constituencies. Figures 3.4 and 3.5 present histograms for the two variants of the rapid onset natural disaster count variables, graphically illustrating the zero-inflation and skewness of the respective variables. As illustrated by the summary statistics in table 3.5, the other natural disaster

count variables are similarly zero-inflated and skewed to the left.

Figure 3.4: Histogram of Rapid Onset Natural Disaster Counts in $t - 1$ per Sub-National Unit

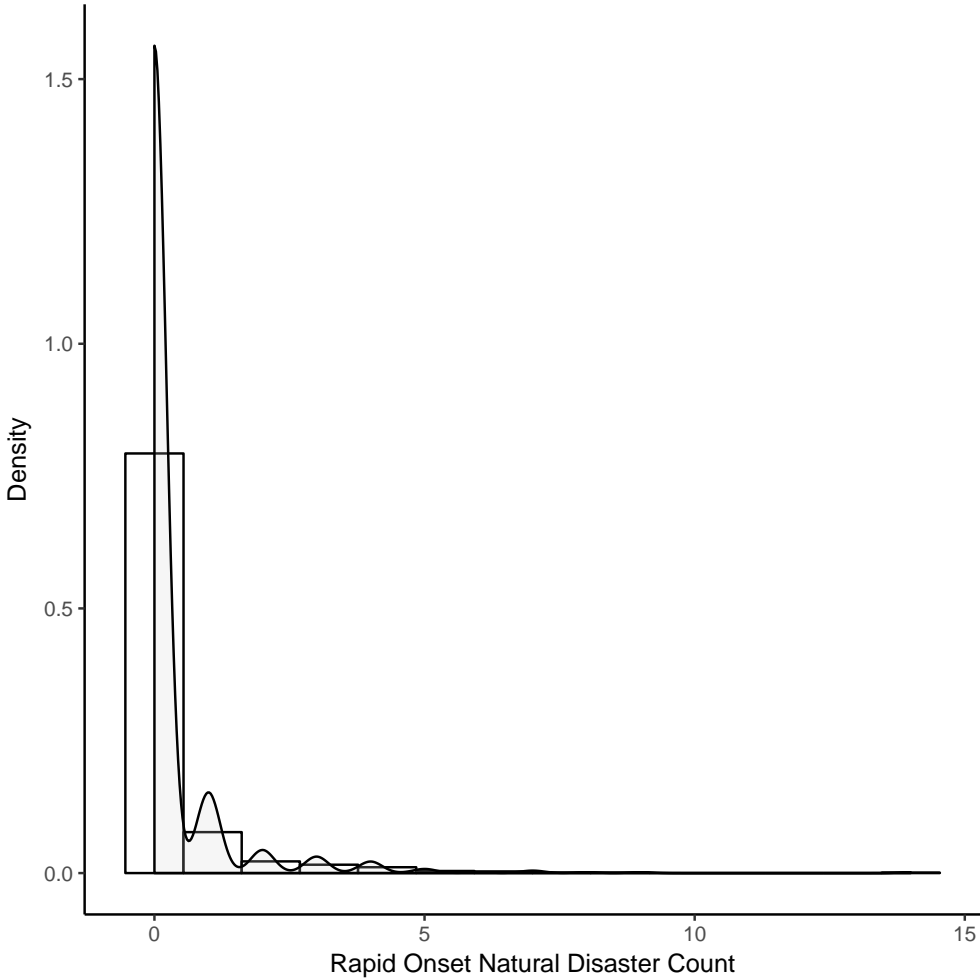
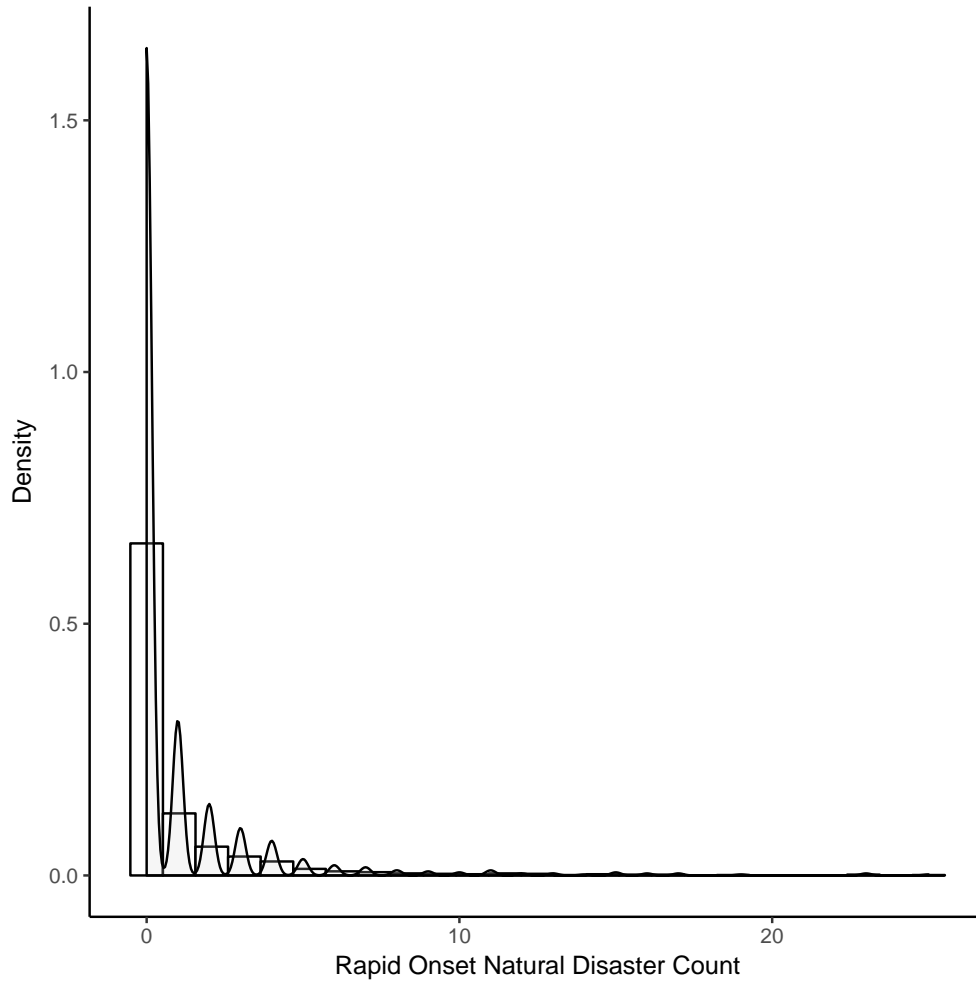


Figure 3.5: Histogram of Rapid Onset Natural Disaster Counts in $EP - 1$ per Sub-National Unit



Natural Disaster Risk Data

An index controlling for a population's inherent exposure to natural processes needs to be included in the analyses because an electorate used to natural processes and disasters may react differently to such events than one without extensive prior experience. It is conceivable that electorates' reward or punishing behavior for natural disasters might differ according to

their prior experience with natural disasters. There are two possible expectations for electorates with frequent exposure. On the one hand, experienced societies could punish harder for natural disasters because they expect their leaders to invest into successful preventative measures. On the other hand, electorates that know about their heightened exposure could punish less because natural hazards are part of the daily life. On the contrary, in societies with less natural hazard exposure it could be easier for the incumbent to be the knight in shining armor and thus, to be rewarded for natural disasters.

In order to qualify the effect of natural disasters on incumbent electoral performance based on natural hazard exposure, a measure has to be introduced that can effectively split the sample into societies with much experience and exposure and those with few exposure. However, finding a measure which provides prior knowledge about a unit's inherent exposure to natural hazards, and thus, a proxy of how much the population is used to being the target of natural hazards, is delicate. One has to consider both the size and proximity of natural hazards to a population while taking into account the vulnerability of that population. This interaction is bound to produce endogeneity via bidirectional causation: a natural hazard can move towards a populated area, or a populated area can move into hazardous regions further increasing its exposure.

The natural risk dimension of the Index for Risk Management (INFORM) is included in this analysis to control for electorates' experience with natural disasters at the national level. The INFORM of the Inter-Agency Standing Committee Reference Group on Risk, Early Warning and Preparedness and the European Commission understands natural disaster risk as the product of three dimensions (from INFORM 2017, 21): hazard & exposure,

vulnerability, and lack of coping capacity. The hazard & exposure dimension is divided into the categories ‘natural’ and ‘human’. The natural risk index is, like all INFORM components, a 10 point scale with zero representing minimal exposure risk. The indicators used to calculate the natural risk index are exposure to natural hazards (earthquakes, floods, tsunamis, cyclones, droughts) measured in “average annual population exposed per country” (INFORM 2017, 25). (For droughts, INFORM also includes the “average annual events per country”.) All other components of the individual dimensions are human in nature: conflict, displaced persons, socio-economic, and institutional variables. The inclusion of these factors certainly is reasonable for the purpose of forming the INFORM. However, the inclusion of the full INFORM in statistical applications might introduce problematic endogeneity if any of its factors, for example population, economic development, and conflict measures, are included as separate controls in the model specification. For the purpose of this analysis, I include only the hazard category of the hazard and exposure dimension. Separate socio-economic controls are discussed below.

This study treats INFORM’s natural risk index as constant over time to greatly extend its coverage in the time-series at hand. Disentangling the natural hazard component from the rest of the INFORM has an additional advantage. A country’s exposure to natural hazards is a very sticky attribute: shorelines, fault zones, and average annual temperatures do not change much over time. This allows to carry back the exposure risk value of a country back in time and fill missing values. Especially since INFORM otherwise would cover only the years 2013 to 2017, this procedure generates a lot of usable observations.

The average within country standard deviation of the INFORM’s hazard risk in its

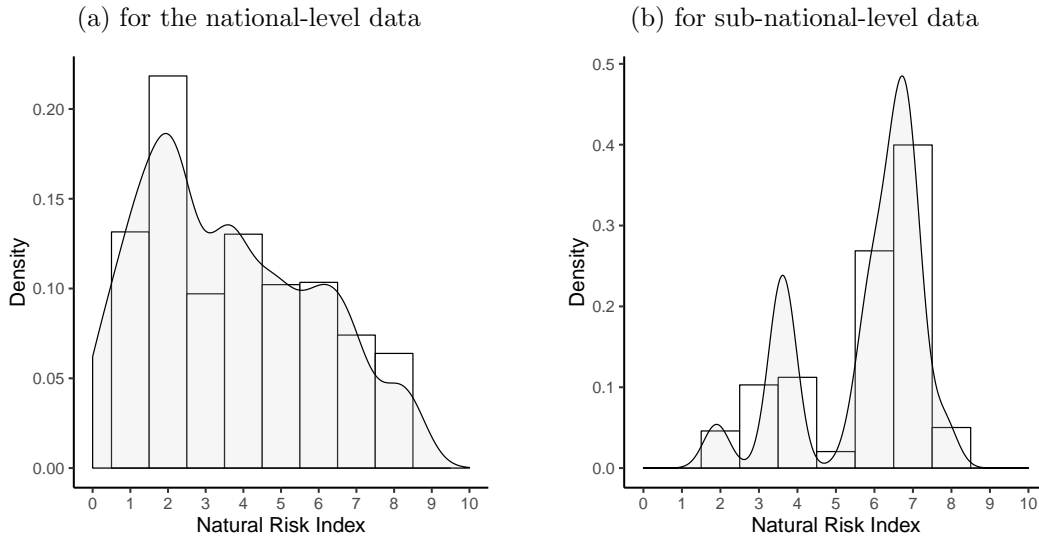
unedited data version is 0.043, whereas the average between standard deviation is 1.905 while the natural risk variable ranges from 0.1 to 8.4. Due to the negligible within but significant between variation over the available 5 year span, it makes practical sense to use the oldest observation (from 2013) and use it as a constant across time. Again, this tactic makes sense theoretically, too: the exposure to natural hazards is conditional on very sticky attributes such as location of the cross-sectional unit, distance to a fault zone or ocean, etc. These attributes change slowly enough to treat the variable as a constant. Therefore, I use INFORM's natural hazard risk index to proxy a country's inherent exposure to and experience with natural hazards.

Tables 3.6 and 3.7 include the summary statistics for the natural hazard risk variable for the national-level and constituency-level data sets. The minimum value for risk is greater in the sub-national-level data set due to its regional concentration on Latin America which is more exposed to natural processes. Figure 3.6 presents histograms of the INFORM variable in the national-level and sub-national-level data sets.

Patronage, Clientelism, and Corruption

The theory section motivated the inclusion of a measure of clientelism, or patronage. The theory expects the government to invest less in prevention than in relief on the account that prevention policies (e.g. building codes) could be seen as obstacles or at least nuisances by the electorate, whereas relief efforts may result in the acting government to be perceived as the knight in shining armor. Thus, assuming politicians' main goal is to be reelected, such dynamics lead to an incentive system where the government prefers relief over prevention or

Figure 3.6: Histograms of the INFORM Variable



mitigation. Furthermore, this dynamic could be strengthened or dampened depending on the electorate’s reliance on patronage. It is conceivable that an electorate which is accustomed to (and thus develops an expectation of) patronage from its political representatives will reward the incumbent government in elections after natural disasters if the government has sent help. On the other hand, a non-clientelistic society likely relies on a highly institutionalized system with policies and procedures which regulate relief efforts. In such a system it is conceivable that the electorate punishes the incumbent government despite relief efforts in order to complain that not more had been done to prevent the disaster in the first place.

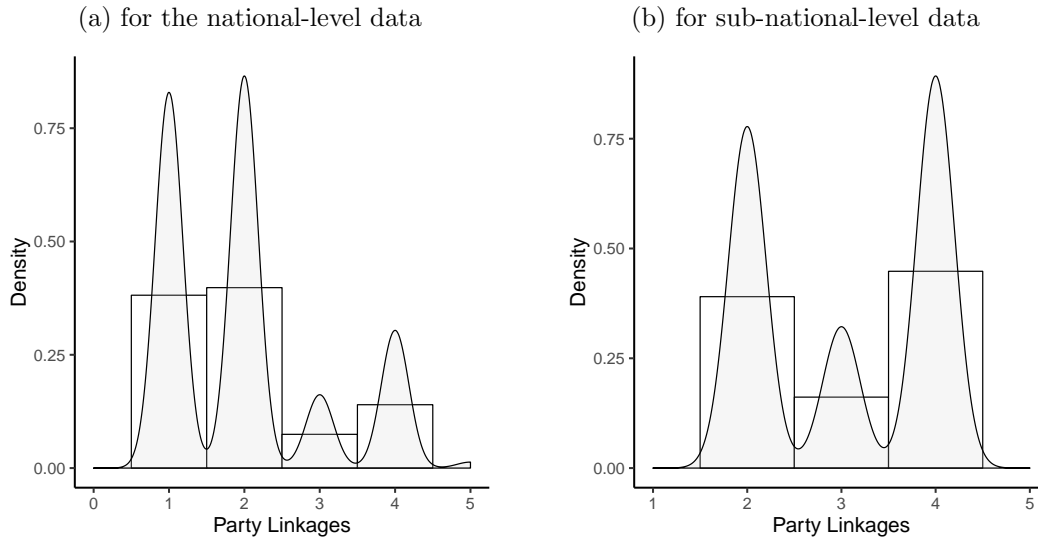
The analyses below will condition the effect of natural disasters on incumbent electoral performance on the degree of corruption present at the time of election. The Varieties of Democracy (VDEM) project (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Bernhard, Fish, Glynn, Hicken, Knutsen, Krusell, et al. 2017; Pemstein et al. 2015) has

two variables which get at how much clientelism is present at the country-year level. The variable on party linkages is derived from country-expert surveys and asks “Among the major parties, what is the main or most common form of linkage to their constituents?”, where “a party-constituent linkage refers to the sort of ‘good’ that the party offers in exchange for political support and participation in party activities.” (Coppedge et al. 2016, 126). In the original coding, possible answers range on a 5 point scale from “clientelistic” (0) to “Policy/programmatic” (4) (126). Further information about the methodology can be derived from the cited sources. Here it shall suffice to say that the analyses below will be using the variable *v2psprlnks* from the VDEM data set, as this is the preferred measure to make comparisons across time (32).

The second variable dealing with the issue of patronage asks how targeted, or ‘particularistic’, spending in the national budget is. This variable (*v2dlencmps*) also relies on country-experts, asking “Considering the profile of social and infrastructural spending in the national budget, how ‘particularistic’ or ‘public goods’ are most expenditures?” where “particularistic spending is narrowly targeted on a specific corporation, sector, social group, region, party, or set of constituents. Such spending may be referred to as ‘pork’, ‘clientelistic’, or ‘private goods’.” (195). The 5 point scale ranges from “Almost all of the social and infrastructure expenditures are particularistic” (0) to “Almost all social and infrastructure expenditures are public-goods in character. Only a small portion is particularistic.” (4) (195).

The linkage variable, *v2psprlnks*, was chosen over the spending variable, *v2dlencmps*, for the analyses below because the link between voter and elected representative is the closer

Figure 3.7: Histograms of the Linkage Variable



proxy for electorates' expectation of patronage, especially with incumbent party vote share as the dependent variable. The two measures $v2psprlnks$ and $v2dlencmps$ are correlated at around 0.5 in the VDEM data set. Hence, the inclusion of both variables in the same model might be problematic. Tables 3.6 and 3.7 show the summary statistics for the linkage variable for the national-level and constituency-level data sets. Figure 3.7 presents histograms of the linkage variable in the national-level and constituency-level data sets.

Economic Controls

The literature review suggests controlling for economic variables such as economic growth, foreign aid, and either inflation or unemployment. Data for the gross domestic product (GDP) was obtained from the Penn World Tables (PWT; version 9.0 with the latest update of August 18, 2016) (Feenstra, Inklaar, and Timmer 2015a). This study uses the PWT's real

GDP from the expenditure side (*rGDPe*) as it is recommended as a measure to compare living standards across countries and time as compared to the output side which is a better indicator of the respective economies' productivity (Feenstra, Inklaar, and Timmer 2015b, 6). The *rGDPe* is reported in million 2011US dollars at chained purchasing power parities (PPPs). This purges the GDP measure from inflation and exchange rate effects, making it comparable across time and countries. In order to account for different countries having different population sizes and thus, different sizes of GDP, comparability across countries is further increased by using the PWT's population variable to calculate *rGDPe* per capita.

The World Bank's World Development Indicators (version Oct. 14, 2016) has national estimates of both inflation and unemployment data. The consensus in the economic voting literature is that these measures are correlated too much to include both of them simultaneously in a statistical model. Needing to choose between the two and having some foresight on data availability in the sub-national empirical chapters, it seems reasonable to focus on unemployment rates: The constituency-level analyses draw most observations from Latin America. Given the Latin American debt crises and the exorbitantly high inflation rates during those time periods, unemployment rates may be easier to deal with practically, and they might be less driven by international factors than inflation rates. Thus, unemployment rates could be a better proxy of how the electorate perceives the domestic economy.¹¹

Theory also suggests potentially different impacts of international humanitarian aid on electoral performance of incumbents based on considerations of differing reward-punishment behavior by electorates based on their expectation of patronage. Thus, total received hu-

11. In the end, the usage of unemployment or log transformed inflation rates did not yield a substantive difference in the analysis.

Table 3.6: Descriptive Statistics of the Control Variables in the National-Level Data

Statistic	N	Mean	St. Dev.	Min	Max
Natural hazard risk index	783	3.615	2.282	0.100	8.400
Party Linkages	658	1.991	1.035	1	5
GDP pc change (lag)	737	3.543	5.656	-31.017	36.702
GDP pc (lag)	739	9.469	0.906	6.510	11.418
unemployment rate (pct., lag)	431	9.453	5.948	0.200	37.600
Hum. aid (lag)	793	0.385	1.748	0.000	28.581
World Econ. Crisis (lag)	793	0.284	0.451	0	1

humanitarian aid is included as a control in the models. Foreign aid data was downloaded from the Organization for Economic Cooperation and Development's (OECD) Creditor Reporting System (OECD 2017) as gross disbursements in US dollars and converted to million real 2011US dollars per capita using the PWT's GDP price level time-series for the USA and PWT's population data. Thus, the GDP and aid measures are using the same scale. The OECD's total gross humanitarian aid disbursements include all of OECD's Official Development Assistance (ODA) loans, ODA grants, and other official flows (non export credit) in the humanitarian aid grouping: emergency response, reconstruction relief and rehabilitation, and disaster prevention and preparedness (sector codes 720, 730, and 740 respectively).

A control is also added for world wide economic recessions. The economic crisis dummy is coded 1 for the years 2007-09, 1999-00, 1991-92, 1987, 1979-83, 1973-74 and zero otherwise.

Tables 3.6 and 3.7 present the descriptive statistics for the national and world-aggregate controls in the national-level and constituency-level data sets respectively. The descriptive statistics vary between the national-level and the constituency-level data sets because of the differing numbers of observations. As national-aggregate controls, the variables are constant across constituencies for given country-years.

Preliminary Analyses

This section presents bivariate and other exploratory analyses of the data. The goal of the preliminary analyses is to explore bivariate relations and conditioning effects of hypothesized interactive variables. The preliminary analysis uses bivariate Pearson correlations to explore bivariate relations, Welch t-tests to explore difference in means between observations which experienced the treatment effect (natural disasters) and those who did not (control group), and graphs as well as incremental (type II) F-tests to assess model fit with and without the proposed interaction terms.

