

Remembrance of Regimes Past

Essays on the Historical Legacy of Political Violence

by

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Declaration

I, the undersigned **Vlad Surdea-Hernea**, candidate for the degree of Doctor of Philosophy at the Central European University Doctoral School of Political Science, Public Policy, and International Relations, declare herewith that the present thesis is exclusively my own work, based on my research and only such external information as properly credited in notes and bibliography. I declare that no unidentified and illegitimate use was made of work of others, and no part of the thesis infringes on any person's or institution's copyright. I also declare that no part of the thesis has been submitted in this form to any other institution of higher education for an academic degree.

Vienna, November, 2024

Vlad Surdea-Hernea

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— Larry David

"You can be passionate about anything." — Jerry Seinfeld

Publication Status and Co-author Contribution

Per the regulations of the *Doctoral School of Political Science*, *Public Policy and International Relations* at the Central European University, doctoral students have the opportunity to submit a paper-based dissertation. The paper-based dissertation consists of three or more high-quality academic papers, at least one of which has reached the revise & resubmit stage of publication in a prestigious, peer-reviewed academic journal. The doctoral candidate must be the sole author of at least two of the papers, including the paper that has reached the revise & resubmit stage.

In accordance with these requirements, the first paper (**Chapter 2**) was published in the *European Political Science Review* in November 2024. The second paper (**Chapter 3**) is currently under revise & resubmit at *Comparative Political Studies*, while an earlier version of the third paper (**Chapter 4**) is currently under revise & resubmit at *Public Choice*.

In addition, **Chapter 3** was co-authored with Aurelian-Petruş Plopeanu, Senior Researcher at the Department of Social Sciences and Humanities, Alexandru Ioan Cuza University of Iaşi. The original idea for the chapter came from Vlad, based on one of his previously published papers¹. The data collection was done in close collaboration with Aurelian. Aurelian obtained the archival data on the Gulag, I developed the list of participants and injuries, conducted the interviews and the survey, and was responsible for developing the causal identification strategy and the R code. Aurelian was primarily responsible for data cleaning, while I was responsible for subsequent checks. The first version of the manuscript was written by me and then edited by Aurelian.

¹Surdea-Hernea, V. (2024). Political violence and long-term tax morale: Evidence from Romania's 1989 anti-communist revolution. *European Journal of Political Research*.

Acknowledgements

I never saw myself doing a PhD in Political Science. Math was the original plan, then Law - the kind of degrees my Eastern European family could brag about at dinner parties (read barbecues). And part of me hoped to get one in English Literature or Creative Writing, though I never seriously pursued that. But Political Science? A harder sell, to myself and to others, and frankly, some people back home still think I studied to become a senator or even the president of Romania. My high school maths professor was surely disappointed when I told him what I was going to do for my undergraduate degree, but now I hope there will be enough equations in the dissertation to make him happy. So here I am, and for once I didn't get completely bored and abandon ship. Sure, I've changed my dissertation topic so many times that my supervisor probably needs a flow chart to keep track, but the core question has never changed: what drives political behaviour?

Brilliant researchers everywhere are studying why people vote or protest, but when I settled in Vienna right after Covid, I did so with the intention of studying something else: how history shapes who we are and how we act. Finding an entire community studying historical legacies through institutions and culture (with a 2024 Nobel Prize in Economics to show for it) didn't deter me, although there is that moment in your PhD career when you finally have to admit that you might not be writing your magnum opus next year, and maybe, just maybe, you won't be replacing the people who have been studying this field for decades to soon. But on the positive side, I've realised that scientific progress is not about chasing novelty for its own sake, but about testing theories repeatedly, across settings and methods, and making sense of patterns. There is little point in pretending that every paper we write, every dissertation we defend, is groundbreaking and fills some imaginary gap; it would be a mess if the ground was broken so often. Instead, I value being a pattern spotter. This thesis reflects the patterns I've found. For now.

My journey through these three and a half years has been made much smoother by my partner-turnedfiancé, Letitia. She's been my unwavering supporter - reading drafts, checking my research on a daily basis, and eventually becoming my co-author (not in this dissertation, but watch out soon for our publications). Even when I was reluctant to discuss my work, her persistence pushed me to think deeper and ultimately made me a better writer. Which is really all I ever wanted to be. Some people want to write as intellectually sharp as Giovanni Sartori and Seymour Martin Lipset, or as fast and crisp as Daron Acemoglu. I just felt I had to be more like Barbara Kingsolver, but for academics.

For almost three decades, my family has been my anchor. It's no small thing to have that safety net. Even when they didn't understand what I was doing or why, their simple 'good for you' meant everything. Sometimes that's the purest form of approval - your family wanting the best for you, even if they're not sure what that looks like, as you probably don't really know either. Thank you.

My supervisor, Michael Dorsch, marked my PhD years in ways he may not fully realise. His first year class introduced me to papers that still guide my research approach and made me an avid reader of the *Quarterly Journal of Economics* and *Journal of Political Economy*. In addition to being a genuinely kind person - a rare quality that I value above all else - Michael brought me into his historical persistence project as an RA, a decision that ultimately set the direction of my dissertation. On the same project, Anand Murugesan's detailed and constant feedback transformed my relationship with code and built my confidence. His ongoing reminder to "think carefully about standard errors" became my mantra, especially important in our field.

My committee members, Carsten and Mariyana, each brought their unique strengths to the table. Carsten, true to form as *THE* political scientist, pushed me to think deeply about conceptual categories and their role in the social world. Naturally, I am not inclined to be so rigorous, so this went down well. And the fact that he continued to work with me despite our major differences on what constitutes causality in the social world, well, that has to count for something. Mariyana's meticulous line edits and granular comments were unparalleled. The fact that she went through every sentence of every paper, not only critiquing but offering tons of actionable suggestions, was beyond my expectations, knowing how busy people are. Together they improved my work on both a macro and micro level. Our panel meetings were also extremely productive and enjoyable, far from the nightmarish experiences that some of my colleagues talked about.

Many others deserve my appreciation. In this sense, I would like to thank the people who provided feedback, comments, or even just replied to my emails when I asked them about a particular methodological or substantive point: Quintin Beazer, Sascha Becker, Alexander Bor, Aron Buzogány, Alexander de Juan, Mihaly Fazekas, Donald Grasse, Peter Hull, Evelyne Hübscher, Holger Kern, Levi Littvay, Guy Michaels, Arieda Muço, Anand Murugesan, Eugenia Nazrullaeva, Anja Neundorf, Roman Olar, Leonid Peisakhin, Dóra Piroska, Kelly S. Ragan, Toni Rodon, Arturas Rozenas, Dean Schafer, Valentin Seidler, Roya Talibova, Jan Vogler, and Florian Weiler. A special thanks goes to Dóra Piroska, who took me as her RA for one year despite our complete epistemological mismatch, and helped secure my next step after the doctorate.

Whenever I re-read my papers, including those in this dissertation, I always seem to find something I do not like. I think maybe I should have devoted more space to the causal mechanisms, given a better historical background, done a specific robustness check, written another paragraph on the operationalisation of a variable, or defended my causal identification strategy against another endogeneity threat. I know that people who will read my papers will ask these questions, and that's legitimate, that's how it works, we challenge each other. At the end of the day this dissertation is not perfect and writing this is basically me accepting that fact. Nevertheless, with the support of all the people mentioned above, and with a work ethic that I am proud of, in the words of one of the greatest of all time, Larry David, I think this PhD has turned out to be *prett-ay*, *prett-ay*, *prett-ay*, *good*.

Abstract

This dissertation examines how political violence creates persistent legacies that shape long-term political behaviour. Combining quantitative causal inference tools with historical evidence in three European case studies, I demonstrate how violence embeds in community structures, influencing electoral choices, protests, and policy preferences long after perpetrators and victims are gone. The effects persist via distinct but potentially complementary mechanisms: weaponizing collective memory, forming anti-regime norms, and altering civil society infrastructure. The dissertation consists of three papers, each presented as a separate chapter.

In the **first paper**, I explore how community-level war losses affect long-term electoral behaviour. Using a dataset geolocating all French soldiers who died in World War I, I show that communities with higher death rates exhibit greater electoral support for the far-right. I provide a theoretical explanation, supported by empirical evidence, of how these persistent effects propagate: communities more exposed to war horrors develop stronger in-group preferences over out-groups. In France, where the in-group is primarily national, this translates into higher demand for nationalism, supplied by far-right parties.

In the **second paper**, together with my co-author, we examine how exposure to political repression shapes collective behaviour and dissent in high-threat environments. We develop a theory of repressioninduced norm formation, arguing that proximity to repressive institutions fosters anti-regime identities and increases dissident behaviour, even when dissent remains costly. Using original data on the Romanian Gulag, we show that localities hosting labour camps or extermination sites during communism experienced disproportionately more severe injuries during the 1989 Revolution, a phenomenon that cannot be explained by the composition of the repressive apparatus. We provide both quantitative and qualitative mechanistic evidence that this effect is partly driven by anti-communist norms developed in response to the Gulag, not merely by shifts in political opportunities.

Finally, in the **third paper**, I investigate whether exposure to authoritarian rule, which often suppresses pro-environmental groups threatening regime stability, has lasting effects on political behaviour after democratization. Empirically, I exploit Germany's post-WWII division into a liberal democracy and a Soviet-style autocracy using a geographic regression discontinuity design. I show that districts in the former East have lower climate policy equilibrium levels, and that climate-ambitious parties receive less electoral support. These effects are largely independent of East Germans' preferences in other political conflicts and can also be observed within the East, suggesting a direct, persistent impact of authoritarianism on environmental attitudes. Moreover, I show that this legacy increases polarization over climate policies, making the former East Germany fertile ground for climate-sceptical parties.

Contents

Al	ostrac	t		ii
Li	st of I	Figures		vii
Li	st of]	Tables		ix
1	Intr	oductio	n	1
	1.1	Histor	ical legacies of political violence: a state of the field	1
	1.2	Contri	butions of the Dissertation	5
	1.3	A note	on causality and methods	7
	1.4	Structu	are of the Dissertation	10
2	Echo	oes fron	n the trenches: World War I and contemporary support for the far-right in	L
France		12		
	2.1	Introd	uction	12
	2.2	The po	olitics of memory and electoral behaviour: a theoretical framework	14
		2.2.1	Shared war experiences and the formation of in-group/out-group preferences .	14
		2.2.2	The lingering effects of shared war experiences	17
	2.3	Resear	ch design	21
		2.3.1	Data	21
		2.3.2	Model specification and causal identification	25

CONTENTS

	2.4	Results	and discussion	29
		2.4.1	Main analysis	29
		2.4.2	Evidence from shifting political conflict equilibria	33
		2.4.3	Robustness checks	36
	2.5	Discuss	sion and conclusions	37
3	The	repress	ion-dissent nexus in high-threat environments: Evidence from the Roma-	
	nian	40 Gulag		
	3.1	Introdu	action	40
	3.2	A norn	native theory of dissent in high-threat environments	43
	3.3	Roman	iian communism and the Gulag system	46
	3.4	Data .		49
		3.4.1	Independent variable: Presence of Gulag facilities in a locality	49
		3.4.2	Dependent variable: Severe injuries during the 1989 Revolution	51
		3.4.3	Control variables: confounders and alternative explanations	53
	3.5	Causal	identification strategy	54
	3.6	The lo	ng-term effect of the Gulag on dissent during the 1989 Revolution	59
	3.7	Repres	sion and the creation of anti-regime norms	65
		3.7.1	Exposure to the Gulag and Communist Party membership	65
		3.7.2	Exposure to the Gulag and economic grievances	67
		3.7.3	Exposure to the Gulag and post-1989 political behaviour	69
	3.8	Pathwa	y case analysis	71
		3.8.1	The chronology of the Revolution in Timisoara	73
		3.8.2	Further empirical evidence from Timisoara	76
		3.8.3	External validity of the qualitative evidence	79
	3.9	Conclu	isions	83

CONTENTS

4	Atta	ck on cl	imate: The persistent effects of authoritarianism on the climate equilibria	85
	4.1	Introdu	action	85
	4.2	Theore	tical argument	87
		4.2.1	The role of the pro-environmental milieu in structuring attitudes and preferences	88
		4.2.2	Political regimes and the environmental milieu	90
	4.3	Enviror	nmental organizations in East Germany	91
	4.4	Data .		94
		4.4.1	Independent variable	94
		4.4.2	Dependent variable	94
	4.5	Causal	identification strategy	95
		4.5.1	Modelling selection intro treatment status	96
		4.5.2	Geographic regression discontinuity design	97
	4.6	Results	and discussion	100
		4.6.1	Main GRDD estimation	100
		4.6.2	Robustness checks and sensitivity analysis	103
		4.6.3	Correlated policy equilibria	105
		4.6.4	Heterogeneous effects of exposure to communism	109
		4.6.5	Climate policy dispersion	111
	4.7	Conclu	sions	115
5	Con	clusions		117
	5.1	Summa	rry of empirical findings	117
	5.2	Legacie	s of political violence: quo vadis?	120
Ap	pend	ices		147
A	Арр	endix to	Chapter 1	149
	A.1	Robust	ness checks for the main analysis	149
	A.2	Quality	of contact with immigrants	150

CONTENTS

	A.3	Additional figures	151
B	App	endix to Chapter 2	156
	B.1	Additional evidence	156
С	Арр	endix to Chapter 3	171
	C.1	A. Additional evidence	171

List of Figures

2.1	Visual summary of the theoretical argument	20
2.2	Map of WWI death rates across French municipalities	22
2.3	Directed acyclic graph (DAG) representing the causal model underlying our theoretical	
	argument	28
2.4	When to control for third variables, based on Cinelli, Forney, and Pearl (2022)	39
3.1	Geography of the Romanian Gulag	50
3.2	Distribution of independent and dependent variables	53
3.3	Directed acyclic graph (DAG) representing the causal model underlying our theoretical	
	argument	54
3.4	Effect of Gulag facilities in the vicinity of a locality	64
3.5	Determinants of the decision to participate in the Revolution of 1989	80
4.1	Directed acyclic graph (DAG) representing the causal model underlying my theoretical	
	argument	98
4.2	GRDD plot based on Calonico, Cattaneo, and Titiunik (2015)	100
4.3	Summary of the robustness protocol	104
4.4	Correlation plots for policy equilibria in Germany	106
4.5	Intensity of Stasi activity and vote shares of B90/The Greens	111
4.6	Typology of societies based on their policy equilibrium and dispersion	113

LIST OF FIGURES

B1	Histograms of death rates across Fre	nchd departments .	 155
	0	1	

List of Tables

2.1	Effect of WWI death-rate on support for FN (1993-2017)	29
2.2	Heterogeneous effects of WWI death-rate on support for FN (1993-2017)	30
2.3	Individual-level effects of WWI death rates	32
2.4	Effects of WWI death-rates on contact with immigrants	32
2.5	Effect of WWI death-rate on socio-political equilibria	34
2.6	Effects of WWI death-rate on RILE equilibrium	35
3.1	Detailed Summary Statistics by Treatment Status	55
3.2	Relationship between Gulag geography	58
3.3	Results: Selection-on-observables	60
3.4	Results: Selection-on-observables in rural areas	62
3.5	IV results	63
3.6	Exposure to the Gulag and membership in the Communist Party	67
3.7	Economic grievances and the memory of political violence	69
3.8	The effect of the Gulag on anti-governmental protests post-1990	70
3.9	The effect of the Gulag on anti-communist voting patterns post-1990	71
4.1	Effect of the East German regime on the climate policy equilibrium	101
4.2	Effect of the East German regime on climate policy equilibrium (donut regions)	102
4.3	Sensitivity analysis based on Oster (2019)	105
4.4	Effect of the East German regime on the residualized climate policy equilibrium	108

LIST OF TABLES

4.5	Climate policy equilibria: heterogeneous treatment based on the intensity of Stasti activity 11	.0
4.6	Effect of the East German regime on the climate policy dispersion	.4
A1	Effect of WWI death-rate on support for FN(1993-2017)—No spatial covariates 14	i9
A2	Effect of WWI death-rate on support for FN(1993-2017)—No spatial covariates or Moran	
	eigenvectors	50
A3	Effect of WWI death-rate on support for FN(1993-2017)—No Moran eigenvectors 15	50
A4	Effect of WWI death-rate on support for FN(1993-2017)—Fitted values of death rates	
	(region)	51
A5	Effect of WWI death-rate on support for FN(1993-2017)—Propensity score matching. 15	51
A6	Effect of WWI death-rate on support for FN(1993-2017)—Non-parametric CBPS match-	
	ing	52
A7	Effect of WWI death-rate on support for FN(1993-2017)—GLM-based PS matching . 15	52
A8	Effect of WWI death-rate on support for FN(1993-2017)—Different leaderships 15	52
A9	Effect of WWI death-rate on support for FN —Pre-1990 data	53
A10	Individual-level effects of WWI death rates by age profile	53
A11	Effect of WWI death-rate on support for FN(1993-2017)—Additional covariates (post-	
	treatment)	53
B1	Effects of WWI death-rates on quality contact with immigrants	64
B2	Effect of WWI death-rate on support for right-wing parties in 1910-1914 (placebo) 15	64
B1	Grid cell clustered standard errors	56
B2	Continuous independent variable	57
B3	Binary dependent variable	57
B4	Binary dependent variable and continuous independent variable	58
B5	Removing outliers in terms of Gulag facilities	\$8
B6	Removing outliers in terms of Gulag injuries	59
B7	Closeness to a high-repression area, 3 facilities	59

LIST OF TABLES

B8	Closeness to a high-repression area, 2 facilities
B9	Closeness to a high-repression area, 5 facilities
B10	Exposure to the Gulag and pressure to become a secret police informant
B11	Robustness check: severity of Securitate presence
B12	Alternative IV results
B13	Number of participants as DV
B14	Results: Selection-on-observables with logged DV
B15	Results: Selection-on-observables with Poisson regression
B16	Results: Selection-on-observables with negative binomial regression
B17	Results: Selection-on-observables with zero inflated negative binomial regression 164
B18	Results: Selection-on-observables without top and bottom 1% of observations based on
	severe injuries
B19	Results: Selection-on-observables without top and bottom 2% of observations based on
	severe injuries
B20	Results: Selection-on-observables without top and bottom 5% of observations based on
	severe injuries
B21	Results: Selection-on-observables without top 5% of observations based on severe injuries 166
B22	Results: Selection-on-observables without top 2% of observations based on severe injuries 167
B23	Results: Selection-on-observables without Bucharest
B24	Results: Selection-on-observables without Bucharest and Timisoara
B25	Results: Selection-on-observables without Timisoara
B26	Results: Selection-on-observables with IHS DV
B27	Results: Selection-on-observables with square root DV
B28	Placebo Test: First Stage Results for Distance to Nearest Facility
B29	Results: Selection-on-observables with injuries normalized by population density 170
C_1	Effect of the East Common maximo an electrical energy of the Doo /The Common 171
CI	Effect of the East German regime on electoral support for B90/The Greens 1/1
C^{2}	Effect of the East German regime on the climate policy equilibrium-Euclidean distance 172.

LIST OF TABLES

C3	Effect of the East German regime on the climate policy equilibrium-Nuclear power $\ . \ .$	172
C4	Effect of the East German regime on the climate policy equilibrium-Urban networks	173
C5	Effect of the East German regime on the climate policy equilibrium-Propensity score	
	matching	173
C6	Effect of the East German regime on the climate policy equilibrium-No Moran eigen-	
	vectors	174
C7	Effect of the East German regime on the climate policy equilibrium-No grid cell FEs	174
C8	Effect of the East German regime on the climate policy equilibrium-No pre-processing	
	algorithm	175

1

Introduction

1.1 Historical legacies of political violence: a state of the field

Most people go through life assuming some degree of agency over their actions, both in their daily activities and in their engagement with political institutions. When citizens vote in democracies, they often attribute their choices to ideological preferences or strategic considerations about immediate electoral outcomes. Similarly, participants in collective action such as mass demonstrations typically frame their involvement as a response to the perceived mishandling of specific issues by the authorities. In both examples, cause and effect seem to follow each other closely. While such proximate drivers of individual decision-making are undoubtedly important, I argue that there are more distal, structural reasons why certain categories of people are systematically more likely to engage in certain forms of political action than others. In short, I maintain that contemporary political behaviour is partly shaped by historical legacies.

In this dissertation, I examine one such legacy¹, that of political violence². Specifically, I examine how

¹I adopt the framework of Wittenberg (2015), in which arguments for the existence of legacies require three components: (1) an outcome that cannot be fully explained by contemporary factors, (2) an alleged historical antecedent to the outcome that is either a cause of the outcome or at least an enabling condition, and (3) a mechanism that fills in the temporal links between the historical antecedent and the outcome.

²I define political violence according to Davenport (2007), as the use of physical sanctions against an individual or organ-

experiences of political violence can persist across generations, transmitted through community norms and inter-family ties, and ultimately influence how citizens interact with political structures far removed from those who initially committed the harm.

The idea that the experience, or even the memory, of political violence has consequences that go far beyond shaping the immediate responses of victimised populations is far from novel. Indeed, over the past decade(s), research on the historical legacies of political violence has become a vibrant, multidisciplinary field, generating numerous publications in leading journals in political science, sociology and economics, and developing its own epistemic infrastructure of dedicated journals, conferences, handbooks, and seminars (Walden & Zhukov, 2020). This scholarly interest gained traction as part of a broader turn within political science and economics that led scholars to pay more attention to how the past shapes contemporary outcomes (Acharya, Blackwell, & Sen, 2016; Charnysh, Finkel, & Gehlbach, 2023; Cirone & Pepinsky, 2022) by fusing the conceptual framework developed by comparative historical analysis (Mahoney & Rueschemeyer, 2003) with the quantitative methods and approach to causal inference popularised by the credibility revolution (Angrist & Pischke, 2010). While early work on historical legacies focused primarily on how formal institutions such as property rights persist over time (Acemoglu, Johnson, & Robinson, 2001, 2002), research on political violence moved beyond these narrow formal institutional concerns to examine how the exercise of power - particularly through state-sponsored violence, war and repression - imprints itself on the fabric of society.

Recent scholarship has documented a wide range of effects of political violence across domains of political and social behaviour. For example, there is evidence that exposure to violence can significantly alter political participation and preferences. N. Lupu and Peisakhin (2017) find that descendants of Crimean Tatars deported in 1944 retain strong ethnic identities today, identities that drive their anti-Russian political behaviour. Similarly, communities affected by Stalin's terror in the 1930s show persistent political mistrust of Soviet and post-Soviet authorities, expressed through political action when the opportunity structure allowed (Rozenas & Zhukov, 2019), while the German occupation of World War II contin-

isation to impose significant costs and deter activities and beliefs perceived as challenging the government. Political violence can be imposed by external actors, like in the case of inter-state wars, or by domestic one, usually through state-sponsored repression.

ues to shape voting patterns in countries such as Greece (Fouka & Voth, 2023). Social behaviour and intergroup relations are also shaped by the lingering influence of violence. Some studies suggest that repression may increase political participation and promote pro-social behaviour within victimised communities (Hartman, Morse, & Weber, 2021) as well as empathy towards out-groups (Dinas, Fouka, & Schläpfer, 2021; Wayne & Zhukov, 2022). Conversely, other research suggests that a history of political violence can weaken institutional trust and civic engagement over generations, leading to suboptimal social and economic performance in the long run (Cassar, Grosjean, & Whitt, 2013; Conzo & Salustri, 2019).

This literature identifies several key transmission channels that embed these effects in community structures and individual behaviour long after the original events, thereby explaining the formation of such stable legacies. First, political violence often triggers the formation of collective memories that serve as repositories of shared trauma (Alexander, 2004; Fouka & Voth, 2023; Halbwachs, 1992). These memories forge not only passive reminders, but active forces that shape how communities perceive themselves and their relationship to the existing and expected nature of political authority. Through formal institutions such as schools and informal channels such as family narratives, these collective memories are continually reconstructed and passed on to subsequent generations (Wertsch, 2002), often without immediate intention. This process is particularly powerful when political violence is experienced indiscriminately at the community level, as shared victimisation fosters stronger bonds and more resilient transmission mechanisms (Volkan, 2001).

Second, exposure to political violence can fundamentally alter local institutions and social networks. This occurs through a form of institutional stickiness - the tendency for rules and practices developed in response to violence to remain in place even after the original threat has disappeared. Such a phenomenon is very common in post-communist countries in Eastern Europe (Pop-Eleches & Tucker, 2017). For example, communities may maintain information-sharing networks or mutual support systems originally devised to cope with high levels of state repression, which then become permanent features of local social organisation.

Third, political violence often results in the intergenerational transmission of both trauma and adap-

tive behaviour (Argenti & Schramm, 2010; Dinas et al., 2021; Kazlauskas, Gailiene, Vaskeliene, & Skeryte-Kazlauskiene, 2017; N. Lupu & Peisakhin, 2017). Parents who have directly experienced violence may socialise their children differently, imparting specific political attitudes or behavioural strategies that reflect their own experiences. This vertical transmission of attitudes and values can persist even when children have not directly experienced the original violence, due to what scholars refer to as "chosen trauma" - the unconscious transmission of a shared mental representation of a massive traumatic experience from one generation to the next, using the same reference point in the form of the perpetrator of violence and his symbolic successors (Volkan, 2001).

Fourth, violence can reshape the physical and demographic landscape of affected areas in ways that create path dependencies. The destruction of infrastructure, forced migration, and changes in local economic structures can set communities on different development trajectories that continue to influence political behaviour long after the episode of violence has ceased (Charnysh & Finkel, 2017; Haran Diman & Miodownik, 2024; Nunn, 2008). These material legacies often interact with and reinforce psychological and social mechanisms of persistence, thereby perpetuating the long-term effects of political violence.

Finally, political actors can deliberately cultivate and manipulate memories of violence for contemporary political purposes, transforming historical experiences into what Zerubavel (1996, 2003) calls "mnemonic communities". This instrumental use of collective memory perpetuates the behavioural and attitudinal effects of violence across generations, particularly when political entrepreneurs use these historical narratives to mobilise support and legitimise their agendas among groups who feel their current status is undermined by historical injustices (Dehdari & Gehring, 2022; Fontana, Nannicini, & Tabellini, 2023).

This dissertation examines such mechanisms of persistence in three different contexts and focusing on different types of political violence: interstate war (World War I in France), (post-)totalitarian repression (the Romanian Gulag), and authoritarian supression of the civil society infrastructure (East Germany). In each case, I trace how the original violence produced lasting effects through distinct but complementary channels: the weaponisation of collective memory by the far-right in France, the formation of anti-regime norms through shared victimisation in Romania, and the destruction of civil society infrastructure in East Germany. By examining these diverse cases through both quantitative and qualitative lenses, this dissertation contributes to our understanding of how political violence becomes embedded in the social fabric of communities and continues to shape political outcomes decades later.

1.2 Contributions of the Dissertation

This dissertation makes four main contributions to the literature on the historical legacies of political violence.

First, by exploring research questions in less studied contexts, such as Romania, the dissertation enhances the external validity of existing findings when the results are in line with expectations, and generates new theories when the results challenge the existing consensus. Consistent with the recent trend towards single-country studies in comparative political science (Pepinsky, 2019), each chapter uses detailed historical knowledge and original data to examine how political violence has shaped outcomes in specific settings, incrementally adding credibility to the overall body of work. However, unlike purely idiographic accounts, my three case studies address broader patterns in the literature. For example, while the impact of communist repression on anti-regime attitudes has been extensively studied in the Soviet context (Rozenas & Zhukov, 2019), exploring similar dynamics in Romania assesses the generalisability of these findings to different institutional and cultural settings (Cirone, Spirling, et al., 2021), while avoiding the portrayal of communism as a monolithic force (Pop-Eleches & Tucker, 2017; Simpser, Slater, & Wittenberg, 2018).

Second, this dissertation never reports correlational evidence without attempting to link it to robust arguments about the mechanisms through which political violence persists, following best practice in the field of historical persistence (Acharya, Blackwell, & Sen, 2022; Voth, 2021). These proposed mechanisms are not simply post-hoc explanations, but theoretically grounded processes that connect political actors and could operate in other contexts where similar conditions prevail (Gailmard, 2021a, 2021b). Thus, by specifying the scope conditions for each mechanism, this dissertation contributes to the construction of middle-range theories of how historical episodes of violence create lasting legacies on political

behaviour.

Third, this dissertation advances causal inference methods in historical political economy by rigorously evaluating the conditions under which historical violence can be considered exogenous, an important discussion in the field (Caicedo, 2021; Callis, Dunning, & Tuñón, 2022; Kocher & Monteiro, 2016). Rather than assuming that historical events provide clean natural experiments, each chapter models the selection process leading to exposure to political violence and employs appropriate empirical strategies to mitigate potential endogeneity. In addition, the dissertation demonstrates how mixed-methods approaches - combining quantitative causal inference with qualitative process tracing - can enhance both the internal and external validity of findings on the historical legacies of violence.

Fourth, each chapter explicitly addresses its limitations and the conditions under which its findings may not apply. Rather than claiming definitive answers, the chapters invite dialogue with other scholars studying similar phenomena. This is important as the use of design-based quantitive methods can lead to marginal causal effects being treated as main determinants of a political phenomenon (Rozanas, 2021). By transparently discussing methodological challenges and theoretical scope conditions, the dissertation contributes to cumulative knowledge about how political violence shapes long-term outcomes. This approach is consistent with recent calls for greater openness about uncertainty in political science research, and recognises that even imperfect evidence can advance scientific understanding if its limitations are clearly stated.

In terms of content, the research questions addressed in this dissertation are not inherently new to the study of political violence. However, neither is our understanding of the previous answers so definitive as to justify moving on from these questions without further improving both internal validity (i.e. the empirical designs) and external validity (i.e. the range of cases from which we draw evidence with a claim to generalisability). This dissertation is an attempt to improve both.

1.3 A note on causality and methods

Most readers will agree that, from a methodological point of view, my dissertation constitutes an exercise in the use of quantitative methods to study the impact of historical episodes of violence. After all, I produce (multiple) regression tables in all chapters, and some of the empirical designs that I spend many pages defending as valid causal identification strategies, such as regression discontinuity designs or instrumental variables, are the epitome of modern quantitative work (Dunning, 2010; L. Keele, 2015; Samii, 2016; Sekhon, 2009). However, I think this is mostly true in letter rather than in spirit. Studies that examine the lasting effects of historical events, such as episodes of political violence, often rely more heavily on correlational and qualitative methods - such as case studies, narratives, pattern matching, historiographical evidence and interviews - than those that examine immediate, proximal relationships, even if they do not acknowledge it. There are two main reasons for this tendency.

First, the lack of intentional randomisation in most historical contexts forces researchers to rely on settings that, with additional structural assumptions, approximate randomisation - so-called "as-if" or quasi-random shocks (DiNardo, 2010), which should always be taken with a grain of salt (Dunning, 2008). Making a convincing case for this as-if randomness requires much more than presenting simple balance tests on observed covariates, since the differences that introduce bias could come from extremely unexpected conditions, rendering the former insufficient for conditional independent to hold. Instead, it requires a focused historical understanding of how a particular situation unfolded and what factors shaped the environment under consideration (Cirone et al., 2021; Gailmard, 2021a, 2021b; Kocher & Monteiro, 2016). Ideally, the collection of causal evidence from different sources and with different degrees of internal validity can serve as a macro robustness check for results predicated on key causal identification assumptions. The search for the perfect natural experiment, which does not require contextual knowledge, seems rather elusive in the field of historical persistence (Dippel, Leonard, et al., 2021), and even more so in the study of political violence, where the revealed and stated behaviour of actors often differ significantly even in the short term, let alone over time.

Second, historical causes are inherently distal, as there is a significant temporal gap between the cause

and the observed effect. For the cause to exert influence over time, some mechanism must transmit its effects across generations (Ehring, 1985, 1987). While ideally one would provide mechanistic evidence of quality comparable to that of the main relationship studied, this often remains an aspiration due to data limitations that hinder capturing historical norm formation processes or community-level dynamics in detail. Moreover, quantitative methods that consider mechanisms in terms of intermediate variables, such as causal mediation analysis (Imai, Keele, Tingley, & Yamamoto, 2011), have extremely stringent identification assumptions that are realistically unlikely to hold in the case of historical legacy arguments. In the absence of such direct tests, correlational evidence (e.g. heterogeneity analysis) and even more so qualitative evidence (e.g. case studies, pattern matching, and process tracing) become essential to relate results to theory in a way that takes account of contextual dependencies and specificities (Falleti & Lynch, 2009; Møller, 2024; Verghese, 2024).

With this in mind, it has never occurred to me to abandon a research question I was interested in just because there is no perfect method for providing a definitive answer, both in terms of a causal relationship $X \rightarrow Y$ and the mechanisms of transmission and persistence that link X and Y. Instead, in this dissertation, the aim of each chapter has been to contribute to the body of causal knowledge while acknowledging the empirical limitations of the methods used. Of course, some research designs are simply better than others given their respective objective. As Sekhon (2009) succinctly put it, "Without an experiment, natural experiment, a discontinuity, or some other strong design, no amount of econometric or statistical modelling can make the move from correlation to causation persuasive." Further extending this argument, qualitative knowledge derived from one case study can hardly make the move towards robust correlational cross-case evidence. Thus, while conducting process tracing is not the same as running an experiment in which we could play God and redraw maps and change people's innate preferences, causal knowledge, of different relative value is produced from the former as well (Kubinec, 2022), and in the study of historical persistence it might be necessary to rely on it.

