

Democracy in the Crossfire: The Electoral Fallout of Extremist Political Violence

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Abstract

What are the effects of political violence on election outcomes in established democracies? We argue that exposure to political violence raises citizens' demands for security. Existing accounts disagree on whether this demand rewards incumbents, conservative parties, or the radical right, but they agree that it should cost extremist parties perceived as contributing to instability. Using a novel police dataset of 148 incidents of political violence, almost all from the far left or the far right, during Greece's snap parliamentary elections of May and June 2012, we employ a difference-in-differences design at the voting center level in Athens and Thessaloniki. We find no evidence that political violence affects voter turnout. Further, we find only tenuous evidence of an electoral boost from violent incidents for populist radical right parties. Political violence does, however, reduce the combined vote share of extremist parties in exposed areas. Separating far-left from far-right incidents suggests that each extremist party loses support after violence from its own ideological side, though the evidence is stronger for the far right and we treat the pattern as descriptive. We argue that the backlash against extremist parties as a whole speaks to research on how voters punish parties associated with violence, as well as to wider debates about democratic resilience in times of crisis.

Keywords: political violence, far right, far left, elections, democratic resilience, Greece, difference-in-differences

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1 Introduction

Political violence in established democracies poses a growing challenge in an era of “poly-crisis,” where interconnected economic, social, and political upheavals amplify one another (Lawrence et al., 2024). A growing body of research documents the electoral consequences of such violence, showing that terrorist attacks and extremist incidents can shift voter preferences toward conservative and radical right parties (Berrebi and Klor, 2008; Brouard, Vasilopoulos, and Foucault, 2018; Eger and Olzak, 2023; Krause and Matsunaga, 2023), alter turnout (Getmansky and Zeitzoff, 2014), and reshape incumbent evaluations (Montalvo, 2011; Aytac and Çarkoğlu, 2021). Yet important gaps remain. Most studies focus on single terrorist events or on violence from one ideological pole, typically the far right (Krause and Matsunaga, 2023; Eger and Olzak, 2023). Few examine how far-left (FL) and far-right (FR) violence operate simultaneously within the same political environment, or trace effects across the full party spectrum, from extremist to radical to mainstream parties.

In this paper we ask: How does exposure to FL and FR political violence affect voter turnout and party choice in established democracies? We theorize that political violence shapes electoral support by activating voter preferences for stability and security. Mortality salience, system justification theory, and the rally-around-the-flag effect offer competing priors about where those preferences send votes. All three predict a turn toward parties perceived as forces of order, though they disagree on whether incumbents, conservative parties, or the radical right benefit. In contrast, extremist parties, those that reject democratic institutions outright, are likely to lose support, as voters distance themselves from political actors associated with instability and violence.

We test these expectations using Greece’s twin parliamentary elections of May and June 2012, which offer distinctive methodological advantages. The narrow 42-day window between elections provides a quasi-experimental setting in which socioeconomic factors remain essentially constant, which allows for a difference-in-differences design at the voting center level. We leverage a novel police dataset recording 148 incidents of political violence in Athens and Thessaloniki, most from the far left and a smaller share from the far right. Only 5.4% of these incidents received national media coverage, which we argue lends confidence that our estimates capture direct, local exposure rather than media-mediated awareness. We complement our baseline estimates with placebo tests across multiple pre-treatment periods, sensitivity analysis for omitted variable bias (Cinelli and Hazlett, 2020), and matched-sample estimation to assess the robustness of our findings.

Our results reveal two distinct electoral responses to violence. The more robust is an electoral backlash against extremist parties. Exposure to violence reduces the combined

vote share of the two extremist parties, and violence recorded in earlier periods does not predict this decline. We focus on this combined penalty because it is stable, whereas the result for each party on its own changes with the inference method and the specification. Violence is also associated with increased support for radical right parties, driven primarily by the anti-establishment ANEL rather than by incumbents, though this association is less robust. Specifically, it is identified almost entirely off the municipality of Athens. It also does not hold once we control for the radical right’s May vote share or for the violence recorded shortly before the May election. The extremist penalty, in contrast, holds under both controls, as well as under covariate adjustment, matching, and the 500 m and 1000 m distance thresholds. We find no evidence that political violence affects voter turnout.

These findings contribute to debates on democratic resilience during periods of crisis (Merolla and Zechmeister, 2009; Ruggeri, Daxecker, and Prasad, 2025). Where prior work has examined violence from a single ideological source or focused on individual terrorist events, we provide evidence from a setting where FL and FR violence co-occur and where their effects can be traced across the full party spectrum. The Greek case, a post-authoritarian EU democracy experiencing severe economic crisis and political violence from both extremes (Dinas and Rori, 2013; Rori, Georgiadou, and Roumanias, 2022), shares features with other European democracies that have confronted rising political violence (Ravnald et al., 2023; Eger and Olzak, 2023). While our findings are drawn from a single national context and should be generalized with caution, the pattern of voter rejection of extremist alternatives under conditions of violence offers insights into democratic resilience, even as the consolidation of support around radical challengers suggests that such resilience may come with its own complications.

2 Definitions and conceptualizations

2.1 Political violence

Within the broad spectrum of political violence, our research narrows its scope to actions specifically designed to influence governance and political opponents by causing physical harm. The 148 violent episodes reported between the two elections range from vandalism such as damaging symbols, to outright violence such as stabbings, shootings, violent street protests, and even terrorist attacks, carried out by actors ranging from highly organized groups that claim responsibility for their attacks to spontaneous participants in escalating protests. We describe the incidents, their perpetrators, targets, and escalation levels in detail in Section 6.

To clarify the violence under research, we follow the typology of [Kalyvas \(2019\)](#), which classifies violence by whether the perpetrator and the target are state or non-state actors. The violence relevant to this study is initiated by non-state actors against both state and non-state entities, and stems from political, social, or cultural motivations.

We abstract from the meso-level dynamics specific to coordination within perpetrating groups, as conceptualized in Charles Tilly’s account of collective violence ([2003](#)) and in [della Porta’s \(1995\)](#) organizational approach. While most violent episodes in our study occur during mass protests that escalate into violence, the data also include terrorist attacks involving anti-immigrant, anti-elite, and anti-state coordinated violence. Despite occurring in the context of an electoral campaign, we do not classify this violence as "electoral violence" per se, since it lacks the coercive component directly associating violence with the electoral process ([Birch, Daxecker, and Höglund, 2020](#), p.4). The perpetrators are non-state actors with broader, non-electoral goals, such as overthrowing the political system or intimidating political, religious, or ethnic groups.

2.2 Extremist and radical political parties

The distinction between radical and extremist political parties is crucial for understanding their impact on democratic systems. Extremist parties are defined by their antithetical stance towards political pluralism and are characterized by authoritarianism and a drive for power concentration. These parties not only oppose the existing political establishment, but also fundamentally challenge the very principles of democracy. This opposition is often rooted in a belief that the survival or success of their in-group necessitates hostile actions against an out-group, legitimizing violence as a potential method of action. Extremist groups are "naturally driven toward action against the source of the putative evil," which may include both violent and non-violent actions ([Zúquete, 2023](#), p.xii). [Backes \(2023\)](#) provides a definition of FL extremism, describing it as an ideology that combines a radical democratic, egalitarian understanding of politics with a rejection of the institutionalized democratic procedures, often justified by a perceived higher moral authority or revolutionary imperative. As such, in the Greek context, the revolutionary, anti-capitalist ANTARSYA is classified as an extremist FL party ([Hawkins, Kaltwasser, and Andreadis, 2020](#)). Beyond Greece, the Irish SP-AAA and SWP-PBPA, the French NPA, the Norwegian R and the Socialist Workers Party (UK) belong to the revolutionary extreme left ([Escalona, Keith, and March, 2023](#), p.12). On the other side of the political spectrum, extremist FR parties are those that are anti-systemic and anti-democratic and display elective affinities with historical fascism ([Mudde, 2007](#)). Examples of contemporary FR extremist parties include the Italian CasaPound, the Hun-

garian Jobbik, the Cypriot ELAM and the Greek Golden Dawn (HA), all of which embrace authoritarian ideologies which promote nativist and xenophobic agendas that fundamentally oppose democratic principles (Georgiadou, Rori, and Roumanias, 2018; Ellinas, 2020).

Similar to their extremist counterparts, populist radical right parties (PRRPs) can be detrimental to democratic institutions and liberal norms. Scholars have demonstrated how the success of PRRPs influences mainstream parties to move toward more right-leaning positions (Abou-Chadi and Krause, 2020) and can make voters more open to policies that align with PRRP agendas (Valentim, Dinas, and Ziblatt, 2025). While acknowledging these parties’ roles in the erosion of liberal institutions and norms, it is important to distinguish between the radical FR and extremist FR parties. PRRPs share a critical stance towards the establishment and advocate for illiberal policies on issues such as immigration and public order, but they do not reject democratic principles outright (Ignazi, 2003). Following this definition, in the Greek context we identify as PRRPs the Popular Orthodox Rally (LAOS), Independent Greeks (ANEL) and the most recent Greek Solution (EL), whereas in the European context the French RN, the Austrian FPÖ, the Swedish SD, the Italian LN and FdI fall within the same classification (Georgiadou, Rori, and Roumanias, 2018; Rooduijn et al., 2024).

The same distinction should be made between extremist and radical left parties (RLPs). Within the many varieties of radicalism (Gomez, Morales, and Ramiro, 2016), scholars define RLPs as those which are located to the left of social democratic parties on economic issues, reject the underlying socioeconomic structures of contemporary capitalism and aspire to implement alternative economic and power structures (Escalona, Keith, and March, 2023), while they qualify as extreme those that are hostile to contemporary democratic regimes and endorse the revolutionary path or violence to gain power (March, 2012). The RLPs display high heterogeneity, encompassing conservative communists (the Greek KKE, the Portuguese PCP, the Italian PCI), reformist communists (the French PCF, the Cypriot AKEL), democratic socialists, also termed new RLPs (Gomez, Morales, and Ramiro, 2016), such as the Greek SYRIZA, the Portuguese BE, the France Insoumise (FI), and the Spanish IU (Escalona, Keith, and March, 2023). We acknowledge that the boundary between radical and extremist parties is not always sharp in public discourse, and that some parties classified here as “radical” are sometimes described as “extreme” in media and political commentary (Mudde, 2007). Nevertheless, we argue that the analytical distinction matters. Radical parties, however adversarial their rhetoric, operate within democratic rules, whereas extremist parties reject democratic institutions outright. This distinction is consequential for how voters respond to political violence, as the perceived association between extremism and instability may drive differential electoral penalties. We therefore study these groups

separately.

3 Previous scholarship on political violence and its effects on electoral outcomes

In democratic contexts, political violence is unlikely to resonate positively, even when the underlying demands may be viewed as legitimate. Although some circumstances might justify its use, violence is typically associated with negative connotations (Muñoz and Anduiza, 2019). Political violence is perceived as taboo (Van Aelst and Walgrave, 2001), often clashing with the values and needs of the broader population and is associated with a complex state of emotional arousal (Godefroidt, 2023), with fear and anger being the most widely experienced emotions following political violence (Vasilopoulos, Marcus, and Foucault, 2018; Vasilopoulos et al., 2019). Consequently, individuals are less inclined to support or identify with politically violent action.

Scholarship on the repercussions of political violence on voter behavior within democratic contexts examines its effects on voter turnout, support for incumbents, and opposition parties. Starting with voter turnout, on the one hand, this type of political violence can galvanize voter engagement, which can lead to an increase in voter turnout as citizens are motivated to reclaim their sense of agency and push for change (Vasilopoulos, 2018; Balcells and Torrats-Espinoza, 2018; Vasilopoulos and Brouard, 2020; Sønderskov et al., 2022; Leon-Ablan and John, 2024). Conversely, it can also act as a deterrent, fostering feelings of vulnerability and a diminished interest in participating in high-risk political activities, leading to reduced voter turnout (Guvercin, 2017; Nussio, Bove, and Steele, 2019; Rigutto, Sapara, and Agyapong, 2021; Godefroidt, 2023). Mixed effects are also observed, depending on the type of election (Brown and Zoorob, 2022).

The relationship between political violence and support for incumbents is complex and contingent. Violent protests often lead to backlash, which undermines support for incumbents. Some studies indicate that incumbents may be penalized in the aftermath of terrorist attacks, particularly if the public perceives their response to the crisis as inadequate (Bali, 2007; Montalvo, 2011; Leon-Ablan and John, 2024). Conversely, other studies suggest a rally around the flag effect, where incumbents may actually gain support following violent events, an outcome that can be significantly influenced by governmental reactions, the nature of the violence, and how these events are portrayed by the media (Berrebi and Klor, 2008; Chowanietz, 2011; Koch and Tkach, 2012; Aytac and Çarkoğlu, 2021; Brown and Zoorob, 2022; Godefroidt, 2023). There is also a third order of influence, stemming from studies that

show no effect for the incumbent (Balcells and Torrats-Espinosa, 2018) or both positive and negative (Falcó-Gimeno, Muñoz, and Pannico, 2023).

Terrorist attacks and collective violence have a notable effect on political conservatism, with many studies finding increasing support for FR policies and parties in their wake (Eger and Olzak, 2023; Krause and Matsunaga, 2023). This shift is often mediated through emotional responses to perceived threats, where the heightened sense of insecurity and fear drives voters towards more authoritarian, security-focused political stances (Berrebi and Klor, 2008; Merolla and Zechmeister, 2009; Butler, 2013; Getmansky and Zeitzoff, 2014; Brouard, Vasilopoulos, and Foucault, 2018; Lindén, Björklund, and Bäckström, 2018; Vasilopoulos, Marcus, and Foucault, 2018; Vasilopoulos et al., 2019; Rees and Smith, 2022; Pickard, Efthyvoulou, and Bove, 2023; Godefroidt, 2023; Vlandas and Halikiopoulou, 2025). Recent evidence extends this pattern to anti-system voting more broadly. In interwar Italy, exposure to political violence raised support for anti-system parties, which predominantly benefited the nationalist right (Viganò et al., 2025).

Lastly, the presence of political violence generally leads to a decline in support for extremist parties. Public support for social movements decreases significantly with the outbreak of riots and violence more broadly (Muñoz and Anduiza, 2019). Voters tend to distance themselves from parties and movements associated with or endorsing violence, which likely reflects a broader societal preference for stability and a system-justifying attitude under threat conditions. This backlash can significantly reduce public support for movements or candidates linked to violence (Jost, Banaji, and Nosek, 2004; Ullrich and Cohrs, 2007; Simpson, Willer, and Feinberg, 2018; Feinberg, Willer, and Kovacheff, 2020; Pickard, Efthyvoulou, and Bove, 2023). Recent work sharpens the logic behind this backlash. In mature democracies, voters withdraw support from politicians who instigate or condone political violence (Krakowski and Morales, 2025). Despite these general trends, however, the specific electoral effects of political violence on different types of extremist parties remain underexplored.

4 Theorizing electoral participation and vote choice in the presence of political violence

Voters decide whether to participate in elections based on the perceived benefits and costs of voting compared to abstaining (Downs, 1957; Riker and Ordeshook, 1968). The benefits include influencing governmental stability and policy direction (Downs, 1957), which becomes more significant during crises like the 2012 Greek elections, as well as fulfilling a civic duty, affirming political allegiance, and expressing partisan preferences (Riker and Ordeshook,

1968).

Political violence is intrinsically tied to the political process through the government’s responsibility to maintain peace, order, and security, which is a core expectation that citizens place on law enforcement (Wilson, 2013). Violence might therefore stimulate electoral participation by heightening citizens’ demands for government action, including increased policing or repression. In studies examining the effects of terrorism, feelings of anger following an “unjust” attack are thought to even fuel a preference for high-risk, retaliatory measures (Godefroidt, 2023). In other words, the instrumental benefits of electoral participation increase as political violence intensifies (Brown and Zoorob, 2022), especially when residents believe that relief from neighborhood decline and personal risk depends on government intervention.

However, the relationship may also work in the opposite direction. Political violence can foster feelings of vulnerability, helplessness, and anxiety that reduce citizens’ willingness to engage in political activity (Guvercin, 2017; Nussio, Bove, and Steele, 2019; Rigutto, Sapara, and Agyapong, 2021). The perceived costs of participation, including physical risk and a sense that the political system is unable to ensure basic security, may outweigh its expected benefits (Godefroidt, 2023). In contexts where violence is diffuse and perpetrators span the ideological spectrum, the resulting disillusionment may suppress turnout rather than stimulate it. The theoretical expectations therefore point in competing directions, and we formulate our hypothesis accordingly:

H1: Incidents of political violence are associated with voter turnout, though the direction of the effect is theoretically ambiguous.

Understanding how political violence shapes vote choice in established democracies involves analyzing voter reactions to disruptions in stability and security. We argue that political violence can recalibrate voters’ preferences, prompting a shift toward parties perceived as forces of order, such as incumbents, conservative, or radical right-wing parties, while driving support away from extremist parties seen as contributing to instability. We note here that the psychological accounts we draw on below are competing priors rather than a single cumulative mechanism. Further, because our design observes aggregate returns rather than individual attitudes, we treat these theories as rival interpretations to be weighed against the pattern of results, and we phrase our expectations accordingly.

Individual and collective responses to political violence are triggered by both cognitive and emotional mechanisms (Godefroidt, 2023). Cognitively, confronting the unpredictable nature of death heightens mortality salience, which amplifies attitudes toward political issues and figures and prompts a general shift toward conservatism, irrespective of prior ideological

leanings (Burke, Kosloff, and Landau, 2013). System Justification Theory (SJT) sharpens this expectation, holding that individuals are motivated to perceive existing social, economic, and political arrangements as legitimate and just, especially when confronted with existential threats such as political violence (Jost, Banaji, and Nosek, 2004; Jost and Van der Toorn, 2012; Sterling, Jost, and Shrout, 2016), so that the heightened salience of security and stability draws voters toward the status quo. Emotionally, political violence is associated with negative discrete emotions such as anxiety, fear, anger, and sadness (Godefroidt, 2023), which system-justifying attitudes have been shown to palliate following terrorist attacks (Vasilopoulos and Brouard, 2020). To cope with this anxiety, individuals adopt politically risk-averse attitudes, endorsing existing arrangements regardless of the ideology of those representing them (Kosloff, Landau, and Burke, 2016; Bäck et al., 2023), and strengthen their attachment to and trust in the nation and its leaders (Godefroidt, 2023), a phenomenon commonly referred to as the “rally-’round-the-flag” effect (Mueller, 1970). In line with SJT, Mortality Salience Theory (MST), and the rallying psychological process, we posit that voters exposed to such threats are more likely to see the benefits of supporting the incumbent, who is perceived as a defender of the existing system:

H2: Incidents of political violence are expected to be positively associated with the electoral score of the incumbent.

Beyond the psychological background of support to the status quo, MST and SJT evidence a conservative shift also linked to vote choice (Burke, Kosloff, and Landau, 2013; Bäck et al., 2023). A chronic fear of death and a low tolerance for ambiguity are closely associated with political conservatism (Jost, Banaji, and Nosek, 2004), and conservative ideology is evidenced to reduce uncertainty and threat (Jost and Napier, 2011; Jost et al., 2007). SJT suggests that in the face of political violence, voters shift towards more conservative views to maintain stability and order (Jost and Van der Toorn, 2012), favoring conservative parties with strong positions on security and law enforcement. As a result, we expect that:

H3a: Incidents of political violence are expected to be positively associated with a shift in voter support towards conservative parties.

In line with the conservative shift hypothesis, a substantial body of findings documents that political violence boosts support for radical right-wing parties (Berrebi and Klor, 2008; Kibris, 2011; Getmansky and Zeitsoff, 2014; Brouard, Vasilopoulos, and Foucault, 2018; Rees and Smith, 2022; Eady, Hjorth, and Dinesen, 2023; Krause and Matsunaga, 2023). Affective Intelligence Theory explains this shift through distinct emotional responses and appraisals (Marcus et al., 2019), with emotional reactions driving the impact of ideological dispositions

on support for authoritarian policies (Vasilopoulos, Marcus, and Foucault, 2018; Vasilopoulos et al., 2019). Voters exposed to political violence become more sensitive to issues of security and order, and, disillusioned with establishment parties perceived as ineffective or responsible for the ongoing crisis, may favor radical right-wing parties that promise drastic change or a return to an idealized order. As such, we hypothesize:

H3b: Incidents of political violence are expected to be positively associated with the electoral score of radical right parties.

Because the mechanism runs through the demand for security and order, it privileges radical right parties only where they credibly own those issues, which we examine for the Greek case in Online Appendix A.2.

An important question is whether FL and FR violence produce symmetric or asymmetric electoral effects. Theoretically, there are reasons to expect both. On the one hand, the psychological mechanisms outlined above may operate regardless of the perpetrator’s ideology, with any violence that heightens threat perception triggering a conservative shift. On the other hand, the perpetrator’s ideological identity may matter. FR violence may activate concerns about immigration and public safety that specifically benefit radical right parties, as documented in Germany (Eger and Olzak, 2023; Krause and Matsunaga, 2023). FL violence, by contrast, may activate concerns about social disorder and anti-state radicalism that benefit conservative incumbents more broadly. Because our empirical setting features substantial FL and FR violence occurring simultaneously, we are able to test whether these types of violence produce differential electoral effects.

Lastly, we consider the implications of political violence for fundamentally anti-systemic and extremist political parties. Research on the effects of terrorism has highlighted attitudinal and emotional reactions that foster outgroup hostility and ingroup solidarity (Godefroidt, 2023). Extremist parties seeking to undermine the democratic status quo often face significant voter backlash when perceived as connected to acts of violence, whether through alignment, endorsement, or organizational ties. As concerns about law and order become more prominent among a threatened, anxious, and fearful electorate, voters tend to distance themselves from political choices that appear to endorse or perpetuate violence, instead favoring stability and the preservation of democratic order. We therefore expect that:

H4: Exposure to political violence is expected to be negatively associated with the electoral score of extremist parties.

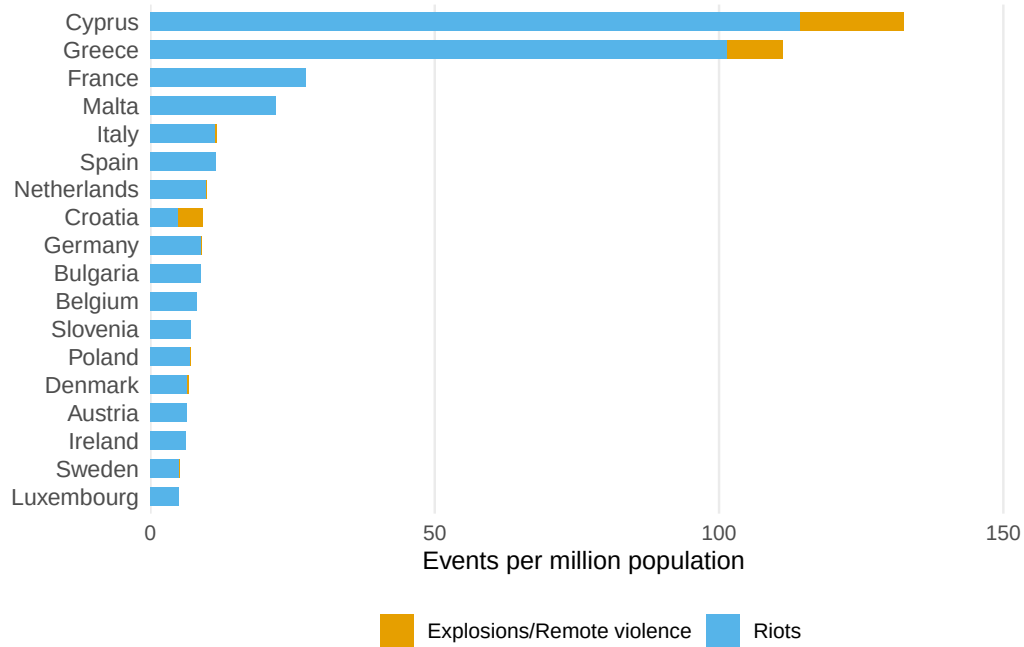


Figure 1: Political violence in the EU per million population (2018–2024). Greece and Cyprus record far higher rates of riots and explosions than other member states. Countries with fewer than five events per million are excluded. Data from ACLED ([Raleigh, Kishi, and Linke, 2023](#)).

5 Political violence and elections in the context of the Greek financial crisis

Since the 1974 transition to democracy, ending a seven-year military junta, Greek politics were dominated by a stable two-party system, with the executive controlled by single-party majorities and the country’s modernization tightly linked to Europeanization. This trajectory ended abruptly in 2009 with a sovereign debt crisis, exacerbated by harsh austerity measures and reforms, leading to economic and political collapse. The economy endured the longest recession of any advanced European democracy, losing 25.9% of GDP between 2008 and 2015 ([Matsaganis, 2018](#)). The crisis fueled polarization, with voters abandoning mainstream parties for radical and extremist challengers ([Dinas and Rori, 2013](#); [Rori, 2016](#)).

As shown in Figure 1, Greece and Cyprus record the highest per-capita rates of political violence in the EU, roughly four times more events per million inhabitants than France and over ten times the rate of most other member states. Such sustained political violence in established democracies is not without historical precedent. Italy and Germany, for example, experienced significant extreme political violence during the 1960s-1980s (on the Italian “Years of Lead,” see the incident-level data of [Costalli et al., 2025](#)), and similar dynamics

are observable today across diverse national contexts (Eger and Olzak, 2023; Krause and Matsunaga, 2023; Ruggeri, Daxecker, and Prasad, 2025). While manifestations of violent political extremism existed in the post-1974 democratic regime (Andronikidou and Kovras, 2012; Kassimeris, 2013), political violence reached a new peak following the 2008 murder of a 15-year old student by a police officer. The widespread riots that ensued marked the emergence of a new generation of FL violent organizations (Rori and Georgiadou, 2023). Less than two years later, the economic crisis brought tens of thousands of citizens to the streets (Gerodimos, 2015; Karyotis and Rüdiger, 2018), while violent right-wing extremism became increasingly prevalent in newly formed strongholds in immigrant areas of Athens (Dinas et al., 2016). Social movements motivated by a variety of causes, movements of indignation and anger, antifascist mobilization, and ongoing strikes became the norm (Georgiadou et al., 2019; Malamidis, 2020). Concurrently, political violence stemming from both the FR and FL unfolded through old and new repertoires, either in the wake of mass mobilizations or in more or less organized forms independent of them (Rori, Georgiadou, and Roumanias, 2022).

