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# Voting Behavior and Electoral Choice Using Causal Inference Methods for Observational Data

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München, 2018



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# **Voting Behavior and Electoral Choice Using Causal Inference Methods for Observational Data**

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zur Erlangung des Doktorgrades  
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# List of Abbreviations

<b>a.o.</b>	and others
<b>adj. R<sup>2</sup></b>	adjusted R-squared
<b>AG</b>	Arbeitsgruppe [working group]
<b>ATE</b>	average treatment effect
<b>ATT</b>	average treatment effect on the treated
<b>BES</b>	British Election Survey
<b>BFG</b>	Bundesanstalt für Gewässerkunde [German Federal Institute of Hydrology]
<b>BMI</b>	Bundesministerium des Inneren [Federal Ministry of the Interior]
<b>BMVg</b>	Bundesministerium der Verteidigung [Federal Ministry of Defense]
<b>CDU</b>	Christlich Demokratische Union Deutschlands [German Christian Democratic Union]
<b>chap.</b>	chapter
<b>comp.</b>	compare
<b>CSU</b>	Christlich-Soziale Union in Bayern [Bavarian Christian-Social Union]
<b>DID</b>	difference-in-difference
<b>EB</b>	entropy balancing
<b>EE</b>	European Election
<b>EEC</b>	European Economic Community
<b>EP</b>	European Parliament

<b>EU</b>	European Union
<b>e.g.</b>	exempli gratia [for example]
<b>EITM</b>	empirical implications of theoretical models
<b>fn.</b>	footnote
<b>HND</b>	Hochwassernachrichtendienst Bayern [Bavarian Flood Alert Agency]
<b>i.e.</b>	id est [this is to say]
<b>IV</b>	instrumental variables
<b>LATE</b>	local average treatment effect
<b>LFU</b>	Bayerisches Landesamt für Umwelt [State Environmental Agency of Bavaria]
<b>LUA</b>	Landesumweltamt Brandenburg [State Environmental Agency of Brandenburg]
<b>mm</b>	millimeter
<b>OLPR</b>	open-list proportional representation
<b>OLS</b>	ordinary least squares
<b>ONS</b>	Office for National Statistics
<b>PR</b>	proportional representation
<b>PSM</b>	propensity score matching
<b>RDD</b>	regression-discontinuity design
<b>SATT</b>	sample average treatment effect on the treated
<b>SMD</b>	single-member district
<b>SPD</b>	Sozialdemokratische Partei Deutschlands [German Social Democratic Party]
<b>SSR</b>	Sächsische Staatsregierung [Saxonian State Government]
<b>UK</b>	United Kingdom
<b>Ukip</b>	United Kingdom Independence Party
<b>US</b>	United States

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# Summary

Which role do the costs and benefits of voting play for the decision of a voter to participate in an election? Does turnout affect the aggregate choice of the electorate? Are incumbents held to account by voters? How do electoral institutions affect the link between incumbent performance and voting behavior? And how can we address these questions given the complex socio-economic and political environment citizens, politicians and parties interact in?

In this thesis, I answer questions on “Voting Behavior and Electoral Choice Using Causal Inference Methods for Observational Data.” I inquire representation, accountability and responsiveness, and the role institutions and information play therein – all central factors for understanding how democracy works. These questions are notoriously challenging to address. Strategic incentives and complex interrelations of all involved actors give rise to serious endogeneity problems. This dissertation therefore draws on a design-based approach, using quasi-experimental evidence to contribute to our understanding on the link between citizens, elected representatives and policy. I employ recent methodical innovations in difference-in-difference estimation, matching strategies or instrumental variables, mostly when analyzing administrative electoral data on the aggregate level, augmented by individual level survey data to study the causal mechanisms involved.

My research presented in this cumulative dissertation is partially co-authored with Thomas Däubler, Patrick Kuhn, Arndt Leininger and Steffen Zittlau.<sup>1</sup> A first set of papers is concerned with individual vote choice and aggregate electoral outcomes following information shocks and institutional differences. I show whether and how voters use information to mandate future leaders and hold politicians to account for their performance, and that this link is mediated by institutional features. In particular, I argue that information revealed by sudden shocks such as natural disasters is used in a forward-looking selection logic and not only following a retrospective reward-punishment mechanism. However, the institutional features that affect the linkage between voters and politicians matter a great deal; especially the electoral system affects whether primarily party based or as well candidate centered accountability can occur. We make the argument that voters can more easily hold politicians to account for scandalous behavior when voting in an open list proportional representation

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<sup>1</sup>In the following, when using personal pronouns, the use of “I” implies that I address general points on my broader research agenda or concrete research findings from a single-authored paper, the use of “we” indicates that I address concrete research findings from a co-authored piece.

systems with a two-dimensional party and candidate choice, as compared to single-member district elections. Finally, I argue that incumbents enjoy an electoral advantage (potentially endangering accountability) and show that this can even spill over to other electoral arenas.

In a second set of research papers, I assess causes and consequences of electoral participation as a precondition for representation and responsive governance. We show that voting costs matter. E.g., weather shocks likely increase the personal costs of voting and hence depress turnout; as these shocks are not only geographically clustered but affect some citizens more than others, this has consequences for aggregate electoral choice and representation. I trace the electoral effects of turnout and argue that changes in costs can have consequences for which parts of the population are mobilized to vote; therefore, moderate increases in turnout need not increase the representativeness of turnout. These consequences are important for evaluating institutional features that affect the cost-benefit calculation of electoral participation. Particularly, we show that electoral timing, in our case concurrently held elections, can strongly influence the benefits of electoral participation and lead to substantial increases in turnout. This again has consequences for which groups in the electorate turn out, hence as well for average voter characteristics and finally representation and policy choice.

My work draws on causal inference methods for observational data and highlights the role of research design for estimating internally valid effects. All papers use a difference-in-difference strategy: I make the general argument that selection bias from unobservable confounders can be mitigated by using a difference-in-difference framework and/or employing fixed effects models. Drawing on placebo and balance tests, the differencing strategy can even identify causal effects outright where it is plausible that event-affected observations would have counterfactually followed the same trend as control cases. In three applications, we suggest a difference-in-tiers approach, exploiting the fact that voters vote in two tiers in many German elections, where party factors affect the vote similarly across both tiers, but candidate effects plausibly affect only one of the votes. Where differencing methods are not sufficient to induce as-if-random treatment assignment, I combine them with matching approaches to construct valid control groups. Finally, in one case each I draw on an instrumental variable setting and on (geographical) discontinuities to answer the substantive questions I am interested in. Overall, the papers in this thesis show that the design based approach to political science questions is a highly useful methodological perspective to isolate substantively intriguing relationships between social variables.

This thesis is organized as follows: In the following introductory chapter, I summarize my core findings and link them to the broader literature on electoral systems, vote choice and participation, as well as the credibility revolution in political science. Subsequently, the published version of four, the working paper version of two of these papers, and the respective paper appendices follow as individual chapters.<sup>2</sup>

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<sup>2</sup>Where an article is already published, I provide only a summary and then link to the officially published version of said article (following §16 IV 2, Promotionsordnung [Doctoral Regulations] der Ludwig-Maximilians-Universität München für die Sozialwissenschaftliche Fakultät, 18 March 2016).

# Zusammenfassung

Welche Rolle spielen die Kosten und Nutzen des Wahlprozesses für die Beteiligungsentscheidung? Hat die Höhe der Wahlbeteiligung elektorale Konsequenzen? Wie werden Repräsentanten ausgewählt, wie werden sie zur Rechenschaft gezogen? Beeinflussen institutionelle Regeln die Beziehung zwischen Performanz und Abstimmungsverhalten? Und wie lassen sich solche Fragen angesichts komplexer sozialer und politischer Interaktionen untersuchen?

In dieser Dissertationsschrift widme ich mich Fragen zu „Voting Behavior and Electoral Choice Using Causal Inference Methods for Observational Data.“ Ich untersuche Aspekte von Repräsentation, der Verantwortlichkeit von Mandatsträgern, von Responsivität sowie der Rolle von Institutionen und Informationen hierbei. Diese Themen sind zentral für das Verständnis demokratischer Prozesse. Die Untersuchung dieser Fragen stellt jedoch eine methodische Herausforderung dar: Vielfältige strategische Anreize und komplexe Beziehungen zwischen den beteiligten Akteuren führen zu Endogenitätsproblemen, die zwingend bei der empirischen Analyse beachtet werden müssen. In dieser Dissertation wähle ich daher einen design-basierten Ansatz und nutze quasi-experimentelle Techniken – mit dem Ziel, zu unserem Verständnis der Beziehung zwischen Bürgern, gewählten Repräsentanten und Politikentscheidungen beizutragen. Ich nutze methodische Innovationen in der Weiterentwicklung von Differenzenschätzern, Matching-Ansätzen oder Instrumentalvariablen. Dabei analysiere ich überwiegend administrative elektorale Aggregatdaten. Umfragedaten auf Individualebene kommen zusätzlich zum Einsatz, um kausale Mechanismen tiefergehend zu untersuchen.

Meine Forschung in dieser kumulativen Dissertationsschrift ist teilweise in Koautorschaft mit Thomas Däubler, Patrick Kuhn, Arndt Leininger und Steffen Zittlau entstanden.<sup>3</sup> Die ersten drei Aufsätze beschäftigen sich mit der Frage ob bzw. wie Wähler in ihrer Wahlentscheidung Informationen nutzen und auf welche Weise Institutionen Verantwortlichkeit beeinflussen. Insbesondere argumentiere ich, dass Informationen zu Regierungshandeln, hier durch das plötzliche Auftreten einer Hochwasserkatastrophe, nicht nur retrospektiv im Sinne einer Belohnungs-Bestrafungs-Logik, sondern auch prospektiv zur Selektion und Mandatierung einer ‚guten‘ Regierung genutzt wird. Institutionelle Faktoren mediierten, inwiefern solche Faktoren auf die Wahlentscheidung Einfluss haben können: Besonders das Wahlsystem bestimmt

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<sup>3</sup>Mit der Nutzung des Personalpronomens „ich“ beziehe ich mich im Folgenden auf generelle Aspekte meiner breiteren Forschungsagenda oder auf konkreter Ergebnisse von Studien in Alleinautorschaft. Die Nutzung von „wir“ impliziert dagegen, dass ich Ergebnisse von Arbeiten in Koautorschaft referenziere.

mit darüber, ob Rechenschaft gegenüber dem Wähler primär parteizentriert oder auch kandidatenzentriert stattfindet. Wir vergleichen dazu Prozesse der Verantwortlichkeit in einem offenen Listenwahlsystem und einem System relativer Mehrheitswahl in Einerwahlkreisen: Nach einem öffentlichen Skandal erlaubt Ersteres dem Wähler nicht nur eine Entkopplung von Partei- und Kandidatenentscheidung, sondern ist zudem responsiver bezüglich kleiner Änderungen in Stimmenanteilen. Die Institution eines offenen Listenwahlsystems befördert daher kandidatenzentrierte Verantwortlichkeit. Dies gilt jedoch nur *ceteris paribus*. Amtsinhaber besitzen etwa einen elektoralen Bonus: Ich zeige, dass dieser sogar in andere elektorale Arenen überspringen kann.

Die zweiten drei Aufsätze behandeln Ursachen und Konsequenzen von Wahlbeteiligung als grundlegendem Faktor für gute Repräsentation und responsive Politik. Wir weisen nach, dass die Kosten der Wahl eine Rolle spielen. Wetterereignisse beeinflussen diese etwa, was zu einem Rückgang der Wahlbeteiligung führt. Da ein derartiger Rückgang zum einen geographisch geclustert ist, zum anderen manche Bürger stärker auf Veränderungen hinsichtlich der Kosten reagieren, hat dies Konsequenzen für die Wahl des Elektorats und Repräsentation. Ich argumentiere hier insbesondere, dass auch eine Erhöhung der Wahlbeteiligung elektorale Effekte haben kann, die nicht zwangsläufig auf höherer Repräsentativität beruhen. Für die Bewertung der Auswirkungen institutioneller Regelungen, die die Nutzen und Kosten der Wahl beeinflussen, ist dies ein wichtiger Aspekt. Wir zeigen etwa, dass der Wahlzyklus, hier die Gleichzeitigkeit mehrerer Abstimmungen, einen starken Einfluss auf die aggregierte Wahlbeteiligung haben kann. Dies impliziert, dass sich manche gesellschaftlichen Gruppen relativ mehr, andere relativ weniger beteiligen. Dadurch unterscheidet sich der Medianwähler, was wiederum vermutlich Implikationen für Policy-Entscheidungen der Regierung hat.

Diese Anwendungen greifen alle auf Methoden der kausalen Inferenz mit Beobachtungsdaten zurück und betonen die Rolle des Forschungsdesigns für intern valide Ergebnisse. Insbesondere nutze ich Differenzenschätzer als Methode, die generell geeignet ist, Selektionsverzerrung durch unbeobachtete, zeitinvariante Störfaktoren zu reduzieren. Der Ansatz erlaubt gar direkt kausale Schlussfolgerungen, wenn Placebo- oder Balance-Tests es plausibel erscheinen lassen, dass Beobachtungen der Kontrollgruppe tatsächlich den kontrafaktischen Trend der Treatmentgruppe (d.h. den Trend ohne das Treatment) abbilden. In drei Anwendungsfällen zeige ich die Eignung von Differenzenschätzern über die temporale Dimension hinaus, etwa indem wir diese auf aggregierte Unterschiede in Abstimmungsmodi (Erst- und Zweitstimme) beziehen, um gemeinsam zugrundeliegende Parteifaktoren implizit zu kontrollieren und Kandidatenfaktoren in der Wahlentscheidung zu isolieren. Wo die zentrale Annahme an Differenzenschätzer, parallele Trends, möglicherweise verletzt ist, kombiniere ich sie etwa mit Matching-Ansätzen, um valide Kontrollgruppen zu konstruieren. Einzelne Anwendungen nutzen zudem Instrumentalvariablenschätzer oder geographische Diskontinuitäten, um Effekte zu identifizieren. Insgesamt zeigen diese Anwendungen, dass eine design-basierte methodische Herangehensweise an sozialwissenschaftliche Fragestellungen sehr gewinnbrin-

gend ist und es gelingen kann, die substanziell interessierenden Variablenbeziehungen auch mit Beobachtungsdaten quasi-experimentell zu isolieren.

Diese Arbeit ist wie folgt strukturiert: Im folgenden Einleitungskapitel fasse ich meine zentralen Ergebnisse zusammen und Verknüpfe diese mit der Wahlsystem-, Wahlentscheidungs-, und Partizipationsliteratur sowie mit der methodologischen Literatur zu kausaler Inferenz (mit Beobachtungsdaten). Daran schließen sich in sechs Kapiteln die veröffentlichte Version von vier, sowie die Manuskript-Version von zwei Aufsätzen an. Zuletzt folgen in sechs weiteren Kapiteln die jeweiligen Appendizes.<sup>4</sup>

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<sup>4</sup>Bei Artikeln, die bereits veröffentlicht sind, enthalten die jeweiligen Kapitel nur eine Zusammenfassung, gefolgt von einer Verlinkung zur veröffentlichten Version des Artikels (entsprechend §16 IV 2, Promotionsordnung der Ludwig-Maximilians-Universität München für die Sozialwissenschaftliche Fakultät, 18. März 2016).



# Chapter 1

## Introduction

Elections serve two important purposes: They aggregate information and preferences; and they give voters the opportunity to select future leaders and hold representatives to account. Both functions are central pillars of a representative democracy. Concerning the latter, to effectively select delegates and hold them to account, two aspects are fundamental: First, voters need sufficient capabilities, particularly information, to not only participate in the electoral process but make good aggregate decisions. Second, the institutional incentives for electoral participation and choice need to be construed in such a way that they facilitate the selection of high-quality representatives, accountability and, in the end, an overlap between citizen preferences and legislative and executive policy making. Concerning the former, when aggregating preferences, citizens of all backgrounds need to take part in the electoral process, as (equal) responsiveness depends on (equal) electoral participation. This again requires institutional incentives shaped in a way that furthers representative participation.

My dissertation relates to each of these factors under the title “Voting Behavior and Electoral Choice Using Causal Inference Methods for Observational Data.” Hence, I study voting behavior, which I understand broadly as the individual level decision making processes surrounding the political participation and the vote choice decision. Therein, a particular focus of this thesis lies on electoral choice. On the one hand, I inquire how the electorate makes an aggregate choice with a specific focus on the mediating role of informational and institutional factors. On the other hand, where I assess electoral participation, I do so with the explicit or implicit goal of understanding the relationship between aggregate turnout and aggregate electoral choice. Methodically, I use innovative research designs and recently developed quasi-experimental methods for the analysis of observational data: The difference-in-difference approach, matching and instrumental variable estimators, and regression discontinuities. This is important, as the complex setting in which political actors interact in leads both to selection effects and unobserved confounding, hence identification is a challenging endeavor. With these methods, I then inquire how static systems with well-defined basic conditions react to externally induced turbulences – weather shocks and natural disasters, suddenly revealed political scandals or institutional reforms. These turbulences induce

shockwaves – changes in incentives and/or the leeway of actors – that I exploit to identify the mediating effect of institutions and information on turnout and electoral choice.

This chapter provides an introduction to the substantive questions and the methods applied. Chapter 1.1 introduces the broader state of the literature on incentives in electoral systems, voter decision making and the electoral participation decision. Following these broader introductions to the respective fields, each of the paper contributions of this dissertation is linked to the literature and summarized in short with its respective hypotheses and main findings. Chapter 1.2 then gives an overview on the credibility revolution in political science, its focus on identification through research design, and the strengths and limitations of the approach. The methods applied in the subsequent chapters are shortly discussed, their choice is justified and related to the ongoing discussion in political methodology, with references to similar literatures in statistics, economics and sociology.

The following Chapters 2 to 7 then constitute the individual papers for this cumulative dissertation, together with the respective Appendix Chapters A to F.<sup>1</sup> The papers are, in order of appearance:

- Chapter 2: Rudolph, Lukas and Thomas Däubler (2016): “Holding Individual Representatives Accountable: The Role of Electoral Systems”, in: *The Journal of Politics*, 78:3, pp. 746-762.
- Chapter 3: Rudolph, Lukas (2017): “Selecting Good Types or Holding Incumbents Accountable? Evidence from Reoccurring Floods”, manuscript.
- Chapter 4: Rudolph, Lukas (2017): “Die Münchner Ergebnisse im Bundes- und Landesvergleich: Ein Ude-Effekt in München?” [“The Munich Results of the Federal and State Elections in Comparison: An Ude-Effect in Munich?”] In: Klima, André, Helmut Küchenhoff, Mirjam Selzer, and Paul W. Thurner (eds.): “Exit Polls und Hybrid-Modelle. Ein neuer Ansatz zur Modellierung von Wählerwanderungen” [“Exit Polls and Hybrid Models: A New Approach to Model Voter Transitions”]. Springer: Wiesbaden, pp. 21-35.
- Chapter 5: Leininger, Arndt, Lukas Rudolph and Steffen Zittlau (2018): “How to Increase Turnout in Low Salience Elections. Quasi-Experimental Evidence on the Effect of Simultaneous Second-Order Elections on Political Participation”, in: *Political Science Research and Methods*, 6:3, pp. 509-526.
- Chapter 6: Rudolph, Lukas and Patrick M. Kuhn (2018): “Natural Disasters and Political Participation: Evidence from the 2002 and 2013 Floods in Germany”, in: *German Politics*, 27:1, pp. 1-24.
- Chapter 7: Rudolph, Lukas (2017): “Turning Out to Turn Down the EU: The Mobilization of Occasional Voters and Brexit”, manuscript.

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<sup>1</sup>Where a paper is already published, I only provide a summary followed by links to the publisher’s version in the respective chapter (in accordance with §16 IV 2, Promotionsordnung [Doctoral Regulations] der Ludwig-Maximilians-Universität München für die Sozialwissenschaftliche Fakultät, 18 March 2016).



## 1.1 Institutions, Information and Voting Behavior in Comparative Perspective

### 1.1.1 The Rules of the Game: Electoral Systems and Accountability

*“Only the ballot box provides regular opportunities for the public to select representatives, to hold governments to account, and to kick the rascals out, where necessary. Electoral systems are commonly regarded as some of the most basic democratic structures, from which much else flows.” (Norris, 2004, 3)*

#### Electoral systems and the incentive structure driving electoral choice

Despite the importance of electoral systems, scholarly attention only picked up in the last decades. Early scientific work centered around the social choice approach<sup>2</sup>, most prominently Arrow (1951), or otherwise was “polemic in tone” (Grofman, 2016, 524). The empirical implications of electoral systems were first systematically analyzed by Rae (1967). Recently, the effects of electoral systems and particular electoral rules and regulations became a central focus of work in comparative politics and political economy. Several large research programs can be identified, two of which are particularly relevant for this thesis (see for an in-depth introduction Grofman, 2016):

Following the rational choice approach, a first research tradition highlights the mediating effect of electoral systems in representative democracies: They shape the strategic incentives voters, candidates/politicians and parties face (Cox, 1997).<sup>3</sup> Parties and candidates are seen as office-seeking vote-maximizers and citizens as maximizing their expected utility from the electoral process (Downs, 1957). This approach provides a concise logical framework for analyzing electoral system effects (Norris, 2004, Chapter 1): Starting point are the incentives generated by electoral systems. These stem from the number of votes a voter has; whether she can abstain; options for cumulative voting; district magnitude; and electoral rules (Thurner, 1998, 20-24). Political actors will then adapt their behavior to these rules in order to increase their electoral chances. Particularly the electoral threshold and the ballot structure will influence what types of candidates are fielded and what type of goods (programmatic or particularistic benefits) are offered to voters. Voters then respond rationally to the electoral options presented. More broadly, research in the tradition of the rational choice approach has highlighted three stages where electoral rules affect actors and give rise to coordination

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<sup>2</sup>See Thurner (2008) for an overview.

<sup>3</sup>As highlighted by Cox (1997, Chapter 1), their respective preferences and the formation of expectations are the two other necessary elements of the strategic situation. In fact, Cox (1997) emphasizes that incentive structures affect first and foremost the expectations of actors; these expectations then shape the coordination of citizens and elites. However, electoral rules affect as well voters’ utility function, independent of strategy (Singh, 2010).

problems (Cox, 1997, Chapter 15): preferences to votes (and seats, i.e. representation); seats to portfolios; seats and portfolios to policy.

Important theoretical work in these fields has e.g. investigated the impact of electoral systems on voters' utility calculations (Singh, 2010), including electoral participation (Bowler, Lanoue, and Savoie, 1994); how electoral systems influence sincere vs. strategic voting (Satterthwaite, 1975; Alvarez and Nagler, 2000; Pappi and Thurner, 2002); the influence of electoral rules on information aggregation, updating of beliefs and strategic signaling (Dewan and Shepsle, 2011); the conditions under which electoral systems influence party systems (Duverger, 1951; Cox, 1997; Morelli, 2004); the connection of electoral rules to party competition and the spatial theory of voting (Cox, 1990); the relation between electoral systems and political accountability through checks and balances (Persson, Roland, and Tabellini, 1997); the impact of electoral systems on elite incentives for corruption (Myerson, 1993) or, more broadly, public goods provision (Lizzeri and Persico, 2001). The insight that actors face strategic incentives and coordination challenges and that the local incentives vary over electoral systems unifies this approach. Some of these are well understood, for example how district magnitude affects the viable choice set (Cox, 1997) or the extent to which the electoral system induces collective vs individual representation (Colomer, 2011). For others, however, more research is needed. For the first stage (preferences to votes and votes to seats), which attributes of the presented choice set voters weigh when deciding is important. This concerns particularly the relative importance of party and candidate factors in electoral competition (see Chapter 2).

A second research tradition focuses on empirically identifying the effects of electoral rules in a systematic fashion, relating electoral system characteristics to outcome variables at the political system level. Scholars have taken a descriptive and/or historical-empirical approach (Nohlen, 1986), an empirical-analytical approach at the macro level (Rae, 1967; Lijphart, 1990) or, more recently, an empirical-analytical approach focusing on comparisons at the subnational level for the sake of better identification (Snyder, 2001). This extensive literature generally focuses on five core questions, which reflect the theoretical work outlined above (comp. Grofman, 2016, 526f.): the influence of electoral systems/rules on voter turnout (this question is taken up again in Section 1.1.3 and Chapter 5); on proportionality of representation; on party proliferation; on party competition; and on the match between citizen preferences and policy. The empirical approach shows that electoral systems have diverse consequences. This begins with the fundamental dichotomy between plurality elections in single-member districts (SMD) and PR representation in multi-member districts: While the first are argued to promote single-party governments with high government accountability and accountability of politicians to their constituents, the latter system leads to coalition governments with more diverse and more proportional representation (Htun and Powell, 2013). Beyond the broad questions of government formation, accountability and proportionality, electoral systems have been shown to matter for a diverse set of outcomes, e.g. the provision of public goods (Sawat, 2011), the protection of citizen's rights (Cingranelli and Filippov,

2010) or the level of political corruption (Kunicová and Rose-Ackerman, 2005). Much of this comes down to what the strategic approach terms incentives for candidates and parties on cultivating a personal vote (Carey and Shugart, 1995) and to the accountability mechanisms voters can rely on (Anderson, 2007).

In this dissertation, two papers are concerned with the role of electoral institutions on the first stage (preferences to votes/seats), where they provide both theoretical and empirical contributions. Rudolph and Däubler (2016) (Chapter 2) take a closer look how voters' utility calculations theoretically differ in open-list proportional election (OLPR) and in single-member district (SMD) electoral systems and how individual accountability is affected at the vote and seat allocation stage. Empirically, these effects are tested in a unique setting, contrasting the performance of the same set of candidates after exposure in a political scandal under different electoral rules. Leininger, Rudolph, and Zittlau (2018) (Chapter 5) assess how the electoral calendar, especially concurrent (i.e. simultaneous) elections alter voter utility calculations and how this affects electoral participation. Empirically, this is tested for an electoral reform in the German state of Lower Saxony, leading to exogenous overlaps in the electoral cycles of mayoral and European Parliament (EP) elections (see Subchapter 1.1.3 for a summary).

### **Electoral systems and individual accountability (Chapter 2)**

In this thesis, Chapter 2 most directly takes up the effects of electoral systems, linking them to accountability<sup>4</sup> at the individual level. Holding public officials to account at the ballot box is a central, though difficult democratic task citizens face in modern democracies. Regularly, citizens seem to not succeed in this job: It is e.g. troubling that corrupt politicians are in many contexts reelected (de Sousa and Moriconi, 2013). As summarized by De Vries and Solaz (2017), three steps are necessary for accountability to work: The presence of information; the correct attribution of blame; a behavioral response. A behavioral response does not necessarily follow, though, even if information signals are correctly perceived and attributed: The literature has explained this both with the lack of viable alternatives and with an explicit or implicit trade-off with other choice characteristics. Turning again to the example of a corrupt politician, she may have ideological characteristics that especially voters on the edges of the political spectrum do not find in other options on the ballot (Charron and Bågenholm, 2016), and/or promise benefits for which the voter is willing to accept her behavior (see e.g. the vote buying literature, Mares and Young, 2016). This can explain reelection.

However, the perspective of rational choice institutionalism highlights that behavioral response is strongly shaped by the 'rules of the game', i.e. which incentives the political actors face (Norris, 2004). What is lacking, however, is a well-developed formal argument whether

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<sup>4</sup>I follow Fearon's 1999, 55 of accountability, whereby A is accountable to B if there is a common understanding that A is supposed to act on behalf of B and B has some form of empowerment to sanction or reward A for his (in)action and/or performance.

and when electoral rules shape citizen’s incentives to sanction a politician’s misbehavior. Additionally, as electoral systems do hardly lend themselves to experimental manipulation, the endogeneity problem needs to be solved to empirically assess how accountability works under different electoral rules.

With the work in Rudolph and Däubler (2016) (Chapter 2), we contribute to filling this gap. Theoretically, we contribute to understanding how electoral incentives shape the translation of preferences over candidates into votes and seats under different electoral systems. These are central for accountability enhancing or diminishing selection. We propose a model of voter decision making in SMD and OLPR electoral systems. Particularly, we assess the extent to which voters can trade off party and candidate features when deciding. We argue that OLPR is more conducive to individual accountability for two reasons: First, OLPR decouples party and candidate choice. In the party-centered political systems most prevalent in Western Europe, the combined party-candidate choice under SMD would allow only voters nearly indifferent between parties to sanction politician misbehavior. Under OLPR, on the other hand, a candidate cannot compensate for misbehavior by relying on the party label, as voters have substitute candidates from the same party on the ballot. Second, OLPR is more vote elastic, i.e. small changes in voting behavior are more consequential at the seat allocation stage. Hence, our core hypothesis follows: “For a given shock to candidate integrity, electoral punishment is more consequential under OLPR than under SMD” (Rudolph and Däubler, 2016, 750).

Empirically, the study draws on a within-system comparison and uses an exogenous shock to candidate integrity in order to identify causal effects. A unique setting in the German state of Bavaria allows us to test our hypothesis: Candidates for the Bavarian state parliament compete in two tiers. Hence, we can compare performance for the same candidate set under different electoral rules. Examining a political scandal exposing numerous politicians months before election day, we then show that scandal politicians are punished more consequential in the OLPR tier. In our case, the relative effect size of electoral punishment (estimated as punishment effect as the share of an average control group outcome) amounts to about 40% under OLPR, but only 5% under SMD. This implies that individual accountability is stronger in the former as compared to the latter system, which confirms our hypothesis. This result has broader implications: It does not only help to explain ex-post punishment, but as well illuminates the strategic incentives political actors face to cultivate a personal vote (and conversely the relative risk they take when engaging in corrupt behavior).

Of course, the arguments presented in Chapter 2 singularly address the question whether electoral systems differ in how they enable voters to hold individuals to account. These findings are in line with cross-country evidence that voting on candidates is associated with lower corruption levels (Persson, Tabellini, and Trebbi, 2003). However, when engaging in constitutional design, policy makers need to consider other electoral system effects related to politician and candidate behavior: E.g., it has been suggested that open-list systems and intra-party competition can as well increase the incentives for corruption as (expensive) can-

didate campaigns require financial resources (Chang, 2005). More research on these questions is needed, and will ultimately lead to a thorough understanding of the role of electoral institutions for the selection and accountability of representatives, benefiting electoral engineering (Norris, 2004).

### 1.1.2 Electoral Choice, the Role of Information and Judging Incumbents

*“Competitive elections create a relationship of formal accountability between policy makers and citizens – electoral rewards and punishments can be handed out on election day. Ideally, this formal accountability leads to better governance. [...] Yet, the] theoretical literature has taught us two major lessons: incentives are driven by the incumbents desire to impress the voters, and this desire often conflicts with the normative imperative to advance the voters interests.” (Ashworth, 2012, 184)*

#### Grasping voter decision making

Elections serve as a device that ties policy to electorate preferences, this is one of their central functions.<sup>5</sup> However, the relation between elections and policy can arise through two mechanisms: On the one hand side, elections allow voters to decide between different options, i.e. select good party/politician types<sup>6</sup>; on the other hand, elections allow voters to punish or reward incumbent performance (Dewan and Shepsle, 2011, 324ff). Both mechanisms lead to correlations of voting behavior with incumbent performance, which a large literature has empirically traced – on the macro level, e.g. economic performance, as well as on the individual level, e.g. corrupt behavior (see for reviews of the literature Healy and Malhotra, 2013; Ashworth, 2012; Anderson, 2007).

To explain whether and how citizens hold their representatives to account and/or select future leaders, a theoretical framework has to be provided, though.<sup>7</sup>

For this framework, I first discuss the rational choice approach as a benchmark. Here, voters (the demand side) and politicians (the supply side) are understood as rational actors, maximizing their expected utility given preferences and side constraints on a political market (a detailed discussion is given in Thurner, 1998). This leads to the famous conceptualization of the voting process by Downs (1957, Chapter 3), the ‘basic logic of voting’: Voters derive utility from the future behavior of political actors. Assuming a choice situation between two

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<sup>5</sup>Next to aggregating preferences/information (Dewan and Shepsle, 2011).

<sup>6</sup>Following Fearon (1999, 59) I understand ‘good type’ as a party/politician sharing voter preferences, would be working to implement these preferences (i.e. is not corruptible), and is competent to implement optimal policies from the voter perspective.

<sup>7</sup>See for an extensive review of the grand schools in voting behavior and important current topics in electoral research Falter and Schoen (2014).

parties  $a$  and  $b$ , a voter will have to calculate the party differential,  $E(U_{t+1}(a)) - E(U_{t+1}(b))$ , i.e. the expected utility a voter will derive from party  $a$  as opposed to party  $b$  being in power in the future time period ( $t + 1$ ). While conceptually simple at first sight, this proposition entails important substantive questions, addressed by a whole subsequent research program: How voters can derive expectations (Downs (1957, Chapter 3)), what role information and the costs of acquiring information play (Downs, 1957, Part III), which cues voters then use (Kahneman, 2003; Lupia, 2016), what the size of this party differential has to be to be worthwhile to take action (and vote) (Riker and Ordeshook, 1968; Thurner and Eymann, 2000), how this calculation changes for multi-party elections and/or with multiple dimensions of decision making (Enelow and Hinich, 1984; Davis, Hinich, and Ordeshook, 1970). It as well has implications for the optimal strategy of parties and all other political actors – leading to the famous spatial model of politics, the Median voter theorem and the subsequent discussion thereon (see for an overview Dewan and Shepsle, 2011; Thurner, 1998).

This economic theory of voting stands in contrast to the social-psychological approach of the Michigan School (Campbell, Converse, Miller, and Stokes, 1960), highlighting long term party identification (which itself developed in social context) and short term evaluations of candidates and issue positions as individual-level determinants of vote choice. This approach is popular, as it flexibly allows the incorporation of candidate valence evaluations and issues into the voting process, together with additional background variables (e.g. voter personality or institutional structure) that affect the voting procedure (Arzheimer, 2007).<sup>8</sup> However, the Michigan approach has been criticized for viewing voters as ‘fools’ (Key, 1966), lacking necessary knowledge and ideological structure (Healy and Malhotra, 2013, 286).

Discussing the assumptions underlying Down’s contribution<sup>9</sup>, the economic theory has been extended to incorporate such aspects as well: For example, policy motivation of candidates (implying a commitment problem on the side of politicians) (Fiorina, 1990) or candidate valence as separate component of voter’s utility function<sup>10</sup> have been fruitfully incorporated into the spatial approach (see Dewan and Shepsle, 2011; Thurner, 1998). Lately, even a

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<sup>8</sup>It as well stands in contrast to the Columbia School (Lazarsfeld, Berelson, and Gaudet, 1968): This approach explains voting behavior through sociological determinants, i.e. socio-economic status, religion, geo-location; it as well highlights processes of information acquisition and political communication, which voters are argued to apply selectively, confirming their structurally determined preferences. Overall, the (micro-)sociological approach of the Columbia School highlights the role of social structure, and the social groups a voter is embedded in. This links to a fourth theoretical approach, the macro-sociological approach (Lipset and Rokkan, 1967) highlighting the role of social cleavages for voting behavior (comp. Pappi, 1977; Schmitt-Beck, 2007a,b).

<sup>9</sup>Most importantly, the spatial theory of voting with its policy convergence or median voter theorem (Downs, 1957; Hotelling, 1929; Black, 1958), by which competition for votes leads to a social optimum as policy offers by parties align with the preference of the median voter, relies on several assumptions: rational voters with exogenous (single-peaked) policy preferences; office-seeking and vote-maximizing parties; sincere voting; parties as homogenous actors; plurality elections; complete information; democratic competition (comp. Dewan and Shepsle, 2011, 313; Thurner, 1998, Chap. 2.2)

<sup>10</sup>I.e. voters do not only care about policy; this aspect will be picked up again in Chapter 4 and Subchapter 1.1.2 when discussing the incumbency advantage and is part of the model in Rudolph and Däubler (2016).

unifying approach for both the spatial theory and the Michigan School has been proposed (Adams, Merrill, and Grofman, 2005; Healy and Malhotra, 2013).

With this background, one of the central questions can be addressed that the rational-choice approach opened up with great clarity: the forward looking nature of the democratic selection problem and how this links to questions of political accountability. Healy and Malhotra (2013) discuss how the rational choice approach and the Michigan approach lead to three models of voting behavior that help understanding empirical patterns that follow a reward-punishment logic.

Starting point for the first model is again Downs (1957), who discusses whether and how voters derive the party differential and act upon it. Downs (1957, Chapter 3) highlights the role of current performance evaluations for expectations on the future, weighted with a ‘trend factor’. Past performance would only be used to decide on ties (a citizen then votes for the current incumbent and her policies if she exceeds an idiosyncratic performance standard). Overall, the conceptualization by Downs leads to voting based on current performance of the incumbent and counterfactual current opposition performance, projected to the future, where extrapolation from past performance is used only in rare circumstances – the task of the citizen is clear: to select a future government, and information from past and present is only useful as easily accessible cue for future expected utility. Closely linked to Downs understanding, voting hence can be seen as manifestation of a selection mechanism (Fearon, 1999). For this, voters have to use the available knowledge on the portfolios of politicians and parties. Fearon (1999) highlights the extrapolation from past behavior, though: An incumbents’ action in the past carries cues for her future behavior and as it does so, voting based on incumbent performance during the term serves not only as punishment device from a retrospective ‘sanctioning view’, but from a prospective view as a base for ‘selecting good types’ (Thurner and Pappi, 1998).

This first perspective emphasizes that politicians receive a mandate to act upon, implementing policy preferences of citizens; however, given the difficulty of the task of selection, citizens might just resort to rewarding and punishing past actions to generate accountability and induce good behavior (Manin, Przeworski, and Stokes, 1999). Therefore, second, Downs approach is modified in the settings discussed by Key (1966); Fiorina (1981) and Ferejohn (1986), which highlight retrospective evaluations as such: Key (1966) emphasizes an understanding of the voting process as judgment over past policies. He takes a strong empirical focus of how economic performance affects incumbent electoral performance, whereby non-aligned citizens induce accountability by rewarding well-performing politicians and deselection of under-performers. Fiorina (1981) focuses on a reward-punishment mechanism by which the voting decision is based on past incumbent evaluations, as ultimately voters care about policy implementation and hence only performance evaluations can help in adequately assessing future performance. Ferejohn (1986) focuses on the more general problem how voters can incentivize politicians to implement their policy-preferences, as self-interested actors may not act upon past policy promises. This opens up a view on the nature of the democratic

elections as a principle-agent relationship, featuring prominently in modern accountability models (Ashworth, 2012).<sup>11</sup>

Third, voters may decide based on cues (psychological view) – as emphasized in the Michigan school (Healy and Malhotra, 2013), they have restrictions in taking up and processing information (Lupia 2016: Chapter 1). There always is a tradeoff between knowledge necessary for competent decision making and the restrictions on information collection. Cognitive shortcuts and heuristics then help in decision making (Kahneman, 2003). This is already indicated in Downs (1957, 258) observation that, given the low probability of changing the outcome “it is irrational to be politically well-informed because the low returns from [information] data simply do not justify their cost”, hence rational actors will only rely on “free data acquired accidentally”, which in turn is likely biased. Consequently, in the economic voting literature it is not so clear how well voters are able to trace the responsibility of governments and politicians due to cognitive and institutional barriers (Anderson, 2007).