Consequently, one of the desiderata driving the dissertation was that one can and should learn from potentially biased research designs (Little & Pepinsky, 2021). In Chapter 2, the validity of the (quasi-)natural experiment ultimately rests on an understanding of the combat dynamics of World War I on

the Western Front - knowledge that is more the province of historians than econometricians. Similarly, in Chapter 3, reconstructing the process of Gulag genesis and demonstrating exactly how the theorised mechanism propagated during the Romanian Revolution of 1989 required interviewing participants, conducting surveys, and tracing the process through archival resources. In Chapter 4, explaining the variation in the magnitude of the estimates around the former East-West German border required not only running robustness checks and observing how the coefficients change in response to additional covariates or different estimators, but also reading newspapers documenting pro-environmental protests in the 1970s and 1980s. In all chapters, I have included sensitivity analyses so that the reader can see, to the extent that current methods allow, how far off my assumptions need to be in order to explain my causal effects. I think this should gradually become the norm when working with observational data, complementing the now obligatory section on robustness checks.

Throughout these chapters, I have made my assumed causal models transparent by consistently presenting and defending the directed acyclic graphs (DAGs) that underlie my empirical designs. While DAGs can be controversial (Heckman & Pinto, 2015, 2024; Imbens, 2020), and I am aware that some readers may have preferred that I express my causal identification assumptions using explicit potential outcome notation rather than an equivalent visual representation, my aim from the outset has been to contribute to the debate on the historical legacies of political violence in a transparent and accessible manner. I believe that the use of DAGs facilitates discussions about causal assumptions. If someone does not find the DAG for one of my papers credible, I do not see this as a reproach, but as an opportunity to engage in a clear dialogue in which we can identify the sources of disagreement as transparently and openly as possible. Too often, political science debates have focused not on productive discussions of causal assumptions and their applicability, but on competitions to demonstrate whose results are more 'robust' to often arbitrary sets of covariates (Achen, 2005; Clarke, 2005, 2009).

Finally, I wrote this dissertation fully aware of the challenge of appealing to multiple epistemic communities through mixed-methods research. The experience of each chapter during the publication process reflects this challenge: some reviewers suggested cutting the qualitative sections, while others advised the exact opposite. Nevertheless, I firmly believe that blending quantitative and qualitative inferences is

not only beneficial, but necessary, in order to study the historical legacies of political violence.

1.4 Structure of the Dissertation

This dissertation consists of three main chapters that examine the persistent impact of political violence on political attitudes, followed by a final concluding chapter.

Chapter 2 examines the enduring influence of the First World War on modern French support for the far-right Front National (FN). It argues that high WWI death rates fostered in-group preferences and nationalism, leading to support for the FN. By linking higher WWI death rates to FN vote share, the analysis shows that historical trauma shapes political orientations by decreasing support for multiculturalism and increasing nationalism. Individual-level survey data confirm this pattern, highlighting how historical events fuel exclusionary in-group preferences.

Chapter 3 examines dissent under authoritarian regimes, focusing on Romania's Gulag system to show that repression can foster anti-regime norms that compel collective opposition. An analysis of localities that hosted Gulag facilities reveals intensified resistance during the 1989 Revolution, illustrating how collective memories of repression drive dissent beyond strategic calculations. I then provide evidence of how emerging social norms, as opposed to changes in opportunity structure alone, are responsible for this dynamic.

Chapter 4 analyses the impact of authoritarian repression on climate policy demand, using the East German context to show how repression dismantled pro-environmental norms. Comparing districts on both sides of the former inner-German border, the study finds a negative effect of repression on the demand for ambitious climate action, holding the supply of policy options constant. Using data on the distribution of Stasi agents, I then show that this effect can be observed even within East Germany: more "repressed" regions are less environmentally ambitious. I show that these effects are largely independent of other political conflicts.

Chapter 5 synthesises the empirical findings, placing them in a broader context and linking them to current research on the historical legacies of political violence. Building on these findings, I theorise

future directions for the field, suggesting that the persistence of political violence may extend beyond traditional political attitudes, potentially influencing areas such as collective identity formation and societal polarisation.

2

Echoes from the trenches: World War I and contemporary support for the far-right in France

"The war has ruined us for everything." — Erich Maria Remarque, All Quiet on the Western Front

2.1 Introduction

Support for the far-right is closely linked to attitudes derived from in-group favouritism among the electorate, usually in the form of nativism and exclusionary nationalism (Golder, 2016; Mudde, 2019). However, how this in-group favouritism evolves over time in the face of external shocks, and more specifically whether it is endogenous to some historically entrenched processes and events, remains a largely unexplored topic in political science. To fill this gap, I examine how the shared experience of war, one of the most powerful shocks a human society can experience, has lasting effects on the level of exclusionary nationalism exhibited by a community, and how such forms of nationalism translate electorally into support for far-right political parties.

Empirically, I exploit a recently published dataset on the casualties of the French army during World War I (WWI) (Gay & Grosjean, 2023). Between 1914 and 1919, 8.3 million citizens joined the military

to fight against the Central Powers led by Imperial Germany, of whom a staggering 1.3 million were killed in action, 16% of the total conscript population and about 4% of the total pre-war population. By leveraging variation in department-level death rates, I show that French departments with higher death rates exhibit, in the long run, higher levels of support for the Front National (FN)¹, the main contemporary French far-right party. Drawing on robust evidence from psychology and sociology, I theorise that this association persists because the communities exposed to higher WWI death rates develop a strong in-group preference that manifests itself in the form of a higher demand for nationalism and a lower demand for multiculturalism and internationalism, precisely the ideological positions supplied predominantly by the FN.

These findings contribute to several strands of literature. First, I add to the growing body of work showing how the experience of war fosters nationalism. Up to this point, most of this work has focused on the short-term effects of war and wartime violence (Acemoglu, De Feo, De Luca, & Russo, 2022; Cagé, Dagorret, Grosjean, & Jha, 2023; De Cesari & Kaya, 2019; De Juan, Haass, Koos, Riaz, & Tichelbaecker, 2024; Koenig, 2023). This paper suggests that if the right mnemonic infrastructure is in place to maintain and reproduce the memory of war, and if political actors exist that are motivated to exploit this infrastructure, then the effects can persist over a very long period of time. In particular, this paper tests a very similar argument to that proposed by Cagé et al. (2023) and De Juan et al. (2024) on the short-term effects of WWI on electoral politics in France and Germany, but also adds a robust theoretical mechanism explaining why and under what conditions such effects may persist.

Second, I build on the literature characterising the long-term effects of war and, more broadly, of large-scale political violence (Costalli & Ruggeri, 2019; Rodon & Tormos, 2023; Rozenas, Schutte, & Zhukov, 2017; Villamil, 2021). A gap in this literature is the very strong focus, often due to data availability, on modern and contemporary civil wars. However, this form of violence tends to be confined to specific geographical areas, making the findings of these papers less generalisable. In contrast, this paper explores WWI as its setting, one of the most important interstate conflagrations in history, to demonstrate similar patterns of behavioural change as theorised by some scholars (Cunningham & Lemke, 2013), thus

¹The party is now, since 2018, officially known as *Rassemblement national*, the National Rally.

complementing existing studies discussing the role of intrastate violence. One paper that already looks at the effect of historical state actions in the early 20^{th} on long-term attitudes and behaviour in contemporary France is Gehring (2021); however, in that case, the author focuses less on extreme structural violence, such as the one experienced by communities during wartime, and more on the comparative institutions that can either contain or produce negative actions.

Finally, I contribute to the emerging strand of empirical work discussing the impact of WWI on future developments across Europe, and in particular in France. To the best of my knowledge, this is the first contribution in political science that attempts to understand the central role of WWI in the politicoeconomic trajectory of the French population, similar to work that exists in other European contexts (Acemoglu et al., 2022; De Cesari & Kaya, 2019; De Juan et al., 2024; Koenig, 2023). In doing so, I complement work in economics, such as Gay (2023), which demonstrates the long-term impact of the Great War on the French labour market, in particular its gender dimension.

2.2 The politics of memory and electoral behaviour: a theoretical framework

2.2.1 Shared war experiences and the formation of in-group/out-group preferences

The impact of political violence on societal beliefs, attitudes and behaviour is a well-established area of research in social sciences (Acharya et al., 2016; Rozenas et al., 2017; Walden & Zhukov, 2020; Zhukov & Talibova, 2018). An extreme form of political violence, war, is known to deeply scar communities by inflicting costly physical destruction, altering the demographic composition of the population and, above all, producing a grim legacy of wasted human life (Besley & Reynal-Querol, 2014; Gay, 2023; Hadzic, 2018).

One pattern established in the literature is that a shared experience of war can heighten social capital within a community, increasing cooperative and altruistic behaviour (Bauer et al., 2016). However, such

pro-social behaviour is often confined to one's own in-group, often defined in ethnic, racial or national terms. War renders individuals more attuned to the fragility of their own experience as a community, making them willing to protect the survival of that particular experience at the expense of others (De Juan et al., 2024). This is corroborated by experimental research in psychology, which shows that experiencing the death of a loved one increases one's sense of belonging to a broader identification structure, such as kin or nation (Bauer, Cassar, Chytilová, & Henrich, 2014). In societies where such a structure of identification extends beyond consanguinity, personal loss is not even a necessary condition for triggering heightened identification, as the *indirect victimisation* produced by learning the stories of others in the same community is sufficient. In fact, recent empirical studies have shown that in the presence of extreme violence, acting as a bystander is sufficient to trigger persistent behavioural chances (De Juan, Gläßel, Haass, & Scharpf, 2023).

As mentioned before, most often, an increase in in-group identification comes at the expense of a decrease in out-group affinity (Bauer et al., 2014; Canetti, Hall, Rapaport, & Wayne, 2013). There are several reasons why this might happen in the case of war. First, out-group members may be perceived as having a wholly different experience of war, shaped by alternative sources of communal trauma which, in an environment characterised by limited collective memory resources, would compete directly for salience with the experience of in-group members. Second, the experience of the out-group may be antithetical to that of the in-group and, although both are subjective in nature, could lead to members of the out-group being perceived as a threat to the validity of the in-group experience. Third, in more extreme cases, the out-group could be blamed for the trauma that produced the in-group experience if the two groups were on opposite sides of the conflict.

Then, how does this in-group preference manifest itself in terms of political behaviour? In democracies, the most obvious form of potential manifestation is through voting, by supporting political parties whose message is similarly favourable to the in-group at the expense of the out-group. An obvious candidate for tracing the political effects of in-group preferences would therefore be the far-right, which throughout modern European history has consistently displayed (ultra)nationalist tendencies, nativism, chauvinism and xenophobia, all elements of extreme support for the national in-group.

Empirical evidence supports this hypothesis, showing that communities most affected by war tend to disproportionately support far-right political parties or otherwise exhibit behavioural patterns congruent with far-right ideology, at least in the short run. Acemoglu et al. (2022) show that variation in the number of casualties inflicted on the Italian army during WWI was a major factor in the rise of fascism in some regions of the country. Similarly, Koenig (2023) shows that German regions with more WWI veterans gradually moved to the right of the political spectrum, supporting conservative parties that enabled the erosion of democracy and the rise of the Nazi Party. Providing even more direct evidence of the link between WWI and Nazism, De Juan et al. (2024) show that in the inter-war period, support for nationalist parties was significantly higher in places with above-average wartime human losses. Finally, Cagé et al. (2023) find that French communes that were more strongly represented during the Battle of Verdun, led by WWI hero and future leader of Vichy France, General Philippe Pétain, also had higher levels of Nazi collaboration during the German occupation of World War II.

But how do far-right parties manage to frame the experience of war in terms of national identity at the expense of a more localised imagined collective (e.g. the commune), given that local communities within the same country experience war very differently, but the national experience is homogeneous, or at least reconstructed as homogenous ex-post? Kocher, Lawrence, and Monteiro (2018), building on the historical work of Keith (2013), suggest that the answer lies in the way modern states preserve the memory of war. When societies rely on a compulsory system of public education, children are exposed to a common national curriculum that includes the creation of a single image of collective trauma, thus producing the national in-group². However, inter-community local differences in the remembered and reconstructed wartime experience are not erased by the school system, but become sources of variation in the perceived salience of the national trauma. In other words, people from all regions of a war-affected country remember the conflict, but the strength of the memory is shaped by regional losses. In the case of France and WWI, following this logic, one would expect that French departments exposed to more wartime losses are more likely to have a strong attachment to the nationally defined in-group. This varia-

²This is not to say that the far-right manipulates the education system in order to weaponise the memory of the war, but that it indirectly benefits from the fact that the curriculum construes a shared experience at the national level, which reinforces the French people as the in-group towards which preferences are generated.

tion in the intensity of remembrance is also likely to persist because the practice of memory is endogenous to local institutions: political elites from departments more affected by the war are more likely to push for more stringent memorialisation and commemoration, thereby reinforcing the strength of the local narrative and thus the local preference for the in-group.

2.2.2 The lingering effects of shared war experiences

The remaining question is whether the in-group favouritism triggered by the shared experience of war within a community, which seems to translate into support for the far-right, persists over time, or whether the shock is transitory. At a more general level, the idea that significant historical conditions have long-term effects has gained traction in recent decades (Cirone & Pepinsky, 2022), but there is limited empirical evidence that wars constitute such a critical juncture, most of it based on episodes of civil war rather than interstate violence. There is no a priori reason to believe that interstate violence would necessarily deviate from the patterns established in civil wars, but in the absence of explicit evidence we are faced with a gap, and need to rely on the literature on the persistent effects of civil wars.

In this sense, Costalli and Ruggeri (2019) argue that civil unrest changed the electoral geography of Italy by 1960, increasing the organisational capacity of the Italian Communist Party in the very same regions where Communists had fought against Nazi and Fascist forces during WWII. Drawing on the Spanish Civil War, Villamil (2021) shows that regions where clandestine leftist networks operated during the war and were exposed to state-sponsored atrocities show higher levels of support for leftist parties in the long term. Finally, Rozenas et al. (2017), who examine the long-term effects of Stalinist deportations in 1930s Ukraine, show that communities exposed to such atrocities are systematically less likely to support pro-Russian parties.

Theoretically, for war to be a critical juncture in the evolution of a society's political equilibria, two necessary conditions must be met. First, there must be (at least) a causal mechanism, embedded in the socio-political context, through which the lasting effect of the war propagates. Second, there must be a political actor who becomes a mnemonic entrepreneur, willing to exploit the memory of war to activate the latent in-group preferences for electoral purposes. More specifically, we would need to identify
an entity that directly benefits from the association between in-group preferences and nationalism, most likely by emphasising the latter as part of its political strategy. When both elements are present, the mechanism of persistence becomes clear: an interested political actor, who is a supplier of nationalism within the political system, leverages the infrastructure of collective memory to activate the community's latent preference for in-group members, which translates into an increased demand for nationalism.

The first consideration, then, is what is the infrastructure through which the memory of war is propagated. A large body of work in sociology, anthropology, and international relations explores precisely the politics of memory - how societies maintain, reproduce and instrumentalise shared experiences, such as wars - and their consequences (Malinova, 2021; Maurantonio, 2014; Müller, 2002; Uhl & Golsan, 2006). The politics of memory are the central link between war as a lived experience and war as a source of enduring nationalistic sentiments.

The starting point for making this connection is to recognise that nationalism, both as an ideology and as a form of political praxis, is rooted in the development of historical representations of shared narratives (Bell, 2003). But not all historical narratives are created equal. Central to the formation of the most enduring narratives of nationalism are episodes of collective trauma that trigger a social response of sufficient magnitude to construct and reinforce a common sense of belonging (Bell, 2006; Edkins, 2003). Wars, especially the World Wars, are the epitome of such trauma, and there is empirical evidence of a link between wars, including WWI, and displays of nationalism (Cagé et al., 2023; De Juan et al., 2024).

This sense of nationalism, built on the foundations of shared historical experiences such as war, is reinforced and reproduced through the practice of memory. This practice is especially pronounced in Western democracies in relation to large-scale events such as the World Wars. To avoid repeating the mass violence that marked their history in the first half of the 20th century, most European countries created multiple channels through which citizens could learn, mourn and remember what did not need to be repeated. Remembrance has become an integral part of the social fabric of politics, an essential cog in the nation-building machine (Turner, 2006). This phenomenon is particularly pronounced in France, where it has been at work since the interwar period (Sherman, 1999). Visual reminders, including monuments, statues and cemeteries, are found throughout these countries and there are very few communities without

any of them (Borghi, 2021), ensuring access to political memory and, from the perspective of memory entrepreneurs, access to sources of identity (re)formation. Complementing these visual queues, school curricula try to explain the meaning of the World Wars and the connection between them in order to give children a sense of French historical completeness, a foundation of nationalism (Shapiro, 1997).

The politics of memory provide a plausible infrastructure for how the memory of war could have proliferated in direct relation to nationalist attitudes. The next consideration is which actors would be interested in exploiting this infrastructure. Theoretically, we would need to identify an agent who would win from this action, and who has the organisational power to perform it. The latter condition is more important because it limits the class of actors to be analysed to those most relevant in the political system, which for the time being remain the political parties. In fact, many studies show that political parties and their leaders are among the most important mnemonic actors, as they tend to take on the role of memory entrepreneurs whenever political remembrance suits them (Korycki, 2023). Among them, far-right political parties, such as the FN in France, mobilise precisely that part of war trauma that resonates with their target electorate, constructing an out-group by *othering* another country, a domestic political elite, an ethnic group or immigrants from a distant nation (Couperus, Rensmann, & Tortola, 2023; Couperus, Tortola, & Rensmann, 2022; Soffer, 2022; Zavatti, 2021). In this sense, French patriotism and the homogeneity of national identity, to which the far-right has appealed over the years, are closely linked to the wartime mobilisation of WWI (Purseigle, 2023; L. V. Smith, Audoin-Rouzeau, & Becker, 2003). This is because nationalism as an all-encompassing category, sometimes in exclusionary forms, was consolidated in France during this period (Fuller, 2014; Tombs, 2003).

In addition, the losses of WWI could be perceived as the French state sending its citizens to a certain death, creating an environment of mistrust. Institutional mistrust is one of the most persistent narratives in societies, passed on from generation to generation (Becker, Boeckh, Hainz, & Woessmann, 2016; N. Lupu & Peisakhin, 2017). In the long run, accumulated mistrust benefits the far-right (Berning & Ziller, 2017), which is able to portray itself as creating a rupture with the status quo and the remaining institutions against which people are hostile, weaponizing not only the memory of war but the cynicism



Figure 2.1: Visual summary of the theoretical argument

towards formal institutions that this memory engenders³.

We therefore have both the infrastructure and the political actors who could use it. On the basis of these theoretical considerations, as applied to the case of France post-WWI, I delineate the following argument. The deaths experienced by local communities during WWI increased in-group preferences which, in the case of France, manifest themselves as preferences for the French nation, i.e. nationalism. Moreover, because different death rates act as triggers of different intensity, they produce shifts in ingroup preferences of different magnitude and hence differential demand for nationalism. Since far-right parties are the main suppliers of political nationalism, both in terms of discourse and policy, I expect that the demand for nationalism will in turn increase the electoral appeal of such political forces. Given that since the interwar period, far-right movements and parties have instrumentalised the memory of the war to consolidate their position and justify their (ultra)nationalist positions (Cagé et al., 2023), I hypothesise that support for the far-right is persistently higher in French departments where the death rate during WWI was higher. As the main far-right party in France, I expect the main beneficiary of this effect to be the FN⁴. A visual summary of the argument can be seen below, in Figure 2.1

The argument is not that economic factors do not shape patterns of support for the far-right in France, or even to deny that they may be a central cause. Recent literature has shown that economic and

³This is not to say that the far-right is the only actor capable of mobilising the infrastructure of collective memory. For example, progressive parties could try to showcase the sacrifices of French people of African origin in combat in order to create a more inclusive perception of Frenchness. For reasons of space, I leave such alternative mnemonic actors and the legacies they might exploit to further research and concentrate on the far-right and nationalism.

⁴This does not mean that other far-right organisations haven't or don't benefit from weaponising the memory of the Great War, but the FN is an interesting case, given its current position as a mainstream political party. Following the logic of Gerring (2007), the FN can be considered a pathway case for the study of the legacy of WWI on voting for the far-right.

cultural factors work in concert to favour the far-right when the circumstances are right for this type of actor (Margalit, 2019; Rodrik, 2021; Scheiring, Serrano-Alarcón, Moise, McNamara, & Stuckler, 2024). Moreover, it could also be the case that the deaths of WWI shaped the deindustrialization trajectory of the French departments, further reinforcing the effect of weaponizing the memory of loss. This is especially likely given the strong correlation between death rates and the industrial profile of each French departments (Gay & Grosjean, 2023), as well as the long-term effect of WWI on the French labour market (Gay, 2023). My argument does not contradict claims of such additional reinforcing mechanisms; the more modest claim is that deaths during WWwe are a contributing, persistent factor in inducing support for the FN⁵. Future work should consider whether far-right political parties, which weaponise deeply rooted community norms about culture, are more successful in their efforts where there are appropriate economic circumstances.

2.3 Research design

2.3.1 Data

My main independent variable is the department-level death rate in WWI, calculated as the ratio between the number of deceased French soldiers born in a department and that department's total population according to the 1911 census, the last before the start of war. I get these from the *Morts Pour la France* database, compiled by Gay and Grosjean (2023), which is based on over 1.5 million individual files from the archives of the Bureau of Archives of Victims of Contemporary Conflicts. The average departmentlevel death rate in France during WWI was 3.89% of the population, with a standard deviation of 0.7. The department with the highest death rate was in Lozère, with 5.53%, while the department with the lowest death rate was Hauts-de-Seine with 1.25%. Alternatively, I also calculate the median death-rate in each department as a function of the number of deaths in French communes, in order to deal with the risk of outliers. The spatial distribution of the independent variable is shown in Figure 2.2. In the

⁵In other words, while cultural practices may be a sufficient condition for the success of the far-right, specific economic circumstances may be a necessary condition. Ultimately, this paper serves as an 'effects-of-causes' rather than an all-encompassing 'causes-of-effects' study (Mahoney & Goertz, 2006).



Figure 2.2: Map of WWI death rates across French municipalities

Appendix, Figure B1 shows histograms of the distribution of death rates across the French departments, demonstrating that a) there is significant spatial variation that is not visibly clustered, and b) the two operationalisations of the independent variable exploit different variations and are thus complementary⁶.

In terms of dependent variables, I leverage various measures of contemporary support for the farright in France. For the main analysis, I measure the share of votes received by FN in national legislative elections since 1990⁷. I use data from the European NUTS-level dataset (EU-NED), which allows for a smooth matching based on the spatial characteristics of French departments (Schraff, Vergioglou, & Demirci, 2023)⁸. In terms of scope conditions, I only look at post-1990 elections for three reasons⁹.

⁶As a placebo test, I show my independent variable does not affect support for right-wing parties before WWI (Table B2). ⁷Table A9 shows the results are robust when including election results prior to 1990.

⁸I have chosen to look at legislative elections because, in the case of the presidential elections in France, strategic considerations, often directly related to the position of the FN, have made voting for the FN candidate less of a valid measure of revealed electoral preference, or at least make it difficult to disentangle attitudes from strategy. In addition, data on presidential elections are less accessible at a similarly granular level. However, in order not to overlook this dimension completely, I also use survey data to look at support for Marine Le Penn in national elections.

⁹In the Appendix, I also show results before and after 2010, in Table A8. The results, stronger for earlier years, align with my theoretical argument, as one would expect historical legacies to slowly fade over time, yet not abruptly.

First, while the FN's consolidation began with the 1986 legislative elections (DeClair, 1999; Shields, 2007), their remarkable results were due to an atypical electoral system for France, proportional representation, which had been implemented by François Mitterrand to cushion the expected defeat of his party (Knapp, 1987). To understand the long-term electoral fortunes of the contemporary French far-right, it is necessary to exclude this particular outlier from the analysis. Second, French politics, traditionally more dominated by far-left parties than other West European party systems, changed markedly with the fall of the USSR (Bull, 1995). Looking for patterns of far-right support after 1990 thus allows to disentangle the complex effects of the Cold War as an external driver of political behaviour and to focus on domestic factors. Third, this period corresponds to a more modern version of the French party system, which is of primary interest in deciphering contemporary political events (Bornschier & Lachat, 2009). In the Appendix, I show that using all electoral results from 1986 onwards does not affect the results.

To avoid any ecological fallacy, I also examine support for the FN at the individual level. For this, I use the *Sub-national Context and Radical Right Support in Europe* (SCORE) survey (Evans & Ivaldi, 2021), which provides a nationally representative sample of 19,454 French respondents and a series of relevant questions, such as (1) whether they voted for Marine Le Pen in the first round of the 2017 presidential election, or (2) how they rate the FN on a Likert scale.

Finally, to verify my main argument on the mechanisms of support for the far-right, I construct additional dependent variables that capture not only the shift in revealed electoral preferences (i.e., explicit support for the FN), but also the electoral distribution around certain salient conflicts of the multidimensional French political space. Formally, $\mathbf{X} = \{X_1, X_2, X_3, ..., X_N\}$ is the set of political parties participating in each given election in France. Each party $X_K \in \mathbf{X}$ has an ideological position $I_{K,C}$ for every political conflict C (e.g., taxation, nationalism, agriculture, etc.). The positions are measured before the election, and subsequently we measure the electoral performance of all parties through the vote share π_K that they have obtained in the first election after revealing their ideological position $I_{K,C}$. With this information, I compute E_C , the *political conflict equilibrium* over each conflict C:

$$E_C = \sum_{K=1}^{N} I_{K,C} \times \pi_K \tag{2.1}$$

The political conflict equilibrium¹⁰, as a measure, satisfies a number of intuitive conditions that one would expect from any metric of the central tendency of electoral preferences. First, it aggregates individual party positions based on their electoral performance. This means that it takes into account the preferences of the majority of the electorate. If a party with a particular position $I_{K,C}$ wins a significant share of the vote, that position will have a greater impact on E_C . However, smaller parties with extreme positions won't disproportionately affect the equilibrium unless they win a significant share of the vote π_K . In other words, it summarises the average preference of the voters for the different ideological positions supplied by the parties.

Second, the political conflict equilibrium varies as parties' ideological positions or electoral performance change from election to election. This is important because it allows the measure to adapt and reflect changing political landscapes or evolving voter preferences over time. Furthermore, E_C varies not only across elections but also across space, as different electoral constituencies produce different weights for the ideological positions of political parties due to the different vote shares obtained by the parties across electoral constituencies, making this measure also suitable for sub-national analysis, like in the case of this paper.

Third, E_C does not inherently favour party systems with more or fewer parties, important in the case of the ever-changing French system. While the number of terms in the sum depends on the number of parties N, the weight of each term is determined by the party's vote share π_{X_i} . This further ensures that E_C can be used to compare political equilibria over time, regardless of the exact number of parties positioning themselves on different political conflicts.

While the main dependent variable used in this paper captures explicit support for the far-right, the political conflict equilibria provide corroborating evidence for my theory, as they measure whether the entire political space has gravitated towards the positions endorsed by the far-right, and whether these changes are (partly) driven by WWI. I calculate political conflict equilibria for conflicts relevant to far-right parties: multiculturalism, nationalism, traditionalism, internationalism, Europeanism, and the uni-dimensional right-left index. In terms of data, I once again rely on EU-NED for department-level vote

¹⁰I use the term equilibrium because after the election, the citizenry has no incentive to deviate until new ideological positions have been announced.

shares obtained by French political parties, and on the Manifesto Project dataset (Lehmann et al., 2022) for measures of party positions on the issues of interest. Following established trends in the text-as-data literature, I use the log transformation proposed by Lowe, Benoit, Mikhaylov, and Laver (2011) to scale political preferences from coded political texts.

2.3.2 Model specification and causal identification

I estimate the following regression using ordinary least squares (OLS):

$$Y_{t,d} = \text{year}_t + \text{grid-cell}_d + \gamma \cdot \text{death}_{\text{rate}_d} + \phi' \cdot \boldsymbol{G_d} + \rho \cdot \boldsymbol{X} + \epsilon_{t,d}, \quad (2.2)$$

 $Y_{t,d}$ is the vote share obtained by the FN in department d during election t; year_t and grid-cell_s are year-fixed and grid-cell-fixed effects, respectively, which absorb the impact of time-invariant and regioninvariant features of departments; $\phi' G_d$ describes a set of spatial covariates, including the degree of urbanization of a department, as well as whether the department is predominantly coastal or mountainous; \boldsymbol{X} are the respective Moran eigenvectors of the grid; the main coefficient of interest is γ , which measures the difference in vote share between departments induced by differential WWI death rates; $\epsilon_{t,d}$ is the error term.

The necessary condition for γ to represent a causal effect is that WWI death-rates are not determined by factors that also determine contemporary support for the FN¹¹. In the language of natural experiments, the "assignment mechanism" of the WWI death rates needs to be as-if-random, which would ensure that the departments are, in expectation, homogeneous in terms of pre-WWI characteristics

Fortunately, the question of what factors best explain the differences in rates between departments is not a new one for historians. There is a large body of literature on how the French army conducted its military operations between 1914 and 1919 and on the factors that determined battlefield mortality (Greenhalgh, 2014; Porch, 1988; Prete, 1985). In a nutshell, death rates during that period were a function of the shifting territorial priorities of the war effort, which can be broken down into three elements: recruitment patterns, troop deployment and battle strategy (De Juan et al., 2024). The latter is plausibly

¹¹If this necessary condition is satisfied, γ represents the average treatment effect (ATE) of WWI death rates.

unrelated to the political preferences of the citizens of the French departments, so only the first two must be examined.

In the beginning, citizens recruited from the same department were usually sent to the same front, ensuring a fairly homogeneous distribution of deaths as combat situations became more dispersed. As the war of attrition progressed, people from all corners of France were sent precisely when and where they were most needed, regardless of their cohort, socio-demographic characteristics or department of origin. From that point on, death-rate heterogeneity was determined by how the French authorities decided to respond to the industrial needs of their war machine, to the disruptions caused by the German occupation of the north-east, and to the shortages of artillery weapons in the early stages of the conflict (Bostrom, 2016). Naturally, the more urban departments, where citizens were equipped with skills more suited to industrial work, became the primary sources for compensating for production shortfalls. Accordingly, as demonstrated by Gay and Grosjean (2023), the strongest predictor of death rates among the population of a department is the degree of urbanisation explains over 75% of the variation in death rates between French departments, and once urbanisation is taken into account, other socio-demographic or geographical characteristics do not significantly improve the predictive power¹².

Thus, while mortality is not distributed as-if-randomly, once the effect of urbanisation is taken into account, we could treat the distribution of World War I mortality rates as a quasi-natural experiment (Di-Nardo, 2010). However, as the relationship I am investigating occurs in the long run, with several other events taking place, including another World War, we need to be wary of spatial noise that might capture alternative causal channels (Kelly, 2020). I carry out the adjustment protocol that alleviates concerns over alternative explanations in two steps.

First, I control for the degree of urbanization G_d of each department using spatial data on the French population density. In addition, to improve robustness and decrease uncertainty in the estimates, I include Moran eigenvectors X in the regression model; including these eigenvectors is a statistical tech-

¹²This is crucial because it implies that pre-WWI political and demographic differences between departments cannot explain the main independent variable; even if they are related to the dependent variable, they are not confounders by construction and their inclusion in the regression is not necessary (Cinelli et al., 2022).

nique that explicitly incorporates any spatial patterns predictive of the outcome into the regression model, thus avoiding the estimation being sensitive to spatial noise (Dray, Legendre, & Peres-Neto, 2006; Griffith & Peres-Neto, 2006). Thus, even if the effect of post-WWI events that took place in France is correlated with the effect of WWI on electoral options, the Moran eigenvectors would remove most of these associations, leaving only the direct effect of interest. Then, I include polynomials in the latitude and longitude of the centroid of each department, which deal with potential spatial gradients. As suggested by Kelly (2020), I control at least for 2^{nd} degree polynomials, with and without interaction terms between coordinates, but not any higher than the 3^{rd} degree to avoid over-fitting (Gelman & Imbens, 2019).

I do not control for economic characteristics of the departments after WWI that might explain support for the FN, as these are post-treatment variables that potentially capture mechanisms through which WWI death rates induce patterns of support for the far-right (Rosenbaum, 1984). In this regard, I follow the advice of methodologists to avoid over-controlling just for the sake of it (Achen, 2005; Clarke, 2005, 2009) and instead follow a theorized data-generating process.

Second, I reweight the observations in my dataset based on their degree of urbanisation using entropy balancing (Hainmueller, 2012). Entropy balancing is an algorithm that calibrates the unit weights for the control group so that the specified sample moments of the covariate distribution (e.g. mean, variance, skewness) of the reweighted control group approximate those of the treatment group (Watson & Elliot, 2016). This algorithm is doubly robust to linear regression, meaning that the causal effect of WWI death rates on contemporary support for the far-right is well identified if either one of the matching or regression models I use is correctly specified, even if the other is not (Zhao & Percival, 2016).

To further reduce the threat of selection bias by restricting identifying variation to a more plausible subset of within-period and within-region variation, I add two sets of fixed effects (Mummolo & Peterson, 2018). First, year-fixed effects, which I include because of election-specific political dynamics that should not be pooled during estimation. Second, grid-cell fixed effects, which, following the approach of Doucette (2024), divide observations into spatial units along latitude and longitude and capture unobserved confounding related to the natural distribution of French departments in physical space¹³. I avoid

¹³Grid-cell fixed effects divide the French map into dozens of rectangles of equal size, capturing all the structural elements specific to each region that have remained stable over time, including geography and baseline population characteristics.



Figure 2.3: Directed acyclic graph (DAG) representing the causal model underlying our theoretical argument.

including administrative unit fixed effects, such as those based on NUTS2 units, because these were created after WWI based on decisions that are credibly related to the post-war economic situation, and could lead to over-control for post-treatment variables, a major threat in historical political economy (Homola, Pereira, & Tavits, 2024; Pepinsky, Goodman, & Ziller, 2024a, 2024b). In the Appendix, I show nevertheless that replacing grid-cell fixed effects with NUTS2 regional effects does not alter the results. The full causal model underlying my model specification is shown in Figure 2.3 in the form of a directed acyclic graph (DAG).