We argue that the Greek case, while specific in its particulars, offers instructive parallels for other advanced democracies grappling with political violence. The May and June 2012 parliamentary elections occurred at the peak of Europe’s "polycrisis," where interconnected crises, such as economic instability and political upheaval, amplified each other’s effects (Lawrence et al., 2024), and the dynamics of violence and democratic resilience observed in Greece speak to conditions that persist across Europe and beyond (Hutter and Kriesi, 2019).

The chronic character of political violence in Greece raises an apparent puzzle. If citizens are habituated to violence, why would the incidents we study move their vote at all? We argue that the distinction between *ambient awareness* and *direct, local exposure* matters here. Habituation operates on the former, while our treatment captures the latter. Nearly all incidents in our data (94.6%) never reached national media and would have registered mainly through local exposure, where spatial proximity sharply amplifies threat perception (Gouseti, 2016). If anything, habituation to background violence should attenuate the marginal response we estimate, thus making our findings conservative.

6 Research design

6.1 Data description and summary statistics

Our electoral data consist of results aggregated from the precinct level to the voting center level for the cities of Athens and Thessaloniki. Voting centers have 1 to 22 precincts (mean =

7.5, median = 7) with registered voters varying from 501 to 15,565 (mean = 4,249, median = 3,853). We investigate the electoral scores of the nine parties listed in Table 1: the seven that gained parliamentary representation in the May and June 2012 snap elections, together with LAOS and ANTARSYA, which fell below the 3% threshold but which we retain, respectively, as a radical right party recently in the incumbent coalition and as the far-left extremist counterpart to Golden Dawn.

Following the distinction between radical and extremist parties developed in Section 2, Table 1 summarizes the ideological classification of each party examined in this study.

Table 1: Party Classifications by Ideological Position

Party	Abbreviation	Classification
New Democracy	ND	Centre-right (incumbent)
Panhellenic Socialist Movement	PASOK	Centre-left (incumbent)
Democratic Left	DIMAR	Centre-left
Communist Party of Greece	KKE	Radical left
Coalition of the Radical Left	SYRIZA	Radical left
Popular Orthodox Rally	LAOS	Populist radical right
Independent Greeks	ANEL	Populist radical right
Golden Dawn	HA	Far-right extremist
Anticapitalist Left Cooperation	ANTARSYA	Far-left extremist

In operational terms, LAOS and ANEL participated in electoral competition and accepted democratic norms, making them radical right parties, whereas Golden Dawn’s explicitly anti-democratic ideology, neo-Nazi symbols, and documented connections to organized political violence mark it as FR extremist. On the left, ANTARSYA’s revolutionary anti-capitalist program and rejection of parliamentary democracy mark it as FL extremist, while KKE and SYRIZA, despite their radical positions, operated within the democratic system. Online Appendix A.2 compares these assignments against expert surveys and coded party manifestos, which further helps to separate extremist parties from their radical counterparts on the dimension of commitment to political pluralism. ANTARSYA, though electorally marginal, anchors the revolutionary anti-capitalist left most directly implicated in crisis-era violence (Escalona, Keith, and March, 2023; Rori and Georgiadou, 2023; Malamidis, 2020; Rori, Georgiadou, and Roumanias, 2022). Its inclusion allows us to test whether the extremist penalty is symmetric across both poles, though we rest our substantive claim on the family-level penalty rather than on ANTARSYA’s individual estimate.

Political violence is operationalized using a unique dataset of incidents of political violence recorded daily by the Greek police. Specifically, there were 148 incidents of political violence recorded by the police in the two cities under study between the elections. These records include a full description of a violent act deemed as being politically motivated, including time

and exact location of the incident, as well as a variety of characteristics such as actors, targets and escalation. Following the coding schema offered by [Rori, Georgiadou, and Roumanias \(2022\)](#), our dataset also distinguishes between FR and FL incidents.

While police data provides comprehensive coverage, we acknowledge potential reporting biases. FR political violence may be underestimated, as immigrant victims might avoid reporting crimes to the police due to deportation fears or distrust of authorities, whereas incidents such as property damage to public buildings are systematically recorded regardless of victim reporting. To the extent that such underreporting introduces measurement error in our treatment variable, it would attenuate our estimates toward zero, making our findings conservative, and even non-random underreporting of FR violence would misclassify treated precincts as control and thus work against finding significant effects. Municipality fixed effects further absorb systematic differences in reporting propensities across local areas.

To evaluate potential reporting biases and media spillover effects, we cross-checked our police dataset against newspaper and online media coverage during the same period. Of the 148 incidents in our dataset, only 8 (5.4%) were reported in national media sources, with 3 additional incidents reported in media but not recorded by police. The vast majority of incidents thus remained invisible to the broader public, which demonstrates the advantage of comprehensive police data over media-based measures and suggests that our findings reflect direct, local exposure to political violence rather than media-mediated awareness of distant events.

Our measure of escalation (low vs. high) assesses the possibility of harm to human targets, ranging from acts that pose no threat of physical harm, such as occupations of buildings, threats, and jeering, to severe acts that can cause injury or loss of life, such as arson, armed attacks, bombings, and assassinations.

Figure 2 summarizes the incidents by attacker ideology, target type, and escalation. Far-left (FL) incidents were more frequent than far-right (FR) incidents (120 vs 24), material targets more common than human targets (100 vs 48), and high escalation incidents more common than low escalation ones (99 vs 49). Material targets included vehicles, businesses, and political or state buildings, while human targets primarily involved civilians rather than state actors. FL violence was predominantly carried out by anarchist groups attacking state symbols and property, while FR violence was largely perpetrated by Golden Dawn members and sympathizers targeting immigrants and left-wing activists. More detailed descriptive statistics on the characteristics of the attacks, means used, organizations claiming responsibility, and specific targets and actors are given in Online Appendix A (Tables A1 and A2). Previous findings show that urban areas such as Athens and Thessaloniki experience higher levels of politically motivated violence than the rest of the country ([Rori, Georgiadou,](#)

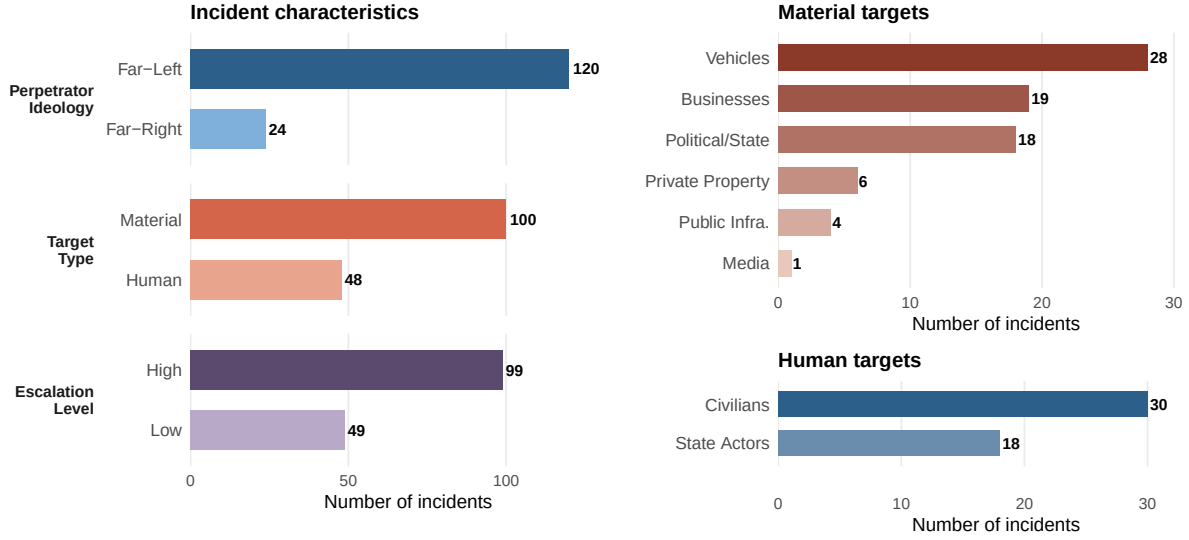


Figure 2: Distribution of political violence incidents between the two elections. The left panel summarises incident characteristics by perpetrator ideology, target type, and escalation level. The right panels break down material and human targets into substantive categories. Categories with lower frequencies, “Racial” and “Unknown” in ideology (each $n = 2$), are excluded for clarity.

and Roumanias, 2022), and the precise geo-location of incidents allows accurate counts within any radius of voting centers.

6.2 Methodology and identification strategy

Let Y_i^j denote the voting share of a party y in voting center i in election j , where $j \in \{\text{May}, \text{June}\}$. We assume that all socioeconomic factors affecting the vote in center i (e.g. regional unemployment, immigration as well as national unemployment, immigration, inequality etc.) remain constant in the 42 days between the two elections. Assume a typical voting function for the voting share of the party in either the first (May 2012) or second (June 2012):

$$Y_i^j = a_0 + T_k \cdot \mathbb{1}(j = \text{June}) + a_1 \text{Violence}_i + a_2 \text{Unemployment}_i + a_3 \text{Immigration}_i + \dots + u_i \quad (1)$$

where Violence is a measure of the violence preceding the elections in the area of the voting center. The term $T_k \cdot \mathbb{1}(j = \text{June})$ captures a common shift (parallel trend) in the vote share between May and June for reasons affecting all precincts within a specific area k .

Equation (1) cannot be estimated consistently because of likely endogeneity in the occurrence of political violence; for example areas with more anti-systemic attitudes are likely to both have higher incidence of political violence and vote more for anti-establishment or

radical parties. To correct for endogeneity of political violence we adopt a difference in differences methodology:

Taking first differences between June and May vote shares yields:

$$\begin{aligned}\Delta Y_i &= T_k + a_1 \Delta \text{Violence}_i + a_2 \Delta \text{Unemployment}_i + a_3 \Delta \text{Immigration}_i + \dots + \Delta u_i \\ &= T_k + a_1 \Delta \text{Violence}_i + v_i \\ &= T_k + a_1 \text{Treatment} + v_i\end{aligned}\tag{2}$$

In equation (2), our Treatment is the incidents of violence that took place between the two elections ($\Delta \text{Violence}_i = \text{Violence}_i^{\text{June}} - \text{Violence}_i^{\text{May}}$). Equation (2) allows identification of the effect of a variation in treatment between areas which experienced violence and areas without incidence of violence between the two elections on the difference in the vote share of party y (Diff-in-diff with continuous treatment design). Our design depends on the parallel trends (T_k) assumption. Note that the parallel trend assumption we use is very mild as it permits precincts in different municipalities to experience different parallel trends. This is a relaxation of the rather restrictive traditional parallel trends assumption.

Equation (2) is the model that differencing implies when the covariates are constant across the six weeks. The specification we estimate throughout adds those covariates in levels alongside the municipality effects:

$$\Delta Y_i = T_{k(i)} + a_1 \text{Dosage}_i + \gamma' X_i + v_i,\tag{3}$$

where Dosage_i is the number of incidents within 750m of voting center i during the inter-election window, $T_{k(i)}$ is the fixed effect of the center's municipality, and X_i includes seven control variables. Specifically, we include postal code-level mean family income and the following variables at the local community-level: shares of residents with a doctorate, a master's degree, a bachelor's degree, the share without Greek citizenship, the unemployment rate, and the youth unemployment rate.¹ Although these characteristics do not change between May and June, their coefficients allow the common May-to-June shift to vary with local observables, just as T_k allows it to vary across municipalities, so the parallel trends assumption is required to hold conditional on them.

From an electoral perspective, the parallel trends assumption is reasonable, since the rather elaborate electoral law in Greece of "reinforced proportionality", which imposes a

¹Family income is for 2011, from the Independent Authority for Public Revenue (AADE); the other variables are from the 2011 population census. Online Appendix A.1 gives definitions and summary statistics.

majoritarian logic by favouring the first party with a bonus of 50 seats, leaves little room for calculating how electoral gains or losses in specific areas translate to seats in the Parliament. The total shift for each party (common parallel trend) can be then easily calculated by summing up over all municipalities $T = \sum_k T_k$. Individual parallel trends are estimable and can be obtained by extracting the municipality-level fixed effects from the estimated regression.²

For inference, we report Conley spatial-HAC standard errors (Conley, 1999) with a 1 km cutoff for all coefficients, since the overlapping 750 m exposure buffers induce spatial dependence across neighbouring voting centers. For several outcomes the Conley standard error is smaller than its HC1 counterpart. This reflects the concentration of the identifying variation in about 11 heavily exposed centers in central Athens rather than negative spatial dependence.³ The 1 km cutoff approximates the range over which the estimated effect of violence decays, and our conclusions are insensitive to alternatives of 0.5 and 2 km (Online Appendix B.1). Online Appendix B.1 also reports a leverage-adjusted Conley estimator, a more conservative version that corrects for the influence of those few heavily exposed centers.

We also compare our primary inference with three alternative methods. The alternatives are clustering on 1 km grid cells, a wild bootstrap that treats each of the 41 municipalities as a single cluster, and randomization inference, which relocates each incident within its municipality. We treat Conley inference as primary and municipality clustering as a stress test. Clustering by municipality is called for when municipalities are sampled or when treatment is assigned at that level (Abadie et al., 2023), and neither holds here. Treatment varies at the level of the voting center, which is much more granular than the municipality, and our sample is every voting center in the two metropolitan areas. While failing the stress test does not overturn a result, passing it shows that the result holds regardless of how we model spatial dependence. Section 7 reports the comparison in Table 2 and states which results depend on the choice.

Furthermore, areas may have responded differently to the May 2012 result, which broke the established two-party system and lifted radical and extremist parties. Two checks on this concern recur in our results. First, violence may be located where a party did unusually

²The parallel trends assumption is not directly testable in our two-period framework (Riveros-Gavilanes, 2023; Dette and Schumann, 2024). The tight temporal window between the May and June 2012 elections strengthens identification by minimizing confounding from slow-moving demographic change, but it also means that the nearest prior election (2009) is too distant to serve as a credible pre-treatment period for trend testing. We instead provide indirect support for the assumption through placebo tests using pre-treatment violence and earlier electoral periods (Online Appendix D.1).

³We use a Bartlett kernel, and the covariance matrix is positive semi-definite for every outcome and cutoff we report. Moran’s I of the residuals is positive or indistinguishable from zero for every outcome. Least squares forces the residuals of the heavily exposed centers to offset one another, and the kernel reads that offsetting as negative covariance. The diagnostics are in Online Appendix B.1.

well or poorly in May, and such results tend to drift back in June, which would produce an association without any effect of violence. We therefore test whether the placement of violence tracks the May result and add each party’s May vote share as a control (the mean reversion adjustment, Online Appendix C.7). Second, violence tends to recur in the same neighborhoods, which may have been shifting politically for unrelated reasons. To address this, we add the violence recorded shortly before the May election as a control (the nearest-window control, Online Appendix D.3). In addition, we estimate our models at varying distance thresholds and repeat them on a matched sample (Online Appendix C). We also verify that policing itself did not change between elections. The inter-election period was governed by a caretaker administration with no mandate to alter security policy, and the ratio of arrests to incidents shows no departure from trend (Online Appendix C.6).

6.3 Defining control and treatment groups

Our study investigates electoral choice in 603 voting centers in the metropolitan areas of wider Athens and Thessaloniki. We consider the proximity of political violence incidents to the voting center, specifically within a 750m radius. Our control group consists of voting centers with no incidents of political violence between the two consecutive parliamentary elections of 2012. The 750m radius was chosen as it represents an “extended neighborhood” which allows residents to feel affected by the incident while maintaining variation between treated and non-treated centers. This radius also allows for comparable control units with similar socioeconomic characteristics to the treatment group and provides enough power for our regressions.

We recognize that violence effects might extend beyond this radius, potentially through social networks, word-of-mouth, or local media coverage. However, our focus on direct local exposure is theoretically motivated as psychological research suggests that proximity amplifies threat perception and emotional responses to violence (Gouseti, 2016). Our supplemental analyses in Online Appendix C.4 show that the results are robust to widening the radius to 1000m, while narrowing it to 250m, where fewer incidents are captured and statistical power is lower, weakens some estimates. We argue that, if anything, broader diffusion of violence effects beyond our radius would bias our estimates downward, making our findings conservative.

Our treatment group consists of voting centers around which *at least one incident of political violence* took place, with varying exposures, and we exploit this variation to assess the impact of increasing doses of violence on electoral outcomes. Figure 3 maps the spatial distribution of violence exposure across voting centers in the Athens metropolitan area (Thes-

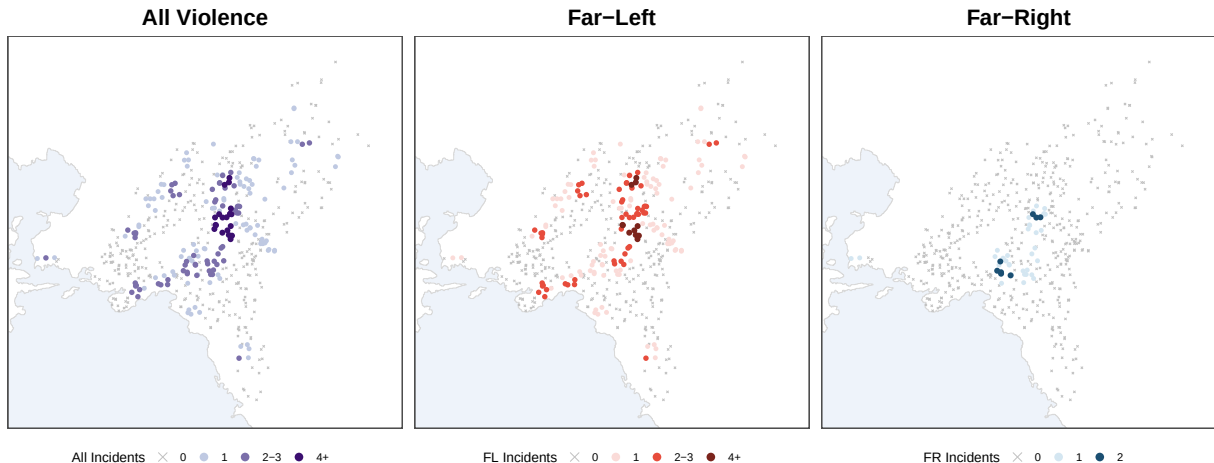


Figure 3: Spatial distribution of political violence exposure across voting centers in the Athens metropolitan area. Each dot represents a voting center; the left panel shows total incidents within 750m, the center panel far-left incidents, and the right panel far-right incidents. Darker shading indicates higher exposure to violence; grey crosses mark unexposed centers.

saloniki, where all recorded incidents were perpetrated by far-left actors, is omitted from the figure but included in the full regression sample). The geographical distribution reveals that exposed and unexposed units are geographically interspersed, something corroborated by our matching analysis in Online Appendix C.

7 Findings on the effects of political violence on turnout and vote choice

We next turn to the effects of political violence on turnout and vote choice, which are illustrated in Figure 4. Panel (a) shows the estimates for turnout and the three party families, and panel (b) the estimates for each party, ordered from the extreme left to the extreme right. Tables reporting the full set of statistical results are included in Online Appendix B. To aid interpretation of effect sizes, we note that of the 603 voting centers in our sample, 223 (37%) experienced at least one incident of political violence within 750m between the two elections. Among treated centers, the median exposure is 1 incident (mean = 2.1, interquartile range: 1–2, 90th percentile: 3). The distribution is right-skewed, with a maximum of 20 incidents concentrated in central Athens.

To test our first hypothesis (H1), we examined the effect of the number of political violence incidents (of all types) on voter turnout. Our results show a negative but negligible effect that does not reach statistical significance ($p = 0.69$), with each additional incident

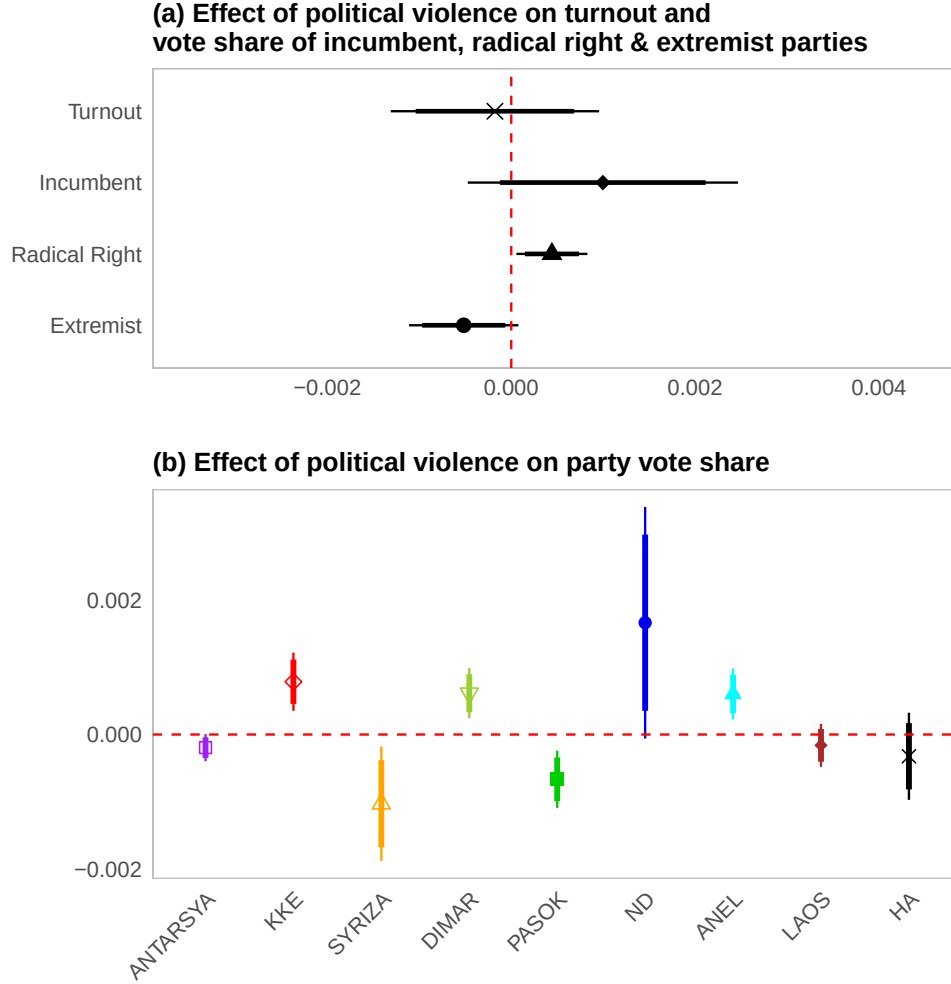


Figure 4: The effects of political violence on voter turnout and vote choice. (a) Shows the impact of political violence on turnout and on the vote share of incumbent, radical right, and extremist party families. (b) Presents party-specific effects, ordered from extreme left to extreme right. Plots display estimated coefficients with 95% (thick) and 99% (thin) confidence intervals, computed from Conley spatial-HAC (1 km) standard errors. Models include control variables, omitted here for clarity. Full results are in Online Appendix B.

associated with less than a 0.03% change relative to baseline turnout. This estimate remains statistically indistinguishable from zero in matched-sample estimation and at every distance threshold we consider, from 250m to 1000m (Online Appendix C.3–C.5).

We next turn to the effects of political violence on vote choice. Our theoretical framework posited that incidents of political violence would increase the electoral score of the incumbent party (H2). For H2, our analysis estimates the effect of political violence on the combined vote share for the incumbent parties ND and PASOK. The results show a positive effect, with each incident associated with roughly a 0.5% increase relative to the incumbent fam-

Table 2: The violence coefficient under alternative inference methods. Main 750 m specification with covariates and municipality fixed effects; point estimates are identical across columns and only the inference differs. Conley: spatial-HAC standard errors with a Bartlett kernel and a 1 km cutoff (primary). Grid-cell cluster: standard errors clustered on 1 km×1 km cells. Municipality bootstrap: wild-cluster bootstrap- t over the 41 municipalities, 1,999 Rademacher draws. Randomization: each incident relocated to a randomly drawn voting center in its own municipality and the 750 m counts rebuilt, studentized statistic, 99,999 draws. Online Appendix B.1 adds the leverage-adjusted Conley and the municipality CR2 columns, the remaining parties, and the diagnostics behind each column. ** $p < 0.01$; * $p < 0.05$.

Outcome	Coefficient	p -value of the violence coefficient			
		Conley 1 km	Grid-cell cluster	Municipality bootstrap	Randomization
Turnout	−0.0002	0.686	0.759	0.206	0.751
Incumbent	0.0010	0.081	0.289	0.562	0.526
Radical Right	0.0004	0.003**	0.009**	0.542	0.022*
Extremist	−0.0005	0.025*	0.024*	0.025*	0.049*
ND	0.0017	0.013*	0.125	0.462	0.369
ANTARSYA	−0.0002	0.013*	0.129	0.629	0.102
Golden Dawn	−0.0003	0.199	0.215	0.049*	0.260

ily’s baseline support, though the estimate does not reach statistical significance ($p = 0.08$ under Conley inference, 0.14 under the leverage-adjusted estimator). While the direction is consistent with our hypothesis, the estimate is imprecise and we do not treat it as a firm finding.

Turning to H3a, we explore the relationship between political violence and support for mainstream conservative parties. The results show effects in the expected direction. ND, the conservative coalition partner with a long-standing association with public order (Online Appendix A.2), shows a positive effect, with each incident associated with a 1.3% increase relative to ND’s baseline vote share and a roughly 3.8% gain at the 90th percentile of treatment intensity. The estimate is significant under Conley inference ($p = 0.013$) but under no other inference method in Table 2, and it weakens under the mean reversion and nearest-window adjustments (Online Appendices C.7 and D.3). We therefore treat the association as suggestive rather than established.

H3b asks whether the radical right gains documented in other settings replicate in ours. At face value they do, as the family-level effect is positive and significant at the 1% error level ($p = 0.003$), with each incident increasing the radical right vote share by roughly 0.3% of its baseline support. The composition of the effect, however, cautions a straightforward interpretation of a security-driven radical right premium. ANEL, which was the emerging radical right party at the time, experienced a significant boost, gaining about 0.6% of its baseline support per incident ($p < 0.001$), yet its manifestos contain no security emphasis in any election it ever contested (Online Appendix A.2). In contrast, LAOS was the one radical right party with a genuine security profile, but it had been a partner in the coalition

government before the elections and was seen as complicit in the austerity measures. Its association with political violence is negative but not statistically significant (-0.5% of its baseline per incident, $p = 0.20$), although FR violence alone significantly reduces its vote share (Online Appendix H). The opposing directions of these two parties attenuate the family-level estimate.