The preliminary analysis finds (a) some support for influence of natural disasters on incumbent electoral performance, (b) otherwise unexpectedly small correlations between the individual explaining variables and the dependent variable, (c) some support for a conditioning effect of patronage expectations, and (d) little support for electorates' previous experience with natural disasters conditioning the effect of new natural disasters on incumbent electoral performance.

Table 3.7: Descriptive Statistics of the Control Variables in the Sub-National-Level Data

Statistic	N	Mean	St. Dev.	Min	Max
Natural hazard risk index	1,176	5.729	1.548	1.900	7.800
Party Linkages	1,176	3.058	0.914	2	4
GDP pc change (lag)	1,176	4.647	6.804	-18.060	26.740
GDP pc (lag)	1,176	8.770	0.622	6.898	9.954
unemployment rate (pct., lag)	1,045	7.566	4.498	0.200	24.002
Hum. aid (lag)	1,176	0.943	1.853	0.000	10.105
World Econ. Crisis (lag)	1,176	0.338	0.473	0	1

Bivariate Correlations

The correlation matrices in tables 3.8 and 3.9 indicate only small correlations of the explaining variables with incumbent electoral performance, here operationalized as *change* in incumbent party vote share as compared to the last election, in both the national-level and the constituency-level data sets. The occurrence of rapid onset natural disasters in the previous period is correlated with the dependent variable at -0.023 and -0.007 in the national and sub-national data sets. While small in absolute terms, the signs are in the expected direction: the occurrence of natural disasters is expected to yield decreased incumbent party vote share. When correlated with incumbent party vote share, the other natural disaster occurrence counts do not achieve higher absolute correlations than $|0.135|$ in the one-year lags and $|0.186|$ for the electoral period lags (the maximum values are achieved by the hydrological natural disaster counts). When correlated with incumbent party vote share change, the highest absolute correlations are $|0.046|$ in the one-year lags and $|0.081|$ for the electoral period lags (the maximum values are achieved by the geophysical natural disaster counts). These are seemingly negligible correlations, yet with the expected signs. Further exploration of the hypothesized relationship between incumbent party vote share and the occurrence of natural disasters is required.

Humanitarian aid disbursements and world economic crises correlate with incumbent party vote share change at -0.041 and -0.028 respectively in the national-level data and at -0.023 and -0.213 in the constituency-level data. Again, the bivariate correlations are small in absolute terms, yet in the generally expected direction. The correlation for humanitarian aid disbursements with incumbent party vote share change changes to -0.125 and 0.037

respectively in the national and constituency-level data sets when the samples include only clientalistic societies (i.e. observations for values bigger than 2 of the party linkage variable as coded in this study). The sign change for the constituency-level correlation to positive is expected due to theory: clientalistic societies are expected to reward incumbents for securing international aid. In non-clientalistic societies, the correlation of humanitarian aid disbursements with incumbent party vote share is 0.014 and -0.119 respectively in the national and constituency-level data sets. The sign change to positive of the former value is unexpected.

The control variables (level as well as change of GDP, and unemployment) also show little correlation with the dependent variable, this is with either incumbent party vote share and incumbent party vote share change. Further, the directional signs point in the opposite direction than hypothesized in the economic voting literature. This is very unexpected given the strong support of the economic voting literature as reviewed above. Note however, that the support of that literature is based on multiple regression (as opposed to this bivariate correlation measure) and has shown high dependence on model as well as sample selection. The controls will be included in the analyses below and their performance will be monitored.

Table 3.8: Pairwise Pearson Correlation Matrix for the National-Level Data

	1	2	3	4	5	6	7
incumbent vote share change	1						
no. rapid disasters,	-0.023	1					
GDP pc change	-0.055	-0.049	1				
GDP pc	-0.109	0.039	-0.12	1			
unemployment	0.008	-0.165	-0.077	-0.261	1		
Hum. aid	-0.041	-0.024	0.044	-0.165	0.063	1	
World Econ. Crisis	-0.028	0.028	-0.081	0	-0.027	0.057	1

The columns follow the same ordering than the row names; column names were removed to fit table.

Table 3.9: Pairwise Pearson Correlation Matrix for the Constituency-Level Data

	1	2	3	4	5	6	7
incumbent vote share change	1						
no. rapid disasters,	-0.007	1					
GDP pc change	-0.106	0.027	1				
GDP pc	-0.086	0.088	0.024	1			
unemployment	0.147	-0.055	0.068	-0.175	1		
Hum. aid	-0.023	-0.022	0.181	-0.218	-0.061	1	
World Econ. Crisis	-0.213	0.148	0.001	0.055	-0.056	-0.115	1

The columns follow the same ordering than the row names; column names were removed to fit table.

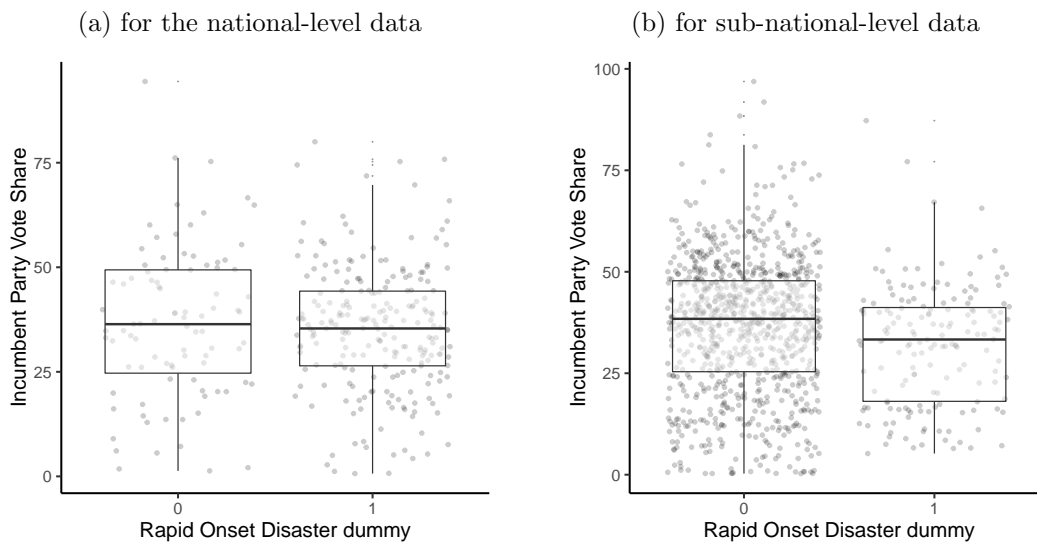
Rapid Onset Disasters and Incumbent Party Vote Share

This section further explores the bivariate relation between the natural disaster counts and incumbent electoral performance. I find evidence which supports the hypothesis of a relationship despite the seemingly negligible correlation of the two variables in tables 3.8 and 3.9. First, I present box plots and second, I calculate Welch tests. Note that both approaches expect the sample to be split into a treatment and a control group by a factor variable with only two outcomes, treatment or no treatment. Thus, the natural disaster occurrence count variables in this section are recoded as dummy variables coded 1 if there was at least one such event in the specified time frame and zero otherwise.

The following two graphs in figure 3.8 are boxplots of incumbent party vote share conditioned on the occurrence of rapid onset natural disasters. The point clouds have been jittered for better visibility. Again, we see that a majority of the observations did not experience the treatment effect. Both graphs seem to indicate that mean incumbent party vote share is lower in elections following in the year after a rapid onset natural disaster. Formal tests will have to help shine light on the hypothesized negative relationship between incumbent party vote share and rapid onset natural disasters.

T-tests in the national-level data show that the observed difference in means between

Figure 3.8: Boxplots of Incumbent Party Vote Share Conditioned on the Occurrence of Rapid Onset Natural Disasters



control and treatment groups in figure 3.8 may be systematic. Welch’s unequal variances t-test posits the alternative hypothesis that the true difference in means is not equal to 0. The test results (not shown here) suggest that we can reject the null hypothesis of equal means in groups with and without rapid onset natural disasters. Incumbent party vote shares are on average about 4.01 percent points lower in observations that experienced at least one rapid onset natural disaster in the year before the election. This difference is statistically significant at the $\alpha = 0.01$ level with a p-value of the test statistic of 0.00054.

T-tests on split samples of the national-level data do support the hypothesized conditioning effect of the natural risk index, yet do not support the hypothesized conditioning effect of the party linkage variable. In experienced societies (natural risk index > 4)¹², incumbents

12. The cutoff to split the sample is arbitrary. The substantive results do not change for cutoffs at 3.5, 4.5, or 5.

receive on average about 1.34 percentage points less in elections following a year with at least 1 rapid onset natural disaster. In non-experienced societies, incumbents receive on average about 7.31 percentage points less in elections following a year with at least 1 rapid onset natural disaster. Only the latter difference is statistically significant (p-value $\ll 0.01$). In clientalistic societies (i.e. when the value of the party linkage variable is > 2), incumbents receive on average about 11.78 percentage points less in elections following a year with at least 1 rapid onset natural disaster. In non-clientalistic societies, incumbents receive on average about 0.55 percentage points less in elections following a year with at least 1 rapid onset natural disaster. Only the former difference is statistically significant (p-value $\ll 0.01$).

Conditioning Effects of Natural Disaster Risk and Party Linkages

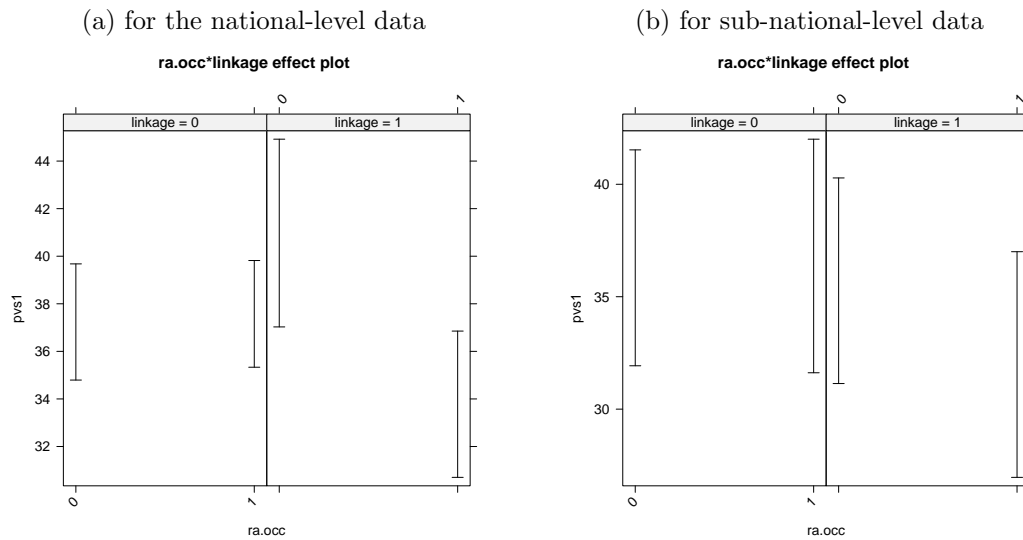
This section explores the conditioning effect of the natural disaster risk and party linkage variable a little further and finds that support for the conditioning effect of patronage expectations but not for experience with natural processes. The t-tests above yielded mixed results: either the difference between groups was not statistically significant (or substantive), or the size of differences was defying theoretic expectations. Note however, that the t-tests and correlations above do not take into account the nested structure of the data. Pooling the observations may have biased the results. This section relies on ANOVA's incremental (type II) F-test after minimally specified hierarchical regression models to assess whether model fit is improved by including the hypothesized interaction terms. The incremental (type II) F-test tests the null hypothesis that all of the interaction regressor coefficients are zero. Since at this point we are only interested in the F-tests for the respective interaction

terms the models are minimally specified and not reported here. Minimally specified refers to the hierarchical models regressing incumbent party vote share only on its lag, change in real GDP per capita in the last year, a counter of elections observed for the respective country, country and constituency (where applicable) fixed effects, and the respective interaction term including its base effects.

ANOVA's incremental (type II) F-tests suggest that the models generally fit better with the party linkage interaction than without. The F-tests suggest that the models generally fit better when the interaction of the rapid onset natural disaster event dummy with the party linkage dummy is included. The respective interaction terms are significant with p-values below the $\alpha = 0.01$ and $\alpha = 0.5$ levels in the national and constituency-level data sets respectively. Thus, we can reject the null hypothesis that all regression coefficients for the different levels of the tested linkage factor variable are zero. Substantively, this result is robust to changing the operationalization of the variables from a rapid onset natural disaster event dummy to a count and of the party linkage variable from a dummy to a categorical variable with 5 possible levels. Any combination of these measures yields F-tests which suggest the inclusion of the linkage interaction term. From the other natural disaster variables only the geophysical natural disaster count and event dummy yield statistically significant F-tests for the linkage interaction in the national-level data set. In the constituency-level data, the event dummies for slow onset, climatological, and hydrological natural disasters yield significant F-tests.

Figure 3.9 complements the F-tests above graphically. The graph explores the effect of at least one rapid onset natural disaster in the year before a national lower house election

Figure 3.9: Effect Plot of the Interaction of the Rapid Onset Natural Disaster Event Dummy with the Party Linkage Dummy



on incumbent party vote share in that election, conditioned on whether or not the society is generally clientalistic.

ANOVA's incremental (type II) F-tests suggest that the models generally do not fit better with the natural risk index interaction than without. Only the slow onset natural disaster event dummy yields a statistically significant F-test when interacted with the natural risk index. There is also no evidence of improved model fit when the natural risk index variable is transformed from an interval variable with a possible range from 0 to 10 to a dummy variable coded 1 when the respective society ranks high on the risk index and zero otherwise. Tested cutoff values for high ranking on the risk index were 4, 5, and 6.

Conclusion

This chapter described the data gathering process and explored the relationship of the individual explanatory variables with the dependent variable. Close attention was paid to the nuances of each data source, coding decisions, assumptions, and data corrections. The goal of the preliminary analyses was to explore bivariate relations and conditioning effects of the hypothesized interactive variables.

The preliminary analysis uses bivariate Pearson correlations to explore bivariate relations, Welch t-tests to explore difference in means between observations which experienced the treatment effect (natural disasters) and those who did not (control group), and graphs and incremental (type II) F-tests to assess model fit with and without the proposed interaction terms.

While mixed, the results of the preliminary analysis do point towards the hypothesized relationships. First, there is some support for the general influence of natural disasters on incumbent electoral performance (c.f. hypothesis 1). While small in absolute terms, the correlation of natural disaster occurrence and incumbent party vote share is in the expected, negative, direction. The t-test finds a statistically significant difference in incumbent party vote share in societies which experienced rapid onset natural disasters in the year before an election as opposed to the control group. On average, electorates might punish incumbents for natural disasters. Second, there is some evidence for the conditioning influence of an electorate's experience with natural processes. The t-test found no significant difference in incumbent vote share after rapid onset natural disasters in experienced societies, yet a significant and substantive difference for non-experienced societies who punish incumbents for

natural disasters. Out of the two possible theoretical expectations, these results may suggest that experienced societies do not punish incumbents for natural disasters because they are part of the daily life. My expectation for non-experienced societies was that they, on average, end up rewarding incumbents for natural disasters because incumbents have easy opportunity to be the knight in shining armor. This, so far, is not supported by the data. In any case, the incremental (type II) F-tests, however, did not suggest that the models would perform better when the disaster-risk interaction is included. Third, there is some evidence towards the conditioning effect of clientalism (c.f. hypothesis 2), yet not as expected. The t-tests suggest that there is no discernible difference in incumbent party vote share after rapid onset natural disasters in non-clientalistic societies. However, incumbents in clientalistic societies are punished significantly and substantively. Maybe the initial theoretical expectation of the clientalism interaction needs to be reframed: maybe non-clientalistic societies rely on the quality of their institutions and do not punish incumbents for random external shocks, while clientalistic societies expect the 'big man' to prevent such random shocks in the first place. In any case, the incremental (type II) F-tests, do indeed suggest that the models would perform better when the disaster-linkage interaction is included.

Summary

The analyses in the following chapter are based on two data sets, a national-level data set with national aggregates for all variables and a constituency-level data set which includes national aggregates for the economic and institutional controls while the natural disaster data is disaggregated to the constituency level. The time-frame for the time-series was

constrained to the post World War II period.

The dependent variable of this study is incumbent electoral performance measured as incumbent party vote share in the first round of national lower house elections. An incumbent is the political party in the national lower house which emerged as the main governing party holding the head of government (HoG) portfolio in the last election and which is standing for reelection in this election. Incumbent electoral performance refers to the vote share the incumbent received in the first round of a national lower house election. The dependent variable was calculated using data from the Constituency Level Electoral Archive (CLEA) (Kollman et al. 2017).

The main explanatory variables are counts of natural disasters (divided in the six families) before an election. The operational definition of natural disaster is a natural process which either killed 10 or more people, or affected more than 100 people. The six natural disaster families are geophysical, hydrological, meteorological, climatological, and slow as well as rapid onset natural disasters. Data for these count variables are from the Emergency Events Database (EM-DAT) (Guha-Sapir, Below, and Hoyois 2017)) for the national-level data set and from the DesInventar Project for the constituency-level data set.

The natural risk dimension of the Index for Risk Management (INFORM) is included in this analysis to control for electorates' experience with natural disasters at the national level. The risk index ranges from 0 to 10 with 10 representing maximum risk. The index originally covered only 5 years of each included country, but is treated as constant over time due to the sticky attribute of a country's exposure to natural hazards (e.g. presence of shorelines, distance to fault zones, etc.).

A party linkage variable from the Varieties of Democracy (VDEM) project (Coppedge, Gerring, Lindberg, Skaaning, Teorell, Altman, Bernhard, Fish, Glynn, Hicken, Knutsen, Krusell, et al. 2017; Pemstein et al. 2015) describes electorates' expectation of patronage in return for casting their ballots. The categorical variable *v2psprlnks* ranges from "Policy/programmatic" (1) to "clientelistic" (5).

Economic controls include national gross domestic product (GDP) per capita, change of the national GDP per capita within the last year, national unemployment rate, a dummy for world economic crises, and countries' per capita receipts of international humanitarian aid. Data for these variables was derived from the Penn World Tables, the World Bank, and the Organization of Economic Cooperation and Development's Creditor Reporting System. The data was converted to real 2011US dollars at chained purchasing power parities where applicable.

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vote share in societies which experienced rapid onset natural disasters in the year before an election as opposed to the control group. On average, electorates might punish incumbents for natural disasters. Second, there is some evidence for the conditioning influence of an electorate's experience with natural processes. The t-test found no significant difference in incumbent vote share after rapid onset natural disasters in experienced societies, yet a significant and substantive difference for non-experienced societies who punish incumbents for natural disasters. Out of the two possible theoretical expectations, these results may suggest that experienced societies do not punish incumbents for natural disasters because they are part of the daily life. My expectation for non-experienced societies was that they, on average, end up rewarding incumbents for natural disasters because incumbents have easy opportunity to be the knight in shining armor. This, so far, is not supported by the data. In any case, the incremental (type II) F-tests, however, did not suggest that the models would perform better when the disaster-risk interaction is included. Third, there is some evidence towards the conditioning effect of clientalism (c.f. hypothesis 2), yet not as expected. The t-tests suggest that there is no discernible difference in incumbent party vote share after rapid onset natural disasters in non-clientalistic societies. However, incumbents in clientalistic societies are punished significantly and substantively. Maybe the initial theoretical expectation of the clientalism interaction needs to be reframed: maybe non-clientalistic societies rely on the quality of their institutions and do not punish incumbents for random external shocks, while clientalistic societies expect the 'big man' to prevent such random shocks in the first place. In any case, the incremental (type II) F-tests, do indeed suggest that the models would perform better when the disaster-linkage interaction is included.

Chapter 4

Methodology

Most studies in social sciences start by assuming the ideal experiment. However, the research question, the observed data, and the real data generating process (DGP) (i.e. the process which the real world follows and which researchers try to approximate with their models) usually make scholars realize that the ideal situation is not given. As a result, statistical methods have to be adopted to deal with violations of the Gauss-Markov assumptions.

In this case, too, the nature of the research question requires a data set which inherently violates the assumptions that the observations are independent from each other and that the error term of the statistical model is independent and identically distributed (iid). To have *iid* errors means that all error terms have the same variance but are not correlated with each other. In other words, the error term of one observation is unrelated to the error term of the other observations. When the variance of the error term is constant, we refer to *homoscedasticity*. When the error process is not homoscedastic, we speak of *heteroscedasticity*, i.e. the error variance differs from unit to unit. Cross-sectional heteroscedasticity, that is multiple units ($N > 1$) observed for one point in time ($T = 1$), differs from panel heteroscedasticity ($N > 1$ and $T > 1$) where we further assume that the error variance of each unit *does not* differ across time (Beck and Katz 1995, 636).

The following sections derive a frequentist mixed-effects model which can account for

various expectations about the data used in this research project. Textbooks and articles on this topic are too many to cite here. Instead, I refer the reader to the two main text books which influenced me: Gelman and Hill (2007) and Rabe-Hesketh and Skrondal (2005). In the text below, I will also refer to specific scholarly writings when I discuss specific issues, not general knowledge. It will become clear throughout the following sections that statistical models have to be adapted to the research question and the data at hand. Failure to do so leads to over- or under-confidence in the results, and, generally speaking, to low quality research. The third section of this chapter reviews the ecological fallacy and limits of interpreting statistical results and translating the results back to the real world. The discussion strikes a balance between theoretical modeling needs and the limitations of resources and data quality in order to find the final model specification to be used in the following analysis.