I report Conley standard errors, which are heteroskedastic and two-dimensional autocorrelation consistent (Conley & Kelly, 2024; Conley & Molinari, 2007)¹⁴.

¹⁴Since my sample contains information on all 93 French departments, with WWI affecting them all, as well as with deaths during WWI not occurring at the department level, I do not cluster the standard errors when aiming to recover average treatment effects (Abadie, Athey, Imbens, & Wooldridge, 2022). However, per Conley and Kelly (2024), my approach to inference

Results and discussion 2.4

2.4.1 Main analysis

Table 2.1 reports the results of estimating Equation 2.2. Columns (1)-(4) use average WWI death rates at the department-level as the independent variable, while (5)-(8) use median death rates as a robustness check, as they are less susceptible to outlier communes within a department. The estimated effects are statistically significant at conventional levels, with the two-sided p < 0.05 (and, with one exception, p < 0.01), providing strong evidence to reject the null hypothesis that there is no effect of WWI death rates on support for the FN. Drawing on Columns (1)-(8), I find that an increase of 1 percentage point in WWI death rate in a French department predicts, on average, an increase of between 0.013 and 0.017 in the vote share of the FN¹⁵. Since FN's average share in the period after 1990 was 11%, the difference caused by WWI represents 11.8-15.4% of this figure, making the estimates not only statistically significant, but also substantial in terms of magnitude¹⁶.

	DV: Vote share for <i>FN</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	IV: Mean death-rate			IV: Median death-rate				
ATE	0.013*** (0.005)	0.013*** (0.004)	0.017*** (0.005)	0.019*** (0.004)	0.013*** (0.005)	0.010** (0.004)	0.017*** (0.006)	0.015*** (0.004)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.
R^2	0.876	0.877	0.878	0.879	0.875	0.876	0.877	0.878
N.	546	546	546	546	546	546	546	546
Notes: Conley standard errors in	brackets.							

Table 2.1: Effect of WWI death-rate on support for FN (1993-2017)

 $p^{***} p < 0.01; p^{**} p < 0.05; p^{*} < 0.1$

To check the likelihood that spatial noise is driving the effect, I compute Moran's I statistic, which

is already significantly more conservative.

¹⁵To put the results in perspective, I compute the Shannon information value, a log-transformation of the p-value, which reduces the interpretation of uncertainty to an intuitive game of chance: getting the same ATEs as in models (1)-(4) by chance would be as surprising as getting 11 to 18 heads on a fair coin (Cole, Edwards, & Greenland, 2021). Even in models (5)-(8), getting the same results by chance would be as surprising as getting 6 to 9 straight heads on a fair coin.

¹⁶The results remain positive when analysing the impact of death-rates from the mean WWI death rate on support for the FN across different election years, but the analysis is largely underpowered in these cases.

measures the degree of spatial autocorrelation in the regression residuals (Conley & Kelly, 2024). All eight values are close to zero, indicating that significant threats to validity from spatial autocorrelation have been addressed and that the results can be meaningfully interpreted. I also compute Geary's C statistic, which is more sensitive to local spatial autocorrelation. For all model specifications, its value is close to 1, indicating randomness in the distribution of observations.

To test whether these results are driven by unobservable characteristics of the French departments before WWI, such as differential political preferences, I conduct several sensitivity analyses that explicitly ask how much unobservable characteristics should matter relative to observables in order to cancel out the effect (Cinelli & Hazlett, 2020; Oster, 2019). If results are not sensitive, then even if such differences exist, they would not affect my estimated model. Following Oster (2019), I show that the influence of unobservables would have to be 84-238% of that of observables to explain away the ATE, which is unlikely given the very high levels of R^2 in Columns (1)-(8) of Table 2.1. Similarly, following the approach developed by Cinelli and Hazlett (2020), I show that unobservables would have to explain 23.1-25.4% of the residual variance of both the independent and dependent variables, which is again logically possible but highly unlikely.

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(0)		
			(-)	())	(0)	(/)	(8)		
	IV: Mean death-rate				IV: Median death-rate				
Rural X Death-rates0.040*(0.013)	** 0.046*** (0.015)	0.034*** (0.011)	0.035** (0.014)	0.041*** (0.011)	0.044*** (0.012)	0.035*** (0.009)	0.039*** (0.013)		
Year FEs Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial 2 nd	$2^{\rm nd}$ with int.	$3^{\rm rd}$	3^{rd} with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	3^{rd} with int.		
R^2 0.876	0.877	0.878	0.879	0.875	0.876	0.877	0.878		
N. 546	546	546	546	546	546	546	546		

Table 2.2: Heterogeneous effects of WWI death-rate on support for FN (1993-2017)

As a next step, I provide evidence of effect heterogeneity, i.e. the impact of WWI death rates on support for the far-right is not uniformly distributed across French departments. Based on my theoretical argument, one would expect that communities where the politics of memory are a more active part of

social life would also be more affected by the FN's politicisation of the practice of remembrance. While there are many potential sources of variation in how the politics of memory are enacted, one obvious candidate is the degree of urbanisation of a department. First, less urbanised departments tend to have a better grasp of local developments, as well as a better range of community organisations, including churches, that curate and cultivate the memory of loss. Second, people in less urbanised departments are more likely to have a strong attachment to family and kinship, and thus to react more strongly to losses that have occurred in their vicinity. Both points are aligned with my theoretical argument, and, hence, if my argument if correct, we should observe a stronger effect of WWI death rates in less urbanized, and therefore rural, communities. Formally, therefore, I test whether (lack of) urbanisation is a moderating variable by interacting the main independent variable with a categorical variable that precisely measures the degree of urbanisation in a region. Higher values of the *rural* variable indicate a lower degree of urbanisation in that department.

Table 2.2 reports the results of this estimation. For both mean and median death-rates, the interaction term is positive and statistically different from zero at conventional confidence levels. This means that not only do higher death rates in WWI increase support for the far-right in France, but that the degree of support increases monotonically with the lack of urbanisation. Returning to theory, this finding could indicate that departments where memory is more thoroughly preserved are more affected by the lingering effects of such memories.

Next, I conduct the individual-level analysis using panel data from the SCORE survey. I regress two dependent variables measuring support for the far-right on the WWI death rate in a respondent's department, as well as a set of socio-demographic and regional context control variables. Table 2.3 reports the estimates. Columns (1)-(4) report results on whether the death rate increases self-reported support for the FN, while Columns (5)-(8) report results on whether the same independent variable predicts and increases the likelihood that citizens will vote for Marine Le Pen, the leader of the FN, in the 2017 presidential election. For both dependent variables, the estimates are positive and statistically significant at p <0.01. The effect magnitude is robust to the inclusion of different sets of covariates ¹⁷.

¹⁷The effect is slightly higher for older people, in line with my theoretical argument, because younger generations are less likely to actively carry the memory of loss (Table A10).

	DV: Support for the FN				DV: Vote for Marine Le Pen in 2017			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean death-rate	0.117*** (0.030)	0.117*** (0.030)	0.101*** (0.029)	0.101*** (0.029)	0.013*** (0.004)	0.033*** (0.007)	0.028*** (0.007)	0.028*** (0.007)
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department-level controls ¹⁸	No	Yes	No	Yes	No	Yes	No	Yes
Individual-level controls ¹⁹	Yes	No	Yes	Yes	Yes	No	Yes	Yes
R^2	0.030	0.030	0.086	0.086	0.001	0.009	0.063	0.079
N.	13645	13645	13645	13645	14197	14197	14197	14197

Table 2.3: Individual-level effects of WWI death rates

Notes: Conley standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

Table 2.4: Effects of WWI death-rates on contact with immigrants

	(1)	(2)	(3)	(4)
Mean death-rate	-0.058***	-0.058***	-0.052***	-0.052***
	(0.006)	(0.006)	(0.006)	(0.006)
Region FEs	Yes	Yes	Yes	Yes
Department-level controls	No	Yes	No	Yes
Individual-level controls	Yes	No	Yes	Yes
R^2	0.075	0.075	0.114	0.114
N.	13645	13645	13645	13645

Notes: Conley standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

Table 2.4 then provides individual-level evidence to support my theoretical argument and the proposed mechanism. Using data from the same representative SCOPE survey, I show that French citizens from departments with higher WWI death rates are less likely to interact with non-French immigrants, a finding already established in the literature (Evans & Ivaldi, 2021). The relationship holds even after introducing regional dummies as additional covariates, suggesting that the reason for this positive association is not that some regions are by default more exposed to higher levels of immigration, as the variation used to recover the estimate comes from within each region. Consequently, it is more likely that individuals who are more likely to support in-group preferences because they come from regions that were historically exposed to a more brutal memory of violence during WWI will behave in a manner consistent with theoretical expectations: as in-group preferences translate into nationalistic attitudes, the latter reduce citizens' willingness to interact with people from other nations/cultures/countries, such as

immigrants. In the Appendix I show that WWI death rates are not a good predictor of whether interactions with immigrants, when they occur, are positive or negative (Table B1). This demonstrates that the weaponised memory of war losses reduces the willingness of individuals carrying this memory to engage with an out-group, but once they have done so, possibly due to actions beyond their control such as demographic shifts, their exclusionary preference dissipates.

2.4.2 Evidence from shifting political conflict equilibria

Since the main consideration derived from the theoretical argument is thoroughly supported by empirical evidence, I now proceed to provide evidence that WWI not only altered the final expression of electoral preferences, i.e. the vote for the FN, but also shaped the underlying political equilibria in French society. This also provides indirect evidence for the theorised mechanisms, since if the ideological positions associated with the far-right gain traction as a result of the weaponisation of the memory of WWI, it implies that economic considerations cannot fully explain the causal relationship. First, I re-estimate Equation 2.2 but change the dependent variable to the battery of political equilibria on nationalism, multiculturalism, traditional values, internationalism, and Europeanism. Table 2.5 reports the results of this estimation.

The most direct test of my argument comes from Column (2), that is whether WWI death rates are positively associated with a higher nationalism equilibrium, i.e. whether the point at which social demand for nationalism meets the party system's supply of nationalism is higher in departments that suffered more battlefield losses during WWI. The estimated effect for this channel is positive, substantial and statistically significant at the 5% confidence level. Columns (1) and (4) complete the story of how the memory of the war was propagated in a manner favourable to the French far-right. The deaths in WWI triggered a psychological response in French citizens that led them to have a strong preference for the in-group (the French people). As Column (1) shows, over time this reduced the equilibrium level for multiculturalism, as department where the memory of the war was more activated (i.e. departments with more losses) became more suspicious of "outsiders". This is corroborated by Column (4), as the equilibrium point for internationalism has decreased, suggesting that citizens are less likely to demand

a French presence in multilateral international affairs from the political parties they support, a sign of self-imposed isolation.

	Multiculturalism (1)	Nationalism (2)	Traditional values (3)	Internationalism (4)	Europeanism (5)
Mean death-rate	-0.006*** (0.001)	0.019** (0.009)	0.015 (0.010)	-0.014*** (0.004)	0.007 (0.010)
Year FEs Grid-cell FEs Spatial covariates Pol. degree	Yes Yes 2 nd	Yes Yes Yes 2 nd with int.	Yes Yes Yes 3 rd	Yes Yes Yes 3 rd with int.	Yes Yes Yes 3 rd
R ² N. Notes: Conley stan	0.960 546 dard errors in brack	0.845 546 ets.	0.932 546	0.961 546	0.755 546

Table 2.5: Effect of WWI death-rate on socio-political equilibria

***p < 0.01; **p < 0.05; *p < 0.1

In the historical political economy literature, it is often difficult to come up with meaningful placebos for causal inference because they must share a similar data-generating process with the main dependent variable, apart from the role of the independent variable under study (Eggers, Tuñón, & Dafoe, 2023). In this case, the placebo outcomes are dependent variables traditionally associated with the far-right, but for which there is no reason to believe that there should be a positive effect of higher WWI death rates: Europeanism and Traditional Values²⁰. As the results in Columns (3) and (5) are not statistically significant and change sign with the inclusion of different covariates, I can confirm that my model does not consistently yield a positive effect regardless of what the dependent variable is. This confirms that the reason for the association between deaths in WWIr and contemporary support for the far-right is theoretically grounded and not a random result of data noise.

Of course, a potential endogeneity threat arises from the fact that an electorally strong FN could have shaped these political conflicts. The question then becomes to what extent the political conflict

²⁰Crucially, the argument is not that these political conflicts are not relevant to the French far-right. For example, Europeanism is extremely relevant for the FN, especially France's relationship with the European Union. The results in Table 4 do not invalidate this, but show that the deaths during WWI did not improve the FN's standing by making the French less Europeanist. This is in line with my theoretical argument, which does not mention supranational structures but focuses on nationalism, for which the results are, as expected, positive and significant.

equilibria are the result of such endogenous party dynamics, and to what extent they are caused by pathdependent historical causes. This question is beyond the scope of this paper and would require a model that predicts how well the FN's standing influences other parties, similar to the work of Abou-Chadi and Krause (2020). What the current results show, however, is that the memory of the war has triggered a change in the level of these equilibria. It is very likely that the FN itself, by weaponising this historical loss for the French people, has changed its electoral programme precisely in terms of political conflicts such as nationalism or multiculturalism.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
IV: Mean death-rate					IV: Median death-rate				
ATE	3.147*** (0.848)	3.478*** (1.154)	3.124*** (0.783)	2.359** (1.035)	3.062*** (0.584)	3.157*** (0.784)	2.673*** (0.576)	2.495*** (0.441)	
Year FEs Grid-cell FEs Pol. degree	Yes Yes 2 nd	Yes Yes 2 nd with int.	Yes Yes 3 rd	Yes Yes 3 rd with int.	Yes Yes 2 nd	Yes Yes 2 nd with int.	Yes Yes 3 rd	Yes Yes 3 rd with int.	
$\frac{R^2}{N}.$	$0.817 \\ 546$	0.817 546	$0.817 \\ 546$	0.817 546	0.075 546	0.075 546	0.114 546	0.114 546	
<i>Notes: Conley</i> *** $p < 0.01$;*	standard er $*^{*}p < 0.05;$	rrors in brackets. $p^* < 0.1$							

Table 2.6: Effects of WWI death-rate on RILE equilibrium

The final piece of empirical evidence comes from Table 2.6. If my theory is correct, then departments with higher death-rates would have shifted towards more nationalist tendencies over time, which on the traditional unidimensional right-left (RILE) scale would imply a shift to the right. Columns (1)-(4) provide such empirical evidence by estimating equation 2.2 with the RILE equilibrium level as the dependent variable. Irrespective of how I flexibly control for the geographical coordinates of a department, higher WWI death rates significantly lower the RILE equilibrium point, i.e. they make communities more right-wing. This result adds another link in the causal chain: higher WWI death-rates increase in-group preferences, making people more right-wing, which in turn is reflected in lower community-level equilibria for political conflicts such as multiculturalism and internationalism, and higher for nationalism.

2.4.3 Robustness checks

To verify the structural validity of my theorised causal model, I perform a series of robustness checks on the main results.

First, I show that the main specification of Equation 2.2 produces similar coefficients when removing the spatial covariates and relying only on entropy balancing for adjustment (Table A1). Removing the Moran eigenvectors from the specification preserves the sign of the results, but also significantly increases the degree of uncertainty in the estimates, which is to be expected given the large time lag between the initial historical conditions and the current results (Tables A2-A3). However, the stability of the direction of the results suggests that the effect of WWI death rates is positively associated with support for the far-right, even if one were to dismiss the relationship as non-causal. The results remain positive and significant also when removing the weights produced through entropy balancing.

Second, I use an alternative independent variable, calculated as the residuals of the regression of death rates at the department level on the average death rates of all other departments within the same grid-cell, except for the one in cause. This 'leave one out' approach ensure that if systematic unobservables connect the causes of combat losses and contemporary patterns of electoral support, then they are removed insofar as they are stable within the same area. Crucially, as grid-cells are defined in terms of geographical rather than administrative space, this method should remove threats that have had 'spillover' across the same region of the country, regardless of whether that region coincides with the territorial unit to which a department belongs. The estimated effects, reported in Table A4 are positive, significant at p<0.01 and support the results already established in Table 2.1.

Third, I show that the results are not sensitive to alternative preprocessing algorithms. While entropy balancing is doubly robust to linear regression, the more popular choice for reweighting in political science is propensity score matching. Accordingly, I show in Table A5 that the sign and size of the recovered effects are virtually unaffected when I correct for differences in the level of urbanisation across departments before 1914 using covariate balancing propensity score matching (Imai & Ratkovic, 2014). The same applies to alternative methods of obtaining propensity scores, such as non-parametric covariate balancing propensity scores, such as non-parametric covariate balancing balancing propensity scores.

ancing propensity scores (Fong, Hazlett, & Imai, 2018) and propensity scores derived from generalised linear models (McCaffrey et al., 2013) (Tables A6-A7).

2.5 Discussion and conclusions

Does the experience of war have a lasting effect on the political behaviour of citizens? In this paper I show that the answer is yes, but not unconditionally. While French departments with higher death-rates during WWI do indeed show greater support for the FN and some of its leaders, such as Marine Le Pen, this relationship is not automatic. In order for war to trigger in-group preferences that translate into support for the far-right, two conditions must be met. First, there needs to exist a strong physical and social infrastructure enabling the political remembrance of war memories. Second, there needs to exist a politically-motivated agent that is willing, able, and capable to exploit this infrastructure to achieve some sort of electoral goal. While both conditions are met in the case of post-WWI France, there is no guarantee that these results will generalise to different contexts where the politics of memory are less embedded in social life, or where far-right political parties have less access to sufficient organisational and logistical resources.

Nevertheless, this paper has multiple implications for future research. Existing studies have largely focused on the transitory shocks to political beliefs, attitudes and behaviour induced by war. While the historical persistence literature has been criticised for making broad generalisations based on underdeveloped theory, this paper suggests that, given a sound theoretical framework and sufficient attention to the mechanisms that constitute the causal chain between historical conditions and contemporary outcomes, such studies can prove insightful. Future studies should therefore benefit from discussing, even if only briefly, whether the causal effects they claim to identify are of a short-term nature or whether they propagate over longer periods of time. This distinction is not only interesting, but could also provide readers with the necessary knowledge to understand which events truly represent critical junctures in the development pathways of the society, equipping them with better tools to address the consequences of such events.

I have also shown that, in terms of political equilibria, the experience of war changes only those associated with in-group/out-group preferences. Crucially, although these lead to higher levels of support for the FN, they do not translate into acquiescence to all the political narratives promoted by this party. For example, there is no effect of WWI death rates on the equilibrium point for Europeanism. This suggests that while far-right parties may manipulate the memory of the war to their own electoral advantage, the targeted segment of the population offers only conditional support to political conflicts that reflect their latent preferences, rather than dogmatically embracing all positions endorsed by the far-right. Future research should benefit from such a finding, allowing for a more nuanced discussion of why some people vote for the far-right; as this paper suggests, it may be due to a limited supply of parties that match latent preferences, rather than a primarily demand-driven phenomenon.

Crucially, this paper does not argue that nothing else happened in France after WWI, or that the WWI experience has a unique causal power that does not interlock with the consequences of political action after 1919. Massive political events, such as World War II or the end of the colonial empire, could potentially dilute or amplify the behavioural consequences of WWI, thus altering the magnitude of their joint effect. Instead, my main argument is that understanding contemporary political behaviour requires a careful study of the historical factors that have shaped people's attitudes to politics. There may be several such critical junctures, and they may interact in complex ways. The more modest point I make in this paper, therefore, is that the memory of extreme political violence, such as the experience of war, is persistent, and investigating its persistence might lead us to discover new ways of studying electoral politics.

Notably, however, all events that occurred after the end of WWI, that influence support for the farright in France, and that were (potentially) caused by WWI, are not confounders of the relationship examined in this paper. Such events, in order to confound, must be causes of both the independent and the dependent variable in order to invalidate the recovered effects due to omitted variable bias (Cinelli et al., 2022; Cinelli & Hazlett, 2020; Steiner & Kim, 2016); as long as they are on the path between the independent and the dependent variable, controlling for them would imply controlling for post-treatment effects, or in other words, removing part of the effect we are interested in estimating (Montgomery, Ny-



Panel A. Z is a confounder

Panel B. Z is not a confounder (it is a mechanism)

Figure 2.4: When to control for third variables, based on Cinelli et al. (2022)

han, & Torres, 2018; Pearl, 2015). Formally, adjusting in this scenario would be a form of over-control that would violate the back-door criterion (Pearl, 2009; Pearl & Paz, 2014). Figure 2.4 visually summarises this distinction, following Cinelli et al. (2022, pp.8-9).

In terms of interpreting the results, the role of WWII is much more important; by not controlling for WWII, the interpretation of the effects estimated in this paper is that of death-rates in WWI on support for the far-right, potentially *through* the way in which WWI has shaped WWII, which in turn has shaped far-right electoral dynamics. Staying from the statistical language, another way of explaining this is that if we identify an effect of WWI death rates, and hypothesis that WWII also induced a causal effect on support for the far-right, the latter would be an indirect mechanism of the former, and not a parallel explanation. Such an ex-post mechanism (i.e., WWII) is crucial to understanding the unfolding of the causal trigger (i.e., WWI) and should not be removed unless the goal of the analysis is to identify some form of controlled direct effect, which is not the case in this paper as it would not be very well defined (Pearl, 2022)²¹.

²¹Finally, even if we assume that WWI and WWII induced separate effects, controlling for the latter has no bearing on the magnitude of the former, being a neutral and not a good control in this unlikely scenario where the two wars are not related (Cinelli et al., 2022, pp.7-8).

3

The repression-dissent nexus in high-threat environments: Evidence from the Romanian Gulag

"Every man always has handy a dozen glib little reasons why he is right not to sacrifice himself." — Aleksandr Solzhenitsyn, The Gulag Archipelago 1918–1956

3.1 Introduction

The legacy of political violence is one of the most powerful forces fueling anti-regime beliefs and attitudes (Walden & Zhukov, 2020). According to prevailing theories, whether these translate into outright dissent depends on the political opportunity structure¹, with opposition mobilising when the expected costs of further repression are lower than the benefits of dissent (Kilavuz, Grewal, & Kubinec, 2023; Rozenas & Zhukov, 2019; Wang, 2021; Zhukov, 2023). However, this reasoning does not adequately explain dissent

¹We define the political opportunity structure, following Tarrow (1994, p.85), as the "consistent - but not necessarily formal or permanent - dimensions of the political environment that provide incentives for people to undertake collective action by affecting their expectations for success or failure".

in high-threat environments where the expectation of reprisals is high - a relatively common phenomenon (Anisin, 2019) - nor does it explain what motivates dissidents in such circumstances.

To better understand dissent in high-threat environments, we develop and test a new theory anchored in the logic of appropriateness (March, 1994), based on the premise that when communities have endured or witnessed a history of extreme repression, their collective identity is structured in part by the memory of violence (Bautista, González, Martínez, Muñoz, & Prem, 2023; De Juan et al., 2023; N. Lupu & Peisakhin, 2017; Rozenas et al., 2017; Zhukov & Talibova, 2018). When this component of identity becomes salient, individuals, acting as members of their victimised community rather than as purely utility-maximising agents, engage in dissent not just strategically, as a result of risk-reward calculations, but intentionally, as a constitutive part of who they are and who they have been socialised to believe they should be. Dissent becomes the expression of a deontological norm about how individuals from a community with a shared value system ought to behave (Bateson, 2024; Kurzman, 2008; Pearlman, 2013, 2018).

We illustrate our argument with one of the most brutal, yet understudied, episodes of political violence in the 20th century, the Gulag system of labour camps, penal colonies, and extermination centres established by the communist regime in Romania after 1945, in which more than 500,000 people died and millions more were affected (Boldur-Lăţescu, 2005; Deletant, 2001a; Frunza, 1990). We believe this is an extreme case of communist repression and a typical case of dissent in high-threat environments (Seawright & Gerring, 2008). Using an original dataset encompassing the universe of Gulag facilities in Romania, we demonstrate a significant link between the presence of these facilities and heightened dissent against the regime during the December 1989 revolution. Our analysis employs both selection-onobservables and instrumental variable (IV) approaches, the latter exploiting the logistical considerations of the regime in establishing the Gulag system.

Across several model specifications, including OLS and IV estimations, we find that localities hosting a Gulag facility experienced significantly more confrontations with authorities (Tables 3.3-3.5). On average, these confrontations resulted in approximately 2.9 more people being seriously injured per locality, an almost fivefold increase compared to the sample mean of 0.605 severe injuries. Drawing on data

from the Life in Transition Survey (LiTS) III, we rule out variation in the intensity of local repression as an alternative explanation (Table B11). We also provide evidence that the formation of anti-communist norms, our theorised mechanism, drives this effect: the presence of the Gulag reduced local Communist Party membership, reinforced economic grievances manifested during the Revolution, and continued to influence protest and voting behaviour against neo-Communist elites even after democratisation (Tables 3.6-3.9). Through process tracing in the pathway case of Timisoara, the starting point of the Revolution, we also show how lingering memories of political violence generated anti-communist norms that motivated people to confront the regime despite the high-threat environment of December 1989. Figure 3.5, which leverages data from an original survey, provides evidence that the qualitative insights are generalizable to the population of dissidents that have participated in the Revolution.

We make several contributions to the literature. First, we bring a new perspective to the debate on the repression-dissent nexus. For decades, empirical results on this question have been mixed. While Rozenas and Zhukov (2019) have attempted to provide a unifying framework that can explain both sets of findings, we still lack a deeper understanding of why and when individuals are willing to disregard further repression in a high-threat environment. We propose a shift from conceptualizing dissent as purely an algorithmic process to a more complex interplay of rational choice and social norms about what constitutes appropriate behavior. This approach brings current research practices in political science into alignment with findings from other fields such as sociology and anthropology (Kurzman, 2008; Pearlman, 2013).

Second, we contribute to the literature examining the persistent effects of political violence (N. Lupu & Peisakhin, 2017; Rozenas et al., 2017; Rozenas & Zhukov, 2019; Zhukov & Talibova, 2018). While many studies using credible causal identification strategies have been published on the legacy of Gulag-like structures (Homola, Pereira, & Tavits, 2020; Miller & Smith, 2015; Nikolova, Popova, & Otrachshenko, 2022), their reliance on a limited number of single-country cases restricts the generalizability of the proposed arguments (Pepinsky, 2019). By departing from traditional settings such as Nazi Germany and the Soviet Union and examining a less studied country, Romania, we help assess the external validity of conventional findings and establish proper scope conditions.

Third, we contribute to the literature highlighting the central role of communities and communal

norms in ensuring the persistence of historical institutions and events (Charnysh & Peisakhin, 2022; Neundorf & Pop-Eleches, 2020). We provide evidence that understanding the long-term links between repression and dissent is predicated on grasping how communities and their constituent meso-level organizations operate, how they curate the memory of political violence, and how they transmit social norms to future generations. Methodologically, we argue that validation of this causal chain is best achieved through a mixed-methods framework that provides both cross-case evidence for the existence of a causal effect and within-case evidence for the unfolding of the causal mechanism. We implement an original case selection algorithm for pathway cases to perform process tracing in an appropriate setting for valid causal inference, building on Gerring (2007).

3.2 A normative theory of dissent in high-threat environments

Through repression, autocracies inadvertently cultivate persistent anti-regime values (Balcells, 2012; Bautista et al., 2023; De Juan et al., 2023; Homola et al., 2020; N. Lupu & Peisakhin, 2017). In their seminal paper, Rozenas and Zhukov (2019) show that the translation of such values into overt opposition depends on the political opportunity structure, in particular the expected probability of reprisals. Low-threat environments, where risks are minimal, tend to encourage dissent, while high-threat environments tend to suppress it. This happens as communities have a threshold of acceptable violence, beyond which the opposition's ability to recoup losses by attracting new dissidents diminishes, thus limiting the likelihood of dissent (Zhukov, 2023).

However, this explanation on when does repression trigger dissent leaves a crucial question unanswered: how do communities determine 'their' specific threshold of acceptable violence? Without an answer, we cannot predict whether a given level of threat will prove too high for opposition mobilisation, nor can we fully understand the factors that shape the relationship between repression and dissent. We argue that the threshold should not be seen as fixed, but rather as endogenous to each community's history of political violence. Specifically, we maintain that communities with more severe histories of repression develop robust anti-regime norms over time, making them more likely to tolerate higher levels

of retaliation during episodes of dissent.

To better understand this alternative mechanism of dissent, we decompose the decision-making process of dissidents into two main logics: the logic of consequences and the logic of appropriateness (March, 1994). Current theories privilege the logic of consequences, suggesting that attitudes are translated into dissent based on citizens' cost-benefit assessments in uncertain environments. While individuals may weigh different costs and benefits in their utility functions, this approach ultimately reduces the decision to dissent to a rationalist-like calculus for all actors. Even research that incorporates affective responses into decision-making, such as Young (2019), typically frames emotions merely as amplifiers of perceived risk despite evidence from sociology and anthropology suggesting a more complex reality: emotions can independently drive political action and override individual utility considerations (Bateson, 2024; Kurzman, 2008; Pearlman, 2013, 2018).

According to those theories rooted in a mainly consequentialist logic, dissent in high-threat environments should be a very limited phenomenon. However, empirical evidence shows that many protests occur in autocratic systems even under such high-threat conditions (Anisin, 2019; Goodwin, 2001; Schock, 2005)². To explain what could be seen as deviant cases, we appeal to the logic of appropriateness, which holds that political action is driven by social norms that frame what is 'appropriate' in particular contexts, with individuals acting in response to the question: "What would someone like me do in this situation?" (March, 1994, p.57). This situational sensitivity, which enables people to navigate complex moral spaces, is heightened in episodes of rupture with the status quo, such as dissent in an otherwise stable autocracy (Pearlman, 2018, p.884).

How does this apply in the case of the repression-dissent nexus? Following exposure to political violence, citizens may come to see dissent not only as a means of sanctioning the government, but also as a meaning-making practice that allows them to express their (anti-regime) identity and fulfil their moral obligations to the community, regardless of the cost (Pearlman, 2018, pp.883-887). The process begins when individuals experience political violence directly or, more commonly, when they encounter nar-

²To explain this using purely consequentialist logic, one could argue that dissidents operating in high-risk environments derive unusually high benefits from overthrowing the regime. But even so, this explanation fails to address why the same dissidents would continue to protest as the threat level in this particular scenario rises, an often observed dynamic (Pearlman, 2018).

ratives of violence through family accounts and social interactions (De Juan et al., 2023; N. Lupu & Peisakhin, 2017; Rozenas & Zhukov, 2019), as a manifestation of collective trauma (Alexander, 2004, Chapter 1). Subsequently, these shared narratives are consolidated and institutionalised as collective memory by local organisations such as churches, schools and community groups, which act as both repositories and transmitters of violent experiences over time (Halbwachs, 1992; Jelin, 2003; N. Lupu & Peisakhin, 2017; Volkan, 2001).

The process of norm formation through collective memory is further reinforced by multiple mechanisms: intergenerational transmission ensures continuity down the line, community rituals provide regular reaffirmation, and social sanctions encourage compliance (Bicchieri, 2005; Wertsch, 2002; Zerubavel, 2003). As these norms become entrenched, communities tailor their practices to conform to and strengthen them, creating a self-perpetuating cycle that deepens their impact how individuals related to the perpetrator of violence. Paradoxically, continued repression often serves to intensify these efforts, bolstering a shared sense of victimhood and resistance (Scott, 1990) and further entrenching anti-regime norms and identities within the community.

That dissent is driven by anti-regime norms is consistent with findings that individuals with strong group identities are willing to endure seemingly irrational losses in order to maintain the status of their social group and the narratives that motivate it (Bonomi, Gennaioli, & Tabellini, 2021; Shayo, 2009). This can be further understood by considering how repression shapes community dynamics: as anti-regime attitudes emerge, the decision to dissent evolves from a matter of individual welfare to an embodiment of the expected response of the group archetype (Bonomi et al., 2021, p.2377-2378). Consequently, if the ideal behaviour of this group archetype is dissent - that is, if the imagined reference individual would disregard further reprisals - then community members who share these norms are likely to follow suit³

We identify two necessary conditions for dissent in high-threat environments. First, the collective memory of repression must be strong enough to override, or at least subordinate, risk-reward calculations. Thus, our argument applies to scenarios of indiscriminate political violence that are capable of forging

³From a rationalist perspective, while repression continues to impose significant costs on dissenters, these are consistently outweighed by the moral costs of inaction, which include both the disutility of preference falsification and the psychological distress of deviating from the group archetype.

communal identities in opposition to the perpetrators and their successors. Second, the availability of opportunities for dissent must be limited. When opportunities for dissent are rare, potential dissidents tend to evaluate each opportunity carefully, which means that they are more likely to consider their moral obligations to their victimised community. The decision not to participate in dissent on a given occasion may mean not only postponing the opportunity for future dissent, but possibly foregoing it altogether, which carries a high cost in terms of the lost expressive value of dissent (Kuran, 1997).

The scope of our theory is primarily limited to intentional dissent—defined as initial waves of protest where participants have no expectation of the demonstration's future scale. This scope condition is crucial for three reasons. First, it aligns with our theoretical emphasis on the logic of appropriateness and anti-regime norms, as early dissenters are more likely acting on internalized norms and collective memories rather than responding to immediate political opportunities. Second, it allows us to examine dissent less influenced by collective action dynamics, providing a clearer view of the long-term, community-level processes we've theorized. Third, it helps isolate our proposed mechanisms from confounding factors that emerge in later stages of protest movements, such as safety in numbers or changing political opportunities⁴. By focusing on intentional dissent, we can more accurately test our theory's predictions about the relationship between historical repression, norm formation, and high-risk collective action.