A second reason for caution is where the violence occurred. Section 6.2 raised the possibility that violence is located where particular parties did unusually well or poorly in May. Regressing each center’s exposure on the May 2012 vote shares (Online Appendix C.7) shows that placement does track the May result, though not for the extremist parties. Pooled exposure rises with the local ND share (11.1 incidents within 750 m per unit of vote share, about 0.11 per percentage point, $p = 0.048$) and falls with the ANEL share (-31.5 , $p = 0.044$). The associations for PASOK ($p = 0.052$) and KKE ($p = 0.060$) fall short of the 5% error level, and neither extremist party’s May share predicts placement (Golden Dawn $p = 0.32$, ANTARSYA $p = 0.11$). Far-left incidents account for most of the window’s exposure, so their placement reproduces the pooled coefficients almost exactly. Far-right incidents are no more frequent in Golden Dawn areas ($p = 0.24$), and their association with the LAOS share falls just short of the 5% error level ($p = 0.051$). Violence thus occurred where ANEL had been weaker in May, and those are the centers where ANEL gained the most in June. Once we control for the May result, the radical right family-level estimate changes sign and loses significance (p moves from 0.003 to 0.57), while the extremist penalty strengthens (from $p = 0.025$ to $p = 0.004$).

Taken together, the radical right gain is driven by a party with no security profile, is concentrated in the municipality of Athens (Online Appendix I), and does not survive the mean reversion adjustment or the nearest-window control (Online Appendix D.3). The evidence therefore suggests that the radical right gains documented elsewhere do not replicate robustly in mid-2012 Greece, where no radical right party combined credibility on security with distance from both the violence and the discredited incumbent coalition (Online Appendix A.2).

Moving to H4, we observe a statistically significant negative effect of political violence on the vote share of extremist parties. Each incident reduces the extremist family’s vote share by roughly 0.6% of its baseline support ($p = 0.025$). At the 90th percentile of exposure, this implies a roughly 1.8% decline relative to baseline. The penalty holds under every inference method in Table 2, including the municipality bootstrap ($p = 0.025$) and randomization inference ($p = 0.049$), and both extremist parties show clean placebo profiles across all pre-treatment windows. These findings suggest that political violence is associated with diminished voter support for extremist parties and align with our theoretical expectation

that heightened sensitivity to law and order leads voters away from parties perceived as endorsing or perpetuating violence.

Which of the two parties accounts for the penalty is less certain, because it depends on the inference method. Under Conley inference it falls on the FL extremist ANTARSYA, which loses about 1.4% of its baseline support per incident ($p = 0.013$), while the estimate for the FR extremist Golden Dawn (HA) is negative but not significant (-0.4% of baseline per incident, $p = 0.20$). ANTARSYA’s estimate is also significant under the leverage-adjusted estimator ($p = 0.026$), but not under the three other methods in Table 2 or under the mean reversion adjustment (Online Appendices B.1 and C.7). Under the municipality bootstrap the opposite holds. Golden Dawn is significant ($p = 0.049$) and ANTARSYA is not ($p = 0.63$). The family is nevertheless estimated more precisely than either party. Within municipalities, the two parties’ deviations from the model tend to offset each other, so their combined vote share is less noisy than either party’s alone (Online Appendix B.1).

In sum, the results differ in strength across our hypotheses. We find no evidence that violence affects turnout (H1), and the gain for the incumbent parties is not statistically significant (H2). Table 2 sets out how each result fares under the four inference methods, for the four family outcomes and the three parties whose verdicts depend on the method. The extremist penalty (H4) is the only result that is significant in all four columns. The radical right gain (H3b) is significant in three, and the estimates for ND (H3a) and ANTARSYA under Conley inference alone. Beyond our hypotheses, the party-specific results also show heterogeneity on the left, where PASOK and SYRIZA lose support while KKE and DIMAR gain. Online Appendix B.6 discusses these patterns, which we treat as exploratory.

7.1 Disentangling far-left and far-right violence

Our theoretical framework raised the question of whether FL and FR violence produce symmetric or asymmetric electoral effects. To explore this, we estimate a single model that includes both far-left and far-right exposure, with covariates and municipality fixed effects. Figure 5 focuses on the two extremist parties, ANTARSYA and Golden Dawn, where the pattern is most striking. Full results for all parties are reported in Online Appendix H.

The results point to an ideologically patterned asymmetry. Estimating each extremist party’s vote change on far-left and far-right exposure jointly, ANTARSYA’s estimate reaches significance for far-left violence but not for far-right ($p = 0.007$ versus $p = 0.23$), and Golden Dawn’s is the reverse ($p = 0.028$ versus $p = 0.55$). The two poles also differ sharply in the size of their per-incident effects. FL incidents are frequent but individually small in effect, and each reduces ANTARSYA’s vote share by approximately 1.7% of its baseline support.

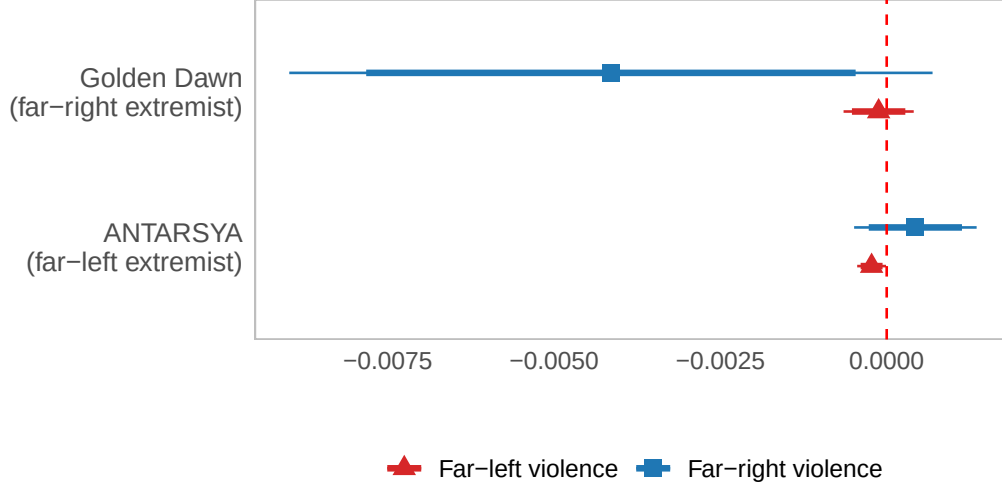


Figure 5: Effects of far-left and far-right violence on extremist party vote share. Each party shows its far-left and far-right exposure coefficients from a single joint model that includes both poles. Thick lines represent 95% confidence intervals; thin lines represent 99% confidence intervals, from Conley spatial-HAC (1 km) standard errors. FR violence coefficients are larger in magnitude but less precisely estimated due to fewer incidents (24 FR vs 120 FL). All models include covariates and municipality fixed effects. The Wald test of equality of the two effects ($\beta_{FL} = \beta_{FR}$), with the FL–FR difference and its confidence interval, is reported in the text and in Online Appendix H, Table H1.

FR violence, though rarer, has a much larger per-incident penalty. A single FR incident reduces Golden Dawn’s vote share by approximately 5.7% of its baseline support.

The p -values above compare each coefficient with zero, not with each other. We therefore test whether each party’s far-left and far-right coefficients are equal ($\beta_{FL} = \beta_{FR}$). At the primary 1 km cutoff, the Wald test rejects equality for Golden Dawn, whose far-right penalty exceeds its far-left penalty by 0.0040 (FL–FR = 0.0040, 95% CI [0.0002, 0.0078], $W(1) = 4.30$, $p = 0.038$), but not for ANTARSYA (FL–FR = -0.0007 , 95% CI [-0.0014 , 0.0001], $W(1) = 2.87$, $p = 0.090$). Both verdicts depend on the bandwidth. Golden Dawn’s rejection holds at 0.5 and 1 km but not at 2 km ($p = 0.062$), and its p -value is 0.051 under the leverage-adjusted estimator, while ANTARSYA rejects only at 2 km ($p = 0.027$). With only 43 voting centers exposed to far-right violence, the contrast is underpowered (Online Appendix H, Table H1).

The pattern is also not stable across specifications. When the joint model is re-estimated under the mean reversion and nearest-window adjustments (Online Appendix H, Table H2), far-right violence continues to penalize Golden Dawn under both ($p = 0.024$ and 0.021), whereas the far-left penalty on ANTARSYA survives the nearest-window control ($p = 0.005$) but disappears once ANTARSYA’s May share is controlled for ($\beta = 0.00003$, $p = 0.38$). The family-level penalty on far-left exposure, by contrast, strengthens under both adjustments.

Taken together, these results suggest that each extremist party is penalized by violence from its own side, but the evidence is stronger for Golden Dawn than for ANTARSYA, and we treat the pattern as descriptive rather than as a finding in its own right.

8 Conclusion

This study analyzes the impact of FL and FR political violence on turnout and vote choice in Greece, leveraging the narrow 42-day window between the May and June 2012 snap parliamentary elections. Our difference-in-differences design at the voting center level, combined with a comprehensive police dataset of 148 incidents, allows us to estimate the effects of local exposure to political violence on electoral outcomes across the full party spectrum.

Our analysis yields two core findings of unequal strength. First, political violence is associated with diminished support for extremist parties. This penalty is stable for the two parties combined, and it holds under every inference method we apply. Which of the two parties accounts for it is less certain, since ANTARSYA does under Conley inference and Golden Dawn under the municipality bootstrap and the mean reversion adjustment. We therefore report the finding for the family as a whole. Second, exposure to political violence is associated with increased support for radical right parties, driven primarily by the emerging ANEL. This radical right association is the less robust of the two. It is identified almost entirely off the municipality of Athens and does not survive adjustment for mean reversion or for violence in the nearest pre-treatment window (Online Appendices C.7, D.3 and I), so we treat it as suggestive rather than established. We interpret this as evidence that the radical right gains documented elsewhere do not replicate robustly where the radical right lacks a credible security profile (Online Appendix A.2). The conservative ND shows a positive coefficient that is significant under Conley inference alone, and the incumbent family as a whole is statistically indistinguishable from zero. We therefore find no support for consolidation around the governing parties. Further, we find no evidence that political violence affects voter turnout.

These results are buttressed by an extensive robustness battery, reported in full in the Online Appendix. The extremist family outcomes have clean placebos in every pre-treatment window, and their estimates *strengthen* under both of the adjustments that weaken the radical right, specifically the nearest-window control (Online Appendix D.3) and the mean reversion correction (Online Appendix C.7). Sensitivity analysis following [Cinelli and Hazlett \(2020\)](#) (Online Appendix F), matched-sample estimation, alternative distance thresholds, a falsification test on invalid and blank ballots, and dose-response specifications (Online Appendix G) all yield substantively similar results, though the extremist estimate attenuates

at the shortest (250 m) radius and blank ballots show a small borderline association that does not survive multiple-testing correction (Online Appendix D.4). Finally, leave-one-out exclusion shows that no single municipality outside Athens drives the findings. After excluding the municipality of Athens itself (103 voting centers; our largest identifying cluster), the Extremist coefficient retains its sign and magnitude but loses statistical significance, while the Radical Right coefficient attenuates to approximately zero.

Our results are reduced-form associations between local exposure to violence and shifts in vote share. Given that our aggregate design cannot observe the individual-level attitudes that generate them, the mechanisms below are competing interpretations we weigh against the pattern of results rather than channels we identify directly. First, a *democratic backlash* reading holds that voters withdraw support from parties perceived as connected to violence or instability. This is the more robust of the two findings. The vote share losses experienced by extremist parties are statistically significant at the party family level, rest on clean pre-treatment placebos, and survive our full robustness battery. Its implications are also straightforward. Support declines for parties on both ideological poles that are associated with violence, which is consistent with a normative commitment to peaceful democratic competition and with the blame attribution logic documented in other mature democracies (Krakowski and Morales, 2025). Second, a *fear-driven consolidation* reading holds that heightened threat perception leads voters to seek security and order. We are less confident in this interpretation. Empirically, it rests on the less robust of our two findings. Theoretically, the identity of the beneficiary is inconsistent with the accounts that motivated the consolidation expectation. System justification and rally effects predict gains for the established order, yet the incumbent coefficient is insignificant and the principal winner is an anti-establishment challenger whose manifestos never emphasized security at all (Online Appendix A.2). This pattern is more consistent with affective intelligence, in which anxious voters defect toward challengers who channel their insecurity, than with a reward for security issue ownership.

This paper is not without limitations. First, our design identifies effects of local exposure to violence but cannot fully rule out spatial spillovers. Incidents near one voting center may influence behavior at neighboring centers through social networks or local media, which would attenuate our estimates by contaminating the control group. Our donut-test, horse-race, and grid-cell fixed effects analyses (Online Appendix E) seek to address this concern. Effects are concentrated within 750m and largely absent in the surrounding ring, and the extremist backlash survives even when comparing voting centers within the same $1\text{km} \times 1\text{km}$ grid cell ($p < 0.001$). Second, our design cannot fully disentangle reactions to political violence from responses to ordinary crime; adding granular crime data or individual-level

surveys would sharpen the mechanism test. Third, the narrow time window yields relatively few FR incidents, which limits statistical power for the disaggregated FL versus FR analysis. Fourth, the placebo tests show that the location of violence tracks differential local trends for a substantial part of the party system (the radical right in the window nearest the election; PASOK, KKE, and SYRIZA more broadly; Online Appendix D). Our claims therefore rest on the extremist family, which is the one set of outcomes whose placebos are clean in every window. Fifth, our aggregate design speaks to electoral outcomes but not to the party-level behavior that may mediate them. Parties differ markedly in their relationship to violence, most sharply Golden Dawn (Ellinas, 2020), and how each party framed or condemned specific incidents could help adjudicate the blame attribution channel we invoke for the extremist penalty. Systematically coding party communications for the 42-day window is a data collection effort we leave to future work.

Despite these limitations, our findings contribute to understanding democratic resilience in times of crisis. The Greek case, while drawn from a single national context during an acute economic and political emergency, shares structural features with other European democracies that have confronted rising political violence (Ravndal et al., 2023; Eger and Olzak, 2023; Krause and Matsunaga, 2023). The extremist backlash we document, where voters reject parties on both ideological poles that are associated with violence, suggests that democratic norms can prove resilient even under conditions of sustained political instability. At the same time, to the extent that it holds, the consolidation of support around ANEL rather than established parties introduces an important qualification. The electoral reallocation triggered by violence may weaken extremist parties while empowering radical challengers who channel insecurity without endorsing violence themselves. Whether such reallocation strengthens or strains democratic institutions in the longer run is an open question that deserves further investigation across different institutional and cultural contexts.

CRediT authorship contribution statement

Lamprini Rori: Conceptualization, Investigation, Writing (review & editing). **Costas Roumanias:** Conceptualization, Methodology, Formal analysis, Data curation, Writing (review & editing). **Constantine Boussalis:** Conceptualization, Investigation, Methodology, Formal analysis, Data curation, Writing (original draft), Writing (review & editing).

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Data availability

The replication code and data for this study will be made available through the Harvard Dataverse upon acceptance of the article.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the authors used Anthropic Claude Code for assistance with code generation. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Online Appendix for: “Democracy in the Crossfire: The Electoral Fallout of Extremist Political Violence”

Abstract

This appendix documents the data on which the main text rests and reports in full the tables and supplementary analyses that the text summarizes. By way of organization, Appendix A describes the electoral, violence, and socioeconomic data and validates our party classifications against expert surveys and coded manifestos. Appendix B presents the complete regression tables behind the figures in the text, together with the details of our inference under spatial dependence and the multiple-testing corrections. Appendices C through E collect the robustness, placebo, and spatial analyses. These cover alternative exposure radii and matched samples, mean reversion and changes in policing, temporal and spatial placebos, the nearest pre-treatment window as a control, a falsification test on invalid and blank ballots, grid-cell fixed effects, and the distance-decay gradient. The remaining sections approach our findings from four further angles: (1) sensitivity analysis for omitted variable bias (Appendix F), (2) the dose-response form of the treatment (Appendix G), (3) a joint model that separates far-left from far-right violence (Appendix H), and (4) a leave-one-out analysis over municipalities, with Athens the case that matters (Appendix I). Throughout, we report Conley spatial-HAC (1 km) standard errors as our primary basis for inference.

Keywords: political violence, far right, far left, elections, democratic resilience, Greece, difference-in-differences.

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A Detailed data sources and variable description

A.1 Data sources

A.1.1 Electoral data

Our electoral data were gathered from the Greek Ministry of the Interior’s official elections outcomes site.¹ Addresses of electoral precincts were retrieved from the Greek Ministry of the Interior electoral registry web site.² Exact geolocation was obtained through the Google Maps API. Precinct votes were aggregated at the school level as described in the main paper.

Table A1: Political violence incidents by kind, means and organization.

Kind	Frequency	Means of attack	Frequency
arson	53	gas/flammable material	37
attack against persons	50	physical violence	35
damage to property	20	unknown	19
unknown	9	molotov cocktail	12
explosion	5	guns	9
insult	3	stones	9
shooting	2	bats	8
stabbing	2	gas	5
threatening letter	2	verbal attack	5
insulting symbols	1	fireworks/flares	3
threatening phone call	1	knife	2
		bottles	1
		dynamite	1
		package-envelope	1
		throwing yogurt	1
Total	148		148
	(a)		(b)

Organizations	Frequency
GOLDEN DAWN	9
CHILDREN PLAY	4
AGGRESSIVE ANTI-STATE FIGHT	1
ANTIFASCIST PATROL OF METAMORPHOSIS AREA	1
BRINGERS OF FIRE	1
CONSPIRACY OF THE CELLS OF FIRE	1
E.O.	1
REVOLUTIONARY ORGANIZATION 18TH NOVEMBER	1
SOCIAL BALANCERS	1
SOME OF THE 34,9%	1
ZERO TOLERANCE	1
Total	22
	(c)

A.1.2 Data on incidents of political violence

Incidents of political violence were recorded and classified by the Greek Police. Each incident was accompanied by a detailed description of the location, actors, victims and circumstances surrounding the incident, means of attack, arrests, and number of injured parties. The

¹Available at <https://www.ypes.gr/ekloges/ethnikes-ekloges/lipsi-archeion-apotelesmaton-ethnikon-eklogon>.

²Available at <https://www.ypes.gr/ekloges/eklogikoi-katalogoi-kai-eklogiko-soma/eklogika-tmimata>.

incidents were checked by the authors to ensure that they fall within our definition of Political Violence. Addresses were retrieved by code and manually in cases where exact address was missing and the incident had to be cross-checked against other internet sources in order to extract a location. We classified incidents by type of actor (FL-FR-hooligans), target (human, material, both), and escalation (low or high). A summary of our classification is given in the main text. Summary statistics of qualitative information about the attacks reported by the police are presented in Table A1.

Summary statistics of target of incidents of political violence by target type (subcategories of targets as reported by the Greek Police) is given in Table A2.

Table A2: Political violence incidents by target type.

Material target	Frequency	Human target	Frequency
Vehicles	28	Police/Security	16
Professional Spaces	18	Citizens	11
HQ/Residencies of Politicians	11	Immigrants/Refugees	9
Electoral Precincts	9	Political Party Members	4
Houses/Buildings	6	Media	2
Immigrant Housing	4	Organized Sports Fans	2
State Buildings	4	Politicians/Public Figures	2
Banks	3	FR-FL Activists	1
Possession of Guns	3	Students (Higher Ed)	1
Garbage Containers	2		
Monuments/State Symbols	2		
Embassies/Consulates	1		
Forest	1		
Infrastructure	1		
Lawyers/Judges	1		
Media	1		
Public Transport	1		
Religious Spaces	1		
Schools	1		
Squats/Movement HQ	1		
Universities	1		
Total	100		48
	(a)		(b)

A.1.3 Socioeconomic controls

The unique quasi-experimental design of our data ensures that socioeconomic factors affecting voting behavior and political violence remain constant within the window of the twin elections. Furthermore, we relax the parallel trends assumption as described in our Methodology section of the main paper, by allowing different (fixed effects) shifts for different municipalities. One additional way to control for factors that have affected local areas differently, is to include socioeconomic controls at the finest-grid data availability allows for.

We include the 2011 mean family income level by postal code, as reported by the Greek Independent Authority for Public Revenue (AADE). Furthermore, we include the following demographic characteristics, at the local community (Τοπική κοινότητα) level, as reported

in the 2011 Census Panorama Statistics:³ the percentage of the population holding a PhD degree, the percentage holding a Master’s degree, the percentage holding a Bachelor’s degree, the percentage with citizenship other than Greek, the unemployment rate of the working-age population, and the youth unemployment rate.

A.1.4 Summary statistics

Table A3 reports summary statistics for the 603 voting centers in our sample at the 750m radius.⁴ The top panel describes the treatment variable. The middle panel reports May 2012 vote shares for each party, which serve as the pre-treatment baseline. The bottom panel summarises the socioeconomic covariates used in all specifications.

Table A3: Summary statistics (750 m sample).

	Mean	SD	Min	Max
<i>Treatment</i>				
Dosage (all violence)	0.7778	1.8917	0.0000	20.0000
Treated (dosage > 0)	0.3698	–	–	–
<i>May 2012 vote shares</i>				
Turnout	0.6871	0.0956	0.0053	0.8537
PASOK	0.0899	0.0131	0.0482	0.1380
ND	0.1311	0.0382	0.0565	0.3116
KKE	0.0942	0.0305	0.0119	0.2222
SYRIZA	0.2051	0.0360	0.0477	0.3006
Golden Dawn (HA)	0.0730	0.0225	0.0296	0.2129
ANTARSYA	0.0136	0.0044	0.0000	0.0299
DIMAR	0.0637	0.0112	0.0000	0.1043
ANEL	0.1060	0.0173	0.0370	0.1572
LAOS	0.0298	0.0063	0.0000	0.0534
<i>Socioeconomic covariates</i>				
Family income	2.3555	0.8168	1.4137	12.2879
PhD %	0.0069	0.0057	0.0009	0.0604
Master’s %	0.0265	0.0189	0.0043	0.1645
University %	0.1412	0.0516	0.0385	0.3209
Foreign citizenship %	0.1155	0.0737	0.0237	0.2620
Unemployment %	0.0648	0.0137	0.0162	0.1007
Youth unemployment %	0.0243	0.0055	0.0059	0.0411
Observations	603			

³Available at <https://panorama.statistics.gr/en/>.

⁴The electoral register lists one further center, a school in the municipality of Poros, whose address the geocoder resolved to a street in Korydallos some 56 km away, which is how a station on an Aegean island came to sit inside an Athens sample. We exclude it, since its distance to any incident describes the Korydallos address rather than its own voters. The second station at the same school geocoded correctly and falls outside our study area, as do all other island municipalities.

A.1.5 Political violence trends in Greece

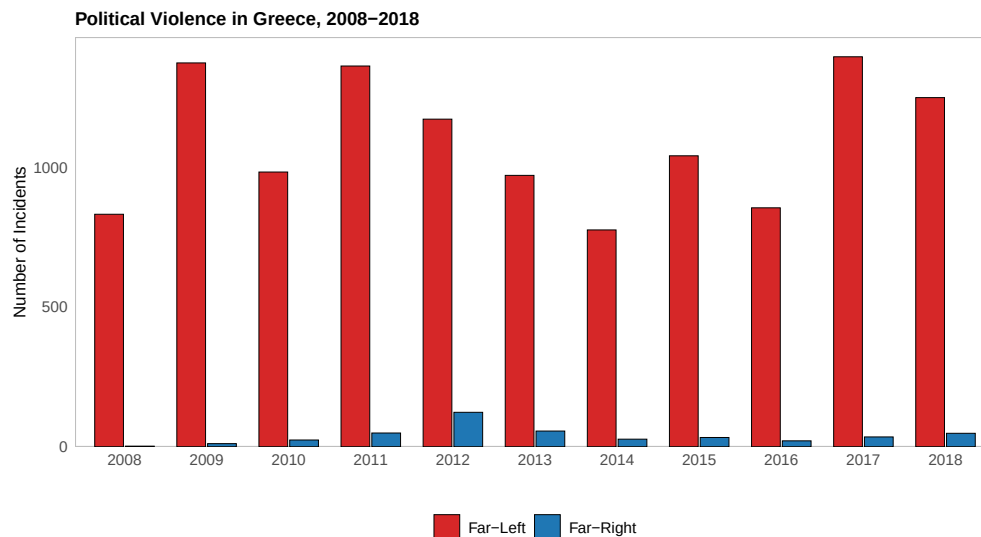


Figure A1: Aggregate yearly data on political violence in Greece (2008–2018). Data provided by the Greek police.

Figure A1 shows the trends in political violence in Greece from 2008 to 2018, the period of the financial crisis. The data highlight a consistent pattern of unrest throughout the crisis years, with the number of incidents rising and falling over time.

The values for 2012, the year of our study, are not outliers in this series. They align closely with the overall trends observed during the crisis, sitting neither at a peak nor a trough. This consistency reassures us that the findings from our study are not driven by any idiosyncratic effect unique to 2012 but are instead reflective of broader patterns of political violence during the crisis.

Interestingly, political violence remained high toward the end of the crisis, with incidents in 2017 exceeding those recorded in any earlier year of the crisis. Moreover, as shown in Figure 1 of the main text, political violence persisted beyond the official end of the crisis in 2018, with Greece continuing to experience some of the highest rates of political violence in the EU. This sustained unrest highlights how political violence in Greece during the period of study was not simply a reaction to short-term economic hardship but rather a symptom of deeper and more enduring societal tensions.

A.1.6 Indicative goals of violent non-state actors

Two of the most recognizable perpetrators during the period under study are the far-right extremist party and militia Golden Dawn (HA) and the anarcho-revolutionary terrorist organization Conspiracy of the Cells of Fire (CCF). According to the 1980s statutes of Golden Dawn, the organization aimed to "reveal the real dimension of national-socialism," propagate the ideals of racial purity and white cultural supremacy, and oppose democracy, human equality, Christianity, Marxism, capitalism, and Judaism. Its ultimate objective was to

"demolish the rotten status quo" and establish a racially exclusive nation-state for Greeks (Georgiadou, 2013; Georgiadou, 2020). The Conspiracy of the Cells of Fire (CCF) advocates for revolutionary nihilism and anarchist individualism, aiming to "become part of a generalized attack against the present state" and to advance the revolutionary cause internationally through "a new, rabid armed struggle" (Kassimeris, 2013).