Rudolph (2017b) (Chapter 3) takes up the question whether voters are forward looking, backward looking or both, and whether they are rational in doing so. For this, I use the case of two centennial floods in two German river systems, each shortly before general elections. I argue that these suddenly occurring external shocks that incumbents have to handle provide voters with new information and test several hypothesis on voter decision making.

### **Voting behavior and incumbent performance: Retrospective, prospective, or both? (Chapter 3)**

Testing theories on how voters decide is empirically challenging. Ex-ante citizen mandate decisions and ex-post electoral voter judgements as well as incumbent and challenger behavior taking anticipated voter response into account theoretically co-occur and influence each other. It is hence difficult to disentangle forward looking, backward looking and cue-based decision making of voters.

In this spirit, a growing literature analyzes external shocks, especially natural disasters. These extreme events are argued to give voters information on how to evaluate the government: As Cole, Healy, and Werker (2012) show in their model, a drop in personal welfare following natural disasters can with some likelihood be attributed to incumbents actions and could therefore lead to vote losses. They confirm this empirically for extreme whether events in an Indian setting. Similarly then, one can expect that beneficial policy at the individual level should lead to vote gains. Healy and Malhotra (2009) find nuanced results: They argue that voters are myopic in that they react to beneficial personal-level disaster relief, but not to collective disaster preparedness expenditures. Therefore, on the one hand, voters are supposed to punish incumbents for disaster damage. On the other hand, the effective management of adverse events is expected to lead to vote gains for incumbents as long as their policy

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<sup>11</sup>Recent theoretical work even points to potential trade-offs between effective accountability and electoral selection (Ashworth, Bueno de Mesquita, and Friedenber, 2017).



response is positively assessed by the electorate. Numerous studies have provided empirical evidence for this latter claim: For example, Bechtel and Hainmueller (2011) estimate an average increase of the incumbent party's vote share of 7 percentage points in affected districts of the German 2002 Elbe flooding, which they explain with "voter gratitude" following generous disaster recovery funds and successful disaster management. This is one example in a growing literature on natural disasters and incumbent performance (see Heersink, Peterson, and Jenkins, 2017; Cole, Healy, and Werker, 2012; Lazarev, Sobolev, Soboleva, and Sokolov, 2014; Carlin, Love, and Zechmeister, 2014; Eriksson, 2016; Achen and Bartels, 2004, 2016; Fowler and Hall, 2016; Malhotra and Kuo, 2008; Healy and Malhotra, 2009; Bodet, Thomas, and Tessier, 2016; Flores and Smith, 2013).

Still, the findings from disaster studies are largely representations of a very general mechanism in the literature: They test the 'retrospective voting' view in the spirit of Fiorina (1981) or Key (1966) with the benefit of plausibly exogenous exposure to economic grievance/benefits. Additionally, they test whether voters apply 'blind retrospection' (Achen and Bartels, 2004, 2016), i.e. show signs of seemingly irrational or cue-based behavior. However, the understanding of voters' choice as prospective in modern accountability models (Ashworth, 2012) would imply, first, that voters actually take into account not only incumbents' behavior, but as well counterfactual challengers' actions and, second, that past information is relevant primarily for the evaluation of an incumbents' 'type', and therefore contingent on counterfactual expectations of his behavior (Fearon, 1999).

Chapter 3 takes up these questions more broadly for the case of a repeated natural disaster in Germany, the centennial floods in the Elbe and Danube river systems in 2002 and 2013, both occurring right before elections at the federal and state level. To distinguish between retrospective and prospective choice I propose especially two hypotheses:

First, when voters reward/punish incumbents for their actions, disaster affectedness should be the primary driver of electoral response. However, for prospective choice it would be enough to observe disaster management and thereby gain new information on expected incumbent performance in the future. I therefore propose to test whether spill-overs to geographically adjacent regions are present in the cases at hand. While a positive electoral response would be expected in disaster affected regions from both pro- and retrospective models, spill-overs should follow from a prospective view only. I find evidence for this in all cases.

Second, when disaster and disaster response could be treated constant over cases, differences in voter response would be a sign that the information signal 'beneficial policy' is not interpreted in isolation by voters. Especially, if incumbent and potential challenger can be expected to pursue the same policy, one would empirically not expect a change in electoral outcomes from a 'selection' perspective. I interpret this as holding the economic shock constant, but analyzing it under different informational side-constraints. This line of reasoning has not been taken up theoretically or analyzed empirically in the disaster literature but would be an important test for the question whether voters decide backward or as well

forward looking. Indeed, I find evidence that electoral gains after effective flood relief decline with repeated exposure.

These findings provide evidence that voters are not merely following a retrospective reward-punishment logic. However, note that this does not invalidate the retrospective voting and/or psychological view – specific findings from the case indicate that all three theoretical models contribute to understanding voting behavior: Particularly, I find indications for substantial decay effects in line with myopic voters (Healy and Malhotra, 2009): Even in directly affected areas, the floods lose relevance very quickly after flood occurrence and incumbent evaluations return back to their pre-flood levels. Additionally, flood effects are stronger in directly compared to indirectly affected areas, indicating that reward-punishment mechanisms play a role after all.

#### **Spill-overs of an incumbency advantage: Candidate characteristics matter (Chapter 4)**

What are threats to accountability? One institutional effect that has been studied extensively is the incumbency advantage. These difficult to measure (Erikson and Titiunik, 2015; Gelman and King, 1990) effects are reported from many empirical contexts. It is argued that due to better access to (state) resources, informational advantages, focused media coverage, access to pork politics (Ansolabehere, Snyder, and Stewart, 2000; Levitt and Wolfram, 1997; Lopes da Fonseca, 2017), or increased familiarity (Abramowitz, 1975), incumbents enjoy an electoral bonus over their competitors. Strategic candidate entry and exit is argued to be an important part of the story as well (Ashworth and Bueno de Mesquita, 2008; Hall and Snyder, 2015).

An incumbency advantage can endanger accountability, though (see Stonecash, 2008, Chapter 1): If incumbents are relatively safe, and high-quality challengers potentially deterred, incumbents need not be similarly reactive to changes in public opinion, are less likely to pay close attention to their constituents, less likely to be held to account for misconduct, and overall both responsiveness of incumbents and the match of citizen preferences and policy may decrease. Additionally, political campaigns might center more around building personalities and less on issues and policies. Recent scholarship has, drawing on various causal inference methods, established that incumbency effects are likely present in many democratic contexts (Lee, 2008; Ansolabehere, Snyder, and Stewart, 2000; Lopes da Fonseca, 2017).<sup>12</sup>

While these arguments have mostly been developed for majority/plurality electoral systems, incumbency (at lower government levels) has been noted to be an element of ‘personal vote earning attributes’ under OLPR (Shugart, Valdini, and Suominen, 2005), and does spill-over to the PR party-vote in mixed systems (Hainmueller and Kern, 2008).

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<sup>12</sup>Examples from other settings highlight that institutional context is important for incumbency effects to manifest, though (Klašnja and Titiunik, 2017). Additionally, a literature question is whether these effects are growing in size over years, which is unclear (Stonecash, 2008). It has also been shown that (personal) incumbency effects produce spill-overs to concurrently held races (Hainmueller and Kern, 2008).

Rudolph (2017a) (Chapter 4) adds to the understanding of these spill-overs in a mixed-system with an SMD and OLPR tier. As argued in Chapter 4, spill-overs from incumbency can extend across time and electoral level: Incumbents for one electoral arena can garner an electoral bonus if standing in different elections. In this case, I tested whether a mayor standing in a region-wide district for the Bavarian state parliament election garnered an electoral bonus in his home municipality, and in the tier where he appeared on the ballot. This would be best explained with a familiarity bonus and is consistent with psychological theories of memory recollection in preference formations (Spälti and Brandt, 2017). It is as well consistent with a specific ‘personal-vote seeking advantage’, as having shown local service in local office makes this candidate likely to effectively work in favor of these constituents again. The incumbency effect likely induced voters on the margin between one of the competitor parties and the mayoral party to select him and his party with their second, regional ballot vote. This gave the (opposition) SPD a substantial local electoral boost for the statewide race of around 3-4 percentage points. The causal mechanism is confirmed when analyzing individual voter transitions and voter transition motives (Thurner, Klima, and Rudolph, 2017; Küchenhoff, Shao, and Alkaya, 2017).

This transferability of an incumbency effect adds new interpretations to the incumbency literature: As in the case at hand, it allows opposition parties to counteract the incumbency advantage the governing candidates and party enjoy at least partly by fielding politicians from other electoral arenas. Future work could investigate where parties field such candidates with proven local ties, and how challenger quality impacts these decisions. As well, changes in incumbency status as an example of candidate characteristics can be an important explanation for voter transitions between parties and elections in OLPR/PR systems (Klima, Küchenhoff, Selzer, and Thurner, 2017).

### 1.1.3 Voting Benefits, Voting Costs and the Causes and Consequences of Electoral Participation

*“[D]emocratic responsiveness depends on citizen participation, and equal responsiveness depends on equal participation” (Verba, 1995, 2).*

#### Why electoral participation is relevant and how it can be explained

Democracy, as ‘rule by the people’,<sup>13</sup> requires not only direct linkages between representatives and represented, but as well broad participation for responsiveness<sup>14</sup> of decision makers to electorate preferences. Principle-agent models (see Gailmard, 2014) imply, as representatives (agents) are electorally accountable to their constituents (principles), that only citizens that

<sup>13</sup>From greek ‘dēmokratía (direct democracy) and greek ‘dēmos’ (people) and ‘kratós’ (rule/force).

<sup>14</sup>Government responsiveness is understood here as “adoption of policies that are signaled as preferred by citizens” (Manin, Przeworski, and Stokes, 1999, 9).

turn out to vote matter to the principle. The interests of abstainers will thus be disregarded in the policy making process. A similar insight follows from a selection perspective, if voters choose in a forward-looking way politicians that share their preferences (Mansbridge, 2009).<sup>15</sup> This will be consequential with respect to policy when non-voters have, on average, different preferences compared to voters (Besley and Burgess, 2001).

Thus, when theoretically inquiring vote choice, and empirically investigating determinants of accountability and democratic quality, electoral participation is a fundamental aspect to study. A second set of papers in this dissertation hence investigates causes and consequences of electoral participation.

Turnout in Western democracies is declining since the 1970s (Kostelka, 2017), which has led to increasing concerns that elections under low turnout imply biased representation and lead to public policy in favor of only a subset of the electorate (Lijphart, 1997). As those who are less likely to turn out with voluntary voting can be expected to be citizens with lower average social status (Tingsten, 1937), low turnout levels potentially disadvantage disadvantaged citizens even more. This is conceptually an old and well-known problem of democratic decision making (Buchanan, 1974). Scholarship has since long studied who does (not) participate and how changes in voting benefits and costs relate to participation, representation and public policy (see for reviews of the empirical literature Cancela and Geys, 2016; Blais, 2006). As well, a broad empirical literature has investigated the counterfactual question whether full participation would make a difference for electoral outcomes, government formation, and policy, drawing on various methodological approaches: Observational studies come to mixed conclusions (see e.g. the summary in Lutz and Marsh, 2007). Turnout can be influenced by some, though not all institutional designs, and does not matter under all conditions and not always in the same direction. Simulations and modeling of full participation based on survey data (e.g. Kohler, 2011; Kohler and Rose, 2010; Leininger and Heyne, 2017; Bernhagen and Marsh, 2007) have helped to explore under which conditions turnout can make a difference. This research has indicated that turnout can affect electoral results, but matters only in rare cases for government formation (for the German case, see Kohler (2011)). It is unclear, though, whether the preferences of non-voters can be adequately assessed through survey methodology (misreporting and systematic non-response are issues). Additionally, the turnout decision is inherently related to electoral choice (Thurner and Eymann, 2000) and hence aggregate turnout and aggregate electoral results are potentially endogenous. A recent wave of quasi-experimental studies therefore approaches the effect of turnout in concrete, context depended institutional settings with high internal validity. Closest to the question of full participation is the quasi-experimental literature on the turnout, electoral and policy effects of compulsory voting. This literature has e.g. shown that turnout increases with the institution of compulsory voting, that electoral outcomes change if the baseline turnout rate (i.e. the average participation rate under voluntary voting) is not too high, and that outcomes and

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<sup>15</sup>An important difference between these two views is whether turnout in the past (that selected the incumbent) or future turnout anticipated by the incumbent matters for representation.

policy shift in tendency towards left-wing parties and policies (e.g. Ferwerda, 2014; Fowler, 2013; Bechtel, Hangartner, and Schmid, 2016).<sup>16</sup>

This gives rise to scholarly and policy advice to seek institutional designs that increase electoral participation (Lijphart, 1997; Tillmann and Reichmann, 2016). Lijphart (1997) e.g. argues specifically that, for fairness concerns and to reduce bias in representation, societies should use all available means to maximize turnout. While compulsory voting is a prime candidate to achieve this end, it is not feasible in many contexts.<sup>17</sup> Therefore, interest in less forcing institutional designs has sparked recently. Lijphart (1997) recommends amongst others concurrent elections (increasing benefits) or reducing explicit and implicit voting costs (e.g. registration costs, opportunity costs of voting). Three papers in this dissertation contribute to these questions. First, the effect of compulsory voting on electoral participation for the understudied case of two second-order elections is investigated (Chapter 5). Second, the particular effect of natural disasters on participation via changes in voting costs and changes in social capital is investigated (Chapter 6). Third, whether turnout makes a difference, here the electoral consequences of increased turnout, is studied for the example of the United Kingdom EU membership referendum (Chapter 7).

In order to be able to discuss the foundations and implications of these questions, a theoretical understanding of the voting process is necessary.

Again, starting with Downs, a rational voter would always vote unless she is indifferent between parties given voting is costless (Downs, 1957, 50).<sup>18</sup> Introducing costs to the voting process, Downs (1957, Chapter 14) acknowledges that (mild) voting costs lead to abstention. Importantly, he first notes that voting costs will bear larger on citizens with lower socio-economic status, implying a potential disenfranchisement of this part of the population. Second, he notes that variations in costs will have a large influence on representation. Third,

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<sup>16</sup>This is at least the case as long as the sanctions attached to non-compliance with compulsory voting are not decreasing for citizens with lower socio-economic status (Cepaluni and Hidalgo, 2016).

<sup>17</sup>See Lijphart (1997) for a discussion of this. Additionally, the argument of increasing turnout via explicit force through compulsory voting institutions has attracted critique. Saunders (2012) e.g. argues that high turnout is not necessary for democratic legitimacy – as Saunders (2012, 307) puts it, “higher turnout is not necessarily democratically better than lower turnout [and] the mere opportunity to vote is sufficient to realise the value of democracy.” See Scully, Jones, and Trystan (2004) for an empirical example that low turnout can simply imply apathy towards the political realm by some citizens, but not opposition towards the political system as such. Particularly, Saunders (2012) argues that disproportional participation is not necessarily problematic if underrepresented groups just have less stake in the outcome and thereby participate less. He agrees that as soon as it is disadvantaged citizens that are less likely to participate it would indicate a necessity to increase turnout by institutional change. Turnout would then be instrumental to achieve greater social justice.

<sup>18</sup>Downs actually differentiates between parties offering different platforms (indifference between those leads to abstention); or, in case they offer similar platforms, a voter would turn to an incumbent performance evaluation relative to his ideal, comparing government performance with other past or hypothetical governments; abstention occurs if this assessment is neither good nor bad, i.e. if the voter is indifferent to a new government by the incumbent.

he highlights that an additional benefit from voting<sup>19</sup> is necessary to explain the empirical turnout levels observed in modern democracies – otherwise voting will not be rational, as the likelihood of being pivotal (i.e. making or breaking a tie) by casting one vote are just too small, and otherwise the act of voting will have no bearing on an actors future utility.

Downs suggestion, the long-run participation value for sustaining democracy, does not depend on any individual’s participation, however, as sustaining democracy is a public good and the individual voters’ contribution to it is just too small (Olson, 1965) – in the rational framework outlined above, voting therefore should not occur at all. This gives rise to the paradox of voting, whereby rational choice theory has difficulties in explaining why people turn out. This has since been addressed by numerous scholars. Riker and Ordeshook (1968) formalized the problem and carried on the solution suggested by Downs. They include a ‘D-term’, capturing non-instrumental benefits of voting, to the equation. Other theory-immanent fixes to the paradox of voting are summarized in Dowding (2005).<sup>20</sup> As highlighted by Thurner (1998, 54f.), even if the rational choice approach has difficulty explaining voter turnout, the theory is well placed to explain the choices voters make, what the (anticipated) consequences of/for party and candidate behavior are, and, most importantly, what we can expect with marginal changes in the parameters (e.g. electoral closeness or a decreasing party differential) (Barry, 1970; Grofman, 1993). Additionally, the rational choice approach allows to disentangle theoretical mechanisms that lead to the same observed outcomes, which lends itself to further investigation (e.g. following the spatial model, alienation and indifference may both lead to abstention (Thurner and Eymann, 2000)). In this dissertation, the focus will lie on explaining changes in parameters. I therefore follow the canonical approach of Riker and Ordeshook (1968), explaining turnout by adding a ‘D-term’ in a linear-additive cost-benefit calculus of voting.

On the individual level, assume the binary participation  $T$  of a voter in an election is determined by

$$T = \begin{cases} 1 & , R > 0 \\ 0 & , \text{else} \end{cases} , \text{ where} \\ R = p|B| + D - C.$$

An individual participates if her total expected utility (‘reward’  $R$ ) from the vote in an election exceeds zero. The utility from the vote is composed of four parameters: First, the

<sup>19</sup>He calls this ‘long-run participation value’ from voting, together with the realization that democracy cannot be sustained if no-one votes; he later discusses social prestige for voting and/or feelings of guilt for non-voting, dismisses them from consideration, though, as they are non-political factors outside the scope of his model.

<sup>20</sup>The most important arguments are: that voting costs are actually very low; that the ‘B-term’ comprises global utility calculations (social preferences), and is therefore much larger than previously suggested; that the p-term is systematically misperceived by voters and/or should be calculated differently following game-theoretic models; that voters are actually not maximizing expected utility but follow the minimax regret criterion.

expected utility differential  $B$  she gains from her election specific favorite outcome  $a$  over the alternative  $b$ . We can understand  $B$  as  $B = E(U_{t+1}(a)) - E(U_{t+1}(b))$  in the Downsian framework. This (party) utility differential is multiplied with the subjective probability of being the decisive voter  $p$ . In any modern democratic setting, while  $p$  will depend on the closeness of the race and the size of the electorate, it will in most cases be very small. For US presidential elections, Riker and Ordeshook (1968) e.g. estimate it to  $10^{-8}$ . What is new now is the explicit formulation of a  $D$ -term entering positively, which encompasses utility gained from the act of voting as such (irrespective of the electoral outcome). Finally, the cost term  $C$  contains various costs a voter has to bear for the voting act and its preparation, e.g. information costs, transportation costs, opportunity costs for the time spent. Electoral participation will then depend on whether benefits outweigh these costs.

As the probability of casting a vote that changes the outcome (a vote that makes or breaks a tie) is very small, a positive reward, and hence individual turnout, can be most readily explained by the  $D$ -term. This  $D$ -term encompasses, first and foremost, the affirmation of partisan preferences. Additionally, voters potentially derive satisfaction from compliance with the ethic of voting, affirming allegiance to the political system, as well as efficacy, or even satisfaction from decision making as such (Riker and Ordeshook, 1968, 28). This list is not exhaustive – later, these components have been picked up as ‘expressive benefits of voting’ (Fiorina, 1981; Brennan and Lomasky, 1993) (comp. detailed discussion in Thurner, 1998, 48ff.; Thurner and Eymann, 2000).

Of course, the voting process could be conceptualized much broader from the start: For example, Thurner (2009) and Thurner and Eymann (2000) address the problem whether voters decide simultaneously or in a consecutive order when considering vote choice and participation/abstention. The papers in this dissertation take the more standard approach of either investigating vote choice or participation, as this is directly related to the empirical research designs chosen. When assessing participation, the papers in Chapters 5, 6 and 7 all draw on the Riker-Ordeshook-Framework to theoretically derive a hypothesis and interpret results. Although more complex theories of electoral participation have been developed (e.g. Feddersen and Sandroni, 2006; Myatt, 2015), for the problem-driven research approach taken in this dissertation, the Riker-Ordeshook-Framework serves its purpose very well: How can we expect behavior to change, if initial endowments and/or incentives change? The approach taken here is a comparison of states; theoretical changes in parameters (e.g. increase in voting costs; increase in the utility differential; differences in pivotality due to electorate size; etc.) lead to expectations on changes in aggregate turnout, which is then empirically tested.

### **Natural disasters, voting costs and electoral participation (Chapter 6)**

Chapters 6 and Chapter 7 study the relationship between participation and the costs and benefits of voting in more detail. In particular, these studies contribute to the question to what extent voting costs and (external) increases therein lead to disproportionate electoral participation.

Rudolph and Kuhn (2018) (Chapter 6) study whether natural disasters in- or decrease political participation. Our research has a threefold contribution: We contribute to understanding the outcomes of the 2002 and 2013 federal election, where the flood disasters right before elections in both years impacted the electoral campaigns of all parties. As flooding has been suggested to have affected electoral results (Bechtel and Hainmueller, 2011; Pappi, Shikano, and Bytzeck, 2004), our study contributes to understanding the potential mechanism behind (mobilization vs. persuasion). As well, we connect to a small, but growing empirical literature (Fair, Kuhn, Malhotra, and Shapiro, 2017; Sinclair, Hall, and Alvarez, 2011; Bodet, Thomas, and Tessier, 2016; Remmer, 2014; Chen, 2013; Kauder and Potrafke, 2015) that investigates how electoral participation and natural disasters are connected. This is substantially important in its own right, as the expected increase in natural disasters occurrence in coming decades (IPCC, 2013) will render an understanding of the politics of high water and other weather extremes more important. Finally, this literature as well reveals more broadly how and which voters react to these shocks, and how this affects accountability channels, as natural disasters can be interpreted as an economic, psychological and informational shock for affected citizens (Ashworth, Bueno De Mesquita, and Friedenber, 2017; Gailmard and Patty, 2014).

For the case at hand, we propose two competing theoretical mechanisms: Natural disasters might positively affect turnout, if they build social capital (due to wide-spread community self-help efforts). Social capital would positively affect the D-term<sup>21</sup> and hence disaster occurrence could translate into increased aggregate turnout. The opposite would follow if voting costs increase due to natural disaster damage at the individual level – this concerns both the direct and opportunity costs of voting. Drawing on flood exposure from the 2002 and 2013 Elbe and Danube floods, and a difference-in-differences approach with entropy balancing, we find evidence for the latter hypothesis: Flood municipalities see a moderate decline in turnout, and particularly so those municipalities that were hit relatively harder.

In a broader sense, this indicates that natural disaster victims, and more broadly economically disadvantaged citizens, have less incentives to participate. This is all the more remarkable as for these citizens (future) natural disaster preparedness, medium-term relief, and reconstruction were on the ballot. If this pattern holds more generally, this indicates that in a PR electoral system geographically concentrated shocks to electoral participation will lead to a systematic under-representation of affected citizens. It is therefore particularly relevant to reduce the costs of voting in such environments (e.g. via increased administrative flexibility in registration procedures or the postal voting application process) (Stein, 2015).

### **Electoral stakes and unequal participation in the Brexit referendum (Chapter 7)**

Abstention by certain groups might reflect the extent to which they feel affected by a decision and as such would not be problematic – as increased weighting of those with higher stakes in a

<sup>21</sup>E.g., it would affect the ‘satisfaction from affirming allegiance to the political system’ (Riker and Ordeshook, 1968, 28)



decision making process may actually be desirable from a normative point of view (Brighouse and Fleurbaey, 2010). This is particularly so with referendum decisions, where single issues are decided upon and citizens do not per se delegate legislative/executive decision making for a certain period of time. However, whether citizens are subjectively concerned is not necessarily reflecting the degree to which these citizens' interests are affected by the election (Saunders, 2012).

Chapter 7 investigates this question for the turnout increase observed with the European Union membership referendum in the United Kingdom (UK), drawing on an instrumental variable design with rainfall as a shock to voting costs. The paper contributes first of all to understanding the Brexit decision, where research is just beginning to build<sup>22</sup> and findings on the role of turnout are contradictory. Additionally, the paper contributes more broadly by helping to understand how turnout affects referendum outcomes through differential mobilization. Theoretically I argue that this is especially important for referendum decisions, where individual policy preferences and participation incentives can be more easily correlated compared to parliamentary elections.

Empirically, I first of all show that the increase in turnout was heterogeneous with respect to Leave/Remain support. Among occasional voters marginal to rainfall those leaning towards Leave were more likely to participate. Pre-referendum survey evidence supports the picture that occasional voters were not per se split on the referendum issue – but that turnout intention was higher among latent Leave supporters, and the more so the closer referendum day came.

This evidence highlights that turnout matters for electoral outcomes and policy decisions. Groups of citizens that do not vote are not represented by elected officials. In the case of referendums their uncounted opinion is of course neglected directly. In previous research, it has especially been highlighted that the preferences of voters and non-voters likely differ. As this research has shown, voters with lower participation likelihood may even have on aggregate similar partisan preferences, but their mobilization potential differs depending on certain issues on the ballot. This implies differential participation. In the end, policy may then not reflect the preferences of all enfranchised citizens.

### **Election timing and the benefits of participating (Chapter 5)**

Chapter 5 investigates to what extent institutional reforms can change turnout incentives and how changes in electoral benefits relate to the turnout decision. While compulsory voting will in most contexts be the most effective institution to close the participation gap (but see Cepaluni and Hidalgo, 2016), the democratic case for compulsion is debatable – citizens have a right to abstain (Lever, 2010). Lijphart (1997) suggests other institutional features to maximize turnout, among them holding less salient, i.e. second-order, elections concurrent to first-order elections.

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<sup>22</sup>See e.g. the issue 19:4 (2017) in the *British Journal of Politics and International Relations* (Henderson, Jeffery, Wincott, and Wyn Jones (2017))

The fact that turnout in second-order elections increases when combined with ‘more important’ elections is not surprising. It is a well-established finding in the literature (Geys, 2006): Cross-country evidence stems from a wide range of elections, be it legislative and presidential races (Stockemer and Calca, 2014) or European, local and national elections (Schakel, 2011) just to name two examples. Going beyond Lijphart’s proposition, Leininger, Rudolph, and Zittlau (2018) (Chapter 5) study, however, the effect of combining two second-order elections, where turnout is relatively low in both instances.

We contribute to the literature especially in two regards: First of all, we add to a small literature that provides causal estimates whether and to what extent concurrent second-order elections increase turnout (Fauvelle-Aymar and François, 2015; Fukumoto and Horiuchi, 2016; Garmann, 2016). Counterfactual turnout rates create clear incentives for strategic election timing, hence electoral calendar decisions are likely correlated with aggregate voting behavior (Hartney and Nickerson, 2012). The recent literature takes the debate an important step forward by seriously addressing the endogeneity problem. It has consistently shown that concurrent local elections increase turnout by about four percentage points. This literature has not yet causally investigated, though, what effects can be expected with two second-order elections at vastly different levels of government. We therefore investigate turnout effects for European Parliament and local elections. Secondly, we are the first to investigate mechanisms that lead to the hypothesized increase, particularly whether effect size systematically varies with the expected  $p$ - (competitiveness) and  $D$ -term (non-instrumental benefits of voting) at the municipality level.

We theoretically argue that the benefits of voting with combined elections should increase in the Riker-Ordeshook-Framework. Both the  $pB$ - and the  $D$ -term will be higher for an average citizen. On the other hand, at least parts of the voting costs remain constant over elections. Thus, concurrent elections have “economies of scale” (Aldrich, 1993, 261) to the voter. At least some citizens that would not vote in a singular election are now pushed above their participation threshold. We therefore expect turnout to increase when combining two second-order-elections.

We test this empirically for the effect of concurrent local elections on EP election turnout in Germany. Drawing on an institutional reform introducing staggered local mayoral elections in a quasi-random fashion across Lower Saxonian municipalities, we are able to show that concurrent mayoral elections can increase EP turnout by about 20 percent (10 percentage points). The effects are most pronounced in small villages and competitive concurrent elections. This indicates that both increases in the combined  $pB$ -term and the combined  $D$ -term contribute to explaining this effect. We as well provide evidence for the external validity of our findings, drawing on fixed-effects comparisons of state-level EP-turnout with and without concurrent local elections for the 1979-2014 period. Our findings lend support to the argument that the concurrency of elections at different government levels increases turnout much more substantially than the concurrency of two local elections.

As well, our results are relevant for the debate on causes of the observed turnout increase in Germany, where the personalization of the debate has been discussed as a reason.<sup>23</sup> Our estimates imply that more than 80% of the much noticed increase in European Election (EE) turnout in Germany between 2009 and 2014 (from 43.3 to 48.1 percent) was likely due to the introduction of concurrent local elections in several German states (3.4 percentage points).

Finally, this last result implies as well that a substantial part of the voting population was potentially predominantly interested in local, not European policy, when casting their ballot for the EP. Survey evidence from Lower Saxony supports this findings.<sup>24</sup> This, together with the findings from the previous two subsections, links to an ongoing debate: There is a growing economic literature that asks whether the participation of voters marginal to the electoral process, i.e. those less interested in politics, might lower the average quality of vote choice. Then, low turnout could as well constitute a “blessing in disguise” (Rosema, 2007). Formal models can show that in some contexts voluntary participation Pareto-dominates compulsory voting (Krishna and Morgan, 2011; Börgers, 2004). Particularly, Hodler, Luechinger, and Stutzer (2015) present a model that exemplifies a likely trade-off involved: Lower voting costs reduce the bias in representation, but potentially increase bias from interest group politics – they argue that uninformed voters who participate with lower costs are as well those likely to be swayed by special interests. Empirically, they provide evidence that the introduction of postal voting in Switzerland (i.e. lower costs) is associated with on average less knowledgeable voters and, as a suggested consequence, lower welfare expenditure. This is in line with survey evidence from Switzerland after concurrent referenda, where, while turnout increases, the average levels of political knowledge of voters decreases (Schmid, 2015). Similarly, Fowler and Margolis (2014) argue with a survey experiment that lacking information prevents American voters to vote in line with their preferences. However, note that Schmid (2015) reports as well an increase in information search behavior of these new voters. In the medium term, there is some hope that an information effect would be off-set (Lijphart, 1997).

#### 1.1.4 Outlook

Overall, the works assembled here all contribute to our understanding of two elements of a high quality democracy: participation and (vertical) accountability (Diamond and Morlino, 2004). Who participates in the electoral process and what are the consequences of unequal participation? Which circumstances and incentive structures further both the accountability and the selection of political elites acting in the interest of citizens?

On the one hand, the papers in this dissertation emphasize the importance of turnout for the responsiveness of democracy - changes in voting costs and changes in benefits have substantial impact on who participates in elections. This has implications for electoral results.

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<sup>23</sup>This was refuted, see e.g. (Hobolt, 2014)

<sup>24</sup>Average local attachment is significantly higher among voters in municipalities with 2013 concurrent EP and mayoral elections in Lower Saxony. This survey evidence is displayed in an earlier working paper version of this paper, available at <https://www.researchgate.net/publication/309091333>.

Importantly, political actors themselves will anticipate the cost-benefit-calculus of voters. In the Lower Saxonian case, competing state governments synchronized, desynchronized and resynchronized electoral cycles of mayoral and local council elections<sup>25</sup> with the explicit aim of affecting the turnout decision of citizens. It can be expected that this is motivated by electoral consequences of concurrent elections.<sup>26</sup> These questions are all the more important as turnout increases must not necessarily lead to electoral outcomes that are more representative of the population's preferences, especially given the low turnout baselines in second-order elections.

But not only participation matters for policy that aligns with the preferences of citizens. On the other hand, accountability and selection mechanisms need to work. I particularly highlight the role of electoral institutions in getting incentives right in this thesis – for example, the fact that among all parties competing in the Bavarian state parliament the CSU was particularly implicated in the Bavarian relatives affair is likely no coincidence: Their MPs are predominantly elected in SMD districts, where individual accountability is much lower compared to the OLPR tier (which is much more important for all other parties' candidates).

Both theoretically and empirically the debates the papers in this dissertation contribute to are open. Particularly intriguing is the relationship among electoral participation and accountability mechanisms, where institutional reforms that affect the former might have detrimental effects on the latter (and vice versa).

For example, the relationship between concurrent (second-order) elections, increased turnout and improved aggregate voter decision making is not straightforward. Numerous studies have shown that public policy seems to shift towards policy preferences of disadvantaged citizens with increasing turnout. Bechtel, Hangartner, and Schmid (2016) e.g. argue that compulsory voting in Switzerland, due to monetary fines, brings disproportionate amounts of poorer voters to the polls, who support redistributive policy. Similarly, Fowler (2013) argues that compulsory voting in Australia increased turnout among less advantaged citizens and shifted public policy in line with their preferences (increasing pension spending); likewise, Fujiwara (2015) shows that the enfranchisement of poor voters in Brazil led to higher turnout rates for the latter and subsequent increases in healthcare spending. However, recent work by Bracco and Revelli (2017) shades doubt on whether this would follow from turnout increases with concurrent elections as well. They highlight that when elections of higher and lower salience are combined, a contamination of electoral considerations might occur – despite higher turnout, this could potentially weaken accountability for the race with lower salience, as issues particular to this level of government do no longer affect electoral outcomes. Whether this conclusion extends to other contexts and/or concurrent elections of equal

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<sup>25</sup>An SPD-led state government introduced direct mayoral elections in 1995 with 5 year terms, to be held concurrently with local council elections. A CDU-led government in 2001 increased mayoral term lengths to 8 years, explicitly such that they would rarely be held concurrently with local council elections. An SPD-led government in 2013 again shortened term lengths to 5 years.

<sup>26</sup>See ongoing research by Rudolph and Leininger (2017) that shows that in Lower Saxony especially the SPD, who introduced direct mayoral elections in Lower Saxony and aims at holding them concurrently to local council elections, profits from concurrency.

salience remains open, however. In the case of Lower Saxony, ongoing research (Rudolph and Leininger, 2017) will investigate to which extent concurrent local council and mayoral elections affect electoral outcomes and through which channels.

As well, the question ‘who participates’ directly links to the question of accountability mechanisms under different electoral systems. As Chapter 2 shows, open list PR is furthering accountability of individual MPs relative to an SMD electoral system (given party-centered voting). However, it is likely different types of voters that generate this accountability: Under SMD, the model by Rudolph and Däubler (2016) shows that it is those indifferent between parties that can choose based on candidate characteristics, and hence can de-select poorly performing candidates. These indifferent voters are more likely to be voters with lower than average partisan attachment, information levels and hence turnout probability. Getting these voters to the polls might thus increase this specific type of accountability under a SMD electoral system. However, under OLPR it is more likely that core partisans deselect poorly performing candidates. Citizens marginal to the electoral process as such, just motivated to participate by certain institutional designs and/or external decreases in voting costs, may lack the necessary information levels and engagement to perform the more complex selection of candidates under OLPR. With increasing turnout, and thus increasing votes for a party list and/or top-ranked candidates, the deselection of poorly-performing candidates at the top of the lists might become less likely. Future work should analyze how voters select under OLPR (see ongoing research by Däubler and Rudolph, 2017, who investigate these mechanisms theoretically and empirically).

Last but not least, the perspective of rational choice institutionalism highlights that the decisions of all actors are interwoven (Norris, 2004). Political actors anticipate citizen behavior and vice versa. This complicates any empirical analysis and emphasizes, first, a methodological focus on improved research designs. The goal would be to identify singular causal relationships and, potentially, mediating variables. Second, a strong relation between theory and empirical work is necessary to identify substantively interesting cases and to interpret results. This will be discussed in the following section.

## 1.2 The Credibility Revolution in the Social Sciences

*“Causal inference has always been the name of the game in applied econometrics.” (Angrist and Pischke, 2009, 113).*

### 1.2.1 Overview

As in the social sciences in general, research in voting behavior is prone to endogeneity concerns. Political actors anticipate how citizens behave: they design institutions accordingly and send informational signals strategically. Likewise, citizens take the consequences of their actions and the institutional constraints they face into account and act strategically as well. What is cause and what is effect when analyzing the role of institutions and information on political participation and/or choice is therefore difficult to investigate.

#### **Experimentation in the political sciences**

In an ideal world, experimental methods could circumvent the endogeneity problem directly. By randomly assigning a stimulus, this would allow the clean identification of causal effects in a researcher-controlled environment. The accumulation of experimental evidence over many contexts could then lead to a broad evidence base on important social science questions (Banerjee, 2008). This approach has been fruitfully taken where feasible: Lab, survey and field experiments led to the credible identification of theoretically important mechanisms in the social sciences in general, as well as in research on political institutions and political behavior (McDermott, 2002; Keuschnigg and Wolbring, 2015; Kittel, Luhan, and Morton, 2012; Faas and Huber, 2010; Druckman, Green, Kuklinski, and Lupia, 2011). Particularly, field experiments would be the method of choice for a researcher-controlled investigation of social science questions in a real-world environment (Baldassarri and Abascal, 2017; Grose, 2014; Gerber and Green, 2017). For example, party and civil society organizations have teamed up with researchers to implement ‘get out the vote’-experiments in many contexts. This research has credibly shown that some, though not all citizens can be incentivized to participate in the political process, and helped in finding answers on the behavioral foundations of turnout (Gerber and Green, 2017).

However, for many research questions a researcher-controlled experimental approach would be inaccessible, unethical or impracticable. While survey and lab experiments have great advantages, they as well raise concerns related to their artificial environment, an oftentimes unrepresentative subject pool, experimenter bias, and, most importantly, external validity (McDermott, 2002). While external validity can be partially addressed with field experiments, not all questions can be answered with this methodology either (Grose, 2014). Practical reasons (costs and logistical hurdles) as well as ethical and feasibility concerns (constitutional and legal boundaries, i.e. the need for democratic legitimization of institutional choice and the necessity of a fair democratic process), in many cases prohibit a field-experimental approach.