3.3 Romanian communism and the Gulag system

In the final stages of the Second World War, the Allies reluctantly granted the Soviet Union a controlling interest in Romania. This meant a takeover by the Moscow-backed communists, beginning with the installation of the puppet government of Petru Groza in March 1945 and the abolition of the monarchy in favour of a People's Republic in December 1947 (Deletant, 2001b, Chapter 2). The one-party communist regime was formalised with the adoption of the Constitution of 13 April 1948, which was a facsimile of the Soviet Constitution of 1936. At the heart of this document was Article 32, which stated that citizens had the right to associate and organise, provided that their activities did not violate the democratic

⁴While norms may also motivate later dissidents, these stages are less relevant to our study of dissent in high-threat environments and make it difficult to disentangle the effects of repression from those of cascading.

order. This was the legal pretext used to justify some of the most brutal forms of repression in the name of safeguarding democracy.

The communist regime then developed a Gulag system of labour camps, penal colonies and extermination sites designed to spread terror among the "ideological enemies of the people" (Deletant, 2001b). Built on the Stalinist model, the Gulag was designed to systematically exterminate those who challenged the regime through a myriad of methods ranging from physical violence to starvation, forced labour and degrading living conditions (Deletant, 2018, Chapter 7). Initially, the Gulag consisted of 74 facilities with a capacity of only about 15,000 people. It soon developed into a dense network of 44 prisons for political prisoners, 61 extrajudicial investigation, storage and subjugation sites, 72 forced labour camps, 63 deportation centres, 10 psychiatric institutions, 93 mass graves and political assassination sites, and several penal colonies (Institutul de Investigare a Crimelor Comunismului şi Memoria Exilului Românesc, 2013, pp.1-2).

The repression was carried out primarily by the Romanian secret police, the Securitate (Deletant, 2016, chapters 1-2), considered the "tip of the sword" of the regime (Boldur-Lăţescu, 2005, p.22). Estimates suggest that, including civilian informers, the Securitate comprised some two million Romanians over the duration of the regime, making it one of the largest institutions of its kind, second only to the East German Stasi and the Soviet KGB⁵.

When these prisons and the small forced labour camps that surrounded them proved inadequate to the scale of the Securitate's actions, the government resorted to creating penal colonies where tens or even hundreds of thousands of citizens were sent to serve extrajudicial sentences on mass construction projects such as the Danube-Black Sea Canal (Deletant, 2018, Chapter 7). By 1960, some 520,000 young people had passed through these camps and colonies (Tismăneanu, 2006, pp.201-202). The communist regime was also responsible for the expulsion of more than 44,000 families to other regions, usually strategically located in barren areas, where they had to live in inhumane conditions, and for the administrative punishment of 82,700 people (Tismăneanu, 2006, pp.289-298). Some estimates put the total number

⁵The experimental methods of torture used by the Securitate have remained in the memory of Romanians as some of the most inhumane acts of the government, where so-called re-education through torture was pursued in some Gulag facilities (e.g. Suceava, Pitesti, Gherla, and Targu Ocna).

of political prisoners at over 1.1 million, of whom around 500,000 died (Boldur-Lățescu, 2005, p.18). Others mention that the repression may have affected at least 2 million people (Frunza, 1990).

After 1965, the regime moved towards de-Stalinisation, replacing mass repression with targeted violence, totalitarian surveillance and ideological indoctrination. The last batches of political prisoners that were still trapped in the Gulag were pardoned by the new leader Nicolae Ceausescu, in the face of international pressures. However, this was by no means an era of liberalisation. Instead, under the personalist rule of Ceausescu, the regime mixed elements of socialism with ultra-nationalism, along the lines of North Korea's Juche philosophy (Deletant, 2016). Crucially, the Securitate prevented the formation of new opposition movements, as the pre-existing ones had been completely destroyed in the Gulag. As a result, Romania was among the countries in Central and Eastern Europe with the fewest opportunities for dissent and therefore the fewest protests.

In December 1989, after a series of relatively peaceful revolutions in Central and Eastern Europe, the anti-communist Revolution in Romania took place against the backdrop of a gruelling domestic economic crisis. It was the only violent revolution against communism in Europe, with more than 2,000 people severely injured and nearly 1,000 killed (Petrescu, 2014). While the other Revolutions clearly represented a relative shift in the political opportunity structure compared to the pre-1989 period, the Ceausescu regime remained committed to ensuring its survival and demonstrated a strong commitment to repression from the early stages of the Revolution. However, despite the high-threat environment and the increasing scale of reprisals, dissidents continue to protest, with the Revolution spreading from its initial hotbed in Timisoara to several major cities and the capital, Bucharest.

3.4 Data

3.4.1 Independent variable: Presence of Gulag facilities in a locality

We have compiled a dataset that geolocates every labour camp, extermination site, and mass grave that made up the Romanian Gulag, and matched them to one of 3,181 localities in the country⁶. To do so, we draw on the list of such facilities drawn up by the Presidential Commission for the Study of the Communist Dictatorship (Tismăneanu, 2006), which allows us to determine their exact latitude and longitude coordinates. The geographical distribution of the Romanian Gulag is shown in Figure 3.1.

The main independent variable is a binary indicator that takes the value 1 if a locality hosts at least one of the 148 facilities and 0 if it does not. The results are robust to using an alternative operationalisation: the Euclidean distance from a locality to the nearest Gulag facility (Tables B2, B4). Using the presence of, or distance from, repressive infrastructure to measure exposure to political violence is a common practice in political science. For example, Bautista et al. (2023) use the spatial distribution of military bases to study the effects of repression on anti-regime voting patterns. Similarly, Homola et al. (2020) employ the distance from Nazi concentration camps to show that proximity to such infrastructure leads to long-term patterns of xenophobia. Finally, Charnysh and Finkel (2017) use the distance from Treblinka to measure whether Germans were more likely to have acquired the valuables of the Jewish population.

We argue that the presence of a Gulag facility in a locality made the community more aware of the repression, facilitating the formation of anti-communist norms (see Tables 3.6-3.9 for evidence). This happened through three main channels documented by historians and and ethnographers (Ciuceanu, 2001; Constante, 1995; Mazilu, 2004; Nicolau & Niţu, 1993; Oprea, 2002; Troncotă, 1999). First, the repressive infrastructure served as a symbolic representation of political violence to which local residents had access, either through direct contact with victims during their imprisonment or through word of mouth spread by observers or meso-level social organisations such as churches. Second, the Gulag was partly

⁶We limit our scope to the most extreme units, namely labour camps and assassination sites with their associated mass graves. The decision to group these types of sites together is motivated by their common history of memorialisation, as these are the types of sites that the population understood to constitute the Gulag (Ciuceanu, 2001; Constante, 1995; Mazilu, 2004; Oprea, 2002; Troncotă, 1999). In the case of Romania, assassination sites and mass graves were mostly associated with existing labour camps (Tismăneanu, 2006).



Number of Gulag facilities 0 I 2 3 4

Figure 3.1: Geography of the Romanian Gulag

Note: This map shows the distribution of the Gulag facilities examined in this paper across the Romanian localities. The map is divided into 100 spatial groups, 81 of them containing at least one locality, based on latitude and longitude; the groups are separated by dotted lines.

staffed by local residents for tasks such as cleaning, cooking and sometimes even disposing of corpses. Third, some Gulag survivors remained close to the region where they were tortured, serving subsequently as living embodiment of repression for the other members of the community. These channels of norm-formation are consistent with the psychology literature showing that witnessing atrocities, or learning about them from familiar sources, can traumatise bystanders by causing them to feel shame and guilt for not being able to stop the violence (De Juan et al., 2023, pp.8-9).

Our argument relies on these people, residents of the localities that hosted Gulag facilities, as carriers of social norms, who either witnessed the violence themselves or learned about it as carriers of anti-regime

social norms, similar to Charnysh and Finkel (2017) and Homola et al. (2020). However, we do not consider in our model whether Gulag survivors also developed persistent anti-communist values that they passed to future generations. We leave this discussion, in the spirit of N. Lupu and Peisakhin (2017) and Rozenas et al. (2017), for future research, while acknowledging that multiple causes may have jointly shaped the protest dynamics in 1989 (Anisin, 2018).

3.4.2 Dependent variable: Severe injuries during the 1989 Revolution

As our main dependent variable, we use the number of people who were severely injured in each locality during confrontations with the government forces in 1989. We draw on the pre-validated list of injuries compiled by Surdea-Hernea (2024)⁷, allowing us to match each entry in the list with its corresponding locality⁸. The results are similar when using a binary indicator of whether at least one severely injured person comes from a particular locality (Tables B3-B4).

Severe injuries are our preferred way to operationalize dissent during the 1989 Revolution for two reasons, both of them validated through numerous discussions with local historians and members of NGOs curating the memory of the Revolution. First, being injured in the Revolution, as opposed to merely participating, reveals a two-step process involving participation in dissent as well as direct confrontation with regime officials, and thus an explicit expression of anti-regime behaviour⁹. Thus, our measure captures variation in the level of dissent across localities and takes into account mainly *intentional* dissent, as opposed to political participation driven purely by informational cascades as the Revolution unfolded. This increases the content validity of our measurement, given that our theoretical argument concerns primarily intentional dissent of people with anti-communist values (Adcock & Collier, 2001, pp.527-538)¹⁰

⁷The list is based on official data collected by the Romanian government in collaboration with organisations of the revolutionaries. The level of disaggregation is not an issue, as it is taken directly from the official documents of Romanian citizens, to which the government had full access. This mitigates concerns such as those raised by Croicu and Eck (2022).

⁸According to our theoretical argument, it does not matter where people were injured, but where these injured people come from, as the socialisation into communal anti-communist norms, nurtured by the presence of the Gulag facility, takes place in the place of birth. This assumes that people grew up where they were born, which is appropriate for communist Romania, where there was very little internal mobility among the youth before 1990 (Rotariu & Mezei, 1998, pp.134-136).

⁹Being declared severely injured was not determined by the level of medical care received during the Revolution, but expost by declaration and eyewitness accounts. Therefore, our measure does not implicitly capture variation in access to medical care.

¹⁰We acknowledge that once protests are underway, if the political opportunity structure continues to shift towards a low

Second, the number of participants, a more conventional measure of the scale of dissent, is less credible in the case of this Revolution because of the high incidence of *fake* revolutionaries who tried to take advantage of being considered participants without actually taking part in the events or even having been part of the repressive apparatus in order to obtain tax benefits (Adevărul, 2016; Redacția Comunitatea Liberală, 2024; Stan, 2013)¹¹. By comparison, being recognised as a severely injured person required medical documentation that was not easy to falsify, did not bring significant extra-benefits to those already classified as participants, therefore reducing the incentives for artificial inflation of this count. However, we still show that our results are qualitatively similar if we use the number of participants instead of the number of severe injuries (Table B13)¹².

One concern about the validity of severe injuries as a measure of intentional dissent is that it may capture both the incentive to dissent and the pre-existing distribution of repressive infrastructure that may correlate with anti-communist sentiment instilled by the Gulag. If the latter dominates, we would not be measuring dissent but repression itself: a stronger repressive apparatus will mechanically increase injuries. We guard against this threat by showing that controlling for the size of the Securitate apparatus in a county (see Section 3.7 for details on measurement) does not remove the effect of Gulag facilities on the number of injuries (Table B11). The effect of the Securitate itself is not significant¹³. In addition, we discuss the sequencing of the deployment of regime forces during the unfolding of the Revolution in more detail in Section 3.8, demonstrating that it cannot explain away our argument. Figure 3.2 shows

threat environment, holding anti-regime norms is likely to lose its status as a necessary condition for dissent. However, this paper is particularly interested in the first wave of dissidents operating in an overtly high-threat environment. In December 1989, most severe injuries occurred in the first days of the Revolution, while the number of participants exploded after the violence began to subside (Petrescu, 2014, pp.326-329).

¹¹This would not bias our results if the inflation in the number of participants varied randomly, or if it were evenly distributed across localities. However, there is strong anecdotal evidence to suggest that fake revolutionaries were more likely to receive their certificates in certain counties, depending on the political composition of the local governing bodies and on the local level of corruption, especially in the judiciary (Andreescu, 2023; Bursa, 2016; Cotidianul, 2017; HotNews, 2024; Romania Liberă, 2016). Certain counties with a high number of participants and injured were more insulated from the fake revolutionary phenomena due to the presence of important NGOs curating the memory of the Revolution.

¹²Alternatively, using the number of people killed per locality would be even more problematic. The legal process that allowed the children of the victims to receive state benefits was also riddled with corruption (Adevărul, 2016). In addition, some victims were never recognised because the communist regime burned the bodies. These activities were not evenly distributed throughout the country, but concentrated in some of the centres of the uprising, such as Timisoara (Florea et al., 1995).

¹³While we perform this robustness check to demonstrate that our results are not primarily driven by variation in the supply of repression capacity, we acknowledge that the Securitate distribution is likely endogenous to the geography of the Gulag. Therefore, following the advice of Acharya et al. (2016), we do not include Securitate related covariates in our main models to avoid post-treatment bias.



Figure 3.2: Distribution of independent and dependent variables

the distribution of the independent and dependent variables, and already provides preliminary evidence that places that hosted a Gulag facility have a higher number of severe injuries.

3.4.3 Control variables: confounders and alternative explanations

The effect of the Gulag on injuries during the Revolution can be directly observed in a bivariate regression setting (columns 1 of tables 3.3-3.5). However, we also demonstrate that the results are robust to the inclusion of a series of covariates dealing with two potential threats: confounding and alternative theories unrelated to the geography of the Gulag (Cinelli et al., 2022). We statistically adjust for the main factors that could have affected both the number of injuries and the likelihood of having a Gulag facility in one's own locality: i) population density before the communist takeover, measured in 1933, and ii) whether or not a locality was part of the Habsburg Empire, a strong predictor of institutional (dis)trust in the region (Becker et al., 2016; Vogler, 2023).

Then, to improve the precision of our estimate whilst eliminating some alternative explanations, we


Figure 3.3: Directed acyclic graph (DAG) representing the causal model underlying our theoretical argument.

adjust for factors that most probably influenced the protest dynamics: i) the distance to Timisoara and Bucharest, the two main centres relevant for the opposition mobilisation in December 1989, ii) the distance to the nearest point of the railway network and the distance to the nearest water source, which could influence the ability of both dissidents and the regime to reach a locality, and iii) the population density based on the 1977 census as a proxy for the size of the repressive apparatus that is less likely to be endogenous to the Gulag geography. As an alternative, we show in the Appendix that normalizing the number of severe injuries by the population density doesn't affect our estimates (Table B29).

The complete causal model assumed in this paper is shown in Figure 3.3.

Table 3.1 provides summary statistics for the relevant variables, separated by treatment status.

3.5 Causal identification strategy

We exploit spatial variation to demonstrate that the presence of a Gulag facility in one's own locality made citizens more likely to intentionally dissent during the anti-communist Revolution of 1989, leading to

Variable	Did not host a Gulag facility				Hosted a Gulag facility			ity
	Min	Max	Mean	SD	Min	Max	Mean	SD
Severe Injuries	0.00	42.00	0.37	1.75	0.00	381.00	6.00	12.00
Population Density in 1977	1.33	4897.35	94.29	455.49	21.26	8086.83	379.15	1600.75
Distance to Railway	0.00	56.89	8.01	12.34	0.86	51.52	9.11	11.25
Distance to Water Source	0.00	88.47	21.16	18.15	14.96	68.62	17.36	14.12
Former Habsburg Province	0.00	1.00	0.27	0.44	0.00	1.00	0.40	0.49
Population Density in 1933	21.26	190.96	65.42	33.54	27.73	190.96	64.14	36.76
Distance to Bucharest	8.30	487.97	236.20	110.15	117.24	424.40	249.74	98.61
Distance to Timisoara	6.79	651.23	326.99	155.12	136.45	653.85	343.54	162.24

higher rates of severe injury. For the causal effect to be well-identified, the distribution of Gulag facilities must be as-if-random (Kocher & Monteiro, 2016, pp. 954-955); that is, after a theoretically-guided statistical adjustment for critical antecedents, it must be independent of the conditions that preceded the creation of the Gulag which also influenced patterns of mobilisation in 1989.

First, under a selection-on-observables assumption, we argue that the factors considered in the authorities' decision to select the location(s) of the Gulag were primarily logistical, unrelated to the spatial distribution of political preferences in pre-1945 Romania.

The Gulag was established in locations where previous regimes had built prisons and internment camps (Institutul de Investigare a Crimelor Comunismului și Memoria Exilului Românesc, 2013, pp.230-233). As these were built in parallel by Romanian, Austro-Hungarian, Russian and Ottoman administrations throughout the 19th and early 20th century, it is unlikely that the decision on where exactly to build such structures was systematically correlated with the characteristics of the various populations and, in particular, with anti-socialist attitudes. This is all the more true since, before the end of the Second World War, communism or socialism had almost no organic tradition in the Romanian-speaking provinces (Adam, 2018). Moreover, the individual files of dissidents sent to the Gulag do not indicate any intention to distribute prisoners among particular facilities; on the contrary, prisoners with similar backgrounds and from the same regions were sometimes sent to opposite parts of the country, depending

on the uncoordinated decisions of local decision-makers (Institutul de Investigare a Crimelor Comunismului și Memoria Exilului Românesc, 2013, pp.233-289).

One concern is that the continued presence of Gulag facilities in a locality could be conflated with regional developments that correlate with anti-communist attitudes (Kelly, 2020, p.4). To address this challenge, we add grid-cell fixed effects to the estimation, which we compute following the approach of Doucette (2024, pp.8-9). Alternatively, one could use administrative unit fixed effects (e.g. NUTS3 fixed effects), but the counties during the communist era were created after most of the repression in the Gulag had already taken place and differ significantly from the pre-World War II territorial organisation; their introduction could lead to post-treatment bias (Pepinsky, Goodman, & Ziller, 2023, pp.2-4). Similarly, using pre-War administrative units is not possible because of major territorial changes before and after 1945. We also include a second-degree polynomial in latitude and longitude to control for directional gradients (Kelly, 2020, p.4). In spirit, this makes our approach similar to the two-dimensional geographic regression discontinuity design introduced by Dell (2010), assuming that the Gulag forms a set of discontinuities. We estimate the following equation:

Severe Injuries_i =
$$\beta_0 + \beta_1 \text{Gulag Presence}_i + \mathbf{X}_i \boldsymbol{\beta} + f(\text{Lat}, \text{Lon})_i + \gamma_{\text{grid}_i} + \epsilon_i$$
 (3.1)

where Severe Injuries_i is the number of severely injured individuals in locality *i*. The key independent variable, Gulag Presence_i, is a binary indicator for whether locality *i* hosted a Gulag facility. \mathbf{X}_i is a vector of controls, including population density and distances to major cities. $f(\text{Lat}, \text{Lon})_i$ represents a second-degree polynomial in latitude and longitude to capture spatial trends. γ_{grid_i} are grid-cell fixed effects that control for unobserved regional characteristics. ϵ_i is the error term.

Second, we employ an IV strategy that leverages exogenous variation in the distribution of Gulag facilities that is driven by distance to similarly situated units¹⁴. We estimate the following two-stage least squares (2SLS) equations:

¹⁴Distance-based instruments are well established in the historical persistence literature (Becker & Pascali, 2019; Nunn, 2008).

First Stage: Gulag Presence_i = $\alpha_0 + \alpha_1$ Distance to Nearest Gulag_i + $\mathbf{X}_i \boldsymbol{\alpha} + f(\text{Lat, Lon})_i + \gamma_{\text{grid}_i} + \nu_i$ (3.2)

Second Stage: Severe Injuries_i =
$$\beta_0 + \beta_1 \text{Gulag Presence}_i + \mathbf{X}_i \boldsymbol{\beta} + f(\text{Lat, Lon})_i + \gamma_{\text{grid}_i} + \epsilon_i$$
 (3.3)

where Distance to Nearest Gulag_i is the instrumental variable that predicts Gulag Presence_i in the first stage, and Gulag Presence_i is the fitted value from the first stage used in the second stage regression. \mathbf{X}_i includes control variables, $f(\text{Lat}, \text{Lon})_i$ is a second-degree polynomial in latitude and longitude, and γ_{grid_i} are grid-cell fixed effects.

We use a specific feature of the Romanian Gulag to restrict the variation in the spatial distribution of labour camps to a credibly exogenous part. Due to the limited capacity of the resistance movement, the number of Gulag facilities needed outside the regular prison system was limited, and the pace of opening new facilities declined as repression became more effective in the late 1950s (Deletant, 2001b). Most of the new camps were only built for specific political projects (e.g. the Danube-Black Sea Canal) in cases where no camps were available in the same region as the project¹⁵.

Therefore, we use the distance from each municipality to the nearest Gulag facility as a predictor of whether a municipality itself hosted such a facility¹⁶. We show that this distance is strongly correlated with the main independent variable according to the most stringent statistical tests available (Lee, Mc-Crary, Moreira, & Porter, 2022; Stock & Yogo, 2005). To be a valid instrument, it must also satisfy an exclusion restriction: the distance from a municipality to the nearest facility should not affect how many people were severely injured during the 1989 Revolution, except through reducing the probability that a

¹⁵Conceptually, if the decision to establish a labour camp in municipality X is driven by logistical and political reasons, we eliminate the latter by focusing on the part of the decision that is influenced by the existence of regime alternatives. The assumption behind this is that the communist regime, when faced with the choice of whether to establish a camp between two municipalities with similar characteristics, will boil down to rationally picking the one without immediate substitutes.

¹⁶We only take into account the distance to other Gulag facilities that were opened before the one in question, thus avoiding problems of temporal ordering.

municipality itself hosted a facility.

The primary concern is that people living near Gulag facilities would become more aware of overall repression in the country and thus develop stronger anti-regime attitudes, making them more likely to participate in the Revolution and thereby be injured, regardless of whether the facility was in their locality. To alleviate concerns, we show that the effect of labour camps on the injury count is very local, losing significance 10 km outside the boundaries of a municipality (Figure 3.4).

DV: Existence of Gulag facility in a locality						
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to resistance movement	0.001 (0.978)	0.001 (0.749)	0.001 (0.786)	0.001 (0.757)	0.001 (0.822)	0.001 (0.818)
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X
Spatial lags	X	X	X	X	X	\checkmark
N.	3181	3180	3180	3180	3180	3180

Table 3.2: Relationship between Gulag geography

Notes: P-values for Conley standard errors in brackets. ***p < 0.01; **p < 0.05; *p < 0.1

A second threat to exogeneity is that the distance to a Gulag is correlated with the distribution of the resistance movement in the first years after the communist takeover. To check this, we review historical literature that identifies such resistance movements in the first years of the Petru Groza government (late 1940s and early 1950s), and compile a list of those that were active before the main phase of the Gulag establishment (Brişcă, 2004; Dobre et al., 2003; Ionițoiu, 1996; Onișoru, 2003). We then geolocate the resistance movements from the list, match them to our main dataset, and run a series of regressions to test for any correlation between the locations of these movements and the locations of Gulag facilities. We show such correlations be absent according to Table 3.2, thus strengthening the confidence we have in our exclusion restriction¹⁷.

¹⁷More broadly, the instrument could be related to spatial characteristic that made it easier for citizens to participate in

Finally, we also conduct a placebo test to further validate our instrument by examining the subset of municipalities without Gulag facilities. In these cases, the first stage of the 2SLS is effectively zero, as there is no variation in the endogenous variable (presence of a Gulag facility). Consequently, the instrument—distance to the nearest Gulag facility—should exhibit no relationship with the outcome variable (severe injury count). We confirm this by estimating the reduced form relationship between the injury count and the instrument among these municipalities and find no significant correlation (Table B28).

3.6 The long-term effect of the Gulag on dissent during the 1989 Revolution

In this section, we present the main results of our regression analysis, which show that repression led to higher levels of intentional dissent against the communist regime in the long run. Table 3.3 presents the estimates under the selection-on-observables assumption, which we estimate using ordinary least squares (OLS). The dependent variable for all six model specifications is the number of severe injuries from a locality during the Revolution, with a mean value of 0.605. All coefficients are significant at conventional levels, based on p-values calculated for Conley standard errors with a 50 km kernel. The results remain significant when using grid-cell clustered errors (Table B1). Column (1) shows a bivariate correlation between the presence of a Gulag facility and injuries during the Revolution. The estimates in Column (2) then introduce the covariates discussed in Section 3.5. Starting with Column (2), the coefficient remains stable to the introduction of additional covariates, indicating robustness.

Columns (3)-(6) introduce entropy balancing as a pre-processing algorithm before the regression. Using a prespecified set of covariates, entropy balancing generates a set of weights for the observations in the sample such that the covariate distributions of the treatment and control groups in the processed data match exactly on all prespecified moments (e.g. mean, variance) (Hainmueller, 2012)¹⁸. In Columns (3)-(4), we reweight the observations according to their distance from the so-called "Romanian martyr

the Revolution. This is similar to the challenge faced under the selection-on-observables assumption, and we address it by including the battery of spatial covariates in both stages of IV estimation.

¹⁸An important property of entropy balancing is doubly robustness to linear regression: if either one of the selection models or the regression model for the outcome is well specified, the estimated effect will be consistent (Zhao & Percival, 2016)

DV: Number of severe injuries from a locality (Mean=0.605)						
	(1)	(2)	(3)	(4)	(5)	(6)
Gulag facility	5.627* (0.052)	2.095*** (0.050)	1.953*** (0.007)	1.725** (0.021)	1.984*** (0.006)	2.002*** (< 0.001)
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X
Spatial lags	X	X	X	X	X	\checkmark
R^2 Adj	0.024	0.320	0.345	0.346	0.303	-
N.	3181	3180	3180	3180	3180	3180
Moran's I	-0.002	-0.007	-0.012	-0.017	-0.008	-
Oster's' δ	-	1.62	1.76	1.76	1.78	-

Table 3.3: Results: Selection-on-observables

Notes: P-values for Conley standard errors in brackets. ***p < 0.01; **p < 0.05; *p < 0.1

cities", a list of places from which an above-average number of revolutionaries come, in terms of participation, to mitigate the risk of a selection-intro treatment effect. In other words, we use entropy balancing to ensure that the comparison between localities is not driven by ease of access to the main points of mass mobilisation across Romania, nor by proximity to major urban areas. Crucially, the distinction of "martyr cities" was based less on the number of victims and injuries, and more on the symbolic contribution to the advancement of the Revolution, which limits the threat of matching on post-treatment variables. Then, in Column (5), we reweight observations based on spatial characteristics, which serves as a main robustness check against spatial noise being the source of our causal effect. In addition, Columns (3)-(6) include, in the spirit of Rozenas et al. (2017), Moran eigenvectors to capture spatial patterns on the right side of the model equation (Dray et al., 2006), and Columns (4)-(6) include grid cell fixed effects.

For the first five specifications, we compute Moran' I for the residuals, a statistical measure of spatial autocorrelation (Conley & Kelly, 2024; Kelly, 2020). Since the values are close to 0, we can proceed with a meaningful interpretation of the coefficients. On average, the presence of a Gulag facility in one's locality increases by 2 the number of people from that locality severely injured during the Revolution,

more than three times the mean value of the dependent variable. However, as the first five specifications are estimated using linear models, spatial interactions between units could remain an issue. Column (6) serves as a robustness check, estimating the same specification as in Column (3) via maximum likelihood estimation of a spatial simultaneous autoregressive lag model. Spatial weights for this model are computed using a K-nearest neighbours algorithm, assuming that the distribution of Gulag facilities is influenced by at most 10 localities in the same region.

Next, we implement the sensitivity analysis algorithm developed by Oster (2019), which measures the degree of selection on unobservables relative to observables that would be required to explain away an effect (pp. 191-196). This protocol assumes that the relationship between the independent variable and unobservable factors can be estimated from the relationship between that variable and the observable factors as revealed by the movements of R^2 (Oster, 2019, p.187)¹⁹. To cancel out the effect of the presence of a Gulag facility on severe injuries, the effect of unobservables would have to be 1.62-1.78 times higher than that of observables, an unlikely scenario given that our main models already include 49 synthetic variables that captures every possible spatial pattern (Dray et al., 2006).

Estimating our selection-on-observables model using OLS poses two challenges. First, given that our dependent variable is a count, the results could be driven by some outliers rather than consistent patterns. Second, given that the majority of Romanian localities did not experience protests in December 1989, the effects could be driven primarily by the skewness of the distribution of injuries rather than by our theorised mechanism. In the Appendix, we provide extensive evidence against this threats. First, we show that the estimates are stable for different functional forms of the dependent variable, including logarithmic, square root, or inverse hyperbolic sine transformations (Table B14 and Tables B26-B27), as well as for different estimators appropriate for count variables, such as Poisson (Table B15), negative binomial (Table B16), and zero-inflated negative binomial (Table B17) regressions.

It is also possible that our results are mainly driven by outliers. In this case, while the estimated effect would still be valid, it would no longer support our theoretical argument, but would instead be the product of a statistical artefact. We show that removing the bottom and top 1%, 2% and 5% of observations

 $^{^{19}}$ We assume a maximum value of R^2 of 1.5 times the maximum value in Column (5), a more conservative approach than the 1.3 suggested by Oster.

based on the value of the dependent variable does not affect our results (Tables B18-B20). The same is true for removing only the top 2% and 5% of observations with the highest injury count (Tables B21-B22). Furthermore, we show that our result remains similar even if we exclude Bucharest and Timisoara, the cities with the highest number of injuries and the focal points of the Revolution, from the regression (Tables B23-B25).

DV: Number of severe injuries from a locality (Mean=0.605)					
	(1)	(2)	(3)	(4)	(5)
Gulag facility	0.446**	0.435**	0.424**	0.435**	0.432**
	(0.031)	(0.035)	(0.031)	(0.026)	(0.029)
Covariates	X	\checkmark	\checkmark	X	\checkmark
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark
Grid cell FEs	X	X	X	\checkmark	\checkmark
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark
Spatial lags	X	X	X	X	X

Table 3.4: Results: Selection-on-observables in rural areas

Notes: P-values for Conley standard errors in brackets. ***p < 0.01; **p < 0.05; *p < 0.1

Finally, an alternative theory would be that the higher number of serious injuries in places that housed Gulag facilities was due to the latter serving as potential symbolic sites for protest. This would be similar to the case of East Germany, where anti-communist manifestations took place near places like the Berlin Wall, not necessarily because of communal norms fostered by the Wall, but because it was logistically easier to expect mobilisation around such symbolic points. To guard against this threat, we estimate our models in both rural and urban areas, defined on the basis of population density (above and below 150 inhabitants per km^2 , based on the OECD definition). If community norms were at play, and not just a combination of symbolic location and accessibility, then we should also observe our effect in rural areas. Table 3.4 shows the results of this robustness check. Although the effect is smaller, it is still significant at conventional levels and consistent across different model specifications. Given the very low internal mobility in communist Romania between rural localities, as well as the barriers the regime put in place against people joining the fight in the main urban centres during December 1989, these results suggest

Table 3.5: IV results

Second stage DV: Number of severe injuries from a locality						
	(1)	(2)	(3)	(4)	(5)	
Gulag facility (fitted)	6.161***	2.393**	2.506**	2.384**	2.600**	
	(< 0.001)	(0.033)	(0.019)	(0.021)	(0.027)	
First stage DV: Presence of Gulag facility						
Distance to facility	0.005***	0.005***	0.007***	0.010***	0.009***	
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	
Covariates	X	\checkmark	\checkmark	\checkmark	\checkmark	
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	
Grid cell FEs	X	X	X	\checkmark	\checkmark	
N	3181	3180	3180	3180	3180	
Cragg-Donald F statistic	421.88	420.44	437.33	444.69	444.69	
Notes: P-values for Conlev standard errors in brackets.						

the existence of a social norm even in villages that were exposed to the Gulag.

***p < 0.01; **p < 0.05; *p < 0.1

We then move to the IV results, reported in Table 3.5. The first stage of the estimation suggests that, regardless of the covariates and FEs included, distance to another Gulag facility predicts the presence of such a facility in a locality: the higher the distance to a Gulag facility, the more likely that another one was opened in a locality. In other words, as discussed earlier, the communist regime established new camps only when there was no other camp in operation in a given geographic region. How strong is this relationship? First, the F-statistics are significantly above the Stock-Yogo critical value for maximum acceptable bias relative to the OLS estimator (Stock & Yogo, 2005). Second, the coefficients remain statistically significant after adjusting according to the valid t-ratio approach developed by Lee et al. (2022), where the F-statistic must be above 104.7 to ensure significance at the 5% level compared to OLS.

Moving to the second stage, we find, again across a variety of specifications, that variation in the presence of Gulag facilities driven by logistical concerns, a credible exogenous variable, is strongly predictive of an increase in the number of severe injuries in these locations. The magnitude of the effect is larger than under selection-on-observables, but there is more uncertainty around the point estimates. Nevertheless,

all estimates are significant at the 95% level.

The main threat to validity, which would be consistent with the increased size of the effects in Table 3.5 compared to Table 3.3, is that memories of repression from neighbouring communities are also cultivated within each community, thus constructing a composite effect of indirect victimisation in one's own community and spillovers from neighbouring regions. To address this, we examine the strength of the effect induced by the presence of Gulag facilities. Specifically, we create a set of binary indicators to determine whether a locality had a Gulag facility in its vicinity. We then control for the presence of the facility in each locality in our main regression models after including the binary indicators. For the exclusion restriction to hold, the effect of the new independent variables should be insignificant. Figure 3.4 confirms that our instrument passes this hoop test: once we control for the presence of a Gulag facility in a locality, the presence of one in the neighbourhood is not statistically significant, largely indistinguishable from the null, and its sign is not stable.