Nested Observations

Consider a standard single-level regression model as in Equation (4.1) where y_i is the estimated value of the dependent variable for observation i , β_0 is the intercept, β_1 is the estimated parameter for independent variable x of observation i , and ϵ_i is the error term for observation i . ϵ_i is the difference of the observed value of the dependent variable \hat{y}_i and the estimated value y_i . We assume that the error term is distributed normally with a mean of 0 and a constant variance σ_ϵ^2 .

$$y_i = \beta_0 + \beta_1 x_i + \epsilon_i \quad \text{with } i = 1, 2, \dots, n \tag{4.1}$$

$$\epsilon_i \sim N(0, \sigma_\epsilon^2) \quad , \quad Cov(\epsilon_i, x_i) = 0 \quad \text{and} \quad Cov(\epsilon_{i1}, \epsilon_{i2}) = 0$$

In such a single-level model, we expect the covariance between an observation x_i and the corresponding error term ϵ_i to be zero. Similarly, we expect the covariance of the error terms of different observations to be zero as well. In formula Equation (4.1) we have defined that the covariance between an error term with itself $Cov(\epsilon_i, \epsilon_i) = Cov(y_i - \hat{y}_i, y_i - \hat{y}_i)$ is the variance of ϵ_i defined as σ_ϵ^2 . Formula Equation (4.2) represents the residual covariance matrix of a single-level model with 3 ($n = 3$) observations. The diagonal elements of the matrix are correlations of the respective error terms with themselves. As stated in formula Equation (4.1), the off-diagonal items are expected to be zero.

$$Cov(\epsilon_i, \epsilon_i) = \begin{matrix} & \epsilon_1 & \epsilon_2 & \epsilon_3 \\ \begin{matrix} \epsilon_1 \\ \epsilon_2 \\ \epsilon_3 \end{matrix} & \begin{bmatrix} \sigma_\epsilon^2 & 0 & 0 \\ 0 & \sigma_\epsilon^2 & 0 \\ 0 & 0 & \sigma_\epsilon^2 \end{bmatrix} \end{matrix} \quad (4.2)$$

In praxis, however, observations often are not independent from each other. Instead, they are clustered, i.e. the “data [has] some type of natural grouping, where the observations are thought to be independent between groups but dependent (in some way) within groups” (Beck 2012, 3). For example, pupils within the same school, countries within the same region, or the federal units within a given country are usually subject to common influencing factors. If one was to estimate the model in formula Equation (4.1) with clustered data, the problems would be manifold. First, due to the dependence of observations, the standard errors would be underestimated which would lead to higher false rejection rates of the null hypothesis. Second, one would not be able to distinguish the influence of the independent

variable from the common influencing factor. The regression coefficients would be biased (i.e. over- or underestimated) due to omitted-variable bias. Mathematically, the regression coefficients would include not only the partial derivative of the dependent variable with respect to the respective explaining variable, but also part of the effect of the omitted variable. Third, and finally, one would introduce endogeneity into the model due to the resulting correlation between the explanatory variables and the error terms. Thus, we have to correct our expectations about the error term in Equation (4.1) and the corresponding residual covariance matrix in order to reflect the common influences.

In order to incorporate the new expectations in the covariance matrix, we have to combine the model in formula Equation (4.1) with a variance component model to form a random intercept model as in formula Equation (4.3).¹³ This two-level model allows for one common influencing factor u per cluster j in the data. The error term ϵ_{ij} of the single-level model is now broken up into two parts: $y_{ij} - \hat{y}_{ij} = \epsilon_{ij} = u_j + e_{ij}$, or a random part e_{ij} which is similar to the residual in formula Equation (4.1) and specific to each observation i , and a fixed part u_j which represents the common influencing factor for each individual i in cluster j . The random intercept model assumes that the residuals for different clusters as well as observations are uncorrelated, and that residuals do not correlate with the covariates. Further assuming that this model represents (or at least closely approximates) the true DGP (i.e. the model is specified correctly), one can resume the analysis of the data.¹⁴

13. One could extend the random intercept model to a random coefficient model, allowing the influence of each variable to vary by cluster. This specification would be useful if the assumption of parameter homogeneity seems unreasonable.

14. White (1980), Beck and Katz (1995), and many others introduced forms of standard errors designed to account for cross-sectional heteroscedasticity. As the number of groups grows larger in the sample, these *robust* standard errors become closer to the ones in the real DGP and ordinary least squares (OLS) estimates are not only consistent but also efficient. However, the standard in the social science literature has become

$$\begin{aligned}
y_{ij} &= \beta_0 + \beta_1 x_{ij} + u_j + e_{ij} \quad \text{with} \quad i = 1, 2, \dots, n \quad j = 1, 2, \dots, N \\
e_{ij} &\sim N(0, \sigma_e^2) \\
u_j &\sim N(0, \sigma_u^2)
\end{aligned} \tag{4.3}$$

$$\begin{aligned}
Cov(u_{j1}, u_{j2}) &= 0 \quad \text{and} \quad Cov(u_{j1}, e_{i1j1}) = 0 \quad \text{and} \quad Cov(e_{i1j1}, e_{i2j1}) = 0 \\
Cov(u_{j1}, e_{i1j2}) &= 0 \quad \text{and} \quad Cov(e_{i1j1}, e_{i2j2}) = 0 \\
Cov(u_j, x_{ij}) &= 0 \quad \text{and} \quad Cov(e_{ij}, x_{ij}) = 0
\end{aligned}$$

The variance covariance matrix of the residuals of a two-level model with 6 observations from 3 clusters looks as follows.

$$\begin{aligned}
&Cov(\epsilon_{ij}, \epsilon_{ij}) = \\
&\begin{matrix} & \epsilon_{11} & \epsilon_{21} & \epsilon_{12} & \epsilon_{22} & \epsilon_{13} & \epsilon_{23} \\ \begin{matrix} \epsilon_{11} \\ \epsilon_{21} \\ \epsilon_{12} \\ \epsilon_{22} \\ \epsilon_{13} \\ \epsilon_{23} \end{matrix} & \left[\begin{array}{cccccc} [\sigma_u^2 + \sigma_e^2] & \sigma_u^2 & 0 & 0 & 0 & 0 \\ \sigma_u^2 & [\sigma_u^2 + \sigma_e^2] & 0 & 0 & 0 & 0 \\ 0 & 0 & [\sigma_u^2 + \sigma_e^2] & \sigma_u^2 & 0 & 0 \\ 0 & 0 & \sigma_u^2 & [\sigma_u^2 + \sigma_e^2] & 0 & 0 \\ 0 & 0 & 0 & 0 & [\sigma_u^2 + \sigma_e^2] & \sigma_u^2 \\ 0 & 0 & 0 & 0 & \sigma_u^2 & [\sigma_u^2 + \sigma_e^2] \end{array} \right] \end{matrix} \tag{4.4}
\end{aligned}$$

When fitting multi-level models, researchers can interpret the β parameters just like in a

to blindly apply robust standard errors without thinking critically about their usefulness and assumptions (see King and Roberts (2015), and Maddala (1998) cited in Beck (2012)). “There is no such thing [as] a standard error that is ‘robust’ to any and all violations of assumptions“, “a given way of estimating robust standard errors is correct [only] in the presence of the specified violation” (2). King and Roberts (2015) even argue that a difference between regular and robust standard errors should be understood as evidence towards misspecification of the model which requires the researcher to respecify the model. Instead of simply using a common type of robust standard errors, I formulate expectations about the model which I derive from previous experience with this kind of data, and include variables which are supposed to influence intra- or inter-cluster variability. Therefore, I can claim all errors for myself and the reader has complete access to the assumptions on which my results are conditioned.

single-level regression model. β_1 in equations Equation (4.1) and Equation (4.3) represents the predicted change in y for a 1 unit change in x_1 . The variance components to be estimated and interpreted are σ_u^2 and σ_e^2 . σ_u^2 is the residual variance at level 2, or the stage in the hierarchy under which the observations are clustered (e.g. the continent when looking at multiple countries), and σ_e^2 is the residual variance at level 1, or at the smallest unit in the data. In order to determine whether the residual variance at higher levels is significant and warrants the use of multi-level modeling, one needs to fit the model with and without σ_u^2 and do a likelihood ratio test.

The variance partitioning coefficient ρ is helpful to determine how the residual variance is distributed. The maximum value of ρ is 1 and the minimum is 0. A large value of ρ means a lot of clustering in the data, a small value means little clustering.

$$\rho = \frac{\sigma_u^2}{\sigma_u^2 + \sigma_e^2} \quad \text{with} \quad \rho = [0, 1] \quad (4.5)$$

A multi-level model such as in formula Equation (4.3) allows to answer two types of research questions: questions concerning means (How does one variable influence another?), and questions concerning variances (How much of the variance is due to common influencing factors?). Whether σ_u^2 and σ_e^2 are of substantive interest or included merely because the clustering in the data presented a nuisance and prevented the researcher from fitting a single level regression model as in formula Equation (4.1), depends on the research question at hand.

Whether or not one can interpret the results of the models above only with respect

to the data sample or the population beyond that depends on the assumptions about the cluster-specific effects u_j . Rabe-Hesketh and Skrondal (2005, 95-97,158-160) explain that the term u_j can either be interpreted as random (as done in equation Equation (4.3)), or as a fixed effect. The fixed effect specification does not assume u_j to be independent from e_{ij} . Among other reasons, fixed-effects should be used if there is an expectation that units inherently differ. For example, two countries can be inherently different and not assuming $Cov(u_j, e_{ij}) = 0$ might be beneficial. However, the random-effects specification has multiple advantages. First, u_j in the random-effects specification is reestimated for each sample which leads to bigger standard errors, and thus, more conservative significance tests. Also, random-effects allow to interpret the results beyond the sample for the overall population. Finally, random-effects allow for the estimation of cluster-specific variables (159). For one, the beauty of multi-level models is the possibility to include variables that change between clusters but have no or few variability within clusters (e.g. whether or not the country has a presidential or parliamentary system). And estimating the effects of cluster-level covariates is arguably like consciously including factors which we believe distinguish one unit from the other. For these reasons, the models in this study employ the random-effects specification.

Time Dependencies

The previous section presents a solution on how to deal with grouped data. Now, consider the influence of time when grouped data is collected over time, that means each individual i in its respective cluster j is measured once per time period t . Time periods can be days, months, quarters, years, or as in this case, election cycles. The introduction of a time

dimension to the model introduces the problems of time dependencies. For example, if one was to guess a person's income of next year, one would make the estimate based on the knowledge of that person's income in this year. Similarly, a country's GDP in the year $t + 1$ is probably somewhere close to its GDP in year t . We can extend equation Equation (4.3) to incorporate the time dimension:

$$\begin{aligned}
 y_{ijt} &= \beta_0 + \beta_1 x_{ijt} + u_j + e_{ijt} & \text{with } i &= 1, 2, \dots, n \quad j = 1, 2, \dots, N \\
 & & t &= 1, 2, \dots, T \\
 e_{ijt} &\sim N(0, \sigma_e^2) \\
 u_j &\sim N(0, \sigma_u^2)
 \end{aligned} \tag{4.6}$$

Formally, a researcher working with cross-sectional time-series data likely needs to address one or more of the following issues which prevent the error term to be *iid* (from Beck and Katz 1995, 645):

- Panel heteroscedasticity: The variance of the error term varies between clusters but not within clusters and not across time. (This latter addition of the time dimension distinguishes panel heteroscedasticity from the cross-sectional heteroscedasticity mentioned above.) $Var(\epsilon_{ijt}^2) \neq Var(\epsilon_{ij't'}^2)$, but $Var(\epsilon_{ijt}^2) = Var(\epsilon_{ij't'}^2)$, so that $Var(\epsilon_{ijt}^2) = \sigma_j^2$
- Contemporaneously correlated errors: $Cov(\epsilon_{ijt}, \epsilon_{i'jt}) = Cov(\epsilon_{ij't'}, \epsilon_{i'jt'}) \neq 0$ for $t \neq t'$, but $Cov(\epsilon_{ijt}, \epsilon_{ij't'}) = 0$
- Unit-specific serially correlated errors: $\epsilon_{ijt} = \alpha_j \epsilon_{ijt-1} + v_{ijt}$, with v_{ijt} shocks that are temporally independent, identically distributed, zero-mean random variables. Note the

subscript of α_j : the j signifies *unit-specific* serially correlated errors. With ϵ_{ijt-1} we assume an $AR(1)$ process.

- Common Serially Correlated Errors. $\epsilon_{ijt} = \alpha\epsilon_{ijt-1} + v_{ijt}$, where $v_{i,t}$ are incoming shocks which affect each unit in the same way: α has no subscript.

As with grouped, or cross-sectional, data, one could use the old school solution of using robust standard errors designed for TSCS data (such as panel-corrected standard errors as in Beck and Katz (1995)), or to consciously model the violations. Obviously, the latter option generates sound models by allowing different dynamics in the DGP. When considering TSCS data, it usually makes sense to expect serial correlation of the errors, so that we expect the measurement of an individual to be correlated over time: $E(\epsilon_{ijt}) \neq E(\epsilon_{ijt'})$. In that case, we need to formulate Equation (4.6) with serially correlated errors:

$$y_{ijt} = \beta_0 + \beta_1 x_{ijt} + u_j + e_{ijt} \quad \text{with} \quad e_{ijt} = \alpha e_{i-1jt} + v_{ijt} \quad (4.7)$$

where the error term e_{ijt} is divided into a random part v_{ijt} and its lag, i.e. its measure in the previous period. α is the magnitude of the influence of the previous error on the current one. When $\alpha < 1$, then any influence of previous periods cedes soon and the process is stationary. Equation Equation (4.7) presents an autoregressive process of order 1. Higher order processes simply include higher lags of the errors. When dealing with daily data or even shorter time intervals, higher lags can make sense. Since this particular research project has one observation per electoral period (usually every 4 years), higher order lags are unlikely.

Alternatively, equation Equation (4.6) can be extended to a lagged dependent variable

model with cluster-specific intercepts as in Equation (4.8) which can be appropriate when the best indicator of the dependent variable is its observation in the previous time period. “A useful feature of such models is that they can be used to distinguish between two competing explanations of within-subject dependence: *unobserved* heterogeneity (represented by the random intercepts) or *state dependence* (represented by the lagged response)” (Rabe-Hesketh and Skrondal 2005, 273). Note, equation Equation (4.8) assumes *iid* errors.

$$y_{ijt} = \beta_0 + \gamma y_{ijt-1} + \beta_1 x_{ijt} + u_j + e_{ijt} \quad (4.8)$$

Beck and Katz (2011, 335) show that the serially correlated and lagged dependent variable “specifications are special cases of the autoregressive distributed lag (ADL) model” in equation Equation (4.9). The similarity is as follows: equation Equation (4.7) constrains θ in Equation (4.9) to $\theta = -\beta_1\gamma$, and equation Equation (4.8) constrains θ in Equation (4.9) to $\theta = 0$.

$$y_{ijt} = \beta_0 + \beta_1 x_{ijt} + \gamma y_{ijt-1} + \theta x_{ijt-1} + u_j + e_{ijt} \quad (4.9)$$

Beck and Katz (2011, 340) argue that the link between equations Equation (4.7) through Equation (4.9) allows the researcher to specify his or her model in three different ways and test whether $\theta = -\beta_1\gamma$ or $\theta = 0$. As γ in Equation (4.9) approaches zero, it might get difficult to correctly rule out either the lagged dependent variable or the serially correlated errors. However, as γ approaches zero, the weight of the wrongly included lagged dependent variable and thus, the bias introduced thereof, approaches zero. Likewise, the bias of wrongly

including the serially correlated specification also approaches zero. Beck and Katz, therefore, conclude that wrongly including such dynamic controls is not as bad as wrongly excluding them. This goes against the understanding of omitted-variable bias in other settings. It is thus wise to start by estimating Equation (4.9) and trying to reduce the model to either Equation (4.8) or Equation (4.7) afterwards.

Note, however, that Equation (4.9) still makes some significant assumptions. First, the model still assumes homoscedastic errors. The hierarchical design allows for group-specific effects but the variance of the random intercepts is constant $Var(u_j) = \sigma_u^2$. Rabe-Hesketh and Skrondal (2005) use their hierarchical design combined with robust standard errors to allow for panel heteroscedasticity. Second, the model assumes a continuous dependent variable. Dynamic control techniques for binary TSCS data are different (see for example Beck, Katz, and Tucker 1998).

Third, the model assumes stationary data. Beck and Katz (2011, 342) and Rabe-Hesketh and Skrondal (2005, 311) provide model specifications for integrated or moving average series respectively. However, Beck and Katz (2011, 343-4) argue that some non-stationary tools are not useful “for many, if not most, political economy TSCS datasets”. The reasoning is simple. In most cases the number of time periods in political economy applications is low (less than 100, usually less than 60 years) compared to daily economic data. Thus, even if there is a trend in political economy applications, it has little time to run a full cycle. Further, any time series test based on asymptotic assumptions about T has little power in applications with $T \ll 100$. Also, political economy data sets have many variables that are slowly changing, and are often bound between 0% and 100% or can only take on the values

0 or 1. Such variables are on a short leash and applying non-stationary techniques on these variables may simply be overkill.

Moreover, note that Equation (4.9) can be difficult to estimate. Due to the initial-conditions problem which rises when a lagged dependent variable is combined with a random intercept, so called Nickel-bias (named after Nickell 1981) biases the results enormously (Beck and Katz 2011, 342; Rabe-Hesketh and Skrondal 2005, 273-4). Beck and Katz (2011, 342) state, however, that the bias becomes negligible when $T > 20$ so that returning to OLS is possible. Finally, OLS produces inconsistent results when estimating a model with a lagged dependent variable and serially correlated errors. Such a model needs to be estimated with MLE (339). Recently, both Spencer (2003) and Kripfganz (2016) have worked on providing better estimation techniques for such dynamic hierarchical models.

The next, and final, section takes a step back from the nuances of modeling the collected data with respect to the research question. Instead, the section focuses on the limits of interpreting the results and drawing inference from aggregate data to individual voting behavior.

Limits of Interpreting the Results

Recall, the goal of this study is to make inference about the impact of natural disasters on national lower house elections across countries and time. Thus, the dependent variable concerns a voting outcome and the main explanatory variable is the occurrence of a natural disaster. Short of a voter-level survey which covers multiple countries as well as elections and compares voting intentions or outcomes before and after natural disasters, such research

questions can only be answered by collecting and analyzing aggregated data. Voter-level surveys cost both money and time. Instead, researchers with such problems conduct their research on an aggregated level which is close enough to the individual voter in order to best approximate the hypothesized relationship, and yet far enough of the individual voter to make data collection easier, sometimes possible. That means, the researcher has to compromise internal validity in order to increase external validity, that is being able to compare voting behavior across geographical units and time. The unit of analysis consequently changes from the voter in a given year, or voter-year, to the country-year or even country-constituency-year. The latter represents a temporal two-level model such as in equation Equation (4.6). The section on data collection yields more insights on how the availability of data forces researchers to compromise on aggregated levels and drives the sample. This is obviously also a time when to consider construct validity and whether data is missing at random or systematically. The focus here, however, shall lie on the interpretation of the results once the data are analyzed.

Changing the unit of analysis changes inference. David Singer (1961) discusses the aggregation of analysis in the field of International Relations and refers to the level-of-analysis problem. Higher levels of aggregation make data collection easier and yield a degree of “comprehensiveness” (80) which is unobtainable without aggregation. However, aggregation overlooks individual differences and creates a “highly homogenized image” of the sampled population. Such smoothing of the data prevents “*causal* statements” and limits researchers to “*correlative* statements” (82). In Singer’s (1961) words, “... no matter how persuasive the deductive logic – one may speak only of correlation, not of consequence.” Consider, for

example, a data set and its corresponding analysis which allows to conclude that a one unit increase in personal income causes a β change in probability that a voter chooses Y . Now, when the same relationship is studied at the national level, income is measured most likely by national GDP (which begs the question of construct validity), and the inference changes to “On average, a one unit increase in average national income is expected to coincide with the electorate increasing its average level of choice of Y by β .” Thus, causal interpretation is downgraded to adjustments of expectations about averages, and the group-level data is only used to make inference about the group, not individual voters.

The logical mistake of inferring individual behavior from group-level data is referred to as “ecological fallacy”. At least since Robinson (1950) the logical problem of inferring individual behavior, individual correlation, from group data which yields ecological correlation, is widely recognized. Since then, the topic has been discussed by various authors in their respective disciplines (see, for example Selvin (1958) and Schwartz (1994), but especially King (1997) and the issues included in volume 38, issue 2 of the *International Journal of Epidemiology*). The basic idea is that an individual correlation is based upon data collected for indivisible subjects, such as persons, while ecological correlation is based on data collected on groups of indivisible subjects. Robinson (1950, 354) argues that internal and ecological correlation are both constructs derived from frequency tables. However, internal correlation is based upon the actual observed frequencies in the table, whereas ecological correlation is based on the table’s margins. Since the numbers in the margins of a frequency table can be derived from a multitude of combinations of numbers within the table, Robinson concludes that ecological correlations do not allow inference on the individual correlation. Thus, Robinson (1950, 357)

concludes that ecological correlation is “meaningless”.