A promising route in these cases is ‘naturally’ occurring randomization, i.e. variation in the independent variable of interest that is exogenous to the outcome in question. For example, in rare cases randomization is explicitly built into the institutional design by political actors for reasons of fairness and unbiased selection. E.g., following an electoral reform in 1994 France, departmental councilors were to be elected for 6 year terms, with half the council renewed every three years. To choose which councilors would start to be elected first, lots were drawn. Fauvelle-Aymar and François (2015) use this peculiarity to study the consequences of election timing. Similarly, in an Indian setting, a third of village councils has the council chair reserved for a female chairperson – which councils, however, is chosen by random draws. This helps to identify the effects of female representation on policy choices (Chattopadhyay and Duflo, 2004).

These examples highlight how naturally occurring variation can be gainfully exploited for scientific purposes. The question then arises when and under which conditions observational data can be treated ‘as if’ random.

### The design-based approach

Scholarship since long recognizes this challenge. At least since the early 1900s, among the “fundamental difficulties in the scientific study of political processes” have been (and still are) listed: machinery for measuring the political world and statistical tools for analyzing them; isolating causal relations sufficiently; and drawing inferences from observational data without researcher-controlled experiments (Merriam, 1923, 287-289). In inspiration from and close collaboration with related fields, political methodologists increasingly focus on the causal inference problem. By now, this has evolved into one of the central trends in the political science discipline<sup>27</sup> (Clark and Golder, 2015; Titiunik, 2015; Keele, Titiunik, and Zubizarreta, 2015). This literature specifically turns its agenda to the assumptions needed to give statistical estimates a causal interpretation, and to differentiate whether statistical models are to be interpreted descriptively or causally (Keele and Titiunik, 2015a). Experimental designs serve as benchmark, but the design-based analysis of observational data, drawing on natural and quasi-experimental justifications is the focus of this work.<sup>28</sup> This methodological

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<sup>27</sup>Next to the logical rigor of formal theory and, as an important recent development, the opportunities presented by big data – however, even given the trend to big data, causal research designs to reduce bias become no less important as inductive causal modeling or causal modeling based on machine-learning is next to impossible (Titiunik, 2015); see as well Kauermann and Küchenhoff (2016) for the importance of statistical modeling with big data.

<sup>28</sup>There is some conceptual ambiguity in the definitions of ‘natural’ or ‘quasi’-experiments. An experiment as such is defined by the constituting element of researcher control over the experimental stimulus and explicit design of the (field/lab/survey) experimental study. This is already less clear for natural experiments (Robinson, McNulty, and Krasno, 2009). Natural experiments imply an exogenous stimulus (‘act of nature’). Dunning (2008) therefore defines a natural experiment by a random or as-if-random treatment assignment process (that is not under the control of the researcher). The defining feature would be that the selection process is unrelated to the outcome variable of interest (hence as-if-random). However, any naturally occur-

viewpoint is becoming more and more a standard in applied empirical research in the social sciences. Drawing on the ground-breaking conceptualization of potential outcomes in the Neyman-Rubin-Holland-Model (Rubin, 1974, 1977; Neyman, 1923; Holland, 1986) political science has embraced the counterfactual approach to causal inference (see below for a short summary of the model). For the anglo-saxon context, this approach has been popularized at least since King, Keohane, and Verba (1994), and is now standard in many introductory (e.g. Kellstedt and Whitten, 2013) or advanced (e.g. Box-Steffensmeier, Brady, and Collier, 2009; Morgan and Winship, 2015; Best and Wolf, 2015) text- or handbooks for political science methodology. As well, it is available in easily accessible methodological reviews (e.g. Gangl, 2010a; Berger and Wolbring, 2014; Keele, 2015). In the German research community, the approach is gaining ground (Faas and Huber, 2010), mostly though with direct reference to experimental research when incorporated in German language text- or handbooks of (political) methodology (e.g. Diekmann, 2007; Behnke, Behnke, and Baur, 2010; Bräuninger, Bächtinger, and Shikano, 2012; Baur and Blasius, 2014). Some notable exceptions, e.g. the summaries by Legewie (2012) or Bauer (2015) or method-focused introductions on matching (Gangl and DiPrete, 2004; Gangl, 2010b) or fixed effects estimation (Brüderl, 2010) explicitly relate to design based inference with observational data.

### 1.2.2 Natural Experiments: Invoked Assumptions and an Overview on the Applications

When thinking about causal inference, the potential outcome framework is a useful starting point, as it outlines the identification problem very clearly. The framework can shortly be summarized as follows: Assume a binary stimulus ('treatment')  $T$  and a continuous outcome variable  $Y$ , where we are interested in assessing the effect of treatment status on manifestations of the outcome. The counterfactual approach starts with the observation that  $Y_i^1$ , i.e. the outcome for individual  $i$  under treatment (hence  $Y^1$ ), and  $Y_i^0$ , the outcome under control state, would need to be simultaneously observed to determine a causal effect on the individual level. Hence, we would need to be able to observe counterfactual realities. All subsequent methodological discussion then center around the question whether and when the observed

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ring stimulus is oftentimes not really exogenous, but rather external to the system. Deaton (2010) makes this very important point in his discussion of the design-based approach, arguing that even an alphabetized selection process (where every third entity in the order of the alphabet was assigned to a stimulus) may be confounded (see as well Sekhon and Titiunik, 2012). Natural experiments are thus regularly not different from quasi-experiments, where random treatment assignment (by both nature or the researcher) may be absent. In any case, the point of the design based approach is to clearly outline the selection process and propose design based or statistical corrections that make as-if-random assignment plausible (Robinson, McNulty, and Krasno, 2009). I therefore follow the conceptual ambiguity of many scholars using the terms interchangeably (e.g. Gangl, 2010a) and refrain from strictly distinguishing natural from quasi-experiments. The conclusion to this chapter then takes up a fruitful conceptualization by Dunning (2008), distinguishing research designs by the plausibility of as-if-randomness concerning the relationship of independent and outcome variable of interest.



outcomes in two groups, where  $G = 1$  denotes that group members actually received the stimulus ( $G = 0$  otherwise), and where subsequently average outcomes over the groups can be observed. The question then is to what extent  $E(Y_i^1 - Y_i^0) = E(Y_i^1|G = 1) - E(Y_i^0|G = 0)$ . As the left hand side cannot be observed, only careful theoretical argumentation and skillful placebo tests can provide an indication whether this equation holds; otherwise, the researcher risks to confound treatment effects with selection bias. Selection bias in this framework can be characterized by the difference in group outcomes, had the intervention not occurred, i.e. selection bias  $s$  is given by  $s = E(Y_i^0|G = 1) - E(Y_i^0|G = 0)$  (derived by rearranging the above equation).

The potential outcome framework is, of course, not the only possible conceptualization of the causal inference problem. Pearl (2010) provides a good overview on directed acyclic graphs (DAGs), which are a useful approach as well. DAGs are a tool for identification, particularly in settings with selection on observables – there, they help to structure and represent the researchers conceptualization of the selection problem (Keele, 2015). As well, they are highly useful for conceptualization when considering whether and which variables (not) to control for non-parametric identification (Elwert and Winship, 2014).

Returning to the potential outcome framework, randomization will lead to  $s \rightarrow 0$  in expectation, as by design randomization implies identical group characteristics in expectation. The question however is, under which conditions research designs with observational data will achieve this end. Five major causal inference techniques for observational data are available to the researcher: difference-in-difference (DID), or more generally fixed effects estimation; matching; regression discontinuity designs; and instrumental variables. Cutting edge research investigates further how concepts of causality can extend to causal mediation analysis and causal mechanisms (Imai, Keele, Tingley, and Yamamoto, 2011; Hedström and Ylikoski, 2010). The studies performed for this thesis take up one or several of the five methods to answer questions on voting behavior and electoral choice.<sup>29</sup> In the following, these methods will only be discussed in brief with a focus on necessary assumptions and why these methods were chosen for the applications at hand. The individual papers in this dissertation each present the employed methods in more detail. For in-depth overviews on the strengths and limitations of the methods, a broad literature exists: For difference-in-difference estimation, Lechner (2010) is a good resource, as is Brüderl and Ludwig (2015) for fixed effects estimation. Likewise, a detailed introduction to matching techniques is available in Rosenbaum (2010) or Gangl and DiPrete (2004). A broader overview on instrumental variables is given by Imbens, Rubin, and Angrist (1996) (see as well Sovey and Green, 2011). The regression discontinuity approach is discussed in detail in Lee and Lemieux (2010). Textbook-length introductions are available e.g. in Angrist and Pischke (2009); Wooldridge (2010); Imbens and Wooldridge (2009); Morgan and Winship (2015).

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<sup>29</sup>Additional techniques, such as control function methods, sensitivity analysis, bounds, or synthetic control groups are part of the causal inference toolbox as well, but will not be discussed here.

## Difference-in-difference estimation

I first of all turn to the difference-in-difference approach. Ususally, when an external event affects some units of observation but not others, it is not necessarily the case that these two groups would counterfactually be comparable with respect to their potential outcomes. However, the DID approach builds on the insight that even if not comparable in levels, they might well be with respect to trends (Card and Krueger, 1994). Hence, the development of a control group provides the counterfactual for what would have happened in the treated group absent the intervention. Obviously, repeated observations, at least two, need to be present in this case. Besides this data requirement, the DID-approach is weakly preferable to any cross-sectional analysis, as identifying assumptions are weaker. Even with a fully randomized treatment, using a DID approach should lead to efficiency gains and improved model fit if additional covariates are used.<sup>30</sup>

The crucial assumption for an unbiased DID is  $E(Y_{i,t}^0 - Y_{i,t-1}^0 | G = 1) = E(Y_{i,t}^0 - Y_{i,t-1}^0 | G = 0)$  (Lechner, 2010; Brüderl, 2010), i.e. that changes in outcomes over time  $t$ , with  $t \in 1, 2$ , would be similar both in the treated and in the control group absent the treatment ('parallel trends assumption'). If this is the case, it can plausibly be argued that comparing trends between group outcomes in a pre-post-intervention design gives the researcher an estimate for the Average Treatment Effect on the Treated (ATT).<sup>31</sup>

While the identifying assumption can be made plausible when observing parallel trends in pre-treatment periods, or by assessing the balance of (trends) in potential confounding variables, whether trends are indeed parallel in the treatment period is untestable. Especially for longer time periods, an assumption of parallel trends can become difficult to uphold.<sup>32</sup>

When estimating a DID model, a standard OLS regression framework with unit- and time-level fixed effects is equivalent to a (non-parametric) difference-in-difference estimator for the

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<sup>30</sup>Comp. Table 1 of Leininger, Rudolph, and Zittlau (2018) for an example: We estimate the effects of concurrent elections to turnout with a DID and a cross-sectional design. Both are valid, as we can assume selection to have been plausibly exogenous to turnout rates. Consequently, both models yield very similar point estimates (10.2 as opposed to 9.7 percentage points increase in turnout through concurrency), the DID model has smaller standard errors, though (0.4 compared to 0.6).

<sup>31</sup>If the researcher wants to estimate an unconditional Average Treatment Effect, the stronger assumption is needed that the intervention has a similar average effect in the treated and as well, counterfactually, all the control observations. Additional assumptions invoked are the Stable Unit Treatment Value Assumption, which implies particularly that the intervention does not spill-over to control group observations, i.e. that there are no interactions between units; it as well has to be assumed that there is No Effect on the Pre-Treatment Population (NEPT), i.e. that there are no anticipation effects in the groups; finally, if additionally time-varying covariates  $X$  are used to justify the parallel trends assumption, these must not be exogenous to the treatment (Lechner, 2010).

<sup>32</sup>Important contributions to the literature do not address this finding with sufficient caution. E.g., one of the pioneering contributions for the method in political science by Bechtel and Hainmueller (2011) use a parallel trend assumption for election results for a time-period of 9 years and four elections, which is potentially introducing bias to their estimates on the effects of flood exposure to incumbent party vote shares in treated regions due to geographically clustered time-variant confounders (comp. Chapter 3).

case of two time periods (Brüderl and Ludwig, 2015). This strategy is mostly employed in the projects in this dissertation. Where more than two time-periods are drawn upon, a fixed effects estimator with time- and unit-fixed effects is used, drawing as well on the parallel trends assumption.<sup>33</sup>

The difference-in-difference approach can be adapted very flexibly. Importantly, differencing is not only possible over time (Brüderl and Ludwig, 2015, have made a similar argument). As shown in Chapter 2 and 4, the method is applicable in any circumstance where through differencing one can eliminate unobservable confounders that affect both differenced units similarly. In Chapter 2, Rudolph and Däubler (2016) develop the difference-in-tiers approach as specific strategy for assessing the person-specific impact of the Bavarian relatives affair. Under the assumption that party valence should affect party choice equally in both tiers, the valence of the district candidate is to influence only the decision regarding the first vote.<sup>34</sup>

### Matching techniques

Matching or related methods are applied in Rudolph and Däubler (2016) and Rudolph and Kuhn (2018) (Chapters 2 and 6). Matching addresses the causal inference problem directly via the conditional independence (or ignorable treatment assignment) assumption: Assuming that selection into treatment is a function of covariates, selection bias can be reduced by non-parametrically accounting for imbalance in pre-treatment confounders between treated and control observations (Stuart, 2010). Hence, it is assumed that  $E(Y_i^0|G = 1, \mathbf{X}) = E(Y_i^0|G = 0, \mathbf{X})$ , i.e. conditional on covariates outcomes under control condition would not differ. Importantly, matching estimators rest on the strong assumption that these covariates are observable. As instructively summarized by Gangl and DiPrete (2004) (see as well Stuart, 2010), for most matching estimators the observations  $j$  from the control group are used to mimic counterfactual observations  $i$  from the treatment group, hence an average treatment effect on the treated is calculated. This can be grasped by a doubly weighted comparison of outcomes, as  $ATT_{\text{matching}} = \sum_{i|G=1} w_i * (Y_i^1 - \sum_{j|G=0} h_{ij} * Y_j^0)$ . Such a matching estimator is a comparison of the outcome for each treatment group observation  $i$ , contributing with

<sup>33</sup>Additional statistical assumptions apply, particularly that the idiosyncratic errors have constant variance and are uncorrelated over time. If this is not the case, clustered standard errors would be a best practice (Cameron and Miller, 2015), which are applied as a conservative approach throughout.

<sup>34</sup>We thus estimate the ATT for the candidate valence effect of the scandal via the difference of CSU first vote share  $Y_{FV,d}$  and CSU second vote share  $Y_{SV,d}$  in districts  $d$  with  $(Y_{FV,d} - Y_{SV,d}) = \alpha T + (C_{FV,d} - C_{SV,d})\beta + (\epsilon_{FV,d} - \epsilon_{SV,d})$ . If we assume that the party valence effect of the scandal is constant over tiers,  $\alpha$  identifies the candidate valence effect. The covariates  $C$  include observable factors that influence the voting decision differently at both levels. We control especially for candidate quality and list quality (i.e. regional electoral district). Note that all unobserved factors that affect party choice to a similar extent over both tiers drop out of this equation. This estimation assumes that we have identified all unobservable confounders that affect the difference in party choice. To substantiate this assumption, results of a fixed effects specification over time are as well reported, which additionally accounts for any unobserved time-constant candidate factors.

weight  $w_i$  to the ATT, to a with  $h_{ij}$  specifically weighted average counterfactual for the observation from the control group. Matching algorithms then differ by how these weights are constructed: In particular, variants of exact matching assign one (or few) control group matches to each treatment group observation (e.g. stratification, nearest neighbor matching, caliper matching). Other matching estimators (e.g. mahalanobis matching, kernel matching) use weighted control group means to mimic counterfactual treatment group outcomes (Gangl and DiPrete, 2004, Table 1).

In this understanding, matching serves as strategic subsampling from both the treatment and control group in order to achieve maximum comparability (Morgan and Winship, 2015, 142). This has two advantages: On the one hand side, it improves the balance between control and treatment group observations, which has the potential to substantially decrease selection bias. Crucial are then assumptions what degree of difference between observations is acceptable. On the other hand, it compares only ‘comparable’ observations, i.e. treatment-control group pairs that are balanced. Treatment effects are thus estimated from a region of common support. This leads to increased transparency when interpreting findings: As effects can substantially only be interpreted for the region of common support, the researcher is prevented from extrapolating to regions off the support. For example, if certain covariates fully determine control or treatment group status, it is impossible by definition to separate the effect of treatment from selection bias for these observations.<sup>35</sup>

Importantly, there is no general ‘best’ approach (Morgan and Winship, 2015, 170f.): Various matching estimators have been proposed, each with their own strenghts and weaknesses that are more or less relevant depending on the application. Chapter 2 makes use of a recently developed approach, coarsened exact matching (CEM) (Blackwell, Iacus, King, and Porro, 2009; Iacus, King, and Porro, 2012). This is a variant of exact matching – as exact matching is hardly feasible with more complex social processes that lead to treatment assignment (as exact matches will not be present with many, and especially metric, covariates in finite samples), CEM allows the researcher to pre-specify treatment variables that can be coarsened, i.e. where ranges can be used that contain comparable information. In the case at hand, candidates implicated in the Bavarian relatives affair likely had specific characteristics<sup>36</sup> that could be meaningfully coarsened.<sup>37</sup> This allowed us to estimate particularly credible effects for a subgroup of treated candidates. Depending on the amount of

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<sup>35</sup>This distinguishes matching as well from regression analysis, which can otherwise just be understood as a different way of constructing the weights to sum the covariate-specific effects (Angrist and Pischke, 2009, 54).

<sup>36</sup>Particularly, they were relatively older and with more legislative experience, as relative employment regulations that some of these candidates exploited stem from the 1990s. This in turn, however led to e.g. a higher observed placement of implicated candidates on the Bavarian open lists. This is an important confounder as ballot position impacts counterfactual vote shares (Faas and Schoen, 2006; Däubler and Rudolph, 2017).

<sup>37</sup>Particularly, we specified age in age brackets of 10 years; more importantly, we used knowledge on ballot placement effects (Faas and Schoen, 2006) to coarsen pre-electoral list position, differentiating 1st, 2nd, 3rd, 4th-6th, 7th-10th place and all other ranks.

pre-treatment characteristics included, more or less treated and control observations have to be discarded, though. Importantly, where treatment and control observations are discarded to improve balance (and the case for an unbiased estimate) this changes the estimated ATT to a local (Sample) Average Treatment Effect on the Treated (SATT), which is potentially more difficult to interpret. Overall, the matching solution was very useful, given we had to use a cross-sectional research design: We could not use DID as even for those candidates that ran in previous time periods, the within-party competitive environment had changed too much.

CEM is less feasible with higher-dimensional data and where there are no good theoretical reasons for applying coarsening. For such cases, entropy balancing (EB) has been proposed as a data pre-processing technique by Hainmueller (2012). With entropy balancing, the researcher can adjust the first, second and potentially higher moments of covariate distributions between a treated and a control group. EB keeps all treated observations, hence the SATT as an estimand remains unchanged with this technique. The weights obtained can be passed e.g. to a standard (weighted) regression estimator or (weighted) difference-in-means test. We apply entropy balancing in Chapter 6: Observing that electoral participation in flooded and unflooded municipalities in Saxony and Bavaria does not follow parallel trends in the pre-treatment period, we cannot use a simple DID estimator. However, DID can be combined with matching in such a setting.<sup>38</sup> We use entropy balancing (Hainmueller, 2012) to generate weights such that both our treatment and control group follow a parallel trend prior to the treatment. Applying this reweighting of control group observations in the treatment period should make both groups more comparable on background characteristics when estimating treatment effects.

Both matching applications used in this dissertation do not rely on an estimation of the ‘propensity score’ (Rosenbaum and Rubin, 1983), the dominant matching technique to date. Propensity score matching (PSM) has attracted some critique recently (e.g. by King and Nielsen (2016, 1), who argue that “[the] propensity score should not be used for matching”). PSM proceeds in two steps. First the probability of treatment assignment is estimated drawing on available covariates with a logit function. Second, treatment effects are estimated on observations with comparable propensity scores. This, however, implies that observations with the same propensity score must not be comparable on the set of their background characteristics. This requires that the researcher manually invests in balance checking and regularly has to iterate between different propensity score models to achieve this balance (Hainmueller, 2012). While PSM is useful when approximating a completely randomized experiment with one or few covariates, in most cases matching should be understood at mimicking a blocked experiment; for this, other matching algorithms would be suited better (King and Nielsen, 2016).

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<sup>38</sup>See Lechner (2010) for a broader discussion on how combining DID with matching is a fruitful future avenue for causal inference designs (e.g. using matching on the temporal structure in the data to improve DID inference).

Overall, recent developments in matching methods, such as those applied in Chapters 2 and 6, emphasize that observational data can potentially contain a hidden experimental data set (King and Nielsen, 2016), but that caution has to be applied which technique to use and how to interpret results. Most importantly, matching increases the balance of covariates in treated and control groups. When the assignment mechanism is plausibly ignorable given these covariates, matching is useful in identifying causal treatment effects. Additionally, compared to other techniques such as standard linear regression, matching reduces modeling assumptions and prevents the researcher from drawing conclusions off the common support of the data.

### Instrumental variables and regression discontinuities

Just shortly, I will address strengths and limitations of the instrumental variables (IV) approach (applied in Chapter 7) and the regression discontinuity design (RDD) (applied as geographic regression discontinuities in Chapter 5 as robustness test).

Both IV and RDD can be understood as local experiments. When necessary assumptions are met, this has the great advantage that even with cross-sectional observational data causal estimates can be obtained. Of course, in combination with other methods, particularly a DID estimator, inference can be additionally strengthened (this approach is taken in this thesis).

With IV, the researcher assesses the relation between two variables  $X$  and  $Y$ , which are potentially endogenously related – for example, Chapter 7 assesses the relation between observed aggregate turnout and vote shares, which is likely confounded as expectations on turnout might influence the individual calculus of voting. The researcher could then use an instrument  $Z$  that is related to  $X$  but not to  $Y$  – in the case at hand, and in line with a broad literature, I use rainfall (Knack, 1994), arguing that rain impacts the turnout decision (through the voting cost function), but not voter decision making. The instrument hence induces variation in  $X$  that is exogenous to  $Y$ . The effect of this variation in  $X$  can then be estimated and provides for a causal local average treatment effect (LATE) (Sovey and Green, 2011; Imbens, Rubin, and Angrist, 1996). Of course, whether the method can be applied hinges on an in the end untestable assumption: whether  $Z$  really influences  $Y$  only through  $X$ . This has to be made plausible through theoretical arguments and a careful observation of the selection process (Dunning, 2008, 2012). For the effect of rainfall, it has e.g. been suggested recently that rainfall might impact vote shares directly through the mood of citizens. From the perspective of the economic theory of voting, this should theoretically not matter too much, as psychological concerns are not part of the calculus of voting.<sup>39</sup> Empirically, where this connection has been explored, it has been shown to increase risk aversion (Meier, Schmid, and Stutzer, 2016; Bassi, 2013). This should in the case at hand counteract the effects I find. Future research could investigate whether a potential direct impact of rainfall on voting behavior really exists and whether it could invalidate the large

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<sup>39</sup>With psychological theories, this conclusion would be different, of course.

literature that uses rain as an instrument for turnout (see for an overview Arnold and Freier, 2015). A more important caveat of the method is discussed below in more detail: With IV, estimates are derived for local variation in  $X$  (only a LATE is estimated). Whether this LATE is substantially interesting depends on the context and the instrument (Deaton, 2010).<sup>40</sup>

With an RDD, the researcher exploits the fact that at a certain threshold of  $X$  assignment to treatment status changes (Thistlethwaite and Campbell, 1960). E.g., with very close elections, winners and losers are ‘as-if’-randomly determined. Researchers can then exploit the randomness at this local threshold to derive the effect of treatment status on outcomes of interest (within the group of very close elections, RDD has e.g. been used to estimate the incumbency advantage (Lee, 2008)). Of course, careful attention has to be given to the concrete assignment mechanism, and especially to the questions whether agents can sort at the threshold (de la Cuesta and Imai, 2016). As well, only a LATE is estimated – whether effects can be generalized depends on the concrete setting. Recently, the concept of the RDD has been extended to take advantage of geographic discontinuities (Keele, Titiunik, and Zubizarreta, 2015; Keele and Titiunik, 2015b). A geographic RDD would exploit a boundary change that affects treatment assignment, arguing that observations very close to either side of the boundary are otherwise identical concerning their potential outcomes. Chapter 5 uses this approach to estimate the effect of concurrent local elections on European Parliament election turnout. Treatment varies at the state level, where some German federal states introduced concurrent local elections. Comparing municipalities just at the boundaries, we show that treated municipalities see much higher aggregate turnout levels. In this respect, the question of compound treatment effects has to be debated (Hernán and VanderWeele, 2011). Especially at geographic boundaries, regularly more than one politically relevant factor changes at the threshold. In the concurrent-election case, any change in state boundaries e.g. implies different state party organizations responsible for the local EP campaign. Whether and to what extent such compound treatments confound the estimates is impossible to test directly. Placebo tests (as applied in Chapter 5) can help to substantiate necessary assumptions, though.

### 1.2.3 Limitations and Challenges

The causal inference approach focuses the attention of the researcher to isolating one causal relationship  $X \rightarrow Y$ , taking seriously the complex, and potentially confounding, causal structure this relationship is embedded in – central challenges are selection or unobserved confounders and reverse causality. Particularly, the approach aims not only for greater transparency

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<sup>40</sup>In Chapter 7, I therefore show with survey data that the change in voting costs through rainfall is likely relevant for the broader heterogeneity in voting costs among the population of occasional voters in the Brexit-case.

concerning identifying assumptions (Angrist and Pischke, 2010), but guides the research to choosing settings and methods that can reduce these assumption to a minimum.

### A continuum of plausibility

Dunning (2008) proposes to locate research designs on a continuum. The extremes denote observational studies<sup>41</sup> and, respectively, randomized experiments. In this spirit, Figure 1.1 locates the studies in the following chapters on a one-dimensional ordinal scale with respect to the strength of invoked identifying assumptions. Strongest, i.e. closest to the experimental ideal, certainly is Leininger, Rudolph, and Zittlau (2018): As shown in Chapter 5 treatment assignment is most likely exogenous to counterfactual turnout levels. This is supported by numerous placebo and balance tests, similar results using geographic regression discontinuities at the state border, and in-depth research on the assignment mechanism. Consequently, DID and cross-sectional results in the study hardly differ, as would be expected from an experimental stimulus.

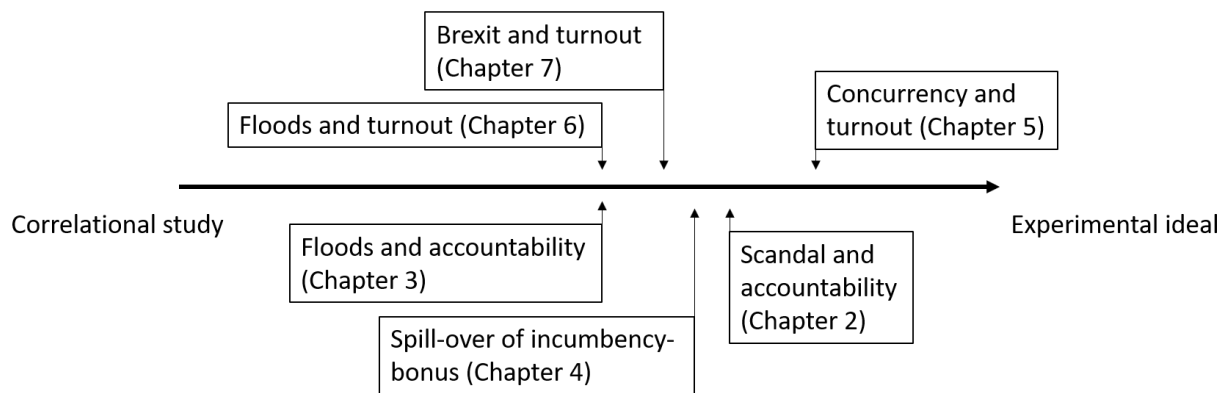


Figure 1.1: Locating the studies on a one-dimensional ordinal scale with respect to the strength of invoked identifying assumptions (inspired by Dunning, 2008)

Identification is strong in the case of Rudolph and Däubler (2016) as well. As outlined in Chapter 2, the affair was largely based on decision making of members of parliament in the early 2000s that supposedly is unrelated to electoral standings in 2013. Placebo tests for the SMD electoral level work well – pre-treatment period trends show no difference between later scandal districts and later control group districts; the specific difference-in-tiers-approach, as well over time, lends robustness to these results. Concerning the OLPR analysis, both regression and exact matching estimates point in a similar direction, irrespective of the amount of covariates used for the latter case. That estimated treatment effects hardly change with and without inclusion of (time-varying) controls is a good sign for the independence of

<sup>41</sup>I denote observational studies as research where it is known that observations self-select into the states of dependent variables interesting to the researcher, but where the degree of this self-selection is unknown.



treatment assignment to at least these observed potential confounders and the strength of the research design.

The remaining studies rely on slightly stronger assumptions. In these cases treatment can be considered external to the system, though not necessarily exogenous (Deaton, 2010). This is an important distinction. On the one hand, weather events and natural disasters are timed randomly and are at least partly events beyond the control of society. However, geographical vulnerabilities influence the ex-ante risk for weather events and flooding (not to speak of ex-ante disaster preparedness decision making). Additionally, weather shocks are usually spatially correlated. In the case of Chapter 7, rainfall affected particularly the south-east of England. To caution against false-positive effects, I therefore use a difference-in-difference approach in combination with instrumental variables (Lechner, 2015). In this case, I use rainfall to instrument for the turnout *increase*. This should remedy at least part of the potential unobserved confounders. Again, placebo tests on unaffected outcomes (prior election turnout; postal voting turnout) support the research design.

More difficult is an application where there is a risk that geographic clustering and unobserved confounders induce bias not only in levels, but potentially as well in trends. In the case of the effect of Elbe and Danube floods on voting behavior in Germany, it is e.g. likely that the location of the treatment groups in former East Germany and Bavaria violate the common trend assumption: Observed and unobserved traits<sup>42</sup> potentially lead to bias when estimating effects under the parallel-trend assumption over long time-periods as in Bechtel and Hainmueller (2011). This is not always easily testable due to specific mechanisms of the treatment that can be observationally equivalent.<sup>43</sup> Overall, the interpretation of coefficient estimates as ‘causal’ hinges on the ability of the control group to mimic counterfactual treatment group outcomes. E.g., by combining DID with entropy balancing (see Chapter 6) and by drawing on a fixed-effects framework with more than two time-periods (see Chapter 3), additionally using flexible slopes (Brüderl and Ludwig, 2015), it is still possible to reduce necessary assumptions and make identification relatively plausible. Triangulation, by comparing results from aggregate with survey data, helps in assessing the strength of identification. Finally, a fruitful route is the reduction of the control group to a more credible counterfactual (here, we e.g. use ‘close-by’-regions and estimate effects not only at the district, but as well the municipality level). This helps as well in generating more credible inference, though for the sake of a loss of generality. Finally, even where ambiguity concerning internal validity remains, the potential threats to identification are spelled out, which helps for the interpretation of substantive conclusions.

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<sup>42</sup>E.g. the differing party system in former East (SPD, CDU and PDS/Linke as main parties) vs. West Germany (SPD and CDU), as well as in Bavaria (CSU dominance).

<sup>43</sup>In the case of the Elbe floods, both spill-overs and selection effects could explain a differing trend in unaffected districts close to flooded areas. For example, as shown in Chapter 3 the whole East, not just Elbe-flood affected districts, are on the long-term SPD-supporting trend identified by Bechtel and Hainmueller (2011) as long-term flood effect.

### Internal and external validity

By focusing on maximal internal validity, these applications highlight how a focus on research design can lead to credible causal inference. However, a second issue has to be highlighted: What is the population of interest the causal effects are estimated for? This relates to the question whether and when a finding can be generalized to other populations. This question is most obvious with IV and RDD estimates that identify a LATE by definition, i.e. an effect that is valid for the part of the population complying with the instrument or close to the threshold. Effects can only be generalized to the whole study population if it is plausible to assume a constant effects framework. This must not be the case, of course (Deaton, 2010).

Similarly, the assumptions for average treatment effects are regularly more challenging compared to assumptions for average treatment effects on the treated: For an ATT, it suffices that the control group mimicks counterfactual treatment group behavior. But to estimate an ATE, the treated group would as well need to be comparable to the full population with respect to its response to treatment. In the case of the Bavarian relatives affair (comp. Chapter 2), we estimate an ATT. Placebo and balance tests allow to plausibilize that counterfactual outcomes for the treated group would have developed as in the control group. For treated subjects, we hence ascertain effects. However, treated candidates were likely peculiar types. We therefore cannot generalize to the full population of Members of Bavarian Parliament (or the likely size of punishment in any other scandal). Substantively, this still allows us to draw two general conclusions: First, a scandal such as the Bavarian one can lead to electoral punishment. Second, holding both shock and set of implicated politicians constant, punishment is substantively larger under an open-list as compared to a single-member district system. Which contextual factors enable or prohibit similarly enhanced accountability with OLPR in other electoral contexts would require additional studies, though.

As highlighted by Imbens (2010) it is important to clarify what one can actually learn from any empirical study: Design-based inference will both make transparent the credibility of results and highlight the population to which results are applicable. At the same time, only with knowledge on the credibility of the evidence can a proper basis be developed for both improving theory and for good policy decisions.

#### 1.2.4 A Black Box Approach? Room for Theory and Ways Forward

Given the discussion of the design-based approach so far, how can generalizable macro-evidence be generated and where is room for theory? In their widely discussed contributions, Deaton (2010), Rodrik (2009) and Sims (2010) highlight this as important drawbacks of the credibility revolution. By concentrating on the isolation of singular causal effects, we learn what works, but not necessarily how and where it works. This is a black box approach to social science research that will leave important questions unanswered (Imai, Keele, Tingley, and Yamamoto, 2011). The application of experimental techniques does indeed not require

reference to a theoretical model. But without this reference, the mechanism linking cause and effect will remain obscure and it is questionable whether science can progress this way (Deaton, 2010). Additionally, if methodological considerations (i.e. the plausibility of identifying assumptions) define which research is acceptable to the discipline, this will necessarily restrict the universe of questions that can be addressed (and answered) by political science. In applied causal inference research, it oftentimes is the quasi-/natural experimental variation researchers become aware of that defines the research topic, and thus subsequently the questions that are addressed (Gelman, 2009).

One avenue to progress is relatively clear, though: Of course, the strengths of singular causal inference papers lie in identifying causal effects. To what extent such effects travel across contexts is unknown. Thus, broader research programs are necessary to generalize, at best by using different (causal) research designs (Keele and Titiunik, 2015a). Importantly, when developing such research programs, broad theoretical considerations should lead the researcher to the selection of contexts for new studies (Samii, 2016). Evidence from various context can then support generalizable claims.<sup>44</sup> Additionally, cutting-edge research on causal inference methodology is developing that helps to separate potential competing mechanisms and to identify mediators of causal effects (Imai, Keele, Tingley, and Yamamoto, 2011). This as well, will help to link causal inference back to theory.

Finally, for the broader question where to turn the flashlight of empirical research to, collaboration between theoretical and empirical researchers is necessary. (Formal) theory lends itself to the same logical rigor as causal inference. Both methods are well-suited to assess *ceteris paribus* changes in a broader system. This points to the fruitfulness of an ‘Empirical Applications of Theoretical Models’ (EITM) approach that combines theorizing and empirical work such that literatures from (formal) theorizing and empirical analysis can build on each other (Aldrich, Alt, and Lupia, 2008).<sup>45</sup>

Of course, this approach will be at odds with the limitations of causal inference methods at times – certain research questions will hardly lend themselves to (quasi-/natural) experimental analysis (Rodrik, 2009). A methodological openness is therefore highly desirable. And only by understanding the limits of causal identification, incentives for developing new methods for better inferences in such boundary cases can arise (Keele and Titiunik, 2015a).

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<sup>44</sup>E.g., a strength of Rudolph and Kuhn (2018) (see chapter 6) is that evidence on a negative impact from flood exposure on turnout stems from two very different flood events under different socio-economic contexts and times (relatively poor and recently democratized 2002 Saxonia and relatively rich 2013 Bavaria). This helps to more broadly connect the findings to the Riker-Ordeshook model of voting (Riker and Ordeshook, 1968). Similarly, Leininger, Rudolph, and Zittlau (2018) discuss the turnout effects of concurrent elections for the case of the 2014 EP elections in Lower Saxony with high internal validity, using a natural experiment; we then show that these effects are likely generalizable to all German states and over time, drawing on a fixed-effects model for state level electoral outcomes from 1979 to 2014.

<sup>45</sup>In Chapter 2, building on decision-theoretic models of voter participation (Riker and Ordeshook, 1968; Thurner and Eymann, 2000) and vote choice (Adams, Merrill, and Grofman, 2005; Maurer, Thurner, and Debus, 2015), we develop both a theoretical and empirical argument on the electoral system effects of candidate valence shocks in an EITM fashion.

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## Chapter 2

# Holding Individual Representatives Accountable: The Role of Electoral Systems (Rudolph and Däubler 2016)

This research has been published in *The Journal of Politics*, 2016: Issue 78, Volume 3, pp. 746-762, ISSN: 0022-3816 (print); 1468-2508 (online), under authorship of Lukas Rudolph and Thomas Däubler (University of Mannheim).

### 2.1 Summary

*Voters are reluctant to sanction representatives for individual misconduct if they have to balance candidate-level and party-level factors in their choice, but this trade-off is affected by the electoral system. Our general theoretical model explains why individual accountability can empirically occur in single-member district (SMD) systems but is expected under less restrictive conditions using open-list proportional representation (OLPR). The latter not only decouples party and candidate choice but also makes seat allocation more vote elastic. For a thorough empirical test of our argument, we draw on real-world evidence from state-level elections in Bavaria, Germany, which are held under an unusual mixed-member system. Exploiting a recent public scandal involving one-third of representatives, we examine how electoral punishment of the same candidates by the same voters differs across electoral rules. Drawing on difference-in-differences as well as matching/regression estimators, we show that electoral punishment is substantially larger under OLPR than under SMD systems.*

### 2.2 Published Article

The reader is kindly referred to the above-mentioned source for the full article, which is available online at doi: 10.1086/685378. The data and code necessary to reproduce the numerical

results in the paper are available in the JOP Dataverse<sup>1</sup> under doi: 10.7910/DVN/E2187A . The appendix is available as supplemental material on the article site of Journal of Politics (see as well Appendix A).