Figure 3.4: Effect of Gulag facilities in the vicinity of a locality

We further confirm this by measuring the distance from each locality to the nearest grid cell, outside the grid cell to which it belongs, and checking whether this area was a high repression area, defined as hosting at least 2, 3, or 5 Gulag facilities. We then control for this binary variable in both the OLS and IV regressions; its coefficient is insignificant, and the main estimates remain virtually unchanged (Tables

B7-B9). Finally, we look at an alternative instrument for the presence of Gulag facilities, which interacts three logistical considerations that the regime likely took into account when deciding where to locate new units: i) the distance from other facilities, ii) the difference in elevation between a locality and its neighbors, and iii) the distance to the nearest railway network. This more stringent instrument thus exploits (quasi-) immutable characteristics of the terrain that are likely to have been relevant to the regime. Table B12 shows the results of estimating this new IV setup with a 2SLS estimator, confirming the previous results, including the magnitude of the causal effect.

3.7 Repression and the creation of anti-regime norms

In the previous section, we show that exposure to political violence can lead to dissent even in high-threat environments. We argue that this is possible because of the anti-regime norms fostered by repression which facilitate long-term opposition (see Figure 3.3). Establishing this mechanism is critical because previous research has shown that, under certain conditions, political violence can instead lead to support for the repressive regime through a phenomenon of cognitive dissonance (Homola et al., 2020). In the absence of data on the political preferences of the Romanian population during the communist regime, we follow an approach similar to De Juan et al. (2024) and test the "observable implications that should be true" (Bennett & Checkel, 2015, p.277) if our theorised mechanism is present. While this is not a doubly-decisive test of our theory against possible alternatives, we show that the cumulative empirical evidence from the proposed series of hoop tests converges towards our preferred explanation (Mahoney, 2012).

3.7.1 Exposure to the Gulag and Communist Party membership

If exposure to the Gulag instilled anti-communist norms, we would expect that people from localities that hosted a Gulag facility would be less likely to become members of the Romanian Communist Party (RCP)²⁰. This is because, if one holds anti-communist values, joining the RCP would imply a strong

²⁰Unlike other communist parties in Eastern Europe, membership of the RCP was compulsory for anyone in a supervisory position in relevant industries, even if they only supervised one other person in a company. By December 1989, the RCP had

falsification of one's preferences (Kuran, 1997), which would entail a high moral cost (Kuran, 1997). While RCP membership was mostly determined by factors unrelated to political preferences (e.g. being a requirement for certain jobs), differences in membership levels between regions facing the same structural constraints may reveal differences in preferences for the regime (Gilison, 1968).

To test whether the presence of Gulag facilities leads to lower RCP membership, we rely on the Life in Transition Survey (LiTS) III, conducted by the European Bank for Reconstruction and Development in 2016 in 34 countries, including Romania. Three items ask whether the respondent's mother, father or other family members had joined the Communist Party before 1989. Taking advantage of the representative structure of the data, we aggregate the responses at the county level and calculate the likelihood that an individual's relatives in a given county had been members of the RCP²¹.

We then regress these indicators on our main independent variable, which measures the presence of a Gulag facility in each locality. To avoid the effect being driven solely by the strength of the repressive apparatus in each county, we control for the county-average likelihood that one's relatives were pressured to become informants for the secret police. We compute these values from additional LiTS III items. We also include in our model population density, a flexible function of latitude and longitude, and grid cell fixed effects to account for spatial mismatch between the locality and the county.

Table 3.6 shows the results of these regressions. For all three items, the presence of a Gulag facility in one's own locality significantly reduced the likelihood of a relative being a member of the RCP, in line with our theoretical expectation. Given the high baseline levels of RCP membership in all counties and the small differences between counties, this is a difficult hoop test of our theorised mechanism and provides significant evidence in support of its validity (Goertz, 2006). Moreover, we find no significant effect, positive or negative, of the presence of Gulag facilities on the Securitate's efforts to turn citizens into secret police informants, which serves as a proxy for the strength of communist repression after the Gulag closed circa 1965 (Table B10). We interpret this as evidence against the alternative theory that the

close to 5 million members. This is different than, for example, East Germany, were joining the communist party was limited to 'meritorious' individuals.

²¹As a caveat, LiTS responses were not available for some counties, which could bias our results if these specific regions were either hot-spots of resistance or had high levels of tolerance for communism. However, at a first glance, these counties are distributed all across the country, in place with both low and high concentration of Gulag facilities.

DV: Membership in the Romanian Communist Party						
	Mother	Father	Other relative			
Gulag facility	-0.001**	-0.001**	-0.001**			
	(0.014)	(0.016)	(0.049)			
Covariates	\checkmark	\checkmark	\checkmark			
Grid cell FEs	\checkmark	\checkmark	\checkmark			
N. 2684 2684 2684						
Notes: P-values for Conley standard errors in brackets.						

Table 3.6: Exposure to the Gulag and membership in the Communist Party

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.01

presence of the Gulag in the pre-1965 period led to higher levels of covert repression in the post-1965 period, which could have also explained the protest dynamics in 1989.

3.7.2 Exposure to the Gulag and economic grievances

Since repression cannot continue indefinitely without increasing the chances of regime change, autocracies often seek legitimisation by improving the socio-economic conditions of the population (Gerschewski, 2013). However, if public consent is tied to development, then a break in the government's ability to increase social welfare can disrupt legitimisation and trigger anti-regime reactions (Gerschewski, 2018). If the mechanism we theorise is present, we should see even stronger responses driven by economic grievances in places with a history of repression. In other words, social norms should moderate the relationship between economic grievances and dissent. Thus, empirically, we regress the number of severe injuries during the Revolution on an interaction term between the presence of a gulag facility and a variable proxying for economic grievances.

For the latter, we compile an original geocoded dataset of firms established by the communist regime after 1945 at the locality level. On the face of it, the presence of communist-established firms should improve the regime's standing with the local population and potentially reduce citizens' willingness to dissent. In Romania, however, the regime's goal of distancing itself from international markets and adopting

the North Korean model of economic insulation meant that the entire economic apparatus was geared towards exports in order to pay off outstanding debts (Murgescu, 2016). The extreme austerity measures adopted to achieve this had disastrous consequences: while annual growth averaged 11.2 % in the 1970s, in line with a social contract based on development, it fell to 2.6 % in the 1980s. This decline peaked in 1989, when the Romanian economy contracted by 5.8% of GDP, largely due to a government-sponsored crackdown on domestic consumption, including basic needs such as food, healthcare or housing (Ban et al., 2012).

In the collective memory, the last decade of the communist regime is epitomised by empty shelves in shops, food ratios, increased workload and intensity without additional remuneration, and lack of even the most basic medical care despite seemingly booming industrial capacity (Tănase, 2014). In line with the well-established theory that economic grievances drive political participation (Kern, Marien, & Hooghe, 2015), we would therefore expect firms to serve as symbolic reminders of the inability of the regime to fulfill its obligation upholding social order. Therefore, people living in localities that host a firm that served their needs before the 1980s should have stronger grievances because they have 'lost something', as opposed to people from less industrially and agriculturally productive places for whom the perceived welfare has changed less. In accordance with this theory, we should observe that people from localities where a firm is located should have developed stronger anti-communist feelings, which should manifest themselves in the form of more severe injuries.

Table 3.7 reports the results of three different specifications, where the coefficient of interest is that of the interaction term. The only covariates included are the flexible function of latitude and longitude and population density. The estimates confirm that economic grievances increased dissent in 1989 and that exposure to the Gulag strengthened this relationship. Two conclusions emerge. First, these results provide further evidence in support of our mechanism, as it is consistent with theoretical expectations derived from the grievance theory of political participation. Second, it refutes alternative theories based purely on a risk-reward analysis; within this framework, it makes little sense why dissent was stronger precisely in places where grievances were more intense, since these localities did not historically face a more lenient political opportunity structure in December 1989 (Deletant, 2016).

DV: Number of severe injuries from a locality (Mean=0.605)					
	(1)	(2)	(3)		
Firm	0.617***	0.603***	0.617***		
	(<0.001)	(<0.001)	(<0.001)		
Gulag facility X Firm	1.736***	1.740***	1.736***		
	(<0.001)	(<0.001)	(<0.001)		
Covariates	Х	\checkmark	Х		
Grid cell FEs	Х	\checkmark	Х		
Spatial lags	Х	Х	\checkmark		
N.	3180	3180	3180		

Table 3.7: Economic grievances and the memory of political violence

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.01

3.7.3 Exposure to the Gulag and post-1989 political behaviour

Previous literature has shown that attitudes and behaviours shaped by the experience of communism can survive long after democratisation (Pop-Eleches & Tucker, 2020; Simpser et al., 2018). Thus, if the Gulag did indeed foster anti-communist norms, we should be able to observe traces of these norms even after the regime change in 1990²².

First, we examine whether localities that hosted a Gulag facility experienced more anti-government protests in the early years of transition, up to 1992, when relevant government portfolios were primarily held by former communist apparatchiks. In addition, many government actions were seen as antidemocratic, and some dissidents even accused the leadership of a slow return to autocratic practices through political violence (Gledhill, 2005). To collect a comprehensive sample of anti-government protests in Romania, we first compiled a preliminary list based on the Mass Mobilization Project. We then searched the local press for each of the protests mentioned, and kept them in our sample if they featured an anticommunist message, even if it was not central to the manifestation. Finally, we geocoded the protests, which allowed us to run a Poisson regression of the protest count in each locality on the presence of a

²²This is particularly likely in the Romanian case, given that in the absence of lustration laws in the aftermath of the Revolution, many of the post-communist political elites of the 1990s were still former officials of the PCR (Stan, 2006).

Gulag facility, together with spatial controls and adjustment for population density. Table 3.8 confirms that localities that hosted a Gulag facility had a higher number of protests after 1990.

DV: Number of anti-governmental protests, 1990-1992 (Mean=0.023)						
	(1)	(2)	(3)			
Gulag facility	6.298***	3.581***	0.534***			
	(<0.001)	(<0.001)	(<0.001)			
Covariates	Х	Х	X			
Covariates	Х	\checkmark	Х			
Spatial lags	Х	Х	\checkmark			

Table 3.8: The effect of the Gulag on anti-governmental protests post-1990

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.01

Next, we examine whether people from places that hosted a Gulag facility were less likely to support candidates with an overtly communist past in presidential elections. We have chosen two relevant elections that are best able to reveal anti-communist voting patterns, which in turn, we argue, reveal persistent anti-communist norms. First, we look at the 1992 elections, which saw the first major confrontation between a reformist pro-democracy alliance and the direct heirs of the RCP, led by Ion Iliescu, the former communist youth leader once seen as Nicolae Ceausescu's likely successor (Tismaneanu & Kligman, 2001). Second, we look at the 2004 elections, where Adrian Nastase, the leader of the Social Democratic Party, was portrayed by his rival as a perpetuator of corrupt communist practices. While other elections have also included debates about the communist part, these two have focused on the issue and thus primed citizens to reflect on their autocratic experiences. We look at the first round of these two elections, when voters have more opportunities to reveal their sincere preferences rather than vote strate-gically. Table 3.9 shows that localities that hosted a Gulag facility had lower vote shares at the county level for presidential candidates considered to be successors to the communist regime, in line with our theoretical expectation.

Taken together, these results (Tables 3.8-3.9) show that behaviour consistent with anti-communist norms that persisted after the demise of the communist regime is more likely to be observed in places

DV: Vote share for the 'communist' successor candidate				
	(1)	(2)		
Gulag facility	-5.071** (0.021)	-3.528*** (<0.001)		
Covariates	\checkmark	\checkmark		
N	3180	3180		

Table 3.9: The effect of the Gulag on anti-communist voting patterns post-1990

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.1

that hosted Gulag facilities. Moreover, alternative theories that would explain the dynamics of protest in 1989 solely in terms of the political opportunity structure, and hence the risk-reward analysis of dissidents, would fail to explain the consistent correlation between the Gulag experience and revealed political behaviour after the fall of communism. Similarly, theories that deny the role of anti-communist norms and focus on the heterogeneous response of the Securitate in December 1989 would fail to explain why the same patterns of dissent resisted post-democratisation.

3.8 Pathway case analysis

To illustrate how anti-regime norms have motivated Romanian citizens to engage in intentional dissent during December 1989, we conduct theory-testing process tracing in a singular pathway case (Ulriksen & Dadalauri, 2016). Pathway cases are those where both the causal condition of interest (i.e. the presence of a Gulag facility in a locality) and the outcome (i.e. severe injuries from the locality) are present, and the causal effect is of significant magnitude (Gerring, 2007, p.238-239)²³. Their diagnostic power derives from a simple consideration: if the mechanistic argument underlying an identified causal effect is correct, then the crucial case in which the causal effect is strongest should provide the clearest instance of how that

²³Since pathway cases only become apparent after the cross-case causal inference was performed, they are by design diagnostic tools aimed at further exploring the depth of the causal relationship, not at expanding its breadth (Gerring & Cojocaru, 2016, p.405-406)

causal mechanism operates (Eckstein, 1975; Gerring, 2007)²⁴.

One major challenge for selecting an appropriate pathway case is accurately determining the magnitude of the causal effect of the Gulag on severe injuries in each Romanian locality. In this regard, most pathway cases are usually selected through heuristics or predictive algorithms rather than causal inference methods (Gerring, 2016; Weller & Barnes, 2014), limiting their validity of results. Instead, we propose a novel algorithm in which causal forests are used to approximate the causal effect in each locality, allowing for their subsequent ordering and finally, for case selection²⁵. We calculated the conditional average treatment effect (CATE) as an approximation of the causal effect of the Gulag in each of the 3,181 localities in Romania, and then, following a theory-driven assessment of which of the localities with high CATEs might produce generalisable results, selected Timisoara, the city where the Revolution began, as a pathway case²⁶.

We follow the best practices in process tracing established by Collier (2011) and Ricks and Liu (2018). We first establish a timeline of events in December 1989, and then explain why this particular unfolding is better explained by considering anti-regime norms based on the logic of appropriateness, rather than two alternative theories: (i) a major shift in the political opportunity structure exploited by dissidents with exceptionally low levels of risk aversion, and (ii) a favourable dynamic of the repressive apparatus of the communist regime.

²⁴In other words, the visibility of the causal mechanism needs to be monotonic in the treatment effect, which makes the presence of the theorised causal mechanism in the pathway case a necessary condition for claiming the more general presence of that mechanism in the population of cases, and its absence strong evidence for rejecting the original theory.

 $^{^{25}}$ Causal forests are algorithms that aim to accurately measure how the effect of a causal factor X varies across the sample, while correctly predicting the value of the causal effect induced (Athey & Imbens, 2016; Athey, Tibshirani, & Wager, 2019; Wager & Athey, 2018). While random forests are ensemble learning methods based on constructing a large number of decision trees in order to minimize the prediction error for an outcome Y between the leaves of each tree (Breiman, 2001), causal forests attempt to simultaneously maximize the difference in treatment effects between the leaves while accurately estimating the average treatment effects (Wager & Athey, 2018). This dual objective distinguishes causal forests from many other ML-based causal inference tools that explicitly address only the prediction problem.

²⁶The theory-driven selection, rather than relying solely on CATEs, was undertaken to ensure that the selected pathway case was informative of a wider population of cases, an imperative for case study research (Gerring, 2004).

3.8.1 The chronology of the Revolution in Timisoara

Between 13 and 15 December 1989²⁷, the first signs of unrest appeared in Timisoara. Citizens belonging to the Reformed Church gathered in a peaceful vigil to support the local pastor, Laszlo Tokes, against the regime's politically motivated attempts to relocate him. This action came after stories of Tokes being stabbed in front of his wife and daughters by masked assailants, believed to be members of the Securitate, had spread through the city. The crowd was multi-ethnic, included people of different ages and, crucially, people with well-paid jobs who came with benefits in kind. Timisoara's communist mayor addressed the crowd and asked them to disperse, but this was unsuccessful and instead some members of the demonstration began chanting pro-democracy slogans.

As the protests continued on December 16, police used water cannons to disperse the crowds and then began using physical force, beating people who appeared to resist. As more people gathered, the communist mayor attempted to convince the core dissenters that he would reverse the decision to evict Pastor Tokes. By this point, however, the demonstrations had gained momentum, with people chanting for freedom, liberty, the fall of authoritarianism and, symbolically, Romania's former national anthem, which had been banned by the communists. On that day, many workers and students joined the movement against the dictator, which led to direct reprisals by both the police and the Securitate, who beat and killed dozens of demonstrators.

On 17 December, regime forces in Timisoara were armed with war weapons and ordered to engage in active combat with the demonstrators who had occupied the city's central squares, while also attempting to break into the Communist Party headquarters. However, as some Securitate leaders admitted in interviews after 1990, the local forces were understaffed and unprepared for such protests. Moreover, most of them had little experience in suppressing violence, as such events had not occurred for decades. This was especially important given that protests now included a large proportion of workers from local factories, including higher-ups, who were chanting directly for the overthrow of the regime rather than for

²⁷The timeline of the Revolution in Timisoara is based primarily on the local historical accounts of Balaj (1991), Costinaş (2016), Mioc (2002), as well as the edited volumes of Calafeteanu (2009) and Oşca (2009). Details about the Securitate have been gathered from the various interviews given over the years by former officers of the secret police, as well as from the accounts of C. Lupu (2023).

improved economic conditions, as had been the case in the very small strikes of the late 1970s and early 1980s. All in all, the composition of the repressive apparatus was not anymore prepared to resort to violence than in any other part of the country. The first shots were fired at around 5pm near the Orthodox Cathedral, just outside the city centre, and one protester counted over 350 bodies on the ground.

Nicolae Ceausescu left the country on 18 December for a diplomatic visit to Iran and declared martial law in Timisoara. Before leaving, Ceausescu instructed the local RCP leaders to suppress the ongoing demonstrations, but did not reinforce the repressive apparatus in any meaningful way. This decision, to leave the country amid dissent, ultimately did not reduce the scale of the protests, but increased their symbolism, as dissidents now carried national flags with the communist emblem cut out, sang patriotic songs that had been banned since 1947, and chanted slogans referring to the crimes of the communist regime, including the mass deportation of local people to the penal colony of Bărăgan, as well as the atrocities in the local detention center. Local protests by factory workers turned into a general strike, with the army killing dozens of workers who refused to return to work despite assurances of higher wages. As the death toll mounted, the Securitate was tasked with removing bodies from morgues and hospitals and transporting them to Bucharest for disposal to hide the consequences of the violence. This dynamic continued the next day, but by this time martial law had completely failed and thousands of people gathered in the streets, even though the army and the Securitate were still using military equipment against them.

The next two days, 20 and 21 December, were the culmination of these local demonstrations. Nicolae Ceausescu gave a national speech condemning the events in Timisoara, which had begun to spread to neighbouring towns, and promised a series of economic reforms that would directly benefit the population and address what he saw as the discontent that was fuelling the dissent. Crowds in Bucharest, who had compulsorily gathered to watch the speech, began booing the dictator; similarly, dissidents in Timisoara, who had heard about the speech and the reactions from the live audience, continued their efforts against the local authorities. Tens of thousands of people had gathered throughout Timisoara, and the mayor, along with several army and Securitate generals, were tasked with negotiating with the dissidents.

After the events in Bucharest, the ongoing protests became a national issue and most citizens heard

about the clashes with the regime; this had not been the case before, as what had happened in Timisoara remained unknown to a large part of the population. This only changed when university students went on their Christmas holidays and, on their return to Western cities, from Bucharest, found out about the ongoing situation and spread the news to their friends and acquaintances²⁸. Protests throughout the country were met with limited resistance from the regime force, especially after 21 December; this happened as the violence seemed not be deter the growth of the movements, or their spread in neighbouring localities.

On 21 December, trains full of workers from other parts of the country arrived in Timisoara in a lastditch attempt by the regime to crush the protests. This happened precisely because the Securitate was short of personnel for demonstrations of this size, and some parts of the repressive apparatus began to refuse to cooperate with their superiors, considering joining the protests or at least not getting involved. But when the workers brought it to compensate for the understaffed saw that the demonstrators were not destroying the city, as their factory bosses had told them at the order of the local RCP leaders, they joined the rallies. In the afternoon, the dissidents organised themselves into the Romanian Democratic Front, with the aim of reforming the internal regime through democratisation. Seeing what was happening, a large number of the military laid down their arms and joined the demonstration. By the end of the day, the people of Timisoara declared themselves the first Romanian city free of communism. The Revolution continued throughout the country until Nicolae Ceausescu was executed on Christmas Day.

Three elements are crucial based on this chronology. First, the protests were dominated by anticommunist messages rooted in democratic principles and the memory of historical repression, rather than by material matters or strategic considerations. Second, dissidents were unwilling to cooperate with the authorities, even when there might have been some personal benefit; this was true throughout the period, despite variations in the level of repression. Third, and most importantly, the size and scope of dissent increased over time, despite the rapid and significant increase in state-sponsored violence. The

²⁸The fact that people in Bucharest were uninformed about what was happening in Timisoara and the surrounding towns was confirmed to us in three interviews with people who were students at the Law Faculty of the University of Bucharest in December 1989. According to their recollections, this was the first time that the trains home were monitored by the military, but they were not told why. When they arrived home in western Romania, their families told them what had happened, which they had learned from Radio Free Europe.

scope of the protest was monotonically increasing, not decreasing, in the level of repression. Even if the dissidents had an unprecedented threshold of tolerable violence, the increase in the size of the protests cannot be reconciled with the increase in violence. Fourth, the composition of the repressive apparatus seems to have been orthogonal to the outcome of the protests, especially in Timisoara and the other so-called Martyr Cities where the main protests occurred. In practice, based on the recollection of the generals tasked with repression during December 1989, this meant that the number of injuries in deaths was primarily a function of the motivation of the participants, rather than of the degree of violence condoned by their local command offices.

Taken together, these facts clearly point to a chronology that is more likely to occur in an environment where protesters are driven primarily by norms and moral obligations rather than strategic considerations of the threat environment. In such cases, seeing people willing to sacrifice themselves in the name of shared anti-communist values could motivate others to follow their actions, as the early dissidents embodied the archetype of anti-communism to which community members aspired.

3.8.2 Further empirical evidence from Timisoara

The existence of anti-communist norms in Timisoara, shaped by communist violence and the Gulag experience in particular, is well documented by local historians (Costinaş, 2016; Neumann, 2007; Sitariu, 2004). From the outset, the Western part of Romania was one of the most resilient regions, as communism tried to stifle the inter-ethnic civic ethos that had been cultivated by the Habsburg Empire for over a century (Buzărnescu, Pribac, Neagu, & Buzărnescu, 2004). Many citizens of German origin were sold by the communist regime to the Federal Republic of Germany for profit, while minority rights were suppressed to ensure a uniform attitude among citizens. This proved to be a failure over time, mainly due to the mobilising role of the remaining minority churches, which continued to foster communitarian values until the Revolution, which was triggered precisely by an act of repression against a Hungarian ethnic (Mioc, 2002).

The Gulag experience, especially the mass deportations from Timisoara and its surroundings to the Bărăgan plain, which served as a penal colony, increased the hostility of the population towards the regime

(Marineasa, Vighi, & Sămănță, 1996; Spijavca, 2004). Some of those who returned after several years of this extrajudicial punishment spread details of the violence they had been subjected to, prompting a student movement that attempted a large-scale protest in support of the 1956 Hungarian Revolution (Sitariu, 2004). This was the only protest of its kind in 1956 in Romania and was immediately crushed by the authorities, with dozens of students sentenced to prison, many of them to be executed in special political prisons that were part of the Gulag network, including in Timisoara (Sitariu, 2004).

Notably, anti-communist values were not a direct emanation of the economic discontent of the population, as Timisoara and western Romania in general remained relatively developed (Jurma, 1994). Moreover, the anti-communist protests of 1956 took place against a backdrop of rapid growth and improved access to health services and education. However, these material factors did not prevent demonstrators from explicitly demanding "Communists to leave the country" (Sitariu, 2004, p.48).

These attitudes remained latent for most of the following decades, especially after Nicolae Ceausescu took over the communist part of Romania and moved from the Gulag model to one based on selective terror by the Securitate. However, as soon as the Soviet Union announced its policy of openness and transparency under Gorbachev, underground movements quickly emerged in Timisoara. Throughout 1988 and 1989, they painted numerous graffiti with anti-regime messages on the Communist Party head-quarters, and anti-Ceausescu manifestos were distributed throughout the city (Costinaş, 2016). Such acts of symbolic dissent intensified as the citizens of Timisoara became aware of the revolutionary wave spreading through Central and Eastern Europe and became an integral part of the repertoire of emerging resistance throughout 1989. The manifestos, whose existence and circulation became an undeniable reality in October-November 1989, focused less on the disastrous economic situation and more on the need for a new political regime based on the ideals of freedom, democracy and genuine popular sovereignty (Costinaş, 2016).

Any rival hypothesis would require that anti-communist social norms would be less important in the decision-making process of potential dissidents than the cost-benefit calculations they might make. Pragmatically, this would mean that the economic grievances of Timisoara's citizens were sufficiently high compared to the costs of violent repression and that the chances of improved material conditions as

a result of dissent were positive. This is unlikely to have been the case for at least three reasons.

First, the citizens of Timisoara had access to Western media in the form of Radio Free Europe and the Voice of America, along with Yugoslav television and radio. They learned how the small strikes of miners and workers in the 1970s and 1980s had been crushed by the Ceausescu government and, therefore, that any attempt to negotiate with the communist authorities for better working conditions or welfare provisions would be fruitless. If we assume that people are rational, then the citizens of Timisoara, who had comparatively more information about the environmental threat as well as its negotiating tactics, should not have been the ones to trigger a revolutionary movement in December 1989. The fact that they did so proves that they acted against rational calculations, not because of them.

Second, even if a rational calculation had triggered the events of 15 December, this would not explain why the protests continued into the following week. In the first few days of the demonstrations, it was clear that the chances of the regime being overthrown were close to zero, the violence of the army and the Securitate was increasing in scope and scale, and yet more and more people were joining the crowds protesting against the regime. From a rational point of view, this made no sense, as more information about the high-threat environment and low chances of success should reduce participation, not increase it.

Third, the protesters directly rejected any compromise based on higher wages, pensions or benefits in kind and instead drew up a list of demands on political issues, including freedom of speech, assembly and religion (Costinaş, 2016). This is not to argue that the dire economic situation was not a distinct cause of the Revolution, but rather that the straw that broke the camel's back was a normative offence committed by the regime, one that had nothing to do with the individual welfare of the citizens of Timisoara and that in no way changed the political opportunity structure. One of the most famous cases illustrating this is the story of Daniel Gavra, a local railway worker who lost his leg fighting against the Securitate. Gavra said he "lost a leg, but lit the first candle" because he was responsible for distributing candles for Laszlo Tokes' vigil on 16 December (Elliott, 1990). Gavra's story quickly spread among the demonstrators, who continued to light candles while he was in hospital - proof of how quickly the community was able to embed the narrative of communist violence and retaliate in a unified way.

These points demonstrate that the logic of consequences cannot fully explain the 1989 Revolution in Timisoara. On the other hand, the local history of anti-communism, shaped by the experience and memory of political violence, lends credence to the theory that in high-threat environments, the logic of appropriateness can lead people to engage in dissent in response to repression. The centrality of anticommunist norms is further highlighted by some of the interviews cited more extensively in the next sub-section, as for some revolutionaries from Timisoara the main driver of political action was to fight the criminal regime for what it had done, not to engage in regime change, which was highly unlikely, especially in the early days of the Revolution. Recognition of the potential martyrdom was also essential in the final stages of the demonstration, when the military realised that the people were not ready to return to the status quo, and for the decision of workers from other parts of the country, brought in by the communist regime, to fight alongside the people of Timisoara (Mioc, 2002). The messages spread by the anti-communist crowd reflected the content of earlier manifestos spread in the city in the first months of 1989. Particularly from 20 December onwards, dissidents listed "oppression", "tyranny", "lack of freedom" and "lack of dignity" before "poverty", as part of their communication of demands (Costinaş, 2016, pp.3-4).

3.8.3 External validity of the qualitative evidence

The case of Timisoara credibly demonstrates, through process tracing, that the Revolution was primarily driven by anti-regime norms developed within the community. The final question is whether this conclusion can be generalised to other localities where major protests took place in December 1989. To provide some evidence for this claim, we conducted an online survey of participants in the 1989 Romanian Revolution, asking them what factors shaped their involvement in active combat. The purpose of the survey was threefold. First, to assess whether respondents considered the period between 16 and 25 December to be a high-risk environment. Second, to assess whether dissidents took the high-risk environment into account when deciding to dissent and, if so, whether it was a central concern. Third, to determine whether identity considerations rooted in anti-communist norms influenced their decision to dissent. When taken together, we aim to show that dissidents in December 1989 against despite acknowledging



Figure 3.5: Determinants of the decision to participate in the Revolution of 1989 Note: The different graphs in this Figure show the responses to the question "Did you take part in the revolution because of X?"

the detrimental POS, due to normative considerations. These responses provide a proof of concept for our theory and validate our empirical setting as a typical case suitable for further empirical investigation.

We reached people via Facebook, through community groups of self-proclaimed revolutionaries, one of the most widespread and active platforms for those still alive to communicate. We first selected all groups moderated by people who were known participants in the Revolution and had no legal history of falsifying their status, and then cross-validated their appropriateness in qualitative interviews. The survey was designed and administered through Qualtrics and remained active between 20-25 November 2023. For each respondent, we manually verified their actual involvement in the events of December 1989, first by confirming their status as revolutionaries against publicly available lists, and then by checking existing case files from the Ministry of Justice to see whether they had been or were currently being prosecuted for allegedly falsifying their participation in the Revolution in order to gain material benefits, a common

practice in Romania. We dropped one observation because we could not verify their status and one because of their legal history. The role of the survey is not to provide information about the Romanian population through a representative sample, but rather, in line with the goals of process tracing, to learn about the experiences of the actual participants and to draw relevant conclusions about their motivation.

Figure 3.5 shows the results of this small survey, with N=101, roughly 2% of the people actively involved in the Revolution. The overall conclusion is that the majority of respondents appear to have acted according to a logic of appropriateness rather than that of consequences, in line with our proposed argument. In deciding whether or not to take part in the Revolution, most of them weighed up whether it was the right thing to do and whether they had a duty to take part, and the answer was overwhelmingly yes to both questions. Fewer respondents, but still a significant majority, confirmed that anti-communist norms were part of their identity, in line with our theory of why people rely on normative considerations when deciding whether or not to engage in dissent. Crucially, most respondents came from outside Timisoara, the city where the Revolution broke out, and therefore had time to learn about the regime's reprisals against the demonstrators.

On the other hand, responses were divided as to whether dissidents were concerned about the personal threat posed by dissent. However, for the majority of those who calculated the risks of political action, these considerations seemed to be outweighed by considerations of appropriateness. Respondents who considered the level of threat generally opted to act regardless of the likely outcome. Finally, and crucially for demonstrating that risk-reward profiles were not central to their decision-making process, a large majority did not consider regime change to be possible, yet were willing to incur the costs of dissent. This rules out the possibility that the Romanian population had an extraordinarily high threshold for tolerated violence, which fuelled their action with the ultimate goal of overthrowing the regime. As the in-depth interviews we conducted in parallel showed, many were willing to sacrifice themselves to make a statement.

Most of these semi-structured interviews took place between October and December 2023. Their purpose was not to achieve information saturation on the topic, but to further confirm instances where dissidents in high-risk environments operate within the logic of appropriateness. In particular, we wanted

to go one step further and show that the reason why anti-communist norms were so salient and engendered a logic of appropriateness was a history of extreme political violence in the form of the Gulag.

First, the interviews directly show that the memory of political violence has shaped how citizens identify themselves. In the words of one protester who mobilised against the regime in the early days of the Revolution: "After what they [the communists] did to my family, I finally had a chance to get them" (Interview, 25 October 2023). Similarly, a protester who took to the streets of Bucharest long after the regime's willingness to use indiscriminate repression had become public knowledge told us: "The communists made life hell for years, how could I watch, how could I look my children in the eye after that?" (Interview, 27 October 2023). Another protester from Timisoara was even more direct: "We were not there for more bread, we were there for justice" (Interview, 22 December 2023).

Second, we found that while risk considerations remained in the minds of citizens, the idea of living with the memory of a missed opportunity overrode any rational calculation; in other words, dissent was the situationally appropriate option. As one protester in Bucharest put it: "I could have died, but so could some of them. That was good enough for me." (Interview, 3 November 2023). The idea that personal sacrifice was taken into account despite the fact that there was nothing remotely certain about the Revolution's ability to remove the dictator was a common theme. One protester from Arad, the second city where the Revolution broke out, explicitly linked the decision to dissent to his family's experience in the Gulag: "They sent my grandfather, who was a supporter of the Peasants [National Peasants' Party], to the Canal. My grandmother was left alone with five children, they were poor, but they were still harassed because they were bourgeois. Bourgeois, but without bread... When I had the opportunity to do something, I felt I owed it to them" (Interview 20 November 20-23). Similarly, a protester from Timisoara revealed: "My family was sent to Bărăgan [mass deportation colony], I wanted to shout their pain, my own didn't matter" (Interview 26 October 2023).

3.9 Conclusions

Why do some people engage in dissent in response to political violence, even in high-threat environments? To answer this question, we have proposed an extension of current theory to include the logic of appropriateness as an underlying rationale for political action in authoritarian regimes. That is, we have argued that while dissent has traditionally been analysed by quantitative scholars through the lens of rational choice, with dissidents weighing the likely costs of repression against the benefits of overthrowing or weakening an authoritarian regime, this cannot explain all cases of dissent. In particular, it fails to explain why people would engage in protest against a violent regime, especially when the chances of those protests succeeding in remedying underlying grievances of the population are low.