A.2 Party classification and ideological groupings

Below we extend our justification, with systematic evidence, of our classification of parties into mainstream, radical, and extremist families, and in particular the radical-versus-extremist distinction on which several of our findings rest. We first restate the conceptual basis of the groupings; we then validate them against three independent bodies of systematic data (expert surveys and coded party manifestos); and we close with a qualitative reading of the primary manifesto text for the one party that no cross-national dataset covers, namely the far-left ANTARSYA.

A.2.1 Conceptual basis

Our groupings follow established scholarship on European party systems (Mudde, 2007; March, 2012; Backes, 2023; Escalona, Keith, and March, 2023). The organizing axis is a party's relationship to democratic institutions, not the mere intensity of its rhetoric. *Radical* parties are illiberal but system-loyal. They contest the status quo and advocate far-reaching policy change while accepting democratic procedures and competing for office within them (in the Greek case, the populist radical right LAOS and ANEL, and the radical left KKE and SYRIZA). *Extremist* parties, by contrast, reject the democratic order itself and are willing to endorse or employ extra-legal or revolutionary means to overturn it (the far-right Golden Dawn and the far-left ANTARSYA). This is precisely the dimension (commitment to democratic pluralism versus its rejection) along which we expect voters to distinguish parties when political violence heightens concern for order and stability. We stress here that the distinction is analytical rather than rhetorical. Some parties we classify as radical are, in everyday commentary, loosely called "extreme," but the two families differ in the theoretically relevant respect of whether they operate inside or against the democratic system.

A.2.2 Validation against expert surveys and coded manifestos

Table A4 assembles four widely used cross-national sources: the PopuList 4.0 family classifications (Rooduijn et al., 2024); the Varieties of Party (V-Party) V2 expert ratings (Lindberg et al., 2022); the 2014 Chapel Hill Expert Survey (Polk et al., 2017); and the 2012 manifesto codings of the Manifesto Project (MARPOR) (Lehmann et al., 2025). They converge on the groupings, but they differ in how sharply they separate radical from extremist parties.

Based on the table above, we land on three main points. First, PopuList validates *family membership* but cannot by construction separate radical from extremist parties. It codes Golden Dawn, ANEL, and LAOS identically as far-right and populist, and SYRIZA and KKE as far-left, using binary flags with no democracy dimension. Second, the discrimination we need comes from V-Party's *commitment to political pluralism*, an expert rating of exactly the "accepts versus rejects democratic institutions" axis that defines our distinction. On

Table A4: Validation of party classifications against systematic cross-national data. V-Party *commitment to pluralism* (v2paplur) is centered at zero; *lower* values indicate stronger rejection of democratic institutions. *Opponent respect* (v2paopresp): lower = more demonizing. CHES GALTAN and anti-elitism run 0–10; higher = more authoritarian/anti-elite. MARPOR *rile* is the left–right scale (–100 to +100), from the May 2012 manifestos. ANTARSYA is absent from all four sources (it never cleared an inclusion threshold) and is validated qualitatively in §A.2.4.

Party	Family	V:plur	V:opp	GALTAN	a-elite	rile
Golden Dawn (HA)	Far-right extremist	–2.82	–3.79	10.0	10.0	38.2
ANTARSYA	Far-left extremist	<i>absent from all sources; see §A.2.4</i>				
ANEL	Populist radical right	–0.36	–1.88	8.6	9.2	–12.3
LAOS	Populist radical right	–0.52	–2.05	8.3	9.0	7.0
SYRIZA	Radical left	0.46	–1.79	2.1	8.6	–35.6
KKE	Radical left	0.25	–0.86	5.8	9.8	23.5
ND	Centre-right (incumbent)	2.05	–0.88	7.0	2.3	22.7
PASOK	Centre-left (incumbent)	2.02	–0.93	4.0	2.8	–13.5
DIMAR	Centre-left	2.05	1.74	2.6	4.3	–6.7

this measure Golden Dawn is a stark outlier (–2.82), separated by roughly two full points from the radical right ANEL and LAOS (–0.36 and –0.52), which in turn sit well below the mainstream ($\approx +2.0$); on the left, SYRIZA and KKE score *positive* (+0.46, +0.25), consistent with parties that operate within the system, whereas the extremist ANTARSYA, by its own manifestos, does not (§A.2.4). The opponent-respect ratings tell the same story, with Golden Dawn (–3.79) by far the most demonizing actor. Third, and this is the necessary caveat, the *aggregate* authoritarianism indices do *not* discriminate. CHES GALTAN and anti-elitism compress all three far-right parties near the ceiling (GALTAN 8.3–10.0; anti-elitism 9.0–10.0), and V-Party’s summary anti-pluralism index similarly saturates (Golden Dawn 1.00 versus ANEL/LAOS 0.93). These broad scales register that a party is illiberal but not *how* it relates to democracy. The MARPOR *rile* score is uninformative here for a different reason. Because it aggregates issue-emphasis categories that mix the economic and cultural dimensions, the statist KKE scores to the right, as its heavy emphasis on political authority is a right-coded item, while the anti-austerity ANEL scores to the left, as its economic positions are left-coded, neither of which reflects the party’s place in the Greek system. The distinction therefore rests on the democracy-specific components (commitment to pluralism and opponent respect) and not on the composite indices, and we present it on that basis.

A.2.3 Manifesto-based issue emphasis: is “security” owned by the Greek radical right?

The radical right gains documented in other settings presuppose that radical right parties credibly own security and order. We examine whether this held in Greece using the MARPOR manifesto data, combining the law and order (per605) and military (per104) categories into a single security emphasis measure. Because manifesto shares are compositional, a crisis election can mechanically crowd out security emphasis for every party; we therefore report each party’s full coded history rather than the 2012 manifestos alone. Table A5 summarizes.

Three patterns stand out. First, the security-heaviest party in the system, in both

Table A5: Manifesto emphasis on security-related issues across all coded Greek elections (MARPOR quasi-sentence shares, %). Security emphasis is the sum of law and order (`per605`) and military (`per104`); nationalism is national way of life (`per601`). Dashes: the party did not yet exist or has no coded manifesto in the period. MARPOR’s June 2012 entries for ND, ANEL, Golden Dawn, and DIMAR re-code the May documents, so a single 2012 column is reported. LAOS’s pre-2012 value is its single coded manifesto (2007); the PASOK series ends in 2015; pre-2012 SYRIZA includes its predecessor Synaspismos. ANTARSYA is not coded by MARPOR; its 2012 manifestos contain no law-and-order or pro-military appeals and instead frame the state’s security apparatus as an adversary (§A.2.4).

Party	Family	Security emphasis			Nationalism
		1974–2009	2012	2015–2023	2012
Golden Dawn (HA)	Far-right extremist	–	16.13	13.73	20.43
LAOS	Populist radical right	7.40	5.46	–	12.17
ANEL	Populist radical right	–	0.00	0.00	16.77
ND	Centre-right (incumbent)	4.12	0.00	4.17	4.54
PASOK	Centre-left (incumbent)	1.85	0.73	0.26	1.96
DIMAR	Centre-left	–	5.25	–	0.13
SYRIZA	Radical left	0.05	0.18	1.23	0.00
KKE	Radical left	0.46	0.00	0.00	3.51

periods reported here, is the far-right extremist Golden Dawn, at two to four times the emphasis of any other party. Second, the only radical right party with a genuine security profile is LAOS, while ANEL, the party our estimates identify as the principal radical right beneficiary of violence, records zero security emphasis in every manifesto of its existence; its profile is nationalist (16.8% in 2012, rising to 46.2% by 2015) and anti-establishment, not security-oriented. Third, ND’s long-run security emphasis (4.1% on average before 2012, returning to comparable levels after 2015) collapses to zero in the crisis-era manifestos, whose agendas were consumed first by the economy and then by the bailout question. We note that manifesto emphasis captures programmatic documents rather than campaign rhetoric, and ND’s 2012 campaign placed heavy weight on public order.

These patterns speak directly to Hypothesis 3b. In Greece in 2012 no radical right party combined security credibility with clean hands. The genuine security owner, Golden Dawn, was itself identified with the violence and suffered the extremist penalty; LAOS’s security profile was offset by its complicity in the incumbent coalition; and ANEL, the sole radical right gainer, had no security profile at all. The failure of the documented radical right regularity to replicate robustly in our setting (main text, Section 7) is thus interpretable as a scope condition rather than an anomaly. Violence rewards the radical right where it owns order, and in Greece in 2012 it did not. The same evidence cautions against a security-ownership reading of ANEL’s fragile gain, which our robustness analyses in any case attribute largely to Athens-specific dynamics and mean reversion.

A.2.4 Qualitative validation of the ANTARSYA classification

ANTARSYA is absent from CHES, PopuList, V-Party, and MARPOR alike, because it never approached the electoral thresholds those projects use for inclusion. We therefore validate its *extremist* classification directly from the primary source those projects would themselves code, namely the party’s own 2012 electoral manifestos (the May 6 Electoral Declaration,

Εκλογική Διακήρυξη, and the June 17 Programme, Πρόγραμμα), retrieved from antarsya.gr.⁵ Read against the operational criterion for left extremism (rejection of institutionalized democratic procedures in favor of a revolutionary path; Backes 2023; March 2012), the texts place ANTARSYA unambiguously on the extremist side, in its own words:

- Most directly, the manifestos reject parliamentary procedure as the vehicle for change, which is the defining criterion of left extremism. The party writes that it “cannot walk this road relying merely on parliamentary procedures” (‘Δεν μπορούμε να βαδίσουμε σε αυτόν τον δρόμο με στήριξη απλώς και μόνο σε κοινοβουλευτικές διαδικασίες’).
- They characterize the existing democratic order not as a system to be reformed but as one to be broken. It is described as “the current system of a peculiar parliamentary dictatorship of the markets” (‘το σημερινό σύστημα μιας ιδιότυπης κοινοβουλευτικής δικτατορίας των αγορών’) and, in the May declaration, as a “bourgeois parliamentary ‘dictatorship’” (‘της αστικής κοινοβουλευτικής «δικτατορίας»’).
- In place of elections, they call for insurrection and revolutionary overthrow, holding that change will come “only through the force of insurrection and the threat of radical overthrow” (‘Μόνο με την δύναμη της εξέγερσης και την απειλή της ριζικής ανατροπής’). The June program is titled “Overthrow! Not renegotiation and submission” (‘ΑΝΑΤΡΟΠΗ! ΟΧΙ ΑΝΑΔΙΑΠΡΑΓΜΑΤΕΥΣΗ ΚΑΙ ΥΠΟΤΑΓΗ’).
- They envisage an extra-legal seizure of power and armed self-defense against the state, arguing that parties of the left “must impose their own government and their own power ... with ... self-defense against the ‘deep state,’ the mechanisms of repression” (‘πρέπει να επιβάλουν τη δική τους κυβέρνηση και τη δική τους εξουσία ... με ... αυτοάμυνα απέναντι στο «βαθύ κράτος», των μηχανισμών καταστολής’).
- The party describes itself in the same terms. The May declaration closes by calling for a “united-front, anticapitalist and subversive Left” (‘ΜΕΤΩΠΙΚΗ ΑΝΤΙΚΑΠΙΤΑΛΙΣΤΙΚΗ ΚΑΙ ΑΝΑΤΡΕΠΤΙΚΗ ΑΡΙΣΤΕΡΑ’), and the party’s name carries the same message twice over. ANTARSYA is an acronym of Anticapitalist Left Cooperation for the Overthrow (ΑΝΤικαπιταλιστική ΑΡΙστερή ΣΥνεργασία για την Ανατροπή), and it is pronounced identically to *ανταρσία*, the Greek word for “insurrection,” so that the name the party chose for itself is also a call to revolt.

Throughout, the language of overthrow (ανατροπή) fuses with that of revolution (επανάσταση) and rupture (ρήξη), and is set explicitly *against* SYRIZA-style renegotiation (επαναδιαπραγμάτευση), that is, revolutionary transformation of the system rather than a change of government within it. This is the textual counterpart, on the far left, of the anti-democratic profile that the expert data isolate for Golden Dawn on the far right, and it confirms that ANTARSYA belongs with the extremist rather than the radical family. The same reading also locates ANTARSYA

⁵Both manifestos are preserved in the Internet Archive, and our quotations are taken from those captures. The May 6 Electoral Declaration (Εκλογική Διακήρυξη), originally at antarsya.gr/media/Antarsya_Ekloges12_diakirixi.pdf, is archived at https://web.archive.org/web/20120707093536/http://antarsya.gr/media/Antarsya_Ekloges12_diakirixi.pdf (captured 7 July 2012). The June 17 Programme (Πρόγραμμα), originally at antarsya.gr/media/2012_06_12_antarsya_programma.pdf, is archived at https://web.archive.org/web/20120619210756/http://antarsya.gr/media/2012_06_12_antarsya_programma.pdf (captured 19 June 2012, two days after the June 17 election).

at the opposite pole from security-issue ownership. Its manifestos contain no law-and-order or pro-authority appeals and instead cast the state’s repressive apparatus as the adversary, consistent with its appearance in Table [A5](#) as a party with no security emphasis to code.

B Inference, multiple testing and full regression tables

This section reports the complete regression tables underlying the figures in the main text. We first set out our approach to inference under spatial dependence, and then present the estimates for turnout and for the incumbent, radical right, extremist, and individual party outcomes.

B.1 Inference under spatial dependence (Conley standard errors)

This subsection describes how we assess the uncertainty of our estimates. Because treatment is a count of incidents within overlapping 750 m buffers and the differenced outcomes are spatially autocorrelated, voting centers are not independent observations, and our inference has to account for this. Throughout the regression tables in this appendix we therefore report Conley spatial heteroskedasticity- and autocorrelation-consistent (“spatial-HAC”) standard errors (Conley, 1999) as our primary inference, using a 1 km distance cutoff, for every coefficient. We use the Bartlett kernel which weights a pair of centers d km apart by $1 - d/c$ inside the cutoff c . Its kernel matrix on the 603 locations is positive semi-definite at 0.5 and 1 km, and so therefore is the covariance estimator. At 2 km the kernel matrix is not (38 negative eigenvalues), and we verified directly that the covariance matrix is positive semi-definite for every outcome at that cutoff as well. One kilometre is the approximate range over which the estimated effect of violence decays, since violence in the 750–1000 m ring adds nothing once violence within 750 m is accounted for (Appendix E), and it is bracketed by the 0.5 and 2 km cutoffs we also report. The wider cutoff also has a mechanical rationale. Two 750 m buffers overlap whenever their centers lie within 1.5 km of each other, so the 2 km cutoff covers every pair of centers whose exposure counts share an incident, and its verdicts match those at 1 km. Table B6 shows that the main specification conclusions are insensitive to this choice. The radical right and extremist *family* effects retain their significance across HC1 and all three Conley cutoffs, while the turnout and incumbent nulls are unaffected.

A correction for spatial dependence usually widens confidence intervals. However, in our data, we found that the correction tended to narrow the confidence intervals. Below we explain why we believe this might be the case. For most outcomes, the Conley standard error is smaller than its HC1 counterpart. For ND and ANTARSYA, this difference changes whether the estimate is statistically significant. Under HC1 the ND estimate is marginal ($p = 0.073$) and the ANTARSYA estimate passes the 5 percent threshold ($p = 0.044$) but fails the false discovery rate correction of Appendix B.2; under Conley inference both are significant ($p = 0.013$) and ANTARSYA survives the correction. Since the Conley variance equals the HC0 variance plus the kernel-weighted cross-products of neighbouring scores, $s_i = \tilde{d}_i \hat{u}_i$, where $\tilde{d}_i$ is the treatment net of the covariates and municipality effects and $\hat{u}_i$ the residual, a narrower interval can only arise when these cross-products are negative on balance. Table B7 reports this decomposition for every outcome.

We first rule out negative spatial dependence, that is, the possibility that the errors of neighbouring centers move in opposite directions. We consider it first because it is the one case in which the narrower intervals would be justified. If neighbouring errors offset one another, the estimate is more precise than the conventional standard errors suggest. We find no evidence of this. Moran’s I of the residuals within 1 km is positive for ND (0.17, $z = 6.9$), the incumbent family, SYRIZA, KKE, Golden Dawn and the extremist family,

Table B6: Robustness of the violence coefficient to the standard-error estimator (main 750 m specification). Each cell reports the p -value of the violence (dosage) coefficient under heteroskedasticity-robust (HC1) and Conley spatial-HAC standard errors at 0.5, 1, and 2km cutoffs. Point estimates are identical across estimators. ** $p < 0.01$; * $p < 0.05$.

Outcome	Coefficient	<i>p</i> -value of the violence coefficient			
		HC1	Conley 0.5 km	Conley 1 km	Conley 2 km
<i>Party families</i>					
Incumbent	0.0010	0.240	0.139	0.081	0.052
Radical Right	0.0004	0.004**	0.003**	0.003**	0.006**
Extremist	−0.0005	0.015*	0.014*	0.025*	0.026*
<i>Turnout</i>					
Turnout	−0.0002	0.702	0.694	0.686	0.588
<i>Individual parties</i>					
ANTARSYA	−0.0002	0.044*	0.018*	0.013*	0.005**
KKE	0.0008	<0.001**	<0.001**	<0.001**	<0.001**
SYRIZA	−0.0010	0.003**	0.002**	0.002**	0.007**
DIMAR	0.0006	0.002**	0.001**	<0.001**	<0.001**
PASOK	−0.0007	<0.001**	<0.001**	<0.001**	<0.001**
ND	0.0017	0.073	0.025*	0.013*	0.006**
ANEL	0.0006	<0.001**	<0.001**	<0.001**	<0.001**
LAOS	−0.0002	0.123	0.116	0.198	0.181
Golden Dawn	−0.0003	0.162	0.153	0.199	0.185

and indistinguishable from zero for the remaining outcomes. We argue instead that the narrowing reflects the concentration of the identifying variation. The municipality of Athens holds between 74 and 94 percent of the score mass for every outcome, and for ND and the incumbent family the negative cross-products come almost entirely from pairs among the eleven centers in central Athens with five or more incidents within 750m. Because least squares constrains the scores to sum to zero, large scores that sit in one compact cluster must offset one another, and the kernel reads that offsetting as negative covariance between neighbours. The narrowing is thus what the estimator produces on this design even when the errors are independent. To show this, we hold every regressor, coordinate and municipality fixed and give the actual residuals independent random signs. The observed ratio of the Conley to the HC0 variance falls at the 42nd percentile of the resulting distribution for ND, at the 48th for ANTARSYA and at the 29th for the incumbent family, which indicates that the narrowing we observe is what independent errors would produce. Among the family outcomes, the extremist family is the exception. Its ratio of 1.30 falls at the 99th percentile, so its wider Conley interval reflects genuine positive dependence.

Having explained where the narrowing comes from, we now turn to what it means for our inference. Because the narrowing arises from the design and not from the data, the Conley standard errors are smaller than they should be. In the same draws, the mean Conley standard error is between 70 and 99 percent of the true one, depending on the outcome, and 70 and 71 percent for the incumbent family and ND (Table B7, “SE ratio”). For this reason, Tables B7 and B8 also report a second, more conservative p -value from a leverage-adjusted Conley estimator. It is computed from the same Bartlett sandwich with the residuals scaled

by $1/(1 - h_{ii})$ as in HC3, and in the same draws it is between 82 and 108 percent of the true standard error. We keep Conley as our primary estimator, since it is designed for the kind of dependence that overlapping buffers create. We use the leverage-adjusted p -values as a robustness check. Under the adjusted estimator the extremist family is at $p = 0.046$, ND at 0.032 and ANTARSYA at 0.026, while the radical right family (0.006) and the incumbent null (0.136) are unaffected.

Table B7: Diagnostics of the Conley (1 km) variance of the violence coefficient (main 750 m specification). Moran’s I : residuals of the model, binary 1 km distance-band weights, row-standardised, with its z -statistic. V_C/V_{HC0} : the Conley variance as a share of the HC0 variance; within and across: the kernel-weighted cross-products of neighbouring scores, split by whether the pair shares a municipality, as shares of the HC0 part. Athens share: the municipality of Athens’ share of the sum of squared scores. Pctile indep.: the percentile at which the observed variance ratio falls among 1,000 draws in which the actual residuals are given independent random signs. SE ratio: the mean Conley standard error in those draws as a share of the true standard error. The kernel matrix is positive semi-definite at 0.5 and 1 km and not at 2 km; the full sandwich is positive semi-definite for every outcome at every cutoff. ** $p < 0.01$; * $p < 0.05$.

Outcome	Coefficient	Moran's I , 1 km			Off-diagonal / diag.		Athens share	Pctile indep.	SE ratio	p , 1 km	
		I	z	V_C/V_{HC0}	within	across				Conley	Lev.-adj.
<i>Party families</i>											
Incumbent	0.0010	0.161	6.5	0.49	−0.51	0.000	92%	0.29	0.70	0.081	0.136
Radical Right	0.0004	0.021	0.9	1.02	0.02	0.002	74%	0.72	0.94	0.003**	0.006**
Extremist	−0.0005	0.055	2.3	1.30	0.31	−0.005	92%	0.99	0.87	0.025*	0.046*
<i>Turnout</i>											
Turnout	−0.0002	0.024	1.0	0.98	−0.02	−0.001	94%	0.94	0.87	0.686	0.714
<i>Individual parties</i>											
ANTARSYA	−0.0002	0.017	0.7	0.71	−0.29	0.002	93%	0.48	0.82	0.013*	0.026*
KKE	0.0008	0.108	4.4	0.77	−0.23	0.001	78%	0.42	0.82	<0.001**	<0.001**
SYRIZA	−0.0010	0.186	7.5	0.99	−0.02	0.004	76%	0.76	0.88	0.002**	0.005**
DIMAR	0.0006	0.029	1.2	0.58	−0.42	−0.002	84%	0.12	0.82	<0.001**	<0.001**
PASOK	−0.0007	−0.015	−0.5	0.96	−0.04	−0.004	90%	0.71	0.88	<0.001**	<0.001**
ND	0.0017	0.170	6.9	0.57	−0.43	0.000	92%	0.42	0.71	0.013*	0.032*
ANEL	0.0006	0.013	0.6	1.05	0.05	−0.002	77%	0.69	0.99	<0.001**	<0.001**
LAOS	−0.0002	−0.013	−0.5	1.56	0.56	−0.002	86%	1.00	0.82	0.198	0.269
Golden Dawn	−0.0003	0.071	2.9	1.29	0.30	−0.002	93%	0.97	0.83	0.199	0.257

Spatial dependence can be modelled in more than one way, and a Conley cutoff is only one of them. Table B8 therefore reports five further inference methods beside it: the leverage-adjusted Conley estimator just described; cluster-robust errors on the 1 km grid cells used in Appendix E; municipality-clustered errors, computed with the bias-reduced CR2 estimator with Satterthwaite degrees of freedom (Bell and McCaffrey, 2002) and again through a wild-cluster bootstrap (Cameron, Gelbach, and Miller, 2008) with 1,999 Rademacher draws; and randomization inference that re-randomizes the locations of the 89 incidents that fall within 750 m of a voting center. Each incident is relocated to a randomly drawn voting center within its own municipality, so that the number of incidents per municipality and the overlap of neighbouring buffers are preserved, and the 750 m counts are rebuilt from the relocated points. Because the relocated exposure is less dispersed than the observed one, the test statistic is the studentized coefficient rather than the coefficient itself (Young, 2019; Chung and Romano, 2013), and the p -value is the share of 99,999 draws in which it exceeds the observed value in absolute value.

Table B8 shows how each result fares under the six inference methods described above. The extremist family penalty is the only result that remains significant under all six ($p = 0.025$ under Conley inference, 0.046 under the leverage-adjusted estimator, 0.024 under grid-cell clustering, 0.042 under municipality CR2, 0.025 under the wild bootstrap and 0.049 under

randomization inference). The radical right family is less robust. It holds under Conley, the adjusted estimator, grid-cell clustering and randomization inference ($p = 0.022$) but under neither municipality method (CR2 $p = 0.195$, bootstrap $p = 0.54$). ND and ANTARSYA are the least robust of the three, holding under Conley and the adjusted estimator only. They fail grid-cell clustering ($p = 0.125$ and 0.129), municipality CR2 (0.127 and 0.254), the wild bootstrap (0.46 and 0.63) and randomization inference (0.37 and 0.10), which is why we make the extremist claim for the family rather than for either party.

Two columns of Table B8, municipality CR2 and the municipality wild bootstrap, treat each municipality as a single cluster. Given that these two columns display mostly non-significant results, we believe that further clarification is needed. First, municipality clustering answers a question about a design we did not use. Treatment varies at the level of the voting center, which is much more granular than the municipal level. Each center’s exposure is the number of incidents within 750 m of it, whereas a municipality contains about 15 centers on average (603 centers in 41 municipalities). Our sample is also the population of voting centers in the two metropolitan areas rather than a sample of clusters (Abadie et al., 2023). We therefore report municipality clustering as a stress test, and a demanding one, since the identifying variation is concentrated in Athens and the effective number of clusters for the violence coefficient is only 1.4 of 41 (Carter, Schnepel, and Steigerwald, 2017). This concentration is also why we use CR2 rather than the standard analytic CR1 estimator. With the coefficient fit largely within Athens, the scores inside each municipality cancel and the CR1 variance collapses, which pushes its p -values below HC1 even for null outcomes; the leverage adjustment in CR2, together with the Satterthwaite degrees of freedom, corrects for this. Second, the wild bootstrap reverses the Conley picture within the extremist family. Golden Dawn reaches significance ($p = 0.049$) while ANTARSYA does not ($p = 0.63$), yet the family ($p = 0.025$) is estimated more precisely than either party. This is because the family’s municipality score sums are the sums of the two parties’ score sums, which are negatively correlated across municipalities ($r = -0.52$), so that the family’s sum of squared cluster scores is only 54 percent of the two parties’ combined.

Table B8: The violence coefficient under alternative inference methods (main 750 m specification). Point estimates are identical across methods; only inference differs. Conley: Bartlett kernel, 1 km cutoff (primary). Lev.-adj. Conley: the same sandwich with HC3-scaled residuals. Grid-cell cluster: 1 km×1 km cells. Municipality CR2: bias-reduced cluster-robust estimator with Satterthwaite degrees of freedom, 41 clusters. Muni. wild bootstrap: wild-cluster bootstrap- t , 1,999 Rademacher draws. Randomization inference: incidents relocated within municipality, 750 m counts rebuilt, studentized statistic, 99,999 draws. ** $p < 0.01$; * $p < 0.05$.