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<sup>1</sup><https://dataverse.harvard.edu/dataverse/jop>



## Chapter 3

# Selecting Good Types or Holding Incumbents Accountable? Evidence from Reoccurring Floods<sup>1</sup>

### Abstract

*A growing literature draws on natural disasters to assess how voters hold governments accountable. This literature largely interprets disasters as exogenous shock to voter information and voter reactions to disaster occurrence and disaster management mainly as following a retrospective reward-punishment logic. But theoretically, voters should as well prospectively learn on an incumbent's type from disaster management. I provide new evidence from four large scale centennial floods in Germany (the 2002 and 2013 Elbe and Danube floods), occurring right before elections. I show that robust disaster management led to vote gains for federal or state incumbents in affected districts. Additionally, strong spill-overs to adjacent regions indicate that voters use disasters as well in a prospective logic to learn on incumbent quality. The heterogeneous response to disaster occurrence over time and space indicates, however, that the cue that disasters give likely depend on the timing of disaster and elections as well as on prior beliefs on incumbents, and that psychological factors play a role. Overall, I find support for a retrospective, a prospective, and a psychological perspective on accountability, indicating that voters are neither solely engaging in ('blind') retrospection nor only updating their selection heuristic, but that a mixture of perspectives can likely explain the divergent findings in the literature.*

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### 3.1 Introduction

That voters retrospectively reelect or discard incumbents based on their past performance is a longstanding argument in the discipline (Key, 1966; Fiorina, 1981; Ferejohn, 1986). But although a primary function of elections, it is not so clear how well voters are able to hold incumbents accountable. Cognitive and institutional barriers as well as incentive structures likely play a role in shaping the link between voters and incumbents (see for reviews Anderson, 2007; Healy and Malhotra, 2013; Ashworth, 2012).

As outlined by Healy and Malhotra (2013), a threefold understanding of the accountability link between voters and elected representatives provides useful. First is the retrospective view in the tradition of Key (1966), whereby incumbents are incentivized to act on behalf of voters by the prospects of reelection. Second is a selection perspective: Fearon (1999) highlights that past information is predominantly useful for mandating future leaders. Third is a psychological view (Kahneman, 2003; Lupia, 1994): Voters have to rely on cues and heuristics when facing complex decisions in information scarce environments such as the voting booth. Therefore, cognitive and emotional biases will affect the relation between incumbent performance, voter evaluations thereof and voting.

Empirically, it is challenging to detect and to separate these channels. They are oftentimes observationally equivalent. Are voters deselecting incumbents because they did not meet voters' standards or because voters learned that their quality is insufficient for the next term? As well, how can empirical patterns of incumbent support be detected given that challengers enter or exit strategically and that incumbents send strategic signals depending on the electoral business cycle?

A growing literature consequently draws on external shocks such as natural disasters<sup>2</sup> to infer under which circumstances and how voters react to incumbent performance and changes in their personal environments.

This literature assesses on the one hand whether voters attribute disaster occurrence to incumbents (in)action (Achen and Bartels, 2004, 2016; Fowler and Hall, 2016; Heersink, Peterson, and Jenkins, 2017), and on the other hand whether voters reward (punish) incumbents for their (in)successful management of natural disasters and for the provision of disaster relief. For this, considerable effects in both Western democracies (e.g. Healy, Malhotra, and Mo, 2010; Carlin, Love, and Zechmeister, 2014; Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Healy and Malhotra, 2009; Chen, 2013; Eriksson, 2016) and autocratic or de-

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<sup>2</sup>Sometimes, these events are referred to as exogenous. Natural disasters, as inferred from their qualification as 'natural', are generally seen as events beyond the control of society and are thus external to the system. Particularly, when disasters strike is a random event. This must not imply that the events are exogenous, however, as the geography they hit in is predetermined (Deaton, 2010). This requires an empirical strategy (such as fixed effects models) to deal with potentially correlated errors. Additionally, while the occurrence of disasters is external, the ex-ante-vulnerability of a society to disasters, and the ex-post management of the event is a under genuine political control (e.g. Gaillard, Liamzon, and Villanueva, 2007). I therefore relate to these events as 'external' rather than 'exogenous'.

veloping contexts (Lazarev, Sobolev, Soboleva, and Sokolov, 2014; Cole, Healy, and Werker, 2012; Fair, Kuhn, Malhotra, and Shapiro, 2017) are reported, with few exceptions (e.g. Bodet, Thomas, and Tessier, 2016). Generally, effective disaster management improves an incumbents' reelection prospects among the affected population (Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Healy and Malhotra, 2010). Psychological biases might lead voters to either attribute adverse events to incumbents in a sense of 'blind retrospection' (Achen and Bartels, 2004) and/or lead voters to evaluate performance myopically, though (Healy and Malhotra (2009), but see Bechtel and Hainmueller (2011)).

A recent theoretical literature highlights the fact that voters as well face the task of selecting 'good types' (Fearon, 1999; Ashworth, Bueno De Mesquita, and Friedenber, 2017; Alt, Bueno de Mesquita, and Rose, 2011; Ashworth, Bueno de Mesquita, and Friedenber, 2017; Thurner and Pappi, 1998). From this perspective natural disasters give voters cues on incumbent behavior under stressful circumstances and carry information on otherwise unobserved incumbent quality. This perspective emphasizes that natural disaster management contains valuable information for both affected and non-affected citizens as it signals expected future incumbent performance. Empirically, evidence on such 'demonstration effects' (Lazarev, Sobolev, Soboleva, and Sokolov, 2014) is scarce, though.

The present analysis draws on data from an exceptional exposure to repeated natural disasters to explore how voters hold incumbents to account with the information revealed from disasters occurrence: Four large-scale river floods, which hit southern (Danube river system) and eastern (Elbe river system) Germany both in August 2002 and again in June 2013. The disasters had a very similar aggregate impact with an estimated 8-9 billion Euros of disaster damage in both 2002 and 2013. The political response by federal and state governments was swift and massive: 7-8 billion Euros in emergency relief were distributed and well over 200.000 man days of federal forces employed, publicly judged to be an effective management of the floods (Pappi, Shikano, and Bytze, 2004). Both disasters occurred shortly before general elections in September 2002 and 2013. The present analysis extends Bechtel and Hainmueller (2011), who analyze one of the cases (the 2002 Elbe flood).

In the next section, I first clarify which theoretical expectations would follow from the three models of accountability (retrospective, prospective and psychological view) with respect to flood exposure and disaster relief. I then propose specific hypotheses that can be tested in the case at hand. The unique circumstances of these four floods provide a perfect scenario to explore whether and why adverse events of similar size and with similar incumbent behavior have heterogeneous electoral impacts. Important contextual factors, such as the institutional setting or political culture (Anderson, 2007) are held constant. This allows to identify features that usually do not lend themselves to a cross-case comparison.

The following analysis draws on election data from the 1994 to 2013 general elections in Germany and state elections in Bavaria. A time and united fixed effects model is used to compare changes in the proportional representation (PR) vote shares of federal and state incumbents. District level data is used to assess the large scale average effects of the flood,

municipal level data for a more fine grained analysis of spillovers. Cross-sectional as well as panel survey data allows to corroborate results and additionally assess mechanism.

My results support all three models of accountability, highlighting the complex decision making processes voters employ.

As a core finding, robust post-disaster policy is related to an increase in incumbent vote shares. In line with the literature (Bechtel and Hainmueller, 2011), I find that in north-eastern Germany, where several German states were affected in the Elbe river floods in 2002 and 2013 the federal incumbent is rewarded (chancellors Schröder/Merkel); in Bavaria, for the Danube floods 2002 and 2013, it however is the state level incumbent (governors Stoiber/Seehofer). These findings are consistent with both retrospective and prospective accountability models. In contrast to Bechtel and Hainmueller (2011), I do not find robust evidence for long-lasting electoral rewards after disaster shocks.

Importantly, adjacent non-affected areas see large spill-over effects, indicating that a broader mechanism of ‘demonstration effects’ occurs (Lazarev, Sobolev, Soboleva, and Sokolov, 2014). This is consistent with prospective, but not retrospective accountability models.

While the electoral response to the 2002 flood was large in magnitude, it was dampened in 2013. This can most likely be explained by a slightly larger temporal distance between flood and election, which links to models of myopic voter decision making (Healy and Malhotra, 2009). Cross-sectional survey data indicates that even such extreme events quickly fade from the minds of voters. Additionally, prior incumbent evaluations and repeated exposure potentially matter for the electoral response.

Overall, the case indicates that voters use the information revealed from incumbent behavior after severe events in a complex fashion. Attribution, timing, pre-disaster incumbent evaluation and repeated exposure explain substantial heterogeneity in effects. Theoretically, several of these mechanisms imply that voters use the information revealed by disaster management as well prospectively for candidate selection as opposed to retrospectively in a reward-punishment logic.

In the following sections, the theoretical framework and the state of the literature are discussed, before introducing case, data and research design. Section 3.6 presents results and robustness tests and discusses the findings, before Section 3.7 concludes.

## 3.2 Accountability, Selection and Natural Disasters

The relation between voter preferences, elections and policy can arise through two channels: On the one hand side, elections allow voters to decide between different options, i.e. party/politician types; on the other hand, elections allow voters to punish or reward incumbent performance and deselect underperformers (Dewan and Shepsle, 2011, 324f.). Both mechanisms lead to correlations of voting behavior with incumbent performance, which a large literature has empirically traced – on the macro level, e.g. economic performance, as well as on the individual level, e.g. crisis management or corrupt behavior (see for reviews

Healy and Malhotra, 2013; Ashworth, 2012; Anderson, 2007; Dewan and Shepsle, 2011; De Vries and Solaz, 2017).

I use the comprehensive theoretical overview of Healy and Malhotra (2013) to distinguish three broad accountability models, situate the empirical findings of the disaster literature thus far and develop expectations for the case at hand:

First of all, in the spirit of the ‘sanctioning view’, constituents relate their material fate to their political evaluations and vote choice. What Kiewiet and Rivers (1984, 370) refer to as ‘retrospective voting model’ implies voting in response to economic conditions that is backward-looking, oriented towards incumbents and based upon economic outcomes, not policies. Principle-agent theory would highlight in such a model that citizens provide politicians with incentives to act on their behalf by threatening deselection for bad (economic) outcomes (Gailmard, 2014). A central motivation for this perspective is the observation that, once selected, voters need some enforcement mechanism to discipline politicians as they might be tempted to shirk from their mandate (Ferejohn, 1986, 5; see as well Barro, 1973). Empirically, Key (1966) was the first to trace such a retrospective response to economic outcomes (see as well Fiorina, 1981). Recently, a broad literature has suggested that natural disaster shocks provide good opportunities to credibly draw inferences whether and when such retrospective incumbent evaluations occur: This chapter e.g. builds directly on Bechtel and Hainmueller (2011), who estimate an average increase of the incumbent party’s vote share of 7% in affected districts of the East German 2002 Elbe flooding, which they explain with “voter gratitude”. Similar explanations of retrospective disaster policy evaluations are reported for various election types (e.g. local, national parliamentary, presidential) and developed and developing contexts (e.g. Canada, Chile, Germany, India, Russia, Sweden, US) (Healy and Malhotra, 2009; Gasper and Reeves, 2011; Bodet, Thomas, and Tessier, 2016; Cole, Healy, and Werker, 2012; Lazarev, Sobolev, Soboleva, and Sokolov, 2014). This literature finds both rewards after effective management/flood relief payments and shows that bad management can lead to worse incumbent performance (Eriksson, 2016; Healy and Malhotra, 2010). In some settings, even the evaluation of the democratic system at large is at stake (Carlin, Love, and Zechmeister, 2014; but see Fair, Kuhn, Malhotra, and Shapiro, 2017).

From this, for any natural disaster, a first core hypothesis would follow: Voters reward effective disaster management and disaster relief. Hence aggregate political units that experience effective disaster relief should respond by rewarding incumbents electorally as well. I will investigate this hypothesis for the 2002 and 2013 floods at Elbe and Danube and assess whether incumbents see higher vote shares in flood-affected districts.<sup>3</sup> As in a federal system of shared government responsibility it is a priori unclear to which level voters attribute responsibility (Gasper and Reeves, 2011; Reeves, 2011), I will trace effects on the state

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<sup>3</sup>Note that recent theoretical contributions question this straightforward relationship. Ashworth, Bueno De Mesquita, and Friedenbergh (2017) propose a model where this relationship depends both on prior beliefs of the electorate and the governance production function.

and federal level.<sup>4</sup> Note that the retrospective reward/punishment-logic would not imply spill-over effects to non-flood-affected areas.

Secondly, in the spirit of the ‘selection perspective’, extreme events are argued to give voters cues on how to evaluate the government: They reveal information on incumbent quality that is relevant to assess their future expected performance. At the same time, natural disasters provide a case in point for elected officials to signal their quality to the electorate. Fearon (1999) prominently highlights that voters probably think about elections as a selection problem, where they aim to choose politicians that they see most likely to share their preferences and act upon them. This is close to the original rational choice formulation of the election problem by Downs (1957): in his formulation, rational preferences concern the party differential and are forward looking, although informational constraints imply that retrospective performance assessments can serve as a cue (see Thurner, 1998; Thurner and Pappi, 1998). Importantly, the link between voter behavior and politician behavior is affected by strategic selection and choices (Ashworth and Bueno De Mesquita, 2014). Thus, the surprise element of natural disasters lends itself to circumvent some of the endogeneity and reverse causality problems that researchers experience when assessing standard economic performance measures and campaign indicators. Past research has particularly highlighted that ‘demonstration effects’ can occur (Lazarev, Sobolev, Soboleva, and Sokolov, 2014), it has not linked this directly to a selection perspective, though.

From this, for any natural disaster, several hypotheses follow: Observationally equivalent to the retrospective perspective introduced above, the successful management of adverse events should lead to vote gains for incumbents (Cole, Healy, and Werker, 2012). Importantly, not only affected voters should be responding to the natural disaster, however. The fact that a politician proves to be a ‘good type’ is relevant for the whole electorate. As the information signal ‘successful management’ will most directly be picked up in affected districts and their vicinity, I expect positive spill-overs to adjacent areas from the floods. Another expectation follows from the selection perspective: When assessing whether an incumbent is a ‘good type’, her performance is compared to counterfactual challenger behavior<sup>5</sup>. Holding the policy constant, the better a challenger would be expected to perform, the less incumbent policy will be electorally rewarded.

Third, negative shocks might be attributed to incumbents irrespective of their responsibility. Achen and Bartels (2004) e.g. provide evidence that natural disasters, from weather shocks to shark attacks, correlate both with economic decline and decreasing vote shares of incumbents on average. Vote shares drop independent of the direct responsibility of elected officials for these events. Achen and Bartels (2004) argue that rational voters should only be punished/rewarded for social events (i.e. the management of the shock) and thus suggest that voters drift off into “blind retrospection”. They might just experience emotional biases

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<sup>4</sup>Empirically, this is only possible where electoral evaluations take place for both federal and state incumbent.

<sup>5</sup>Comp. e.g. the selection model in Fearon (1999, 73): “the better the expected performance of the less competent type [...], the more demanding is the optimal performance criterion”.

whereby ‘bad’ events lead to sad feelings that translate into incumbent evaluations (Healy and Malhotra, 2013). In this perspective, voting behavior is irrational and/or the attribution of responsibility flawed.<sup>6</sup> Empirically, the ‘blind retrospection’ argument has both seen recent empirical support (Heersink, Peterson, and Jenkins, 2017) and refutations (Fowler and Hall, 2016) – empirically, it is difficult to separate from the expected positive (negative) response after (in)successful disaster management and relief, though; in this chapter I can only assess the combined effect of robust relief to prior damage. Emotional biases are just one element of psychological aspects of voting behavior, however, when explaining patterns of retrospective voter decision making (Healy and Malhotra, 2013). As information acquisition is costly, voters have to rely on cues. But these cues are only imperfect shortcuts to assess incumbent performance. As well, voters may experience cognitive biases when assessing incumbent performance. In this respect, recency is a noted phenomenon. In the economic voting literature, voters are for example argued to substitute performance in the last year for overall term-performance due to an ‘end-heuristic’ (Healy and Lenz, 2014).<sup>7</sup> In the disaster literature, voters have been shown to behave myopically: They reward ex-post relief payments, but more efficient ex-ante disaster preparedness spending does not cross their time horizon (Healy and Malhotra, 2009).

From this, a further hypothesis follows: Disaster effects are relatively short-lived and influence incumbent performance evaluations only for a limited amount of time.

Finally, the theoretical literature highlights that retrospective and prospective evaluations, i.e. accountability and selection perspectives, are likely ongoing at the same time (Thurner and Pappi, 1998). Fearon (1999) explicitly develops a model that combines a selection and accountability logic. Similarly, Ashworth, Bueno de Mesquita, and Friedenberg (2017) note how forward and backward looking decision criteria might actually inter- and counteract each other. Empirically, these perspectives have not been tested, however, in a combined framework.

### 3.3 The 2002 and 2013 Floods Along Elbe and Danube

Following decades of relative quiet in both the Elbe and the upper Danube basin, beginning with the 2000s two large scale, ‘one hundred year’ floods of similar magnitude hit both river

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<sup>6</sup>The interpretation of these findings is disputed. In some models, the negative association of disaster damage with incumbent vote shares can be considered a rational outcome under information uncertainty (Ashworth, Bueno De Mesquita, and Friedenberg, 2017; Gailmard and Patty, 2014). Cole, Healy, and Werker (2012) argues that rational voters would attribute some share of negative disaster consequences to incumbents as long as responsibility for the event is not fully observed.

<sup>7</sup>As well, in the lab it has hence been shown that recent events carry more weight than temporal distant ones (e.g. Huber, Hill, and Lenz, 2012). And while parts of the literature report long lasting electoral effects of natural disasters, these effects are argued to decay over time as well (e.g. Bechtel and Hainmueller, 2011; Eriksson, 2016).

systems in 2002 and 2013 (Blöschl, Nester, Komma, Parajka, and Perdigão, 2013; Merz, Elmer, Kunz, Mühr, Schröter, and Uhlemann-Elmer, 2014).

These floods affected two very different socioeconomic and political contexts: The Elbe floods primarily struck in the federal states of Saxony and Saxonia-Anhalt, a relatively poor area of Germany still in a catch-up process after the economic decline in post-Cold-War Eastern Germany. The Danube floods mainly hit the southern parts of the state of Bavaria, an economically prosperous area of Germany. Both the 2002 and the 2013 flood are with respect to timing unique, occurring right before general elections: the Elbe floods in August 2002 (August 06, 2002 to September 12, 2002) were followed by federal elections on September 22, 2002; the Danube floods occurred in June 2013 (May 18, 2013 to July 4, 2013) with state elections in Bavaria on September 15, 2013 and federal elections following on September 22, 2013.

Both times concentrated heavy rainfall caused severe flooding in Central Europe, breaking multiple records such as an all-time high along the Elbe in the city of Dresden (7.40m in 2002 above normal) and along the Danube in the city of Passau (7.72m in 2013 above normal – a 500 year high). The record rainfalls in the upstream catchment areas of Elbe, Danube and their tributaries resulted in the adverse of simultaneous flood peaks on various rivers with subsequent breakages of dikes even further downstream in the lowlands of northern Germany (Elbe), Austria and Hungary (Danube). Both the 2002 and 2013 floods caused casualties (21 in 2002 and 8 in 2013 in Germany alone) and tens of thousands of people needed to be evacuated (30,000 in 2002 and 85,000 in 2013 in Germany alone). Both floods were classified as, depending on the location, 20 year to above 500 year flood events. (LUA, 2002; LFU, 2014; Bundesregierung, 2013) The floods caused billions of Euros worth of damage (approximately 9 billion in 2002 and approximately 8 billion in 2013) in Germany<sup>8</sup>.

Figure 3.1 shows state and district boundaries, the Elbe and Danube river system, as well as flood affected districts for 2002 and 2013.

The response of the federal and state governments was swift and massive. Over 200,000 man-service-days of federal forces (270,000 in 2002 and 215,000 in 2013) were employed to stabilize dikes and aid in evacuations (CSU, 2013; BMVg, 2002) and the federal government and the state governments agreed in both cases to each bear 50% of the costs. (BMI, 2013) A considerable part of this aid was handed out very quickly without much red tape, and directly to all affected households that applied, following federal and state level regulations.<sup>9</sup> In both cases the incumbent politicians, Gerhard Schroeder in 2002, and Angela Merkel in

<sup>8</sup>There of course is heterogeneity in the damage suffered by individual districts both within and between the events. The federal state of Saxony for example experienced damage amounting to about 6.1 billion in 2002 (8 of 13 districts severely affected). In the federal state of Bavaria damage amounted to 1.3 billion in 2013 (19 of 96 districts affected).

<sup>9</sup>e.g. in Saxony 2002, 500 Euro per affected person, maximum 2,000 per household, were handed out to all households that applied (Restriction: max. household income below 40.000 Euro) (SSR, 2002); in Bavaria 2013, this ‘instant flood support’ amounted to 1,500 Euro per household without any income restriction (BMI, 2013).



2013, together with their cabinets initiated large scale flood relief funds worth several billion euros (Bundesregierung, 2013).

Germany being a federal state, the political management of such events lies jointly in the hand of federal, state and district level politicians and administrators. While financial contributions were decided at the federal and state level (excluding damage to federal property), implementation of the flood loss compensation programs is in the hands of district officials. In terms of civil society, the flood sparked a large extent of grass-root mobilization among citizens who volunteered filling sand bags, offering shelter, and providing relief goods. Especially in 2013, social media networks were used to steer relief and dike protection work. Information flows were often times uncoordinated by government agencies and even sidelined official channels at times.

Finally, it is important to note how the federal elections in 2002 and 2013 differ from each other. The 2002 election was dominated by a weak incumbent, with polls indicating a clear victory for the opposition candidate, the governor of Bavaria (Hogwood, 2004; Pappi, Shikano, and Bytzek, 2004; Pulzer, 2003). The surprise win of the SPD with incumbent Chancellor Schröder, coming out with a plus in PR votes of only 6,000 and a five seat majority for his SPD-Greens coalition, was consequently convincingly linked to the exogenous shock of the 2002 floods, the robust response of the then SPD government (Bechtel and Hainmueller, 2011) and its successful display of crisis management skills (Bytzek, 2008, 2007). In line with this argument, the 2002 flood was noted to have played a major role in the media coverage of the 2013 electoral campaigns, especially of SPD and Greens (Eilders, Degenhardt, Herrmann, and von der Lippe, 2004). In contrast, the 2013 federal election was dominated by a strong CDU incumbent, Chancellor Merkel, who had lead the German economy successfully through the Euro crisis (Hoff and Hough, 2014).

## 3.4 Data

The following analysis draws on administrative election data and election survey data. Main dependent variable is the PR vote share (i.e. second/party vote share) of the incumbent in a geographic vicinity.<sup>10</sup>

First of all, the broader impact of the floods is analyzed with district level election data (N=402) on all German districts for the general elections 1994, 1998, 2002, 2005, 2009 and 2013 in 2013 boundaries (of these districts, 96 are located in Bavaria and 76 in former Eastern Germany). I gathered additional data on a standard set of control variables of economic voting (available for the time period 1998-2013).<sup>11</sup>

Second, I assembled a data set with municipality level election data for the states of Saxony (1998-2013), Saxonia-Anhalt (1994-2013), Lower Saxony (1998-2013) (Elbe flooded

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<sup>10</sup>I do not consider the first vote, as first vote shares are inconsequential for the federal incumbents standing and local district incumbents are directly responsible for neither flood preparedness nor flood relief.

<sup>11</sup>Data is obtained from the federal statistical office ([www.regionalstatistik.de](http://www.regionalstatistik.de))

states) and Bavaria (1990-2013) (Danube flood) to substantiate treatment effects and assess geographic spill-overs.<sup>12</sup> I additionally added control variables on population density and population size (logged) to capture heterogeneity in rural and urban municipalities.<sup>13</sup>

The coding of flood treatment on district level for 2002 and 2013 is based on districts calling a 'state of emergency' during one of the flood events as reported by the federal government (Bundesregierung, 2013). Flood coding on municipality level draws on flood satellite layers where these are available. Layers exist for the Elbe 2002 and 2013 flood in Saxonia and the Danube 2013 flood in Bavaria and Saxonia-Anhalt.<sup>14</sup> Where flood layers do not exist or where these were unreliable as satellite images were taken before/after flood peaks, I drew on government reports on affected rivers to code municipality affectedness (HND, 2002; BfG, 2014; Bundesregierung, 2013; LUA, 2002; LFU, 2014; AG Hochwasser, 2002).

Third, pre- and post-election survey data is used: Daily polling data (cross-section on district level) from Forsa is analyzed for 2002 and 2013 (weekly  $n = 2500$ ) and from Politbarometer for 2013 (weekly  $n = 1700$ , irregularly conducted in 23 pre-election weeks).<sup>15</sup> Finally, an online panel on 2009-2013 (electoral precinct level,  $N = 1002$ ) for Germany and two Bavarian pre-electoral cross-sectional surveys are used ( $N=3,036$  and  $N=4,730$ ). Within the surveys, respondents are coded as 'flood affected' if they live in affected districts/precincts as defined above.

### 3.5 Empirical Strategy

Events such as floods, although they are external to the system under study, are not under control of the researcher; estimation is therefore at risk of confounding treatment effects with selection bias (e.g. Sekhon and Titiunik, 2012). One approach to decrease necessary assumptions is a difference-in-difference framework (Lechner, 2010), drawing on unit and time fixed effects that control for time-invariant unobserved confounders in treatment and control observations by relating treatment to changes in outcome variables (Brüderl and Ludwig, 2015; Brüderl, 2010). Time-variant bias is controlled for where observable, otherwise,

<sup>12</sup>For the states of Brandenburg and Mecklenburg-West Pomerania, as well affected by the Elbe floods though to a minor degree, I could not obtain a continuous time series of community level electoral data, due to substantial changes in community boundaries following large reforms. These are therefore not contained in the municipality level analysis

<sup>13</sup>Data is obtained from the federal ([www.regionalstatistik.de](http://www.regionalstatistik.de)) and federal state statistical offices of the flood affected Länder Bavaria, Lower Saxony, Saxonia and Saxonia-Anhalt (see <https://www.destatis.de/DE/PresseService/Adressbuch/National.html> for their web-portals).

<sup>14</sup>Obtained from Saxonian State Government (<https://www.umwelt.sachsen.de/umwelt/wasser/8844.htm>) and Vista Cooperation <http://www.vista-geo.de/en/portfolio-items/flood-monitoring/> (many thanks to Heike Bach and Martina Hodrius from Vista for the kind provision of data and help in preprocessing GIS files).

<sup>15</sup>Data is obtained from Gesis, <http://www.gesis.org/en/home/>.

parallel trends have to be assumed. In terms of the Rubin Causal Framework (Rubin, 1974), the approach assumes that for observations  $i, i = 1, n$ , counterfactual electoral outcomes in treatment regions ( $Y_i^0|F = 1$ ) are comparable to observed electoral outcomes in non-affected observations ( $Y_i^0|F = 0$ ). With a difference-in-difference strategy, two time periods  $t$  and a vector of time-variant controls  $X$ , the identification problem contracts to the assumption that cross-sectional bias between treatment and control regions is, conditional on covariates, constant over time, i.e.  $E(Y_{it}^0 - Y_{i,t-1}^0|F = 1, X) = E(Y_{it}^0 - Y_{i,t-1}^0|F = 0, X)$  (Lechner, 2010).

The average treatment effect on the treated (ATT) is then estimated by a fixed effects regression with

$$Y_{it} = \alpha_t + \beta F_{it} + \delta_i + X_{it}\gamma + \epsilon_{it}$$

with  $\alpha_t$  as time dummies to account for time fixed effects,  $F_{it}$  as binary measure for flood exposure,  $\delta_i$  as unit fixed effect, intended to capture unobserved heterogeneity of units,  $X_{it}$  as a vector of time variant controls and  $\epsilon_{it}$  as idiosyncratic error term. This specification will be applied to both the district, municipality and partially the survey data.

As the analysis deals with geographically concentrated events, potential unobserved time-variant confounders most likely are geographical confounders as well. Hints to apply the method cautiously comes from placebo analysis. Appendix Table B.1 reports pre-treatment trends for areas later affected by the flood – while pre-treatment trends for the SPD in the Elbe river system reveal substantively low and insignificant coefficients, coefficients for CDU/CSU vote and the SPD in Bavaria show non-parallel pre-treatment trends. This is likely due to time-varying spatial heterogeneity in the political units.<sup>16</sup>

The following strategies are adopted to address these potential common trend violations:

First and most importantly, where feasible, the district level sample is in a second step restricted to proximate regions, thus analyzing effects within affected states/regions along the Elbe or Bavaria. The underlying assumption is that geographic proximity improves the balance of unobserved confounders. For the district level, models three to six of Appendix Table B.1 e.g. show that geographic proximity substantially improves the placebo analysis. For the municipality level, this strategy is adopted on the one hand by restricting the control sample and on the other hand by using nearest neighbor matching on longitude and latitude,

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<sup>16</sup>This might e.g. stem from differences in party system: Germany has three relatively distinct party systems. In Bavaria, the CSU, sister party of the CDU, competes with distinct conservative profile as dominant right-of-center force. In the rest of western Germany, CDU and SPD are the main competitors. In the former East Germany, on the one hand side the party system is more volatile, with more swing voters, on the other hand the Left Party as successor of the GDR's Socialist Union Party competes partially on par with CDU and SPD. Additionally, the 1998 election saw a large decline of the CDU especially in eastern Germany (column 4 in Appendix Table B.1 estimates CSU second vote shares in an average Eastern district dropping by about 10 percentage points). With the post-unification build-up process taking longer than expected, the government was especially unpopular in the East, which explains the large negative effect specifically for the CDU there (Arzheimer and Falter, 2003; Hough, 2003).

as the average balance of observable and unobservable confounders will plausibly improve relative to any random pair of municipalities.<sup>17</sup>

Second, the baseline models will estimate effects for a long pre/post-treatment time-span, such that short-term fluctuations are less likely to bias results. Intuitively, this invokes the interpretation of treatment effects under parallel trends between treatment period and pre-treatment period as opposed to treatment and an average of pre-treatment periods. Robustness tests report effects with a short term two-period comparison, where time-varying short-term factors are more likely held constant over cases. Additionally, I employ a modeling strategy invoking flexible trends as suggested by Brüderl and Ludwig (2015, 374-381): This approach acknowledges that heterogeneity in time trends between units might be present. Using data from additional time periods, differences in levels and differences in trends are canceled out. The treatment effect is thus estimated allowing for individual-specific slopes. This approach has the great advantage of relaxing the parallel trends assumption altogether and allowing for heterogeneity in trends between treatment and control units.

Third, the relevance of potential violations of the common trend assumption is assessed drawing on rolling cross sectional as well as panel survey data, exploiting variation in respondent location and time to disentangle (treatment induced) spill-overs from (confounding) differences in pre-treatment trends.

Finally, where geographic confounders are observable, they will be included in the analysis. For example, district level controls include election state-year dummies when state governors (with home advantage) run in general elections (here: Bavaria 2002 and Lower Saxony 1998 and 2002).

## 3.6 Results

This section reports findings for the 2002 and 2013 floods in Germany along Elbe and Danube. District level data shows that the incumbents generally profit from relief effort. In northeastern Germany, the federal incumbent gained electorally. In Bavaria, the state level incumbent is rewarded. Municipality level analysis in northern Germany (Saxony, Saxonia-Anhalt and Lower Saxony) and southern Bavaria indicates that flood effects are just as large in close-by compared to directly affected municipalities. Survey data indicates that even broader spill-overs occurred especially in 2002 along the Elbe. Finally, there is substantial variation in the size of rewards over time, which are much smaller in 2013. This suggests that voters take past exposure and the campaign environment into account.

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<sup>17</sup>One tradeoff with this strategy is that a geographic SUTVA violation is plausible: With geographic proximity, the observation of flood response (and consequently information on incumbent quality) is more likely. Comparing geographically close units then implies comparing treated units among themselves, even if these are likely to be more balanced with respect to unobservables. Under the assumption that the relief effect dominates the damage effect, coefficients will be biased towards 0 with this approach.

### 3.6.1 District Level Analysis 2002 and 2013

#### Results for 2002

In the following, results for the Elbe and Danube floods in 2002 and 2013 are reported in Table 3.1 (Elbe Flood 2002 and Danube Flood 2002, Federal Elections), Table 3.2 (Elbe Flood 2013 and Danube Flood 2013, Federal Elections) and Table 3.3 (Danube Flood 2013, State Election).

As can be seen from Table 3.1, along the Elbe the federal incumbent SPD with chancellor Schröder saw an increase in their second vote share, the state incumbent CSU with challenger Stoiber, governor of Bavaria, increased its vote share along the Danube (in Bavaria). Effects are large and significant for the Elbe flooded districts (4.5 percentage points in Model 1), when estimated against the trend in all non-affected and the Elbe states. The effect size drops to 1.6 percentage points in Model 2, when it is estimated against the trend in not directly affected districts in all Elbe states. Two explanations for this drop in coefficients between Model 1 and 2 are possible: Spill-overs to not-directly affected areas, or violations in parallel trends. Analysis of pre-election district level survey data (reported in Appendix Section B.3) indicates that spill-overs are the likely explanation, as SPD-support rises sharply in the weeks after flood exposure. This implies that the true effect in directly affected municipalities is substantively large with around 4.5 percentage points. This analysis replicates a central part of Bechtel and Hainmueller (2011), who used electoral district data to show this effect. Following the argument in Bechtel and Hainmueller (2011) on ‘lasting voter gratitude’, i.e. that voters still reward the SPD in the next federal election of 2005, I as well report tests for these temporal spill-overs. For this, I include a lag for the 2002 flood in Model 1 and 2. Contrary to the analysis in Bechtel and Hainmueller (2011), I cannot replicate their result. This is most likely the case as the temporal spillover effect picked up by Bechtel and Hainmueller (2011) is due to the geographical concentration of Elbe regions in East Germany, with East Germany being on a generally more pro-SPD trend in 2005 compared to the rest of the control group.

Model 3 and 4 recover the effect on electoral returns for the CDU/CSU in Danube-flood-affected districts. As can be seen from Model 3, flood effects are smaller, with 3.0 percentage points, and similarly significant. They are similar in size when estimated only on Bavarian districts.

Hence, for the 2002 Elbe flood the federal incumbent, for the 2002 Danube flood the state incumbent saw a substantial increase in his vote share in flood affected areas. These results are especially remarkable for Bavaria, where both state and federal incumbent appeared on the ballot - electorally, only the former profited. Note that the 2002 electoral bonus for the state incumbent could be interpreted as a punishment of the federal incumbent (the SPD lost as the CSU gained electorally) – Appendix Chapter B.4 discusses, why the effects are consistent with an attribution of responsibility and subsequent rewards to the federal level along the Elbe and the state level along the Danube.

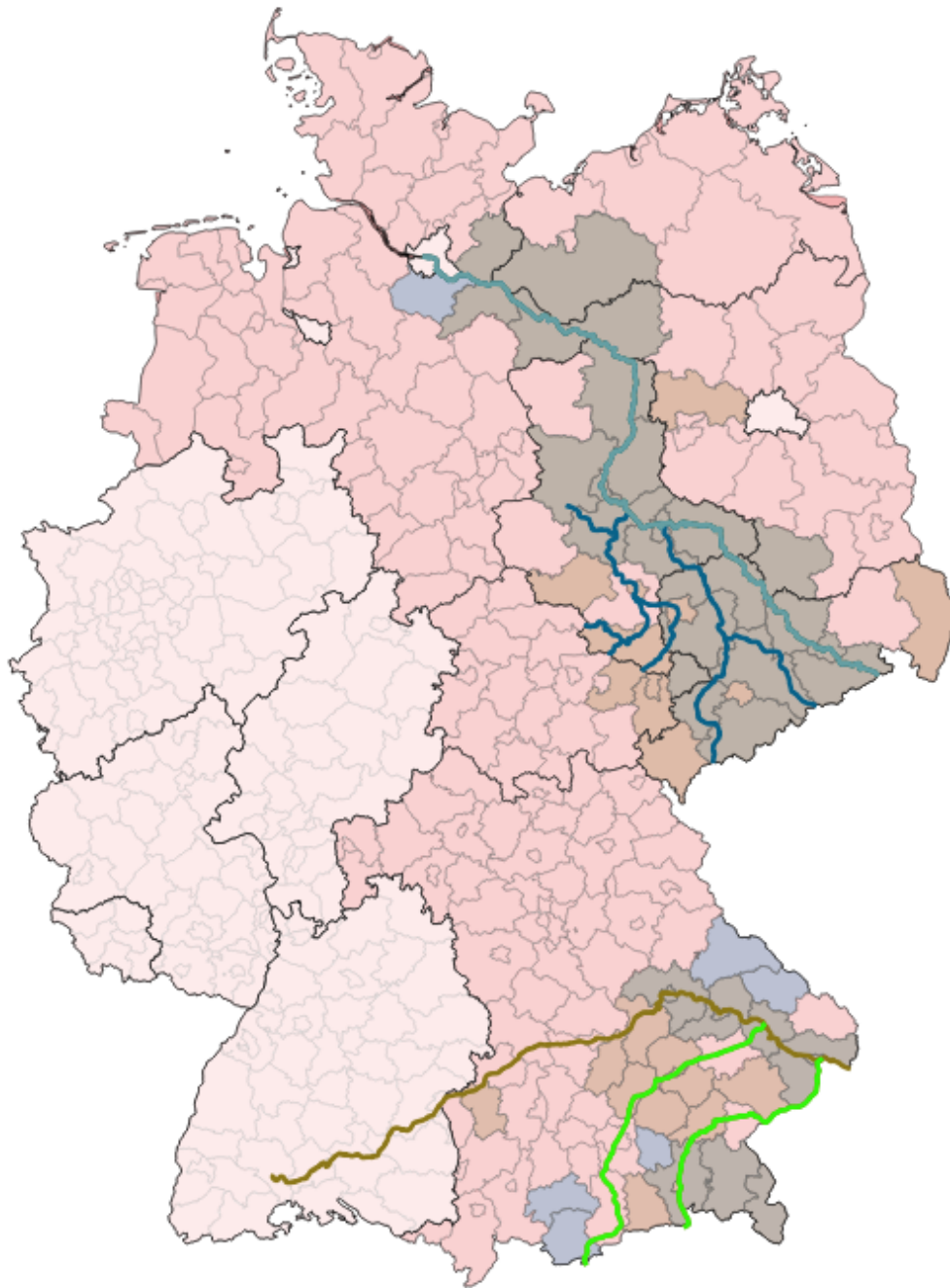


Figure 3.1: Map of German district and state boundaries with Elbe and Danube river systems and disaster districts. The Elbe river system is depicted in light and dark (main tributaries) blue. The Danube river system is depicted in brown and green (main tributaries). Unaffected districts in unaffected (affected) states are depicted in light (darker) pink. 2002 and 2013 affected districts are depicted in dark green. Only 2002 (2013) affected districts are depicted in blue (brown).