In such cases, we argue that we must consider dissent as the appropriate response of people who bear the burden of victimisation to violence. If people have been socialised by various community organisations to view the regime as an out-group responsible for harming the community, then any opportunity to express their identity through action against the regime will be a significant action that complements any potential rational calculations.

To test our theory, we looked at how people from Romanian localities that hosted Gulag facilities, such as extermination centres or penal colonies, had more people who were severely injured during the anti-communist Revolution of 1989. We have demonstrated that localities which hosted Gulag facilities experienced significantly higher levels of dissent during the 1989 Revolution, as measured by the number of severe injuries. This relationship persists across various model specifications, estimators, and robustness checks and sensitivity analyses. Our results suggest that exposure to extreme political violence can foster enduring anti-regime norms within communities, which in turn motivate dissent even in the face of significant personal risk. The mechanism we propose - the formation and persistence of anti-communist norms - is supported by several lines of evidence. We found that localities with Gulag facilities had lower Communist Party membership, stronger reactions to economic grievances during the Revolution, and continued to exhibit anti-communist political behavior in the post-1989 period. Our process tracing analysis of Timisoara, a crucial case in the Revolution, further illustrates how these norms operated to

motivate dissent.

These findings have important implications for our understanding of political behavior under authoritarianism. They suggest that the decision to engage in dissent cannot always be explained by rational choice models alone. Instead, in some contexts, the logic of appropriateness - acting in accordance with internalized norms and identities - plays a crucial role. This is particularly relevant in high-threat environments where the expected costs of dissent would typically outweigh the potential benefits from a purely rational perspective.

This paper also contributes to the literature on the long-term effects of political violence. It demonstrates that the impact of repression can persist over decades, shaping community identities and political behaviours long after the immediate threat has passed. This highlights the importance of considering historical legacies when analysing contemporary political phenomena, particularly in non-democratic societies.

4

Attack on climate: The persistent effects of authoritarianism on the climate equilibria

4.1 Introduction

Tackling climate change is one of the defining political challenges of our time. Yet despite this urgency, the stringency of domestic climate policies varies widely across states, with only a handful of governments demonstrating the level of ambition required to achieve an effective solution in due time (Lamb & Minx, 2020; Mahdavi, Martinez-Alvarez, & Ross, 2022). A key question is whether this state-level heterogeneity in climate policy ambition is due to exogenous variation in the expressed preferences of the voting populations, or whether it is endogenous to the enduring characteristics of domestic political regimes¹. In particular, do democracies foster stronger pro-environmental attitudes among the population, which then translate into electoral demand for ambitious climate action, ultimately leading to more stringent policies?

¹There is a large body of literature using both large-N regression analyses and case studies to explain, at a descriptive or correlational level, whether the type of domestic political regime affects climate outcomes. To the best of my knowledge, no paper has explicitly provided a causal identification strategy to show why these correlational results should be considered causal (Burnell, 2012; Escher & Walter-Rogg, 2023).

In this paper, I examine the reverse question: do authoritarian regimes foster less climate ambition in the long run? To this end, I exploit the quasi-exogenous division of Germany into two states after the Second World War (WWII): East Germany, which became an authoritarian state and satellite of the Soviet Union, and West Germany, which evolved into a liberal democracy. In the 1970*s* and 1980*s*, the East German authorities made a systematic effort to repress environmental movements. These efforts focused on two main objectives: preventing pro-environmental organisations from forming, and cracking down on any spontaneous grassroots environmental movements. I argue that this repression had long-lasting consequences. In particular, it destroyed the organisational infrastructure that people needed to translate their pro-environmental attitudes into meaningful electoral choices (Jones, 1993; Thüsing, 2013).

Using a geographic regression discontinuity (GRDD) augmented with entropy balancing to model selection into treatment, I test whether this differential exposure had a persistent effect on the average level of climate ambition in German districts after reunification in 1990. To do this, I construct a measure of climate equilibrium in each district, defined as the average of political parties' ideological positions on environmental protection, each position weighted by the vote shares these parties received locally.

I confirm that districts from the former East do indeed exhibit significantly lower climate policy equilibria. This means that, given a fixed set of climate policy options, regions exposed to authoritarianism tend to gravitate towards less robust climate regimes. I then confirm that the negative impact of East Germany is not a simple spillover of complementary effects, but has independent causal power. In this respect, I show that East German districts do not vote more for less ambitious climate policies simply because such positions are negatively correlated with their other policy preferences on issues such as welfare (Norris & Inglehart, 2019). Additionally, I show that the negative relationship between the level of repression experience by the population and their climate equilibrium holds even within East Germany, thus alleviating concerns of post-1990 internal migration.

I contribute to three main strands of the literature. First, I build on the growing body of evidence discussing the role of democracy and democratic principles in fostering pro-environmental attitudes among the public (Hanusch, 2017). However, I show that democratisation against a background of historical authoritarianism is not sufficient to induce strong pro-environmental beliefs among the electorate. This

suggests that what matters more than the actual procedural democratic framework is the socialisation of new generations into the principles of political inclusiveness and deliberation, which are known predictors of demand for strong climate action.

Second, I add to the work on the lingering effects of historical political regimes on contemporary political behaviour (Charnysh et al., 2023; Cirone & Pepinsky, 2022), in particular communism (Libman & Obydenkova, 2021; Pop-Eleches, 2007; Pop-Eleches & Tucker, 2017; Simpser et al., 2018). It is no longer a novel claim to say that contemporary political practice is built on centuries of historical baggage. What I demonstrate, more interestingly, is that even when I disentangle the various political conflicts that are correlated within the electorate, historical legacies appear to be strong enough to shape each conflict independently. This is all the more remarkable in the case of climate policy, which is now recognised as a mainstream issue, but was long a niche political issue.

Third, I show that lower levels of climate policy equilibria are associated with higher levels of polarisation on the same issue, demonstrating how the increased salience of environmental protection also makes the topic more fertile for disagreements within the electorate. Political polarisation has become one of the buzzwords of current political science, with fertile research conducted on this topic (Carothers & O'Donohue, 2019). Despite this status, however, we know much more about how much polarisation exists than about the precise issues that generate polarisation.

4.2 Theoretical argument

By now, there is a broad recognition of the fact that differential exposure to a history of political violence can account for some of the regional variation in political behaviour (Charnysh et al., 2023; Cirone & Pepinsky, 2022). This is particularly true in the case of authoritarian regimes, which in their ongoing efforts to extract loyalty from citizens steadily change the relative value of political preferences (Neundorf & Pop-Eleches, 2020). In some instances, attitudes instilled by authoritarian institutions persist decades, if not centuries, after the collapse of the regime (Fouka & Voth, 2023; Grasse, 2023; Rozenas & Zhukov, 2019). However, while scholarship has progressed in identifying such historical critical junc-

tures and the path dependencies they trigger, less has been accomplished in providing credible accounts of the mechanisms through which persistent effects propagate (Haffert, 2022; Neundorf & Pop-Eleches, 2020). While the absence of robust mechanistic evidence is not sufficient to reject a causal claim (Gerring, 2010, pp.1505-1506), the interpretation of causal estimates in the light of a postulated theoretical argument is an abductive exercise that is much more credible when discussed not only in terms of what the legacy of authoritarianism is, but also how that legacy was formed.

To link how authoritarianism might stifle long-term demand for ambitious environmental policies, I propose investigating a mechanism that operates at the meso level, through political and societal organisations. Following previous research, such as Haffert (2022), we know that the nature of the authoritarian regime's engagement with such organisations significantly shapes the way in which the latter organisations contribute to the socialisation of individuals. I argue that the formation of community-wide demand for environmental policy requires the presence of organisations whose main objective is precisely to increase the salience of the environment as a topic of mainstream political debate for a considerable segment of the population.

4.2.1 The role of the pro-environmental milieu in structuring attitudes and preferences

There are at least two reasons why a pro-environmental milieu is necessary for the emergence of strong pro-environmental attitudes among citizens, thereby increasing the demand for ambitious policies.

First, environmentalism is symptomatic of the post-materialist turn in advanced economies, which requires a reasonable level of economic and existential security (Norris & Inglehart, 2019). Achieving and maintaining these preconditions, however, is subject to high-dimensional electoral competition, with the discourse on first-order political cleavages consuming a large share of the public attention (Albright, 2010). Therefore, given the very narrow window of entry for another distinct political conflict (Kenny & Langsaether, 2023), it is difficult for environmentalism to break into the political mainstream organically, through demand-driven channels, especially as it has a lesser ontological status than issues such as economic growth or military engagement. Moreover, pro-environmental attitudes among the popula-

tion appear to be strongly correlated with a specific set of socio-demographic characteristics (Böhmelt & Zhang, 2023; Fisher & Nasrin, 2021), which makes mobilising this group even more challenging, as it involves not only activating preferences, but also transforming a spatially disaggregated network of citizens into a politically stable nucleus.

Instead, the mainstreaming of environmentalism is usually achieved through supply-side mechanisms, with deliberate agents structuring political debates around this issue. Such organisations contribute to the formation of a pro-environmental identity and, as they grow in size and scope, to the affirmation of this identity as a legitimate complement to class, ethnicity, religion, etc. (Jans, 2021). Crosscountry evidence from Western Europe supports this theoretical argument - the history of political environmentalism is inextricably linked to the rise of green parties, which themselves grew out of grassroots movements over decades (Bomberg, 2005, pp.12-21). These organisations, developed since the 1970s, have exploited the political opportunity structure whenever possible to ensure the presence of environmentalism in the political space (Van Der Heijden, 1997).

Moreover, even in societies where environmentalism entered the political space, meso-level organizations are still required to maintain the salience of this topic. This is because, at any point, citizens are only proactively concerned about some policy issues and not about others. For a political conflict to remain prominent, it has to be brought to the fore by an intentional agent, as exogenous shocks can rarely be conducive to salience over a sustained period of time. Even if such an exogenous shock occurs in the case of environmental concerns, e.g. a natural disaster, the political salience of the environment in relation to that disaster is likely to diminish as soon as another such shock occurs, linked to another political conflict, e.g. a public health crisis (Stoddart, Ramos, Foster, & Ylä-Anttila, 2023). A good example of the role of pro-environmental organizations in maintaining salience is the Covid-19 period, where young activist groups continued to push for ambitious action on climate change, building on the momentum the issue had gained in previous years (Fisher & Nasrin, 2021).

Second, environmental challenges, from local pollution to climate change, often involve the presence of a negative externality for which citizens are partly responsible (Postmes, Rabinovich, Morton, & van Zomeren, 2013). Therefore, mobilising for pro-environmental action assumes that the community is
aware of the nature of the externality and that its members are willing to sacrifice individual benefits in the short term in order to avoid long-term negative consequences for the collective (Steg, Bolderdijk, Keizer, & Perlaviciute, 2014). Theoretical and empirical work shows that individuals' willingness to sacrifice material welfare for the good of the group is a monotonic function of their degree of identification with the group (Bonomi et al., 2021; Shayo, 2009).

However, group identification based on the perception of the environment as a common good is not something that individuals can derive from traditional group identities such as class, ethnicity, or religion. Therefore, there is once again a need for an intentional agent whose mission is to forge a new, concurrent identity centred on the environment (Jans, 2021). This is precisely the function of pro-environmental organizations who, through protest, collective action, and community dialogue, construct and curate the identity-building role of the environment.

Conversely, in the absence of an environmental milieu, environmentalism is less likely to become embedded in mainstream political debates, even in advanced economies. Even if it were to emerge as a result of some exogenous shock that brought environmental concerns to the public's attention, without sufficient organisations to ensure adequate engagement with the issue, this is likely to be a short-lived episode. Finally, the absence of an environmental milieu is linked to the absence of an environmental group identity, which makes it less likely that citizens will demand more ambitious pro-environmental policies from government.

4.2.2 Political regimes and the environmental milieu

The existence of an environmental milieu is necessary for an ambitious environmental policy. The next question is whether the creation of such a milieu, which occurred in most countries in the 1970s and 1980s, was a function of the political regime in each country. A straightforward but persuasive answer would be that democracies, by definition, encourage the public expression of individual beliefs and the coalescence of like-minded individuals into identity-based groups, including groups centred on environmentalism. Conversely, authoritarian regimes stifle the formation of ad hoc organisations outside the scope clearly defined by the state authorities.

Historically, environmentalism has not fallen into this category. Yet, there are two reasons why it is likely that authoritarian regimes will explicitly oppose the formation of an environmental milieu. First, authoritarian regimes have been known to (ab)use the environment for economic growth, particularly in the case of former communist states in Eastern Europe. This was possible precisely because of the absence of a strong and congruent opposition movement to raise concerns about environmental degradation and pollution. When anti-pollution protests became much more common in the 1980s, at the height of the communist drive for industrialisation, the authorities engaged in continuous state-sponsored violence against such civic demonstrations. The leaders of any emerging pro-environmental groups were harassed by the secret police, and the organisations themselves were limited in size and scope, and often closely monitored by the state.

Second, pro-environmental organisations tend to provide a platform for complementary political positions, including minority rights, democratisation and liberal values. Thus, the authorities' political violence against the emerging environmental milieu was driven not only by the need to protect their economic growth model from external ideological attacks, but also by considerations of regime survival.

4.3 Environmental organizations in East Germany

East Germany was officially created on 7th October 1949 in the former Soviet zone of occupation previously established by the Potsdam Agreement. The country adopted a constitution modelled on that of the USSR and morphed into an authoritarian communist regime with one of the most extensive repressive apparatuses in the world (Lichter, Löffler, & Siegloch, 2021).

In keeping with other communist countries of the Eastern Bloc, East Germany pursued a very aggressive industrialisation strategy, focusing on the rapid growth of heavy industry in order to catch up with the more developed Western states (Kopstein, 2000). Although formally committed to environmental protection through progressive legislation, in practice East Germany mined more coal than any other country in the world, dumped all the toxic by-products into the soil and adjacent rivers, devastating almost 50% of all the country's forests, damaging more than 9,000 lakes and poisoning the groundwater

(Jones, 1993, p.236). As a direct result, for decades, and particularly in the 1970s and 1980s, the local population experienced some of the worst pollution levels in Europe, with East Germany being dubbed the "cesspool of Europe" (Tuohy, 1990).

The high levels of pollution led to the formation of individual and collective grievances, manifested in letters of complaint to party officials, protests and civil mobilisation (Horz, Marbach, & Steinert, 2023). As public discontent grew, the communist regime classified all information on environmental damage by 1982, effectively closing down public debate on environmental policy. During this period, a large and powerful environmental movement failed to emerge in East Germany (Jones, 1993). While such groups were not banned outright, they were closely monitored and suppressed whenever they appeared to have the potential to become marginally relevant in shaping domestic policy (Jones, 1993, p.240-242).

The Ministry for State Security (Stasi) was tasked with carrying out this repression, infiltrating and sabotaging virtually all environmental movements, while harassing the leaders of such organisations² (Jones, 1993, p.243). The Stasi's sabotage efforts were extremely effective, with a political core of perhaps only fifty minor groups remaining politically active into the mid-1980s, most of them as part of the local churches and the affilitate organizations. These groups varied in size, averaging between ten and thirty members each, giving a total of less than 2000 citizens (Jones, 1993, p.260), including people involved with other political causes who also supported pro-environmental action. Even in the case of the anti-pollution protests that took place during the last phase of the communist regime, the demands of the population were related to public health rather than consciously questioning the lack of environmental policy in East Germany (Horz et al., 2023).

The East German regime's approach, which concealed rather than addressed the underlying problem, contrasted sharply with political developments in West Germany, where environmental concerns were not only publicly expressed but also coalesced in the formation of a federal green party, Die Grünen, which soon became part of regional parliaments before gaining national traction (Frankland, 1988, 1989). The latter was possible because of the decades-long development of green movements through-

²Ensuring that no large environmental movement takes roots in East Germany was not a contingency, but a central tenet of the regime, given that throughout Western Europe the green agenda was inextricably linked with other pro-democratic values (Muller-Rommel, 2019).

out West Germany, particularly in relation to the use of nuclear power (Schmid, 1987). Notably, the level of repression to which East German environmental groups were subjected was significantly higher than even in other Eastern European communist states (Jones, 1993, p.245).

Decades of political violence sponsored by the East German authorities had a long-lasting effect on the mobilisation potential of pro-environmental attitudes among the population, effects that persisted even after reunification with West Germany in 1990. In unified Germany, the leaders of the former West German Die Grünen attempted to consolidate the voices of the surviving East German environmental movement, but these efforts failed, partly because of the enormous heterogeneity of these organisations, and partly because demand for a green party in the East was extremely low, as the first Federal elections in December 1990 showed (Poguntke & Schmitt-Beck, 1994). Moreover, despite their precarious electoral position, the East German *Greens* stressed the protection of their own ideological autonomy and priorities, which had had little time to develop organically before reunification (Poguntke, 1993, pp.382-383). As a result, a national party, B90/Die Grünen, was not officially formed until 1994, five years after reunification, and remained much more relevant in the former West Germany, where environmental groups were concentrated after reunification.

Drawing on this historical excursus, I argue that East Germany is a typical case described by my theoretical model, which makes it suitable for formally probing my causal argument (Seawright & Gerring, 2008, pp. 299-300). In this case, I have an authoritarian regime that directly suppressed the formation of a pro-environmental milieu. These efforts were very effective, as evidenced by the remarkably small number of citizens involved in such movements, especially when compared to West Germany. The question to which I now turn, and which is central to this paper, is whether the persistent attitudes engendered by the actions of the East German communist regime have persisted over time.

4.4 Data

4.4.1 Independent variable

Following reunification in 1990, Germany became a federal state consisting of 16 states (NUTS1 level³) divided into a total of 401 districts (NUTS3 level). I create a binary indicator for whether a district was exposed to the East German regime (1) or not (0), equivalent to whether the district was located east of the inner border before 1990. Of the 401 districts in Germany, 76 score '1', covering six federal states: Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt and Thuringia.

I exclude Berlin from the analysis because of its complex history of engagement with both the West and East German regimes, which could undermine my causal identification strategy due to the likely inter-regime spillover effects that would render the stable unit treatment value assumption (SUTVA) invalid.

The independent variable is, by construction, a composite treatment that bundles all aspects of the East-West regime difference. In the following section, I explain the steps taken to show that the effect identified supports precisely the theoretical argument proposed.

4.4.2 Dependent variable

In order to numerically characterise the way in which exposure to the East German political regime affected voters' electoral preferences with respect to climate policy, I construct a variable that captures the *climate policy equilibrium* in each NUTS3-level district and for each Federal or European election.

Formally, assume that the German party system consists of $\mathbf{X} = \{X_1, X_2, X_3, ..., X_N\}$ political parties participating in a given election, and that each party has an ideological position I_{X_i} on the issue of climate policy C. These ideological positions are measured before the election and then I measure the electoral performance of all parties by their share of the vote π_{X_i} . Data for these ideological positions

³The Nomenclature of Territorial Units for Statistics (NUTS) is a system used by the European Union for socio-economic analysis. It organises territories into three levels: NUTS 1 denotes large socio-economic regions (e.g. German Länder such as Baden-Württemberg (DE1)), NUTS 2 denotes basic regions for regional policy (e.g. Regierungsbezirke such as Stuttgart, Region (DE11)) and NUTS 3 denotes smaller regions (e.g. districts or 'Kreise' such as Stuttgart, Landkreis (DE111)).

is taken from the Manifesto Project (MARPOR). With these information, I compute E_C , the *climate* policy equilibrium as:

$$E_C = \sum_{K=1}^{N} I_{X_i} \times \pi_{X_i} \tag{4.1}$$

The climate policy equilibrium E_C exhibits four key properties that establish it as a robust measure of electoral preference centrality. First, it weights party positions by their electoral success, ensuring majority preferences are properly reflected while preventing small, extreme parties from exercising outsized influence unless they achieve significant vote share. Second, E_C responds dynamically to shifts in both party ideology and electoral performance between elections, capturing evolving voter preferences over time. Third, its spatial variability across electoral constituencies, driven by different local party vote weights, makes it well-suited for sub-national comparative analysis. Fourth, the measure maintains neutrality regarding party system size, as its calculations depend on vote shares π_{X_i} rather than the absolute number of parties N, enabling valid comparisons across different party system configurations.

There are two major limitations to this concept. First, climate policy equilibria may not be the direct result of voters considering a party's climate policy stance X_i , but a spillover from considering the same party's positions on other issues, such as welfare or trade. Second, the same value of E_C can be the result of different electoral distributions, which would have different interpretations. If these limitations are not addressed, the dependent variable would ultimately boil down to the success of the sole mainstream explicitly green party B90/The Greens. I address both of these shortcomings in later sections, showing how to isolate the part of the mainstream vote that is concerned with environmental issues, regardless of the party of choice.

4.5 Causal identification strategy

For the division of Germany in 1945 to be a valid natural experiment, exposure to the two political regimes must be uncorrelated with the pre-war characteristics of the Eastern and Western regions (Becker, Mergele, & Woessmann, 2020, p.144). While this assumption has been tacitly accepted since the publica-

tion of the seminal paper by Alesina and Fuchs-Schündeln (2007), its validation requires the detailed reconstruction of historical processes (Kocher & Monteiro, 2016), leveraging evidence "on the information, incentives, and capacities of key actors with control over the treatment assignment" (Dunning, 2015, p.17-18). Most notably, Becker et al. (2020) has used an impressive collection of historical micro-data to show that the political negotiations between the Allied Powers over the division of Germany into separate zones of occupation took into account pre-war cultural, social and, above all, economic factors⁴.

Formally, the treatment status of the units is correlated with variables that may also influence the outcome, the climate policy equilibrium. To address this potential source of bias, I first model selection into treatment status explicitly using entropy balancing, and then I exploit the continuity of sociopolitical and economic characteristics of the German districts in a geographic regression discontinuity design (GRDD).

GRDDs are one of the more common identification strategies in the field of historical persistence (Cirone & Pepinsky, 2022, p.149), and coupling them with appropriate preprocessing algorithms is a well-known empirical strategy for dealing with potential endogeneity near the cutoff (L. Keele, Titiunik, & Zubizarreta, 2015).

4.5.1 Modelling selection intro treatment status

I preprocess my sample using entropy balancing, an algorithm which has previously been used to improve the internal validity of regression discontinuity designs when the discontinuity is nut fully exogenous (Hainmueller, Hall, & Snyder Jr, 2015; Morris & Shoub, 2023). Using a pre-specified set of covariates, entropy balancing generates a set of weights for the observations in the sample such that the covariate distributions of the treatment and control groups in the processed data match exactly on all pre-specified moments (e.g. mean, variance) (Hainmueller, 2012). An important property of entropy balancing is being doubly robust to linear regression: if either one of the selection model or the regression model for

⁴This observation is confirmed by the significant discrepancies between what eventually became the inner-German border and the coincidental line of contact between the Soviet and Western Allied armies in April-March 1945 (Martinez, Jessen, & Xu, 2023).

the outcome is well specified, the estimated effect will be consistent⁵ (Zhao & Percival, 2016).

I compile the list of balancing covariates following Becker et al. (2020). First, drawing on Rosés and Wolf (2018), I include an average measure of GDP per capita in each region between 1910 and 1938, together with the average share of employees in agriculture, industry and services. This ensures East-West balance in terms of economic background, which had been highly heterogeneous even at the time of WWII (Wolf, 2009). Next, I include the average number of Holy Communions per the number of Protestants between 1900 and 1910 using data collected by Hölscher (2001), which serves as a measure of religious activity, and thus a proxy for civic engagement in each county (Becker & Woessmann, 2013, 2018). By accounting for this variation, I mitigate concerns about East-West differences in trust, a robust predictor of climate policy attitudes and preferences (Fairbrother, Sevä, & Kulin, 2019; E. K. Smith & Mayer, 2018).

Finally, I include spatial characteristics of each county to limit the influence of geography on whether counties near the inner-German border became part of East or West Germany. The latter directly addresses the possibility that historical regions were maintained when the border was drawn (Becker et al., 2020, p.147), with such regions often being defined by geographic rather than purely administrative boundaries.

4.5.2 Geographic regression discontinuity design

I exploit the fact that most social and politico-economic characteristics of German counties are smooth around the border cut-off points (L. Keele & Titiunik, 2016; L. J. Keele & Titiunik, 2015), but the political regime to which the populations in East and West were exposed before 1990 jumps discontinuously⁶.

The essence of the GRDD is to restrict the comparisons between the Eastern and Western districts to a range of theoretically justifiable regions within which the distance from the inner-German border is

⁵Moreover, in addition to reducing concerns of endogeneity, and particularly about the non-random distribution of German countries on both sides of the border, entropy balancing reduces model dependence (Ho, Imai, King, & Stuart, 2007).

⁶Importantly, the local as-if randomness of the distribution near the border is a sufficient but unnecessary condition for the validity of the GRDD. Instead, it is necessary and sufficient that the characteristics of the counties in the vicinity of the inner-German border are smooth with respect to the socio-political and economic factors that could affect the climate policy equilibrium (De la Cuesta & Imai, 2016).



Figure 4.1: Directed acyclic graph (DAG) representing the causal model underlying my theoretical argument.

less than

 $d \in \{25km, 50km, 75km, 100km, 150km, 200km, 250km\},\$

and then to control for the exact distance from the border. To account for potential non-linearities in the way distance from the border affects political behaviour, I control for two-dimensional polynomials in latitude and longitude (Dell, 2010). As shown in the Appendix, the results remain qualitatively similar for polynomials in the Euclidean distance.

In such comparatively homogeneous border regions, it is more plausible that pre-existing differences are less likely to play a role, especially after pre-war baseline differences are corrected for through entropy balancing. Identification follows from the theoretically plausible assumption that the effect of borders as institutions which elicit discontinuous effects on individual-level expectations and values (e.g. trust), is local to the border-adjacent region (Abramson, Carter, & Ying, 2022).

To further ensure that the variation I exploit is local to such homogeneous regions, I include grid cell fixed effects⁷ by splitting the observations into 95 regions along latitude and longitude that capture unobserved confounding related to the natural distribution of districts across space, along the lines of

⁷I include grid cell fixed effects rather than NUTS-level region fixed effects to avoid any collider bias, as the latter regions were officially formed after the separation of Germany (i.e., (Pepinsky et al., 2023).

Doucette (2024, p.8). Following Rozenas et al. (2017, p.1152), I then compute the Moran eigenvectors associated with the grid I use during estimation, which are synthetic covariates that absorb spatial patterns potentially predictive of the outcome, thus reducing the threat of spatial noise.

Finally, I control for voter turnout in each German region to ensure that the effect is not driven by differential effects of political regimes on political engagement, which could artificially inflate the influence of small localities with high mobilisation around environmental clauses. The full causal model underlying my model specification is shown in Figure 4.1 in the form of a directed acyclic graph (DAG).

I report Conley standard errors with a cutoff of 50 km, which are heteroskedasticity and autocorrelation consistent (HAC), accounting for potential spatial autocorrelation in the error term (Conley, 1999; Conley & Kelly, 2024).

4.6 Results and discussion

4.6.1 Main GRDD estimation



Figure 4.2: GRDD plot based on Calonico et al. (2015)

First, following the optimal nonparametric method developed by Calonico et al. (2015), I plot the distribution of German counties around the inner-German border as well as their respective long-term climate policy equilibrium. Figure 4.2 shows that regions to the right of the cutoff points (i.e., former East German counties) exhibit lower equilibria, providing preliminary evidence that exposure to authoritarianism reduces citizens' appetite for environmental action.

Dependent variable: climate policy equilibrium										
					Bandwidth:					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-0.840^{***}	-0.425^{***}	-0.163^{**}	-0.155^{***}	-0.174^{***}	-0.193^{***}	-0.208^{***}	-0.176^{***}	-0.216^{***}	-0.217^{***}
	(0.255)	(0.086)	(0.077)	(0.045)	(0.044)	(0.034)	(0.029)	(0.040)	(0.035)	(0.027)
Interaction	-0.761^{***}	-0.425^{***}	-0.184^{***}	-0.154^{***}	-0.179^{***}	-0.197^{***}	-0.212^{***}	-0.178^{***}	-0.215^{***}	-0.215^{***}
	(0.190)	(0.100)	(0.064)	(0.047)	(0.044)	(0.035)	(0.031)	(0.033)	(0.035)	(0.026)
Quadratic	-1.083^{***}	-0.520^{***}	-0.183^{**}	-0.154^{***}	-0.198^{***}	-0.190^{***}	-0.222^{***}	-0.192^{***}	-0.221^{***}	-0.220^{***}
	(0.008)	(0.007)	(0.008)	(0.011)	(0.013)	(0.015)	(0.016)	(0.017)	(0.016)	(0.017)
Cubic	-1.083^{***}	-0.554^{***}	-0.169^{**}	-0.145^{***}	-0.160^{***}	-0.176^{***}	-0.225^{***}	-0.189^{***}	-0.220^{***}	-0.221^{***}
	(0.374)	(0.187)	(0.084)	(0.041)	(0.042)	(0.047)	(0.035)	(0.027)	(0.036)	(0.029)
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917
<i>N</i> .	336	704	960	1232	1488	1736	2048	2336	2920	3192
\mathbb{R}^2 Adjusted	0.996	0.997	0.997	0.996	0.997	0.997	0.997	0.997	0.997	0.997

Table 4.1: Effect of the East German regime on the climate policy equilibrium

Notes: P-values for Conley standard errors in brackets.

****p < 0.01; ***p < 0.05; *p < 0.1

Next, Table 4.1 reports the results of the GRDD for a wide range of bandwidths, from 25 to 250 of the inner-German border on either side (Columns 1-9), as well as for the full natural experiment (Column 10). As the exact functional form of the spatial relationship is unknown, I use four polynomials in latitude and longitude, ranging from a linear polynomial to a cubic polynomial with interactions between coordinates⁸. Each of the 40 estimates (10 bandwidths \times 4 polynomials) measures the local average treatment effect on the treated units (LATT), which should be interpreted accordingly as the causal effect of exposure to the East German authoritarian regime on the climate policy equilibrium of those districts that were part of East Germany.

The results closely match my theoretical expectations: exposure to the East German authoritarian regime led voters in former East German districts to express a lesser demand for ambitious climate policy, as captured by the lower climate policy equilibria. Coefficients are significant at conventional levels and substantial given the mean climate policy equilibria.

⁸I avoid controlling for polynomials beyond the third degree to avoid overfitting, as suggested by Gelman and Imbens (2019).

While the LATT coefficients remain stable across different polynomial specifications and most bandwidths, the effect is significantly higher in the immediate neighbourhood of the inner-German border. This may be due to the fact that the limited pro-environmental mobilisation that occurred in East Germany was highly concentrated in the immediate proximity of the inner-German border, where several nuclear waste dumps were located (Kirchhof, 2018). This spatially skewed climate action may have initially led to higher levels of climate awareness in the East German regions close to the West, where these protests originated, which could then have triggered a commensurate increase in political violence on behalf of the communist regime, altering long-term equilibrium pathways.

	(1)	(2)	(3)	(4)
	(25-50 km)	(50-75 km)	(75-100 km)	(100-125 km)
Linear	-0.175^{**}	-0.260^{***}	-0.380***	-0.241^{***}
	(0.069)	(0.029)	(0.141)	(0.069)
Interaction	-0.230^{***}	-0.285^{***}	-0.552^{***}	-0.277^{***}
	(0.057)	(0.043)	(0.083)	(0.088)
Quadratic	-0.307^{***}	-0.365^{***}	-0.734^{***}	-0.513^{***}
	(0.053)	(0.069)	(0.078)	(0.124)
Cubic	-0.234^{***}	-0.326^{**}	-0.745^{***}	-0.402
	(0.040)	(0.146)	(0.151)	(0.407)
Mean DV	6.953262	6.912537	6.915264	6.924402
N.	368	256	272	256

Table 4.2: Effect of the East German regime on climate policy equilibrium (donut regions)

Notes: P-values for Conley standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

To address this concern, and thus explicitly test whether the LATT is highly dependent on the inclusion of districts near the inner-German border in the sample, I implement a donut GRDD (Barreca, Guldi, Lindo, & Waddell, 2011). In this setting, observations immediately around the threshold are ignored, while the rest of the estimation is carried out as usual (Cattaneo & Titiunik, 2022, pp.844-845). Table 4.2 shows the results of the doughnut GRDD estimation, which mostly remain significant and consistent in magnitude even after removing the *problematic* slices from the samples. Thus, while the effect of authoritarianism on support for climate action is strongest in regions where confrontation over

climate policy has been manifest, it can be observed throughout the whole country exposed to the regime.

One of the most direct expressions of these East-West divergences in climate policy equilibrium comes from comparing the share of votes received by B90/Die Grünen, Germany's main pro-environmental party, in the two regions. Using the same causal identification strategy, Table C1 shows that in former East German districts, B90/Die Grünen are expected to win a couple of percentage points less than in Western districts. Thus, the effect of authoritarianism on climate policy preferences is not only diffused through the party system, but also concentrated on political parties that explicitly claim ownership of environmental issues.

4.6.2 Robustness checks and sensitivity analysis

To validate the results of the GRDD, I implement a series of robustness checks and sensitivity analyses⁹. I conduct four robustness checks, primarily to address potential alternative explanations and omitted variable bias (Neumayer & Plümper, 2017, pp.133-144). Figure 4.3 visually summarises the protocol.

First, I use the Euclidean distance instead of the polynomial in latitude and longitude to check whether my previous measure overfits spatial noise (Table C2). Similarly, I remove the Moran eigenvectors and grid cell FEs from the specifications (Tables C6-C7). Collectively, these checks show that my results are consistent across different approaches to specifying the nature and functional form of spatial noise.