In contrast to the general agreement over the problem of inferring individual behavior from group data, there is less agreement on the value of ecological correlation. King (1997) starts his book with a section on “The Necessity of Ecological Inferences”, and Pearce (2000, 326) observes that ecological inference is “back in business”. On the one hand, group level data is easily available and provides good grounds for theory generating research. On the other hand, there are influences at the group-level which need to be incorporated in order to minimize omitted-variable bias. Subramanian et al. (2009) demonstrate this point very well with Robinson’s data extended by group-level measures which should have been available to Robinson. They find significant differences between their single and multi-level models in a Bayesian framework.

Given the discussion above, I will proceed very carefully during the interpretation of the results in the analysis. Both, the presentation and interpretation of the statistical results will be careful not to overstate statistical meaning and ecological inference. The goal is to find trends which can be generalized across time and countries. Depending on what the data supports, one can then discuss how the findings impact our confidence in the existing base of knowledge and interpretations derived thereof.

Discussion

In theory, controlling for both types of dependency in TSCS data remains of utmost importance. Plümper and Neumayer (2010, 422) state that not controlling for temporal dependence while controlling for spatial dependence when the DGP would require both,

“lead[s] to upwardly biased spatial effects and may thus cause wrong inferences.” (Also, see their discussion of the estimation results. (2010)) Halleck Vega and Elhorst (2016, 85) also maintain “that approaches that do not simultaneously account for serial dynamics, spatial dependence and common factors, or that ignore one of these issues, may lead to biased inference”. In praxis, however, we see that a big chunk, if not the majority, of methodologically related work does not profit from recent insights in spatial, time-series, and cross-section statistics. Instead, the expectations that lead to spatio-temporal concerns are either ignored or “presumably averted” via unit-fixed and robust standard errors without much, or any, theoretical or methodological discussion (c.f. King and Roberts (2015) and Beck (2001, 2012)).

In praxis, however, any researcher has to eventually find a balance between (1) data availability and quality, (2) limited resources and time, and (3) feasibility of model estimation. Given that this study looks at elections, that means each election cycle is one time period t , and the number of countries and their respective provinces is fixed, it may be difficult to profit from asymptotic behavior in either T or n . Beck, Nathaniel L.Gleditsch and Beardsley (2006, 40) argued that a model with both a temporally and spatially lagged dependent variable (y_{it-1} and $w_i y_{it}$) could not be estimated unless the spatial term was lagged temporally as well ($w_i y_{it-1}$), even when the errors are independent and the temporally lagged dependent variable shows no correlation with the error term. This does not even include more complicated models which would allow spatio-temporal dependencies in the explaining variables.

At the time of writing, I found several approaches which (attempt to) overcome the limits

identified by Beck, Nathaniel L.Gleditsch and Beardsley (2006). I tried various frequentist and Bayesian approaches to estimate model specifications beyond 4.9 but the results were consistent: either convergence was never achieved or the data requirements of the method (or model specification) could not be met given the resource and time constraints. Comparatively, my data sample is bigger than in many other studies but the panel of countries is highly unbalanced, many countries' time-series have gaps (some at random, some were due to 'breaks' in the democratic process), some indicators have little to no variance over time, and many indicators are aggregates with more detailed measured not being available, unreliable, or inconsistent across time and/or countries. The limitations of the data and the data gathering process are detailed in the chapter on Concepts and Measurement.

Striking a balance, I have decided to estimate a final model specification which follows equation 4.8 and approaches equation 4.9. The analysis uses a variance component model in order to account for the clustering of observations by countries and constituencies. This does assume that the residuals as well as cluster specific common intercepts do not correlate with the covariates, that the residuals do not correlate across clusters, and that the clusters do not correlate with each other. This multi-level model is paired with a lagged dependent variable. Both theoretically and practically last election's winner's vote share is a very good indicator of the incumbent's vote share in the next election. Furthermore, the lagged dependent variable can be interpreted as a cost of ruling measure and is thus, of substantive importance for the model specification. The final model specification approaches equation 4.9 because theory and data quality motivate the lagging of the explaining variables. The data at its current granularity level cannot distinguish between events which happened in

the months before an election of a given year and the months after the election yet still in the same year. This is true for the natural disaster and economic variables. Thus, the following model specifications offer the alternatives of lagging by one year or electoral period. This corresponds to estimating θ in equation 4.9 and constraining β_1 to zero. If there was any concern about endogeneity, one could argue that lagging the explaining variables may reduce such concerns.

Other potential time and spatial dependencies are excluded from the modeling process for practical reasons. Due to constraints I neither extended the granularity of the data nor engaged in further spatial coding. Thus, the data did not allow for some spatial modeling approaches. Further, I assume that temporal dependencies either cannot influence the results in a substantive manner because there are few elections per country which in general are four years apart, or the data does not have enough data points per cluster to test and model such dependencies. Finally, I did not use any form of 'robust' standard errors because I side with King and Roberts (2015). Robust standard errors do not yield a performance indicator of how well they actually dealt with the nuisance for which they were designed. Such knowledge is not essential to their operation. I prefer to be aware and open about the limitations of the data and model specification and be cautious in drawing inference from the statistical results in the following analyses.

In conclusion, the method of choice in the following analysis chapter is the estimation of models by fitting linear mixed-effects models via restricted maximum likelihood (REML). Note that while there may be research teams which can employ more sophisticated methods, a quick glance at any of the major political science journals reveals that this study's method

falls securely into current best academic practices. I am looking forward to reading future research which picks up the methodological challenges and weaknesses identified above.

Conclusion

This chapter reviewed modeling considerations which were drawn from expectations about the true data generating process. A research design using data sets with observations clustered by countries and constituencies and collected over time has to yield a discussion of various violations of the assumptions of independence of observations and error terms. Spatio-temporal dependencies can bias estimation results when not modeled in the process. In praxis, however, data quality and estimation feasibility often do not meet the requirements to model everything which should be accounted.

Striking a balance between data quality and resource constraints the following analysis uses linear mixed-effects models with lagged dependent variables. This approach splits the variance into multiple components and lags the dependent and explaining variables for practical as well as theoretical reasons. While modeling some dynamics which can be reasonably expected from the true DGP, this approach still imposes assumptions which may be impossible to hold. Nevertheless, such linear mixed-effects models are within current good academic practices.

Summary

The first section discusses implications of clustered and nested data on the model specification process. The assumptions around independence of observations are violated when

observations are influenced by a common factor or are collected as repeated observations from the same cross-sectional unit. This in turn negatively impacts effectiveness and efficiency of model estimation results. A solution to this problem are variance component models, or mixed-effects models, which have the added benefit that they allow the inclusion of explaining variables of varying level of detail.

The second section discusses implications of temporal dependencies on the model specification process. Temporal dependencies arise when observations are correlated over time. Again, they negatively impact the effectiveness and efficiency of model estimation results. Time dependencies can be modeled via state dependencies or error correlations as suggested above.

The third section reviews the ecological fallacy and limits of interpreting statistical results and translating the results back to the real world. A research design which aims to compare multiple units over time has to use measures which are reliable and comparable across units and time. Often this is achieved through aggregation of measures. For this study this means I have to abstract from the individual voter to the electorate of a given constituency or nation. Changing the level of analysis has to change inference. Inference in this study is now constrained to correlative statements about changes in the measures and their correlation with the average behavior of a group of units, the electorate. Therefore, ecological inference must be careful not to overstate statistical meaning.

The discussion stresses the importance of striking a balance between theoretical modeling requirements on one side and data quality and resource constraints on the other. The method of choice in the following analysis chapter is the estimation of models by fitting lin-

ear mixed-effects models with lagged dependent variables via restricted maximum likelihood (REML). This approach splits the variance into multiple components and lags the dependent and explaining variables for practical as well as theoretical reasons. While modeling some dynamics which can be reasonably expected from the true DGP, this approach still imposes assumptions which may be impossible to hold. Nevertheless, such linear mixed-effects models are within current good academic practices.

Chapter 5

Analysis

The theory suggests that natural disasters affect electoral outcomes via three pathways: (i) natural disasters are direct determinants of incumbents' electoral fate, (ii) natural disasters may act as a catalyst, pushing more unsatisfied citizens towards political action by exacerbating existing conditions or revealing them, and (iii) natural disasters impact elections only if an intervening variable creates the right environment. This chapter examines the (multivariate) statistical evidence for this relationship. Following the theory, I argue that an incumbent's vote share is dependent upon the occurrence of natural disasters, the state of the economy, the respective society's experience with natural hazards and its expectation of patronage, and the incumbent's vote share in the previous election.

I present a narrative which reexamines the effects of natural disasters on incumbent electoral performance with two original data sets. While most related studies focus on one single country (see table 2.1), the following analysis uses elections observed in multiple countries over time in order to deduce generalizable tendencies of natural disasters impacting incumbents' electoral fate. One data set uses the country-election year as the unit of analysis and the second uses country-constituency-election year. The natural disaster variables are populated with two different sources to allow disaggregation to the constituency-level in the second data set. Using two different samples allows to not only test the respective

null hypotheses in multiple settings but also to check the robustness of the results across model specifications and samples. Indeed, comparing and interpreting the results simultaneously yields new insights about the hypothesized relationships between natural disasters and incumbents' electoral performance.

I find that it is difficult to draw conclusive interpretations from the statistical analyses which are to follow. The findings are volatile, varying greatly across samples and model specifications. The base models which include only unconditioned, base effects of natural disasters and which are supposed to replicate the research design of previous studies in this field, find little evidence for a direct effect of natural disasters on incumbent party vote share. I propose that this null-finding could be the results of either (a) there being no generalizable base effect of natural disasters on the dependent variable, or (b) the inclusion of so many elections from different countries over time masquerading the effect of natural disasters because conditional effects are not included. When intervening factors such as international humanitarian aid and the electorates' expectations for patronage are included, the results are similarly volatile. Again, there seems to be little indication of a base effect of natural disasters, but there might be conditional effects of international humanitarian aid and patronage expectations which interact with natural disasters to impact incumbents' electoral performance. The factors of patronage expectations and international humanitarian aid are far from established as factors within the black box between natural disasters and incumbent electoral performance. Nevertheless, this study yields at least a promising first analysis despite the volatility of the results across samples and model specifications.

This chapter proceeds by presenting the study's base models. These base models use a

country-election year unit of analysis and include common variables seen in related studies which motivated the analysis at hand. The two succeeding sections discuss statistical results from models which include terms interacting natural disasters and patronage expectations as well as natural disasters and humanitarian aid and which were estimated on both the national-level and constituency-level samples. This discussion focuses on individual results. The first of these two sections discusses the interaction with patronage expectations and the second section discusses the effect of humanitarian aid. The fourth section then includes model diagnostics to assess model fit and underlying assumptions. Included are residual versus fitted, quartile-quartile plots, and residual versus leverage plots. The fifth section draws the statistical results together allowing the interpretation of the various models and coefficients simultaneously. This discussion is more nuanced and allows more confident acceptance or rejection of the respective null hypotheses. The findings and their implications in the light of the broader literature on natural disasters and incumbent electoral performance will be discussed in the concluding chapter. Given the acceptance or rejection of the respective null hypotheses, this part moves from the discussion of the statistical results to the substantive discussion of the results and their implications for what we think to know about the link between natural disasters and incumbent electoral performance. A conclusion and summary reiterate the main points of this chapter's discussion.

The Base Model

The base model establishes comparability to previous studies by using national aggregates for a country-year unit of analysis. This section aims at replicating the results of similar

studies such as Achen and Bartels (2002, 2013) or Quiroz Flores et al. (2013) and Remmer (2014) which hypothesize a direct effect of natural disasters on incumbent electoral performance (compare to hypothesis 1 in this study). These authors use cross-sectional time-series approaches within a single country or across multiple countries respectively. I follow the model specification of these studies and express the dependent variable, incumbent electoral performance, as a linear function of natural disaster occurrences and (economic) control variables. This specification shall serve as the base model which is going to be compared to the inference drawn from the succeeding models.

The base model is estimated by fitting a linear mixed-effects model via restricted maximum likelihood (REML). The linear mixed-effects model shall be specified as

$$(y_{i,t}|\beta, \sigma_y, \alpha_i, X) \sim N(\mathbf{X}\beta + \alpha_i, \sigma_y^2) \text{ for } i = 1, 2, \dots, n, t = 1, 2, \dots, T \quad (5.1)$$

with

$$\alpha_i \sim N(\mu_\alpha, \sigma_\alpha^2) \quad (5.2)$$

where $N(\cdot|\cdot, \cdot)$ represents the multivariate normal distribution, $y_{i,t}$ is the observed response for cross-sectional unit i at election t , \mathbf{X} is a matrix of explaining variables, β is a vector of constant slopes or regression coefficients, α_i is a vector of varying intercepts per cross-sectional unit i , and σ^2 are variance parameters. Throughout this study I will refrain from using the terms 'fixed' and 'random' effects due to the confusing and even contradictory usage of the terms in the statistical literature (Gelman and Hill 2007, 245-6). Instead, I will follow Gelman and Hill (2007, 246) and refer directly to constant or varying slopes and

intercepts as specified in the models.

Table 5.1 presents estimates for the base model with different specifications for the natural disaster families (lagged by one year) and using the national-level data while omitting varying intercepts by country. The first two models in table 5.1 use different natural disaster family specifications but are otherwise identical. The third model uses the four natural disaster families and includes the national unemployment rate (in percent) as an additional covariate which was previously excluded in order to profit from more observations in the model estimation. The varying country intercepts were excluded from the table because they are not of theoretical importance. They serve to account for clustering in the data during the estimation process; observing any country over time yields observations which are not truly independent of each other within countries and thus, these observations yield less information than truly independent observations in a random sample.

The results in table 5.1 do not suggest a broad electoral impact of natural disasters. We see that the occurrence of geophysical natural disasters in the year prior to elections reduces incumbent party vote share. Geophysical natural disasters include mostly earthquakes which have been found to affect government change sometimes more than the other families (see e.g. Ahlerup (2013b) on government turnover and Ahlerup (2013a) on democratisation). Holding everything else equal, a geophysical disaster is expected to reduce incumbent party vote share. The other natural disaster families do not have a statistically significant impact on the dependent variable, yet most of the coefficients have the expected (negative) sign.

Of the control variables, only lagged incumbent party vote share and the change in per capita GDP perform as expected. Intuitively, incumbent party vote share increases

Table 5.1: Base Models (Disasters in the previous year; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
Incumbent Vote Share	0.593*** (0.042)	0.590*** (0.042)	0.649*** (0.052)
Rapid Onset Disaster	-0.069 (0.143)		
Slow Onset Disaster	-0.555 (0.684)		
Climatological Disaster		-0.112 (0.849)	0.498 (1.083)
Geophysical Disaster		-1.645* (0.886)	-2.619** (1.168)
Hydrological Disaster		-0.366 (0.343)	0.023 (0.430)
Meteorological Disaster		0.168 (0.269)	0.373 (0.335)
GDP pc change	0.211*** (0.078)	0.213*** (0.078)	0.022 (0.109)
GDP pc	-0.653 (0.765)	-0.694 (0.762)	0.418 (0.887)
World Economic Crisis dummy	1.169 (0.893)	1.320 (0.897)	1.982 (1.288)
OECD Humanitarian Aid	0.255 (0.238)	0.257 (0.237)	0.496 (0.357)
Unemployment Rate			0.130 (0.127)
Constant	17.729** (7.691)	18.349** (7.693)	3.362 (9.350)
N	640	640	380
Log Likelihood	-2,434.962	-2,431.401	-1,470.525
AIC	4,889.925	4,886.801	2,967.050
BIC	4,934.539	4,940.339	3,018.273

*p < .1; **p < .05; ***p < .01

Natural disaster occurrences as count per disaster family in the year prior to the election. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the gross domestic product per capita. All monetary variables are in million real 2011USD at chained PPPs. All varying country intercepts were omitted.

as per capita GDP increases. The level of the GDP, the world economic crisis dummy, and the unemployment rate neither have the expected sign nor are statistically significant. This is surprising given the overwhelming statistical evidence from the economic voting literature. The expected sign of humanitarian aid differs according to patronage expectations as indicated in hypothesis 3. Thus, the effects could cancel each other out and produce the insignificant coefficient here. The effect will be monitored in more detail below when hypothesis 3 is tested.

Table 5.2 presents the same models as table 5.1 but with natural disaster occurrences during the previous electoral period (as opposed to the one-year lag). The number of observations is smaller than before because of missing information; an electoral period usually lasts 4 years and I did not calculate the disaster count if natural disaster data was missing for any of the respective country-years. This time the results suggests that both rapid onset natural disasters and geophysical natural disasters affect incumbent electoral performance negatively. Since most geophysical natural disasters (earthquakes) are part of the rapid onset natural disaster family, this result is not too surprising. Per capita GDP continues to perform as expected while the remainder control variables do not show any statistical significance. Lagged humanitarian aid from OECD donors is statistically significant in the third model this time. However, this result seems to be dependent heavily on the specification of the model and the number of observations. Further, the chances are high that at least one of the regression coefficients is wrong given the high number of coefficients in model 3. Thus, I will not interpret this particular finding as the chance of a false positive (type I error) seems too high. As above, all varying intercepts have been omitted from the output.

Table 5.2: Base Models (Disasters in the previous electoral period; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
Incumbent Vote Share	0.640*** (0.039)	0.630*** (0.040)	0.683*** (0.050)
Rapid Onset Disaster	-0.128*** (0.040)		
Slow Onset Disaster	0.043 (0.244)		
Climatological Disaster		0.193 (0.388)	0.421 (0.477)
Geophysical Disaster		-0.463* (0.253)	-0.516* (0.272)
Hydrological Disaster		-0.107 (0.115)	0.048 (0.131)
Meteorological Disaster		-0.093 (0.086)	-0.105 (0.094)
GDP pc change	0.165** (0.072)	0.167** (0.072)	-0.024 (0.107)
GDP pc	0.138 (0.690)	-0.008 (0.693)	1.423 (0.876)
World Economic Crisis dummy	1.172 (0.837)	1.209 (0.836)	1.898 (1.236)
OECD Humanitarian Aid	0.357 (0.221)	0.352 (0.221)	0.638* (0.348)
Unemployment Rate			0.161 (0.120)
Constant	9.157 (7.221)	11.013 (7.276)	-7.551 (9.500)
N	651	651	367
Log Likelihood	-2,443.785	-2,442.794	-1,401.851
AIC	4,907.569	4,909.588	2,829.701
BIC	4,952.354	4,963.330	2,880.471

*p < .1; **p < .05; ***p < .01

Natural disaster occurrences as count per disaster family during the last electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the gross domestic product per capita. All monetary variables are in million real 2011USD at chained PPPs. All varying country intercepts were omitted.

Table 5.3 presents base models for the constituency-level data with natural disaster variables lagged by one year. Remember, the constituency-level data relies on fewer countries but accounts for their first-order sub-national administrative regions and uses a different natural disaster data source to match constituencies with natural disaster occurrences. This model adds constituency varying intercepts to the model specification above. This time we find evidence for the (negative) influence of rapid onset and hydrological natural disasters on incumbent party vote share. Again, most hydrological disasters are part of the rapid onset family. The economic control variables do not perform as expected: either they have an unexpected sign or they do not achieve statistical significance. It is interesting though how many economic controls with the ‘wrong’ sign achieve statistical significance. As above, all varying intercepts have been omitted from the output.

Table 5.4, finally, presents the base models for the constituency-level data and with natural disaster counts per electoral period. This table suggests expected (negative) effects of rapid onset and hydrological natural disasters. The effect of meteorological natural disasters does not seem to be robust to model specification. Slow onset and climatological natural disasters are actually shown to *increase* incumbent party vote share. It is not surprising that both families present this tendency at the same time since most slow onset natural disasters are in the climatological group (e.g. droughts). The sign of these coefficients is surprising and will be monitored in the models below. As above, the economic controls do not perform as expected and all varying intercepts have been omitted from the output.

In sum, there is some evidence for (negative) electoral impact of natural disasters. A consistent effect was found only for rapid onset natural disaster while the negative influence

Table 5.3: Base Models (Disasters in the previous year; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
Incumbent Vote Share	0.405*** (0.028)	0.407*** (0.028)	0.454*** (0.028)
Rapid Onset Disaster	-0.663** (0.331)		
Slow Onset Disaster	1.253 (1.162)		
Climatological Disaster		1.073 (1.220)	0.698 (1.136)
Geophysical Disaster		-2.563 (5.361)	-2.634 (4.987)
Hydrological Disaster		-0.759** (0.385)	-0.827** (0.361)
Meteorological Disaster		0.270 (0.891)	0.067 (0.832)
GDP pc change	0.046 (0.054)	0.048 (0.054)	-0.192*** (0.065)
GDP pc	-2.112* (1.182)	-2.162* (1.186)	-4.080*** (1.403)
World Economic Crisis dummy	2.765*** (0.706)	2.759*** (0.707)	1.934*** (0.709)
OECD Humanitarian Aid	0.777*** (0.242)	0.779*** (0.242)	0.529** (0.245)
Unemployment Rate			-1.070*** (0.153)
Constant	35.337*** (10.652)	35.649*** (10.678)	61.047*** (12.700)
N	1,176	1,176	1,045
Log Likelihood	-4,470.215	-4,466.614	-3,899.359
AIC	8,962.431	8,959.229	7,826.717
BIC	9,018.199	9,025.137	7,896.042

*p < .1; **p < .05; ***p < .01

Natural disaster occurrences as count per disaster family in the year prior to the election. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the gross domestic product per capita. All monetary variables are in million real 2011USD at chained PPPs. All varying country intercepts were omitted.