Table 3.1: 2002 Elbe and Danube flood effects on district level

	Unaffected and Elbe	Elbe States	Unaffected and Bavaria	Bavaria
	(1)	(2)	(3)	(4)
Dep. var.: PR vote share	SPD	SPD	CDU-CSU	CSU
Elbe Flood 2002	4.84*** (0.70)	1.65** (0.70)		
Lag on Elbe 2002	0.64 (0.50)	-0.28 (0.51)		
Danube Flood 2002			2.99*** (0.64)	2.85*** (0.60)
Year 2002	-1.20*** (0.21)	0.64 (0.44)	1.84*** (0.22)	9.78*** (0.37)
Year 2005	-6.24*** (0.35)	-8.03*** (0.70)	-1.29*** (0.28)	0.19 (0.40)
Year 2009	-17.9*** (0.38)	-21.2*** (0.75)	-5.22*** (0.32)	-6.85*** (0.50)
Constant	48.6*** (6.82)	52.7*** (7.69)	65.1*** (5.64)	-58.0 (52.7)
Economic and political controls	Yes	Yes	Yes	Yes
N	1224	548	1060	384
Clusters	306	137	265	96

District level data for Germany (total yearly n=402). Fixed effects estimation with district and election year fixed effects drawing on 1998-2009. Standard errors clustered by district reported in parentheses. Controls on population density, share of employed on district population, share of employed in agriculture, in production, in manufacturing, in construction, in trade, in financial services, in other services, and state incumbent being an SPD incumbent, as well as SPD or CSU governor running for chancellor included. Control group consists of all districts in states unaffected by floods and all unaffected districts within states affected by this respective flood. Dependent variable is the party's PR vote share as indicated. Respective control group means 2002: 41.31 (SPD, unaffected and Elbe states), 44.74 (Elbe states only), 45.23 (CDU-CSU, unaffected states and Bavaria), 58.76 (CSU, Bavaria).

**Results for 2013**

Turning to the 2013 floods, as can be seen from Table 3.2, in the 2013 federal elections the federal incumbent CDU/CSU profited along the Elbe (Model 1 and 2), but not along the Danube (Model 3 and 4). The CDU vote shares in flood-affected regions rose, significant at the 1% level, by about 2.2 percentage points when compared to the trend of districts in non-affected and Elbe states. Compared to close-by districts in the Elbe states, the CDU is up about 1.1 percentage points in flood affected districts (significant at the 5% level). Danube flood affected districts, on the other hand, show a negative trend of -2.4 percentage points compared to districts in states unaffected by the flood and Bavaria. This negative trend however becomes substantially small (-0.35 percentage points) and insignificant with the control group drawing from Bavaria only. Analysis of pre-election district level survey data (reported in Appendix Section B.3) indicates that a common trend violation is more likely, as incumbent support in polling data does not change substantially with disaster occurrence. This indicates that the true effect in directly affected municipalities is substantively small with around 1.1 percentage points along the Elbe and close to 0 along the Danube for federal elections.

However, the state incumbent CSU saw positive electoral returns in flood-affected districts in the State Elections preceding the Federal Elections by one week. As reported in Table 3.3, flood affected districts show significant (at the 5%-level) positive returns of about 1.6 percentage points (Model 1), robust to the inclusion of control variables (Model 2, estimated effect size of 1.9 percentage points, significant at the 10%-level).



Table 3.2: 2013 Elbe and Danube flood effects on district level

	Unaffected and Elbe (1) CDU	Elbe States (2) CDU	Unaffected and Bavaria (3) CDU-CSU	Bavaria (4) CSU
Dep. var.: PR vote share				
Elbe Flood 2013	2.35*** (0.45)	1.01** (0.49)		
Danube Flood 2013			-2.41*** (0.33)	-0.38 (0.31)
Year 2005	-1.69*** (0.32)	-4.58*** (0.66)	0.26 (0.30)	1.02*** (0.35)
Year 2009	-1.90*** (0.35)	-2.70*** (0.79)	-3.22*** (0.33)	-5.46*** (0.39)
Year 2013	6.43*** (0.36)	6.04*** (0.70)	4.49*** (0.36)	1.59*** (0.45)
Constant	21.4*** (4.82)	24.8*** (6.23)	59.9*** (5.61)	69.5*** (7.11)
Economic and political controls	Yes	Yes	Yes	Yes
N	1224	548	1060	384
Clusters	306	137	265	96

District level data for Germany (total yearly n=402). Fixed effects estimation with district and election year fixed effects drawing on 1998 and 2005-2013. Standard errors clustered by district reported in parentheses. Controls on population density, share of employed on district population, share of employed in agriculture, in production, in manufacturing, in construction, in trade, in financial services, in other services, and state incumbent being an SPD incumbent, as well as SPD or CSU governor running for chancellor included. The treatment group consists of districts flooded at respective river. Control group consists of all districts in states unaffected by floods and all unaffected districts within states affected by this respective flood. Dependent variable is the party's PR vote share as indicated. Respective control group means 2013: 40.95 (CDU, unaffected and Elbe states), 39.36 (CDU, Elbe states only), 44.27 (CDU-CSU, unaffected states and Bavaria), 49.60 (CSU, Bavaria).

Table 3.3: 2013 flood effects on district level in Bavaria for state elections

	Bavaria	
	(1)	(2)
Dep. var: PR vote share	CSU	CSU
Floods in district	1.57** (0.63)	1.88* (0.98)
Year 2008	-17.3*** (0.46)	
Year 2013	-13.3*** (0.32)	3.94*** (1.08)
Constant	60.9*** (0.22)	35.7 (36.5)
Economic and political controls	No	Yes
N	270	180
Clusters	90	90

Voting district data for State Level Elections in Bavaria (total yearly n=90). Fixed effects estimation with district and election year fixed effects drawing on the 2003, 2008 and 2013 state election. Standard errors clustered by district reported in parentheses. Control variables include log of population, share of employed, share of immigrants, incoming migration, construction work, district tax income, district debt, district affected by ‘relatives affair’ and allowing for a distinct time trend for northern Bavaria. Control variables only available for 2008 and 2013. Dependent variable is the CSU total PR vote share in an electoral district. Respective control group mean 2013: 46.67 (CSU, Bavaria).

These results indicate that the federal incumbent CDU/CSU was electorally rewarded for flood management, but only in Elbe-affected states, and to substantially a lesser extent compared to 2002 with about 1 percentage point. Similar to 2002, along the Danube, flood exposure did not relate positively to the electoral performance of the federal incumbent. Voters rewarded the state incumbent at the Bavarian state elections, but again to a lesser extent as in 2002. Expressing the treatment effects as a share of control group means<sup>18</sup>, the

<sup>18</sup>Drawing on the coefficients for the main specifications Model 1 and 3 in Table 3.1, and Model 2 in Tables 3.2 and 3.3.

effect size varies between about 12% for the Elbe 2002, 7% for the Danube 2002, 3% for the Elbe 2013 and 4% for the Danube 2013. This gives an indication for the substantially smaller effects in 2013 as compared to 2002.

### 3.6.2 Municipality Level Spill-Overs for 2002 and 2013

Municipality level data allows for a fine grained assessment of spill-over effects. For this, effects for directly affected municipalities (i.e. municipalities along the flooded rivers) are contrasted to indirectly affected municipalities (i.e. other municipalities in affected districts) and municipalities more distant from the flood regions in the states of Saxony, Saxonia-Anhalt and Lower Saxony (Elbe) and southern Bavaria (Danube).

Table 3.4 reports results for affected municipalities along Elbe (Model 1) and Danube (Model 2) in 2002. Affected municipalities along the Elbe see a large increase in SPD vote shares of 4.7 percentage points. This effect is similarly large in close-by municipalities in affected districts (4.3 percentage points) and even higher in other non-affected municipalities of East Germany (5.4 percentage points). For Danube affected municipalities, estimated effects are smaller (1.0 percentage points), with slightly larger effects in close-by municipalities (1.3 percentage points).

Table 3.5 reports results for federal elections in affected municipalities along Elbe (Model 1) and Danube (Model 2) in 2013. Along the Elbe, effects are at about 1.2 percentage points for directly affected municipalities, slightly higher in close-by regions and estimated substantially larger (4.0 percentage points) in other, non-affected municipalities in the East. In southern Bavaria, affected municipalities see a slight decline in CSU vote shares of about 0.5 percentage points (Model 2), with a similarly large effect in close-by municipalities for state elections. However, for the state elections, CSU vote shares did increase significantly by about 1.9 percentage points (Model 3). Effects are significant at the 1%-level.

These results are consistent with the district level results and lend additional detail to the overall picture: Within affected districts, strong spill-over effects are present.

### 3.6.3 Robustness

Besides the consistency of district level and the more fine-grained municipality level analysis, I provide a summary below for additional tests with the data:

I conducted the following robustness tests for the district level regressions, aimed at lending plausibility to the underlying assumption of parallel trends:

Appendix Table B.2 reports results for the main specifications of Tables 3.1 and 3.2 with the dependent variable being the respective competitor party. As expected, results mirror the analysis in the main tables – i.e. when the SPD profits, the CDU loses and vice-versa. This implies that parallel trend violations for specific parties are unlikely to bias results.

Appendix Table B.3 reports results restricting the time fixed effects to the preceding legislative period only. Results are robust to this estimation strategy, although less strong

Table 3.4: 2002 flood Effects on municipality level - Elbe and Bavaria

	Elbe Municipalities	Danube Municipalities
	(1)	(2)
Dep. var.: PR vote share	SPD	CSU
Affected municipality	4.53*** (0.17)	1.03*** (0.25)
Other municipality in affected district	4.21*** (0.22)	1.33*** (0.23)
Other non-affected municipalities in East	5.27*** (0.22)	
Election year 2002	3.64*** (0.21)	8.04*** (0.22)
Election year 1998	5.34*** (0.18)	-5.11*** (0.16)
Election year 1994		-1.07*** (0.11)
Population controls	Yes	Yes
N	2340	5296
Clusters	1061	1324

Municipality level data for the states of Saxonia (1998-2013), Saxonia Anhalt (1994-2013), Lower Saxony (1998-2013) (all Elbe affected) and Bavaria (1990-2013) (Danube). Observations in Bavaria are restricted to southern Bavaria. Fixed effects estimation with municipality and election year fixed effects. Standard errors clustered by district reported in parentheses.

for the Elbe. This implies that parallel trends violations are unlikely to stem from flood-unaffected years.

Finally, Appendix Table B.4 reports results of a fixed effects regression with individual slopes (Brüderl and Ludwig, 2015, 374f.). This approach relaxes the necessary parallel trends assumption altogether and allows for individual district-level trends in party vote shares. Results are generally robust, besides the positive coefficient for CDU vote shares along the Elbe 2013, which is still positive, though close to zero and insignificant.

For the municipality level, I conducted a test drawing on geographic proximity and matching (comp. for a similar approach Keele, Titiunik, and Zubizarreta, 2015), aimed at creating better balance on unobservables and lending credibility to the causality of main

Table 3.5: 2013 flood effects on municipality level in the Elbe states and southern Bavaria

	Elbe States	Southern Bavaria	
	(1)	(2)	(3)
Dep. var.: PR vote share	CSU	CSU	CSU
Other municipality in affected district	1.22*** (0.24)	-0.53*** (0.15)	2.21*** (0.26)
Affected municipality	1.19*** (0.25)	-0.50*** (0.16)	1.86*** (0.30)
Rest of eastern Germany	3.96*** (0.43)		
Year 2009	2.33*** (0.11)	-8.66*** (0.096)	
Year 2013	9.13*** (0.11)	-0.49*** (0.11)	-15.2*** (0.18)
Year 2008			-20.5*** (0.14)
Population controls	Yes	Yes	Yes
N	3183	3972	3972
Clusters	1061	1324	1324

Municipality level data for federal elections (Model 1) for the states of Saxonia (2005-2013), Saxonia Anhalt (2005-2013), Lower Saxony (2005-2013) (all Elbe affected) and for federal (Model 2) and state elections (Model 3) for Bavaria (2005-2013) (Danube). Observations in Bavaria are restricted to southern Bavaria. Fixed effects estimation with municipality and election year fixed effects. Standard errors clustered by district reported in parentheses. Control variables include log of population and population density. Estimated constant is not shown.

treatment effects: Appendix Table B.5 reports results from nearest neighbor matching on latitude and longitude of municipality centroids as well as population density and log population. Results generally confirm the effects for directly affected municipalities, with the exception of the Elbe 2002. There, the treatment effect is still positive, but strong spillovers likely bias against finding effects for directly affected as opposed to close-by not directly affected municipalities.

Finally, Table 3.6 and Table 3.7 lend support to the results found for Elbe and Danube 2013: Table 3.6 estimates a fixed effects regression for a panel of the voting-age population between 2009 and 2013. In Elbe-flooded districts, the evaluation of the CDU/CSU and Merkel increases significantly by about 0.6 points on a 10-point scale. For the evaluation of CDU/CSU, this is as well the case in East Germany. In Bavaria and other unaffected districts, no change in evaluations appears. Similarly, drawing on two cross-sections on Bavarian state level, the attitude towards the CSU (Model 1 and 2) and incumbent Seehofer (Model 3 and 4) increased significantly for flood affected districts, as can be seen from the interaction term in Table 3.7 (by about 0.5 points, robust for the ex- (Model 1 and 3) or inclusion (Model 2 and 4) of individual level controls). Such survey data is not available for the 2002 time-period.

Table 3.6: 2013 panel data on CDU and Merkel rating

	Rating on 10 point scale of	
	(1) CDU	(2) Merkel
Respondent in Elbe flood 2013 district	0.58* (0.31)	0.62** (0.29)
Other district in East Germany	0.48* (0.27)	-0.19 (0.27)
Respondent in Danube flood 2013 district	-0.065 (0.21)	-0.10 (0.23)
Other district in Bavaria	-0.061 (0.20)	0.13 (0.22)
Constant	5.57*** (0.054)	6.30*** (0.056)
Panel wave effects	Yes	Yes
N	5972	6029
Clusters	1002	1002

Estimation with observation and time fixed effects. Robust standard errors in parentheses. Panel data (n=1002) from GESIS fielding in several waves in 2009 and 2013; observations from post-election wave in 2009 and pre-election waves in 2013 used. Post-flood waves (all in 2013) coded as affected if respondent from disaster district.

Table 3.7: 2013 cross-sectional survey data on CSU and Seehofer rating

	Rating on 10 Point Scale of			
	(1) CSU	(2) CSU	(3) Seehofer	(4) Seehofer
Flood in 2013 in district=1	-0.10 (0.14)	-0.25* (0.15)	0.027 (0.17)	-0.067 (0.16)
Flood in 2013 in district=1 × 2013=1	0.51** (0.23)	0.46** (0.21)	0.53** (0.24)	0.47** (0.22)
2013=1	0.33* (0.18)	0.18 (0.16)	-0.17 (0.21)	-0.13 (0.17)
Constant	5.64*** (0.12)	6.00*** (0.18)	5.77*** (0.15)	6.12*** (0.18)
Individual controls	No	Yes	No	Yes
Region controls	Yes	Yes	Yes	Yes
N	7766	7753	7601	7590

Data from two pooled cross-sections from 2011 (on state-level smoking referendum in Bavaria, n=3,036) and 2013 (on state election survey from Making Electoral Democracy Works project, n=4,730). Treatment effect estimated by interaction term of year effect and Danube-flood affected electoral district. Standard errors, clustered by district, in parentheses. Control variables include year effect, as well interacted with ‘relatives affair’ in district in 2013, respondent female, age categories, household size, religion, dummies for low education, high education, self-identified ‘left’, self-identified ‘right’.

Overall, robustness tests with survey data and additional specifications and estimation strategies for the district and municipality level data lend support to the results presented above.

### 3.6.4 Discussion

In line with the literature, this chapter shows that effective disaster management is associated with positive electoral prospects of incumbents. This finding is in line with a retrospective reward-punishment model (Ferejohn, 1986), where effective disaster relief is electorally rewarded by affected constituents. However, this results are consistent with a prospective selection model as well. Three additional findings require a more detailed discussion:

First of all, electoral rewards are similar in directly affected and close-by municipalities. This is a strong indication that what Lazarev, Sobolev, Soboleva, and Sokolov (2014) called ‘demonstration effects’ is driving the observed aggregate response and, more generally, is consistent with a prospective accountability model (Fearon, 1999). As the size of the population directly affected by a disaster will regularly be low relative to the eligible population, this is actually the relevant group for assessing the overall political effects of natural disasters. For example, only about 13% of the German electorate (7.7 of 62 million) actually live in flood-affected districts. Within these, only about 60% actually live in flood affected communities.<sup>19</sup> Again, within these only a margin has experienced a direct threat and/or damage from the floods<sup>20</sup> As unaffected municipalities, and thus unaffected households, show strong reactions to the disaster, it is likely that overall the electoral response is driven by these ‘demonstration effects’. This argument is important, as it adds a new theoretical interpretations to the disaster literature: So far, the accountability literature drawing on disasters as external shocks considers primarily the ‘affected’ population, i.e. those citizens experiencing damage (and disaster relief), being evacuated and/or saved, and links this to retrospective and psychological voting models (comp. review of Oliver and Reeves, 2015). It is e.g. argued that this population shows ‘voter gratitude’ (Bechtel and Hainmueller, 2011) following disaster relief and/or is ‘myopic’ towards disaster preparedness (Healy and Malhotra, 2009). The relevance of ‘demonstration effects’ highlights, however, that much broader processes are going on: natural disasters reveal information on the quality of the incumbent to the public, and this can, as indicated especially for the 2002 flood, make a large difference for voting behavior. Evidence on this can be seen from survey data: Figure 3.2 displays rolling cross-sectional survey data, aggregated to monthly evaluations of the SPD for untreated states, flooded districts along the Elbe and unflooded districts in 2002 eastern Germany. On the one hand side, this assesses whether the positive effects replicate in survey data as well (comp. similar analysis in Bechtel and Hainmueller, 2011). On the other hand, I thereby evaluate whether the behavior of voters in adjacent areas follows similar temporal patterns. As can be seen, the evaluation of the SPD increases substantially with flood onset in the beginning of August in all three regions. This increase is even steepest in non-affected former East German districts. One note of caution concerns the upward trending evaluation of the SPD in Elbe flooded states already in July.<sup>21</sup> Such changes are not expected from retrospective accountability models, but can be explained with forward looking incumbent evaluations (Thurner and Pappi, 1998).

<sup>19</sup>Based on the sample of communities at hand from Bavaria, Lower Saxony, Saxony and Saxonia-Anhalt.

<sup>20</sup>Rough calculation leads to an upper bound of 17% of the population in affected districts and 28% of the population in affected municipalities being directly affected. These figures are calculated by the share of all people evacuated (85.000) and an upper bound for households experiencing damage ((insured damages (180.000) / share of households insured against disaster damage (0.32)) \* average household size (2.01) = 1.301.475) (Bundesregierung, 2013).

<sup>21</sup>Appendix Figures B.1-B.3 show the same evaluations for Danube 2002 and Elbe and Danube 2013. No similarly strong patterns are observed there.



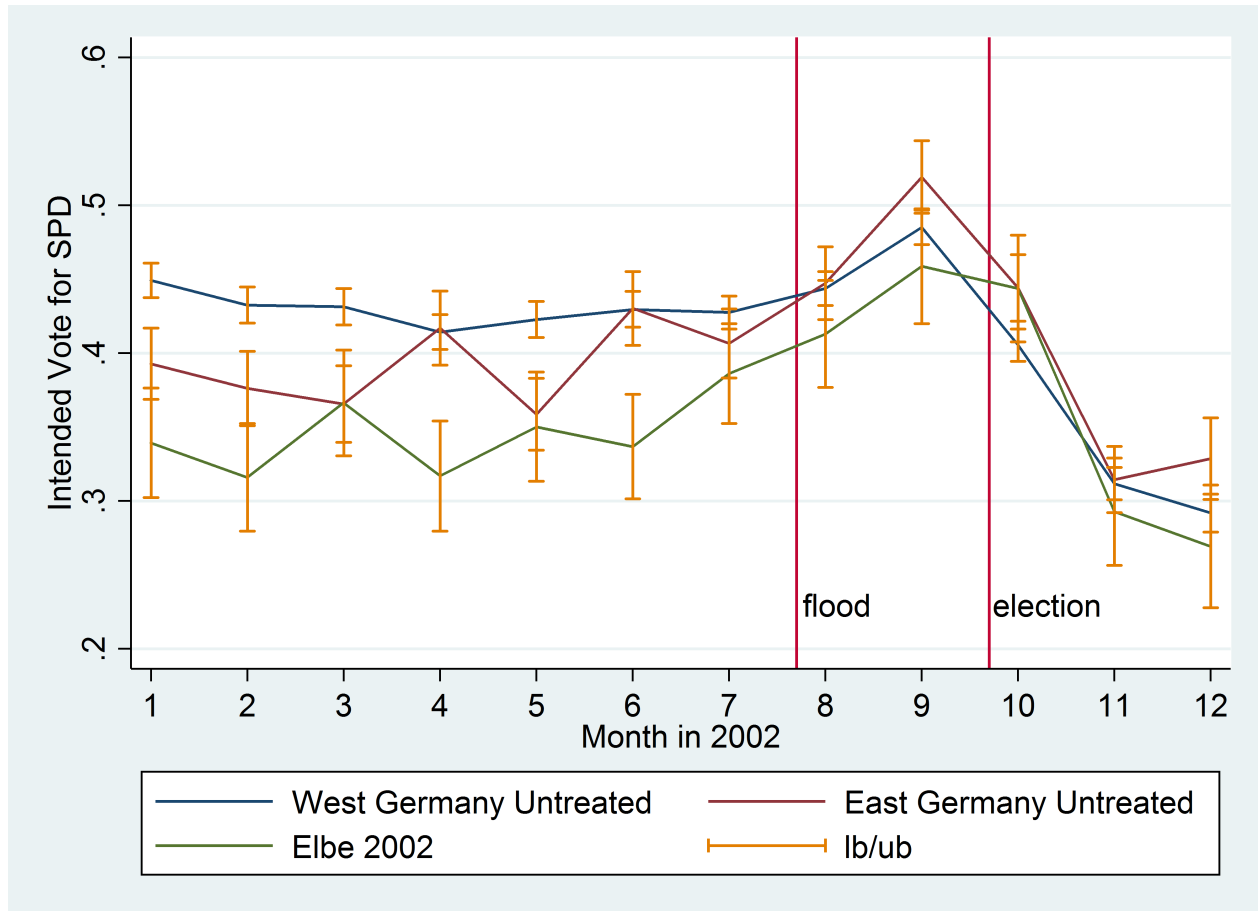


Figure 3.2: Share of voters intending to vote SPD in 2002. The figure reports average monthly vote intention for the ‘SPD’ by subgroups. Respondents are a random drawn of a Germany wide rolling cross-section (average weekly  $n=2500$ , Forsa Study Nr ZA3909). Yellow bars depict 90%-confidence intervals. Subgroups are coded through electoral precincts. Respondents declaring to abstain, giving no answer or reporting to be undecided are excluded.

Second, the attribution of responsibility is key for explaining electoral changes. The observed effects imply that federal incumbents were attributed the response in the states along the Elbe, while state incumbents were attributed the response in Bavaria. This is surprising, as the costs of disaster relief were split between state and federal level and the forces on the ground were as well both federal and state forces working hand in hand. Survey evidence from 2013 is consistent with this presumption: Table 3.7 reports the change in rating of state incumbent Seehofer and his party CSU along the Danube in a cross sectional difference-in-differences set up. In flooded districts, both ratings increase. This increase is substantial with an average of about 0.5 points on a 10-point-scale. The effect stands in contrast to evaluations of Merkel and the CDU (estimated from a 2009-2013 panel of respondents): As indicated

in Table 3.6, the Merkel/CDU rating improved in Elbe affected districts to a similar extent, but not along the Danube. The similar effects for incumbent and party ratings indicate that effects work both through the evaluation of the incumbent and through his/her party. Still, future research has to explore in more detail the factors that led to this differential attribution. Both the aggregate and the survey evidence indicate that the attribution of response to political actors is not straightforward, even when administrative arrangements are relatively transparent. Lazarev, Sobolev, Soboleva, and Sokolov (2014) highlighted the importance of visits on the ground, in an effort to gain visibility. This is an aspect that could matter here as well. For 2002, Appendix Section B.4 summarizes anecdotal evidence that patterns of federal and state incumbent visits in flooded areas go along with the later observed electoral rewards. Alternative explanations are possible, though. Malhotra and Kuo (2008) e.g. emphasize the importance of both partisan bias and adequate information. Especially partisan bias could have led in the CSU stronghold Bavaria to an attribution of disaster management with the state incumbent only. Even more, the role of the federal government was likely more pronounced along the Elbe, where disaster relief and flood management had to be coordinated among multiple states.

Third, the difference in absolute and relative effect size in 2002 and 2013 deserves an explanation. Despite a similar socio-economic environment and similar aggregate disaster impact, the floods differed in their aggregate electoral implications. Below, I point to time discounting as most plausible explanation. This highlights as well that voters experience biases when holding governments to account, in line with psychological voting models.

Given previous findings in the literature, the difference between a four month and a two month duration between flood onset and election should not be that consequential. Existing research on natural disasters and accountability e.g. estimates effects with much longer time-spans: e.g. Cole, Healy, and Werker (2012) estimate for Indian voters a cut-off of a one-year time period when disasters do no longer impact aggregate electoral outcomes. Similarly, Lazarev, Sobolev, Soboleva, and Sokolov (2014) note increased government support one year after forest fires in Russia. Bechtel and Hainmueller (2011) and Eriksson (2016) even argue for a persistent influence of natural disasters on vote choice over several electoral cycles. Still, even if an especially high impact of the disaster just before election day could be expected (as e.g. argued by Chen, 2013), the drops in coefficient size are unexpected. However, Figures 3.3 and Figure 3.4 show that even these extreme events quickly disappear from the minds of voters (see as well Bytzek, 2008). As can be seen from both figures, with flood onset the flood was the most important topic both in affected districts/states and in the rest of Germany. However, the interest in the floods reached its peak 2-3 weeks into the disaster and quickly faded afterwards. While the 2002 flood was still among the top issues for some voters at election day, it was not in 2013.<sup>22</sup> Similarly, federal incumbent support along the

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<sup>22</sup>Bytzek (2008), based on polling data, the timing of symbolic crisis management events and newspaper reporting even argues that 2002 effects could have been larger, had the 2002 elections occurred two weeks closer to the 2002 floods.

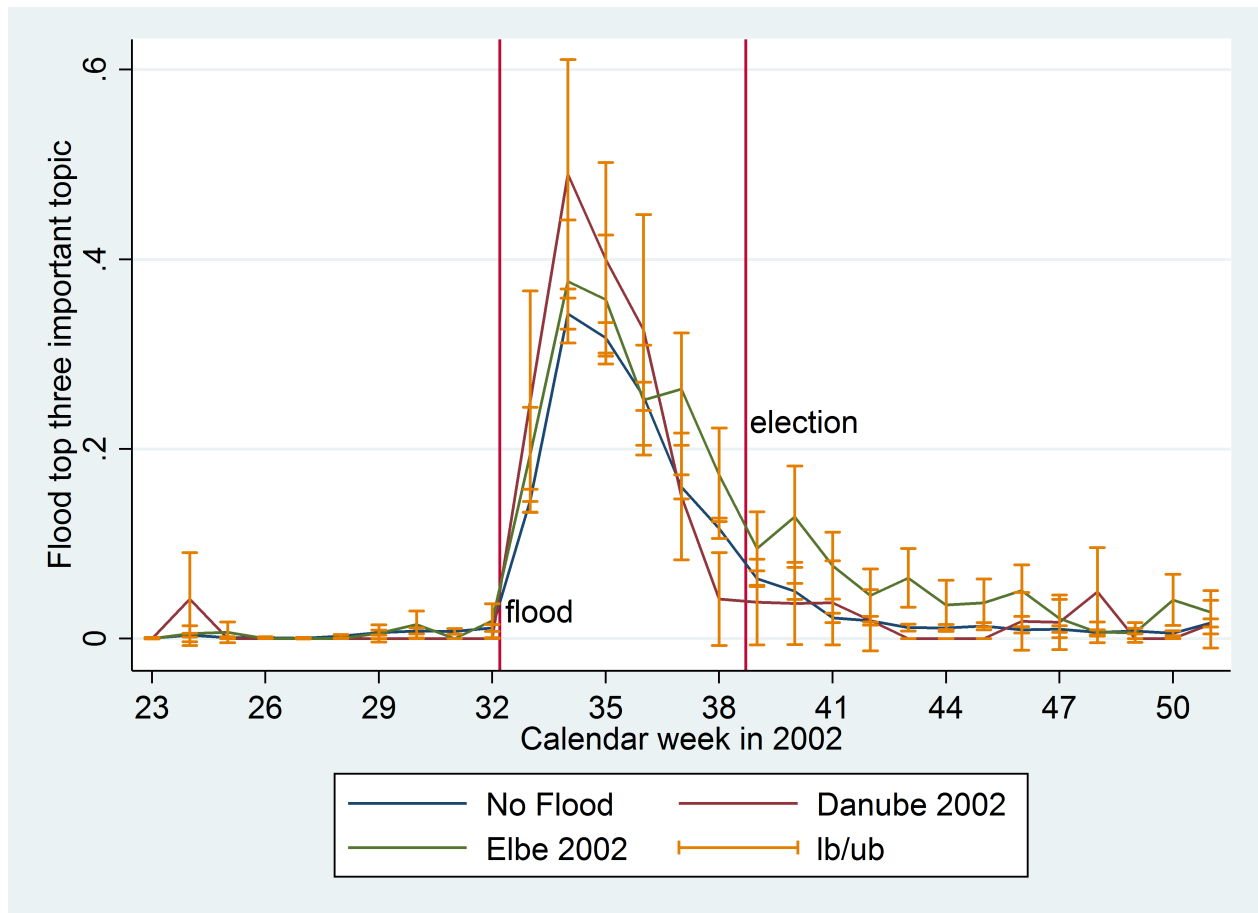


Figure 3.3: Share of population seeing the flood as top two problem in Germany 2002 before elections. The figure reports average weekly mentioning of ‘Floods’ as top two most important problem in the country by subgroups. Respondents are a random draw of a Germany wide rolling cross-section (average weekly  $n=2500$ , Forsa Study Nr ZA3909). Yellow bars depict 90%-confidence intervals. Subgroups are coded through electoral precincts.

Elbe in 2002 saw a remarkable spike just after the floods and into election day - only to fade quickly and even below pre-flood levels just afterwards in flood affected, close-by and more distant districts (comp. Figure 3.2). Especially if ‘demonstration effects’ among non-affected voters are driving the aggregate response, this temporal pattern could explain the reduction in effect strength. The information gained with disaster management was potentially replaced with other relevant campaign events in the meantime. However, this interpretation highlights the role of psychological biases in the retrospective performance evaluation of governments (Healy and Malhotra, 2013). If time is that critical, time discounting could be incorporated into theoretical models when comparing different informational signals over time. Further evidence from Huber, Hill, and Lenz (2012) indicates that the temporal dimension likely

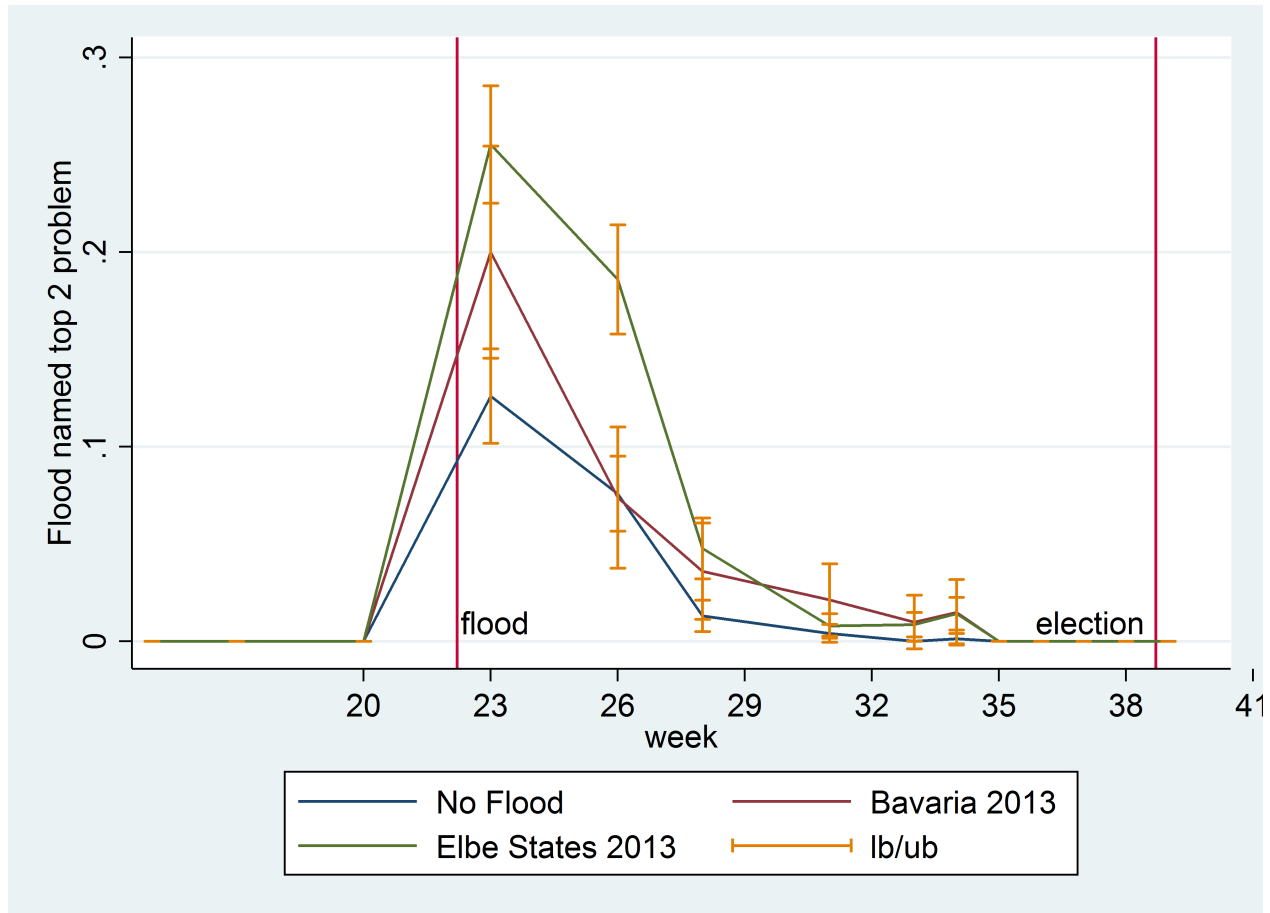


Figure 3.4: Share of population seeing the flood as top two problem in Germany 2013 before elections. The figure reports average weekly mentioning of ‘Floods’ as top two most important problem in the country by subgroups. Respondents are a random draw of a Germany wide rolling cross-section in 2013 with 18 pre-election waves (average weekly  $n=1700$ , Politbarometer Study Nr ZA5677). Yellow bars depict 90%-confidence intervals. Subgroups are coded through states.

plays an important role. They similarly report that subjects in a lab experiment weigh the most recent information more heavily in evaluating incumbents and note that this limits the application of retrospective voting theory - just as well as the selection perspective.<sup>23</sup>

<sup>23</sup>I discuss alternative explanations for this differential flood response between 2002 and 2013 related to expectations of government performance, expected challenger behavior and disaster preparedness in Appendix Section B.5.

## 3.7 Conclusion

This chapter analyzes the linkage between disaster management and voting behavior following large scale river floods in the Danube and Elbe river systems of northern and southern Germany in 2002 and again in 2013. Due to idiosyncrasies in candidate selection and election timing, the electoral performance of the federal incumbent can be analyzed for the Elbe flood, and the electoral performance of the federal and the state incumbent for the Danube flood. I report a general picture of electoral rewards over the four cases, in line with the literature (e.g. Lazarev, Sobolev, Soboleva, and Sokolov, 2014; Bechtel and Hainmueller, 2011; Cole, Healy, and Werker, 2012; Fair, Kuhn, Malhotra, and Shapiro, 2017; Gasper and Reeves, 2011; Healy and Malhotra, 2009). In Elbe flood affected districts, the federal incumbent profits, in Danube flood affected districts the state incumbent – pointing to the importance of the attribution of policy to actors. Most importantly, large spillovers of a similar magnitude as the main effect to non-affected areas are present within affected districts – an indication that voters learn on the incumbent’s type through disaster management, consistent with a selection model of accountability. The present analysis hence goes beyond the argumentation that beneficial policy after disasters (e.g. household-level aid) leads to retrospective rewards by affected voters (comp. e.g. Healy and Malhotra, 2009; Bechtel and Hainmueller, 2011) and indicates that the electorate sees government policy as an information signal for incumbent quality and future performance. Future research could investigate, which dimensions of incumbent quality are affected. It has e.g. been argued that the handling of the 2002 disaster was interpreted as indication of strong leadership skill Pappi, Shikano, and Bytze (2004).

While incumbents have profited largely from effective disaster relief in 2002, they did less so in 2013. In relation to the average control group incumbent vote share, the effect size amounts to 12% for the Elbe flood 2002 and 7% for the Danube flood 2002, but only 3% for the Elbe flood in 2013 and 4% for the Danube flood in 2013. Evidence from weekly cross-sectional data implies that even such extreme events fade quickly from the minds of voters. The smaller average 2013 effect is therefore consistent with the slightly larger timespan between flood event and election in 2013. More broadly, this implies that voters face psychological hurdles when assessing retrospective performance of incumbents and that recency effects are important (Healy and Lenz, 2014).