Outcome	Coefficient	<i>p</i> -value of the violence coefficient					
		Conley 1 km	Lev.-adj. Conley 1 km	Grid-cell cluster	Municipality CR2	Muni. wild bootstrap	Randomization inference
<i>Party families</i>							
Incumbent	0.0010	0.081	0.136	0.289	0.243	0.562	0.526
Radical Right	0.0004	0.003**	0.006**	0.009**	0.195	0.542	0.022*
Extremist	−0.0005	0.025*	0.046*	0.024*	0.042*	0.025*	0.049*
<i>Turnout</i>							
Turnout	−0.0002	0.686	0.714	0.759	0.506	0.206	0.751
<i>Individual parties</i>							
ANTARSYA	−0.0002	0.013*	0.026*	0.129	0.254	0.629	0.102
KKE	0.0008	<0.001**	<0.001**	<0.001**	0.095	0.314	0.008**
SYRIZA	−0.0010	0.002**	0.005**	0.003**	0.157	0.478	0.111
DIMAR	0.0006	<0.001**	<0.001**	0.002**	0.120	0.418	0.005**
PASOK	−0.0007	<0.001**	<0.001**	<0.001**	0.038*	0.108	0.010*
ND	0.0017	0.013*	0.032*	0.125	0.127	0.462	0.369
ANEL	0.0006	<0.001**	<0.001**	<0.001**	0.103	0.241	0.002**
LAOS	−0.0002	0.198	0.269	0.113	0.149	0.234	0.441
Golden Dawn	−0.0003	0.199	0.257	0.215	0.208	0.049*	0.260

B.2 Multiple testing: confirmatory and exploratory hypotheses

Our analysis estimates the effect of violence on turnout, on three party families and on nine individual parties, which raises the concern that some of the coefficients we report as significant reflect the number of tests conducted rather than any underlying electoral response. We therefore distinguish confirmatory from exploratory tests. The five hypotheses stated in the main text (H1 on turnout, H2 on the incumbent, H3a on the conservative ND, H3b on the radical right, and H4 on extremist parties) are pre-specified and are reported without adjustment, since a correction for multiplicity is a remedy for searching across outcomes rather than for testing hypotheses chosen in advance. The party-by-party estimates, in contrast, are exploratory, and for these we report Benjamini-Hochberg (FDR) and Bonferroni (FWER) adjusted p -values beside the raw ones. The Benjamini-Hochberg correction limits the expected share of false positives among the results declared significant. The Bonferroni correction is stricter and limits the chance of even one false positive. Table B9 presents both panels.⁶ All corrections are applied to the Conley (1 km) p -values rather than to the HC1 values, so that the corrected and uncorrected p -values come from the same estimator. Because Appendix B.1 shows that the Conley narrowing for ND and ANTARSYA is a property of the design, the table also reports the raw and FDR-adjusted values under the leverage-adjusted Conley estimator and under HC1, so that it is clear when a result depends on the estimator. Corrections applied to the placebo grid are a separate exercise and are reported in Appendix D.

⁶ND appears in both panels, without adjustment in Panel A as the test of H3a and with the corrections in Panel B as one of the nine party estimates.

Table B9: Multiple testing across the confirmatory and exploratory sets. Panel A reports the five pre-specified hypotheses (the four family outcomes and ND, the test of H3a) without adjustment. Panel B reports the nine party-by-party effects with Benjamini-Hochberg and Bonferroni adjustments, ordered by the raw Conley (1 km) p -value. The Conley spatial-HAC (1 km) columns are the primary inference; the leverage-adjusted Conley and HC1 columns repeat the raw and Benjamini-Hochberg values under those estimators (Appendix B.1). Significance stars on each raw column reflect that column’s p -value. ** $p < 0.01$; * $p < 0.05$.

Hypothesis	Coefficient	Conley 1 km			Lev.-adj. Conley		HC1	
		Raw	BH (FDR)	Bonferroni	Raw	BH	Raw	BH
<i>Panel A. Confirmatory (pre-specified)</i>								
Turnout (H1)	−0.0002	0.686	—	—	0.714	—	0.702	—
Incumbent (H2)	0.0010	0.081	—	—	0.136	—	0.240	—
ND (H3a)	0.0017	0.013*	—	—	0.032*	—	0.073	—
Radical Right (H3b)	0.0004	0.003**	—	—	0.006**	—	0.004**	—
Extremist (H4)	−0.0005	0.025*	—	—	0.046*	—	0.015*	—
<i>Panel B. Exploratory (party-by-party)</i>								
KKE	0.0008	<0.001**	<0.001	<0.001	<0.001**	<0.001	<0.001**	<0.001
DIMAR	0.0006	<0.001**	<0.001	<0.001	<0.001**	0.001	0.002**	0.004
ANEL	0.0006	<0.001**	<0.001	<0.001	<0.001**	0.001	<0.001**	<0.001
PASOK	−0.0007	<0.001**	<0.001	0.001	<0.001**	0.001	<0.001**	0.001
SYRIZA	−0.0010	0.002**	0.004	0.018	0.005**	0.009	0.003**	0.006
ANTARSYA	−0.0002	0.013*	0.017	0.116	0.026*	0.038	0.044*	0.067
ND	0.0017	0.013*	0.017	0.118	0.032*	0.042	0.073	0.094
LAOS	−0.0002	0.198	0.199	1.000	0.269	0.269	0.123	0.139
Golden Dawn	−0.0003	0.199	0.199	1.000	0.257	0.269	0.162	0.162

After the corrections, five party-level results remain significant under every estimator, and two depend on the estimator used. Under Conley inference, seven of the nine party effects are significant at the 5% level before any adjustment, and all seven survive the false discovery rate correction (the largest adjusted value is 0.017). Five of them (KKE, ANEL, DIMAR, PASOK and SYRIZA) also survive the stricter Bonferroni correction, and they survive both corrections under all three estimators.

The other two are ANTARSYA and ND. Neither survives the Bonferroni correction under Conley inference (0.116 and 0.118), nor under either of the other two estimators. Whether they survive the false discovery rate correction depends on the estimator. They do under Conley inference and, more narrowly, under the leverage-adjusted estimator (0.038 and 0.042), but not under HC1 (0.067 and 0.094). Both also fail grid-cell clustering, municipality clustering and randomization inference (Appendix B.1). We therefore treat the two estimates as suggestive rather than established, and no claim in the paper rests on either.

ND has a second role, as the test of the pre-specified hypothesis H3a. Reported without adjustment, it is significant under Conley inference ($p = 0.013$) and under the leverage-adjusted estimator ($p = 0.032$) but not under HC1 ($p = 0.073$). The main text describes ND as suggestive because it does not hold under the alternative inference methods in Appendix B.1, and not because of the multiple-testing correction. Our main claim, the extremist penalty, is made at the family level, where it does not depend on any single party.⁷

The breakdown by far-left and far-right violence is exploratory as well. We do not correct it for multiple testing, because we draw no inferential conclusion from it. As Appendix H sets out, we treat the same-side pattern as descriptive.

⁷Readers who prefer to correct the confirmatory hypotheses as well can note that Benjamini-Hochberg applied across the five gives 0.017 for the radical right family, 0.033 for ND and 0.042 for the extremist family at the 1 km cutoff, and 0.016, 0.041 and 0.034 respectively at 0.5 km. Under the mean reversion adjustment of Appendix C.7, where the extremist estimate strengthens to $p = 0.004$ and the radical right estimate collapses to $p = 0.57$, the same correction returns 0.021 for the extremist family.

B.3 Effects of political violence on party families

Table B10: The table reports the results of our regressions of vote choice on incidents of political violence for parties in the governing coalition, Radical Right, and Extremist parties. The coefficients of violence from this table appear in Figure 4(b) of our main paper. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>		
	Incumbent	Radical Right	Extremist
	(1)	(2)	(3)
Incidents of violence between elections	0.0010 (0.0006)	0.0004** (0.0001)	−0.0005* (0.0002)
family income	0.0241** (0.0034)	0.0005 (0.0007)	−0.0003 (0.0007)
PhD %	2.3318** (0.8525)	−0.4584 (0.2636)	−0.1872 (0.1572)
master %	−0.4508 (0.3086)	0.1518 (0.1109)	0.0973 (0.0741)
university education %	0.1235 (0.0954)	−0.0118 (0.0327)	−0.0276 (0.0219)
foreign citizenship %	0.0075 (0.0465)	−0.0083 (0.0235)	0.0003 (0.0127)
unemployment %	0.2483 (0.2377)	−0.1098 (0.1232)	−0.0535 (0.0549)
unemployment young %	−0.0296 (0.4428)	−0.0025 (0.1753)	0.1262 (0.1181)
F-statistic	43.5814	5.7401	4.5008
p-value	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes
Observations	603	603	603
Adjusted R ²	0.7725	0.2743	0.2182

B.4 Violence regressed on the May 2012 vote shares

Table B11: Violent reaction to the May 2012 electoral results. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>
	Violence
Voter turnout May 2012	−3.1481 (1.6921)
ANTARSYA vs May 2012	85.9288* (42.7393)
KKE vs May 2012	−13.9423 (8.6213)
SYRIZA vs May 2012	−11.3551 (10.1844)
Dimar vs May 2012	−4.0639 (12.6587)
PASOK vs May 2012	9.3299 (7.5695)
ND vs May 2012	−9.3323 (10.6197)
Anel vs May 2012	−22.9597 (12.0641)
Laos vs May 2012	3.7761 (15.6778)
GD vs May 2012	−5.9196 (8.2566)
family income	0.1655 (0.2976)
PhD %	−33.5991 (33.0853)
master %	21.3558 (12.1585)
university education %	−5.7866 (3.6846)
foreign citizenship %	−2.9722 (2.5493)
unemployment %	0.1457 (10.5605)
unemployment young %	50.5745 (34.8215)
F-statistic	4.1013
p-value	0.0000
Municipality FE	yes
Observations	603
Adjusted R ²	0.2270

B.5 Effects of political violence on voter turnout

Table B12: Effect of political violence on voter turnout. This table reports the effects of the total number of incidents of political violence on voter turnout, with socioeconomic covariates and municipality fixed effects. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$. The table corresponds to Figure 4(a) in the main paper.

	<i>Dependent variable:</i>
	Turnout
dosage	−0.0002 (0.0004)
family income	0.0058** (0.0010)
PhD %	0.9673** (0.3418)
master %	−0.3652* (0.1669)
university education %	0.0579 (0.0411)
foreign citizenship %	−0.0004 (0.0218)
unemployment %	0.1647 (0.1118)
unemployment young %	−0.0937 (0.2604)
F-statistic	8.0119
p-value	0.0000
Municipality FE	yes
Observations	603
Adjusted R ²	0.3586

B.6 Effects of political violence on individual party vote shares

Table B13: Effect of political violence on individual party vote shares. Each column reports the effects of the total number of incidents of political violence on the difference in vote shares of the corresponding party between the May and June 2012 elections, with socioeconomic covariates and municipality fixed effects. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$. The table corresponds to Figure 4(b) in the main paper.

	<i>Dependent variable:</i>								
	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
dosage	−0.0002* (0.0001)	0.0008** (0.0002)	−0.0010** (0.0003)	0.0006** (0.0001)	−0.0007** (0.0002)	0.0017* (0.0007)	0.0006** (0.0001)	−0.0002 (0.0001)	−0.0003 (0.0003)
family income	0.0001 (0.0002)	0.0046** (0.0011)	−0.0105** (0.0023)	0.0021 (0.0011)	−0.0030** (0.0005)	0.0271** (0.0036)	0.0003 (0.0007)	0.0002 (0.0004)	−0.0004 (0.0008)
PhD %	0.0852 (0.0881)	0.4620 (0.3065)	−0.5714 (0.5296)	0.3928 (0.2669)	−0.3626* (0.1693)	2.6944** (0.8580)	−0.3998 (0.2281)	−0.0586 (0.1223)	−0.2724 (0.1621)
master %	0.0328 (0.0322)	−0.3215** (0.1169)	0.3187 (0.2014)	−0.3185** (0.1068)	0.1880* (0.0735)	−0.6387* (0.3217)	0.1694 (0.0927)	−0.0176 (0.0505)	0.0644 (0.0697)
university education %	−0.0115 (0.0092)	0.0833* (0.0393)	−0.1369* (0.0646)	0.0599* (0.0289)	−0.0540** (0.0196)	0.1775 (0.0985)	−0.0056 (0.0265)	−0.0062 (0.0138)	−0.0162 (0.0207)
foreign citizenship %	−0.0021 (0.0063)	0.0371 (0.0242)	−0.0249 (0.0388)	0.0012 (0.0139)	−0.0174 (0.0110)	0.0249 (0.0466)	−0.0059 (0.0206)	−0.0024 (0.0073)	0.0023 (0.0127)
unemployment %	−0.0088 (0.0241)	0.0042 (0.1203)	−0.0420 (0.2060)	−0.0501 (0.0806)	−0.0029 (0.0557)	0.2512 (0.2222)	−0.0667 (0.1084)	−0.0431 (0.0388)	−0.0447 (0.0549)
unemployment young %	−0.0058 (0.0599)	−0.5995* (0.2437)	0.2827 (0.4050)	−0.3972** (0.1381)	0.1471 (0.1293)	−0.1767 (0.4436)	0.1873 (0.1698)	−0.1898* (0.0801)	0.1320 (0.1123)
F-statistic	5.0874	19.6696	24.5245	11.2570	6.3950	46.9305	6.3721	3.1659	3.5354
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.2458	0.5982	0.6523	0.4499	0.3008	0.7855	0.2999	0.1473	0.1682

The party-specific estimates in Figure 4(b) of the main text and Table B13 vary within each camp. On the left, each incident is associated with a loss of about 0.7% of baseline support for PASOK, 0.5% for SYRIZA and 1.4% for ANTARSYA, and with a gain of about 0.8% for KKE and 1.0% for DIMAR. On the right, ANEL gains while LAOS shows a negative but insignificant estimate. We treat these estimates as exploratory, and no claim in the paper rests on them. They are also not equally robust. PASOK, KKE and SYRIZA show placebo leakage (Appendix D), and the SYRIZA estimate does not survive the nearest-window control ($p = 0.46$). The ANTARSYA and ANEL estimates do not survive the mean reversion adjustment (Appendix C.7), and none of these estimates holds under the municipality bootstrap (Table B8). With those cautions, we offer two readings of the pattern, which our aggregate design cannot test. SYRIZA’s negative estimate may reflect its association with the 2008 riots. Its refusal to condemn the violence that erupted after a student’s murder, unlike KKE which explicitly did so, may have reinforced voter perceptions linking the party to unrest (Malamidis, 2020). ANEL’s gains are consistent with its position as a fresh challenger whose anti-establishment appeal drew disaffected voters, while LAOS, already weakened by its participation in the pre-election coalition government, loses under far-right violence in the disaggregated results of Appendix H. If these readings are right, voter responses to violence depend on a party’s specific positioning and perceived association with instability as well as on its ideological family.

C Robustness

C.1 Overview of robustness checks

Our baseline findings could be sensitive to the specific distance threshold chosen, to demographic differences between treated and control voting centers, or to changes in policing between elections. This section evaluates each of these concerns systematically, testing varying distance thresholds, discussing the potential influence of media coverage, analyzing matched samples to achieve covariate balance, and examining whether policing changed during the inter-election period. Full analytic tables and figures are provided below.

C.2 Distance from political violence incidents

C.2.1 Results over varying distances from voting centers

Our baseline specification defines treatment using a 750m radius, but the results could be sensitive to this particular choice. To assess this, we varied the radius around voting centers, analyzing 250m, 500m, 750m (base specification), and 1000m radii. Figures C2 and C3 show the coefficients for turnout, party families, and individual party models across these distances.

For turnout and party family vote shares (Figure C2), we find no statistically significant effect of political violence on turnout at any radius. Radical right support increases significantly at 500m, 750m, and 1000m, and at 250m under Conley inference only, with the largest estimate at 250m. The extremist penalty is significant at 500m, 750m, and 1000m, but attenuates at 250m, where the point estimate falls to near zero. For individual party vote shares (Figure C3), results remain robust across radii, with the same exception of ANTARSYA at 250m.

C.2.2 Locality of political violence and national media

A key concern in our research is whether incidents of political violence reach voters through national media. While media dissemination could spread the political effects of violence to distant areas, it does not undermine the locality of the effect; voters are likely more affected by violence in their own neighborhoods than in distant regions.

This possibility does not weaken our findings. If media widely covered these incidents, our estimates would likely understate the true local impact, as media exposure would reduce the role of physical proximity. Thus, our effects may represent a lower bound of the influence of political violence on turnout and voter choice.

To evaluate media influence, we reviewed coverage in newspapers and online sources. Of 148 incidents in our dataset, only 8 were reported in the media, with 3 additional incidents reported but not recorded by police. The small number of media-reported cases prevents their inclusion as a control in regressions. However, the lack of coverage for most incidents supports our conclusion that direct exposure to political violence, rather than media awareness, drives voter behavior.

C.3 Results using a matched sample

A potential concern is that treated and control voting centers differ systematically in their demographic composition, and that these differences, rather than violence exposure, drive the observed electoral patterns. As Figure 3 in the main text shows, some control units are geographically isolated or far from any treatment unit, raising questions about their comparability. To address this, we repeat all our main regressions using matched samples at the 750m distance threshold, ensuring that treated and control units are similar on observable characteristics. Figure C4 plots treatment and control units before and after exact matching. After matching, the control units are spatially much closer to the treatment units. Diagnostics confirming the improvement of the control units after matching are given in detail below.

Repeating the analysis in a matched sample addresses the concern that treated and control voting centers differ in their observable characteristics. Matching balances the two groups on the observed covariates to the extent possible, so that they differ mainly in their exposure to violence. It cannot rule out differences in characteristics that we do not observe. Figure C8 plots the coefficients of all our core regressions, repeated in the matched sample. The turnout estimate remains statistically indistinguishable from zero, and the radical right and extremist family estimates remain significant. In most cases the estimates are qualitatively unchanged and somewhat larger in the matched sample. Two results differ. The ANTARSYA estimate is no longer significant ($p = 0.12$), and the incumbent family estimate becomes significant, a point we return to in Appendix C.5.

C.4 Regression tables at the 250 m, 500 m and 1000 m radii

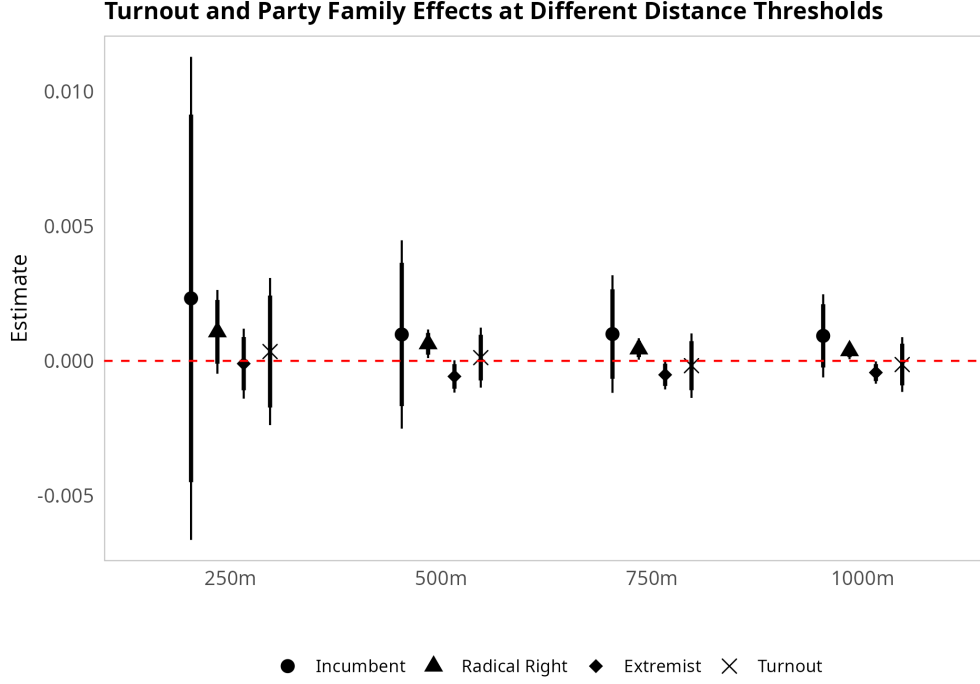


Figure C2: Turnout and party family effects at different distance thresholds. Estimated coefficients and 95% (thick line) and 99% (thin line) confidence intervals, from Conley spatial-HAC (1 km) standard errors, for turnout, incumbent, radical right, and extremist party family vote share models using 250m, 500m, 750m, and 1000m distance thresholds around voting centers. Control variable estimates are suppressed for clarity.

C.4.1 250 m radius

The first robustness exercise restricts attention to violent events occurring at a 250m radius around the voting centers.

Table C1: Mid-election violence and the May-to-June change in vote shares at a 250 m radius. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
incidents between elections	0.0010 (0.0009)	-0.0002 (0.0003)	0.0029** (0.0005)	-0.0043** (0.0010)	0.0018** (0.0005)	-0.0015** (0.0005)	0.0067* (0.0031)	0.0009* (0.0004)	0.0002 (0.0002)	-0.00002 (0.0004)
F-statistic	7.8296	5.4720	19.3293	23.6837	11.8729	5.3801	30.7082	7.1421	3.5033	3.9417
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3175	0.2335	0.5552	0.6071	0.4255	0.2298	0.6692	0.2949	0.1457	0.1669

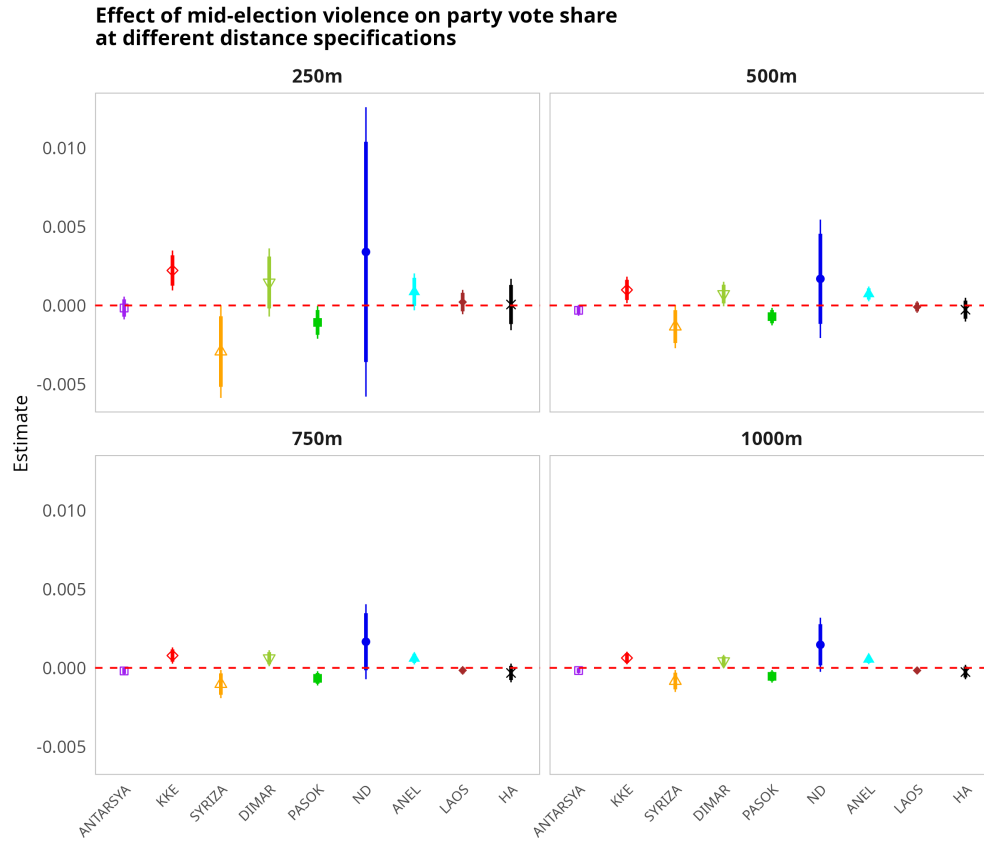


Figure C3: Effect of mid-election violence on turnout and party vote share at different distance specifications. This figure displays the estimated coefficients and 95% (thick line) and 99% (thin line) confidence intervals, from Conley spatial-HAC (1 km) standard errors, for models of turnout and specific party vote shares using 250m, 500m, 750m, and 1000m distance thresholds around voting centers. Note that control variable estimates are suppressed for clarity.