Table 5.4: Base Models (Disasters in the previous electoral period; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
Incumbent Vote Share	0.401*** (0.028)	0.403*** (0.028)	0.450*** (0.028)
Rapid Onset Disaster	-0.399*** (0.139)		
Slow Onset Disaster	0.713*** (0.266)		
Climatological Disaster		0.744*** (0.271)	0.702*** (0.254)
Geophysical Disaster		-0.482 (0.509)	-0.767 (0.476)
Hydrological Disaster		-0.356** (0.162)	-0.311** (0.151)
Meteorological Disaster		-0.524 (0.349)	-0.680** (0.328)
GDP pc change	0.054 (0.054)	0.055 (0.054)	-0.185*** (0.065)
GDP pc	-2.096* (1.173)	-2.042* (1.179)	-4.082*** (1.399)
World Economic Crisis dummy	2.785*** (0.703)	2.807*** (0.707)	1.951*** (0.709)
OECD Humanitarian Aid	0.786*** (0.241)	0.786*** (0.241)	0.548** (0.245)
Unemployment Rate			-1.078*** (0.153)
Constant	35.194*** (10.562)	34.660*** (10.612)	61.166*** (12.655)
N	1,176	1,176	1,045
Log Likelihood	-4,469.173	-4,468.655	-3,900.486
AIC	8,960.345	8,963.310	7,828.972
BIC	9,016.114	9,029.218	7,898.297

*p < .1; **p < .05; ***p < .01

Natural disaster occurrences as count per disaster family during the last electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the gross domestic product per capita. All monetary variables are in million real 2011USD at chained PPPs. All varying country and constituency intercepts were omitted.

of geophysical, hydrological, and meteorological natural disasters seems dependent on the sample. Slow onset and climatological natural disasters showed statistical significance (yet with an unexpected sign) only in the constituency-level data set with natural disaster counts per electoral period. This yields little general evidence to support hypothesis 1; the results are volatile and depend on model specification and the sample.¹⁵

Overall, the economic control variables did not perform as expected, often not reaching statistical significance or having unexpected signs. This is surprising given the amount of evidence for these relations in the economic voting literature. While the unemployment rate, change in GDP, and level of GDP were included based on the literature review yet solely as controls to decrease potential omitted variable bias, the effect of international humanitarian aid is of substantive interest to this study. Note, however, that hypothesis 3 suggests varying electoral responses to international humanitarian aid based on the electorates' expectation of patronage. Thus, we cannot make inference on this measure until this intervening factor is included in the models below.

This section serves as a starting point to make this study comparable to others in this field of research and the results so far suggest two general interpretations. Already we see that replicating the same substantive results from similar studies is difficult: the statistical significance of the measures is highly dependent upon the sample. This can be evidence of (a) that electorates react in different ways to natural disasters and thus, punish (or reward) incumbents at differing degrees, or (b) that the hypothesized direct link between natural

15. On a side note, Slettebak (2012) finds that binary natural disaster measures yield statistical significance easier than count measures. This was replicated (not shown here) for binary indicators of all natural disaster families for one-year lags as well as the electoral period: rapid onset natural disasters continue to be the only indicator with consistent (negative) electoral impact. I have chosen not to present these results here because there is no theoretical reason for this operationalization.

disasters and incumbent electoral performance is flawed. The first interpretation would yield inconsistent results because some electorates might punish incumbents for natural disasters while others reward or act indifferently. The differing reactions would create contradicting tendencies in the data and thus, produce inconsistent results.

The second interpretation would mean that there is no general direct effect of natural disasters on the electoral fate of incumbents. Statistical results which suggest otherwise would then be the result of sample bias and/or by omitting crucial factors which determine which natural disasters leave electorates indifferent and which ones solicit a reward or punishment reaction. As mentioned in the literature review above, such intervening factors could be the media, quality of government response, an electorate's experience with previous natural disasters, and expectations of patronage. The latter two shall be discussed in the analyses below. Either way, the second interpretation would suggest that scholars might have to take inconsistent results as above or null findings such as in Remmer (2014) more seriously: Could this field of research be based on statistical artifacts which have lead scholars to believe in a direct effect of natural disasters on incumbents' electoral performance (as in hypothesis 1 of this study, and as opposed to a conditional effect as in hypothesis 2)? According to Google Scholar, the different versions of articles by Achen and Bartels (2002, 2013) which inspired this field of research, and certainly this study, have received roughly 500 citations at the writing of this study. Most of these citations are used to establish either a general direct electoral effect of natural disasters, or the myopia of electorates. Notwithstanding the excellent quality of the work of Achen and Bartels (2002, 2013), the general effect of natural disasters on incumbents' electoral fate which is often attributed to the results of their work

might have to be questioned.

Intervening Factors: Patronage Expectations

One of the main goals of this study is to determine whether the previous findings of electorates punishing incumbents for natural disasters can be generalized beyond the respective studied samples. As such, this study aims to bridge across the various single-country studies and fill the lack of empirical cross-country cross-time time studies in this field of research (c.f. table 2.1). So far, I have found little support for a direct electoral effect of natural disasters, or better, little to suggest the rejection of the respective null hypothesis. I argued that either there is no causal link (or even correlation) or the effect is conditioned by intervening factors. Obviously, the importance of unit-specific effects and intervening factors increases as one combines multiple cross-sectional units in one data set. Both present a practical problem by introducing omitted variable bias when not included in the model. Theoretically, the hypotheses specified above already suggest differing electoral effects of natural disasters by country and maybe even by election. For example, Germany's Helmut Schmidt and Gerhard Schroeder drew electoral benefits out of natural disasters while Angela Merkel was not able to cash in on a natural disaster which happened during her tenure. In other words, both theory and practical considerations suggest that contradictory tendencies in the data will produce inconsistent statistical results until important characteristics of the samples are included in the models.

This section further explores the electoral impact of natural disasters, now introducing the intervening effects of patronage expectations and electorates' experience with natural

disasters. I argue that there is no direct causal link from natural disasters to incumbents' electoral performance. Instead, I argue that there might be a causal link from natural disasters into a black box which determines whether this natural disasters is going to have an electoral impact in the respective country. Other scholars have already implied this black box by suggesting that, for example, media coverage was one of the intervening factors (c.f. Littlefield and Quenette (2007) and Lazarev et al. (2014) and others cited in the literature review). This section adds to this discussion by positing that an electorate's expectation of patronage and experience with previous natural disasters are part of the black box. Experienced societies are expected to generally behave indifferently, neither punishing nor rewarding incumbents for natural disasters. Inexperienced societies are expected to show either punishment or reward. Electorates expecting patronage in return for votes are expected to generally reward for natural disasters as they give incumbents the chance to come in as 'the knight in shining armor'. Non-clientalistic electorates are expected to generally punish for natural disasters.

The models in this section add interaction effects to the hierarchical model design above. The models below include varying intercepts by country and constituency (where appropriate) in order to account for characteristics specific to each cross-sectional unit at the respective level of analysis. The varying intercepts account for cross-sectional dependence of observations within cross-sectional groups. Interaction effects enter the model in addition to the respective interacted variables' individual base effects. That means, for example, the estimation determines a coefficient for both natural disasters and patronism as well as a third coefficient for the product of these two variables. If both the base effect and the interactive

effect of a natural disaster variable were statistically significant, the results would suggest a direct effect and a conditioned effect of the respective explaining variable on the dependent variable. If only the interaction term was statistically significant one could interpret this as evidence towards accepting the null hypothesis for the base effect (i.e. no direct effect as e.g. in hypothesis 1) and rejecting the null hypothesis of a conditioned effect (as e.g. in hypothesis 2). The models to be estimated in this section shall be specified as follows and differing only in the inclusion of the second level of analysis ($\alpha_{i[n]}$) in the constituency-level analysis and the exclusion thereof in the national-level analysis:

$$pvs_{i,j,t[n]} = \mu + \alpha_{i[n]} + \gamma_{j[n]} + \beta_1 * \mathbf{X}_{i,j,t[n]} + \beta_2 * \mathbf{Z}_{j,t[n]} + \beta_3 * X_{i,j,t[n]} * Z_{j,t[n]} + e_{i,j,t} \quad (5.3)$$

and

$$\alpha_{i[n]} \sim N(\mu_\alpha + \delta * W_{i[n]}, \sigma_\alpha^2) \quad or \quad \alpha_{i[n]} = \mu_\alpha + \delta * W_{i[n]} + \eta_i \quad (5.4)$$

$$\gamma_{j[n]} \sim N(\mu_\gamma + \Omega * Q_{j[n]}, \sigma_\gamma^2) \quad or \quad \gamma_{j[n]} = \mu_\gamma + \Omega * Q_{j[n]} + \omega_j \quad (5.5)$$

with

$$e_{i,j,t} \sim N(0, \sigma_{pvs}^2) \quad for \quad n = 1, \dots, N \quad (5.6)$$

$$\eta_i \sim N(0, \sigma_\alpha^2) \quad for \quad i = 1, \dots, I \quad (5.7)$$

$$\omega_j \sim N(0, \sigma_\gamma^2) \quad for \quad j = 1, \dots, J \quad (5.8)$$

where $pvs_{i,j,t[n]}$ is the dependent variable (here, incumbent party vote share) with n

observations which are grouped by constituency i with $i = 1, \dots, I$, country j with $j = 1, \dots, J$, and election at time t with $t = 1, \dots, T$. $\alpha_{i[n]}$ is a varying intercept for each constituency i , and $\gamma_{j[n]}$ is a varying intercept for each country j . These varying group intercepts are themselves functions of constant intercepts μ_α and μ_γ respectively, as well as time-invariant group specific indicators $\delta * W_{i[n]}$ and $\Omega * Q_{j[n]}$ respectively, where δ and Ω are coefficients and W and Q can be vectors or matrices of time-invariant group specific covariates. ($\delta * W_{i[n]}$ and $\Omega * Q_{j[n]}$ are automatically omitted if no time-invariant group specific indicators are included in the model.)

β_1 and β_2 are vectors of regression coefficients for matrices of the level 1 and level 2 time-variant covariates $\mathbf{X}_{i,j,t[n]}$ and $\mathbf{Z}_{j,t[n]}$ respectively. The bold symbols indicate matrices. β_3 is the coefficient of an interaction term between one level 1 predictor and one level 2 predictor (e.g. the occurrence of a natural disaster in a given constituency interacted with the electorate's national aggregate expectation of patronage). μ is a constant and $\epsilon_{i,j,t}$ is a random error distributed normally with a mean of 0 and variance σ_{pvs}^2 .

Table 5.5 presents regression results of models which follow the structure of equation 5.3 using the national-level data. The national-level data set does not include information for any constituency-level intercepts α_i and interactions are between two national-level indicators. The models were estimated via REML and all varying intercepts were omitted from the output. The linkage variable (based on VDEM's *v2psprlnks* variable as per the chapter on concepts & measurements) is operationalized as a dummy coded 1 for clientalistic societies (i.e. where the linkage variable *v2psprlnks* > 3) with a mean of 0.15, instead of using the operationalization with 5 levels in order to decrease the amount of varying intercepts

Table 5.5: Natural Disasters and Patronage Expectations (Disasters at $t - 1$; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.588*** (0.043)	0.643*** (0.044)	0.424*** (0.134)
rgdpe_pc_change_lag	0.221*** (0.085)	0.234*** (0.088)	0.116 (0.267)
OECDhumaid_lag	0.776* (0.466)	0.712* (0.422)	-0.433 (0.941)
rapid_occurrence	-0.029 (0.158)	-0.006 (0.143)	-0.280 (0.507)
slow_occurrence	-0.889 (0.731)	-1.029 (0.669)	0.784 (3.274)
linkage1	-0.044 (2.558)		
OECDhumaid_lag:linkage1	-1.209 (0.800)		
rapid_occurrence:linkage1	-0.153 (0.376)		
slow_occurrence:linkage1	2.252 (2.368)		
N	584	497	87
Log Likelihood	-2,218.866	-1,842.916	-358.988
AIC	4,463.732	3,703.832	735.977
BIC	4,520.541	3,741.710	758.170

*p < .1; **p < .05; ***p < .01

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the national-level data set. Natural disaster occurrences as count during the last year. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries and electoral periods) were omitted from the result table.

per interaction group and increase the number of observations per group. Overall, the linkage variable has many missing values, reducing the number of complete observations in the national-level data to 584 for 111 countries. Furthermore, the level of GDP and the crisis dummy were not included in any of the following models because they failed to achieve statistical significance when included (not shown here) and to decrease the number of estimated parameters.

Models (1) through (3) in table 5.5 present the interacted effect of rapid and slow onset natural disasters with patronage expectations in order to test hypothesis 2 at the national level. Hypothesis 2 expects contradictory effects of natural disasters in clientalistic and non-clientalistic societies. Model (1) uses an interaction approach while models (2) and (3) use a split sample approach. As the amount of theoretically motivated interactions terms per model increases, the output is increasingly difficult to read and concerns about the number of estimated parameters could be raised. This is especially so when four families of disasters (and their interaction effects) are included as opposed to the two families (rapid and slow onset natural disasters) in table 5.5. Thus, models (2) and (3) were added to the table in order to reduce the number of estimated parameters per model and provide an alternative specification. Models (2) and (3) use a split sample approach as opposed to interacting variables. Model (2) uses only non-clientalistic observations (i.e. where the linkage variable $v2psprlnks \leq 3$) and model (3) uses only clientalistic observations. Note, however, the clientalistic sample in model (3) has only 87 observations yet attempts to estimate the full set of varying intercepts for the 35 country groups in this sub-sample. The section on model diagnostics will further discuss model fit and practical considerations of including the

interaction effects. For now, we are interested in the results of the natural disaster and linkage measures.

Overall, there is little evidence towards hypothesis 2 which expected that clientalistic societies would generally reward incumbents for natural disasters while non-clientalistic societies were expected to generally either act indifferently or punish incumbents. None of the coefficients for natural disaster measures (base and conditioned effects) in table 5.5 reached statistical significance. Conditioned on the model specification, this sample does not warrant the rejection of the null hypotheses of no effect. In other words, table 5.5 does not find a systematic relationship between natural disasters and incumbent electoral performance, at least not as specified.

The tables in Appendix present variations of table 5.5. Table 6.1 in Appendix counts rapid and slow onset natural disasters in the previous electoral period. Both model (1; interaction approach) and models (2) and (3; split sample approach) point in the same general direction: we cannot reject the null hypothesis of no direct (i.e. base or unconditioned) effect while this particular model specification may allow the rejection of the null hypothesis of no conditioned effect (for the clientalistic sample and only in the split sample approach). Tables 6.2 and 6.3 use the natural disaster operationalization with four families (climatological, geophysical, hydrological, and meteorological) in the previous year and during the last electoral period respectively. Similar substantive results hold: the sample does not warrant the rejection of the null hypothesis of no direct effects. In the interacted models, the occurrence of hydrological natural disasters (which are types of rapid onset natural disasters) is expected to be observed with decreased incumbent party vote share in the next election in

clientalistic societies of this sample. Geophysical natural disaster coefficients achieve statistical significance in some specifications, sometimes as a negative base effect and other times as a negative effect only for clientalistic observations. Table 6.2 even finds a statistically significant positive effect for meteorological natural disasters for clientalistic observations.

Table 5.6 presents regression results of models which follow the structure of equation 5.3 using the constituency-level data. Table 5.6 differs from table 5.5 only in the data used to estimate the models and the inclusion of varying intercepts by constituency. Models (1) through (3) present the interacted effect of rapid and slow onset natural disasters with patronage expectations in order to test hypothesis 2 at the constituency level. As previously, the linkage variable is a dummy (coded 1 for $v2psprlnks > 3$) with a mean of 0.45 and all varying intercepts for countries, constituencies, and electoral periods are omitted from the result table.

The results based on the constituency-level data paint a similar picture about the general influences of natural disasters on incumbents' electoral performance. Table 5.6 shows substantively similar results across model specifications: this sample also suggests no systematic direct effect of natural disasters (the null hypothesis cannot be rejected) while it does suggest the rejection of the null hypothesis for clientalistic observations. Specifically, we see in model (1) that the base effects of natural disasters do not achieve statistical significance, i.e. the sample does not suggest an unconditioned baseline effect of natural disasters (c.f. hypothesis 1). Only the interaction terms for rapid onset natural disasters are significant (c.f. hypothesis 2). In models (2) and (3), the disaster variables are only significant in the clientalistic sample with the same signs as in model (1). In other words, according to models

Table 5.6: Natural Disasters and Patronage Expectations (Disasters at $t - 1$; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.412*** (0.028)	0.519*** (0.039)	0.283*** (0.039)
rgdpe_pc_change_lag	0.086 (0.055)	0.166 (0.104)	0.075 (0.066)
OECDhumaid_lag	1.332*** (0.284)	1.257*** (0.301)	-1.572*** (0.575)
rapid_occurrence	-0.209 (0.342)	-0.152 (0.340)	-2.199*** (0.809)
slow_occurrence	0.478 (1.148)	0.572 (1.140)	-1.482 (4.788)
linkage1	-8.254*** (2.665)		
OECDhumaid_lag:linkage1	-1.173** (0.516)		
rapid_occurrence:linkage1	-2.085** (0.919)		
slow_occurrence:linkage1	-1.347 (5.233)		
N	1,176	649	527
Log Likelihood	-4,429.390	-2,445.639	-1,951.802
AIC	8,886.781	4,911.278	3,923.604
BIC	8,957.759	4,956.033	3,966.276

*p < .1; **p < .05; ***p < .01

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the constituency-level data set. Natural disaster occurrences as count during the last year. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries, constituencies, and electoral periods) were omitted from the result table.

(1) through (3) we would expect to observe the occurrence of rapid onset natural disasters to be followed by a decrease in incumbent party vote share in the following national lower house election in clientalistic societies. Tables 6.4 through 6.6 in Appendix generally confirm these tendencies. Various interaction effects and their corresponding coefficients in the clientalistic sample achieve statistical significance while the interactions' base effects remain largely insignificant.

In sum, there is some evidence for differing electoral impacts of natural disaster occurrences on the vote share of incumbent parties in the national lower house. The goal of this section was to expand our expectations about how electorates react to natural disaster occurrences in their voting behavior towards the incumbent party of the national lower house. Motivated by theory, I proposed that there might not be a direct link between natural disasters and incumbent electoral performance, instead I proposed patronage expectations as one possible intervening factor which decides the electoral impact of natural disasters. Hypothesis 2 posits that natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies. Contradictory effects conditioned on patronage expectations would make a baseline reward or punishment effect of natural disasters unlikely. Paired with the results of the base models in section , I thus entered this section with the expectation of only finding statistically significant interaction effects (as well as the respective effects in the clientalistic sample) while expecting the interactions' baseline effects to remain inconclusive.

My expectation of insignificant baseline effects for the interaction effects was confirmed.

The sample did not show a direct effect of natural disasters on the vote share of incumbent parties of the national lower house. Conditioned on the model specification, the sample cannot reject the null hypothesis linked to hypothesis 1. Note again, the statistical results above are far from conclusive. In fact, the results are somewhat volatile as the coefficients of different natural disaster families achieve statistical significance based on the sample and operationalization of said natural disaster variables. Therefore, I would be careful to derive a *general* conclusion of there being no direct effect of natural disasters on incumbents' electoral performance.

Furthermore, the statistical results seem to point in the general direction of expecting lower incumbent party vote share in national lower house elections following natural disaster occurrences in clientalistic societies. Across both the national and constituency-level data we see statistically significant negative coefficients for clientalistic societies more often than not. Different coefficients point towards the rejection of the respective null hypotheses throughout the different operationalizations of the natural disaster variables. This follows hypothesis 2 with respect to different effects of natural disasters based on patronage expectations (even though I find opposed signs on the coefficients). The factor of patronage expectations is far being from established as one of the workings within the black box between natural disasters and incumbent electoral performance. Nevertheless, this study yields at least a promising first analysis despite the volatility of the results across samples and model specifications.

Intervening Factors: Humanitarian Aid

This section refers back to tables 5.5 and 5.6 above but focusses on the humanitarian aid variables. Thus far, the discussion of the results was silent on any but the natural disaster variables and their interaction with patronage expectations; it would have overladed the section above. This section is concerned with hypothesis 3 which posits that incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. Recall, humanitarian aid is measured in gross disbursements in million real 2011USdollars per capita by OECD donors (including ODA Loans, ODA grants, and other Official Flows (non Export Credit)). Note that the lag structure of the aid variable follows the lag structure of the natural disaster variables: when the disaster variables are lagged by one year, so is humanitarian aid. And when the disaster variables count occurrences per electoral period, then humanitarian aid is summed per electoral period.

The national-level sample suggests a positive base effect of humanitarian aid received by OECD donors on incumbent party vote share in the following national lower house election. Table 5.5 and tables 6.1 through 6.3 in Appendix show statistically significant positive base effects of OECD humanitarian aid in models (1) and (2). In other words, the national sample suggests the rejection of the null hypothesis of no base effect across model specifications. The null hypothesis for no conditional effects cannot be rejected.