The observations in this chapter tie to a larger literature on the electoral consequences of distributional policy. It has been observed that incumbents use their discretionary power to deploy federal funds for strategic means (e.g. Francken, Minten, and Swinnen, 2012; Garrett and Sobel, 2003; Neumayer, Plümper, and Barthel, 2014; Reeves, 2011; Brollo and Nannicini, 2012). This could well be true in the case at hand, as especially the former East Germany is considered to have a large share of swing voters (Hough, 2003). The differing rewards for federal and state incumbents as well suggest that policy making could be strategically employed. However, in the analysis at hand these strategic factors are likely held constant over cases. Future research should track to what extent this plays a role (e.g. by exploring

the level of disaster relief effort with larger temporal distance to elections) and how voters react in this strategic interaction (Gailmard and Patty, 2014).

The analysis speaks as well to the literature on ‘blind’ retrospection. This literature assesses whether natural disasters *per se*<sup>24</sup> influence incumbent evaluations and are an indication of irrelevant events influencing voter behavior, with mixed results: Some see a relationship (e.g. Achen and Bartels (2004, 2016); Heersink, Peterson, and Jenkins (2017)) others, in part reanalyzing this data refute a relationship (e.g. Fowler and Hall (2016); Remmer (2014)). The results presented here indicate that the post-disaster management of the shock might be the important variable to look at. While disaster occurrence is external to the political system, the politician’s reaction to the disaster is not and might well be taken up by voters as a signal of her quality. This allows for a reinterpretation of prominent findings: Achen and Bartels (2004) argue that coastal communities in New Jersey affected by the 1916 shark attacks irrationally punished the incumbent president for events outside his control. But besides ‘blind’ retrospection, an in the eyes of voters insufficient cushioning of this shock might just have signaled lacking quality and led to this electoral reaction.

Finally, as the world faces and will face an expected increase in the frequency of severe natural disasters (IPCC 2013), understanding the political consequences of natural disasters and especially their recurrence becomes important. The case at hand is a unique example in this regard, with two centennial floods affecting the very same river systems within a decade. The heterogeneity of electoral reactions over cases points to the need of additional research: How does repeated disaster exposure change voting behavior over a longer time period? When and to what extent do citizens demand more effort in disaster relief? What role do prior beliefs on incumbent and challenger quality play? Answering these questions will be important in a world where disasters of large magnitude will not only become more frequent, but will likely occur within geographical clusters (IPCC 2013).

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<sup>24</sup>See for other events influencing personal livelihoods, though not caused by politicians Bagues and Esteve-Volart (2016), Healy, Malhotra, and Mo (2010), Busby, Druckman, and Fredenhall (2016), but Fowler and Montagnes (2015) and Healy, Malhotra, and Mo (2015).

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# Chapter 4

## Die Münchner Ergebnisse im Bundes- und Landesvergleich: Ein Ude-Effekt in München?

This research has been published in *Exit Polls und Hybrid-Modelle. Ein neuer Ansatz zur Modellierung von Wählerwanderungen*, edited by Klima, André, Helmut Küchenhoff, Mirjam Selzer, and Paul W. Thurner, Springer: Wiesbaden, 2017, Chapter 2, pp. 21-35, ISBN: 978-3-658-15673-2 (print); 978-3-658-15674-9 (online).

### 4.1 Summary

*This chapter is embedded within a broader research project of André Klima, Helmut Küchenhoff, Mirjam Selzer, and Paul W. Thurner, proposing new approaches for estimating voter transitions with an application to voter transitions between 2008 and 2013 state, 2009 and 2013 federal and 2013 state and federal elections in the municipality of Munich, Germany. The chapter starts by demonstrating that the 2013 Munich federal and state election results follow very much the average voting behavior on federal, and even more on state level. This supposes that voter transitions in Munich are potentially similar in other voting districts of Bavaria, or even Germany. In the second part of the chapter, I investigate specific observed differences between the Bavarian trend and the Munich trend for the state elections 2013. I propose a particular candidate effect and interpret this as local incumbency advantage: The former mayor of Munich, Christian Ude, ran as opposition leader for the SPD state election campaign. I therefore expect a particular spill-over of a local incumbency effect in Munich. As the electoral system provides voters with two votes in two tiers, where both votes counts towards the PR result, but Ude was listed as candidate only in the second tier, I assess whether voters use the electoral system in this way. With a difference-in-difference design, I can show that SPD total votes increase by about 2-3 percentage points in Munich, but SPD second*

*votes by about 4-5 percentage points. This increase is likely a consequences of former Green voters splitting their vote in favor of Christian Ude. Theoretically, this would be consistent with a model, where candidate effects work predominantly where voters are nearly indifferent on party terms.*

## 4.2 Published Book Chapter

The reader is kindly referred to the above-mentioned source, online available at doi: 10.1007/978-3-658-15674-9\_2, for the full article. The appendix is available as OnlinePlus-site on the Springer book web pages<sup>1</sup> (see as well Appendix Chapter C).

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<sup>1</sup><http://www.springer.com/de/book/9783658156732>



# Chapter 5

## How to Increase Turnout in Low Salience Elections: Quasi-Experimental Evidence on the Effect of Simultaneous Second-Order Elections on Political Participation

(Leininger, Rudolph and Zittlau 2018)

This research is has been published in *Political Science Research an Methods*, 2018 (online first: 2016), Issue 6, Volume 3, pp. 509-526, ISSN: 2049-8470 (print); 2049-8489 (online), under authorship of Arndt Leininger (University of Mainz), Lukas Rudolph and Steffen Zittlau (University of Mannheim).

### 5.1 Summary

*Voter turnout in second-order elections is on a dramatic decline in many modern democracies. This article investigates how electoral participation can be substantially increased by holding multiple of these less important elections simultaneously. Leading to a relative decrease in voting costs, concurrent elections theoretically have economies of scale to the individual voter and thus should see turnout levels larger than those obtained in any stand-alone election. Leveraging as-if-random variation of local election timing in Germany, we estimate the causal effect of concurrent mayoral elections on European election turnout at around 10 percentage points. Exploiting variation in treatment intensity, we show that the magnitude of the concurrency effect is contingent upon district size and the competitiveness of the local race.*

## 5.2 Published Article

The reader is kindly referred to the above-mentioned source for the full article, online available at doi: [10.1086/685378](https://doi.org/10.1086/685378). The appendix is available as supplementary material on PSRM's article site (see as well Appendix Chapter D). Replication files are available in the PSRM Dataverse at doi: [10.7910/DVN/TJTZT2](https://doi.org/10.7910/DVN/TJTZT2).

# Chapter 6

## Natural Disasters and Political Participation: Evidence from the 2002 and 2013 Floods in Germany (Rudolph and

Kuhn 2018)

This research has been published in *German Politics*, 2018 (online first: 2017), Volume 27, Issue 1, pp. 1-24, ISSN: 0964-4008 (print); 1743-8993 (online), under authorship of Lukas Rudolph and Patrick M. Kuhn (Durham University, United Kingdom).

### 6.1 Summary

*How do natural disasters affect electoral participation? The existing social science literature offers contradictory predictions. A considerable body of research in sociology and psychology suggests that traumatic events can inspire pro-social behaviour, which might increase turnout. Yet, political science has long held that even minor changes to participation costs of low benefit activities can lead to considerable drops in civic engagement. Consequently, natural disasters should reduce electoral participation. We show how these distinct views can be jointly analysed within the Riker-Ordeshook model of voting. This paper then reports results on the impact of the 2002 and 2013 floods in Germany on turnout in federal and state elections in Saxony and Bavaria, conducted few weeks after the floods. Analyzing community level turnout data, and drawing on a difference-in-differences framework, we find that flood exposure has a consistent negative effect on turnout. This indicates that the increase in the costs of voting outweighed any increase in political engagement in our case and stands in contrast to findings from developing contexts, where flood management was convincingly linked to electoral participation.*

## 6.2 Published Article

The reader is kindly referred to the above-mentioned source for the article, online available at doi: [10.1080/09644008.2017.1287900](https://doi.org/10.1080/09644008.2017.1287900). The underlying data to reproduce the numerical results in the paper are available at the Harvard Dataverse at doi: [10.7910/DVN/X3VUSW](https://doi.org/10.7910/DVN/X3VUSW). The appendix can be found as supplemental material on the article site of German Politics (see as well Appendix Chapter E).

# Chapter 7

## Turning Out to Turn Down the EU - The Mobilization of Occasional Voters and Brexit<sup>1</sup>

### Abstract

*Large amounts of low-propensity voters participated in the Brexit referendum. Theoretically, I argue that when turnout baselines are low the issue-specific mobilization potential of these voters helps explain election outcomes. More generally, we can expect that outcomes become more volatile with intermediate increases in electoral participation. This is all the more likely in referendums, with weak partisan preferences and single issues dominating decision making. I find evidence consistent with the argument for the Brexit referendum. I draw on an instrumental variables approach exploiting large amounts of rainfall on referendum day and show that occasional voters marginal to rainfall predominantly supported Leave. Survey data is consistent with the view that especially Leave supporters were susceptible to small variations in voting costs and that turnout intention was heterogeneous with respect to Leave/Remain preferences. This contributes to understanding the Brexit decision and, more generally, highlights the role of turnout for referendum outcomes.*

### 7.1 Introduction

Low turnout potentially poses serious challenges to both the legitimacy of representation and the legitimacy of public policy. As citizens do not abstain at random, low turnout likely

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<sup>1</sup>I thank John Aldrich, Matthias Fatke, Sona Golder, Arndt Leininger, Ingrid Mauerer, Steffen Murau, Oliver Pamp, Christopher Prosser, Paul Thurner, participants at the 2016 EITM summer institute and the 2017 MPSA conference as well as audiences at LMU Munich for helpful comments and suggestions. UK rainfall data provided by the UK Met Office is kindly acknowledged.

implies both representation being skewed and policy being shifted towards the interests of voters as opposed to the eligible population (Lijphart, 1997).<sup>2</sup> As socioeconomic covariates and electoral participation correlate, the core expectation of a large body of research is that left-of-center parties and positions are underrepresented with decreases in turnout (Tingsten, 1937).

Empirically studying the effects of turnout is challenging, however, as turnout decision and electoral choice are endogenously related both at the individual and the aggregate level. A large recent literature therefore focuses on exogenous variation in turnout due to institutional features or external variation in voting costs to investigate the partisan effects of turnout. In many contexts, substantial partisan effects are reported. However, the evidence largely centers around presidential (e.g. Hansford and Gomez, 2010; Fowler, 2015), general and state legislative (e.g. Artés, 2014; Fowler, 2013; Ferwerda, 2014) or local elections (Cepaluni and Hidalgo, 2016; Arnold and Freier, 2015; Finseraas and Vernby, 2014).<sup>3</sup>

We know much less about who turns out and partisan effects of participation in referendums. Much of the evidence stems from the Swiss case: Exogenous increases in turnout in Swiss referendums (due to compulsory voting) led to increased support for leftists policy (Bechtel, Hangartner, and Schmid, 2016). Similarly, drawing on the introduction of postal voting for Swiss federal ballot propositions, Hodler, Luechinger, and Stutzer (2015) show that less educated voters were most sensitive to variations in voting costs.

Beyond the Swiss case, turnout in EU/EEC referendums has been studied as a notable exception, mostly with observational research designs (Hobolt, 2005, 2009).<sup>4</sup> However, so far the literature has not studied extensively whether and how turnout, and particularly increases in turnout, are related to referendum outcomes. Amongst the few papers that address this issue is Qvortrup (2016), who reports a strong cross-country correlation between turnout levels and EU/EEC approving referendum shares in all EU/EEC referendums between 1972 to 2015. The present study contributes directly to this question, as this relationship has not yet been confirmed with a causal identification strategy. This fills an important gap: While

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<sup>2</sup>This widely cited argument is not uncontested. See e.g. Saunders (2012) for a nuanced discussion.

<sup>3</sup>Indeed, these studies usually find a relationship between turnout and vote shares. One set of studies draws on variation in institutional design to establish the partisan effects of increasing turnout. In some contexts the change in representation is marginal (Ferwerda, 2014), but in most quite sizable. The direction of effects has a tendency: There seems to be more evidence that left-of-center parties and positions profit in many contexts (Schmid, 2015; Fowler, 2013, 2015; Bechtel, Hangartner, and Schmid, 2016), in particular settings however it is right-of-center electorates (Cepaluni and Hidalgo, 2016; Berinsky, 2005) or both positions (Finseraas and Vernby, 2014) that see increases in electoral support. This picture is reinforced by another set of studies, using changes in weather conditions to instrument turnout: e.g. Hansford and Gomez (2010) report substantial partisan effects of voter turnout in the US, benefiting the Democrats, and using rainfall as instrument. Partisan effects are noted for rainfall in other elections as well (see Arnold and Freier, 2015, for an overview).

<sup>4</sup>Relatedly, there of course is a large literature explaining turnout, especially lower turnout levels, for European Parliament elections. As these are generally seen as second-order elections, turnout is likely influenced more by domestic factors (Hobolt, 2009, comp. e.g.). However, some EU centered explanatory factors of turnout likely carry over to (high stakes) EU referendums (e.g. De Vreese and Tobiasen, 2007).

it has been argued that turnout intention in EU elections covaries with attitudes towards the EU, especially if they reach a first-order importance (comp. e.g. Hobolt and Tilley, 2014, Chap. 8, Fn. 12), most EU-related research on voting behavior focuses on factors that influence vote choice, not participation (Hobolt and Tilley, 2014).

In the following, I propose that with one-off single-issue referendums, we can expect the mobilization potential of occasional voters to be lopsided, i.e. for some occasional voters their participation likelihood is higher. Hence, whenever issue salience, campaigning or other external factors succeed in mobilizing occasional voters, and hence where we observe increases in turnout, this should affect electoral results. This argument builds on the observation that low propensity voters have heterogeneous preferences (Finseraas and Vernby, 2014). In this light, studies researching turnout and ballot initiatives have argued that salient issues influence the turnout intention (Biggers, 2011), but likely mobilize specific segments of the population over others (Campbell and Monson, 2008).

I apply this question to a recent referendum with high salience, the ‘United Kingdom European Union membership referendum’ (‘Brexit referendum’) on 23 June 2016. Given the expected closeness and the high stakes involved, turnout reached a comparatively high level of 72.2% – in comparison, the last UK general election that saw turnout above 70% was in 1997, and turnout for European Parliament elections never reached 40% in the UK.<sup>5</sup> Pollsters and forecasters had both seen a vote for Remain as likely outcome of the election, the Leave share of 51.9% came as a surprise for many, and the role of turnout has been publicly debated in the aftermath. The setting therefore is an important case to analyze the relevance of an increase in turnout for electoral outcomes.

Drawing on plausibly exogenous variation in turnout due to exceptionally high rainfall on election day in parts of the country and a first differences design, I argue that a stronger turnout increase led to an increase in the Leave share. In line with earlier research I find that rainfall depressed turnout. Subsequently, I report that a (weather induced) variation in turnout of about one percentage point led to an increase in the Leave share of around 0.6-0.7 percentage points in England. Placebo tests on unaffected outcomes (2014 European Election turnout; postal voting turnout) lend support to my identification strategy. Additionally, I draw on survey data from the British Election Survey. Individual-level analysis indicates that small variations in the costs and benefits of voting especially affected Leave supporters. I can as well show that the observed turnout increase was likely heterogeneous with respect to Referendum preferences: Compared to regular voters, occasional voters are not found to exhibit stronger preferences for Leave as such. However, among occasional voters, those holding the sovereignty and immigration issue as most important for their voting decision score higher on the self-reported turnout intention scale. Likewise, close to election day, past

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<sup>5</sup>A similar referendum with high salience in recent times was the 2014 referendum on Scottish independence with an even higher turnout of 84.6%. More generally, EU membership referendums regularly see high participation rates (Hobolt, 2009, 9), close to turnout in general elections (e.g. in Denmark, comp. Green-Pedersen, 2012), but rarely higher.

supporters of the United Kingdom Independence Party (Ukip) have a significantly higher likelihood of participation compared to non-Ukip supporters.

The chapter contributes to the literature on turnout and vote shares in the under-studied context of referendums: My findings show that while left wing parties have endorsed the Remain position, turnout increase in a single-issue context with weak partisan cues must not lead to better representation of these positions. This highlights that with low turnout baselines, the population of occasional voters holds sufficiently heterogeneous views to be important for the outcome (if mobilized). It additionally raises the question how voters resolve conflicts between partisan preferences and issue orientations in referendums (Selb, Kriesi, Hänggli, and Marr, 2009).

As well, the chapter contributes to the emerging literature trying to understand the ‘Brexit’ decision: So far, emerging findings on the role of turnout are mixed: Some report correlations between high turnout and Remain support (Manley, Jones, and Johnston, 2017) others between high turnout and Leave support (Zhang, 2016). The present analysis is the first to use a causal identification strategy. I argue that rainfall on election day serves as an external shock to voting costs. This reveals that marginal occasional voters that participated in the referendum were on average largely leaning towards Leave.

Finally, the chapter adds to a recent, growing literature that shows that weather conditions affect electoral participation in many settings (see Arnold and Freier, 2015, for an overview).

The following sections will discuss under which conditions the mobilization of occasional voters should influence electoral outcomes. Subsequently, the case of the Brexit referendum, the data used and the research design will be summarized. Section 7.6.1 and 7.6.2 then present and discuss the results, before section 7.7 concludes.

## 7.2 When Mobilization Makes a Difference in Referendums

Elections oftentimes do not center around economic issues. This, however is one of the major foundations of Tingstens proposition: Abstention is predicted by low socio-economic status (SES); in turn, when turnout increases, it would follow that the participation of low SES-voters increases relatively, and as their (economic) interests are represented by leftist policies and parties, outcomes should shift relatively in this direction (Tingsten, 1937). But what can we expect with referendums focusing on single policy questions? In the following, I will present a more general theoretical argument on the consequences of increasing turnout in referendums and develop expectations for the case of the Brexit referendum.

Conceptually, let the eligible population consist of regular voters and occasional voters.<sup>6</sup> I understand regular voters as the share of the eligible population that we would regularly

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<sup>6</sup>DeNardo (1980) has made a similar argument and formalized this.



expect to turn out absent motivational factors, e.g. without an intense campaign environment, a salient issue/specific personalities on the ballot, institutional factors, specific weather conditions, etc. Among occasional voters<sup>7</sup> only a fraction turns out. How high this fraction is depends on the above mentioned motivational factors.

The population now is called to vote on a referendum with a binary yes/no choice. A voter selects whichever option gives her a higher (expected) utility (Downs, 1957). At the same time, in line with the Riker-Ordeshook-Model of voting (Riker and Ordeshook, 1968), the participation of voter  $i$  occurs if  $p * (|EU_i(\text{yes}) - EU_i(\text{no})|) + D - C > 0$ . This means that a voter participates if the material benefits of voting (the probability of making a difference for the outcome,  $p$ , times the expected utility increase in choosing one's preferred outcome) and immaterial benefits of voting,  $D$ , are larger than the voting costs,  $C$ .<sup>8</sup> I follow the broad understanding of the  $D$ -term by Riker and Ordeshook (1968, 28), who subsume under the term standard civic duty but as well expressive elements. Particularly relevant here is what they call 'the satisfaction from affirming a partisan preference'.

Importantly, we can expect that the  $D$ -term is heterogeneous over voters. Regular voters will, by definition, have a high  $D$ -term that pushes them above the participation threshold in any election. We can understand these voters as those that derive a large satisfaction from the act of voting itself. For the rest of the population, 'the satisfaction from affirming a [referendum] preference' is then the crucial element that determines their participation. As the act of voting itself does not suffice to bring them to the polls, it will depend on the referendum issue whether a voter reaches her idiosyncratic participation threshold. Hence for any given referendum, some of these voters will be more likely than others to participate. Conversely, there will be voters that are not regularly turning out overall, but certain to participate when specific issues are on the ballot. On the aggregate level, the referendum outcome will then be a weighted share of support for the referendum question in the population of regular voters and occasional voters *that participate*.

In this setting, relevance of occasional voter turnout for aggregate outcomes will depend on three factors. These at the same time help to contextualize empirical findings in the literature:

First, the difference in aggregate preferences of regular and *all* occasional voters matters: This quantity is a priori unknown and would be observable when contrasting unequivocal participation with the participation of only regular voters. The literature drawing on compulsory voting as exogenous shock to turnout has made a case in point that the preferences of these two populations differ in many contexts (e.g. Bechtel, Hangartner, and Schmid, 2016)<sup>9</sup>

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<sup>7</sup>I use the terms occasional voters, peripheral voters, and low-propensity voters interchangeably, to denote eligible citizens who are not expected to turn out regularly over many elections but are responsive to mobilizing factors.

<sup>8</sup>See as well Thurner and Franz U. Pappi (1998) for a more detailed overview on the framework.

<sup>9</sup>Similarly, extant observational research from many contexts has argued in this direction (e.g. White and McAllister, 2007), although this is not uncontested (comp. e.g. Lutz and Marsh, 2007).

Second, the share of occasional voters in the population matters. This quantity can be understood as the ‘turnout baseline’, i.e. the average participation rate that can be expected without specific motivational factors. The higher this turnout baseline, the higher the share of regular voters, and the less relevant are both the turnout rate and the preferences of occasional voters. This is one explanation why in some settings turnout fluctuations have made no or low differences (Ferwerda, 2014): A fixed increase in participation will have a lower impact the higher the ‘turnout baseline’.

Third, and which is the central argument for the case at hand, both occasional voter turnout and the average preferences of those occasional voters that participate likely depend on the potential outcomes of the election. This is to say that there will be factors that correlate both with the distribution of the election-specific  $D$ -term and the preference distribution. On the individual level, common factors determine participation likelihood and choice *within the group of occasional voters*. Hence, given low turnout baselines and an intermediate degree of mobilization, the effect of turnout on referendum outcomes depends on the direction of this correlation. There is no a priori reason which directions electoral outcomes should shift to with such tilted occasional voter participation. Even more, when issues on the ballot go beyond the traditional left-right-scale, we would not even expect a tendency of ‘leftist’ policies/parties profiting. For any referendum, what would matter is whether the desire of expressing partisan preferences is higher in the pro- or remain camp.<sup>10</sup>

Overall, electoral outcomes likely become more difficult to predict with low turnout baselines and fluctuation in the participation of occasional voters. Over many elections, results should become more volatile in such settings. The argument therefore resembles the ‘volatility effect’ posited by Hansford and Gomez (2010), which again builds on DeNardo (1980): The argument is that occasional voters hold lower partisan attachments, or even have no stable political attitudes, and hence, if they vote, are more difficult to predict in their choice. In the setting proposed here, however, even if occasional voters had stable attitudes, their different mobilization potential and the correlation of turnout intention and preferences alone suffices to induce volatility.

Empirically, we can expect this variation in turnout to be particularly relevant in high-stake referendums with low baseline turnout.<sup>11</sup> The EU membership referendum in the UK is an important case to test this. The remainder of this chapter will therefore examine whether the increase in turnout affected electoral outcomes and whether heterogeneity in preferences of occasional voters explains heterogeneity in their turnout intention. We would expect this to be the case, if the degree of Leave/Remain support and mobilization correlate. With a high focus of the pre-referendum debate on values and emotions (Inglehart and Norris,

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<sup>10</sup>One hypothesis that could be investigated in future research is whether the reversal of the status quo is potentially more mobilizing among occasional voters. Reverse arguments have been made, however (e.g. Schuck and Vreese, 2009).

<sup>11</sup>While it would be applicable to any binary choice framework (e.g. as well runoffs in presidential elections), low partisan attachments are key in explaining divergence from Tingsten (1937).

2016) and a vote against a (in most pre-election opinion polls depicted) majority position (Feddersen and Sandroni, 2006), this is to be expected.

## 7.3 The Brexit Case

This section briefly outlines the circumstances of the UK EU membership referendum 2016 and the data used for the analysis in this chapter. Following longstanding internal challenges of EU membership both in the broader public and the Conservative Party, in 2013 then Prime Minister David Cameron promised a referendum if he won the next general election. This was held on 23 June 2016, on the question “Should the United Kingdom remain a member of the European Union or leave the European Union?” (Lynch, 2015). Against expectations of both pollsters (which predominantly saw a lead for Remain<sup>12</sup>) and betting markets (which saw about an 85% likelihood for Remain just before election day<sup>13</sup>), 51.89% of voters opted for ‘Leave the EU’, with Leave leading by 1,269,501 votes. High turnout accompanied the referendum: With 33,577,342 total votes 72.21% of registered voters turned out.

Research on Brexit is currently still ongoing. Various explanations for the referendum outcome have been proposed: On the aggregate level, correlations between higher Leave share, lower average education, income and age have been noted (Arnorsson and Zoega, 2016; Manley, Jones, and Johnston, 2017), as well as unemployment (Zhang, 2016) and additionally a strong tradition of manufacturing employment and poorer public service provision (Becker, Fetzer, and Novy, 2017). Geography has been found to matter less, once socio-demographic factors are taken into account; neither a north-south divide, nor geographical patterns of major party support are argued to match the pattern of referendum outcomes (Manley, Jones, and Johnston, 2017).<sup>14</sup> Underlying reasons for these correlations have been argued to lie in the immigration issue (Viskanic, 2017; Goodwin and Milazzo, 2017), but as well austerity measures and rising inequality (Dorling, 2016). On the individual level, Swami, Barron, Weis, and Furnham (2017) show that in a sample of 300 British adults Leave vote intention was predicted by feelings of national attachment and perceptions of threat from (Muslim) immigrants. Clarke, Goodwin, and Whiteley (2017) argue that, additional to age, education and class effects, individual risk assessments, cost-benefit calculations and emotional reactions towards the EU have are predictors of a Leave vote, as well as attitudes towards campaign leaders (Clarke, Goodwin, and Whiteley, 2017). Finally, findings on the role of turnout are mixed: Some find correlations between high turnout and Remain (Manley, Jones, and Johnston, 2017), some with high turnout and Leave (Zhang, 2016). The present analysis contributes to this open question with a causal identification strategy.

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<sup>12</sup>According to data compiled by The Huffington Post, Remain was leading in 73% of its simulations (see <http://elections.huffingtonpost.com/pollster/uk-european-union-referendum>).

<sup>13</sup>According to data compiled by PredictWise (see <http://predictwise.com/politics/uk-politics>).

<sup>14</sup>This is an important finding as unobserved geographical confounders are a potential threat for the weather based identification strategy used in this chapter (as rainfall is necessarily geographically clustered).

## 7.4 Data

To analyze the effects of this turnout increase I gathered data on the level of local authorities for Great Britain. Data on the 2016 referendum outcome in the UK are obtained from the Electoral Commission for 380 local authorities.<sup>15</sup> Following the GSS coding system, this is data on local authority level, i.e. unitary authorities (for England, Scotland and Wales), non-metropolitan districts (two-tier) and metropolitan boroughs (for England). According to the electoral commission, this is the smallest geographic unit for which electoral results are available.<sup>16</sup> I additionally gathered data on 2014 European Parliament Elections turnout and United Kingdom Independence Party (Ukip) vote share as baseline and additional control, which is available for the same unit of analysis.

Data on rainfall on election day is obtained from the UK Met Office<sup>17</sup> and reflects daytime rainfall (900 - 2100 hours) in millimeters for 248 weather stations across the UK on 23 June 2016. Election day rainfall was especially severe in the south and east of the country, where even some polling stations had to be closed.<sup>18</sup> Rainfall mostly affected regions of the country with an average annual rainfall below the country average.<sup>19</sup> Drawing on shapefiles obtained from the UK Data Service,<sup>20</sup> I calculated rainfall per local administration. For this, I averaged the rainfall of weather stations located within a local administration or, if an administration had no weather station located in it, within a 20 km radius of its centroid (see Figure 7.1).

I additionally gathered data on economic and socio-demographic controls<sup>21</sup> to increase the precision of estimates: This is data on 2011 ethnic composition of local authorities and 2011 migration statistics<sup>22</sup> and data on control variables for population size, density, age structure, local economy<sup>23</sup> (all for 2011) and labor market (for 2016)<sup>24</sup> and is obtained from the UK Office of National Statistics. These are the most recent population statistics available (as of July 2016).

One important note concerns election results for Northern Ireland and Gibraltar: Election data for the referendum is available on the level of UK parliamentary constituencies for Northern Ireland (N=18) and for Gibraltar (N=1). But as neither election data for the

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<sup>15</sup>The data is publicly available at <http://www.electoralcommission.org.uk/>.

<sup>16</sup>Personal communication with the Electoral Commission on 6 July 2016.

<sup>17</sup>Data obtained by personal communication with the Weather Desk Team of the Met Office, 6 July 2016.

<sup>18</sup>See <http://www.metoffice.gov.uk/climate/uk/summaries/2016/june>.

<sup>19</sup>Comp. the spatial distribution of annual UK rainfall in the last decade and June 2016 anomaly rainfall at <http://www.metoffice.gov.uk/climate/uk/summaries/anomacts>.

<sup>20</sup>See <https://census.edina.ac.uk/bds.html>.

<sup>21</sup>A control variable for average education levels/share of high (low) educated was not directly available from the ONS; the education variable is partially proxied by share of labor force employed in high (low) skilled labor, and by the unemployment rate.

<sup>22</sup>Available from <http://www.nomisweb.co.uk/census/2011/>.

<sup>23</sup>I.e. the share of employment in high, and respectively low skilled labor.

<sup>24</sup>Available from <http://www.ons.gov.uk/>.

European Parliament election nor data for control variables is available on the same level, both Northern Ireland and Gibraltar were excluded from the analysis.

Summary statistics for all variables used in the analysis are found in the Appendix for this chapter (Appendix Table F.1).

## 7.5 Research Design

Levels of electoral participation and aggregate vote choice are likely correlated (Hansford and Gomez, 2010). Without addressing the endogeneity involved, conclusions on the relationship between levels of turnout and electoral support are potentially biased. A solution is an instrumental variable, proceeding in two steps: First, the instrument has to be related to the participation decision (first-stage relevance), but not to electoral choice (the exclusion restriction) (Angrist and Pischke, 2009). Second, the exogenous variation induced by the instrument is used to assess the endogenous variation between turnout and electoral choice (second stage). This paper draws on variation in rainfall during election day as a suitable instrument for voting costs, depressing turnout but not influencing vote choice. Numerous studies have used rainfall as instrument for electoral participation, and find that rain is predominantly negatively related to turnout (see Arnold and Freier, 2015).

Thus, in the first stage, the change in turnout is instrumented by rainfall  $R$  on election day (in mm), thus

$$\Delta T_i = \alpha_{1i} + \beta_1 R_i + \mathbf{X}'_i \gamma_1 + \epsilon_i. \quad (2)$$

This then leads to a second stage instrumental variable regression of

$$\Delta Y_i = \alpha_{2i} + \beta_2 \hat{\Delta T}_i + \mathbf{X}'_i \gamma_2 + \epsilon_i. \quad (3)$$

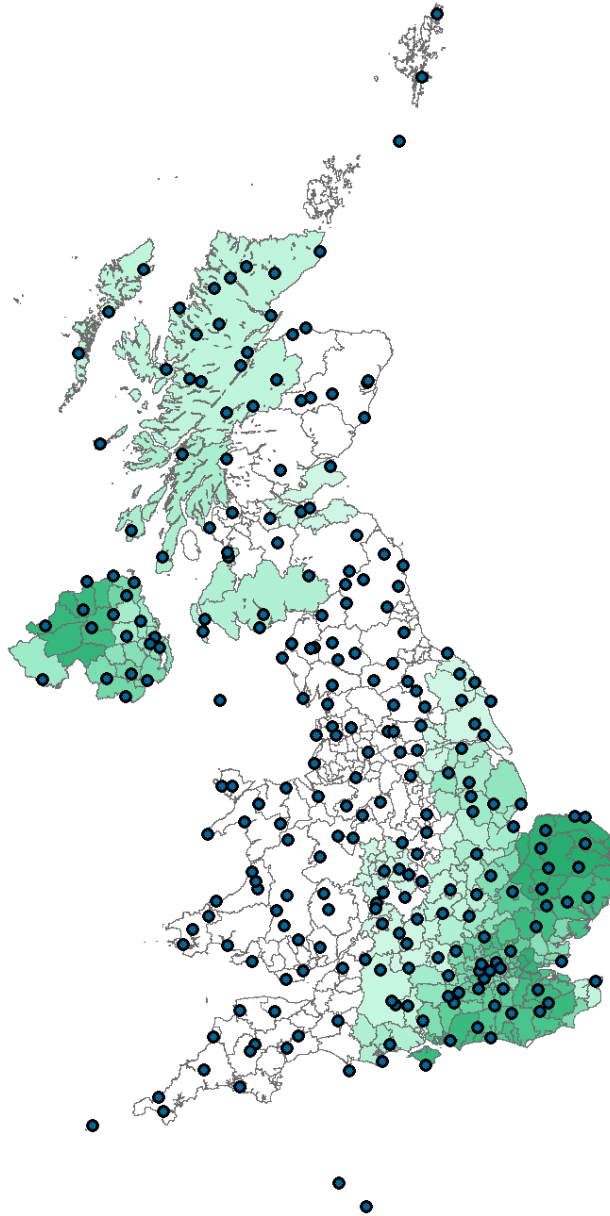
Depending on specification, the analysis additionally controls for a vector  $\mathbf{X}$  of socio-economic and political variables<sup>25</sup> that further increase the precision of estimates and excludes confounders.

For the first stage, this approach is justified: Table 7.1 indicates, as expected, a negative relationship between rainfall and turnout: One millimeter of rainfall on election day is related to a turnout decrease of 0.2 (Model 1, without controls) to 0.1 percentage points (Model 2,

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<sup>25</sup>The analysis uses controls that potentially correlate with aggregate turnout and aggregate Leave support, specifically: the ethnic white population share; the population share being immigrants from outside the UK; population share aged 20-30; population share aged 60+; share of unemployed; share of employed in high skilled labor; share of employed in low skilled labor; change in 2015-2016 unemployment; population density; population size (logged); share of Ukip support in last European Parliament election (for non-delta specification of explanatory variable only); regional dummies for Scotland, Wales, Northern England, Southern England, London.

Figure 7.1: Rainfall in the United Kingdom on election day



The dots depict the 248 weather stations in the United Kingdom and day-time rainfall (900 - 2100 hours) on June 23rd, 2016 (referendum election day). In green, estimated rainfall amounts in local authorities are depicted, from no rain (white) to slight rain (light green (0.2 mm)) and heavy rain (dark green (42.2 mm)).

with controls), with the first difference between referendum turnout and European Parliament turnout as dependent variable.<sup>26</sup> This implies, given average election day rainfall of 4.3 mm, an average effect of rain on turnout of -0.5 (Model 2) to -0.9 (Model 1) percentage points, which is similar in size to what the literature finds (Arnold and Freier, 2015). Instrument relevance is further supported by F-statistics above 10 as recommended as a rule of thumb by Staiger and Stock (1997).<sup>27</sup>

For the second stage, a potential problem for the analysis is the spatial correlation of rainfall. In the case at hand, there is for example no variation of rainfall in Wales, the north of England (North East and North West) and barely variation in Scotland.<sup>28</sup> There is a risk, therefore, that by chance rainfall on election day fell in an area that is politically different even controlling for the variables contained in  $\mathbf{X}$ . I apply three strategies to address this problem:

First of all, the analysis on the one hand explicitly controls for geography.<sup>29</sup> On the other hand, the main specifications of the analysis are restricted to the ‘rainy’ parts of England.<sup>30</sup> As potential unobserved confounders are likely geographically clustered (Johnston and Pattie, 2006, Chap. 7; Johnston, Pattie, and Manley, 2017), the potential for unobserved confounders to bias the estimates is thereby lowered. Binarily distinguishing local authorities with rain and without, reported in Appendix Table F.4, provides support for this strategy: important determinants of Leave such as prior Ukip support and unemployment are both not statistically distinguishable anymore in the sample of ‘rainy’ regions. Still, results for the whole sample are equally reported and substantively similar.

Second, I use a first difference design to control for potential level differences in baseline turnout and Leave share. This allows to relax the assumption of no (geographic) confounding in levels of the dependent variable to no confounding in trends (i.e. the parallel trend assumption (Lechner, 2010; Brüderl and Ludwig, 2015)). Rainfall then is assumed to not

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<sup>26</sup>Results are fully reported in Table F.6. Table F.5 provides evidence that the relevance of rain is assessed similarly for the full sample (Model 1) and with referendum turnout levels as dependent variable (Model 2).

<sup>27</sup>I am not aware of published research that investigates the effect of UK referendum day weather on changes in turnout. Independently, Chris Hanretty in a blogpost (30 June 2016, online at <https://medium.com/@chrishanretty/rain-and-the-referendum-961a03064c4b>) indicates a negative, though insignificant relationship between turnout levels and rainfall once controlling for expected leave share and past EP turnout. His approach differs from mine: He looks at levels not trends (i.e. not explicitly controlling time constant turnout determinants), uses different weather data (pixels on a weather grid), has a different specification of controls, and does not include other socio-demographic controls. These differences in approaches likely explain why Hanretty finds negative, though insignificant effects of rainfall on turnout, while I find a negative, but significant effect of rainfall on the *increase* in turnout. Additionally, there is independent (unpublished) research by Becker, Fetzer, and Novy (2017) who argue that rainfall and train cancellations had a negative effect on turnout in the London area.

<sup>28</sup>Three authorities experience rainfall above 1, none above 2.5 mm.

<sup>29</sup>Control variables include: indicators for Wales, Scotland, North England (regions East Midlands, North East, North West, Yorkshire), South England (regions East, South East, South West, West Midlands), and London.

<sup>30</sup>I.e. to English regions experiencing rain, excluding North East, North West, Scotland and Wales.

Table 7.1: Relevance of instrument

	Rainy regions (England)	
	(1)	(2)
	2016 referendum turnout - 2014 EE turnout	
Average rainfall (mm)	-0.21*** (0.048)	-0.11*** (0.029)
Constant	39.4*** (0.33)	30.0*** (6.26)
Region controls	No	Yes
Socio-economic controls	No	Yes
N	275	275
Adj. R2	0.042	0.79
F-statistic	19.2	43.5

Regression of election-day rainfall (in mm) on election day turnout. Robust standard errors in parentheses.

Control variables included (as indicated) are the ethnic white population share; the population share being immigrants from outside the UK; population share aged 20-30; population share aged 60+; share of unemployed; share of employed in high skilled labor; share of employed in low skilled labor; change in 2015-2016 unemployment; population density; population size (logged); share of Ukip support in last European Parliament election (for non-delta specification of explanatory variable only); regional dummies for Northern England, Southern England, London. Sample is restricted to ‘rainy’ regions in England.