Table C2: Mid-election violence and the May-to-June change in vote shares at a 250 m radius, with socioeconomic covariates. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dosage	0.0003 (0.0008)	-0.0002 (0.0003)	0.0022** (0.0004)	-0.0029** (0.0010)	0.0015** (0.0006)	-0.0011* (0.0004)	0.0034 (0.0025)	0.0009* (0.0004)	0.0002 (0.0002)	0.0001 (0.0004)
family income	0.0058** (0.0010)	0.0001 (0.0002)	0.0049** (0.0011)	-0.0109** (0.0023)	0.0023* (0.0011)	-0.0032** (0.0006)	0.0276** (0.0037)	0.0005 (0.0006)	0.0001 (0.0004)	-0.0006 (0.0007)
PhD %	0.9596** (0.3368)	0.0876 (0.0893)	0.4444 (0.3160)	-0.5394 (0.5369)	0.3813 (0.2703)	-0.3471* (0.1729)	2.6175** (0.8666)	-0.4088 (0.2235)	-0.0558 (0.1208)	-0.2664 (0.1583)
master %	-0.3647* (0.1655)	0.0319 (0.0322)	-0.3122** (0.1186)	0.3052 (0.2023)	-0.3119** (0.1064)	0.1798* (0.0735)	-0.6114 (0.3239)	0.1749 (0.0924)	-0.0185 (0.0503)	0.0632 (0.0683)
university education %	0.0571 (0.0410)	-0.0113 (0.0092)	0.0804* (0.0386)	-0.1328* (0.0636)	0.0581* (0.0285)	-0.0526** (0.0196)	0.1718 (0.0970)	-0.0065 (0.0263)	-0.0065 (0.0137)	-0.0163 (0.0205)
foreign citizenship %	-0.0004 (0.0218)	-0.0018 (0.0063)	0.0361 (0.0242)	-0.0236 (0.0384)	0.0005 (0.0138)	-0.0170 (0.0111)	0.0240 (0.0461)	-0.0064 (0.0207)	-0.0024 (0.0073)	0.0027 (0.0126)
unemployment %	0.1663 (0.1111)	-0.0100 (0.0242)	0.0103 (0.1178)	-0.0511 (0.2012)	-0.0462 (0.0804)	-0.0055 (0.0564)	0.2630 (0.2126)	-0.0640 (0.1080)	-0.0433 (0.0385)	-0.0467 (0.0544)
unemployment young %	-0.1083 (0.2595)	-0.0119 (0.0601)	-0.5865* (0.2414)	0.2747 (0.3979)	-0.3848** (0.1333)	0.1389 (0.1284)	-0.1955 (0.4329)	0.2017 (0.1708)	-0.1933* (0.0790)	0.1236 (0.1105)
F-statistic	8.0532	4.8809	19.5804	24.5355	11.0559	5.9188	46.2847	6.1549	3.1209	3.4422
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3600	0.2363	0.5970	0.6524	0.4450	0.2817	0.7831	0.2913	0.1446	0.1630

C.4.2 500 m radius

Table C3: Mid-election violence and the May-to-June change in vote shares at a 500 m radius. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
incidents between elections	0.0006 (0.0004)	-0.0003** (0.0001)	0.0014** (0.0003)	-0.0022** (0.0005)	0.0009** (0.0002)	-0.0009** (0.0002)	0.0039** (0.0011)	0.0008** (0.0002)	-0.0001 (0.0001)	-0.0003 (0.0002)
F-statistic	7.8553	5.7327	19.4813	23.9225	12.0053	5.6670	31.2994	7.3148	3.4967	3.9879
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3183	0.2438	0.5573	0.6096	0.4284	0.2412	0.6736	0.3007	0.1453	0.1691

Table C4: Mid-election violence and the May-to-June change in vote shares at a 500 m radius, with socioeconomic covariates. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dosage	0.0001 (0.0004)	-0.0003** (0.0001)	0.0010** (0.0003)	-0.0013** (0.0005)	0.0007** (0.0002)	-0.0007** (0.0002)	0.0017 (0.0010)	0.0007** (0.0002)	-0.0001 (0.0002)	-0.0003 (0.0002)
family income	0.0057** (0.0010)	0.0002 (0.0002)	0.0047** (0.0011)	-0.0106** (0.0023)	0.0022 (0.0011)	-0.0030** (0.0006)	0.0273** (0.0037)	0.0002 (0.0007)	0.0002 (0.0004)	-0.0005 (0.0007)
PhD %	0.9603** (0.3367)	0.0847 (0.0877)	0.4510 (0.3116)	-0.5486 (0.5301)	0.3865 (0.2684)	-0.3527* (0.1704)	2.6296** (0.8567)	-0.4026 (0.2284)	-0.0571 (0.1213)	-0.2692 (0.1611)
master %	-0.3652* (0.1655)	0.0313 (0.0320)	-0.3148** (0.1175)	0.3085 (0.2008)	-0.3133** (0.1057)	0.1802* (0.0724)	-0.6148 (0.3212)	0.1751 (0.0931)	-0.0194 (0.0501)	0.0621 (0.0693)
university education %	0.0576 (0.0409)	-0.0118 (0.0092)	0.0840* (0.0393)	-0.1376* (0.0644)	0.0605* (0.0288)	-0.0545** (0.0196)	0.1774 (0.0976)	-0.0049 (0.0265)	-0.0063 (0.0137)	-0.0164 (0.0207)
foreign citizenship %	-0.0003 (0.0218)	-0.0021 (0.0062)	0.0370 (0.0242)	-0.0249 (0.0385)	0.0012 (0.0138)	-0.0177 (0.0110)	0.0256 (0.0462)	-0.0057 (0.0207)	-0.0025 (0.0073)	0.0024 (0.0127)
unemployment %	0.1662 (0.1112)	-0.0095 (0.0240)	0.0091 (0.1194)	-0.0495 (0.2029)	-0.0471 (0.0805)	-0.0045 (0.0559)	0.2609 (0.2135)	-0.0651 (0.1081)	-0.0430 (0.0387)	-0.0461 (0.0546)
unemployment young %	-0.1093 (0.2597)	-0.0085 (0.0592)	-0.5955* (0.2425)	0.2871 (0.4004)	-0.3916** (0.1355)	0.1459 (0.1276)	-0.2113 (0.4338)	0.1942 (0.1692)	-0.1919* (0.0795)	0.1268 (0.1111)
F-statistic	8.0514	5.1278	19.5871	24.5607	11.1166	6.0779	46.3901	6.2866	3.1215	3.4717
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3599	0.2476	0.5971	0.6526	0.4465	0.2882	0.7835	0.2965	0.1447	0.1646

C.4.3 1000 m radius

Table C5: Mid-election violence and the May-to-June change in vote shares at a 1000 m radius. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
incidents between elections	0.0001 (0.0004)	-0.0002* (0.0001)	0.0008** (0.0002)	-0.0013** (0.0003)	0.0005** (0.0001)	-0.0007** (0.0001)	0.0025** (0.0007)	0.0005** (0.0001)	-0.0002 (0.0001)	-0.0003 (0.0002)
F-statistic	7.7950	5.7725	19.8058	24.1104	12.0860	6.1056	32.1442	7.5350	3.6045	4.0904
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3164	0.2453	0.5616	0.6115	0.4302	0.2580	0.6796	0.3080	0.1507	0.1739

Table C6: Mid-election violence and the May-to-June change in vote shares at a 1000 m radius, with socioeconomic covariates. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dosage	-0.0001 (0.0004)	-0.0002** (0.0001)	0.0006** (0.0001)	-0.0008** (0.0003)	0.0004** (0.0001)	-0.0005** (0.0001)	0.0015** (0.0005)	0.0005** (0.0001)	-0.0002 (0.0001)	-0.0003 (0.0002)
family income	0.0059** (0.0010)	0.0002 (0.0002)	0.0047** (0.0011)	-0.0106** (0.0023)	0.0022* (0.0011)	-0.0029** (0.0005)	0.0269** (0.0036)	0.0001 (0.0007)	0.0003 (0.0004)	-0.0004 (0.0008)
PhD %	0.9574** (0.3395)	0.0857 (0.0877)	0.4487 (0.3100)	-0.5452 (0.5289)	0.3844 (0.2684)	-0.3521* (0.1703)	2.6305** (0.8482)	-0.4034 (0.2339)	-0.0582 (0.1227)	-0.2698 (0.1634)
master %	-0.3655* (0.1664)	0.0325 (0.0319)	-0.3188** (0.1169)	0.3138 (0.2002)	-0.3162** (0.1054)	0.1831* (0.0722)	-0.6218 (0.3201)	0.1721 (0.0944)	-0.0190 (0.0502)	0.0632 (0.0701)
university education %	0.0579 (0.0411)	-0.0111 (0.0093)	0.0815* (0.0397)	-0.1343* (0.0646)	0.0588* (0.0292)	-0.0525** (0.0201)	0.1721 (0.0983)	-0.0069 (0.0274)	-0.0058 (0.0137)	-0.0155 (0.0210)
foreign citizenship %	-0.0004 (0.0217)	-0.0018 (0.0063)	0.0360 (0.0241)	-0.0235 (0.0388)	0.0004 (0.0139)	-0.0170 (0.0110)	0.0239 (0.0462)	-0.0065 (0.0210)	-0.0024 (0.0073)	0.0027 (0.0127)
unemployment %	0.1682 (0.1113)	-0.0078 (0.0242)	0.0024 (0.1180)	-0.0407 (0.2007)	-0.0515 (0.0806)	0.0015 (0.0574)	0.2441 (0.2096)	-0.0711 (0.1098)	-0.0409 (0.0391)	-0.0430 (0.0556)
unemployment young %	-0.1030 (0.2588)	-0.0059 (0.0593)	-0.6068* (0.2422)	0.3017 (0.4016)	-0.3986** (0.1358)	0.1572 (0.1284)	-0.2450 (0.4369)	0.1830 (0.1733)	-0.1870* (0.0805)	0.1332 (0.1123)
F-statistic	8.0625	5.1604	19.8411	24.7073	11.1970	6.4171	47.3019	6.4783	3.2175	3.5500
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	603	603	603	603	603	603	603	603	603	603
Adjusted R ²	0.3603	0.2491	0.6004	0.6540	0.4484	0.3016	0.7869	0.3040	0.1502	0.1690

C.5 Matched sample: diagnostics and regression tables

In this section, we report results of matching regression models for estimation of our treatment effects. We perform nearest-neighbor matching using covariate-balancing propensity score (CBPS; Imai and Ratkovic, 2014) distance, with replacement. Figure C4 illustrates the geographical distribution of violence exposure across precincts with and without matching.

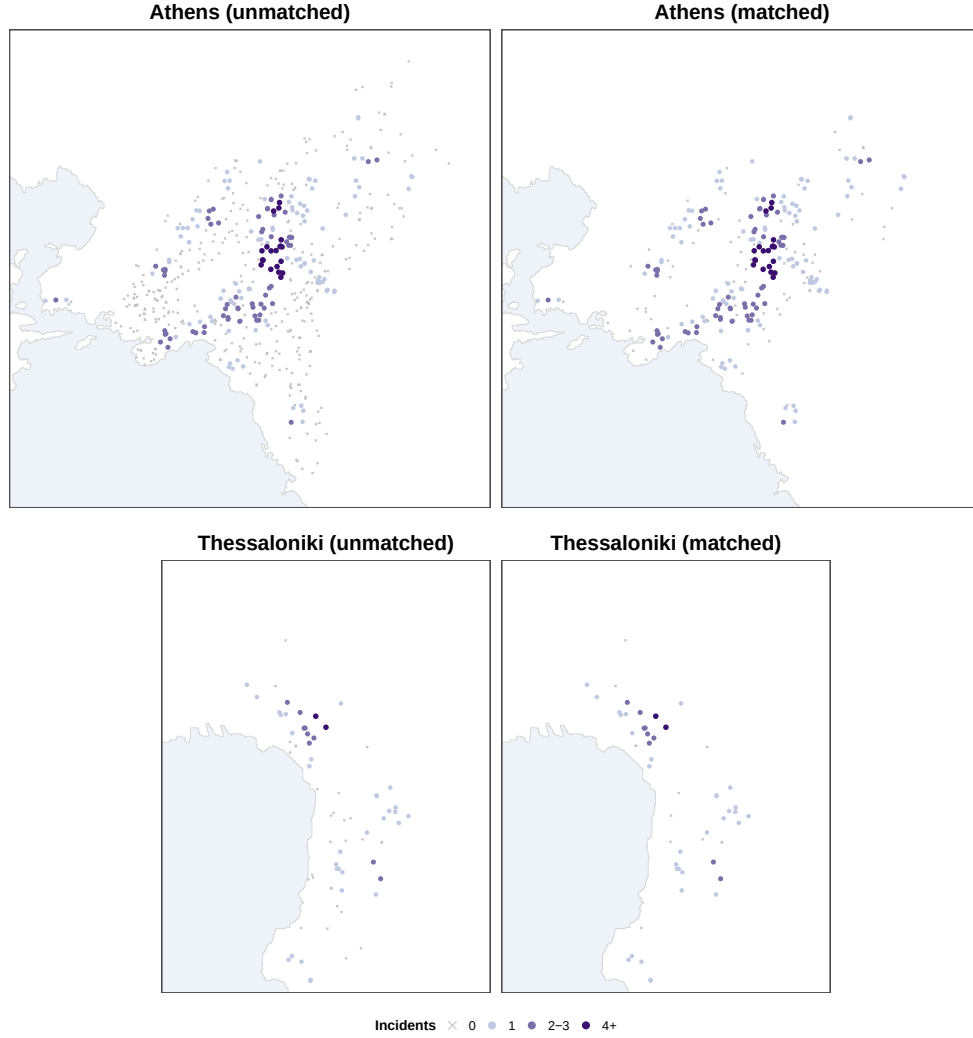


Figure C4: Violence exposure across precincts with and without matching. Each marker represents a voting center. Crosses denote unexposed centers; filled circles denote exposed centers, with darker shading indicating higher dosage.

Figures C5–C7 present diagnostics for our matched samples. Figure C5 depicts the distributional balance between the two samples for each covariate, which improves greatly with matching. Figure C6 presents standardized mean differences and Kolmogorov-Smirnov tests for each covariate, and again the symmetry between the treated and control samples improves after matching. Finally, Figure C7 presents the QQ plots of the two samples before and after matching. Increased balance between the two samples reinforces our confidence that treated and control voting centers are comparable on observable characteristics.

Figure C8 summarizes the matched sample results, mirroring the layout of Figure 4 in the main text. At the party family level (panel a), the pattern is highly stable. The Extremist coefficient is virtually unchanged (-0.0005 in both specifications, $p < 0.05$ under Conley inference and at the margin under the leverage-adjusted estimator of Appendix B.1), and the Radical Right estimate is similar in magnitude (0.0006 vs. 0.0004) and remains significant at

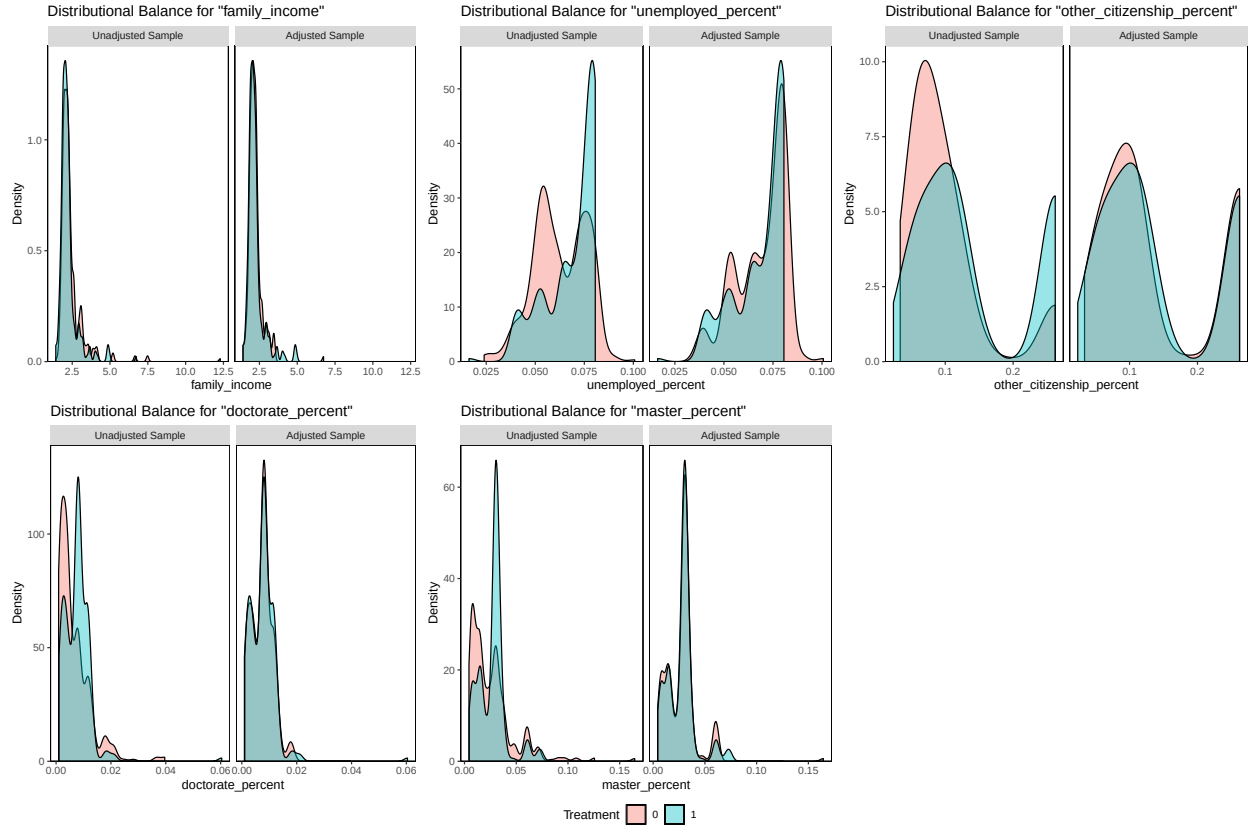


Figure C5: Distributional balance of model covariates.

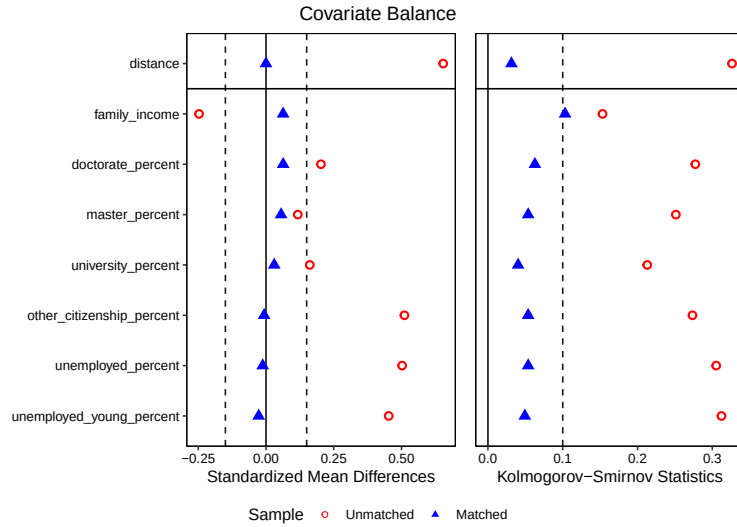


Figure C6: Standardized mean differences and Kolmogorov-Smirnov tests for model covariates.

the 1% level despite the smaller matched sample ($N = 314$). The Turnout coefficient remains statistically indistinguishable from zero in both specifications. The Incumbent coefficient, which does not reach significance in the full sample ($p = 0.081$), does so in the matched one

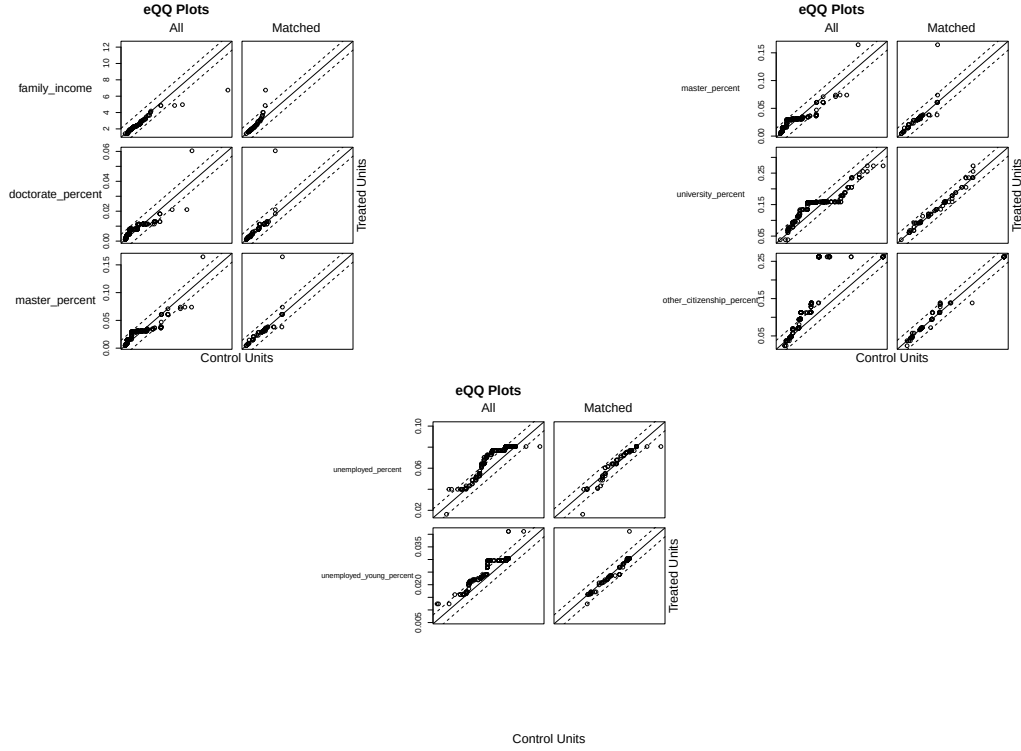


Figure C7: QQ plots of treated and control sample covariates.

(0.0017, $p = 0.009$). Since the matched sample is both smaller and drawn with replacement, and since the effect is absent from every full-sample specification, we read this as a feature of the matched design rather than as evidence of incumbent gains.

At the individual-party level (panel b), seven of the nine party-specific estimates are significant in the full sample. Six retain significance at $p < 0.01$ in the matched sample (KKE, SYRIZA, DIMAR, PASOK, ANEL, and ND). In each case, both the direction and approximate magnitude of the coefficient are preserved. The only result that does not survive matching is the small negative effect on ANTARSYA, whose coefficient attenuates from -0.0002 ($p < 0.05$) to -0.0001 ($p = 0.12$). Given that ANTARSYA is the smallest party in our sample, this attenuation is not unexpected. LAOS and Golden Dawn remain insignificant in both specifications. Overall, the matched sample results reinforce the core findings from the full sample, with wider confidence intervals attributable to the reduction in sample size rather than to any systematic change in the point estimates. Tables C7–C10 report the full regression output underlying Figure C8.

Table C7: Effect of political violence incidents on vote share of Incumbent, Radical Right & Extremist Parties: matched sample. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>		
	Incumbent	Radical Right	Extremist
	(1)	(2)	(3)
Incidents of violence between elections	0.0017** (0.0007)	0.0006** (0.0001)	-0.0005* (0.0002)
family income	0.0174** (0.0037)	-0.0013 (0.0011)	0.0004 (0.0017)
PhD %	-4.5368** (1.5863)	-1.1269 (0.8178)	0.7016 (0.5349)
master %	0.8757 (0.4841)	1.1763* (0.4925)	-0.0678 (0.2153)
university education %	0.2101** (0.0808)	-0.1321 (0.1226)	-0.0635 (0.0459)
foreign citizenship %	0.0123 (0.0446)	-0.1029* (0.0436)	-0.0025 (0.0329)
unemployment %	0.0694 (0.2462)	0.2078 (0.2160)	0.0390 (0.1369)
unemployment young %	-0.1677 (0.4490)	0.1303 (0.4015)	0.0928 (0.2502)
F-statistic	18.8834	4.9386	3.9479
p-value	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes
Observations	314	314	314
Adjusted R ²	0.6956	0.3348	0.2736

Table C8: Mid-election violence and the May-to-June change in vote shares in the matched sample. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
incidents between elections	0.0003 (0.0005)	-0.0002* (0.0001)	0.0011** (0.0002)	-0.0019** (0.0004)	0.0008** (0.0001)	-0.0008** (0.0002)	0.0036** (0.0010)	0.0007** (0.0001)	-0.0001 (0.0001)	-0.0003 (0.0002)
F-statistic	4.5060	3.2806	13.0079	16.5215	9.4356	3.8832	19.5872	5.4779	3.4676	3.4310
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	314	314	314	314	314	314	314	314	314	314
Adjusted R ²	0.2699	0.1938	0.5587	0.6207	0.4707	0.2331	0.6621	0.3207	0.2064	0.2040

Table C9: Mid-election violence and the May-to-June change in vote shares in the matched sample, with socioeconomic covariates. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
dosage	-0.0001 (0.0005)	-0.0001 (0.0001)	0.0009** (0.0002)	-0.0014** (0.0004)	0.0007** (0.0002)	-0.0007** (0.0002)	0.0024** (0.0008)	0.0008** (0.0002)	-0.0001 (0.0001)	-0.0004 (0.0003)
family income	0.0057** (0.0019)	-0.0007 (0.0005)	0.0043** (0.0012)	-0.0072* (0.0036)	0.0016 (0.0014)	-0.0030** (0.0011)	0.0204** (0.0036)	-0.0021 (0.0011)	0.0009 (0.0009)	0.0011 (0.0018)
PhD %	-0.0574 (0.7194)	-0.0687 (0.3380)	-2.6435** (0.552)	6.8737** (1.3460)	-1.7200 (0.9705)	0.4635 (0.5071)	-5.0003** (1.6881)	-1.6352** (0.5589)	0.5083 (0.4569)	0.7703* (0.3664)
master %	0.1983 (0.3803)	0.0228 (0.1226)	0.3065 (0.3204)	-2.1474** (0.5713)	0.1555 (0.2712)	0.2710 (0.1859)	0.6047 (0.5403)	1.2115** (0.2987)	-0.0352 (0.2561)	-0.0905 (0.1442)
university education %	-0.0224 (0.0817)	-0.0247 (0.0195)	0.1220 (0.0729)	-0.0782 (0.1372)	0.0866 (0.0451)	-0.1091* (0.0469)	0.3192** (0.0968)	-0.1069 (0.0787)	-0.0252 (0.0615)	-0.0388 (0.0349)
foreign citizenship %	-0.0022 (0.0390)	0.0049 (0.0124)	0.0805* (0.0382)	0.0330 (0.0522)	0.0166 (0.0224)	-0.0228 (0.0250)	0.0351 (0.0418)	-0.1026* (0.0418)	-0.0002 (0.0102)	-0.0075 (0.0274)
unemployment %	0.0987 (0.1660)	-0.0206 (0.0568)	-0.1966 (0.1893)	-0.2319 (0.3277)	-0.1783 (0.1111)	0.1059 (0.1097)	-0.0364 (0.2499)	0.2746 (0.2001)	-0.0668 (0.0568)	0.0596 (0.1052)
unemployment young %	-0.9394** (0.3299)	-0.0729 (0.1146)	-1.0109** (0.3142)	0.4209 (0.3977)	-0.6037** (0.1962)	0.0789 (0.2262)	-0.2466 (0.4849)	0.4644 (0.2748)	-0.3341 (0.2152)	0.1657 (0.1897)
F-statistic	4.3656	2.7756	14.0692	15.4898	8.6011	3.6472	20.8413	5.0988	3.0823	2.8524
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	314	314	314	314	314	314	314	314	314	314
Adjusted R ²	0.3008	0.1849	0.6255	0.6493	0.4927	0.2528	0.7172	0.3437	0.2102	0.1914

Table C10: Mid-election escalation of violence and the May-to-June change in vote shares in the matched sample. Conley spatial-HAC standard errors (1 km cutoff) in parentheses for all coefficients; significance stars reflect the Conley (1 km) p -value. Robustness of the violence coefficient across HC1 and Conley 0.5/1/2 km cutoffs is reported in Table B6. ** $p < 0.01$; * $p < 0.05$.