Second, because the German climate agenda has been historically linked to anti-nuclear sentiment (Joppke, 2023; Poguntke, 2001), I include a series of covariates measuring whether a country had a nuclear power plant before 1990, the distance to the nearest such plant, the distance to the main nuclear waste dumping sites, and the distance to Chernobyl, the site of one of the most prominent nuclear accidents in history and a driver of anti-nuclear sentiment across Europe (Table C3). In this way, I verify that the differences in equilibria are not driven by my theoretical argument, but rather reflect a spillover of differential preferences for nuclear power.

Third, I include the distances to the largest cities near the inner-German borders as well as the distances to the closest regional capitals (Table C4). This allows me to reject an alternative theory that an

⁹Robustness checks test the stability of the results to the inclusion of additional covariates that summarise alternative theoretical mechanisms, while sensitivity analyses provide the same kind of 'safeguard' against unobservable characteristics.

ad hoc climate ambition was formed in West Germany and propagated through a network of urban connections independent of the effect of the political regime.

Fourth, I check whether the results remain consistent when modelling selection into treatment using alternative preprocessing algorithms, such as the widely used covariate balancing propensity score matching (Imai & Ratkovic, 2014) (Table C5). The results are also qualitatively similar when selection into treatment is not modelled at all (Table C8)



Figure 4.3: Summary of the robustness protocol

Next, I implement the sensitivity analysis protocol¹⁰ developed by Oster (2019), which measures the

¹⁰Sensitivity analysis acknowledges uncertainty about the magnitude of the effect, but it allows us to validate the sign of the effect, which in this paper, as in many others, is much more important for interpretation than the precise magnitude of that effect (Kocher & Monteiro, 2016, p.970). While this is a limited kind of causal knowledge (i.e. was there a causal effect, rather than what was the size of the causal effect), it is one in which I can be more confident and which is unlikely to lead to either Type I or Type II errors (Kubinec, 2022).

degree of selection on unobservables, relative to observables, that would be required to explain away an observed effect (pp.191-196)¹¹. Table 4.3 shows Oster's δ , the measure of this degree of selection on unobservables, for GRDD models using either a linear polynomials or a higher-order polynomial. To cancel out the effect of exposure to the East German regime, unobservables would have to explain a similar amount of variation as the observables, and in some cases even more. While this is logically possible, given the very high levels of R^2 and the robustness to the addition of new covariates, this is a highly unlikely scenario that speaks to the internal validity of the results.

	Bandwidth:										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)			
Polynomial degree:	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km			
Linear	1.64	0.891	0.925	3.00	1.23	0.801	0.590	0.696			
Higher order	1.86	0.793	0.831	2.64	1.23	0.835	0.611	0.744			

Table 4.3: Sensitivity analysis based on Oster (2019)

4.6.3 Correlated policy equilibria

At this point, it is clear that whether a district was in East or West Germany influences the long-term climate policy equilibrium point. However, whether this variation can be meaningfully interpreted as a negative effect of authoritarianism on environmental preferences is a more complex question. The main risk of misinterpretation arises because electoral competition in advanced democracies tends to be multidimensional, meaning that distinct political equilibria, potentially influenced by the same causal factors, may be strongly correlated (Albright, 2010; Kriesi et al., 2006). It may therefore be the case that citizens in the East vote for parties with a less ambitious climate agenda not only because of this agenda, but also because of the other policies that the parties advocate.

¹¹This protocol assumes that the relationship between the main independent variable and the unobservable factors can be estimated using the relationship between that variable and the observable factors (Oster, 2019, p.187).



Figure 4.4: Correlation plots for policy equilibria in Germany

This is not just a theoretical proposition, but an empirical fact, illustrated in Figure 4.4, which simply plots the Pearson correlation coefficient between all policy equilibria that can be computed using the Manifesto Project Dataset. Without reading too much into simple bivariate correlations, two things are evident. Without reading too much into simple bivariate correlations, two things are clear. First, the German political system is clearly multidimensional, but these dimensions are highly interconnected. Second, the climate policy equilibrium (PER501) is particularly correlated with other equilibria.

For example, the negative effect identified could spillover from the fact that environmentally ambitious political parties, while left-leaning, are less focused on traditional left-wing issues such as welfare and social security, attributes known to be disproportionately important to East Germans (Alesina & Fuchs-Schündeln, 2007). It could also simply be a consequence of "economic backwardness" in the East,

with citizens less likely to have developed the post-materialist attitudes that are a necessary precursor to support for environmentally ambitious political parties (Grant & Tilley, 2019; Peisker, 2023).

Thus, conceptually, testing whether the effects identified in the previous subsections actually support my theoretical claim would require isolating the variation in climate policy equilibria that is not due to correlation with other policy equilibria. In essence, one would have to reduce the multidimensional German policy space to a synthetic unidimensional one centred on climate policy and only then test whether authoritarian regimes inhibit the formation of pro-environmental attitudes. This would ensure that other factors that have differed during the transition period, for example in terms of the labour market, are not the main driver of the effect.

To this end, I have adopted the following strategy. First, I compute the policy equilibrium for a large class of policy conflicts, ranging from support for free markets to support for trade protectionism. I then examine the equilibria that are strongly correlated with the climate policy equilibrium, relying on a machine learning algorithm to identify the strongest predictors¹² from nearly 50 different equilibria.

I fit a random regression forest designed for continuous outcomes (Breiman, 2001), an effective treebased algorithm that detects effect heterogeneity, non-linearities, and manages latent interactions among numerous covariates, even when many are irrelevant. This results in a variable importance (VI) measure that ranks all policy equilibria based on their predictive contribution to the climate policy equilibrium (Archer & Kimes, 2008), with the most relevant equilibria focusing on economic planning, education expansion, traditional morality, law and order, civic mindedness, and minority groups. Put simply, when people consider who to vote for given their preferences on climate policy, they jointly consider how German parties position themselves on these particular issues. Next, I residualize the climate policy equilibrium on these six predictors, capturing variation in climate policy equilibria that is independent of the most important other conflicts in the minds of German voters¹³. I use the residuals as the new dependent

¹²Crucially, this is not a causal question, but a predictive one: for computational purposes, it doesn't matter why East Germans would care about the welfare state and climate policy at the same time, it only matters that they would care about this dyad more than a hypothetical economic growth-climate policy dyad.

¹³To explain this approach, it's useful to consider the correlation between these predictors, which implies (imperfect) collinearity. Residualisation is a recognised technique for dealing with this problem, ensuring that the effect attributed to a predictor is solely due to that specific concept, unaffected by correlated variables (García, Salmerón, García, & García, 2020).

variable and re-run the main GRDD analysis ¹⁴.

Dependent variable: rezidualized climate policy equilibrium												
		Bandwidth:										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)		
Linear	-0.487	-0.389^{*}	-0.577^{***}	-0.658^{***}	-0.746^{***}	-0.792^{***}	-0.827^{***}	-0.777^{***}	-0.845^{***}	-0.849^{***}		
	(0.581)	(0.257)	(0.129)	(0.108)	(0.077)	(0.091)	(0.024)	(0.083)	(0.069)	(0.034)		
Interaction	-0.632	-0.503^{**}	-0.610^{***}	-0.657^{***}	-0.746^{***}	-0.802^{***}	-0.835^{***}	-0.780^{***}	-0.845^{***}	-0.848^{***}		
	(0.570)	(0.208)	(0.113)	(0.107)	(0.082)	(0.100)	(0.091)	(0.082)	(0.074)	(0.036)		
Quadratic	0.117	-0.768^{***}	-0.613^{***}	-0.654^{***}	-0.755^{***}	-0.810^{***}	-0.849^{***}	-0.796^{***}	-0.850^{***}	-0.853^{***}		
	(0.704)	(0.193)	(0.115)	(0.106)	(0.081)	(0.076)	(0.066)	(0.081)	(0.085)	(0.051)		
Cubic	0.117	-0.634^{***}	-0.601^{***}	-0.631^{***}	-0.708^{***}	-0.751^{***}	-0.818^{***}	-0.773^{***}	-0.854^{***}	-0.855^{***}		
	(0.704)	(0.399)	(0.183)	(0.088)	(0.071)	(0.103)	(0.129)	(0.175)	(0.088)	(0.053)		
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192		

Table 4.4: Effect of the East German regime on the residualized climate policy equilibrium

Notes: P-values for Conley standard errors in brackets.

*** p < 0.01; ** p < 0.05; * p < 0.1

Table 4.4 reports these results. For the regions defined by a smaller bandwidth, the effect is no longer significant, and for the 25km bandwidth it even changes its sign depending on the polynomial used to measure the distance from the inner-German border. For the larger bandwidths, the effects are significant at all conventional levels and are qualitatively similar to the main estimation.

Two implications are worth discussing. First, based on the discrepancy between Table 4.1 and 4.4, in the immediate neighbourhood of the inner-German border the climate policy equilibrium is much more entangled with other political conflicts than in the rest of Germany (Column 1). One possible explanation is that multiple sites near the inner-German border were used as dumping grounds for nuclear waste, in a non-transparent manner. Since the late 1970s, this has fused environmental concerns in the region

¹⁴It's important to distinguish between controlling for the six equilibria and residualising. When I control for these equilibria, we are essentially 'removing' their influence on the primary relationship under investigation. By contrast, by residualizing and then using the GRDD, I aim to identify how exposure to the East German regime accounts for the variation in the climate policy equilibrium that isn't explained by other equilibria. It would be conceptually flawed to control for these equilibria on the assumption that they influence the degree of exposure to the East German regime, especially given the clear temporal sequence. If I assume that exposure indirectly affects the climate policy equilibrium by influencing other equilibria, then controlling for these equilibria would be tantamount to adjusting for post-treatment variables. This would inadvertently eliminate the effect we're trying to understand.

with concerns about state accountability, democratic participation, and broader notions of capitalism¹⁵ (Kirchhof, 2015, 2018).

Second, the effect of authoritarianism on pro-environmental preferences is largely independent of other electoral considerations (Columns 3-10 and most of Column 2). The magnitude of the effects, coupled with the stability of the coefficients outside the immediate neighbourhood of the inner-German border, is surprising given the low salience of climate policy throughout East Germany and Germany as a whole in the 1990s. This demonstrates the long-term effectiveness of the East German regime's tactics during the 1980s of monitoring every pro-environmental organization and ensuring their lack of development (Jones, 1993). Beyond the case of Germany, this provides an insight into the drivers of environmental preferences: their emergence is a long-term process anchored in the establishment of a milieu of environmental organisations that can coagulate, nurture and curate the attitudes of citizens, like in the border region (25km bandwidth). In East Germany, such milieu arose spontaneously due to cross-border collaboration with anti-nuclear movements in the West, but in the rest of the country, it did not. This corroborates recent work showing that climate protests in recent years have been much more likely to occur, and to be significant in scale, in countries with a rich tradition of green parties and other pro-environmental organisations (Wahlström et al., 2019).

4.6.4 Heterogeneous effects of exposure to communism

To provide further evidence that, in the case of Germany, authoritarianism, through the use and misuse of the repressive apparatus undermined the post-unification climate equilibrium in the East, I draw on county-level data on the intensity of Stasi surveillance activity collected by Lichter et al. (2021), which I then match with post-1990 electoral outcomes. The aim is to show that even within East Germany, differences in climate equilibria are correlated with the degree of repression experienced by the local population. If this is the case, and assuming that the level of repression can be used as a proxy for the level of repression against pro-environmental groups, then it would be much more credible to attribute the

¹⁵However, simply controlling for the distance of each unit to these major sites does not eliminate the effect of interest, as shown in Table C3. This means that there is no direct effect on electoral preferences, but an indirect one that changes how individuals interact the different dimensions of political space in their own electoral calculus.

effects found to the nature of the East German communist regime.

Empirically, I augment the GRDD by interacting the binary indicator of whether a district was part of East Germany with the intensity of Stasi repression, with districts in West Germany automatically assigned a null score for the latter variable. Intensity of repression is operationalized by the density of Stasi operatives in each district, following the preferred definition of Lichter et al. (2021). To prove that the intensity of Stasi repression is a moderator, the interaction coefficient should be different from zero in the extended model. Table 4.5 presents these estimates, which yield two main conclusions. First, the effects of exposure to the East German communist regime are indeed heterogeneous and driven by the experienced level of repression, as predicted by my theoretical argument. Second, because we can identify differences in the climate equilibrium between East and West as well as differences within the East after adjusting for economic differences, concerns about alternative mechanisms are minimised, especially those rooted in post-materialistic attitudes that would separate Eastern and Western German electoral patterns.

Bandwidth:	(1) 50 km	(2) 75 km	(3) 100 km	(4) 150 km	(5) 200 km	(6) 250 km	(7) (Full sample)			
East Germany	-0.035^{***}	-0.035^{***}	-0.034^{***}	-0.032^{***}	-0.034^{***}	-0.036^{***}	-0.036^{***}			
East Germany X Stasi intensity	(0.005) -0.026^{*} (0.016)	(0.005) -0.029^{*} (0.016)	(0.006) -0.152^{*} (0.080)	(0.007) -0.048^{***} (0.014)	(0.008) -0.059^{***} (0.014)	(0.010) -0.066^{***} (0.012)	(0.009) -0.066^{***} (0.012)			
N.	1561	1897	2114	2464	3073	3472	3605			
Notes: P-values for Conley standard errors in brackets.										

Table 4.5: Climate policy equilibria: heterogeneous treatment based on the intensity of Stasti activity

Then, to recover the effect of the degree of exposure to the authoritarian regime *within East Germany* on the most direct way of expressing electoral pro-environmentalism, I regress the vote share received by B90/The Greens in East German districts after reunification on the local intensity of Stasi repression, as well as on a battery of district-level covariates. The control variables are: the regional GDP per capita, the size of the district, the average population in the 1980s, the level of education as measured by the average years of schooling per person, voter turnout in both 1933 and 1990 as a proxy for political participation,

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the presence of large urban areas within the district, whether the district bordered West Germany, and finally whether the district experienced anti-Politburo riots in 1953. Including these controls ensures that economic differences within East Germany are not the main driver of the causal effect, and, more importantly, that the partial effect of differential exposure to repression persists through the theorised channels. Figure 4.5 plots the predicted vote shares of B90/The Greens against the intensity of Stasi, showing that the latter decreases monotonically with the former, as hypothesised.



Figure 4.5: Intensity of Stasi activity and vote shares of B90/The Greens

4.6.5 Climate policy dispersion

While the climate policy equilibrium provides insightful information about the central tendency of the German electorate on environmental issues, the same equilibrium could be the product of different electoral distributions. For example, a small group voting for the far-right and another for the green parties could produce the same equilibrium as the whole of society voting for a centrist party. This leaves open the question of whether the lower climate policy equilibrium in the East is due to the fact that the more environmentally ambitious segments of the population (e.g. young women) have moved to the former West

(Stawarz & Sander, 2019), creating a homogeneously environmentally unambitious region, or whether the increased polarisation caused by the electoral rise of the far right in the Eastern districts plays a more significant role (Arzheimer, 2023; Weisskircher, 2020).

To better describe the preference distribution that makes East Germans less supportive of an ambitious climate policy equilibrium, I measure the impact of the East German regime on the climate policy dispersion, which I measure as the weighted variance of the political parties' position on climate policy, weighted by the share of votes received by each party¹⁶:

$$Var_{C} = \sum_{K=1}^{N} \pi_{K} \times (I_{K,C} - M_{C})^{2}$$
(4.2)

where M_C measures the average position of the party system for policy conflict C. Similar formulas have been used to describe affective polarisation (Wagner, 2021), but this definition focuses the debate on the specific issue of climate policy. Accordingly, a more polarised society, in which parties with climate policy positions further from the centre receive a higher share of the vote, would lead to greater climate policy dispersion than less polarised societies, in which the majority of votes are shared by parties close to the centre.

¹⁶Similar measures for understanding the party system in relation to the electorate have been used in the past, but usually for cross-country or time series national-level analysis; to my knowledge, this is the first paper to use a measure of variance in order to understand the dispersion of policy preferences within a country (Alvarez & Nagler, 2004; Ezrow, 2007).

		CLIMATE POLICY EQUILIBRIUM					
		HIGH	LOW				
CLIMATE POLICY	нісн	Climate ambitious societies driven by popular consensus	Climate dismissive societies driven by popular consensus				
DISPERSION	LOW	Climate ambitious societies with high polarization	Climate dismissive societies with high polarization				

Figure 4.6: Typology of societies based on their policy equilibrium and dispersion

Figure 4.6 provides a simple but informative typology of societies based on the equilibrium and dispersion of climate policy. I already know that the East German counties fall into the second column of the typology due to their low(er) political equilibrium. This means that there is less appetite for ambitious climate policies in this region of the country. Examining the effect of exposure to authoritarianism on the level of dispersion will then tell us exactly which of the four categories East Germany falls into. That is, whether this low appetite is driven by consensus or whether society is polarised on the issue. To clarify this, I replicate the GRDD analysis using the climate policy dispersion associated with each district and each election as the dependent variable.

By examining not only polarisation but also dispersion, I also address one of the biggest challenges in the study of East-West German legacies: post-1990 migration (Heiland, 2004). If the migration of the most pro-environmental individuals were the main driver of the causal effects discussed above, then we should not observe significant polarisation, as the latter can only exist in a context where there are multiple groups that differ in their environmental preferences. By design, therefore, internal migration driven by environmentalism, or its correlates, would work against my argument, making my estimates be conservative lower bounds, as long as the legacy of authoritarianism is also correlated with higher

dispersion.

Table 4.6 reports the results of this estimation. The LATT effects estimated are positive and significant at conventional levels for all bandwidths and polynomials. Thus, I can infer that exposure to the East German regime has massively increased climate policy dispersion. In other words, the former East German districts are considerably more polarised in their preferences for ambitious climate policy: while some citizens may have traditional left-wing preferences, which correlate with the pro-environmentalism of German political parties, a significant proportion of voters have migrated to the far-right, which is quintessentially opposed to ambitious climate action (Lockwood, 2018).

Dependent variable: climate policy equilibrium												
	_	Bandwidth:										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)		
Linear	4.019***	1.801***	1.031***	0.953***	1.067***	1.108***	1.201***	1.005***	1.177***	1.173***		
	(0.957)	(0.320)	(0.176)	(0.135)	(0.082)	(0.045)	(0.057)	(0.166)	(0.137)	(0.108)		
Interaction	3.834***	1.904***	1.173***	0.958***	1.079***	1.123***	1.223***	1.018***	1.176***	1.167***		
	(0.775)	(0.255)	(0.221)	(0.140)	(0.079)	(0.034)	(0.137)	(0.169)	(0.137)	(0.095)		
Quadratic	4.643***	2.351***	1.201***	0.972***	1.146***	1.096***	1.279***	1.085***	1.205***	1.194***		
	(1.659)	(0.317)	(0.233)	(0.150)	(0.082)	(0.020)	(0.139)	(0.061)	(0.138)	(0.116)		
Cubic	10.068^{*}	2.453***	1.179***	0.949***	1.015***	1.016***	1.270***	1.055***	1.195***	1.197***		
	(5.242)	(0.529)	(0.251)	(0.128)	(0.108)	(0.151)	(0.037)	(0.120)	(0.140)	(0.128)		
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192		

Table 4.6: Effect of the East German regime on the climate policy dispersion

Notes: P-values for Conley standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

This finding corroborates existing research which demonstrates that climate change, and environmental issues more broadly, are factors that lead to the further polarization of domestic party systems, especially in the case of Germany (Otteni & Weisskircher, 2022; Weisskircher, 2020). The other side of the argument is also very interesting: while the lower climate equilibria in the East remain stable over

time, the extremely high levels of dispersion also indicate the potential emergence of climate-conscious groups in the former East. While the emergence of climate leaders in post-socialist contexts is a new area of active research, the findings of this paper fit well with case study research showing a larger number of success stories in East German counties with regard to energy transition (Haupt, Kern, & Irmisch, 2022). This has serious political implications, as the confrontation between climate-sceptic and climate-friendly citizens is likely to intensify as climate policy becomes more salient.

4.7 Conclusions

Authoritarian regimes rely on political violence to extract loyalty from citizens, primarily by restricting the ability of civil society to organise around ideals that contradict state doctrine. Environmental groups often become targets of such state-sponsored repression because of their status as catalysts for citizen participation in various areas of civil and political activism. While the effectiveness of violence in suppressing these organisations may vary, in cases where repression is effective, the question is whether its negative effects are limited to a temporal snapshot, or whether they persist in the long term, altering the demand for climate action even after the collapse of an authoritarian regime and the onset of democratisation processes.

In this paper, I provide evidence for the persistence of political violence aimed at dismantling proenvironmental organisations in the climate policy equilibrium by exploiting the separation and reunification of Germany. I show that even decades after reunification, districts in the former East Germany exhibit significantly lower climate policy equilibrium levels than their Western counterparts, even after adjusting for the main pre-WWII differences between districts. I also show that, even taking into account that German citizens make their electoral choices in a multidimensional political space, the East-West difference in climate policy equilibria cannot be explained simply by the lack of post-materialist attitudes in the former East, and thus the legacy of the authoritarian regime has an independent effect on preferences for climate action and environmental protection. As predicted by the existing literature, the lower equilibria in the East are not the product of a homogeneous climate dismissive electorate, but the result of

political polarisation, including the emergence of the climate sceptic AfD in the former East Germany.

What are the main implications of these findings? First, tackling global negative externalities such as climate change may require not only overcoming collective action problems, but also confronting the legacy of authoritarian rule, even in regions of the world where environmental protection is ostensibly part of the mainstream political discourse. Future research should seek to identify the class of political events that might serve this purpose, extending the analysis in this paper to a broader category of processes, institutions and policies that might trigger latent pro-environmental preferences in the electorate.

Second, at a broader level, the demand for climate policy is endogenous to the characteristics of a political regime. This means that if governments want to credibly commit to ambitious climate protection objectives, they ought to create the appropriate context in which citizens could acquiesce to these objectives. A large body of literature describes the conditions that increase the average level of climate awareness in the population, ranging from institutional trust to material well-being. These are likely not simple correlates of pro-environmentalism, but necessary conditions for it.

Nevertheless, my findings emanate from a single country case study, which intrinsically limits their external validity. However, observational evidence seems to confirm that the legacy of historical political regimes matters for the level of climate action in a country, as evidenced by the discrepancy between Eastern and Western, or even Southern and Northern Europe. As such, the precise mechanism(s) of persistence and transmission over time deserve more attention, as learning exactly how the memory of political violence survives over time and continues to shape the formation of environmental attitudes through schools, education and family customs could shed light on these effects that appear to occur locally, nationally and supra-nationally.

5

Conclusions

5.1 Summary of empirical findings

The central insight of this dissertation is that the social fabric is remarkably permeable to episodes of political violence, which percolate through multiple channels to produce lasting changes in political behaviour, changes that outlast the violence itself and sometimes cannot be reversed even by regime change. While recent scholarship has made progress in identifying historical critical junctures and the path dependencies they trigger, less has been done to provide credible accounts of the causal mechanisms through which persistent effects propagate, or at least to explain why this persistence occurs. In other words, the literature has become comfortable with generating examples of legacies of violence without further questioning whether these particular examples tell us anything about a broader population of cases. While quantitive in nature, some of this work remains purely ideographic, in the sense that it has limited generalisability, or at least the scope conditions under which generalisation is possible remain unarticulated. Therefore, by examining such mechanisms in more detail, or at least theorising their presence, across different contexts and types of violence, this dissertation demonstrates how deeply violence penetrates social structures and produces lasting effects by shaping collective memory, altering social norms, and interacting with the organizational infrastructure of the civil society.

CHAPTER 5. CONCLUSIONS

Three empirical cases illustrate the depth of this permeability, each employing distinct causal identification strategies to isolate effects in a variety of historical and geographical settings. In France, World War I casualties reshaped not only demographics but also the way communities remember and interpret their national identity, creating fertile ground for nationalist mobilisation nearly a century later. Using a novel dataset of French WWI casualties and contemporary electoral outcomes, analysis shows that communities with higher death rates exhibit greater electoral support for the far-right Front National, with exposure to WWI increasing FN vote share by 11.8-15.4% compared to the party's average support. In Romania, the presence of the Gulag system changed not only the attitudes of victims but also entire communities' relationship with authority, fostering anti-regime norms that motivated collective action even in high-threat environments. Leveraging both selection-on-observables and instrumental variables approaches, the analysis demonstrates that localities that hosted Gulag facilities experienced approximately 2.9 more severe injuries per locality during the 1989 Revolution, nearly five times the sample mean. This effect persists across multiple empirical specifications and robustness checks. In East Germany, authoritarian repression of environmental groups didn't just eliminate organisations, it fundamentally changed the way citizens engaged with environmental policy. Using a geographic regression discontinuity design augmented with entropy balancing, the research shows effects that persist even after democratisation, as evidenced by significantly lower climate policy equilibria in former East German districts.

This permeability of the social fabric to political violence helps to explain several major puzzles in the literature. First, it explains why the effects of violence often outlast both perpetrators and direct victims - violence fundamentally alters the social structures through which political attitudes and behaviours are transmitted across generations. As the French case shows, even after the original World War I generation has passed, the transformation of local commemorative practices and community organisations ensures that the memory of violence remains politically salient. The weaponization of these memories by political entrepreneurs, particularly far-right parties, helps maintain their relevance across generations. Similarly, in Romania, anti-regime norms fostered by exposure to the Gulag persisted even after most direct witnesses had died, transmitted through family histories, church networks and community interactions, sometimes without an explicit intention behind the transmission.

CHAPTER 5. CONCLUSIONS

Second, it clarifies why seemingly discrete episodes of violence can have such broad effects on political behaviour - the effects of violence diffuse through multiple social channels simultaneously. The East German case shows how the targeting of environmental groups affected not only environmental attitudes but also broader patterns of civic engagement and policy preferences, leading to both lower climate policy equilibria and higher levels of polarisation on environmental issues. Empirical tests show that these effects are partly independent of other policy preferences, suggesting a direct causal channel through the destruction of pro-environmental infrastructure. In France, World War I casualties not only influenced views on war and peace, but also shaped broader notions of national identity and attitudes towards out-groups, as evidenced by the significant negative correlation between World War I death rates and interaction with immigrants. This multi-channel diffusion helps to explain why the effects of violence often appear in seemingly unrelated spheres of political action, as observed, for example, in studies of post-communist legacies (Pop-Eleches & Tucker, 2017; Simpser et al., 2018).

Third, it shows why democratic transitions or institutional reforms alone often fail to overcome these legacies - formal changes cannot easily repair the deep social transformations wrought by violence. As shown in both Romania and East Germany, even after democratisation, communities exposed to repression continue to exhibit distinct political behaviours. In Romania, this manifested in continued anticommunist voting patterns and higher protest participation in the early 1990s, reflecting the persistence of anti-regime norms developed under communism. In East Germany, it appeared in persistently lower support for environmental policies even three decades after reunification, demonstrating how the destruction of civic infrastructure can have lasting effects on political preferences. This persistence occurs because violence is embedded in informal institutions, social networks, and cultural practices that formal changes in the nature of the regime cannot directly address, or at least not in the short term. In particular, this helps to explain why democratisation often fails to eradicate authoritarian legacies and why seemingly similar regime reforms can produce different outcomes in communities with different histories of violence. Ultimately, the permeability of social structures to violence means that its effects are resistant to top-down institutional change.

5.2 Legacies of political violence: quo vadis?

Looking forward, research on the historical legacies of political violence should move beyond establishing persistence to tackle more nuanced questions about variation, transmission, and interaction effects. Several promising directions emerge from this dissertation's findings.

First, research needs to develop more sophisticated theoretical frameworks for understanding when different transmission mechanisms dominate. While this dissertation identifies collective memory, social norms, and civic infrastructure as key channels, their relative importance likely varies systematically across contexts. When does collective memory play the primary role in transmitting violence's effects, as in the French case with WWI deaths? When do social norms dominate, as with anti-regime attitudes in Romania? And when does institutional destruction, like the dismantling of environmental groups in East Germany, prove most consequential?

These questions demand both careful theoretical discussions and empirical innovation that would allow us to causally identify such relative causal effects. Theoretically, scholars need to better specify how different types of violence interact with existing social structures and state capacity to activate particular transmission channels. This might involve integrating insights from historical institutionalism about critical junctures with theories of collective memory and social psychology, thus moving towards a more interdisciplinary approach to the study of the historical legacies of political violence. Empirically, researchers will need creative research designs that can isolate and compare different transmission mechanisms, perhaps leveraging exogenous variation in both the type of violence and the strength of different social institutions.

A more complete taxonomy of mechanisms might further distinguish between active and passive transmission mechanisms, between those requiring continued institutional support versus self-reinforcing social processes, and between mechanisms that operate primarily through elite action versus bottom-up social dynamics. Such theoretical refinement would help move beyond catch-all explanations like "path dependence" to specify precise causal pathways. For example, future work could examine whether vio-lence transmitted through active elite manipulation produces different long-term outcomes than violence

transmitted through passive social learning.

Second, we need systematic investigation of how these mechanisms interact and potentially reinforce each other. This dissertation shows how organizational infrastructure can amplify memory transmission, as with churches preserving anti-regime attitudes in Romania. This would help explain why similar experiences of violence sometimes produce different long-term outcomes across communities with different organizational landscapes. For instance, two communities might experience similar levels of political violence, but their long-term trajectories could diverge based on whether they had robust civil society organizations capable of preserving and transmitting collective memories. This doesn't mean abandoning the idea of historical legacies, but instead looking at the interaction between sequences of legacies. If there is a credible argument to be made about the persistence effects of some episode of violence, there is no reason to believe that episode is the single one inducing such lingering legacies. Researchers ought to dedicate more time towards understanding how legacies could interact, reinforce, or even cancel one another.

Third, research must tackle the spatial dimension of violence's effects more directly. While this dissertation leverages spatial variation for causal identification - whether through examining French departments, Romanian localities, or the inner-German border - future work could theorize more explicitly about geographic patterns of diffusion and interaction. How do violence's effects spread between neighbouring communities? When do spatial clusters of similar attitudes form versus dissipate? Under what conditions do clear geographic boundaries in political behaviour emerge and persist?

Ultimately, as time passes, current events will be seen by future scholars as historical episodes. In light of this humbling understanding of our role as researchers at a very specific moment in time, this dissertation concludes with a simple message: we need better and more creative research designs, we need more varied and diverse empirical settings, and we need better theories that allow for generalisation without absurdly restrictive scope conditions. The field of historical persistence of political violence is here to stay, so we might as well make sure it develops properly.

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Appendices



Appendix to Chapter 1

A.1 Robustness checks for the main analysis

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	IV: Mean death-rate IV: Median death-rat									
ATE	0.019*** (0.006)	0.017** (0.007)	0.022*** (0.007)	0.023*** (0.006)	0.019*** (0.006)	0.017** (0.007)	0.022*** (0.007)	0.023*** (0.006)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.		
R^2	0.872	0.873	0.874	0.873	0.872	0.873	0.874	0.873		
N.	546	546	546	546	546	546	546	546		

Table A1: Effect of WWI death-rate on support for FN(1993-2017)-No spatial covariates

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

		DV: Vote share for <i>FN</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
		IV: Mediar	n death-rat	e		IV: Mediar	n death-rat	e		
ATE	0.019 (0.014)	0.015 (0.016)	0.019 (0.013)	0.018 (0.014)	0.019 (0.014)	0.015 (0.016)	0.019 (0.013)	0.018 (0.014)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	3^{rd} with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	3^{rd} with int.		
R^2	0.858	0.862	0.864	0.867	0.858	0.862	0.864	0.867		
N.	546	546	546	546	546	546	546	546		

Table A2: Effect of WWI death-rate on support for FN(1993-2017)—No spatial covariates or Moran eigenvectors

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

Table A3: Effect of WWI death-rate on support for FN(1993-2017)—No Moran eigenvectors

		DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)			
		IV: Mean	death-rate			IV: Mediar	n death-rat	e			
ATE	0.019 (0.012)	0.015 (0.013)	0.018 (0.010)	0.014 (0.010)	0.014 (0.012)	0.010 (0.012)	0.012 (0.010)	0.008 (0.010)			
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes			
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.			
R^2	0.866	0.868	0.870	0.872	0.865	0.867	0.869	0.871			
N.	546	546	546	546	546	546	546	546			

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

A.2 Quality of contact with immigrants

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)						
ATE	0.015*** (0.004)	0.013*** (0.003)	0.014*** (0.004)	0.014*** (0.002)						
\mathbb{R}^2	0.872	0.872	0.874	0.874						
N.	487	487	487	487						

Table A4: Effect of WWI death-rate on support for FN(1993-2017)—Fitted values of death rates (region)

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

The independent variable is obtained by fitting the values from regressing department-level death rates on the leave-one-out mean of the death rates within the same region.