\*\* (\*, \*\*\*) indicates  $p < 0.05$  (0.1, 0.01)

have by chance affected especially those regions that were on different Leave-support trends compared to the rest of the sample. Again, results for level regressions are equally reported and substantively similar. As baseline, I use European Parliament (EP) election turnout and Ukip vote share (indicating Leave support) rather than the last UK General Election. The EP election is chosen as baseline for three reasons: First, like the referendum, the EP election was first and foremost concerned with European Union membership and immigration as a central issue, other than the General Election where the more general economic issue played the most important role for voters (Treib, 2014; Dennison and Goodwin, 2015; Lynch, 2015; Tonge and Geddes, 2015); second, the EP election follows a proportional representation electoral system that gives voters incentives to reveal their true preferences, other than at the General Election where backers of the Leave position from both Conservatives and Labour



have strategic incentives not to support Ukip depending on the competitive environment in their local constituency (Cox, 1997). Both the focus on the EU/immigration issues and the electoral system make it more plausible to assess the degree of anti-EU-support (as well by voters who might support Labour or Conservatives otherwise (Evans and Mellon, 2016)) by measuring Ukip support in the European Parliament 2014 elections. Third, the reporting of EP election results matches referendum counting areas, while General Election results are reported for different electoral districts. Main independent variable used in the analysis is therefore the instrumented first difference in 2016 Referendum turnout ( $T_{i,t}$ ) and European Parliament election turnout ( $T_{i,t-1}$ ). Substantively, this modeling choice additionally puts the focus on the research question what the impact of the differential *increase* in turnout implies for Leave support.

Finally, while it can in principle not be tested whether rainfall is exogenous to turnout in the case at hand, we can find indications for this in placebo tests: As suggested by Angrist and Pischke (2009), I conduct a placebo test on whether referendum day rainfall is related to unaffected outcomes. For this, Appendix Table F.2 reports regressions of rainfall on turnout at the 2014 European Parliament election. Once dummies for region are controlled for, the placebo relationship is close to zero and insignificant (Table F.2, Model 2 and 5), and even more so with additional controls (Model 3 and 6). The placebo-relationship is even weaker if the analysis is restricted to the rainy regions of England (Model 4 of Table F.2), providing additional support to the strategy laid out above. The difference in pre-treatment postal voting applications and ballot box turnout provides for additional placebo tests: While structural shifts in participation patterns should extend to postal voters as well, rainfall could not affect the postal vote participation decision. Appendix Table F.3 reports regressions of rainfall on postal ballot turnout (i.e. envelopes returned over envelopes issued) at the 2016 Brexit referendum. Again, once geography is controlled for, coefficients are substantially close to zero and insignificant. The same null-finding is reached when assessing the correlation of rain with the overall proportion of the Referendum electorate issued with a postal vote. Likewise, the difference in 2016 Referendum and 2014 European Election postal turnout is not influenced by rain. On the other hand, the negative relationship between rainfall and *polling station* turnout is more negative (though not significantly different) compared to overall turnout, as expected.<sup>31</sup>

## 7.6 Results

### 7.6.1 Results on Aggregate Turnout

Table 7.2 reports results of the instrumental variable regression, with the share of Leave (Models 1-3) and the difference in Leave share and 2014 European Election Ukip share (Model

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<sup>31</sup>Findings available upon request.

4-6) as dependent variable.<sup>32</sup> As can be seen from Model 1 to 3, the larger the increase in turnout between European Election and Referendum, the higher the share of Leave. An increase in one percentage point leads to, on average, a 0.72 percentage point increase in the Leave share, insignificantly estimated without control variables (Model 1). The large standard errors in Model 1 are especially a consequence of the fact that prior Ukip support is not controlled for, which is a strong predictor of Leave support. To increase the precision of estimates, controls are included explicitly in Model 2, and implicitly via a differencing approach in Model 4. Effects are estimated similar in size and significant with sociodemographic (Model 2, effect of 0.67, significant on the 5%-level) and additionally geographic control variables (Model 3, effect of 0.61, significant at the 10%-level). Using a difference-in-difference framework (Model 4-6), a one percentage point increase in turnout leads to an increase in the Leave share around 0.78 percentage points (Model 1, without controls, significant at the 5%-level), estimated similarly strong in Model 2 with sociodemographic controls (effect of 0.80, significant at the 5%-level), and slightly less strong and more imprecise in Model 3 ((insignificant) effect of 0.71). The more imprecisely estimated coefficient in Model 3 is a consequence of the inclusion of region controls within a difference-in-difference framework. This is a very conservative approach, partialling out differential turnout *trends* between regions, so the treatment effect is estimated based on within-region variation in rainfall and turnout trend. It is therefore comforting that throughout all models the estimated coefficients are substantively very similar, and for the most part statistically significant at conventional levels.

To further assess the internal validity of these estimates, I calculated effects with a binary instrument ‘heavy rain’ drawing on local authorities with above median rainfall (see Appendix Table F.8). As expected, coefficients are positive in all cases and similar in size for the main specifications (Model 2 and 3).<sup>33</sup> Furthermore, the results are robust to estimation with the full sample of all of Great Britain (see Appendix Table F.9), thus under inclusion of especially Scotland and Wales in the analysis, where election day rainfall was minimal.

Appendix Table F.10 reports results for a standard regression framework, with the same variables as in Table 7.2 but no instrumentation of the turnout increase. In comparison, the IV results are very similar to the coefficient in a model without control variables (0.82, Model 1). However, including controls leads to estimates close to zero (Models 2 and 4). Results even flip signs drawing on the difference in Leave and Ukip share as dependent variable (Model 3). This underscores the importance of an approach that takes the potential endogeneity between turnout and vote shares into account.

The substantive relevance of the estimates can be assessed using the framework of Fowler (2015). Average Leave support is higher where weather was good and more occasional voters turned out. Among the 0.5% of the electorate voting only in good weather, the Fowler-

<sup>32</sup>Full results are reported in Table F.7.

<sup>33</sup>The models with regional dummies are estimated less precisely; with a binary instrument less variation within regions is left in the instrument.

Table 7.2: Instrumental variable regression of difference in EE to Referendum turnout on Leave share/increase in Leave vote

Dep. var:	(1)	(2)	(3)	(4)	(5)	(6)
	Leave share			Leave share - Ukip		
2016 Referendum turnout - 2014 EE turnout	0.72 (0.65)	0.67** (0.27)	0.61* (0.33)	0.78** (0.34)	0.80** (0.38)	0.71 (0.47)
Constant	26.4 (25.1)	26.5** (10.9)	22.4* (13.5)	-7.56 (13.0)	21.4 (13.1)	18.4 (17.9)
Region controls	No	No	Yes	No	No	Yes
Socio-economic controls	No	Yes	Yes	No	Yes	Yes
N	275	275	275	275	275	275

Instrumental variable regression (two-stage least squares) with rainfall (in mm) as instrument for turnout. Robust standard errors in parentheses.

Control variables included (as indicated) are the ethnic white population share; the population share being immigrants from outside the UK; population share aged 20-30; population share aged 60+; share of unemployed; share of employed in high skilled labor; share of employed in low skilled labor; change in 2015-2016 unemployment; population density; population size (logged); share of Ukip support in last European Parliament election (for non-delta specification of explanatory variable only); regional dummies for Northern England, Southern England, London. Sample is restricted to ‘rainy’ regions in England.

\*\* (\*, \*\*\*) indicates  $p < 0.05$  (0.1, 0.01)

approach implies a Leave share of 90.7% [11.8; 173.5]. Thus, this implies a preference gap in Leave support of 36.6 percentage points between regular and these occasional voters. We should not directly interpret this difference due to strong assumptions; nonetheless, taking the results literally gives numeric support that preferences on EU membership differed strongly between occasional and regular voters (comp. Appendix Section F.3 for details).

To summarize, the IV estimates consistently indicate that increases in turnout due to weather conditions are related to strong increases in Leave share/the increase in Leave share over European Election Ukip support. Results imply that voters whose participation decision was influenced by rainfall (and which therefore abstained) were especially likely to support Leave. As it was on average Leave supporters that were deterred from voting due to bad weather, rainfall has played no role for the outcome of the referendum. However, as voters marginal to slight increases in voting costs seemed to be leaning towards Leave, this indicates more generally that the strong overall increase in turnout played a decisive role for the success of the Leave campaign. Survey evidence supports this argument, as the next section argues.

### 7.6.2 Supportive Survey Evidence

Does the observed effect plausibly generalize beyond weather-induced occasional voters to the larger population of occasional voters?

In the Riker-Ordeshook framework (Riker and Ordeshook, 1968), a voter participates in an election if the reward from voting is positive,  $R = PB - C + D > 0$ . Rainfall ( $r$ ) is affecting the voting cost function  $C_i(r)$  of voters, where we can expect  $\partial C_i(r)/\partial r > 0$  (voting costs increase with rain). But which voters abstain? We can expect voter  $i$  to respond to the instrument (and abstain) when the increase in voting costs due to rain is larger than the (expected) utility of voting without rain, thus if  $R_i|r = 0 < \partial C_i(r)/\partial r$ . As the treatment effect is not outcome-neutral, either the left hand side or the right hand side of this equation has to be correlated with referendum preferences:

Concerning the right hand side, the susceptibility of voting costs to rainfall could be higher for occasional voters (Knack, 1994), e.g. due to wealth and transportation costs. But if rainfall induces abstention of low-income voters across the board, the observed effects are only consistent with a population of occasional (low-income) voters that on average lean towards Leave. However, in survey data I find no support for this potential mechanism. For this, I looked at rolling cross-sectional data in the pre-referendum weeks from the British Election Study.<sup>34</sup> Defining occasional voters as those respondents who abstained at least once in the 2005, 2010 or 2015 general election<sup>35</sup>, a Wald test on differences in Leave support shows no significant difference between occasional voters and the rest of the population (see Table 7.3, Column 1).

We thus have to turn to the left hand side, the rewards of voting conditional on no rain. If the population that just participates (as benefits of voting over costs are just on the margin) leans towards Leave on average, their turnout would be affected by rainfall. This would indicate that the rainfall effect is relevant more generally to all increases in voting costs. In Appendix Section F.4 I discuss in detail some evidence consistent with this argument: A summary of this discussion shall suffice here. First, one incentive in participating stems from the D-term and the desire to affirm a referendum preference. This would imply that the emotionalized debate around EU-membership, focusing on economic consequences (as most important aspect highlighted by the Remain campaign) and national sovereignty/immigration (highlighted by the Leave campaign), was more important for (latent) Leave compared to

<sup>34</sup>I used the EU Referendum Daily Campaign Data from the British Election Study Internet Panel, which is an online sample of YouGov panel members. The BES collected daily data on 500-770 respondents from Great Britain 48 days to one day before the referendum. Data is available online at <http://www.britishelectionstudy.com/data-object/wave-8-of-the-2014-2017-british-election-study-internet-panel-daily-file/>.

<sup>35</sup>The abstention variable is self-reported and recalled, and for both reasons likely biased. However, for results to be interpretable, it is only necessary to assume that this bias is affecting Leave-leaning and Remain-leaning voters similarly.

Table 7.3: Characteristics of regular and occasional voters

	(1)	(2)	(3)
	Leave vote	Economic Issue	Sov./Immig. Issue
Leave vote intention among:			
Regular voters	0.507 (0.00470)		
Occasional voters	0.493 (0.00918)		
Likely turnout with issue most important:			
No		0.897 (0.0105)	0.863 (0.0142)
Yes		0.852 (0.0249)	0.926 (0.0121)
N	18750	1373	1373
F-statistic for mean difference	1.775	2.814	11.49
P-value for mean difference	0.183	0.0937	0.000720

Standard errors in parentheses. Column 1 reports group means for vote intention ‘Leave’ drawing on respondents indicating a vote intention. Column 2 and 3 report the share of respondents announcing ‘Very’ or ‘Fairly likely that I turn out’ and holding issue in column heading as (not) ‘most important’ for their Referendum vote intention. Column 2 and 3 draw on occasional voters only. BES survey weights for representative sample of Great Britain are used.

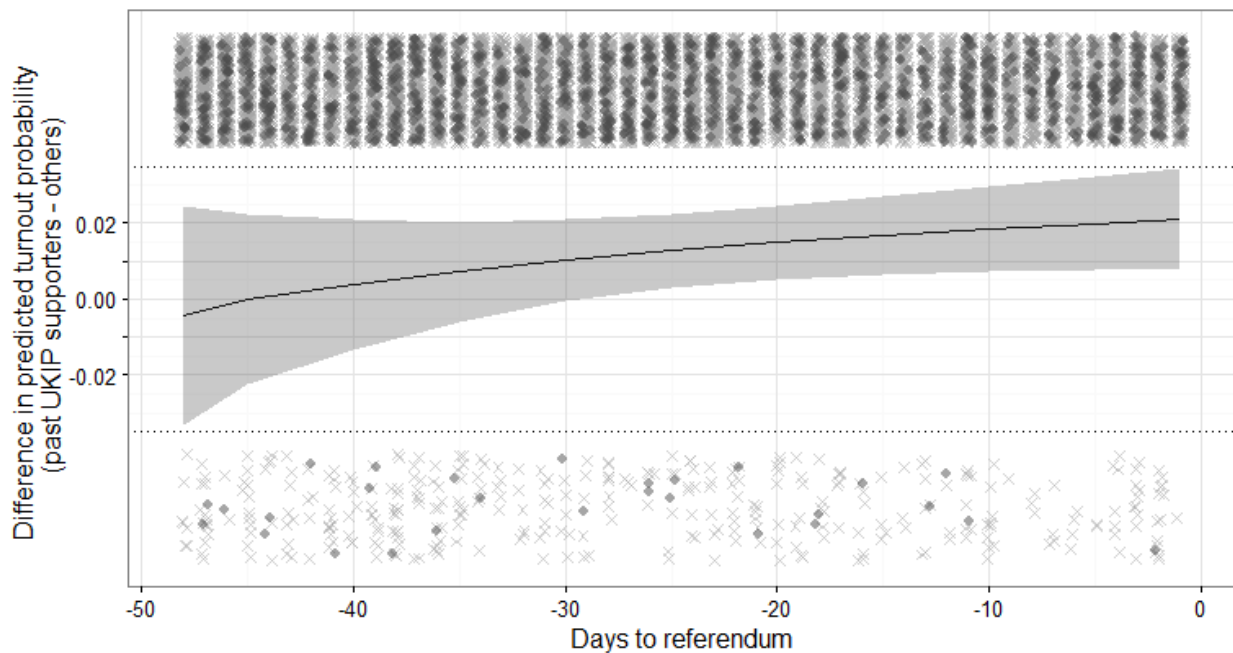
(latent) Remain supporters. Indeed, among occasional voters that see economic consequences as most important, turnout intention is relatively lower. Conversely, a voter that sees sovereignty/immigration as most important issue has an on average higher turnout intention (see column 2 and 3 of Table 7.3). This indicates that occasional voters with latent Leave preferences were more likely to turn out than Remain-leaning occasional voters. Hence, the mobilization of occasional voters was likely lopsided. Second, additional evidence on the perceived closeness of the race indicates that voters that reported their intention to stay home on average favored the Remain side, and at the same time perceived Remain to be winning.

Third, the temporal structure of the survey data allows a final test: The survey questioned part of the survey-population each day, beginning 48 days prior to the referendum. We can thus contrast ‘(very) likely’ turnout intention for Leave/Remain-leaning respondents over time. Figure 7.2 reports a margins plot on whether Ukip supporters in the last general election were more likely to intend to turn out the closer election day came relative to other party supporters. An important feature of this comparison is that confounding factors that influence turnout intention of past-Ukip and other party supporters similarly are implicitly controlled for as long as they influence both groups similarly (e.g. overreporting). Time

seems to play a role for the mobilization of (latent) Leave supporters. 48 days before the referendum, Ukip supporters were equally likely to report an intention to turn out; just before the referendum, however, Ukip supporters are predicted to be two percentage points more likely to turn out.

Overall, the survey analysis supports the IV estimates: Results imply that the referendum motivated (latent) Leave over (latent) Remain supporters and the mobilization of occasional voters due to heterogeneous referendum issue salience played an important role for Brexit.

Figure 7.2: Difference in predicted probability of turnout between past Ukip and ‘other party’ supporters in the 12 pre-referendum weeks



Predictions for average marginal effects from a binary logistic regression regressing self-reported turnout intention (‘fairly’/‘very likely’) on Ukip support at 2015 general election, date of response (1-48 days before referendum) and an interaction term. BES survey weights for representative sample of Great Britain used. 95% confidence intervals are shown. Weighted  $N=12,551$ . Controls for socio-demographics included (comp. Appendix Table F.12, Model 2). Strips indicate distribution of turnout intention variable (upper strip: ones; lower strip: zeros), with crosses representing ‘other party’ supporters ( $N=10,860/N=346$ ) and circles Ukip-supporters ( $N=1,402/N=28$ ).

## 7.7 Conclusion

The present analysis argues that the stark increase in turnout to 72.2% for the United Kingdom European Union membership referendum ('Brexit referendum') was related to an increase in support for the Leave campaign. Given the potentially endogenous relationship between electoral outcomes and electoral participation, I draw on election day rainfall as instrument for turnout. As expected, rainfall is significantly and negatively related to the observed increase in turnout. Subsequently, I show with an instrumental variables analysis that an increase in turnout of one percentage point relative to the European Parliament turnout baseline leads to an increase in the Leave vote of about 0.6-0.7 percentage points.

In interpreting these results, it is important to note that IV regression estimates are internally valid only for the segment of voters that is susceptible to small variations in the costs of voting (due to rainfall). This highlights a methodological perspective where, even if exogenous variation in turnout is used, still only local average treatment effects can be identified, making the comparability of results over time and cases challenging (Angrist and Pischke, 2009; Fowler, 2013). In the case at hand, however, theory and accompanying survey data provide support for the argument that the turnout effect identified is relevant more generally for the observed variation in turnout.

These results might not have been expected given the predominant hypothesis in the literature that turnout benefits left-wing positions and parties. As well in Great Britain, earlier research pointed to an advantage for leftist positions with increasing turnout (McAllister and Mughan, 1986; Fisher, 2007). The political left in Great Britain in tendency supported Remain. As it has been argued that partisan orientations are an important cue for voters in referenda, particularly even in case of diverging partisan orientation and issue-specific attitudes (Selb, Kriesi, Hänggli, and Marr, 2009), this would lead to the expectation that increases in turnout are related to support for Remain as well. However, this is not the case here – highlighting that referenda might follow a distinct logic beyond the traditional relationship of turnout and partisan support. The focus of the Brexit debate on not only the economic impacts of leaving the European Union, but as well immigration and sovereignty might have transcended the traditional left-right-distinction.

The argument has been made that the mobilizational dynamics around Brexit in the long run could revitalize politics in the UK. The argument was aimed at the participation of (Remain leaning) young voters who were mobilized to participate and could participate similarly in future elections (Birch, 2016) – given the results at hand, this is not the full story. Leave-leaning occasional voters have likely been mobilized even more. This is again surprising, as Ukip support (and hence support for Brexit) is at least in parts based on economically marginal and politically disaffected citizens (Ford, Goodwin, and Cutts, 2012), where mobilization would have been expected to be particularly difficult.

In a broader picture the results emphasize a theoretical perspective where fluctuations in turnout with a low baseline likely lead to more volatile outcomes (Hansford and Gomez, 2010). Future research could track more closely two specific mechanisms when mobilization leads to

changes in electoral results. The population of interest would be occasional voters and how heterogeneous their propensity to turn out for a given elections is. In the Brexit example, negative voting (Fiorina and Shepsle, 1989) could be one part of the story, if opposition to the issue at stake (i.e. EU membership) is of higher salience for (latent) Leave supporters compared to the approval of the EU by (latent) Remain supporters. Another perspective is given by the theory of expressive voting (Brennan and Hamlin, 1998; Aldrich and Jenke, 2018): If emotional factors appeal more to (latent) Leave supporters and the issue lends itself more to a logic of expressive voting, this would as well explain heterogeneous Leave-leaning mobilization of occasional voters.

Overall, the results add to a growing recent literature that turnout is critical to understanding electoral outcomes and policy choice in democracies, and even more so in single-issue referendums when partisan attachments are weak. While parts of the literature highlight that increases in turnout lead to policy that is likely in line with median citizen preferences (e.g. Fowler, 2015; Bechtel, Hangartner, and Schmid, 2016), other research implies that average information levels and thus the quality of vote choice might deteriorate with higher turnout of low propensity voters (Schmid, 2015). From this perspective, low turnout constitutes a “blessing in disguise” (Rosema, 2007). The results in this paper indicate that occasional voter turnout was important especially for the Leave vote. This leads to the question whether voting decisions for Leave were as well based on lower average information levels; similarly, it remains an open question whether bias in representation was lowered by the turnout increase and whether preferences of average UK citizens and average UK voters differed.



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# Appendix A

## Appendix for ‘Holding Individual Representatives Accountable’

The appendix for Chapter 2 (Rudolph and Däubler (2016)) is available as supplemental material on the Journal of Politics article site at doi: [10.1086/685378](https://doi.org/10.1086/685378) (as of November 1st, 2017).



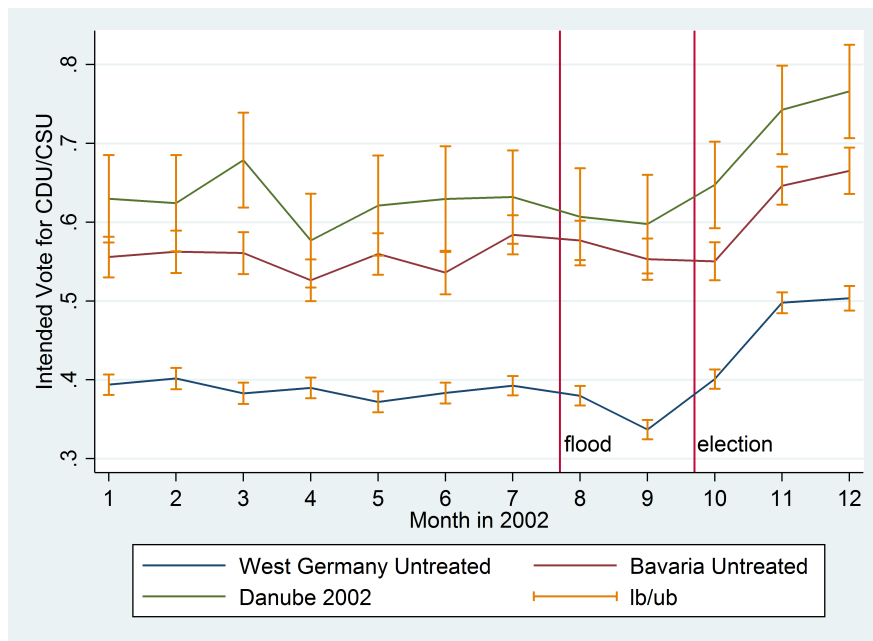


# Appendix B

## Appendix for ‘Selecting Good Types or Holding Incumbents Accountable?’

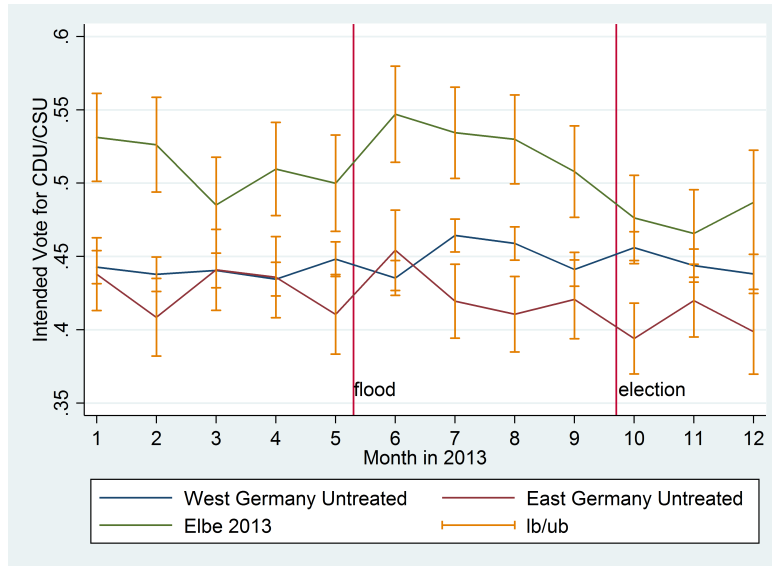
### B.1 Appendix Figures

Figure B.1: Share of voters intending to vote CDU or CSU in 2002.



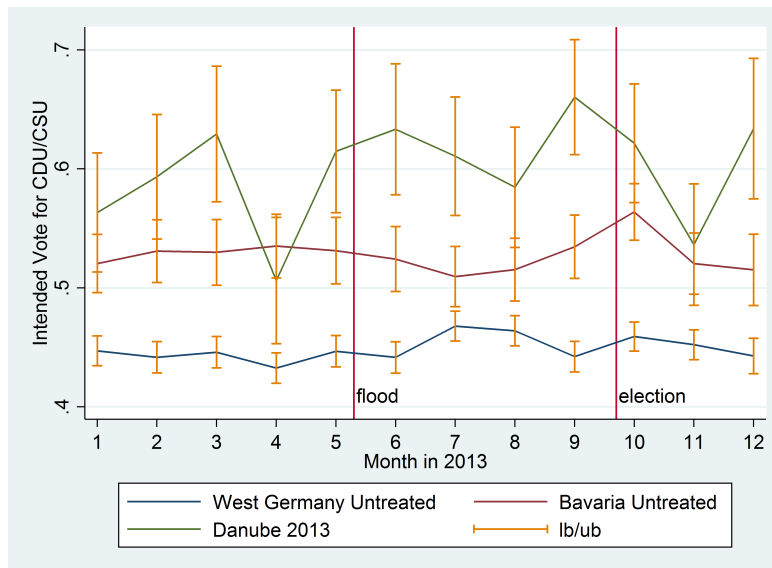
The figure reports average monthly vote intention for the ‘CDU/CSU’ by subgroups. Respondents are a random draw of a Germany wide rolling cross-section (average weekly n=2500, Forsa Study Nr ZA3909). Yellow bars depict 90%-confidence intervals. Subgroups are coded through electoral precincts. Respondents declaring to abstain, giving no answer or being undecided are excluded.

Figure B.2: Share of voters intending to vote CDU in 2013



The figure reports average monthly vote intention for the ‘CDU/CSU’ by subgroups. Respondents are a random draw of a Germany wide rolling cross-section (average weekly n=2500, Forsa Study Nr ZA5927). Yellow bars depict 90%-confidence intervals. Subgroups are coded through electoral precincts. Respondents declaring to abstain, giving no answer or being undecided are excluded.

Figure B.3: Share of voters intending to vote CDU or CSU in 2013.



The figure reports average monthly vote intention for the ‘CDU/CSU’ by subgroups. Respondents are a random draw of a Germany wide rolling cross-section (average weekly n=2500, Forsa Study Nr ZA5927). Yellow bars depict 90%-confidence intervals. Subgroups are coded through electoral precincts. Respondents declaring to abstain, giving no answer or being undecided are excluded.

## B.2 Appendix Tables

Table B.1: Placebo analysis for 1994-1998 vote shares on district level

Dep. var.: PR vote share	Full Germany		Bavaria		East	
	(1) SPD	(2) CDU-CSU	(3) SPD	(4) CSU	(5) SPD	(6) CDU
Elbe 2013	-0.050 (0.50)	-6.24*** (0.71)			0.58 (0.61)	-2.67*** (0.78)
Danube 2013	0.60** (0.25)	2.01*** (0.34)	0.27 (0.25)	-0.14 (0.34)		
Year 1998	4.53*** (0.13)	-5.87*** (0.15)	4.87*** (0.13)	-3.72*** (0.15)	3.14*** (0.45)	-10.6*** (0.45)
Constant	35.7*** (0.060)	43.4*** (0.072)	29.5*** (0.056)	52.4*** (0.066)	33.4*** (0.17)	38.5*** (0.18)
N	804	804	192	192	152	152

District level data for Germany (total yearly n=402). Fixed effects estimation with district and election year fixed effects drawing on the 1994 and 1998 elections. Standard errors clustered by district reported in parentheses. The placebo treatment groups consist of districts flooded at respective river in 2013.

Table B.2: Challenger results for 2002 Elbe and Danube

	Unaffected and Elbe (1) CDU	Unaffected and Bavaria (2) SPD	Unaffected and Elbe (3) SPD	Unaffected and Bavaria (4) SPD
Dep. var.: PR vote share				
Elbe Flood 2002	-0.97*** (0.31)			
Danube Flood 2002		-1.91*** (0.37)		
Elbe Flood 2013			-1.35*** (0.36)	
Danube Flood 2013				0.019 (0.24)
Year 2002	1.25*** (0.18)	-2.80*** (0.14)		
Year 2005	-2.07*** (0.27)	-6.56*** (0.30)	-5.25*** (0.30)	-6.91*** (0.28)
Year 2009	-2.31*** (0.35)	-16.1*** (0.31)	-16.9*** (0.34)	-16.8*** (0.30)
Year 2013			-14.8*** (0.33)	-13.4*** (0.32)
Economic and political controls	Yes	Yes	Yes	Yes
N	1224	1060	1224	1060
Clusters	306	265	306	265

Estimation as in Models 1 and 3 of Tables 3.1 and 3.2 with challenger party vote share as dependent variable.

Table B.3: Two-period estimation for 2002 Elbe and Danube

	Unaffected and Elbe (1) SPD	Unaffected and Bavaria (2) CDU-CSU	Unaffected and Elbe (3) CDU	Unaffected and Bavaria (4) CDU-CSU
Dep. var.: PR vote share				
Elbe Flood 2002	0.95* (0.56)			
Danube Flood 2002		3.59*** (0.71)		
Elbe Flood 2013			0.90** (0.40)	
Danube Flood 2013				0.059 (0.42)
Year 2002	-3.16*** (0.24)	1.58*** (0.30)		
Year 2013			7.81*** (0.22)	7.15*** (0.17)
Economic and political controls	Yes	Yes	Yes	Yes
N	612	530	612	530
Clusters	306	265	306	265

Estimation as in Models 1 and 3 of Tables 3.1 and 3.2 drawing on time periods 1998/2002 and respectively 2009/2013 only.

Table B.4: SPD and CDU-CSU vote shares for the 1998-2013 period with individual slopes

	(1)	(2)	(3)	(4)
Dep. var.: PR vote share	SPD	CDU-CSU	SPD	CDU-CSU
Elbe Flood 2002	4.89*** (0.56)	-0.60** (0.26)	4.03*** (0.54)	0.092 (0.34)
Elbe Flood 2013	-3.25*** (0.37)	2.37*** (0.37)	-0.87 (0.53)	0.040 (0.43)
Danube Flood 2002	-1.86*** (0.54)	2.98*** (0.79)	-1.77** (0.72)	2.93*** (0.91)
Danube Flood 2013	0.28 (0.27)	-2.11*** (0.28)	-0.76* (0.40)	-1.42*** (0.42)
socioeconomic controls	No	No	Yes	Yes
time fixed effects and individual slopes	Yes	Yes	Yes	Yes
N	2010	2010	2010	2010

Treatment effects using individual slopes. Standard errors clustered at district level. Socioeconomic controls on population density, share of employed on district population, share of employed in agriculture, in production, in manufacturing, in construction, in trade, in financial services, in other services and state incumbent being an SPD incumbent included as indicated. Controls on SPD or CSU governor running for chancellor included. The treatment group consists of districts flooded at respective river in the respective year.

Table B.5: Matching effects using close-by municipalities

	Federal Election				State Election
	Elbe 2002 (1)	Elbe 2013 (2)	Danube 2002 (3)	Danube 2013 (4)	Danube 2013 (5)
Dep. var.: diff. in PR vote share	SPD diff.	CDU diff.	CSU diff.	CDU. diff.	CSU diff.
ATE					
Flooded	0.19 (0.22)	0.094 (0.17)	1.48*** (0.22)	-0.86*** (0.25)	3.24*** (0.40)
N	648	648	2056	2056	2056

Average treatment effect using nearest neighbor matching with robust standard errors with 8 neighbors [8=average shared boundaries of a municipality] on latitude and longitude of municipality centroids and population density as well as log population (with bias adjustment for the latter two variables) with difference in treatment period and pre-treatment period vote shares as dependent variable.

### B.3 District Level Survey Data for 2002 and 2013

One important question is the extent, to which the (positive) evaluations of flood management spilled over to non-affected areas. The following analysis of survey data assumes that these spill-overs are geographically concentrated.

Figure 3.2 displays rolling cross-sectional survey data, aggregated to monthly evaluations of the SPD for untreated states, flooded districts along the Elbe and unflooded districts in East Germany. On the one hand side, this assesses whether the positive effects reported above can be found in survey data as well, and additionally whether the behavior of voters in adjacent areas follows similar temporal patterns.

Along the Danube (see Figure B.1), the share of surveyed voters opting for the CDU/CSU increases only marginally in affected states, while it stays constant or is slightly decreasing in non-affected districts. This would not point to strong spill-over effects in Bavaria. The evaluations of Chancellor Schroeder and challenger Stoiber follow similar patterns.

Figure B.2 shows the share of voters opting for CDU in Elbe districts and control regions in 2013. Flood onset in June 2013 is followed by a rise in CDU evaluations along Elbe and in the rest of East Germany – although this increase decays until election day in September. Strong spill-over effects beyond affected districts are unlikely based on these patterns. Similarly, Figure B.3 shows that the intended vote for the CDU/CSU rises, though not significantly, immediately after the Danube flood onset in June 2013 and especially in directly affected districts; the pattern until the election is, although overall increasing, not a clear-cut rise. Again, there is no strong evidence for spill-overs beyond affected districts.

### B.4 Anecdotal Evidence of Differing Political Attention of Actors Schröder/SPD and Stoiber/CDU-CSU to Elbe and Danube Affected Districts

Objectively, federal and federal state level agencies jointly financed the flood compensation programs in 2002 and 2013 and steered the employment of federal forces all over Germany. The decision to declare disaster and to invite external emergency forces into these regions is taken independently at district level. The literature is inconclusive on whether voters in Germany attribute correctly in a setting of shared responsibilities and multiple levels of electoral accountability (comp. e.g. Florack and Hoffmann, 2006; Ade and Freier, 2013). For 2002, anecdotal evidence shows for example a bias in political attention of the actors Stoiber and Schröder to flood regions, which could be directly related to a differing perceived responsibility for disaster management along Elbe and Danube by the electorate: The Schröder government had announced millions of federal Euros for victims in affected regions not even a week into the event, while the CDU/CSU needed three days to position themselves concerning the federal flood compensation plans. But once established, flood compensation was



not only distributed in districts affected by the August 2002 floods, but as well covered those affected in minor floods in June 2013 in south-western Bavaria (BMVg, 2002), presumably because of efforts by the Bavarian government. At the same time, the CSU federal state government under Stoiber in Bavaria adopted a flood compensation scheme for Bavarian flood victims as quickly as Schröder announced the federal plan. Disaster relief communication and the attention of the candidates to the floods followed this pattern. Incumbent Schröder was highly present in the flooded regions in eastern Germany, whilst the challenger Stoiber visited the Bavarian disaster regions first (Bytzek, 2008). The media attention along the Elbe quickly focused on chancellor Schröder. Many claimed that the appraisal of his leadership skills in the electorate profited from his handling of the disaster and the visibility of the federal incumbent government due to symbolic crisis management activities (Pappi, Shikano, and Bytzek, 2004; Bytzek, 2008). Newspaper headlines prominently featured photos of him wearing waterproof boots along the Elbe. At the same time, the challenger Stoiber was criticized for being on a vacation, interrupted only for a brief visit to Passau (along the Danube). A visit to the Elbe followed only several days into the catastrophe. From this perspective of perceived responsibility, it is plausible that Schröder succeeded in depicting his management skills along the Elbe - while Stoiber was seen along the Danube as the relevant actor. Note that these events are compatible with another story: Schröder visited the most severely affected regions (which happened to be on the Elbe); while Stoiber focused on his responsibility as governor only (which happened to be along the Danube).

## B.5 Alternative Explanations for Differential Flood Response Between 2002 and 2013

An additional aspect to consider is the level of pre-election support incumbents have (comp. Figures B.1-B.3): For example, along the Elbe 2002 incumbent Schroeder was trailing in the polls; in surveys, his pre-disaster support along the Elbe was at around 35 to 40 percent; on the other hand, in 2013, Merkel had already reached support levels above 50% in flood-affected districts. Similarly, CSU support along the Danube was around 60% in 2002 and around 45% in 2013, values all well above the national average incumbent rating. Such baseline effects have so far not been systematically analyzed in the empirical disaster literature in particular and the accountability literature in general. With the district level share of supporters  $u$  for the incumbent, the disaster effect  $d$  is naturally bounded by  $0 \leq d \leq 1 - u$ . The higher  $u$ , the lower the share of individuals that can change their voting decision if they receive a positive signal on incumbent quality. In line with this argument, Bechtel and Hainmueller (2011) e.g. argue that persuasion, not mobilization drives the 2002 Elbe effect. In 2013, with higher pre-flood incumbent support, the persuasion effect is therefore likely smaller. For future research, this implies that positive demonstration effects should be relevant especially for low-ranked incumbents, while negative demonstration effects should be relevant

especially for high-ranked incumbents. This makes intuitive sense, as the information gained with observing government performance is ex-ante likely unexpected by a larger share of the electorate in both cases. More generally, it is related to the argument that disaster effects will depend on prior beliefs on incumbents (Ashworth, Bueno De Mesquita, and Friedenber, 2017).

Another aspect, the repetition of disaster exposure might have played a role. For directly affected households, beneficial policy should imply at least short term electoral effects independent of past exposure (comp. Zucco, 2013). But with respect to the indirectly affected population, a repeated exposure can be expected to show diminishing returns – incumbents in 2002 and 2013 responded with a nearly identical policy: Large scale, effective disaster management with federal and state forces and the quick promise of financial reimbursement and reconstruction aid. The 2002 relief scheme thus set an example of a dominant government response strategy as a combination of robust disaster relief and redistribution to affected households that since has been applied as well to other, geographically more limited disasters.<sup>1</sup> However, if the dominant political strategy is a certain level of disaster relief, a voter interested in selecting high quality politicians learns less on incumbent quality in a subsequent disaster: He can expect both incumbent and counterfactual challenger to have invested in similar relief effort. This could explain why we observe lower electoral returns especially along the Elbe comparing the 2002 and 2013 response – governments under both CDU and SPD leadership implemented very similar disaster response programs in the very same area. The results observed are consistent with a theoretical model, where exposure first increases government support through an informational channel (voters (positively) update their view on the quality of the incumbent), while the information gained with subsequent disaster relief is decreasing. Additional micro-level research would need to track this mechanism in future studies. Empirically, private donation data gives an indication of this pattern as well: While the 2002 floods have been marked down as the disaster with the highest aggregate private donation levels in post-unification Germany (with e.g. 300 million Euro for the state of Saxonia alone (WWF 2007)), estimations for 2013 amount to a total of only 108 million Euro (BMI 2013). This general decline in donations would not be affected by the electoral cycle, but it is consistent with a perspective in which voters are updating information with repeated exposure.