	<i>Dependent variable:</i>									
	Turnout	Antarsya	KKE	Syriza	Dimar	Pasok	ND	Anel	Laos	GD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Low escalation incidents	0.0005 (0.0012)	0.0006 (0.0003)	0.0010 (0.0010)	-0.0016 (0.0019)	0.0006 (0.0007)	-0.0009 (0.0008)	0.0018 (0.0026)	-0.0004 (0.0005)	0.00001 (0.0004)	-0.0008 (0.0007)
High escalation incidents	-0.0002 (0.0007)	-0.0003* (0.0001)	0.0008* (0.0004)	-0.0014 (0.0008)	0.0008** (0.0003)	-0.0006 (0.0003)	0.0026* (0.0011)	-0.0011** (0.0002)	-0.0002 (0.0002)	-0.0002 (0.0004)
Family income	0.0056** (0.0019)	-0.0008 (0.0004)	0.0043** (0.0012)	-0.0071* (0.0036)	0.0016 (0.0014)	-0.0030** (0.0011)	0.0205** (0.0036)	-0.0020 (0.0011)	0.0008 (0.0009)	0.0012 (0.0018)
PhD %	-0.1975 (0.7599)	-0.2333 (0.3703)	-2.6707** (0.8782)	6.9192** (1.4159)	-1.6815 (0.9846)	0.5279 (0.5400)	-4.8662** (1.8260)	-1.3633* (0.5837)	0.4720 (0.4503)	0.8632* (0.3843)
Master %	0.2137 (0.3792)	0.0409 (0.1244)	0.3095 (0.3201)	-2.1524** (0.5715)	0.1513 (0.2725)	0.2639 (0.1874)	0.5899 (0.5459)	1.1816** (0.2941)	-0.0312 (0.2554)	-0.1008 (0.1448)
University education %	-0.0205 (0.0820)	-0.0224 (0.0188)	0.1223 (0.0732)	-0.0788 (0.1378)	0.0860 (0.0453)	-0.1100* (0.0469)	0.3173** (0.0967)	-0.1106 (0.0773)	-0.0247 (0.0618)	-0.0401 (0.0350)
Foreign citizenship %	0.0001 (0.0385)	0.0077 (0.0123)	0.0810* (0.0386)	0.0322 (0.0531)	0.0159 (0.0226)	-0.0239 (0.0248)	0.0329 (0.0433)	-0.1072* (0.0427)	0.0004 (0.0103)	-0.0090 (0.0272)
Unemployment %	0.0857 (0.1673)	-0.0358 (0.0557)	-0.1991 (0.1919)	-0.2277 (0.3321)	-0.1748 (0.1118)	0.1118 (0.1104)	-0.0240 (0.2578)	0.2997 (0.2001)	-0.0701 (0.0573)	0.0682 (0.1058)
Youth unemployment %	-0.9180** (0.3348)	-0.0478 (0.1167)	-1.0067** (0.3159)	0.4140 (0.4087)	-0.6096** (0.1996)	0.0691 (0.2275)	-0.2671 (0.5009)	0.4230 (0.2802)	-0.3286 (0.2161)	0.1515 (0.1899)
F-statistic	4.2572	2.9233	13.6780	15.0581	8.3640	3.5525	20.2679	5.0692	3.0018	2.7846
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	314	314	314	314	314	314	314	314	314	314
Adjusted R ²	0.2991	0.2012	0.6242	0.6481	0.4910	0.2506	0.7162	0.3477	0.2077	0.1895

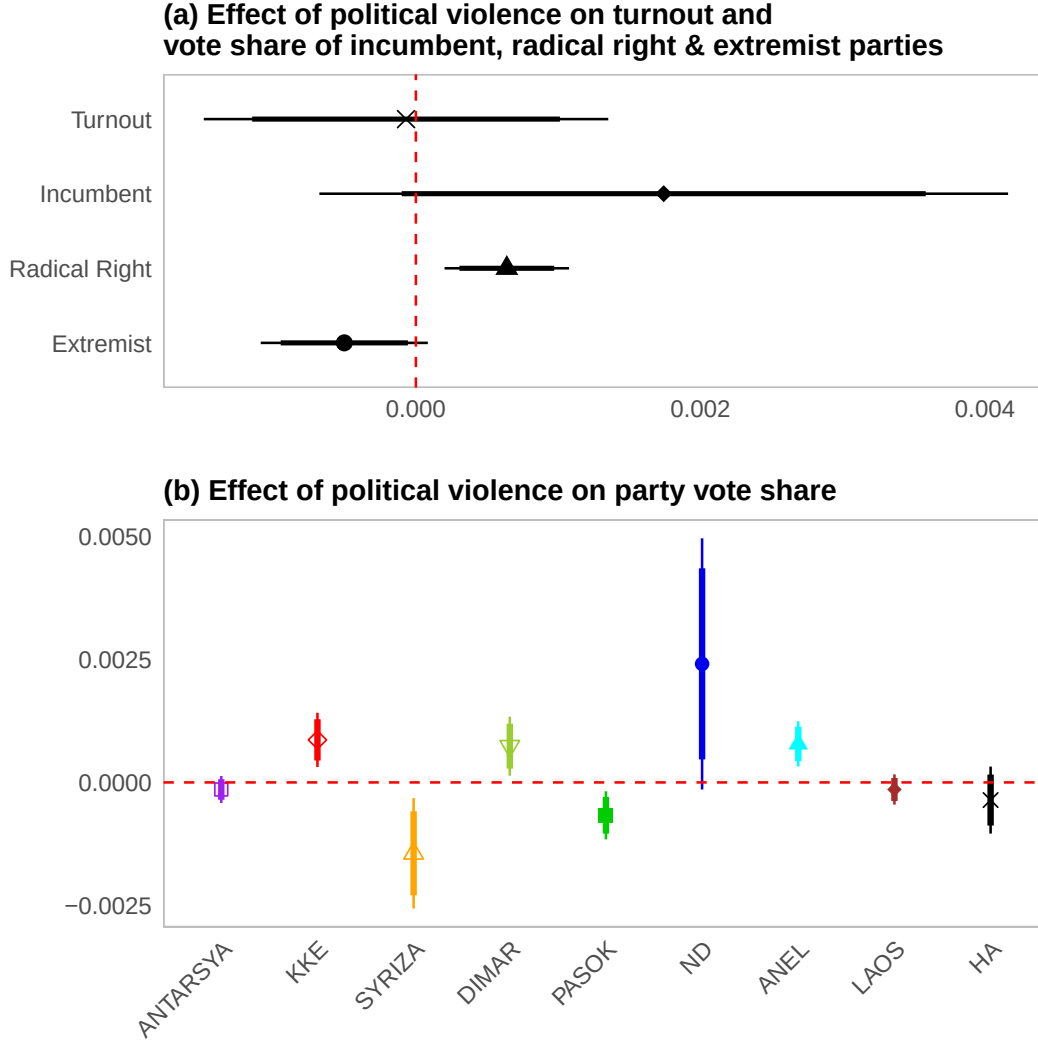


Figure C8: Effect of mid-election violence on turnout and vote shares using a matched sample. Panel (a) displays estimated coefficients for turnout, incumbent, radical right, and extremist party vote shares. Panel (b) displays party-specific estimates, ordered from the extreme left to the extreme right. Thick lines indicate 95% confidence intervals; thin lines indicate 99% confidence intervals, from Conley spatial-HAC (1 km) standard errors. Control variable estimates are suppressed for clarity.

C.6 Changes in policing between the two elections

One factor that could possibly affect incidents of political violence in the interval between the two elections could be a government response to acts of violence, with the aim of reducing violence and alleviating voters' concerns about law and order issues. Formally controlling for changes in policing would require data on police force deployment per day, which is not disclosed by the Greek police. Fortunately, in our setup, such concerns are of rather small relevance for the following reasons. Firstly, in the interval between the May 2012 and June 2012 elections, a temporary interim government came to office until a new government could be sworn in. The interim government had of course no interest in policies that could affect

the electoral outcome and exercised power in a business-as-usual manner. To further ensure that no extreme changes in policing took place in that interval, Figure C9 plots the monthly ratio of arrests to political violence incidents since 2008 with a linear fit and 95% confidence intervals, and the shaded band marks the interval between the two elections. It is reasonable to expect that any significant changes in policing would be reflected in the probability that actors of violence get arrested and hence in the ratio of arrests to incidents. The ratio shows no trend over the period and no discontinuity at the first election. It is 0.32 arrests per incident in April 2012, 0.33 in May and 0.43 in the first 17 days of June. Over the interval itself it is 0.36 (56 arrests for 157 incidents), against a median of 0.29 across the 54 months since 2008. The ratio was higher in the first three months of 2012 and fell in April, that is, before the first election.

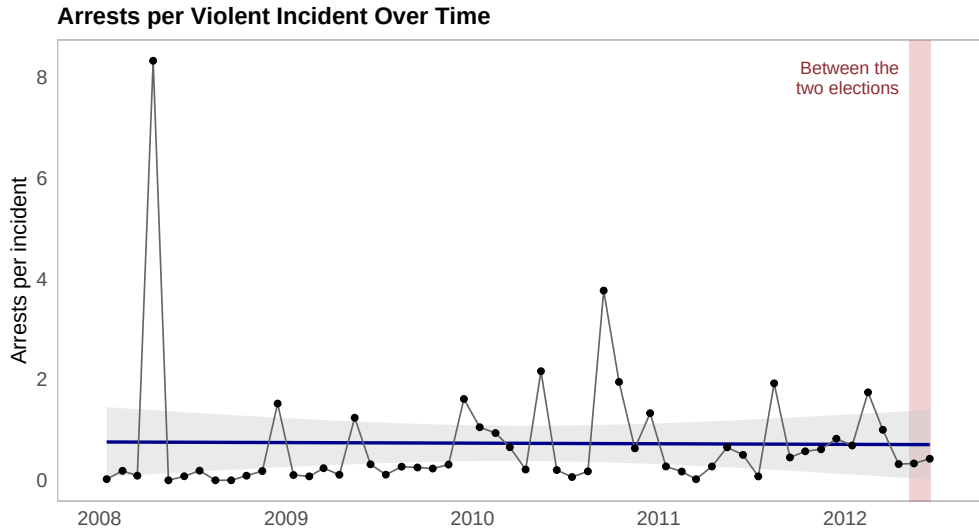


Figure C9: Monthly arrests per incident of political violence, January 2008 to June 2012. Each point is the ratio of arrests to recorded incidents in a month; the last point covers 1–17 June 2012. The line is a linear fit with its 95% confidence interval. The shaded band marks the interval between the elections of 6 May and 17 June 2012.

C.7 Mean reversion and the endogenous placement of violence

This section asks whether our estimates reflect mean reversion rather than a response to violence. Mean reversion is the tendency of a party that did unusually well in a voting center in May to fall back in June, and of a party that did unusually poorly to recover. The concern arises from the timing of the treatment. The violence we study occurs after the May election, so its location may itself respond to the May result. Our outcome is the change in a party's vote share between the two elections ($Y_{\text{June}} - Y_{\text{May}}$), which means that the May share enters it with a negative sign. If violence concentrated in centers where a party did unusually well or unusually poorly in May, dosage would correlate with the vote change even in the absence of a causal effect.

We examine this concern in two steps. The first step asks whether violence occurred where particular parties had been strong or weak in May. Table C11 reports regressions of

dosage, the number of incidents within 750 m of a voting center, on each party's May 2012 vote share. We run them for all incidents and separately for far-left and far-right incidents. Violence is not randomly placed. Dosage is higher where ND was stronger in May ($p = 0.048$) and lower where ANEL was stronger ($p = 0.044$). The association for PASOK is positive but falls short of the 5% level ($p = 0.052$). For the two extremist parties, however, the May share does not predict where violence occurred (Golden Dawn $p = 0.32$; ANTARSYA $p = 0.11$). Far-right incidents are also no more frequent where Golden Dawn was strong ($p = 0.24$).

The second step controls for the May result directly. Table C12 adds the outcome's own May 2012 vote share (Y_{May}) to the main specification. With this control, often called a partial-adjustment specification, the dosage coefficient compares centers where the party started from the same level of support. The extremist family penalty holds. Its coefficient grows by 7 percent and its p -value falls from 0.025 to 0.004. Golden Dawn's individual estimate, which is not significant at baseline, becomes significant under Conley inference (p falls from 0.199 to 0.044), though only at the margin under the leverage-adjusted estimator of Appendix B.1.

The radical right estimate does not hold. Its coefficient changes sign and its p -value rises from 0.003 to 0.575. ANEL, which accounts for the family's gain, loses 93 percent of its coefficient ($p = 0.714$). The first step helps explain why. Violence occurred where ANEL had been weaker in May, and those are the centers where ANEL gained the most in June (the coefficient on its May share is -0.46). Once the May share is held constant, little of the association with violence remains. The ND and incumbent estimates weaken as well, by 47 and 81 percent ($p = 0.062$ and $p = 0.686$). ANTARSYA's individual estimate is not robust to this control either (p rises from 0.013 to 0.244), which is one reason we report the extremist penalty for the family as a whole.

Taken together, the two steps indicate that the extremist penalty is not a mean reversion artifact, while the radical right gain largely is.

Table C11: Does violence locate in particular May 2012 strongholds? Each cell reports the coefficient from a regression of violence dosage (pooled, far-left only, far-right only) on the row party's May 2012 vote share, with municipality fixed effects. Conley spatial-HAC (1 km) p -values. ** $p < 0.01$; * $p < 0.05$.

May 2012 vote share	Pooled dosage		FL dosage		FR dosage	
	β	p	β	p	β	p
ND	11.14*	0.048	10.36	0.067	0.78	0.278
PASOK	22.56	0.052	22.62*	0.050	-0.05	0.967
SYRIZA	-11.73	0.086	-10.15	0.137	-1.57	0.075
KKE	-15.84	0.060	-16.12	0.053	0.27	0.801
Golden Dawn	-8.62	0.319	-10.21	0.228	1.60	0.238
ANTARSYA	73.37	0.110	72.79	0.105	0.58	0.867
ANEL	-31.51*	0.044	-29.13	0.061	-2.38	0.112
LAOS	-0.27	0.988	-6.29	0.719	6.02	0.051

Table C12: Partial-adjustment check: adding the May 2012 outcome level as a control. Dosage coefficients before and after adding Y_{May} , the outcome's own May 2012 vote share, to the main specification. The last column gives the change in the coefficient as a percentage of the absolute value of the baseline coefficient, so a negative entry means that the coefficient moved downward. Conley spatial-HAC (1 km) p -values, with significance stars based on them. ** $p < 0.01$; * $p < 0.05$.

Outcome	Baseline		Adding Y_{May}		$\Delta\beta$ (%)
	β	p	β	p	
Extremist	-0.0005*	0.025	-0.0006**	0.004	-7
Radical Right	0.0004**	0.003	-0.0001	0.575	-116
Incumbent	0.0010	0.081	0.0002	0.686	-81
ND	0.0017*	0.013	0.0009	0.062	-47
ANTARSYA	-0.0002*	0.013	0.0000	0.244	+123
Golden Dawn	-0.0003	0.199	-0.0004*	0.044	-26
ANEL	0.0006**	<0.001	0.0000	0.714	-93

D Placebo and falsification tests

This section examines the main assumption behind our design. We compare the change in vote shares between May and June 2012 across voting centers with more and less exposure to violence, and we interpret the difference as a response to violence. This interpretation requires that the more exposed centers would have followed the same electoral trend as the less exposed ones had the violence not occurred. The concern is that violence tends to recur in the same neighborhoods, and these neighborhoods may have been shifting politically for reasons unrelated to the incidents we study. Because we observe only two elections, we cannot test the assumption directly. We instead report three checks that look for signs of such pre-existing differences.

The first check uses violence from other periods (Appendix D.1). We replace the treatment with the incidents recorded in 2009, 2010, 2011, the period before the May 2012 election, and January 2015, and ask whether they predict the vote change between May and June 2012. This violence took place outside the interval between the two elections, so an association would suggest that violence-prone areas were already on a different path. These placebo tests cover many outcomes and windows, and some will be significant by chance. Appendix D.2 therefore corrects them for multiple testing. The extremist family and its two parties show no significant placebo in any window. Several other parties do, and after the correction their failures become more frequent as the window approaches the treatment period (none in 2009, two each in 2010 and 2011, and five in the May 2012 window).

The second check takes up the failures in the window closest to the treatment (Appendix D.3). A significant placebo in that window can be read in two ways. Violence before May 2012 occurred largely in the same places as violence between the two elections ($r = 0.72$), so the placebo may simply stand in for the treatment. Alternatively, the exposed areas may have been on a different electoral trend. To distinguish the two readings to the extent possible, we add the closest pre-treatment window to the main model as a control. The extremist penalty holds under this control ($p < 0.001$). The radical right, SYRIZA, and ND estimates lose significance, and we treat them as exposed to the risk of differential trends.

The third check uses two outcomes that violence should not affect, the shares of invalid and blank ballots (Appendix D.4). Invalid ballots show no association with violence ($p = 0.37$). Blank ballots show a small positive association ($p = 0.034$) that does not survive a correction across the two outcomes.

Overall, the checks give no sign of a pre-existing trend behind the extremist penalty. The same cannot be said of the radical right gain, which is one reason the main text treats it as the less robust of the two findings.

D.1 Temporal placebo tests

We regress the same 2012 May-to-June vote share changes (Δ outcomes) on violence dosage from periods other than the treatment window. If past violence predicts the 2012 outcomes, this would suggest that violence-prone areas have systematically different electoral trajectories, threatening identification. We construct five placebo windows: violence from 2009, 2010, 2011, the May 2012 pre-election period, and a post-treatment window using January 2015 violence. In each window, we run 10 regressions (turnout plus 9 parties) with the same

specification as our main analysis (covariates, municipality fixed effects, and Conley (1 km) standard errors).

At the party family level, placebo tests are largely reassuring. Of the three family-level tests in the May 2012 pre-treatment window, only Radical Right approaches significance ($p = 0.054$; $p = 0.033$ at the 0.5 km cutoff), while the Incumbent ($p = 0.13$) and Extremist ($p = 0.97$) coefficients are insignificant. The Extremist family shows no significant placebo in any window.

D.2 Multiple testing corrections for the placebo tests

Running 10 tests per window (5 windows $\times$ 10 outcomes = 50 tests total) raises multiple testing concerns. We apply Benjamini-Hochberg (BH) corrections to control the false discovery rate. Table D1 reports within-window corrections (correcting across 10 outcomes per window), which is the natural framing since each window tests a distinct hypothesis.

Table D1: Placebo test summary: within-window multiple testing corrections. Each window tests whether violence from that period predicts 2012 May–June Δ outcomes. Ten tests per window (turnout + 9 parties), based on Conley (1 km) p -values. BH controls the false discovery rate.

Window	Raw sig.	BH sig.	Outcomes surviving BH
2009	2	0	—
2010	3	2	PASOK, Turnout
2011	3	2	PASOK, KKE
May 2012	5	5	PASOK, ND, KKE, SYRIZA, ANEL
2015	2	2	KKE, SYRIZA

Two patterns emerge from these results. First, there is a broad temporal gradient. The most temporally distant window (2009) is perfectly clean under any correction, while windows closer to the treatment period show increasing placebo leakage. Under BH correction, the number of significant failures rises from 0 (2009) to 2 (2010 and 2011) to 5 (May 2012). The 2015 post-treatment window shows 2 BH-significant failures, consistent with the same spatial persistence mechanism operating after the study period. Figure D1 illustrates this pattern for the three party families, plotting dosage coefficients and 95% confidence intervals across placebo windows alongside the main treatment estimate. This is consistent with spatial persistence of violence. The same neighborhoods experience violence year after year, creating mechanical correlation between placebo dosage and treatment dosage. If the pattern instead reflected a fundamental identification failure, with violence-prone areas exhibiting genuinely different electoral trends independent of violence exposure, the 2009 window should be as problematic as 2011; it is not.

Second, there is a clear partition of parties by placebo performance. Table D2 summarizes individual-party results across all five windows.

The parties divide into three groups. First, DIMAR, Antarsya, Golden Dawn, and LAOS have perfectly clean placebo profiles, with none reaching significance in any window under BH correction. The Extremist family (Golden Dawn + Antarsya) is on firm ground, with both components individually clean and the combined family effect significant at $p = 0.025$. DIMAR combines a clean profile with a highly significant main effect, reinforcing confidence

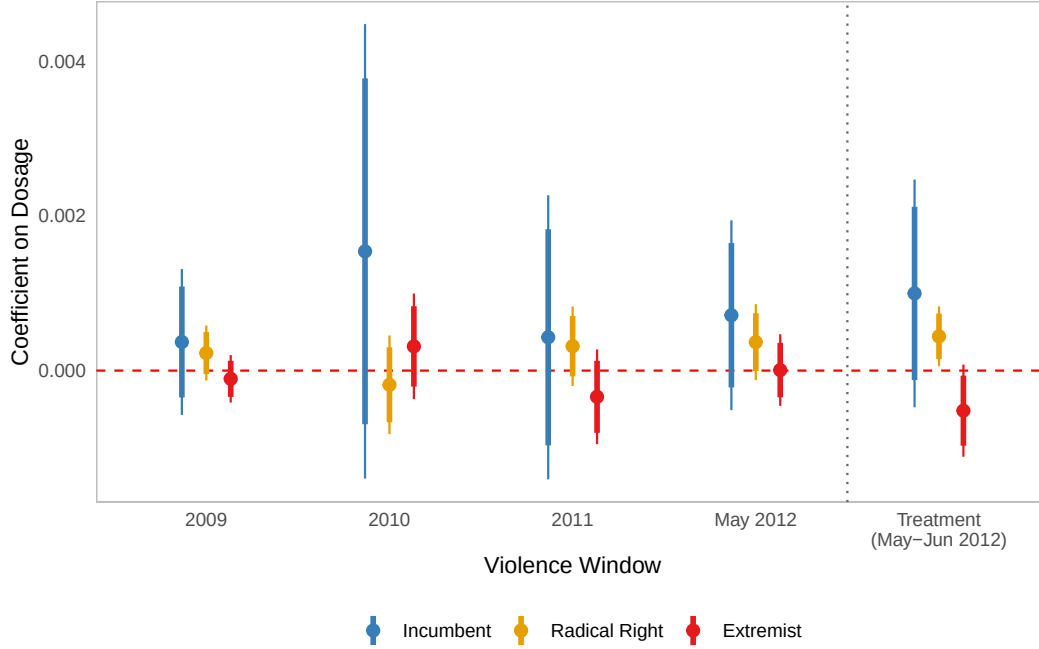


Figure D1: Placebo temporal gradient for party families. Each point shows the estimated coefficient of placebo violence dosage on the June–May 2012 vote share difference, with 95% (thick) and 99% (thin) confidence intervals from Conley spatial-HAC (1 km) standard errors. The dotted line separates placebo windows (left) from the main treatment estimate (right). Placebo coefficients are centered near zero and statistically insignificant for all three families, while the treatment window shows the expected divergence.

Table D2: Individual-party placebo profiles across five placebo windows. “*” indicates $p < 0.05$ after BH correction within each window; “—” indicates not significant. All p -values are computed from Conley spatial-HAC (1 km) standard errors. The rightmost column reports the main effect p -value from the baseline specification for reference.

Party	2009	2010	2011	May 2012	2015	Main effect p
PASOK	—	*	*	*	—	<0.001
ND	—	—	—	*	—	0.013
KKE	—	—	*	*	*	<0.001
SYRIZA	—	—	—	*	*	0.002
Golden Dawn	—	—	—	—	—	0.199
Antarsya	—	—	—	—	—	0.013
ANEL	—	—	—	*	—	<0.001
DIMAR	—	—	—	—	—	<0.001
LAOS	—	—	—	—	—	0.198
Turnout	—	*	—	—	—	0.686

in that estimate. Second, PASOK shows persistent placebo failures across three windows, with coefficients of similar sign and magnitude as the main effect. This suggests that part of the PASOK decline in violence-prone areas reflects pre-existing spatial patterns. Third, ND, KKE, SYRIZA, ANEL, and Turnout show intermediate placebo leakage. KKE fails in three windows, SYRIZA in two, and ND, ANEL, and Turnout in one each (ND in the closest win-

dow, May 2012; ANEL in May 2012, with the 2011 failure just above the threshold; Turnout in 2010, though its main effect is null). The ANEL placebo failure warrants particular attention given its highly significant main effect ($p < 0.001$) and its contribution to the radical right family result, and the ND failure in the closest window is consistent with the covariate test in Section D.3, where the ND estimate weakens under the closest-window control. However, these placebo coefficients are smaller in magnitude than the corresponding treatment effects, and the BH-surviving failures are concentrated in the temporally proximate windows most likely to reflect spatial persistence of violence rather than confounding.

D.3 The closest pre-treatment window as a covariate

The temporal gradient above leaves open whether near-window leakage reflects benign spatial persistence or differential local trends; in a two-period design the two are observationally equivalent. We therefore re-estimate the headline model adding the closest pre-treatment window (May 2012 placebo dosage) as a covariate. If an estimate is not separable from a differential local trend, it should collapse under this control. Table D3 reports the results for every family and party. For the extremist family the more telling evidence is that there is nothing to explain away. Violence from the closest pre-treatment window does not predict the family’s June shift at all ($p = 0.969$), nor Golden Dawn’s ($p = 0.612$), and neither reaches significance in any of the five windows examined above. Both estimates also survive the control, and in fact strengthen under it (family $p = 0.025 \rightarrow <0.001$; Golden Dawn $0.199 \rightarrow 0.009$; ANTARSYA $0.013 \rightarrow 0.012$). While that is the opposite of what a differential trend would produce, we do not rest the case on it. The treatment and placebo measures are correlated at $r = 0.72$ (0.71 once municipality fixed effects and covariates are partialled out, a variance inflation factor near 2.0), and adding a correlated regressor can inflate an estimate as readily as attenuate it. The radical right family and SYRIZA collapse (radical right $0.003 \rightarrow 0.244$; SYRIZA $0.002 \rightarrow 0.461$), and ND weakens ($0.013 \rightarrow 0.137$), so we treat their near-window leakage as a genuine risk of differential trends. KKE and DIMAR leak but remain stable, ANEL weakens but survives ($< 0.001 \rightarrow 0.041$, at the margin under the leverage-adjusted estimator), and PASOK weakens to marginal ($< 0.001 \rightarrow 0.078$).