				DV: Vote sł	nare for <i>FN</i>	V		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		IV: Mean	death-rate			IV: Mediar	n death-rat	e
ATE	0.014** (0.007)	0.011** (0.005)	0.014*** (0.005)	0.019*** (0.005)	0.011* (0.006)	0.008** (0.004)	0.011** (0.005)	0.015*** (0.005)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates Degree of distance polynomial	Yes 2 nd	Yes 2^{nd} with int.	Yes 3 rd	Yes 3 rd with int.	Yes 2 nd	Yes 2^{nd} with int.	Yes 3 rd	Yes 3 rd with int.
<i>R</i> ² N.	0.868 546	0.869 546	0.872 546	0.873 546	0.868 546	0.869 546	0.872 546	0.872 546

Table A5: Effect of WWI death-rate on support for FN(1993-2017)—Propensity score matching

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

A.3 Additional figures

	DV: Vote share for <i>FN</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
		IV: Mean	death-rate			IV: Mediat	n death-rate	2	
ATE	0.011*** (0.003)	0.009*** (0.003)	0.012*** (0.003)	0.013*** (0.002)	0.010** (0.004)	0.008*** (0.003)	0.011*** (0.003)	0.012*** (0.001)	
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Degree of distance polynomial	2^{nd}	2^{nd} with int.	3^{rd}	3^{rd} with int.	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	3 rd with int.	
R^2	0.868	0.869	0.871	0.872	0.868	0.869	0.871	0.871	
N.	546	546	546	546	546	546	546	546	

Table A6: Effect of WWI death-rate on support for FN(1993-2017)—Non-parametric CBPS matching

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

Table A7: Effect of WWI death-rate on support for FN(1993-2017)—GLM-based PS matching

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	IV: Mean	death-rate		IV: Median death-rate						
ATE	0.012** (0.005)	0.010*** (0.003)	0.013*** (0.004)	0.015*** (0.003)	0.010* (0.005)	0.008** (0.004)	0.011** (0.005)	0.013*** (0.004)		
Year FEs Grid-cell FEs Spatial covariates	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves	Yes Yes Ves		
Degree of distance polynomial	2^{nd}	2^{nd} with int.	3 rd	$3^{\rm rd}$ with int.	2^{nd}	2^{nd} with int.	3 rd	3 rd with int.		
<i>R</i> ² N.	0.880 546	0.881 546	0.884 546	0.884 546	0.879 546	0.881 546	0.883 546	0.884 546		

Conley standard errors in parentheses. The ***/** represent significance at the 0.01/0.05/0.10 levels, respectively.

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
		Sub-sample:	: Post-201	0		Sub-sampl	e: Pre-2010)		
ATE	0.004*** (0.001)	0.003** (0.001)	0.004** (0.002)	0.004** (0.002)	0.020** (0.008)	0.018*** (0.006)	0.022*** (0.006)	0.02 <i>6</i> *** (0.008)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial	2^{nd}	2^{nd} with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	2^{nd} with int.	3^{rd}	3^{rd} with int.		
R^2	0.894	0.898	0.912	0.913	0.865	0.867	0.869	0.871		
N.	182	182	182	182	546	546	546	546		

Conley standard errors in parentheses. The ***/** represent significance at the 0.01/0.05/0.10 levels, respectively.

APPENDIX A. APPENDIX TO CHAPTER 1

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
	IV: Mean death-rate					IV: Mediar	ı death-rat	e		
ATE	0.012** (0.005)	0.010*** (0.003)	0.013*** (0.004)	0.015*** (0.003)	0.012** (0.005)	0.014*** (0.004)	0.011** (0.005)	0.013*** (0.004)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial	2^{nd}	2^{nd} with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	2^{nd} with int.	3^{rd}	3 rd with int.		
R^2	0.881	0.881	0.883	0.882	0.880	0.880	0.883	0.886		
N.	647	647	647	647	647	647	647	647		

Table A9: Effect of WWI death-rate on support for FN —Pre-1990 data

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

Table A10: Individual-level effects of WWI death rates by age profile

	I	DV: Support for the FN)				DV: Vote for Marine Le Pen in 2017				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
Mean death-rate X age	0.001*** (0.0003)	0.001*** (0.00006)	0.001*** (0.0005)	0.001*** (0.0005)	0.001*** (0.0003)	0.001*** (0.0005)	0.001*** (0.0004)	0.001*** (0.0004)		
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Department-level controls ¹	No	Yes	No	Yes	No	Yes	No	Yes		
Individual-level controls ²	Yes	No	Yes	Yes	Yes	No	Yes	Yes		
R^2	0.030	0.030	0.086	0.086	0.001	0.009	0.063	0.079		
N.	13645	13645	13645	13645	14197	14197	14197	14197		

Robust standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

Table A11: Effect of WWI death-rate on support for FN(1993-2017)—Additional covariates (post-treatment)

	DV: Vote share for <i>FN</i>									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)		
IV: Mean death-rate			e		IV: Median	ı death-rat	e			
ATE	0.001	0.002	0.001	0.003	0.0009	0.0008	0.002	0.001		
	(0.08)	(0.015)	(0.025)	(0.013)	(0.017)	(0.014)	(0.015)	(0.015)		
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes		
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	2^{nd} with int.	3^{rd}	$3^{\rm rd}$ with int.		
R^2	0.868	0.869	0.872	0.873	0.868	0.869	0.872	0.872		
N.	546	546	546	546	546	546	546	546		

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

	(1)	(2)	(3)	(4)
Deathrate	-0.006 (0.005)	-0.006 (0.005)	-0.006 (0.005)	-0.006 (0.005)
Region FEs	Yes	Yes	Yes	Yes
Department-level controls	No	Yes	No	Yes
Individual-level controls	Yes	No	Yes	Yes
R^2	0.012	0.012	0.018	0.018
N.	13645	13645	13645	13645

Table B1: Effects of WWI death-rates on quality contact with immigrants

Heteroskedasticity-robust standard errors in parentheses. The ***/**/* represent significance at the 0.01/0.05/0.10 levels, respectively.

Table B2: Effect of WWI death-rate on	support for righ	nt-wing parties in	1910-1914 (placebo)
---------------------------------------	------------------	--------------------	---------------------

	DV: Vote share for <i>FN</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		IV: Mean	death-rate			IV: Mediar	n death-rat	æ
ATE	0.001		0.001		0.001		0.001	
-0.000		0.021**		-0.002		-0.002		
	(0.045)	(0.044)	(0.040)	(0.015)	(0.009)	(0.004)	(0.006)	(0.004)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Grid-cell FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatial covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Degree of distance polynomial	2^{nd}	$2^{\rm nd}$ with int.	3^{rd}	$3^{\rm rd}$ with int.	2^{nd}	$2^{\rm nd}$ with int.	$3^{\rm rd}$	$3^{\rm rd}$ with int.
R^2	0.876	0.877	0.878	0.879	0.875	0.876	0.877	0.878

Conley standard errors in parentheses. The ***/** represent significance at the 0.01/0.05/0.10 levels, respectively.



Figure B1: Histograms of death rates across Frenchd departments

B

Appendix to Chapter 2

B.1 Additional evidence

DV: Number of severe injuries from a locality (Mean=0.605)									
	(1)	(2)	(3)	(4)	(5)	(6)			
Gulag facility	5.626*	2.095**	1.954**	1.725**	1.984**	2.002***			
	(0.053)	(0.012)	(0.026)	(0.050)	(0.017)	(<0.001)			
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark			
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark			
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark			
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X			
Spatial lags	X	X	X	X	X	\checkmark			
R^2 Adj	0.024	0.320	0.345	0.346	0.303	-			
N.	3181	3180	3180	3180	3180	3180			

Table B1: Grid cell clustered standard errors

Notes: P-values for clustered standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)	(6)	
Gulag facility	-0.028** (0.034)	-0.011** (0.034)	-0.030** (0.014)	-0.025* (0.051)	-0.023** (0.024)		
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark	
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark	
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark	
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X	
Spatial lags	X	X	X	X	X	\checkmark	
R^2 Adj	0.024	0.320	0.345	0.346	0.303	-	
N.	3181	3180	3180	3180	3180	3180	
Moran's I	-0.002	-0.007	-0.012	-0.017	-0.008	-	
Oster's' δ	-	1.62	1.76	1.76	1.78	-	

Table B2: Continuous independent variable

Notes: P-values for Conley standard errors in brackets.

***p < 0.01; **p < 0.05; *p < 0.1

(1) (2) (3) (4) (5) (6)									
	(1)	(2)	(3)	(+)	())	(0)			
Gulag facility	0.259***	0.207***	0.199***	0.206***	0.209***				
0 ,	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)				
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark			
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark			
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark			
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X			
Spatial lags	X	X	X	X	X	\checkmark			

Table B3: Binary dependent variable

 $^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.1$

DV: Number of severe injuries from a locality (Mean=0.605)									
	(1) (2) (3) (4) (5) (6)								
Gulag facility	-0.001** (0.038)	-0.001** (0.155)	-0.002** (0.018)	-0.001 (0.288)	-0.002** (0.023)				
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark			
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark			
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark			
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X			
Spatial lags	X	X	X	X	X	\checkmark			

Table B4: Binary dependent variable and continuous independent variable

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.1

DV: Number of severe injuries from a locality (Mean=0.605)								
	(1)	(2)	(3)	(4)	(5)	(6)		
Gulag facility	5.627*	2.095***	1.953***	1.725**	1.984***	2.108***		
	(0.052)	(0.002)	(0.007)	(0.021)	(0.006)	(< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X		
Spatial lags	X	X	X	X	X	\checkmark		

Table B5: Removing outliers in terms of Gulag facilities

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; * p < 0.1

DV: Number of severe injuries from a locality (Mean=0.605)								
	(1)	(2)	(3)	(4)	(5)	(6)		
Gulag facility	0.937***	0.809***	0.733***	0.711***	0.787***			
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)			
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X		
Spatial lags	X	X	X	X	X	\checkmark		

Table B6: Removing outliers in terms of Gulag injuries

-

DV: Number of severe injuries from a locality							
	(1)	(2)	(3)	(4)			
Gulag facility	5.662**	2.074***					
	(0.053)	(0.002)					
Gulag facility (fitted)			6.52**	2.251**			
			(0.046)	(0.042)			
Closeness to high-repression area	-0.254	0.097	-0.274	0.091			
	(0.460)	(0.361)	(0.433)	(0.404)			
Covariates	X	\checkmark	X	\checkmark			
N.	3181	3180	3180	3180			

Table B7: Closeness to a high-repression area, 3 facilities

DV: Number of severe injuries from a locality									
(1) (2) (3) (4)									
Gulag facility	5.619**	2.069***							
	(0.053)	(0.002)							
Gulag facility (fitted)			6.082**	2.427**					
			(0.022)	(0.032)					
Closeness to high-repression area	-1.091	-0.489	-1.089	-0.489*					
	(0.309)	(0.101)	(0.308)	(0.098)					
Covariates	X	\checkmark	X	\checkmark					
N.	3181	3180	3180	3180					

Table B8: Closeness to a high-repression area, 2 facilities

Notes: P-values for Conley standard errors in brackets. $^{***}p < 0.01; ^{**}p < 0.05; ^{*}p < 0.01$

DV: Number of severe injuries from a locality								
	(1)	(2)	(3)	(4)				
Gulag facility	5.649**	2.080***						
	(0.053)	(0.003)						
Gulag facility (fitted)			6.368**	2.278**				
			(0.041)	(0.047)				
Closeness to high-repression area	-0.174	0.105	-0.192	0.099				
	(0.492)	(0.453)	(0.453)	(0.473)				
Covariates	X	\checkmark	X	\checkmark				
N.	3181	3180	3180	3180				
Notes: P-values for Conley standard errors in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.01$								

Table B9: Closeness to a high-repression area, 5 facilities

	1		,
	(1)	(2)	(3)
Gulag facility	0.000	0.000	0.000
	(0.258)	(0.507)	(0.397)
Covariates	\checkmark	\checkmark	\checkmark
Grid cell FEs	\checkmark	\checkmark	\checkmark
N.	2684	2684	2684

 Table B10: Exposure to the Gulag and pressure to become a secret police informant

 DV: Membership in the Romanian Communist Party

Notes: P-values for Conley standard errors in brackets. ***p < 0.01; **p < 0.05; *p < 0.01

Table B11: Ro	bustness cl	heck: severi	ty of Sec [.]	uritate p	resence
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DV: Number of severe injuries from a locality (Mean=0.605)							
	(4)	(5)	(6)				
Gulag facility	1.882**	1.747*	1.934***				
	(0.042)	(0.071)	(0.003)				
Covariates	X	\checkmark	\checkmark				
Moran eigenvectors	\checkmark	\checkmark	\checkmark				
Grid cell FEs	\checkmark	\checkmark	\checkmark				
Entropy balancing	\checkmark	\checkmark	X				
Spatial lags	X	X	\checkmark				
R^2 Adj	0.346	0.303	-				
N.	3180	3180	3180				
Moran's I	-0.017	-0.008	-				
Oster's' δ	1.76	1.78	-				

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.01
Second stage DV: Number of severe injuries from a locality								
	(1)	(2)	(3)	(4)	(5)			
Gulag facility (fitted)	6.332*** (< 0.001)	2.474** (0.038)	2.749** (0.011)	2.207** (0.039)	2.686** (0.019)			
Covariates	X	\checkmark	\checkmark	\checkmark	\checkmark			
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark			
Grid cell FEs	X	X	X	\checkmark	\checkmark			
<i>N</i> .	3181	3180	3180	3180	3180			
Cragg-Donald F statistic	66.23	66.85	437.33	69.62	69.62			

Table B12: Alternative IV results

DV: Number of severe injuries from a locality (Mean=0.605)								
	(1)	(2)	(3)	(4)	(5)	(6)		
Gulag facility	5.002*** (<0.001)	4.005*** ((<0.001)	6.645*** (<0.001)	2.705** (<0.001))	3.404*** (<0.001)	4.002**** (<0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	X		
Spatial lags	X	X	X	X	X	\checkmark		
R^2 Adj	0.024	0.320	0.345	0.346	0.303	-		
N.	3181	3180	3180	3180	3180	3180		

Notes: P-values for clustered standard errors in brackets.

 $^{***}p<0.01; ^{**}p<0.05; ^{*}p<0.1$

DV: Number of severe injuries from a locality (Mean=0.605)								
	(1)	(2)	(3)	(4)	(5)			
Gulag facility	0.525***	0.360***	0.327***	0.320***	0.347***			
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)			
Covariates	X	\checkmark	\checkmark	X	\checkmark			
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark			
Grid cell FEs	X	X	X	\checkmark	\checkmark			
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark			
Spatial lags	X	X	X	X	X			

Table B14: Results: Selection-on-observables with logged DV

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	2.777***	1.596***	1.495***	1.314***	1.252***		
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		
Notes: P-values for C	onley standar	d errors in hr	ackets				

Table B15: Results: Selection-on-observables with Poisson regression

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	2.777***	1.596***	1.495***	1.314***	1.252***		
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		

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Table BI6. Results.	Selection-of	1-observables	with neg	itive bind	omial r	egression
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DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	0.556***	0.508***	0.479***	0.476***	0.504***		
	(< 0.001)	(< 0.001)	(< 0.001)	(0.008)	(< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		

Table B17: Results: Selection-on-observables with zero inflated negative binomial regression

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	0.556***	0.508***	0.479***	0.476***	0.504***		
	(< 0.001)	(< 0.001)	(< 0.001)	(0.008)	(< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		
Notes: P-values for Conley standard errors in brackets.							

Table B18: Results: Selection-on-observables without top and bottom 1% of observations based on severe injuries

Notes: P-values for Conley standard errors in brack **** p < 0.01; **p < 0.05; *p < 0.01

Table B19: Results: Selection-on-observables without top and bottom 2% of observations based on severe injuries

	(1)	(2)	(3)	(4)	(5)	
Gulag facility	0.501***	0.469***	0.467***	0.478***	0.491***	
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	
Covariates	X	\checkmark	\checkmark	X	\checkmark	
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark	
Grid cell FEs	X	X	X	\checkmark	\checkmark	
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark	
Spatial lags	X	X	X	X	X	
Notes: P-walues for Conley standard errors in brackets						

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	0.214*** (< 0.001)	0.209*** (< 0.001)	0.205*** (< 0.001)	0.219*** (< 0.001)	0.224*** (< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		
Notes: P-values for Conley standard errors in brackets.							

Table B20: Results: Selection-on-observables without top and bottom 5% of observations based on severe injuries

Notes: P-values for Conley standard errors in bracket **** p < 0.01; **p < 0.05; *p < 0.01

DV: Number of severe injuries from a locality (Mean=0.605)							
	(1)	(2)	(3)	(4)	(5)		
Gulag facility	0.214*** (< 0.001)	0.209*** (< 0.001)	0.205*** (< 0.001)	0.219*** (< 0.001)	0.224*** (< 0.001)		
Covariates	X	\checkmark	\checkmark	X	\checkmark		
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark		
Grid cell FEs	X	X	X	\checkmark	\checkmark		
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark		
Spatial lags	X	X	X	X	X		

Table B21: Results: Selection-on-observables without top 5% of observations based on severe injuries

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.01

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(2)	(3)	(4)	(5)						
Gulag facility	0.501*** (< 0.001)	0.469*** (< 0.001)	0.467*** (< 0.001)	0.478*** (< 0.001)	0.491*** (< 0.001)						
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark						
Spatial lags	X	X	X	X	X						

Table B22: Results: Selection-on-observables without top 2% of observations based on severe injuries

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(4)	(5)								
Gulag facility	2.742***	1.876***	1.610**	1.511**	1.779***						
	(< 0.001)	(0.003)	(0.010)	(0.015)	(0.008)						
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark						
Spatial lags	X	X	X	X	X						

Table B23: Results: Selection-on-observables without Bucharest

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(1) (2) (3) (4)									
Gulag facility	2.278*** (< 0.001)	1.562*** (0.003)	1.289** (0.013)	1.212** (0.018)	1.453*** (0.008)						
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark						
Spatial lags	X	X	X	X	X						

Table B24: Results: Selection-on-observables without Bucharest and Timisoara

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(1) (2) (3) (4) (5)									
Gulag facility	5.188*	1.903***	1.747**	1.535**	1.795***						
	(0.071) (0.003) (0.012) (0.028)										
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	Entropy balancing $X = X$ \checkmark										
Spatial lags	X	X	X	X	X						

Table B25: Results: Selection-on-observables without Timisoara

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; * p < 0.01

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(2)	(3)	(4)	(5)						
Gulag facility	0.650*** (< 0.001)	0.453*** (< 0.001)	0.414*** (< 0.001)	0.405*** (< 0.001)	0.438*** (< 0.001)						
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark						
Spatial lags	X	X	X	X	X						
	, ,		,								

Table B26: Results: Selection-on-observables with IHS DV

DV: Number of severe injuries from a locality (Mean=0.605)											
	(1)	(2)	(3)	(4)	(5)						
Gulag facility	0.681***	0.395***	0.354***	0.336***	0.378***						
	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)	(< 0.001)						
Covariates	X	\checkmark	\checkmark	X	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark						
Spatial lags	X	X	X	X	X						
Notace Duraluse for C	onlaw standar	d among in he	achata								

Table B27: Results: Selection-on-observables with square root DV

Notes: P-values for Conley standard errors in brackets. *** p < 0.01; ** p < 0.05; *p < 0.1

First stage DV : Number of severe injuries from a locality (Subset: No Gulag Facility)											
	(1)	(2)	(3)	(4)	(5)						
Distance to facility	-0.003	-0.004	-0.004	-0.002	-0.002						
	(0.118)	(0.185)	(0.319)	(0.641)	(0.689)						
Covariates	X	\checkmark	\checkmark	\checkmark	\checkmark						
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark						
Grid cell FEs	X	X	X	\checkmark	\checkmark						
Ν	3181	3180	3180	3180	3180						

Table B28: Placebo Test: First Stage Results for Distance to Nearest Facility

DV: Number of	DV: Number of severe injuries from a locality (Mean=0.605)											
	(1) (2) (3)											
Gulag facility	0.012**	0.012***	0.011**	0.011**	0.011**							
	(0.011)	(0.007)	(0.013)	(0.012)	(0.016)							
Covariates	X	\checkmark	\checkmark	X	\checkmark							
Moran eigenvectors	X	X	\checkmark	\checkmark	\checkmark							
Grid cell FEs	X	X	X	\checkmark	\checkmark							
Entropy balancing	X	X	\checkmark	\checkmark	\checkmark							
Spatial lags	X	X	X	X	X							

Table B29: Results: Selection-on-observables with injuries normalized by population density

Notes: P-values for Conley standard errors in brackets. $^{***}p < 0.01; \, ^{**}p < 0.05; \, ^*p < 0.1$

C

Appendix to Chapter 3

C.1 A. Additional evidence

Table C1: Effect of the East German regime on electoral support for B90/The Greens

Dependent variable: vote share of B90/The Greens										
					Bandwidth:					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-0.0600***	-0.0304^{***}	-0.0116^{**}	-0.0111^{***}	-0.0124^{***}	-0.0138^{***}	-0.0149^{***}	-0.0126^{***}	-0.0154^{***}	-0.0155^{***}
	(0.0182)	(0.0061)	(0.0055)	(0.0032)	(0.0031)	(0.0024)	(0.0021)	(0.0029)	(0.0025)	(0.0019)
Interaction	-0.0544^{***}	-0.0304^{***}	-0.0131^{***}	-0.0110^{***}	-0.0128^{***}	-0.0141^{***}	-0.0151^{***}	-0.0127^{***}	-0.0154^{***}	-0.0154^{***}
	(0.0136)	(0.0071)	(0.0046)	(0.0034)	(0.0031)	(0.0025)	(0.0022)	(0.0024)	(0.0025)	(0.0019)
Quadratic	-0.0774^{***}	-0.0371^{***}	-0.0131^{**}	-0.0110^{***}	-0.0141^{***}	-0.0136^{***}	-0.0159^{***}	-0.0137^{***}	-0.0158^{***}	-0.0157^{***}
	(0.0006)	(0.0005)	(0.0006)	(0.0008)	(0.0009)	(0.0011)	(0.0011)	(0.0012)		
Cubic	-0.0774^{***}	-0.0396^{***}	-0.0121^{**}	-0.0104^{***}	-0.0114^{***}	-0.0126^{***}	-0.0161^{***}	-0.0135^{***}	-0.0157^{***}	-0.0158^{***}
	(0.0267)	(0.0134)	(0.0060)	(0.0029)	(0.0030)	(0.0034)	(0.0025)	(0.0019)	(0.0026)	(0.0021)
Mean DV	0.0040	0.0041	0.0042	0.0043	0.0045	0.0045	0.0047	0.0048	0.0049	0.0050

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

APPENDIX C. APPENDIX TO CHAPTER 3

	Dependent variable: climate policy equilibrium											
		Bandwidth:										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)		
Linear	-0.059	-0.375^{***}	-0.162^{**}	-0.158^{***}	-0.192^{***}	-0.196^{***}	-0.206^{***}	-0.166^{***}	-0.198^{***}	-0.201^{***}		
	(0.244)	(0.128)	(0.063)	(0.048)	(0.052)	(0.037)	(0.028)	(0.038)	(0.028)	(0.029)		
Quadratic	0.031	-0.370^{***}	-0.161^{**}	-0.158^{***}	-0.192^{***}	-0.195^{***}	-0.214^{***}	-0.185^{***}	-0.191^{***}	-0.188^{***}		
	(0.247)	(0.088)	(0.071)	(0.052)	(0.055)	(0.034)	(0.026)	(0.025)	(0.031)	(0.034)		
Cubic	0.076	-0.465^{***}	-0.156^{*}	-0.158^{***}	-0.192^{***}	-0.199^{***}	-0.205^{***}	-0.175^{***}	-0.200^{***}	-0.199^{***}		
	(0.239)	(0.084)	(0.092)	(0.052)	(0.061)	(0.039)	(0.020)	(0.029)	(0.027)	(0.025)		
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917		
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192		

Table C2: Effect of the East German regime on the climate policy equilibrium-Euclidean distance

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

respectively.

Table C3: Effect of the East German regime on the climate policy equilibrium-Nuclear power

			Depend	ent variable:	climate polic	y equilibriur	n			
					Bandwidth:					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-1.170^{***}	-0.422^{***}	-0.176^{***}	-0.159^{***}	-0.182^{***}	-0.177^{***}	-0.220^{***}	-0.191^{***}	-0.228^{***}	-0.228^{***}
	(0.294)	(0.108)	(0.058)	(0.044)	(0.044)	(0.035)	(0.031)	(0.032)	(0.047)	(0.034)
Interaction	-1.056^{***}	-0.452^{***}	-0.175^{**}	-0.157^{***}	-0.182^{***}	-0.181^{***}	-0.219^{***}	-0.191^{***}	-0.226^{***}	-0.225^{***}
	(0.403)	(0.161)	(0.073)	(0.046)	(0.045)	(0.037)	(0.038)	(0.027)	(0.039)	(0.033)
Quadratic	-1.056^{***}	-0.486^{***}	-0.163^{*}	-0.146^{***}	-0.198^{***}	-0.189^{***}	-0.212^{***}	-0.183^{***}	-0.237^{***}	-0.232^{***}
	(0.403)	(0.134)	(0.088)	(0.051)	(0.053)	(0.046)	(0.032)	(0.026)	(0.038)	(0.032)
Cubic	-1.056^{***}	-0.651^{**}	-0.166	-0.141^{***}	-0.154^{***}	-0.168^{***}	-0.207^{***}	-0.190^{***}	-0.242^{***}	-0.230^{***}
	(0.403)	(0.273)	(0.122)	(0.042)	(0.043)	(0.042)	(0.036)	(0.027)	(0.038)	(0.033)
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192
\mathbb{R}^2 Adjusted	0.996	0.997	0.997	0.996	0.997	0.997	0.997	0.997	0.997	0.997

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

		Dependent variable: climate policy equilibrium										
					Bandwidth:							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)		
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)		
Linear	-0.938^{**}	-0.414^{***}	-0.183^{**}	-0.167^{***}	-0.199^{***}	-0.217^{***}	-0.225^{***}	-0.202^{***}	-0.219^{***}	-0.223^{***}		
	(0.412)	(0.059)	(0.090)	(0.048)	(0.056)	(0.038)	(0.033)	(0.035)	(0.041)	(0.029)		
Interaction	-0.814^{*}	-0.423^{***}	-0.201^{**}	-0.169^{***}	-0.200^{***}	-0.221^{***}	-0.233^{***}	-0.212^{***}	-0.219^{***}	-0.221^{***}		
	(0.467)	(0.078)	(0.083)	(0.051)	(0.063)	(0.038)	(0.037)	(0.025)	(0.038)	(0.029)		
Quadratic	-1.083^{***}	-0.520^{***}	-0.183^{**}	-0.154^{***}	-0.198^{***}	-0.190^{***}	-0.222^{***}	-0.192^{***}	-0.221^{***}	-0.220^{***}		
	(0.374)	(0.099)	(0.090)	(0.050)	(0.053)	(0.040)	(0.026)	(0.029)	(0.033)	(0.028)		
Cubic	-1.083^{***}	-0.536^{***}	-0.165	-0.157^{***}	-0.181^{***}	-0.182^{***}	-0.254^{***}	-0.228^{***}	-0.218^{***}	-0.218^{***}		
	(0.374)	(0.129)	(0.125)	(0.057)	(0.032)	(0.041)	(0.052)	(0.039)	(0.038)	(0.030)		
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917		
Ν.	336	704	960	1232	1488	1736	2048	2336	2920	3192		

Table C4: Effect of the East German regime on the climate policy equilibrium-Urban networks

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

respectively.

Table C5: Effect of the East German regime on the climate policy equilibrium-Propensity score matching

Dependent variable: climate policy equilibrium										
	Bandwidth:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-0.562	-0.549^{***}	-0.194^{***}	-0.169^{***}	-0.151^{***}	-0.195^{***}	-0.207^{***}	-0.175^{***}	-0.204***	-0.203^{***}
	(0.424)	(0.181)	(0.040)	(0.031)	(0.048)	(0.038)	(0.034)	(0.016)	(0.023)	(0.014)
Interaction	-0.632	-0.520^{***}	-0.183^{**}	-0.164^{***}	-0.152^{***}	-0.189^{***}	-0.214^{***}	-0.178^{***}	-0.206^{***}	-0.204^{***}
	(0.431)	(0.191)	(0.074)	(0.029)	(0.044)	(0.042)	(0.035)	(0.015)	(0.021)	(0.019)
Quadratic	-1.007^{*}	-0.539^{***}	-0.190^{**}	-0.163^{***}	-0.169^{***}	-0.188^{***}	-0.224^{***}	-0.193^{***}	-0.215^{***}	-0.212^{***}
	(0.592)	(0.187)	(0.077)	(0.025)	(0.057)	(0.040)	(0.035)	(0.028)	(0.021)	(0.017)
Cubic	-5.551^{**}	-0.533^{**}	-0.181	-0.170^{***}	-0.154^{***}	-0.174^{***}	-0.242^{***}	-0.202^{***}	-0.216^{***}	-0.214^{***}
	(2.238)	(0.211)	(0.168)	(0.020)	(0.043)	(0.044)	(0.038)	(0.027)	(0.020)	(0.016)
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

APPENDIX C. APPENDIX TO CHAPTER 3

	Dependent variable: climate policy equilibrium									
	Bandwidth:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-0.254^{***}	-0.241^{***}	-0.256^{***}	-0.247^{***}	-0.245^{***}	-0.252^{***}	-0.262^{***}	-0.260^{***}	-0.273^{***}	-0.275^{***}
	(0.096)	(0.021)	(0.019)	(0.007)	(0.005)	(0.010)	(0.011)	(0.014)	(0.009)	(0.008)
Interaction	-0.244^{**}	-0.252^{***}	-0.280^{***}	-0.247^{***}	-0.248^{***}	-0.254^{***}	-0.260^{***}	-0.254^{***}	-0.267^{***}	-0.267^{***}
	(0.103)	(0.034)	(0.017)	(0.009)	(0.007)	(0.005)	(0.009)	(0.009)	(0.031)	(0.007)
Quadratic	-0.220^{***}	-0.328^{***}	-0.283^{***}	-0.252^{***}	-0.255^{***}	-0.248^{***}	-0.268^{***}	-0.263^{***}	-0.267^{***}	-0.267^{***}
	(0.033)	(0.031)	(0.013)	(0.011)	(0.007)	(0.004)	(0.006)	(0.010)	(0.008)	(0.008)
Cubic	-0.264^{***}	-0.323^{***}	-0.266^{***}	-0.243^{***}	-0.231^{***}	-0.241^{***}	-0.271^{***}	-0.264^{***}	-0.267^{***}	-0.267^{***}
	(0.034)	(0.042)	(0.013)	(0.019)	(0.024)	(0.009)	(0.012)	(0.007)	(0.010)	(0.011)
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192

Table C6: Effect of the East German regime on the climate policy equilibrium-No Moran eigenvectors

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

respectively.

Table C7: Effect of the East German regime on the climate policy equilibrium-No grid cell FEs

Dependent variable: climate policy equilibrium											
		Bandwidth:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)	
Linear	-0.200^{***}	-0.209^{***}	-0.221^{***}	-0.235^{***}	-0.235^{***}	-0.213^{***}	-0.195^{***}	-0.169^{***}	-0.159^{***}	-0.153^{***}	
	(0.026)	(0.044)	(0.051)	(0.047)	(0.044)	(0.048)	(0.047)	(0.051)	(0.057)	(0.057)	
Interaction	-0.212^{***}	-0.214^{***}	-0.213^{***}	-0.224^{***}	-0.224^{***}	-0.213^{***}	-0.204^{***}	-0.179^{***}	-0.174^{***}	-0.173^{***}	
	(0.039)	(0.043)	(0.049)	(0.046)	(0.043)	(0.048)	(0.046)	(0.053)	(0.058)	(0.057)	
Quadratic	-0.285^{***}	-0.217^{***}	-0.205^{***}	-0.214^{***}	-0.230^{***}	-0.232^{***}	-0.236^{***}	-0.243^{***}	-0.268^{***}	-0.265^{***}	
	(0.066)	(0.055)	(0.050)	(0.049)	(0.043)	(0.043)	(0.040)	(0.041)	(0.040)	(0.041)	
Cubic	-0.271^{***}	-0.191^{***}	-0.167^{***}	-0.154^{***}	-0.179^{***}	-0.216^{***}	-0.233^{***}	-0.245^{***}	-0.257^{***}	-0.256^{***}	
	(0.074)	(0.057)	(0.043)	(0.054)	(0.046)	(0.047)	(0.042)	(0.036)	(0.034)	(0.036)	
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917	
N.	336	704	960	1232	1488	1736	2048	2336	2920	3192	

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,

	Dependent variable: climate policy equilibrium									
	Bandwidth:									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Polynomial degree:	25 km	50 km	75 km	100 km	125 km	150 km	175 km	200 km	250 km	(Full sample)
Linear	-0.841^{***}	-0.402^{***}	-0.175^{*}	-0.172^{***}	-0.188^{***}	-0.199^{***}	-0.211^{***}	-0.185^{***}	-0.219^{***}	-0.222^{***}
	(0.238)	(0.122)	(0.090)	(0.059)	(0.048)	(0.039)	(0.038)	(0.030)	(0.032)	(0.022)
Interaction	-0.727^{***}	-0.417^{***}	-0.181^{**}	-0.172^{***}	-0.189^{***}	-0.201^{***}	-0.211^{***}	-0.186^{***}	-0.219^{***}	-0.222^{***}
	(0.226)	(0.134)	(0.083)	(0.060)	(0.048)	(0.042)	(0.031)	(0.029)	(0.032)	(0.022)
Quadratic	-0.980^{***}	-0.523^{***}	-0.171^{**}	-0.167^{***}	-0.204^{***}	-0.198^{***}	-0.213^{***}	-0.193^{***}	-0.222^{***}	-0.225^{***}
	(0.377)	(0.111)	(0.086)	(0.063)	(0.047)	(0.041)	(0.028)	(0.024)	(0.029)	(0.021)
Cubic	-0.223^{***}	-0.529^{***}	-0.152^{***}	-0.158^{***}	-0.174^{***}	-0.183^{***}	-0.209^{***}	-0.194^{***}	-0.222^{***}	-0.224^{***}
	(0.083)	(0.150)	(0.029)	(0.058)	(0.047)	(0.050)	(0.028)	(0.024)	(0.029)	(0.023)
Mean DV	6.956	6.954	6.943	6.937	6.935	6.933	6.934	6.932	6.925	6.917
Ν.	336	704	960	1232	1488	1736	2048	2336	2920	3192

Table C8: Effect of the East German regime on the climate policy equilibrium-No pre-processing algorithm

Conley standard errors in parentheses. The ***/**/* represent significance at the 0.001/0.005/0.01 levels,