Last but not least, lacking disaster preparedness might have been highlighted through repeated exposure in 2013. Governments could have been expected to prepared better for a flood event in the case of the 2013 flood. Thus, if the observed aggregate electoral response constitutes a net effect of punishment for occurrence and rewards for good management and relief (Heersink, Peterson, and Jenkins, 2017), observed effects are consistent with the argument that occurrence (or lacking preparedness) was punished more in 2013 with the second

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<sup>1</sup>E.g. as the governor of Bavaria put it, following what the media termed a ‘thousand year flood’ in Bavaria in 2016: ‘Affected households can count on financial help by the state, in levels oriented at the relief distributed after the 2013 flood’ (author’s translation), referring to the instant 1500 Euro per household direct relief transfer of 2013 (Mitterer, 2016).

exposure. However, the literature so far has argued that voters do not take disaster relief into consideration to a large extent, either because they are myopic(Healy and Malhotra, 2009) or because effort in disaster preparedness is difficult to observe(Ashworth and Bueno De Mesquita, 2012). Additionally, it has been argued that the 2013 disaster preparedness levels along the Elbe were actually much better compared to 2002 and that disaster damage with 2002 preparedness levels would have been much larger (Merz, Elmer, Kunz, Mühr, Schröter, and Uhlemann-Elmer, 2014).

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# Appendix C

## Appendix for ‘Die Münchner Ergebnisse im Bundes- und Landesvergleich’

The appendix for Chapter 4 (Rudolph (2017a)) is available as OnlinePlus on the Springer book pages at <http://www.springer.com/de/book/9783658156732> (as of November 1st, 2017).





# Appendix D

## Appendix for ‘How to Increase Turnout in Low Salience Elections’

The appendix for Chapter 5 (Leininger, Rudolph and Zittlau (2018)) is available as supplementary material on the PSRM article site at [doi:10.1017/psrm.2016.38](https://doi.org/10.1017/psrm.2016.38) (as of November 1st, 2017).



# Appendix E

## Appendix for ‘Natural Disasters and Political Participation’

The appendix for Chapter 6 (Rudolph and Kuhn (2017)) is available as supplemental material on the German Politics article site at doi: [10.1080/09644008.2017.1287900](https://doi.org/10.1080/09644008.2017.1287900) (as of November 1st, 2017).



# Appendix F

## Appendix for ‘Turning Out to Turn Down the EU’

### F.1 Appendix Tables

Table F.1: Summary statistics for variables used in the analysis

	Great Britain (1)			Rainy regions (England) (2)		
	mean	min	max	mean	min	max
Turnout in %	73.76	56.25	83.57	75.13	59.25	83.57
Turnout for 2014 EE	35.81	23.74	51.77	36.61	23.74	51.77
2016 Referendum TO - 2014 EE TO	37.94	12.83	49.24	38.52	12.83	49.24
Average rainfall (mm)	3.12	0.00	42.20	4.29	0.00	42.20
Share of population aged 20-30	12.54	7.62	27.93	12.58	7.62	27.93
Share of population aged 60+	23.97	8.44	38.05	23.78	8.44	38.05
Share of unemployed	1.57	0.30	4.70	1.31	0.30	4.20
Change in 2016 unemployment share	-0.07	-0.70	0.80	-0.09	-0.70	0.30
Population density	14.80	0.10	138.70	17.16	0.30	138.70
Population size (log)	11.81	7.70	13.89	11.79	7.70	13.89
Share of employed in high skilled labor	10.90	6.10	23.30	11.55	6.60	23.30
Share of employed in low skilled labor	11.08	3.60	21.10	10.82	3.60	21.10
Share of migrants from outside UK	0.97	0.18	6.45	1.08	0.22	6.45
Share of ethnic group	90.31	28.82	99.26	88.57	28.82	98.82
South England	0.48	0.00	1.00	0.66	0.00	1.00
North England	0.29	0.00	1.00	0.22	0.00	1.00
London	0.09	0.00	1.00	0.12	0.00	1.00
Wales	0.06	0.00	1.00	0.00	0.00	0.00
Scotland	0.08	0.00	1.00	0.00	0.00	0.00
Northern Ireland	0.00	0.00	0.00	0.00	0.00	0.00
UKIP share in EE	29.17	4.90	51.58	31.43	4.90	51.58
Observations	380			275		

Table F.2: Placebo regression on 2014 European Election turnout

	Great Britain			Rainy regions (England)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Turnout for 2014 EE					
Average rainfall (mm)	0.23*** (0.049)	0.037 (0.030)	0.031 (0.028)	0.16*** (0.043)	0.037 (0.030)	0.027 (0.028)
North England		-2.61*** (0.44)	-1.73*** (0.43)		-2.57*** (0.47)	-1.73*** (0.49)
London		4.31*** (0.66)	3.54*** (1.10)		4.31*** (0.66)	4.27*** (1.11)
Wales		-3.87*** (0.75)	-3.22*** (0.71)			
Scotland		-2.16*** (0.76)	-1.47 (0.90)			
Share of population aged 20-30			0.12 (0.12)			0.10 (0.14)
Share of population aged 60+			0.22*** (0.061)			0.16** (0.064)
Population density			-0.030 (0.019)			-0.030 (0.020)
Population size (log)			-0.28 (0.38)			-0.30 (0.45)
Share of employed in high skilled labor			-0.10 (0.15)			-0.21 (0.15)
Share of employed in low skilled labor			-0.56*** (0.13)			-0.57*** (0.14)
Share of ethnic group			-0.11*** (0.027)			-0.073** (0.028)
Constant	35.1*** (0.24)	36.5*** (0.30)	50.6*** (6.49)	35.9*** (0.28)	36.5*** (0.30)	50.3*** (7.37)
N	380	380	380	275	275	275
Adj. R2	0.083	0.29	0.39	0.050	0.29	0.38

Table F.3: Placebo regression on 2016 postal ballot turnout

	Great Britain			Rainy regions (England)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Postal voting turnout					
Average rainfall (mm)	0.020 (0.027)	-0.0015 (0.024)	-0.013 (0.020)	-0.070** (0.027)	-0.0084 (0.024)	-0.017 (0.021)
North England		-1.01*** (0.33)	-0.26 (0.29)		0.098 (0.31)	0.42 (0.32)
London		-5.28*** (0.95)	0.29 (1.05)		-5.27*** (0.95)	0.15 (1.06)
Wales		-2.23*** (0.49)	-1.79*** (0.55)			
Scotland		-4.47*** (0.56)	-4.16*** (0.64)			
Share of population aged 20-30			-0.030 (0.10)			-0.032 (0.11)
Share of population aged 60+			-0.0034 (0.051)			-0.019 (0.058)
Share of unemployed			-0.95*** (0.29)			-0.69* (0.41)
Change in 2016 unemployment share			-0.44 (0.76)			-0.059 (1.03)
Population density			-0.029 (0.017)			-0.030 (0.018)
Population size (log)			0.27 (0.35)			0.24 (0.45)
Share of employed in high skilled labor			0.067 (0.091)			-0.075 (0.097)
Share of employed in low skilled labor			-0.075 (0.11)			-0.22* (0.13)
Share of migrants from outside UK			-0.17 (0.34)			-0.31 (0.34)
Share of ethnic group			0.11*** (0.029)			0.10*** (0.031)
Constant	88.1*** (0.18)	89.4*** (0.21)	78.6*** (5.19)	89.1*** (0.19)	89.5*** (0.21)	82.8*** (6.36)
N	380	380	380	275	275	275
Adj. R2	-0.0017	0.27	0.55	0.010	0.28	0.58

Table F.4: Balance test on covariates by rain/no rain in area

	Great Britain									
	(1)				(2)					
	No rain/ mean	Positive rain/ mean	Diff-In-Means/ se	N Controls	N Treated	No rain/ mean	Positive rain/ mean	Diff-In-Means/ se	N Controls	N Treated
Share of population aged 20-30	12.32	12.71	-0.39 (0.37)	172	208	12.08	12.78	-0.70 (0.51)	78	197
Share of population aged 60+	24.92	23.19	1.74*** (0.51)	172	208	25.56	23.07	2.49*** (0.72)	78	197
Share of unemployed	1.86	1.34	0.53*** (0.09)	172	208	1.30	1.31	-0.01 (0.09)	78	197
Change in 2016 unemployment share	-0.04	-0.09	0.05** (0.02)	172	208	-0.10	-0.08	-0.02 (0.02)	78	197
Population density	9.52	19.18	-9.66*** (2.26)	172	208	9.82	20.07	-10.24*** (3.33)	78	197
Population size (log)	11.82	11.81	0.01 (0.06)	172	208	11.76	11.81	-0.05 (0.08)	78	197
Share of employed in high skilled labor	9.96	11.69	-1.73*** (0.24)	172	208	10.81	11.85	-1.04*** (0.32)	78	197
Share of employed in low skilled labor	11.81	10.47	1.35*** (0.24)	172	208	11.87	10.41	1.46*** (0.35)	78	197
Share of migrants from outside UK	0.71	1.18	-0.48*** (0.09)	172	208	0.77	1.20	-0.44*** (0.12)	78	197
Share of ethnic group 'white'	93.93	87.31	6.62*** (1.22)	172	208	93.23	86.73	6.50*** (1.77)	78	197
region_dummies==South England	0.26	0.66	-0.40*** (0.05)	172	208	0.56	0.70	-0.13** (0.06)	78	197
region_dummies==North England	0.49	0.13	0.36*** (0.04)	172	208	0.44	0.14	0.30*** (0.05)	78	197
region_dummies==London	0.00	0.16	-0.16*** (0.03)	172	208	0.00	0.17	-0.17*** (0.04)	78	197
region_dummies==Wales	0.13	0.00	0.13*** (0.02)	172	208	0.00	0.00	0.00 (0.00)	78	197
region_dummies==Scotland	0.12	0.05	0.07** (0.03)	172	208	0.00	0.00	0.00 (0.00)	78	197
region_dummies==Northern Ireland	0.00	0.00	0.00 (0.00)	172	208	0.00	0.00	0.00 (0.00)	78	197
UKIP share in EE	28.07	30.08	-2.00** (0.96)	172	208	32.09	31.16	0.92 (1.14)	78	197
Observations	380				275					



Table F.5: Additional tests on instrument relevance

	Great Britain	
	(1) 2016 Referendum TO - 2014 EE TO	(2) Turnout in %
Average rainfall (mm)	-0.12*** (0.030)	-0.038** (0.017)
North England	0.55 (0.42)	-1.27*** (0.30)
London	-1.92** (0.95)	0.84 (0.75)
Wales	2.68*** (0.68)	-1.56*** (0.55)
Scotland	1.05 (1.49)	-5.55*** (0.97)
Share of population aged 20-30	-0.15 (0.14)	-0.22* (0.12)
Share of population aged 60+	-0.24*** (0.053)	-0.012 (0.039)
Share of unemployed	-1.64*** (0.31)	-2.51*** (0.22)
Change in 2016 unemployment share	0.78 (1.01)	0.60 (0.59)
Population density	-0.014 (0.016)	-0.033** (0.016)
Population size (log)	-0.022 (0.34)	-0.11 (0.23)
Share of employed in high skilled labor	0.65*** (0.13)	0.32*** (0.10)
Share of employed in low skilled labor	0.17 (0.12)	-0.20** (0.089)
Share of migrants from outside UK	-0.20 (0.43)	-0.31 (0.43)
Share of ethnic group	0.14*** (0.032)	0.088*** (0.023)
Ukip share in EE	0.20*** (0.035)	-0.018 (0.026)
Constant	21.1*** (7.08)	75.2*** (4.90)
N	380	380
Adj. R2	0.69	0.85

Table F.6: Relevance of instrument – full results

	Rainy regions (England)	
	(1)	(2)
	2016 Referendum	TO - 2014 EE TO
Average rainfall (mm)	-0.21*** (0.048)	-0.11*** (0.029)
North England		0.96** (0.44)
London		-2.70*** (0.95)
Share of population aged 20-30		-0.15 (0.16)
Share of population aged 60+		-0.24*** (0.052)
Share of unemployed		-1.47*** (0.37)
Change in 2016 unemployment share		0.88 (1.22)
Population density		-0.012 (0.015)
Population size (log)		-0.35 (0.29)
Share of employed in high skilled labor		0.58*** (0.13)
Share of employed in low skilled labor		0.092 (0.11)
Share of migrants from outside UK		-0.58 (0.38)
Share of ethnic group		0.13*** (0.033)
Ukip share in EE		0.16*** (0.037)
Constant	39.4*** (0.33)	30.0*** (6.26)
N	275	275
Adj. R2	0.042	0.79
F-Statistic	19.2	43.5

Table F.7: Instrumental variable regression of difference in EE to referendum turnout on leave share/increase in leave vote – full results

	Dep. var: Leave share			Dep. var: Leave share - Ukip		
	(1)	(2)	(3)	(4)	(5)	(6)
2016 Referendum TO - 2014 EE TO	0.72 (0.65)	0.67** (0.27)	0.61* (0.33)	0.78** (0.34)	0.80** (0.38)	0.71 (0.47)
Share of population aged 20-30		-0.48** (0.23)	-0.36 (0.24)		-0.40* (0.24)	-0.30 (0.27)
Share of population aged 60+		0.030 (0.10)	0.014 (0.11)		0.061 (0.12)	0.035 (0.13)
Share of unemployed		1.21** (0.58)	1.03 (0.68)		1.22** (0.61)	1.06 (0.74)
Change in 2016 unemployment share		-4.67*** (1.53)	-3.67** (1.59)		-4.96*** (1.64)	-3.86** (1.77)
Population density		0.024 (0.025)	0.00061 (0.025)		0.031 (0.029)	0.0031 (0.026)
Population size (log)		-0.18 (0.45)	-0.15 (0.47)		-0.092 (0.47)	-0.095 (0.49)
Share of employed in high skilled labor		-0.81*** (0.27)	-0.75*** (0.27)		-0.84*** (0.30)	-0.77** (0.30)
Share of employed in low skilled labor		0.79*** (0.18)	0.84*** (0.18)		0.74*** (0.21)	0.81*** (0.19)
Share of migrants from outside UK		-0.038 (0.69)	-0.36 (0.69)		0.066 (0.73)	-0.31 (0.71)
Share of ethnic group 'white'		-0.25*** (0.065)	-0.21*** (0.062)		-0.29*** (0.086)	-0.24*** (0.086)
UKIP share in EE		0.94*** (0.056)	0.97*** (0.060)			
North England			0.78 (0.70)			0.73 (0.78)
London			3.57** (1.50)			3.90** (1.92)
Constant	26.4 (25.1)	26.5** (10.9)	22.4* (13.5)	-7.56 (13.0)	21.4 (13.1)	18.4 (17.9)
N	275	275	275	275	275	275

Table F.8: Instrumental variable regression of difference in EE to referendum turnout on Leave share/increase in Leave vote with binary instrument (above median rainfall)

	Dep. var: Leave share			Dep. var: Leave share - Ukip		
	(1)	(2)	(3)	(4)	(5)	(6)
2016 Referendum TO - 2014 EE TO	1.65*** (0.41)	0.70** (0.32)	0.56 (0.37)	0.64** (0.26)	0.81** (0.40)	0.62 (0.47)
Share of population aged 20-30		-0.48** (0.24)	-0.37 (0.24)		-0.40* (0.24)	-0.33 (0.27)
Share of population aged 60+		0.037 (0.12)	0.0026 (0.12)		0.062 (0.13)	0.014 (0.14)
Share of unemployed		1.25** (0.62)	0.96 (0.75)		1.22* (0.64)	0.95 (0.75)
Change in 2016 unemployment share		-4.69*** (1.57)	-3.60** (1.64)		-4.96*** (1.68)	-3.71** (1.77)
Population density		0.025 (0.027)	0.000058 (0.025)		0.031 (0.031)	0.0017 (0.026)
Population size (log)		-0.17 (0.46)	-0.16 (0.46)		-0.091 (0.47)	-0.12 (0.48)
Share of employed in high skilled labor		-0.83*** (0.29)	-0.72*** (0.28)		-0.84*** (0.30)	-0.72** (0.29)
Share of employed in low skilled labor		0.79*** (0.19)	0.85*** (0.18)		0.74*** (0.21)	0.83*** (0.19)
Share of migrants from outside UK		-0.011 (0.68)	-0.40 (0.67)		0.070 (0.71)	-0.37 (0.68)
Share of ethnic group ‘white’		-0.25*** (0.070)	-0.20*** (0.063)		-0.29*** (0.091)	-0.22*** (0.084)
UKIP share in EE		0.94*** (0.060)	0.97*** (0.065)			
North England			0.85 (0.76)			0.84 (0.78)
London			3.43** (1.59)			3.61* (1.90)
Constant	-9.55 (16.0)	25.9** (11.9)	23.8 (14.9)	-1.92 (10.2)	21.3 (14.0)	21.3 (18.5)
N	275	275	275	275	275	275

Table F.9: Instrumental variable regression of difference in EE to referendum turnout on Leave share/increase in Leave vote with full Great Britain sample

	Dep. var: Leave share			Dep. var: Leave share - UKIP		
	(1)	(2)	(3)	(4)	(5)	(6)
2016 Referendum TO - 2014 EE TO	0.18 (1.14)	0.51** (0.25)	0.66** (0.29)	2.78* (1.49)	2.51 (2.06)	1.63* (0.85)
Share of population aged 20-30		-0.74*** (0.20)	-0.55** (0.26)		-1.03** (0.45)	-0.11 (0.40)
Share of population aged 60+		-0.047 (0.089)	0.011 (0.11)		0.22 (0.38)	0.23 (0.23)
Share of unemployed		1.72*** (0.51)	2.08*** (0.59)		4.96 (3.15)	3.41** (1.34)
Change in 2016 unemployment share		1.56 (1.23)	1.17 (1.27)		-1.60 (4.42)	0.45 (2.19)
Population density		0.015 (0.026)	-0.012 (0.029)		0.089 (0.092)	0.012 (0.042)
Population size (log)		-0.53 (0.43)	-0.56 (0.46)		-1.17 (1.10)	-0.54 (0.70)
Share of employed in high skilled labor		-0.99*** (0.26)	-1.06*** (0.30)		-3.37 (2.18)	-1.53** (0.61)
Share of employed in low skilled labor		0.87*** (0.19)	0.86*** (0.20)		-0.49 (1.17)	0.43 (0.40)
Share of migrants from outside UK		-0.20 (0.72)	-0.47 (0.82)		3.86 (3.38)	-0.23 (1.09)
Share of ethnic group		-0.20*** (0.058)	-0.19*** (0.066)		-0.61 (0.42)	-0.42** (0.18)
UKIP share in EE		0.81*** (0.047)	0.79*** (0.056)			
Wales			-2.80** (1.14)			-2.61 (1.89)
Scotland			0.018 (1.74)			8.86** (3.92)
London			3.94*** (1.35)			6.91** (2.86)
Constant	46.3 (43.5)	43.4*** (8.59)	34.4*** (10.4)	-81.4 (56.6)	34.9 (24.2)	8.08 (22.7)
N	380	380	380	380	380	380

Table F.10: OLS estimate of difference in EE to referendum turnout on Leave share/increase in Leave vote

	(1)	(2)	(3)	(4)
	Leave share		Leave - Ukip	
2016 Referendum TO - 2014 EE TO	0.82*** (0.13)	-0.059 (0.077)	-0.17** (0.074)	-0.038 (0.073)
Constant	22.5*** (5.06)	41.0*** (7.54)	29.2*** (2.93)	41.9*** (7.56)
Region controls	No	Yes	No	Yes
Socio-economic controls	No	Yes	No	Yes
N	275	275	275	275

OLS regression with robust standard errors in parentheses. Control variables as in Table 7.2. Sample is restricted to ‘rainy’ regions in England.

\*\* (\*, \*\*\*) indicates  $p < 0.05$  (0.1, 0.01)

Table F.11: OLS estimate of turnout difference in EE to referendum turnout on Leave share/increase in Leave vote

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	'Rainy regions' (England)				Great Britain			
	Leave share		Leave - Ukip		Leave share		Leave - Ukip	
2016 Referendum TO - 2014 EE TO	0.82*** (0.13)	-0.059 (0.077)	-0.17** (0.074)	-0.038 (0.073)	0.74*** (0.10)	-0.14** (0.064)	-0.26*** (0.058)	-0.14** (0.060)
Constant	22.5*** (5.06)	41.0*** (7.54)	29.2*** (2.93)	41.9*** (7.56)	25.1*** (4.05)	50.6*** (6.58)	33.9*** (2.29)	50.4*** (6.56)
Region controls	No	Yes	No	Yes	No	Yes	No	Yes
Socio-economic controls	No	Yes	No	Yes	No	Yes	No	Yes
UKIP support	No	Yes	No	No	No	Yes	No	No
N	275	275	275	275	380	380	380	380
Adj. R2	0.17	0.92	0.031	0.65	0.14	0.91	0.067	0.66

OLS regression with robust standard errors in parentheses. Control variables included (as indicated) are the ethnic white population share; the population share being immigrants from outside the UK; population share aged 20-30; population share aged 60+; share of unemployed; share of employed in high skilled labor; share of employed in low skilled labor; change in 2015-2016 unemployment; population density; population size (logged); share of UKIP support in last European Parliament election (for non-delta specification of explanatory variable only); regional dummies for Scotland, Wales, Northern England, Southern England, London.

\*\* (\*, \*\*\*) indicates  $p < 0.05$  (0.1, 0.01)

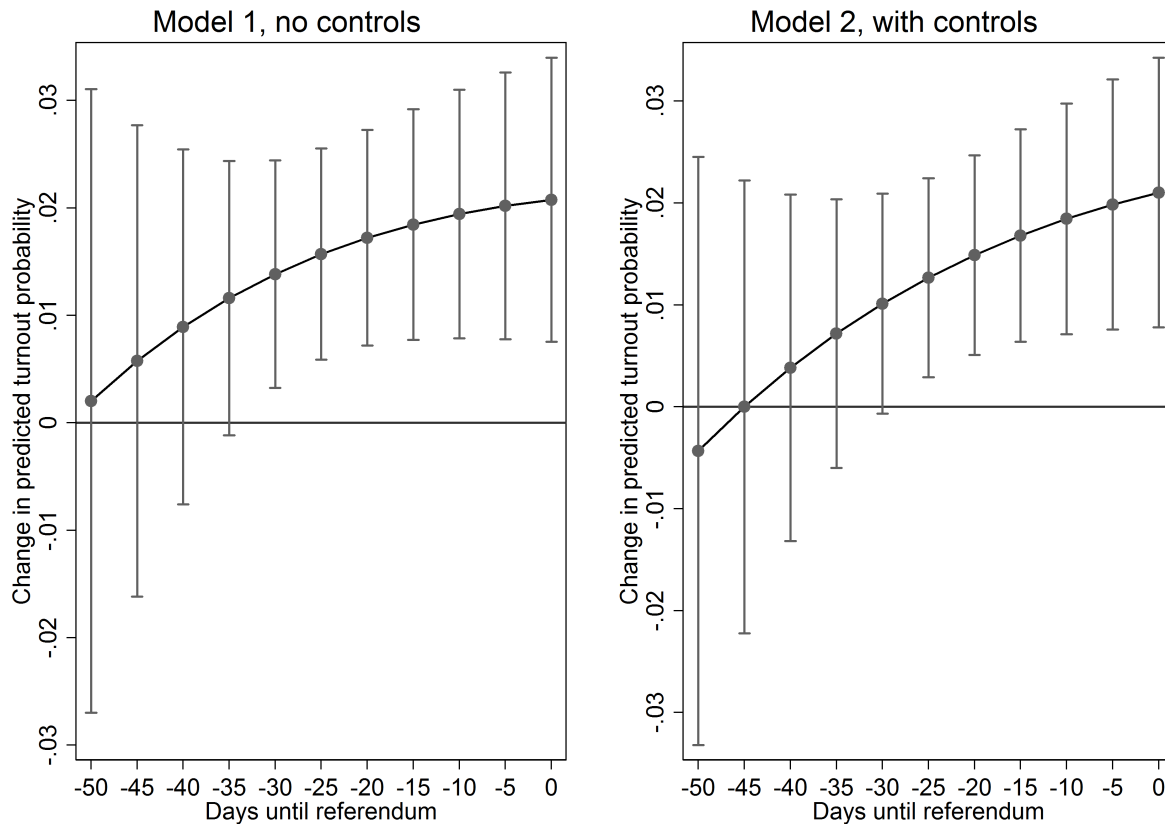
Table F.12: Logistic regression on self-assessed turnout propensity by past Ukip vote and time to referendum in British Election Study

	(1)	(2)
	Referendum turnout likely or very likely	
Referendum turnout likely or very likely		
UKIP vote in 2015 general election=1	1.12** (0.49)	1.15** (0.49)
Days until referendum	0.0062 (0.0050)	0.0029 (0.0055)
UKIP vote in 2015 general election=1 × Days until referendum	0.021 (0.015)	0.025 (0.016)
Age		0.023*** (0.0051)
Female		0.082 (0.13)
Scotland		-0.14 (0.17)
Wales		0.38 (0.28)
Abstention in 2005, 2010 or 2015 general election		-0.92*** (0.13)
Political attention (0-10)		0.35*** (0.027)
Married		0.12 (0.15)
No dependents in HH		0.18 (0.14)
Houseowner		0.11 (0.15)
Unemployed		-0.39 (0.48)
White ethnicity		0.69*** (0.22)
Migrant		-0.15 (0.34)
Working class		0.073 (0.54)
Middle class		-0.13 (0.14)
Constant	3.44*** (0.16)	-0.27 (0.43)
N	12636	12551
Pseudo-R2	0.0038	0.15
Log-Likelihood	-1878.1	-1514.7



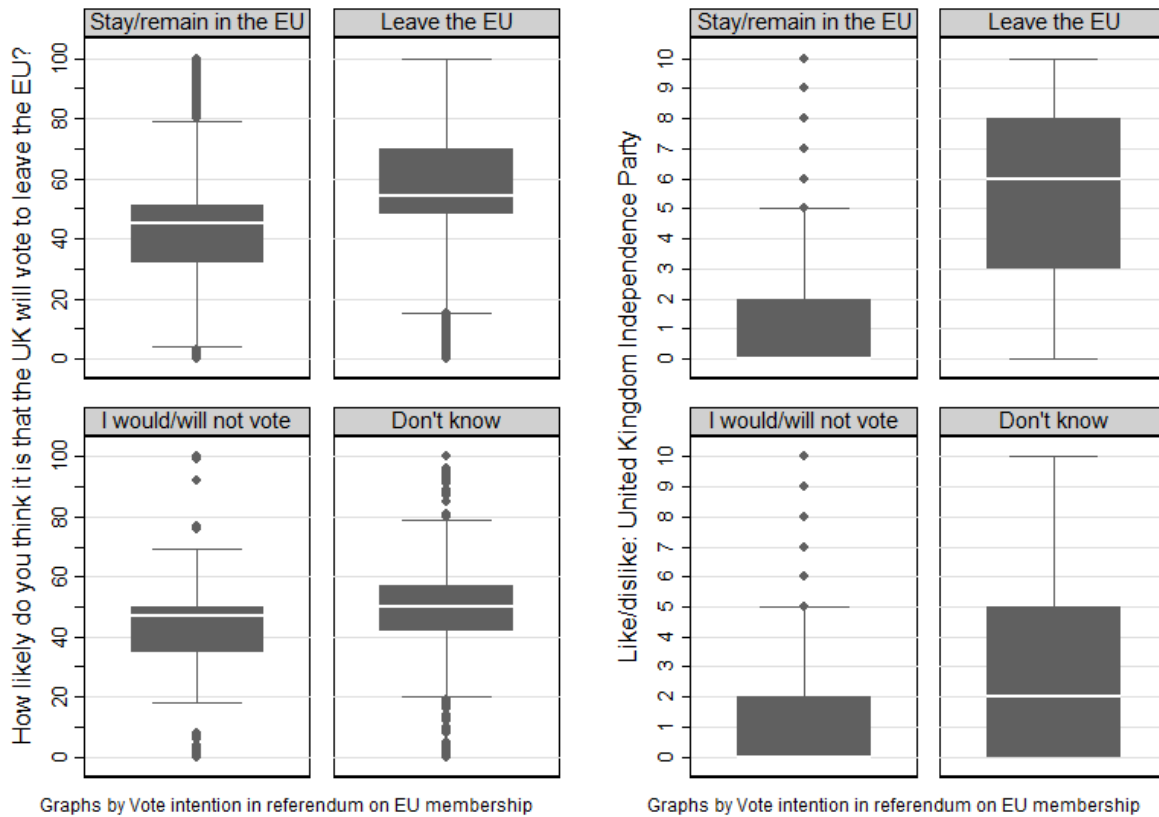
## F.2 Appendix Figures

Figure F.1: Change in predicted probability of turnout between Ukip- and non-Ukip-supporters in the 12 pre-referendum weeks



Predictions for Average Marginal Effects from binary logistic regression with dependent variable self-assessed turnout intention ('fairly likely' or 'very likely') and independent variable whether respondent supported UKIP at general election, whether he/she responded 1-48 days before the referendum and interaction term. BES survey weights for representative sample of Great Britain used. 95% confidence intervals are shown. Weighted N=12,636 (left panel). Right panel (weighted N=12,551) includes control variables as in Model 2 of Table A12.

Figure F.2: Distribution of perceived closeness and preferences towards Ukip by stated referendum vote intention



BES survey weights for representative sample of Great Britain are used. Weighted N's in left panel: 9,293.042 (Remain), 9,444.202 (Leave), 316.122101 (Would not vote), 1,349.634 (Don't know), 20,403 (Total). Weighted N's in right panel: 9,039.741 (Remain), 9,114.877 (Leave), 149.53261 (Would not vote), 1,084.849 (Don't know), 19,389 (Total).

## F.3 Preference Gap Calculation

The substantive relevance of the estimates can be assessed using the framework of Fowler (2015). Taking the results of Table 7.2 literally, it is possible to disentangle the average support for Leave among regular and among (weather induced) occasional voters: With good weather, turnout increases; given the estimates in Table 7.1, Model 2, about 0.5% [0.25;0.75] of the electorate vote only in good weather.<sup>1</sup> Given the positive relation between turnout increase and Leave support (0.61 in 7.2, Model 3), average Leave support is higher where weather was good and more occasional voters turned out. Particularly, among the 0.5% of the electorate voting only in good weather, the Fowler-approach implies a Leave share of 90.7% [11.8;173.5]. Thus, this implies a preference gap in Leave support of 36.6 percentage points between regular and these occasional voters<sup>2</sup> These calculations invoke very strong assumptions<sup>3</sup>. We should therefore not directly interpret this difference; nonetheless, it gives numeric support that an average (weather induced) occasional voter and an average regular voter differ very much with respect to their preferences on EU membership in the case at hand.

This does not imply, however, that persuasion was not relevant in the case at hand. The point estimates suggest that a one percentage point increase in turnout (due to good weather) led to around a 0.7 percentage point increase in the share of the Leave campaign. Overall, turnout increased by around 38.5 percentage points from 36.6% to 75.1%; EU opposition increased by 19.7 percentage points from a Ukip share of 31.4% to a Leave share of 54.1%. We only observe these aggregate figures and cannot tell how much of this effect is due to persuasion, and how much due to mobilization. However, even if we assume a constant effects framework, the average increase in turnout of around 38.1 percentage points would lead to an increase in the Leave vote slightly above 20 percentage points (*e.g.* 23.2 [5.3,45.7] percentage points with an ATE of 0.67 as of Model 2). As we do not observe this increase,

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<sup>1</sup>The effect of one millimeter of rainfall on the difference in turnout is 0.11 [-.17,-.06] (comp. Table 7.1, Model 2). The average decrease in turnout due to average election day rainfall of 4.3 mm is therefore 0.5 [0.25;0.75] percentage points, *i.e.* 0.5% [0.25;0.75] of the electorate ( $P_{kO}$ , the proportion of voters that is marginal to rainfall).

<sup>2</sup>Voters under rainfall are the control group, voting despite rain; those voting only in good weather are ‘treated’ occasional voters. The average predicted increase in turnout under rainfall is 38.0 percentage points, with an average baseline of 37.1 percent EU parliament turnout. Thus predicted turnout under rainfall is 75.1 percent ( $P_R$ , the proportion of regular voters in the population). Finally, the predicted leave share under rainfall is 54.1 percentage points ( $S_R(Y)$ ). Given these predictions, and drawing on the treatment effect of 0.61 (Table 7.2, Model 3),  $S_{kO}(Y) - S_R(Y) = ATE * (1 + P_R/P_{kO})$  calculates 90.7% [11.8;173.5] for the Leave share of occasional voters ( $S_R(Y)$ ) and a preference gap of 36.6 percentage points between regular and occasional voters.

<sup>3</sup>These estimates apply only to an average English districts/boroughs in the sample at hand. Additional assumptions are invoked on the functional form of the predictions of turnout and vote shares, concerning the instrument, and (not) weighting the estimates by varying population size; it is as well left aside that the referendum likely brought other occasional voters to the polls, as predicted turnout under rainfall is still higher than average general election turnout. Additionally, the uncertainty involved is large.

persuasion is likely an important part of the story as well. (Note the wide confidence intervals: For example, for Model 2 of Table 7.2 with an effect of 0.67 percentage points, these are [0.14,1.20]. Predictions based on these models therefore have to be treated with caution.)

## F.4 Referendum Preferences in the Population of Voters on the Margin of Participating

Electoral benefits stem from either the D-term or the PB-term. Understanding the D-term as an expressive element of voting (Dhillon and Peralta, 2002), this would imply that the emotionalized debate around EU-membership, national sovereignty and immigration was more important for (latent) Leave compared to (latent) Remain supporters and thus the former were more inclined to affirm their preference. A similar implication would follow from expressive voting theories (Brennan and Hamlin, 1998), implying that the policy offered by Leave is closer to the ideal point of citizens formerly alienated in the UK political system, which are then mobilized to a relatively higher degree. There is evidence for this argument in the survey data: According to pre-election polls the most important issues<sup>4</sup> for voters for the referendum had been the economy (mentioned by 21 %), immigration (20 %) and UK sovereignty (17 %, trailing are issues with mention < 7%). For voters indicating a preference for Remain, economy reaches 38%; for voters indicating a preference for Leave, immigration and sovereignty are with 35% and 31% the most frequently mentioned topics connected to their referendum-decision. However, in the group of occasional voters those individuals indicating immigration and sovereignty as their most important topic report a significantly higher average turnout intention (conversely a lower turnout intention if a voter states the economy is her most important issue) (see column 2 and 3 of Table 7.3).<sup>5</sup> These results hold for the whole survey population and when using the average response on 4-scale interest in the EU referendum as target variable. This indicates that occasional voters with latent Leave preferences were more likely to turn out than Remain-leaning occasional voters. Hence, the mobilization of occasional voters was likely lop-sided.

Was this lop-sided mobilization potentially driven as well by differing perceptions of vote pivotality? Concerning the PB-term, the treatment effect could imply that occasional voters had a heterogeneous perception of the pivotality of their vote, and those ultimately deciding to cast a ballot had on average a preference for Leave. I find some support for this mechanism among survey respondents: Respondents indicate whether they support Leave, support Remain, or plan to abstain/don't know yet. As reported in detail in Appendix Figure F.2 (left panel), Leave supporters and Remain supporters see their side winning on average, when

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<sup>4</sup>Categorized by the BES-team following the question ‘What matters most to you when deciding how to vote in the EU referendum?’

<sup>5</sup>The sample size is smaller, as only occasional voters are looked upon. Additionally, due to missings in the variables of interests and the survey weight variable, the sample size drops. Results are robust to not using weights.

asked ‘How likely it is that the UK will vote to leave the EU’. However, those respondents reporting an intention to abstain likewise see a victory for Remain ahead. These respondents are not asked whether they would vote Leave or Remain. However, they on average (right panel) have a strong dislike for Ukip, making Remain support their likely average preference. This indicates that voters that stayed home on average favored the Remain side, and at the same time wrongly perceived Remain to be winning. This implies as well that the mobilization of occasional voters was lop-sided.

The arguments above indicate that among occasional voters, latent Leave supporters were more likely to ultimately turn out. The temporal structure of the data allows for a final test whether this argument likely holds. As random subsets of the survey population were questioned each day 48 days to 1 day until the referendum, we can contrast ‘(very) likely’ turnout intention for Leave/Remain-leaning respondents over time. As an indication for (latent) Leave support, I differentiate respondents by support for Ukip in the last general election. Figure 7.2 reports a margins plot on whether Ukip supporters in the last general election were more likely to intend to turn out the closer election day came relative to other party supporters.<sup>6</sup> An important feature of this comparison is that confounding factors that influence turnout intention of past-Ukip and other party supporters similarly are implicitly controlled for as long as they influence both groups similarly (*e.g.* overreporting). It indeed seems to be the case that time played a role for the mobilization of (latent) Leave supporters. 48 days before the referendum, Ukip supporters were equally likely to report an intention to turn out; just before the referendum, however, Ukip supporters are predicted to be two percentage points more likely to turn out.<sup>7</sup>

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<sup>6</sup>Depicted here are differences in average marginal effects with socio-demographic and political controls. However, the logistic regression coefficients and margins plots are nearly identical without using control variables (reported in Appendix Table F.12 and Appendix Figure F.1). Overall, 93% of the respondents assess themselves as likely voters. In the sample with full information on the variables used, this even reaches 97%. The Ukip vote is only slightly understated (10.5% in weighted sample against 12.6% at the election. Bias in the direction of the depicted effect is induced, if Ukip supporters that respond to the survey are systematically more likely to turn out compared to the general population of Ukip supporters or if Ukip supporters overstate their turnout propensity relative to other party supporters.

<sup>7</sup>This finding links to research on the party-specific effectiveness of mobilization; the effect of campaigning on turnout has *e.g.* been shown to be party specific in the 2010 UK general election (Fisher, Fieldhouse, Johnston, Pattie, and Cutts, 2016). Similarly, in a more general note, the geography of abstentions are argued to have favored particularly Labor in recent UK elections (Johnston, Borisjuk, Thrasher, and Rallings, 2012).

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