The constituency-level data also shows positive base effects of humanitarian aid and further rejects the null hypotheses of no conditional effects. Table 5.6 and tables 6.4 through 6.6 in Appendix show statistically significant base effects of OECD humanitarian aid in all

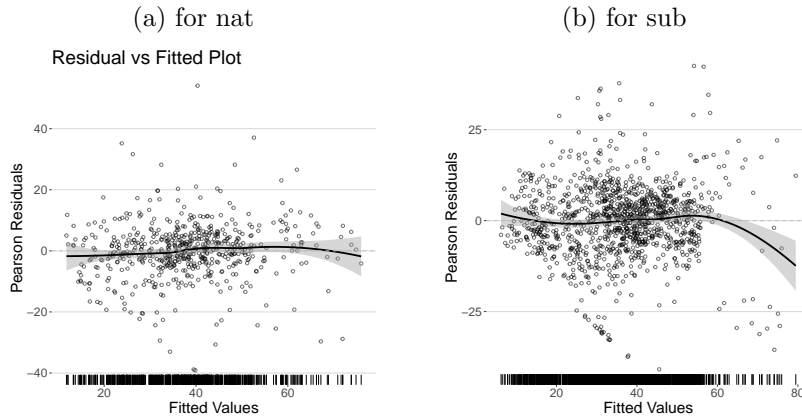
models. Models (1) and (2) in each table show positive base effects while the base effect in the clientalistic sample is positive twice. The interaction effects of humanitarian aid and patronage expectations are statistically significant and negative in all model specifications for the constituency-level data. In other words, the constituency-level sample suggests a positive base effect of international humanitarian aid which is offset by a negative effect in clientalistic societies.

In sum, this section was concerned with hypothesis 3 which posits that incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. The samples, however, suggest that international humanitarian aid flows are generally observed with increased incumbent party vote share yet the effect is offset by negative effects to negligible levels in clientalistic societies. That means, the net effect of international humanitarian aid is not substantively different from zero in clientalistic societies. The results suggest two interesting insights: (a) the (positive) signs of the base effects do not follow (negative) expectations and (b) the clientalistic sample (models (3) in the tables above) has positive effects for aid in two out of three models which is counterintuitive to the theory which lead to hypothesis 3.

Model Diagnostics

This section presents model diagnostics. The goal is to assess model fit and the models' underlying assumptions before moving on to the substantive discussion of the statistical results presented above. I present (a) residual versus fitted plots to assess the linear model specification and variance of the error term, (b) quantile-quantile plots to assess the assump-

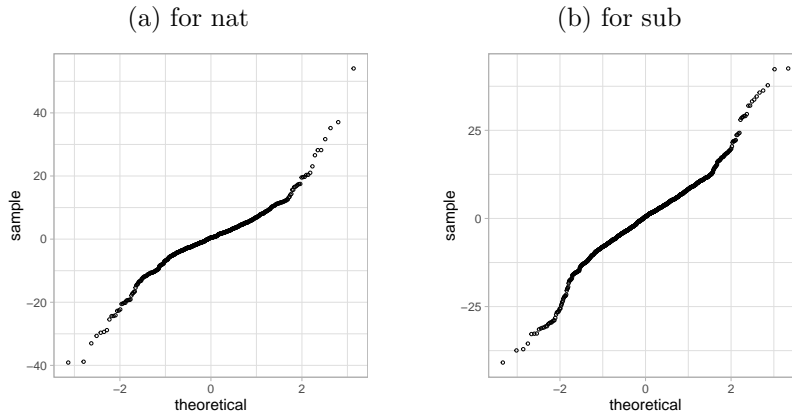
Figure 5.1: Residuals vs. Fitted PLots



tion of normally distributed errors, (c) residual versus leverage plots to identify outliers, and (d) a brief discussion of the inclusion of interactions terms and the impact of the multiple testing problem. All plots are presented for the national-level and constituency-level data as analyzed in tables 5.5 and 5.6 respectively.

Figure 5.1 plots Pearson residuals against the fitted values for the vote share of the incumbent party in the national lower house both for the national and constituency-level data. First, we see that the residuals are dispersed seemingly randomly around the zero line which suggests that the linear model specification is reasonable. The Loess smoother with its 95% confidence band confirms this, following the zero line closely. Second, the residuals hug the zero line tightly, forming a horizontal band, which suggests that the variance of the error term is constant (recall $e_{i,j,t} \sim N(0, \sigma_{pvs}^2)$ in equation 5.3). The slight decrease in residual variance on the left side of the graph and the increase of residual variance on the right are probably due to there being fewer observations in these areas of the graph (compare

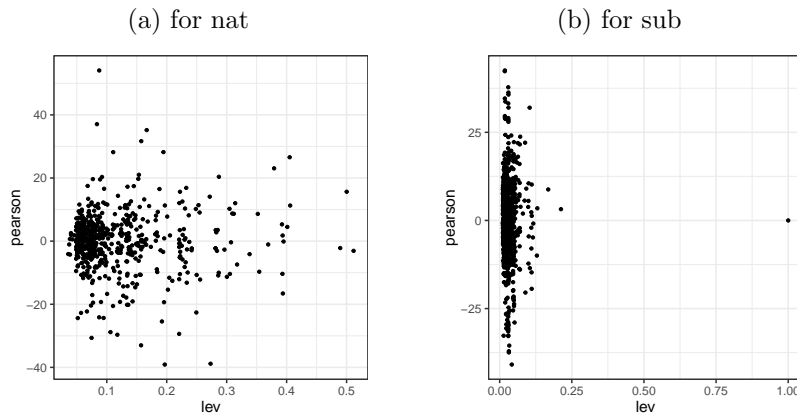
Figure 5.2: Normal Q-Q Plots



the amount of observations with the rug laid on the x-axis). Third, no single residual stands out as to suggest an outlier. The high (in absolute terms) residuals in the middle of the graphs are probably due to sampling variance and few in numbers compared to the amount of residuals tightly hugging the zero line.

The quantile-quantile (Q-Q) plots in figure 5.2 for the models in tables 5.5 and 5.6 respectively suggest that the assumption of normally distributed errors is plausible. A Q-Q plot plots quantiles in order to compares two probability distributions. On the y-axis is the distribution of the sampled standardized residuals and on the theoretically expected quantiles of a normal distribution are on the x-axis. Ideally, one would find the points to form a line of constant slope. In figure 5.2 both distributions match closely in the middle of the graph and divert on the extremes where there are only few observations. While Q-Q plots are easy and quick visualizations to check distributional assumptions, they can just as easily be overinterpreted. I would argue that the deviations from the normal distribution

Figure 5.3: Residual versus Leverage Plots



(even at the ends of the curve) are not severe enough to warrant rejection of the assumption of normally distributed errors. Possibly, the deviations are due to sampling variance and a small sample instead.

The residuals versus leverage plots in figure 5.2 for the models in tables 5.5 and 5.6 respectively suggest that there are no outliers driving the regression results. A residuals versus leverage plot plots the residuals against their leverage scores. The leverage score indicates how much one single observation will pull the regression in its direction. Single observations can possibly have huge impacts on the regression line and 'drive' the results. That means, the results would be different if such observations were excluded. One would look for observations in the lower and upper right hand corners of the residual versus leverage plots in order to identify important outliers. An *outlier* is thus any observation whose value in the dependent variable does not follow the general trend of the data. This is not necessarily problematic. An *influential* outlier is an observation with either extreme values or unusual

combinations in the explaining variables which has a high impact on the resulting regression coefficients, predicted responses, and hypothesis tests.

As per figure 5.2 there are quite a few observations with strong leverage in the national-level data, yet for almost every high positive residual there is a low negative residual pulling the regression line in the opposite direction. Neither the top nor lower right hand corner has a residual which can be expected to have unduly pull on the regression line. Indeed, when the model from table 5.5 is reestimated without any of the observations with leverage above 0.3 (not shown here) the regression coefficients hardly change (they change by 0.006 to 0.148 times their standard errors). Thus, I conclude that the results are not driven by influential outliers.

The right plot in figure 5.2 shows the residual versus leverage plot for the constituency-level data. The lonely point to the very right is the 2005 observation for the Francisco Morazán department in Honduras. Honduras is actually a 'very average' observation, i.e. its values in the explaining variables fall closely to the overall mean of those variables, or at least within the 25% and 75% quantiles, for all but one variable. This observation experienced 2 slow onset natural disasters while all other Honduran departments experienced zero natural disasters in that year. The observation may be influential but its position on the regression line does not make it an outlier. In fact, when the model from table 5.6 is estimated without this observation, the regression coefficients do not change noticeably. Thus, I decided to keep the observation in the sample.

Finally, a note on the interaction terms seems appropriate. Any addition of terms to a model specification should be motivated through theory (just like the rest of the broader

research design). The models presented so far include a fair amount of interaction variables which were motivated by theory. As long as their inclusion is motivated by theory, the potential benefit of insight through their inclusion should outweigh the potential issues of adding parameters to be estimated. This, however, assumes that the sample also generally warrants the additional inclusions. Practically, the interaction may pose a problem by reducing the degrees of freedom and increasing the multiple comparison problem. In fact, increasing the number of hypothesis tests m in a regression from 6 to 10 (by e.g. adding 4 interaction terms of the 4 natural disaster families with the linkage variable) increases the chance of falsely rejecting *at least one* null hypothesis (Type I error) from $1 - (1 - \alpha)^{m=6} = 0.47$ or 47% to $1 - (1 - \alpha)^{m=10} = 0.65$ or 65% with $\alpha = 0.1$ as the target significance level.

Beyond the theoretical motivation for the added parameters, I try to increase confidence in the results above by supplying the split-sample approach, providing different operationalizations of the variables of interest, running the models on two data sets, and calculating incremental (type II) F-tests for model fit with the interactions. The split-sample calculations on the two data sets are provided with the interaction models in tables 5.5 and 5.6 and in tables 6.4 through 6.6 in Appendix . The interaction terms achieved statistical significance, yet not consistently across model parameterization, samples, and approaches. The incremental F-tests (not shown here) also suggest that the models generally fit better when the interaction terms are included. Ergo, thus far the samples do not seem to reject the inclusion of the interaction terms. I have elected to not make Bonferroni adjustments because they are generally too conservative (Green and Britten 1998). Bonferroni and related adjustments would make rejection of null hypotheses in most social science applications rather

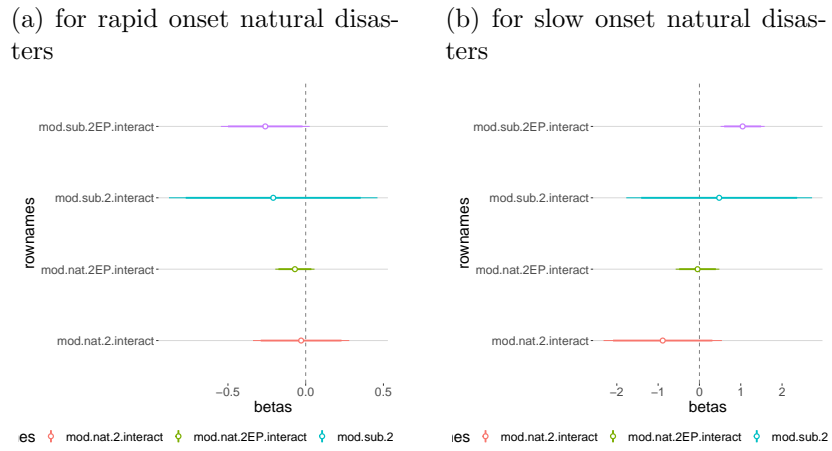
unlikely and even then, they also rely on p -values which usually do not account for model selection uncertainty or sampling which might grossly understate total variability. Hence, I will leave inference to careful interpretation of the statistical results and validation through replication. Incidentally, the main goal of this study is to replicate the findings of previous studies.

In sum, this section evaluated model fit and underlying assumptions, concluding that no grave violations were apparent and deviations from theoretical expectations may likely be the result of sampling variance. In order to assess model fit and the models' underlying assumptions, this section presents (a) residual versus fitted plots to assess the linear model specification and variance of the error term, (b) quantile-quantile plots to assess the assumption of normally distributed errors, (c) residual versus leverage plots to identify outliers, and (d) a brief discussion of the inclusion of interactions terms and the impact of the multiple testing problem. All plots are presented for the national-level and constituency-level data as analyzed in tables 5.5 and 5.6 respectively. The residual versus fitted and quantile-quantile plots did not suggest any concerns. The residuals versus leverage plots did not suggest the exclusion of any influential outliers. The discussion of the interaction variables concluded that they are not only theoretically motivated but that the sample does not suggest their exclusion either.

Discussion

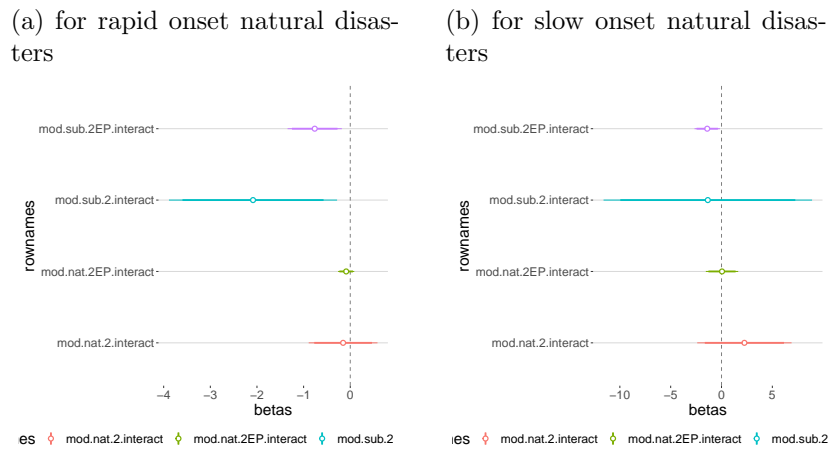
This section expands on the discussion of the statistical results presented above by interpreting the different models simultaneously and rectifying shortcomings of the approach

Figure 5.4: Plot of the Rapid and Slow Onset Coefficients (Base Effects)



(a) Coefficients for rapid onset natural disasters' base effects. (b) Coefficients for slow onset natural disasters' base effects.

Figure 5.5: Plot of the Rapid and Slow Onset Coefficients (Interaction Effects)



(a) Coefficients for rapid onset natural disasters' interaction effects with the patronage dummy. (b) Coefficients for slow onset natural disasters' interaction effects with the patronage dummy.

take above. The discussion of the statistical results so far is incomplete because it noted only whether coefficients reached statistical significance and the samples allowed the rejection of the null hypotheses conditioned on the model specifications. Viewed individually, the results above may seem to point in the general direction of expecting lower incumbent party vote share in national lower house elections following natural disaster occurrences in clientalistic societies. Across both the national and constituency-level data we see statistically significant negative coefficients at least for clientalistic societies more often than not. Similarly, concerning the humanitarian aid measures, the samples may seem to suggest that international humanitarian aid flows from OECD donors are generally observed with increased incumbent party vote share yet the effect is offset by negative effects to negligible levels in clientalistic societies. Note again, however, the statistical results above are far from conclusive. In fact, the results are somewhat volatile as the coefficients of different natural disaster families achieve statistical significance based on the sample and operationalization of said natural disaster variables. Therefore, thus far I would be careful to derive any *general* conclusion about the studied effects.

The discussion of the result tables above categorized individual coefficients as either statistically significant or insignificant in order to provide a careful interpretation. The categorization was dependent upon individual p-values being equal or below an arbitrary yet generally accepted threshold of $\alpha = 0.1$. Publications in the Social Sciences increasingly consider a statistical significance level of $\alpha = 0.1$ instead of $\alpha = 0.05$. The p-value itself denotes only the chance of observing the sample if the null hypothesis was true. Thus, a p-value of 0.1 would suggest that the sample would be observed in 10% of studies due to

random chance. The p-value itself does not indicate whether the null hypothesis is either false or true with the sample being unusual. Determination of this is left to the researcher's insights. One can never accept the alternative hypothesis, only reject or accept the null hypothesis. Above, I have interpreted low p-values to reject null hypotheses because of my confidence in the sample which consists of many elections in different countries at different times. In other words, I hope the scope of the data collection made this a 'usual' sample. The dichotomization into significant and not significant served the goal of interpreting the statistical results carefully in order to allow a clear distinction between the statistical results and my subjective interpretation of what they mean for the hypotheses.

The dichotomization into significant and not significant, however, raises concerns. Gelman and Stern (2006, 328) write "that statistical significance is not the same as practical importance, that dichotomization into significant and nonsignificant results encourages the dismissal of observed differences in favor of the usually less interesting null hypothesis of no difference, and that any particular threshold for declaring significance is arbitrary." In addition, "changes in statistical significance are often not themselves statistically significant" (328). This section attempts to ratify these concerns by providing coefficient plots which display groups of similar coefficients for a more educated interpretation of the result tables above.

The following coefficient plots group similar coefficients from the different result tables in order to paint a more nuanced picture of the results and what theoretical deductions they may allow for my hypotheses. Each coefficient plot marks estimated regression coefficients as hollow points on a continuous x-axis which includes zero. Further, each coefficients' 90%

and 95% confidence interval is indicated by thin and fat lines respectively emanating from the points. A vertical zero-line helps to identify whether the confidence intervals exclude zero and thus, whether the effect is substantial (or, of practical importance). The range of the x-axis depends on the groups' coefficients and their standard errors. The model names on the y-axis remain the same across figures as the coefficient plots display different coefficients from a common set of models.

Figure 5.4 is a coefficient plot of the base effects of slow and rapid onset natural disasters for the models in the tables above and in Appendix . Coefficients are represented with hollow points and confidence intervals. Figure 5.4(a) includes four rapid onset base effects from four different models, two sets of models counting natural disaster occurrences in the last year and in the last electoral period and estimated for the national and constituency-level data. The graph shows that all estimated rapid onset base coefficients lie between -0.03 and -0.26. While they have the expected sign according to hypothesis 1 which itself derives its expectation from the empirical results of previous studies, the effect size is not substantially different from zero. The dependent variable, incumbent party vote share, is on a scale from 0 to 100, hence one would expect electorates to punish incumbents by a maximum of -0.26% points (\pm standard errors) in lost vote share per rapid onset natural disaster occurrence if hypothesis 1 and the coefficients were true. In other words, the effect size would be too small to be of interest even if the sample could confidently reject the null hypothesis.

Figure 5.4(b) includes the four slow onset base effects from the four different models. The estimated slow onset base coefficients lie between 1.05 and -0.89 with standard errors big enough to expect the true coefficient of three out of four models in either the positive or

negative side. Again, the effect size under a true hypothesis 1 would be rather unimportant. Finally, note that the respective coefficient is statistically significant when the slow onset natural disaster variable is changed from counts in the last year to counts in the last electoral period in the constituency-level data. The coefficient changes from 0.48 (± 1.15) to 1.05 (± 0.27), yet the difference between these coefficients of 0.57 ($\pm \sqrt{1.15^2 + 0.27^2} = 1.18$) is not itself statistically significant. That is to say that I would refrain from making inference about electorates considering one time frame and not the other.

Figure 6.1 then plots the coefficients of rapid and slow onset natural disasters interacted with the patronage dummy. As discussed above, neither interaction term reaches statistical significance in the models for the national-level data and the estimated coefficients lie close to zero. By contrast, the interaction term coefficients in the constituency-level data reached statistical significance and at least the coefficient for when rapid onset natural disasters are counted in the last year may be substantively important; the wide 90% and 95% confidence intervals make this coefficient practically interesting by implying the possibility of a big effect. A similar argument for the potential practical importance of the slow onset interaction term could be made in the constituency-level model with natural disasters counted in the last year (assuming a true alternative hypothesis as in hypothesis ??). Nevertheless, the ratio of coefficients which are either statistically significant or of potential practical importance to the number of coefficients presented in the plots may be telling a story itself.

I conclude for hypotheses 1 and 2 that there is not enough evidence to reject their respective null hypotheses. I have presented different model specifications and different samples, instead of cherrypicking the models which ‘perform best’ by yielding agreeable p-

values which could yield interpretations in favor of my hypotheses. The constituency-level data yields a lot of good p-values but the fact that the interaction terms only hit significance here and not in the national-level data should be of concern. Consider the sample size and variance of the explaining variables. While the constituency-level data has more observations than the national-level one (1176 as opposed to 584), the constituency-level data only includes 17 different countries (as opposed to 84 in the national-level data). On the one hand, the constituency-level data increases my confidence in the results of the natural disaster variables because the hypothesized relationship is observed at a more disaggregated unit of analysis. On the other hand, however, having fewer countries in the data set leads to reduced observed variance of national aggregates such as the patronage variable. Compare the variance of the patronage variable in table 3.6 for the national-level data to the variance in table 3.7 for the constituency-level data. The observed range of integers of the patronage variable is decreased from [1,5] to [2,4]. Therefore, the reduced observed variance of the patronage variable combined with the repeated observation of the national aggregate measure across each country's constituencies may *overstate* the importance of (and the statistical results' confidence in) that patronage variable. In fact, the same is true for any other national aggregate measure.

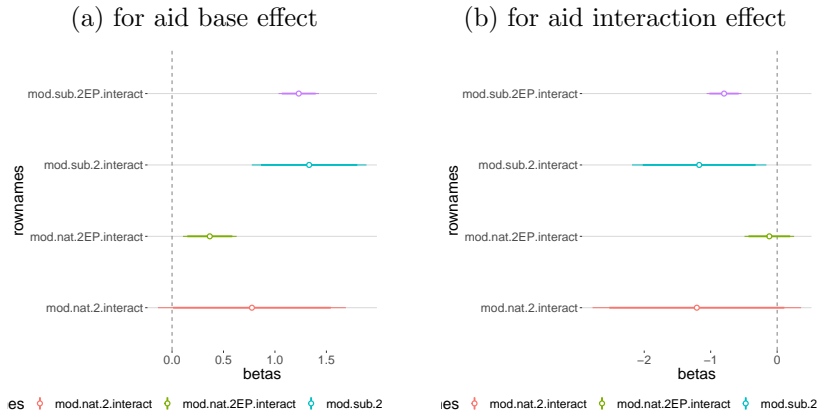
If the reduced variation of the national aggregate measures in the constituency-level data is driving the results, we cannot interpret this as much (if any) evidence towards the importance of patronage expectations. This is why it was so important to divide the statistical interpretation into a step-by-step process focussing first on individual models before combining insights across models specifications and samples. Further, it was important to

separate the discussion of the results from the interpretation so that the reader has the chance to follow my argument and either accept my subjective conclusion (the acceptance of the respective null hypotheses for hypotheses 1 and 2) or derive his/her own conclusion. As described above, statistical results do not prove anything, they merely complement the researcher's subject matter expertise to make an educated guess.

Now let's turn the focus again towards international humanitarian aid which seems to have a generally positive effect on the dependent variable. The discussion of the result tables above yielded the general consensus that the data suggests that the influx of humanitarian aid by OECD donors may on average be observed with increased incumbent party vote share in the next election. The samples seemed to reject the null hypotheses of no effect while the direction of the effect is contrary to the expectations in hypothesis 3. Figure 5.6(a) is a coefficient plot of aid's base effect and figure 5.6(b) presents the coefficients for aid's interactions with the patronage dummy. Again, we see that the base effect is generally positive and statistically significant while it is offset by a smaller and negative effect in clientalistic societies. Thus, OECD humanitarian aid disbursements are found to be observed with increased incumbent party vote share, more so in non-clientalistic electorates than in clientalistic electorates.

Figure 5.7 presents the estimated effect of the interaction of OECD humanitarian aid and patronage expectations on incumbent party vote share clearer by plotting fitted values for the dependent variable against various amounts of OECD humanitarian aid disbursements (in million 2011USD per capita). Figure 5.7 plots the interaction for the constituency-level data and the model using two natural disaster families. The left and right sides of the

Figure 5.6: Plot of the OECD Aid Coefficients (Base & Interaction Effects)

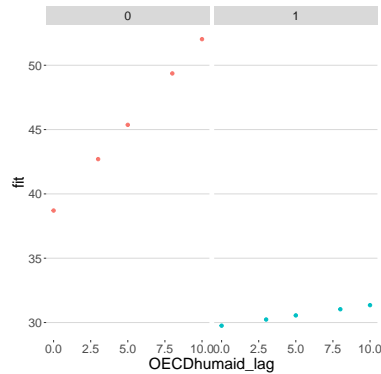


(a) Coefficients for OECD humanitarian aid's base effect on incumbent party vot share. (b) Coefficients for OECD aid's interaction effects with the patronage dummy.

plot present the estimated effects for non-clientalistic societies (linkage=0) and clientalistic societies (linkage=1) respectively. The slopes which are implied by the points suggest that the positive effect of OECD aid is bigger in non-clientalistic societies. In fact, it seems like the offset of the aid effect by patronage expectations is big enough to make the aid effect practically unimportant: expected incumbent party vote share in a clientalistic society raises from 29.76% to 31.35% when OECD humanitarian aid raises from its minimum value of zero to its maximum observed value of 10 million 2011USD per capita.

I conclude for hypothesis 3 that the samples suggest a generally positive effect of OECD humanitarian aid disbursements on incumbent party vote share in the next election. This result is conditioned on the samples and model specifications. Once again the constituency-level data instills more confidence in this conclusion than the national-level sample. Nevertheless, the coefficients plotted in figure 5.6 generally pull in the same direction as opposed to the

Figure 5.7: Interaction Plot of OECD Aid and Patronage Expectations



coefficients for rapid and slow onset natural disasters in figures 5.4 and 6.1.

In sum, this section aimed to expand the discussion of the statistical results presented in the previous sections. The discussion thus far had focussed on a dichotomized interpretation of significant and not significant coefficients. While this interpretation helped to get a first impression of the results, it can distract from practical unimportance and overemphasize arbitrary significance thresholds. Also, the general concerns about model specification, sample selection, and the multiple testing problem still apply. It is evident throughout the various tables and figures that the statistical results are far from conclusive and are dependent on the sample and operationalization of the natural disaster variables. The constituency-level sample consistently produces more agreeable results. One influencing factor for this might be the interaction of constituency-level continuous variable (natural disaster counts) with a national aggregate measure (patronage expectation dummy). The patronage dummy does not change across constituencies for a given year and has small variance across time as well. Thus, confidence of the measure and its related coefficients might be overstated in the models.

This section concluded that the samples cannot reject the null hypotheses of no effects of natural disasters on incumbent party vote shares, while they can reject the null hypothesis of no effect of OECD humanitarian aid disbursements. The effects linked to hypotheses 1 and 2 were judged too volatile to interpret them as to reject the respective null hypotheses. Even under true alternative hypotheses, the estimated coefficients are too small to suggest practical importance of the results. The null hypothesis of no effect for hypothesis ?? might, however, be rejected. Overall, the model specifications across samples point in the general direction of a positive of OECD humanitarian aid on incumbent party vote share in the next national lower house election. The effect differs between clientalistic and non-clientalistic societies: the effect in non-clientalistic societies is sizable while patronage expectations within the electorate seem to decrease the effect into practical unimportance. The following section will discuss these findings and their implications in the light of the broader literature on natural disasters and incumbent electoral performance.

Conclusion

This chapter examined the (multivariate) statistical evidence for the relationship between natural disasters and incumbent party vote share in national lower house elections. Following the theory, I argued that an incumbent's vote share is dependent upon the occurrence of natural disasters, the state of the economy, the respective society's experience with natural hazards and its expectation of patronage, and the incumbent's vote share in the previous election. While most related studies focus on one single country (see table 2.1), this analysis used two original data sets with elections observed in multiple countries over time in

order to deduce generalizable tendencies of natural disasters impacting incumbents' electoral fate. One data set uses the country-election year as the unit of analysis and the second uses country-constituency-election year. The natural disaster variables are populated with two different sources to allow disaggregation to the constituency-level in the second data set. Using two different samples allows to not only test the respective null hypotheses in multiple settings but also to check the robustness of the results across model specifications and samples.

The first sections tested the different hypotheses, established comparability to previous studies, and presented the statistical results. I found replicating the substantive results from similar studies to be difficult: the statistical significance of the measures is highly dependent upon the sample and generally there is little evidence for a direct impact of natural disaster on incumbents' electoral fate as hypothesized in hypothesis 1. Subsequently, I argued that patronage expectations act within the black box between natural disasters and incumbent electoral performance, determining whether a specific natural disaster is going to have an electoral impact in the respective country's next election (see hypothesis 2). Again, the sample could not reject the null hypothesis of no base effect of natural disasters. In addition, if at all, patronage expectations seemed to generally point to the opposite direction than hypothesized. That being said, the coefficients and standard errors were so volatile that I advised caution when drawing any inference from them. Finally, I tested the effect of OECD humanitarian aid on incumbent party vote share in the next national lower house election (see hypothesis 3). The effect differed between clientalistic and non-clientalistic societies: the effect in non-clientalistic societies was sizable and positive while patronage expectations

within the electorate seemed to decrease the effect into practical unimportance. Once more, this result was also counterintuitive given the theoretical reasoning.

The last section was an important addition to the interpretation of the results. The preceding sections relied on a dichotomization into statistically significant and not significant results. However, this method of interpreting statistical results can be problematic as (a) p-values do not actually prove any hypotheses, (b) p-values are based on arbitrary significance thresholds, (c) it distracts from substantive (or practical) importance of coefficients, and (d) the difference between a statistically significant and non-significant coefficient may not itself be significant. The coefficient plots presented in this section served to diversify the method of interpretation. Comparing coefficients and confidence intervals, I made two observations which are especially true for the natural disaster and patronage expectation variables: (i) the statistical significance of the variables of interest were volatile and depended on model specification and the sample, and (ii) the coefficients generally were either too small to be of practical importance (even if they are statistically significant) or the respective confidence intervals were so big that they carry the potential of making the effect practically interesting yet they included zero so that one cannot say what direction the effect will take (in addition to making the coefficient statistically insignificant).

Summary

All models in this chapter are estimated by fitting linear mixed-effects models via restricted maximum likelihood (REML). Estimations vary in specification (a) through the in- or exclusion of interaction terms and varying operationalizations of the natural disaster

variables, and (b) by using two different samples, a national-level and a constituency-level data set. Following the theory, I argue that an incumbent party's vote share in a national lower house election is dependent upon the occurrence of natural disasters, the state of the economy, the respective society's experience with natural hazards and its expectation of patronage, and the incumbent's vote share in the previous election.

Throughout the sections, the economic control variables did not perform as expected by often not reaching statistical significance or having unexpected signs. This is surprising given the amount of evidence for these relations in the economic voting literature. The variables national unemployment rate, level of GDP per capita, and world economic crisis dummy were excluded from all model specifications after the presentation of the base models. These variables were included based on the literature review and solely as controls to decrease potential omitted variable bias. However, they did not seem to improve model fit. Instead, their exclusion freed up degrees of freedom, eliminated the problem of missing data in these variables as well as allowed the usage of more observations to test the null hypothesis of the coefficients of interest, and decreased the multiple testing problem. In other words, I ruled that the benefits of exclusion outweighed the potential benefits of inclusion.

The first section establishes comparability to previous studies by trying to replicate the results of previous studies in this field of research. Previous research, especially single-country studies of the U.S. electorate, had formed the expectation of a negative impact of natural disasters on incumbents' electoral fate. The goal of this section then was to offer validation through replication. However, I found replicating the substantive results from similar studies to be difficult: the statistical significance of the measures is highly

dependent upon the sample and generally there is little evidence for a direct impact of natural disaster on incumbents' electoral fate as hypothesized in hypothesis 1. If at all, rapid onset natural disasters is the only natural disaster family which reaches statistically significant coefficients most of the time. This can be evidence of (a) that electorates react in different ways to natural disasters and thus, punish (or reward) incumbents at differing degrees, or (b) that the hypothesized direct link between natural disasters and incumbent electoral performance is flawed. The first interpretation would yield inconsistent results because some electorates might punish incumbents for natural disasters while others reward or act indifferently. The differing reactions would create contradicting tendencies in the data and thus, produce inconsistent results.

The second section discussed electorates' patronage expectation as a potential factor which could determine which electorates will punish or reward incumbents for natural disasters. I argue that patronage expectations act within the black box between natural disasters and incumbent electoral performance, determining whether a specific natural disaster is going to have an electoral impact in the respective country's next election (c.f. hypothesis 2). Electorates expecting patronage in return for votes are expected to generally reward for natural disasters as they give incumbents the chance to come in as 'the knight in shining armor'. Non-clientalistic electorates are expected to generally punish for natural disasters. If these expectations were correct, the theorized contradictory effects of natural disasters could help to explain why the base models found inconsistent results for the base effect of natural disasters on incumbent party vote share. I concluded that, again, the sample cannot reject the null hypothesis of no base effect of natural disasters (c.f. hypothesis 1). However, several

models point towards different effects of natural disasters based on patronage expectations yet I find opposed signs on the coefficients: according to the various model specifications across the two samples we would expect to observe the occurrence of rapid onset natural disasters to be followed by a decrease in incumbent party vote share in the following national lower house election in clientalistic societies, while there is no such expected effect in non-clientalistic societies. Note though, the results are volatile as the coefficients of different natural disaster families achieve statistical significance based on the sample and operationalization of said natural disaster variables. I do not have a theoretical explanation as to why the different natural disaster family operationalizations would yield different results. In fact, I would have expected rapid onset natural disasters on the one hand and geophysical and hydrological ones on the other hand to yield similarly strong substantive results as they contain the same events. Therefore, I would be careful to derive a *general* conclusion of there being no direct effect of natural disasters on incumbents' electoral performance, yet a conditioned one based on patronage expectations. The factor of patronage expectations is far from established as one of the workings within the black box between natural disasters and incumbent electoral performance. Nevertheless, this study yields at least a promising first analysis despite the volatility of the results across samples and model specifications.

The third section was concerned with hypothesis 3 which posits that incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. The results show (a) the (positive) signs of the base effects do not follow (negative) expectations and (b) the clientalistic sample has positive effects for aid which is counterintuitive to the theory which lead to hypothesis 3. In other words,

the sample suggests that international humanitarian aid flows are generally observed with increased incumbent party vote share yet the effect is offset by negative effects to negligible levels in clientalistic societies. That means, the net effect of international humanitarian aid is not substantively different from zero in clientalistic societies.

The fourth section evaluated model fit and underlying assumptions, concluding that no grave violations were apparent and deviations from theoretical expectations may likely be the result of sampling variance. The residual versus fitted plots suggested that the linear model specification is appropriate, there are no initial concerns about outliers, and the error variance is constant. The quantile-quantile plots did not warrant rejection of the assumption of normally distributed errors. Any deviations at the ends of the curve are possibly due to sampling variance and sample size. The residuals versus leverage plots did not suggest the exclusion of any influential outliers. Finally, the discussion of the interaction variables concluded that they are not only theoretically motivated but that the sample does not suggest their exclusion either.

The fifth section combined the insights from the preceding models by interpreting them together and concluded that there is not enough evidence to reject the respective null hypotheses of hypotheses 1 and 2, while there is evidence to reject the null hypothesis connected to hypothesis 3. Some individual models (discussed in sections one and two of this chapter) suggested that one could expect to observe the occurrence of natural disasters followed by decreased incumbent party vote share in the following national lower house election. However, the coefficient plots presented in this section laid near a different conclusion. The effects linked to hypotheses 1 and 2 were judged too volatile to interpret them as to reject

the respective null hypotheses. Even under true alternative hypotheses, the estimated coefficients are too small to suggest practical importance of the results. The null hypothesis of no effect for hypothesis 3 might, however, be rejected. Overall, the model specifications across samples point in the general direction of a positive effect of OECD humanitarian aid on incumbent party vote share in the next national lower house election. The effect differs between clientalistic and non-clientalistic societies: the effect in non-clientalistic societies is sizable while patronage expectations within the electorate seem to decrease the effect into practical unimportance.

The fifth section was an important addition to the interpretation of the results in sections one through three. The preceding sections had relied on a dichotomization into statistically significant and not significant results. However, this method of interpreting statistical results can be problematic as (a) p-values do not actually prove any hypotheses, (b) p-values are based on arbitrary significance thresholds, (c) it distracts from substantive (or practical) importance of coefficients, and (d) the difference between a statistically significant and non-significant coefficient may not itself be significant. The coefficient plots presented in this section served to diversify the method of interpretation. Comparing coefficients and confidence intervals, I made two observations which are especially true for the natural disaster and patronage expectation variables: (i) the statistical significance of the variables of interest is volatile and depends on model specification and the sample, and (ii) the coefficients generally are either too small to be of practical importance (even if they are statistically significant) or the respective confidence intervals are so big that they carry the potential of making the effect practically interesting yet they include zero so that one cannot say what

direction the effect will take (in addition to making the coefficient statistically insignificant).

Chapter 6

Conclusion

The central question of this study is 'Do electorates punish incumbents for natural disasters?'. This question is often answered in the affirmative, however, the truth may not be so obvious. While the argument of blind, myopic, or irrational voters may be catchy, the empirical evidence is lacking. Indeed, some scholars have been unable to find a generalizable trend across countries and elections (see e.g. Remmer (2014)). I contribute to our understanding of the electoral consequences of natural disasters for incumbent parties by examining the correlational effect of natural disaster occurrences on incumbent party vote shares in subsequent national lower house elections with a bigger scope than comparable studies. I further contribute to the scholarly discussion by providing a more formalized way of thinking about the connection of natural disasters to incumbent electoral performance and survival in leadership positions.

In this concluding chapter, I briefly summarize the theoretical argument, challenges and limitations of the data gathering process, methodology, and analysis, parse out the implications for the broader research program, and show avenues for continuing research. While not without limitations, this study offers insights not only into the hypothesized relationship, but into problems of internal as well as external validity and the aggregation of the unit of analysis in this research program. In addition, I argue that the overarching political

science theories actually allow for the studied event to impact voting decisions. In turn, this realization could motivate several avenues of further research.

I approached the research question in a neutral way with excitement to find out whether I would be able to replicate reward and punishment trends on the one hand or question generalized inference on the other hand. I started with the uncontroversial observations that some political leaders seem to emerge out of natural disasters with higher approval (e.g. Germany's Helmut Schmidt and Gerhard Schroeder) while others seemingly are punished at the polls for natural disasters (e.g. the USA's George W. Bush after hurricane Katrina). Matching observations with academic research, I found a research program which observed many instances in which incumbents are punished for random negative external shocks, such as natural disasters. Some consumers of this research have attributed degrees of external validity to mostly single-country and single-election studies so to derive a general conclusion about electorates on average punishing incumbents for natural disasters. I set out to validate such inference by looking for generalizable trends of electorates rewarding or punishing incumbents for natural disasters across countries and elections.

This study presents an analytical framework on the impact of natural disasters on incumbents' electoral performance and survival chance in leadership positions. The central argument is that electorates consider natural disasters in their performance-based voting decisions. Thus, natural disasters are hypothesized to influence incumbents' performance in elections. I provide possible pathways of a mechanism which had not been formally theorized in previous work. What is innovative about my claim is that I offer a potential link to connect the various studies on the subject matter which have drawn ambiguous or even

contradicting conclusions.

I posit three hypotheses: (i) Natural disasters during an incumbent's term are expected to yield overall smaller incumbent vote share in the following election, (ii) natural disasters during an incumbent's term are expected to yield an overall smaller incumbent vote share in the following election in low-corruption societies and overall higher incumbent vote share in high-corruption societies, and (iii) incumbent governments are rewarded for international disaster aid in high-corruption societies and punished for it in low-corruption societies. The theoretical goal of this study is to provide (i) a possible intervening variable which could link those studies which found negative electoral impacts of natural disasters with those which found positive or ambiguous results, and (ii) an expectation for a general tendency on what reaction incumbent governments can expect from their electorate for bringing in international disaster aid. The empirical goal is to provide a cross-country time-series empirical test of whether the finding of electorates punishing incumbent governments for natural disasters can be generalized beyond the United States of America.

To corroborate my argument for electorates punishing incumbents for natural disasters, I used two original data sets with elections observed in multiple countries over time in order to deduce generalizable tendencies of natural disasters impacting incumbents' electoral fate. One data set uses the country-election year as the unit of analysis and the second uses country-constituency-election year. The national-level data set includes 793 observations, covering 793 national lower house elections in 111 countries. The constituency-level data set includes 1176 observations, covering 74 national lower house elections in 333 first-order sub-national administrative regions of 17 countries. The natural disaster variables are populated

with two different sources to allow disaggregation to the constituency-level in the second data set. Using two different samples allows to not only test the respective null hypotheses in multiple settings but also to check the robustness of the results across model specifications and samples.

To restate the findings, this study demonstrates that the data overall does *not* support any generalizable trends of electoral rewarding or punishment of incumbents in the aftermath of natural disasters. This conclusion holds regardless of patronage expectations. Nevertheless, the implication of this finding is important because it confirms suspicions that the findings of single-country studies cannot be generalized to form an expectation of stupid, blind, myopic, or irrational voters who punish incumbents for random acts of god. In other words, the data underlying this study suggests that voters do *not* punish incumbents blindly for random shocks. The absence of a generalizable trend in either direction suggests that the effects of conditioning and intervening factors may lead rational electorates to cast rational votes. To my knowledge, this study incorporates more countries and elections than any other research project on the same topic. In a science which relies on test and retest, this study serves to validate (or rebuke) the general conclusions some researchers have drawn from various single-country studies.

The other two hypotheses could not be substantiated either. I argued that patronage expectations act within the black box between natural disasters and incumbent electoral performance, determining whether a specific natural disaster is going to have an electoral impact in the respective country's next election. The sample, however, could not reject the null hypothesis of no base effect of natural disasters when the model included patronage

expectation levels. In addition, if at all, patronage expectations seemed to generally point to the opposite direction than hypothesized. That being said, the coefficients and standard errors were so volatile that I advised caution when drawing any inference from them. Finally, I tested the effect of OECD humanitarian aid on incumbent party vote share in the next national lower house election. The effect differed between clientalistic and non-clientalistic societies: the effect in non-clientalistic societies was sizable and positive while patronage expectations within the electorate seemed to decrease the effect into practical unimportance. Once more, this result was also counterintuitive given the theoretical reasoning.

Implications

As stated in the very first sentence of the introduction, the central question of this study is 'Do electorates punish political incumbents at the polls for natural disasters which occurred during the incumbent's term?'. Considering past research on this topic, the empirical goal is to provide a cross-country time-series empirical test of whether the finding of electorates punishing incumbent governments for natural disasters can be generalized beyond the United States of America. In other words, this study was a replication effort, yet with a larger scope and theoretical framework as to why we could expect to see the hypothesized behavior.

The analytical findings had interesting conclusions for research on electoral impacts of natural disaster. First, the data failed to reject the null hypothesis of no direct effect of natural disasters on incumbent party vote share in national lower house elections. This finding is interesting in and of itself. It has to be evaluated in comparison with other studies which have also used research designs targeted to find generalizable trends. Second, consumers of

research studies have to consider limitations of external validity. It is important that studies and their statistical results are interpreted within the limitations of the methodology and scope of the data. Third, the process which lead to the rejection of the null hypothesis highlighted the importance of sample selection and the level of the unit of analysis. This demands a discussion of internal validity in a research program which relies heavily aggregated measures. Let us pick up the last two items for further discussion.

One take-away from this study in particular is the importance of granularity of the unit of analysis. This study used two original data sets at varying levels of granularity to inspect the same relationship. The first data set had the advantage in the amount of elections across countries and time, yet at the level of national aggregates. The second data set included considerably fewer elections and countries, yet matched the region in which the natural disasters happened to the respective electoral districts. This level of analysis is closer to the individual voter, yet does not overcome the problem of ecological fallacy. Still, it has a better chance to isolate the voting behavior of the group of voters who are directly affected by the natural disaster. It also creates a control group within the same country which arguably has more in common with the affected region than electorates in other countries. The difference in results between the two samples highlights the influence sample selection and granularity of the data can have.

Consumers of research studies should be careful to attribute appropriate amounts of external validity. This study echoes Remmer (2014) who already noted that the electoral impact of natural disasters may be overstated. Studies on incumbents' electoral fates after natural disasters are heavily concentrated geographically, as illustrated in table 2.1. It is thus

important that studies and their statistical results are interpreted within the limitations of the methodology and scope of the data. I tried to follow my own advice above by separating the presentation of the results from their interpretation. Also, an analysis is never the estimation of just one model specification with one method. Thus, I presented and discussed various approaches together in order to raise awareness about the limitations of the analysis. In the end, I could not conclude that the data supported the rejection of the null hypothesis of a generalizable direct effect of natural disasters on incumbent electoral performance. Assuming that my data quality checks and procedures worked as intended, this study is also a cautionary tale about overstating the external validity of statistical results. Interpretation of results has to match the research design.

The results also have interesting implications for the overarching theories which laid the foundation for this study's main hypothesis. Recall, the overarching theories were democratic accountability theory and (economic) performance-based voting. Testing whether voters punish incumbents for natural disasters assumes that electorates use elections to cast their votes in approval or disapproval of the incumbent government's performance. Voting for the incumbent then is like rewarding the incumbent for good policy making with another term, while voting against the incumbent is considered punishment for bad performance. Democratic accountability theory establishes that (i) voters and their elected representatives live in a principal-agent relationship and (ii) that elections are used as a reward-punishment mechanism in performance-based voting behavior. The economic voting literature establishes that (i) voters consider events and performance indicators during an incumbent's term to form a decision on whether to reward the incumbent with their vote in the reelection cycle,

and (ii) voters' decisions can be approximated with an additive model which allows for the inclusion of multiple factors and determine each factor's individual contribution to the vote outcome variable.

Natural disasters have impacted theorizing about democratic accountability and performance-based voting because these theories require able voters. Theory and empirics suggest that electorates' evaluations of events and performance indicators are sophisticated enough to engage in performance-based voting. The ideal of the sophisticated voter may not be achievable, but on average electorates seem to make reasonable decisions. Natural disasters entered this dynamic first as catalysts of existing conditions, then as instrumental variables to capture electorates' reactions to negative external shocks. Such studies were tests of the assumption of rational voters using their own economic well-being as a heuristic to judge incumbent performance during their retrospective voting decision. The theoretical expectation was straight forward: a rational voter would not punish for 'bad luck'. A separate theory for the electoral impact of natural disasters was not needed because the theoretical foundation is the retrospective economic voting literature which was tested here with a new, instrumental variable and according to which one would not have expected any statistically significant influence of natural disasters on incumbent vote share or survival chance. The finding of statistically significant influences lead to discussions of unsophisticated, blind, myopic, or irrational voter behavior.

Earlier described as black box, natural disasters seem to activate a process which may or may not lead to incumbents falling out of favor in the eyes of the electorate. I proposed that natural disasters have an electoral impact because (i) natural disasters are direct deter-

minants of incumbents' electoral fate, (ii) natural disasters act as a catalyst, pushing more unsatisfied citizens towards political action by revealing or exacerbating existing conditions, and (iii) natural disasters impact elections only if an intervening variable creates the right environment. While the first option was rejected by the data, the latter two can be understood as establishing natural disasters as a root cause for incumbents' electoral fate: in the absence of the natural disaster, the incumbent would not have had to react and thus, the electorate would not have incorporated the reaction in the punishment and reward mechanism. What drives performance-based voting then, is the proximate cause, or incumbent reaction to the natural disaster. In a single-event study, controlling for the root cause may be substantively equivalent to controlling for the proximate cause. In other words, controlling for the event itself or controlling for the incumbent's reaction to that event may yield the same conclusion for the reward and punishment mechanism. Either one is just an indicator for a single event. However, the same type of root cause (occurrence of a natural disaster) can yield different proximate causes leading to different voting behavior in a cross-section time-series setting and depending on contextual factors. Thus, the respective studies need to control for the proximate causes. Previous studies may have found electoral impacts of natural disasters due to case selection of single events, whereas other studies could not replicate such findings because controlling for the root cause was no longer sufficient. This realization might help explain the contradictory findings in this research program.

The findings of this study may therefore offer a different take on the importance of natural disasters for accountability and performance-based voting. Conclusions about voter sophistication might need revising if natural disasters truly have no direct effect on incumbents'

electoral performance. Voters may actually act reasonably when they assess government preparedness, mitigation, and response to natural disasters and incorporate these perceptions into their voting decision. Depending on the individual's assessment of actual and expected mitigation, she might reasonably cast her vote against the incumbent when she believes the natural hazard should not have escalated into a disaster in the first place. At this point the voter has made an assessment of incumbent performance and incorporated it into the punishment and reward mechanism, or election. As such, democratic accountability is served via retrospective performance-based voting by using one's own situation to assess incumbent performance. In other words, voters might be more sophisticated and democratic accountability might be achieved to a higher extent than commonly held.

Further Research

There still might be much to learn for social scientists if voter behavior after natural disasters is not just irrational. What contexts interact with natural disasters? Which parts of the disaster management process yield the highest return of investment for elected officials? Do natural disasters highlight inequalities, animosities, and ineffective policies and procedures? Can natural disasters be used as catalysts for reform? If yes, what does the window of opportunity look like? Parts of these questions were already examined by scholars who I cited above (see e.g. Pelling and Dill (2006)). Natural disasters are by definition a social event and may present themselves as sources of great knowledge when they are allowed to be more than indicators of myopic voting behavior.

If the bulk of research on the electoral impact of natural disasters shows anything, it is

that contextual variables matter. Natural disasters can exacerbate preexisting conditions and the electorate's perception of natural disaster causes, preparedness, mitigation, and response is the key to proper analysis of the disaster-election nexus. After all, natural disasters are defined as events when natural processes clash in space and time with a vulnerable population. It is, therefore, human contextual variables which by definition cause a natural process to become a natural disaster and which determine how the natural process is otherwise perceived. The literature review already highlighted several studies which considered perception bias, media coverage, or quality of government preparation as well as response. This study used the angle of electorates' expectations of government behavior, in particular patronage expectations, as a potential intervening variable on the electoral impact of natural disasters. Future research could retest and add more contextual information to this discussion.

Government spending could be one more way of approximating patronage expectations. The national-level patronage proxy was chosen for this study because it was available over time and closest to the hypothesized mechanism. Other corruption indices were available and correlated with the patronage measure at very high levels ($>.9$) but they did not approximate the expectation of receiving a reward from voting for a party as well. In other words, I focused on internal validity. I did try to collect data on government spending for the countries in this study. Such data could have been used to ask whether patronage expectations were fulfilled. While theoretically superior, sub-national and even national-level government spending data was not available within the scope of this study.

Further research could also try to measure natural disasters in different ways. The foremost goal of my coding was to make my data and findings comparable to existing studies

while overcoming data availability issues. Hence, this study used dummy and count approaches for the measurement of natural disasters. Alternate approaches could categorize natural disasters by severity (e.g. like hurricane classifications). During the data collection for this study, however, I found that damage to human and physical capital depend heavily on the affected society's development: developed societies seem to experience more physical damage because they have more resources to mitigate and cope. Developing societies seem to experience more loss of life and less damage when expressed in currency. Thus, the same hazard may be a nuisance in some places and a natural disasters in other settings.

The discussion of the statistical results above has been a cautionary tale about drawing inference from highly aggregated samples. Further research could engage survey data. For example, the Latin American Public Opinion Project sometimes includes questions and answers pertaining to natural disaster response and preparedness. I deemed this undertaking to be both out of scope and to yield insufficient amounts of data at the time of data gathering for this study. I am looking forward to reading future studies using this data.

Survey data could also address the timeliness and adequacy of government response. The literature review above signals clearly that some leaders have shown higher quality responses to natural disasters than others. Further, perceptions seem to be just as important as the actual response. A survey could ask for details and assess context in greater detail than this study. The lack of voter-level data is certainly a limitation of this study's design.

The findings of this study make clear that much is still unknown about the electoral impacts of natural disasters. Despite employing a research design targeted towards uncovering generalizable trends, this analysis was unable to replicate the findings of other studies

which had suggested direct electoral effects of natural disasters. Such a fact should lead to reconsidering inference drawn from previous studies, especially since this was not the first unsuccessful cross-sectional time-series attempt to validate an interpretation derived mostly from single-country and even single-event studies. Much remains to be uncovered about electoral behavior after the occurrence of natural disasters. Assuming the validity of my methods and data, one may argue that natural disasters have no direct effect on incumbents' electoral performance. Nevertheless, natural disasters can constitute the root cause for government actions, media portrayal of decision makers, and other factors influencing an electorate's opinion of its elected officials. In that case, electorates would not be blindly reacting to random external shocks but take into account actions which were initiated by the disaster. In other words, conclusions could be made which would positively influence our understanding of voter rationality and sophistication. Then, all research programs which used natural disasters as instrumental variables to show the *irrationality* of voters might have to be revised in parts.

Appendix: Interaction Models for Section

Table 6.1: Natural Disasters and Patronage Expectations (Disasters during $EP-1$; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.622*** (0.042)	0.634*** (0.044)	0.711*** (0.117)
rgdpe_pc_change_lag	0.157* (0.086)	0.190** (0.088)	-0.109 (0.279)
OECDhumaidEP	0.367*** (0.133)	0.344*** (0.124)	0.231 (0.180)
rapid_occurrenceEP	-0.069 (0.064)	-0.060 (0.060)	-0.105 (0.076)
slow_occurrenceEP	-0.046 (0.270)	-0.064 (0.251)	-0.477 (0.971)
linkage1	-1.987 (2.396)		
OECDhumaidEP:linkage1	-0.118 (0.190)		
rapid_occurrenceEP:linkage1	-0.087 (0.089)		
slow_occurrenceEP:linkage1	0.050 (0.812)		
N	550	480	70
Log Likelihood	-2,071.542	-1,771.972	-286.932
AIC	4,169.083	3,561.944	591.863
BIC	4,225.112	3,599.508	612.099

* $p < .1$; ** $p < .05$; *** $p < .01$

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the national-level data set. Natural disaster occurrences as count during the electoral period. OECD humanitarian aid as sum per capita in previous electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries and electoral periods) were omitted from the result table.

Table 6.2: Natural Disasters and Patronage Expectations (Disasters at $t - 1$; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.590*** (0.043)	0.642*** (0.044)	0.494*** (0.128)
rgdpe_pc_change_lag	0.223*** (0.085)	0.229*** (0.088)	0.196 (0.266)
OECDhumaid_lag	0.786* (0.463)	0.720* (0.423)	-0.463 (0.898)
Climatological_occurrence	-0.139 (0.902)	-0.271 (0.831)	0.621 (4.386)
Geophysical_occurrence	-1.046 (0.976)	-1.030 (0.894)	-6.005* (3.123)
Hydrological_occurrence	-0.162 (0.369)	-0.139 (0.337)	-2.160 (1.522)
Meteorological_occurrence	-0.023 (0.281)	0.006 (0.258)	2.453* (1.327)
linkage1	2.121 (2.535)		
OECDhumaid_lag:linkage1	-1.371* (0.795)		
Climatological_occurrence:linkage1	0.739 (3.187)		
Geophysical_occurrence:linkage1	-3.862 (2.385)		
Hydrological_occurrence:linkage1	-2.131* (1.130)		
Meteorological_occurrence:linkage1	2.560** (1.001)		
N	584	497	87
Log Likelihood	-2,208.181	-1,841.918	-351.604
AIC	4,450.362	3,705.835	725.209
BIC	4,524.650	3,752.130	752.334

* $p < .1$; ** $p < .05$; *** $p < .01$

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the national-level data set. Natural disaster occurrences as count during the last year. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries and electoral periods) were omitted from the result table.

Table 6.3: Natural Disasters and Patronage Expectations (Disasters during $EP - 1$; national-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.622*** (0.042)	0.624*** (0.045)	0.690*** (0.125)
rgdpe_pc_change_lag	0.165* (0.086)	0.191** (0.088)	-0.074 (0.284)
OECDhumaidEP	0.376*** (0.133)	0.356*** (0.124)	0.259 (0.173)
Climatological_occurrenceEP	0.130 (0.437)	0.079 (0.407)	-1.464 (2.067)
Geophysical_occurrenceEP	-0.543** (0.276)	-0.578** (0.258)	0.330 (1.273)
Hydrological_occurrenceEP	-0.038 (0.121)	-0.015 (0.114)	-1.024 (0.628)
Meteorological_occurrenceEP	-0.042 (0.135)	-0.024 (0.127)	0.315 (0.299)
linkage1	0.309 (2.545)		
OECDhumaidEP:linkage1	-0.107 (0.186)		
Climatological_occurrenceEP:linkage1	-0.668 (1.609)		
Geophysical_occurrenceEP:linkage1	1.170 (0.908)		
Hydrological_occurrenceEP:linkage1	-1.063** (0.478)		
Meteorological_occurrenceEP:linkage1	0.263 (0.272)		
N	550	480	70
Log Likelihood	-2,065.648	-1,770.288	-282.730
AIC	4,165.295	3,562.575	587.460
BIC	4,238.564	3,608.487	612.194

* $p < .1$; ** $p < .05$; *** $p < .01$

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the national-level data set. Natural disaster occurrences as count during the last electoral period. OECD humanitarian aid as sum per capita in previous electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries and electoral periods) were omitted from the result table.

Table 6.4: Natural Disasters and Patronage Expectations (Disasters during $EP - 1$; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.422*** (0.026)	0.505*** (0.035)	0.292*** (0.039)
rgdpe_pc_change_lag	0.082 (0.052)	0.186** (0.094)	0.044 (0.067)
OECDhumaidEP	1.231*** (0.100)	1.349*** (0.100)	0.236* (0.130)
rapid_occurrenceEP	-0.260* (0.146)	-0.284** (0.136)	-0.926*** (0.261)
slow_occurrenceEP	1.045*** (0.272)	0.979*** (0.257)	-0.820 (0.576)
linkage1	-5.518** (2.435)		
OECDhumaidEP:linkage1	-0.799*** (0.134)		
rapid_occurrenceEP:linkage1	-0.762** (0.297)		
slow_occurrenceEP:linkage1	-1.405** (0.638)		
N	1,176	649	527
Log Likelihood	-4,367.295	-2,373.310	-1,956.654
AIC	8,762.590	4,766.621	3,933.308
BIC	8,833.568	4,811.375	3,975.980

* $p < .1$; ** $p < .05$; *** $p < .01$

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the constituency-level data set. Natural disaster occurrences as count during the electoral period. OECD humanitarian aid as sum per capita in previous electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries, constituencies, and electoral periods) were omitted from the result table.

Table 6.5: Natural Disasters and Patronage Expectations (Disasters at $t - 1$; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.414*** (0.028)	0.522*** (0.039)	0.283*** (0.039)
rgdpe_pc_change_lag	0.087 (0.055)	0.166 (0.105)	0.075 (0.067)
OECDhumaid_lag	1.344*** (0.285)	1.267*** (0.302)	-1.564*** (0.577)
Climatological_occurrence	0.440 (1.298)	0.530 (1.291)	-2.044 (2.588)
Geophysical_occurrence	-4.238 (5.974)	-3.610 (5.926)	-3.713 (9.604)
Hydrological_occurrence	-0.201 (0.401)	-0.096 (0.398)	-2.040** (0.924)
Meteorological_occurrence	0.247 (0.892)	0.097 (0.886)	-3.882 (3.122)
linkage1	-8.097*** (2.666)		
OECDhumaid_lag:linkage1	-1.199** (0.517)		
Climatological_occurrence:linkage1	-1.226 (3.050)		
Geophysical_occurrence:linkage1	-0.270 (11.851)		
Hydrological_occurrence:linkage1	-2.323** (1.052)		
Meteorological_occurrence:linkage1	-1.966 (3.462)		
N	1,176	649	527
Log Likelihood	-4,420.266	-2,441.776	-1,946.830
AIC	8,876.533	4,907.552	3,917.660
BIC	8,967.790	4,961.257	3,968.867

*p < .1; **p < .05; ***p < .01

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the constituency-level data set. Natural disaster occurrences as count during the last year. All explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries, constituencies, and electoral periods) were omitted from the result table.

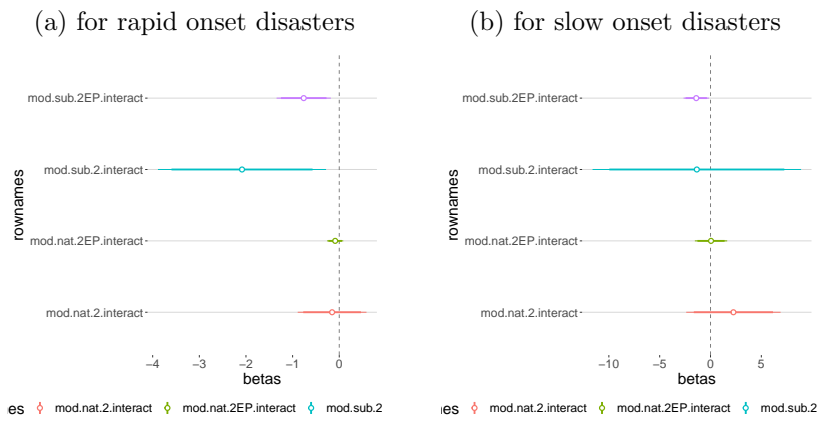
Table 6.6: Natural Disasters and Patronage Expectations (Disasters during $EP - 1$; constituency-level data)

	Dependent Variable: Incumbent Party Vote Share		
	(1)	(2)	(3)
pvs1_lag	0.424*** (0.026)	0.512*** (0.035)	0.292*** (0.039)
rgdpe_pc_change_lag	0.086 (0.053)	0.188** (0.093)	0.036 (0.068)
OECDhumaidEP	1.238*** (0.100)	1.351*** (0.099)	0.242* (0.130)
Climatological_occurrenceEP	1.019*** (0.279)	0.927*** (0.262)	-0.879 (0.567)
Geophysical_occurrenceEP	-1.770*** (0.570)	-2.184*** (0.535)	0.013 (0.793)
Hydrological_occurrenceEP	-0.198 (0.164)	-0.192 (0.152)	-1.005*** (0.324)
Meteorological_occurrenceEP	0.003 (0.402)	-0.002 (0.373)	-1.064* (0.566)
linkage1	-5.168** (2.427)		
OECDhumaidEP:linkage1	-0.800*** (0.134)		
Climatological_occurrenceEP:linkage1	-1.409** (0.631)		
Geophysical_occurrenceEP:linkage1	1.854* (0.979)		
Hydrological_occurrenceEP:linkage1	-0.806** (0.361)		
Meteorological_occurrenceEP:linkage1	-1.637** (0.686)		
N	1,176	649	527
Log Likelihood	-4,359.885	-2,365.792	-1,954.770
AIC	8,755.770	4,755.584	3,933.540
BIC	8,847.028	4,809.289	3,984.747

*p < .1; **p < .05; ***p < .01

The Interaction Of Natural Disasters And Patronage Expectations. Estimated via REML on the national-level data set. Natural disaster occurrences as count during the last electoral period. OECD humanitarian aid as sum per capita in previous electoral period. All other explaining variables are lagged by one year (except for incumbent party vote share which is the incumbent's vote share at the last election). 'GDP pc' refers to the GDP per capita. All monetary variables are in million real 2011USD at chained PPPs. The constant and all varying intercepts (for countries, constituencies, and electoral periods) were omitted from the result table.

Figure 6.1: Plot of the Rapid and Slow Onset Coefficients (Interaction Effects)



(a) Coefficients for rapid onset natural disasters' base effects. (b) Coefficients for slow onset natural disasters' base effects.

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