Table D3: Headline estimates with the closest pre-treatment window as a covariate. “Placebo alone” is the p -value of the May 2012 placebo dosage regressed on the outcome; the remaining columns compare the treatment dosage coefficient before and after adding that placebo as a control. Conley spatial-HAC (1 km) p -values; significance stars reflect the Conley (1 km) p -value. ** $p < 0.01$; * $p < 0.05$.

Outcome	Placebo alone	Baseline		Adding closest placebo		$\Delta\beta$ (%)
	p	β	p	β	p	
Incumbent	0.134	0.0010	0.081	0.0009	0.247	−8
Radical Right	0.054	0.0004**	0.003	0.0003	0.244	−25
Extremist	0.969	−0.0005*	0.025	−0.0010**	<0.001	−102
Turnout	0.848	−0.0002	0.686	−0.0005	0.290	−172
ANTARSYA	0.415	−0.0002*	0.013	−0.0003*	0.012	−38
KKE	0.001	0.0008**	<0.001	0.0008*	0.010	−2
SYRIZA	0.001	−0.0010**	0.002	−0.0005	0.461	+50
DIMAR	0.292	0.0006**	<0.001	0.0009**	0.001	+45
PASOK	<0.001	−0.0007**	<0.001	−0.0005	0.078	+30
ND	0.018	0.0017*	0.013	0.0014	0.137	−17
ANEL	0.007	0.0006**	<0.001	0.0006*	0.041	−3
LAOS	0.600	−0.0002	0.198	−0.0003	0.100	−58
Golden Dawn	0.612	−0.0003	0.199	−0.0008**	0.009	−140

D.4 Falsification: invalid and blank ballots

As an additional validation, we estimate the effect of violence dosage on changes in invalid and blank ballot shares, outcomes that should not be systematically affected by political violence. Violence may shift preferences across parties but should not alter the rate of spoiled or blank ballots. Figure D2 presents the results. Invalid ballots show no association ($\beta = -0.0001$, $p = 0.37$). Blank ballots show a small positive association ($\beta = 0.0001$, $p = 0.034$) that reaches significance at the 5% level but does not survive a Bonferroni correction across the two falsification outcomes ($p = 0.068$) and corresponds to roughly 0.007 percentage points per incident. We read the invalid ballot null as reassuring; the blank ballot association, borderline under Conley inference and absent under HC1 and the leverage-adjusted estimator, is substantively negligible, though its positive sign would also be consistent with a mild protest voting response rather than mechanical noise in the voting process.

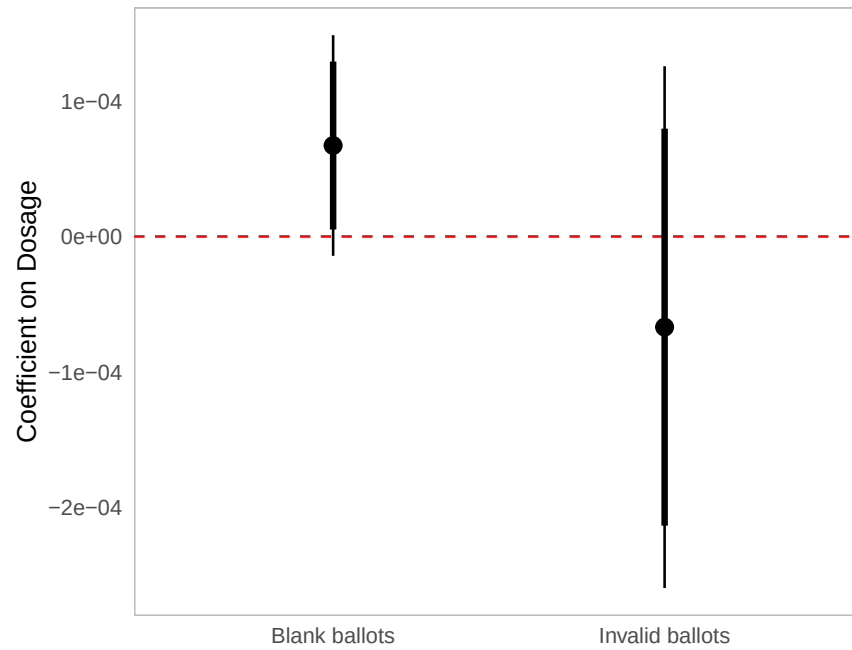


Figure D2: Falsification test: effect of violence on invalid and blank ballot shares. Coefficients and 95% (thick) and 99% (thin) confidence intervals from Conley spatial-HAC (1 km) standard errors, from regressions of Δ invalid and Δ blank ballot shares on violence dosage, with covariates and municipality fixed effects.

E Spatial robustness

Our identification strategy assumes that violence affects voters at nearby voting centers but not those further away. If violence effects spill over spatially (through social networks, local media, or word of mouth), the control group may be partially treated, which would attenuate our estimates. We assess this concern through three exercises. We first compare proximate and distant violence directly through a spatial placebo and horse-race specification, then estimate models with grid-cell fixed effects to absorb finer-grained spatial confounding, and finally decompose the treatment area into concentric rings as a complementary distance-decay check.

E.1 Spatial placebo and horse-race specification

We directly compare the effects of proximate violence (0–750m, our treatment) with distant violence (750–1000m donut ring) on the same outcomes. If only local exposure matters, proximate violence should predict vote share changes while distant violence should not. Figure E1 presents both coefficients side by side.

When distant violence is the sole predictor, 5 of 13 donut-ring coefficients are individually significant at $p < 0.05$: PASOK ($p = 0.004$), ND ($p = 0.010$), KKE ($p = 0.013$), ANEL ($p = 0.014$), and Incumbent ($p = 0.041$). This is not unexpected, since voting centers exposed to violence within 750m are also likely to have violence in the surrounding 750–1000m ring. The key question is whether these donut-ring effects reflect independent distant influence or simply correlate with proximate exposure. The horse-race specification, which includes both proximate and distant violence simultaneously, provides a direct test (Figure E2). In that specification, proximate violence retains significance for seven outcomes (Radical Right, KKE, SYRIZA, PASOK, ANEL, DIMAR, Antarsya; all $p < 0.05$ under Conley inference, six under the leverage-adjusted estimator of Appendix B.1, Antarsya being the exception), preserving seven of the nine individually significant baseline results. The Extremist family ($p = 0.065$) and ND ($p = 0.066$) fall just short of conventional significance but retain coefficients of similar sign and magnitude. By contrast, the donut-ring coefficients become insignificant for every outcome once proximate violence is included (e.g., ANEL $p = 0.086$, ND $p = 0.091$, PASOK $p = 0.13$, Extremist $p = 0.24$). This pattern is consistent with spatially concentrated effects. Once proximate violence is accounted for, distant violence adds no additional explanatory power.

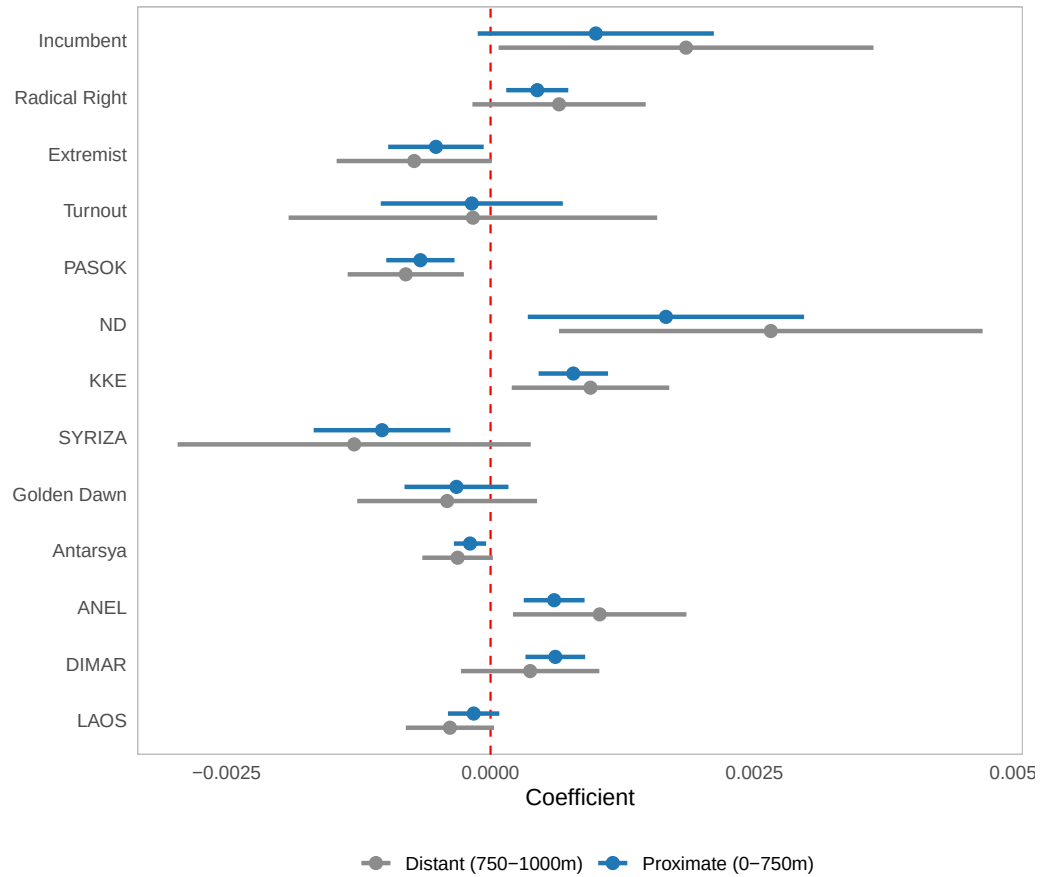


Figure E1: Spatial placebo: proximate vs. distant violence. Blue circles show coefficients on violence within 0–750m (treatment) from the main specification; gray circles show coefficients on violence within 750–1000m (placebo ring) from separate regressions. 95% confidence intervals from Conley spatial-HAC (1 km) standard errors. Models include covariates and municipality fixed effects.

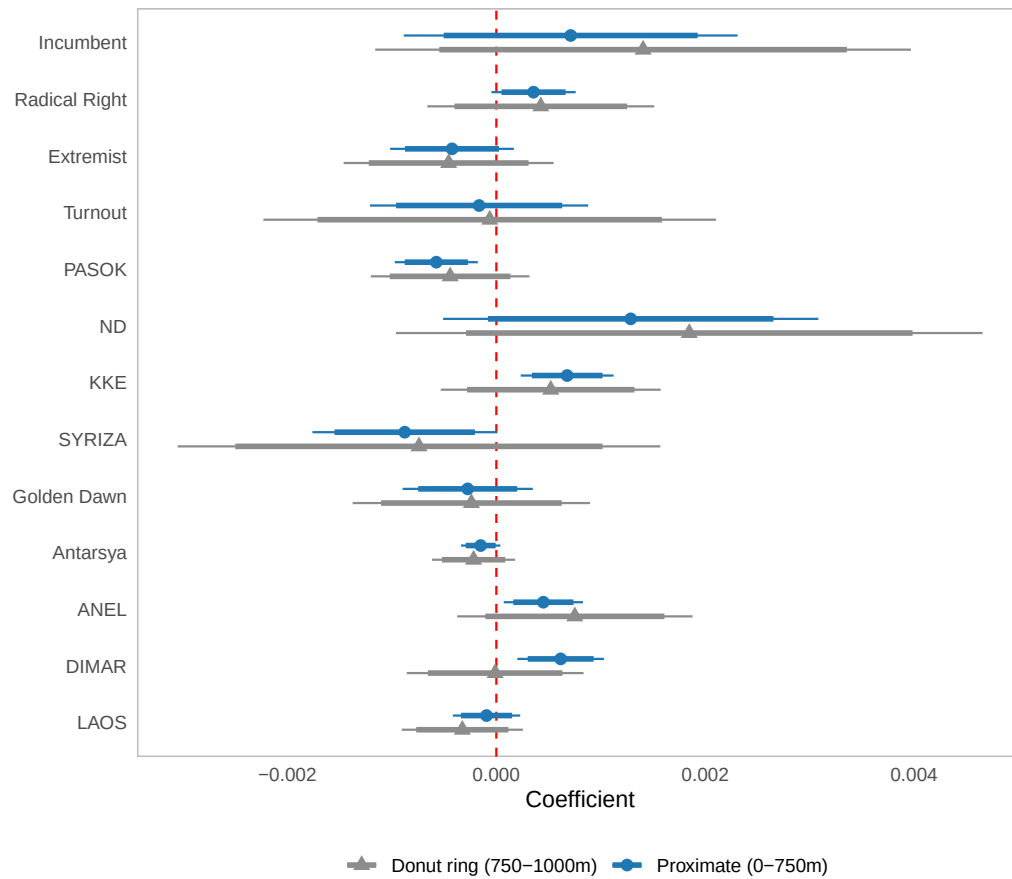


Figure E2: Horse-race specification: proximate vs. donut-ring violence. Both proximate violence (0–750m, blue circles) and donut-ring violence (750–1000m, gray triangles) are included as simultaneous regressors. Thick bars show 95% confidence intervals; thin bars show 99% confidence intervals, from Conley spatial-HAC (1 km) standard errors. Models include covariates and municipality fixed effects.

E.2 Grid-cell fixed effects

To absorb finer-grained spatial confounding beyond what municipality fixed effects capture, we estimate models with grid-cell fixed effects that partition the study area into $1\text{km} \times 1\text{km}$ cells, so that the effect is identified solely by comparing voting centers within the same small geographic area that differ in their proximate exposure to violence. This is a demanding specification that discards all between-neighborhood comparisons, relying exclusively on within-cell variation in violence exposure. Figure E3 maps the resulting grid partition for Athens and Thessaloniki, classifying each cell by whether it contains both treated and control voting centers. Of the 235 grid cells across both cities, only 43 (18%) contain a mix of treated and control precincts and thus contribute identifying variation. The remaining cells are absorbed entirely by the fixed effects, substantially reducing statistical power relative to the baseline municipality FE specification.

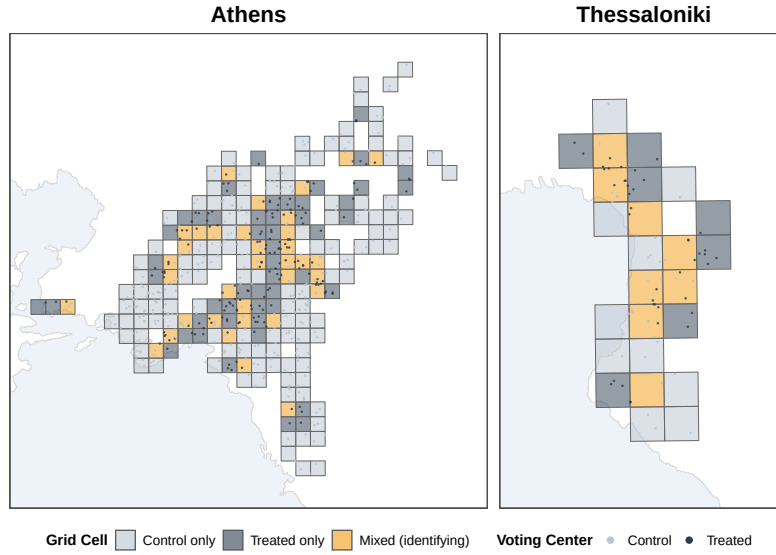


Figure E3: Grid-cell identifying variation ($1\text{km} \times 1\text{km}$). Each square represents a grid cell used as a fixed effect in the grid-cell FE specification. Cells are classified by whether they contain only control precincts (light), only treated precincts (dark), or a mix of both (amber). Only mixed cells contribute identifying variation to the estimates. Dots show individual voting center locations, coloured by treatment status.

The Extremist family effect survives and strengthens under this specification ($\beta = -0.0014$, $p < 0.001$), with a coefficient nearly three times larger in magnitude than the baseline municipality FE estimate ($\beta = -0.0005$). Golden Dawn, which does not reach individual significance in the baseline ($p = 0.199$), becomes significant ($\beta = -0.0011$, $p = 0.014$), DIMAR retains significance ($\beta = 0.0009$, $p = 0.005$), and Antarsya retains significance as well ($\beta = -0.0003$, $p = 0.021$). These party-level verdicts are under Conley inference. Leverage is extreme in the grid-cell specification, and none of the three holds under the leverage-adjusted estimator of Appendix B.1, so we treat them as uncertain; the extremist family holds under both. The larger Extremist coefficient is consistent with attenuation bias in the municipality FE specification, though it may also reflect differences in the composition of identifying variation across the two models.

However, most other results do not survive. The Radical Right family, significant at the 1% level in the baseline ($p = 0.003$), falls to $p = 0.077$. Among the individual parties significant in the baseline, PASOK ($p = 0.53$), KKE ($p = 0.41$), and SYRIZA ($p = 0.57$) lose significance, and ANEL is borderline ($p = 0.053$). This suggests that the extremist backlash finding is identifiable even from within-neighborhood variation alone, while the radical right consolidation and most individual party effects depend on between-neighborhood comparisons that grid-cell fixed effects absorb. Given the substantial reduction in identifying variation, the loss of significance for these outcomes does not necessarily indicate confounding in the baseline specification, but it does indicate that the extremist backlash is the more robust of the paper’s two core findings under this demanding test.

E.3 Distance-decay gradient

As a complementary check, we decompose the treatment area into four concentric rings (0–250m, 250–500m, 500–750m, 750–1000m) and estimate separate coefficients for violence counts in each ring. While the ring decomposition substantially reduces statistical power and leaves most individual coefficients imprecisely estimated, four reach conventional significance. KKE at 0–250m ($\hat{\beta} = 0.0016$, $p = 0.002$) provides the clearest evidence of a proximity gradient, with the innermost ring carrying the largest coefficient and the outer rings positive but smaller and insignificant. The Extremist family ($\hat{\beta} = -0.0008$, $p = 0.004$), Antarsya ($\hat{\beta} = -0.0004$, $p = 0.034$), and ANEL ($\hat{\beta} = 0.0006$, $p = 0.050$), all at 250–500m, are likewise significant under Conley inference, though not under the leverage-adjusted estimator; the Extremist family’s estimate for that ring holds under both. For the remaining outcomes, point estimates are broadly consistent with the main results in sign and relative magnitude, though wide confidence intervals preclude firm conclusions about spatial gradients from this exercise alone (Figure E4). The horse-race and grid-cell specifications above offer more informative tests by preserving the full sample’s identifying variation rather than partitioning it into narrow rings.

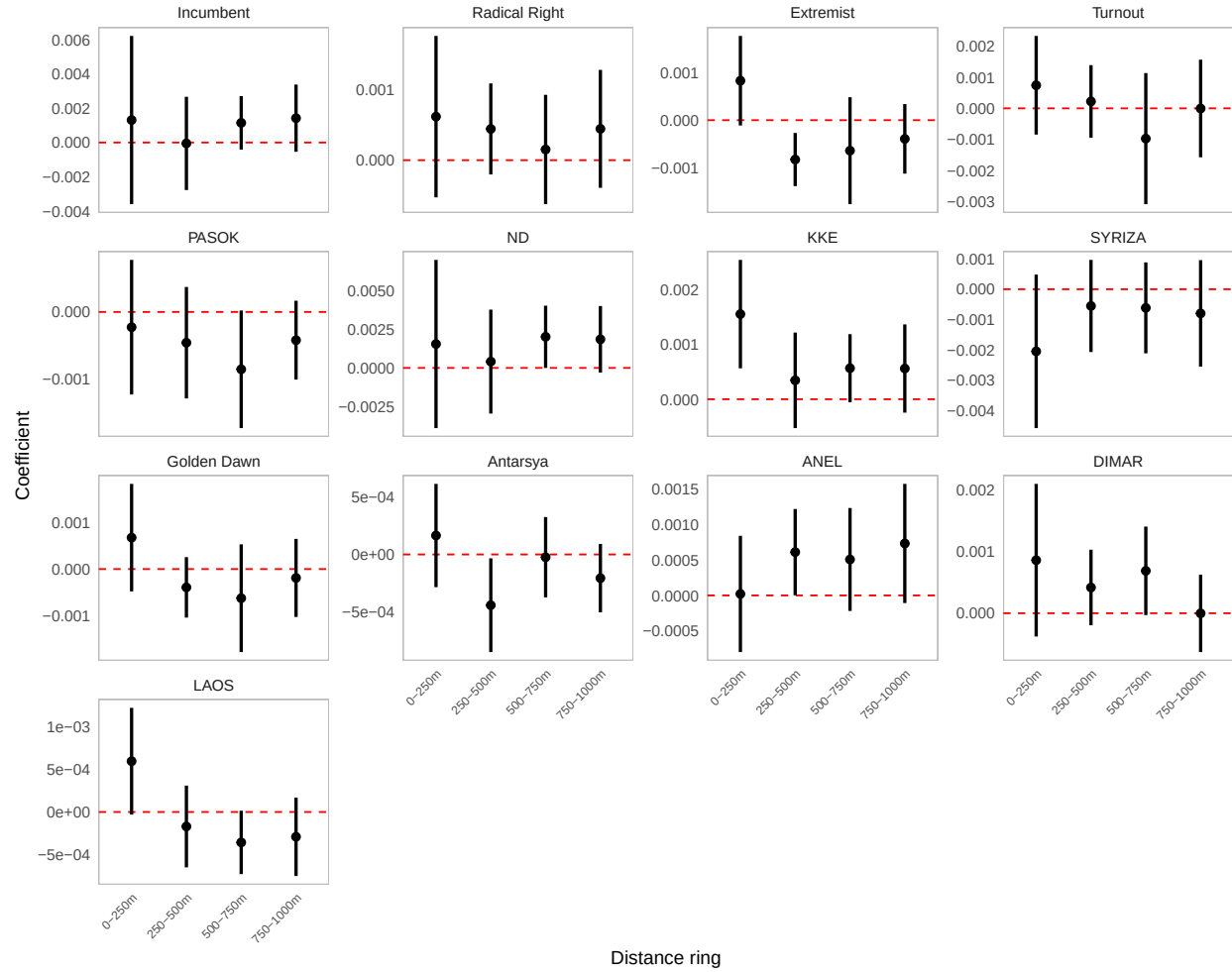


Figure E4: Distance-decay gradient. Coefficients on violence counts in four concentric rings (0–250m, 250–500m, 500–750m, 750–1000m) around each voting center, with 95% confidence intervals from Conley spatial-HAC (1km) standard errors. Each panel is a separate outcome. Models include covariates and municipality fixed effects.

F Sensitivity analysis

Even after controlling for observable covariates and municipality fixed effects, an unobserved confounder correlated with both violence exposure and electoral outcomes could bias our estimates. We assess how strong such a confounder would need to be using the framework of Cinelli and Hazlett (2020). The key quantity is the robustness value ($RV_{q=1}$), which measures the minimum strength of association (in partial R^2 terms) that a hypothetical omitted confounder would need to have with both the treatment and the outcome to reduce the estimated coefficient to zero. Higher values indicate greater robustness. For example, an $RV_{q=1}$ of 0.10 would mean that a hypothetical omitted variable would need to explain at least 10% of the remaining variation in both treatment assignment and the outcome, after accounting for all observed covariates and municipality fixed effects, to fully eliminate the estimated effect.

Table F1: Sensitivity analysis summary (Cinelli and Hazlett, 2020). $RV_{q=1}$: minimum partial R^2 a confounder needs with both treatment and outcome to reduce the point estimate to zero. $RV_{q=1,\alpha}$: to reduce the estimate to non-significance at 5%. $R^2_{Y \sim D|X}$: partial R^2 of treatment with outcome after partialling out controls. Higher RV values indicate more robust estimates. The point-estimate quantities ($RV_{q=1}$ and $R^2_{Y \sim D|X}$) are geometric properties of the OLS fit and do not depend on the standard error estimator; the reported standard errors and the significance-based $RV_{q=1,\alpha}$ are computed from Conley spatial-HAC standard errors (1 km cutoff), consistent with the primary inference used throughout the paper.

Outcome	Estimate	SE	$RV_{q=1}$	$RV_{q=1,\alpha}$	$R^2_{Y \sim D X}$
Incumbent	0.0010	0.0006	0.081	0.000	0.007
Radical Right	0.0004	0.0001	0.079	0.041	0.007
Extremist	-0.0005	0.0002	0.113	0.012	0.014
Turnout	-0.0002	0.0004	0.026	0.000	0.001
PASOK	-0.0007	0.0002	0.163	0.084	0.031
ND	0.0017	0.0007	0.125	0.022	0.018
KKE	0.0008	0.0002	0.137	0.108	0.021
SYRIZA	-0.0010	0.0003	0.103	0.047	0.012
Golden Dawn	-0.0003	0.0003	0.076	0.000	0.006
Antarsya	-0.0002	0.0001	0.109	0.022	0.013
ANEL	0.0006	0.0001	0.115	0.087	0.015
DIMAR	0.0006	0.0001	0.131	0.093	0.019
LAOS	-0.0002	0.0001	0.060	0.000	0.004

The Extremist family has $RV_{q=1} = 0.113$, meaning that a confounder would need to explain at least 11.3% of the residual variance in both dosage and the extremist vote share change (after partialling out covariates and municipality fixed effects) to fully account for the observed negative effect. The Radical Right family has $RV_{q=1} = 0.079$ and $RV_{q=1,\alpha} = 0.041$, indicating that an omitted confounder would need to explain roughly 4% of the residual variation in both treatment and outcome to reduce the estimate to non-significance under Conley inference. We stress, however, that this exercise addresses omitted-variable bias only. It does not speak to the mean reversion and Athens-leverage checks (Sections C.7 and I) under which the radical right estimate proves far less robust than the extremist one. Among

individual parties, Antarsya ($RV = 0.109$), ANEL ($RV = 0.115$), and DIMAR ($RV = 0.131$) have robustness values above 10%. PASOK has the highest robustness value ($RV = 0.163$), but this is tempered by its persistent placebo failures. The $RV_{q=1,\alpha}$ values for Incumbent, Turnout, Golden Dawn, and LAOS are zero, meaning their statistical significance is sensitive to even modest confounding. These are precisely the outcomes that do not reach significance at the 5% level under our primary Conley inference.

Figures F1 and F2 display sensitivity contour plots for the party families and individual parties. Each contour line shows the adjusted estimate for a given combination of confounder–treatment and confounder–outcome partial R^2 values. The red dashed line indicates the threshold at which the estimate would change sign. Diamond markers show benchmarks calibrated to the principal socioeconomic covariates in our specification (family income, unemployment rate).

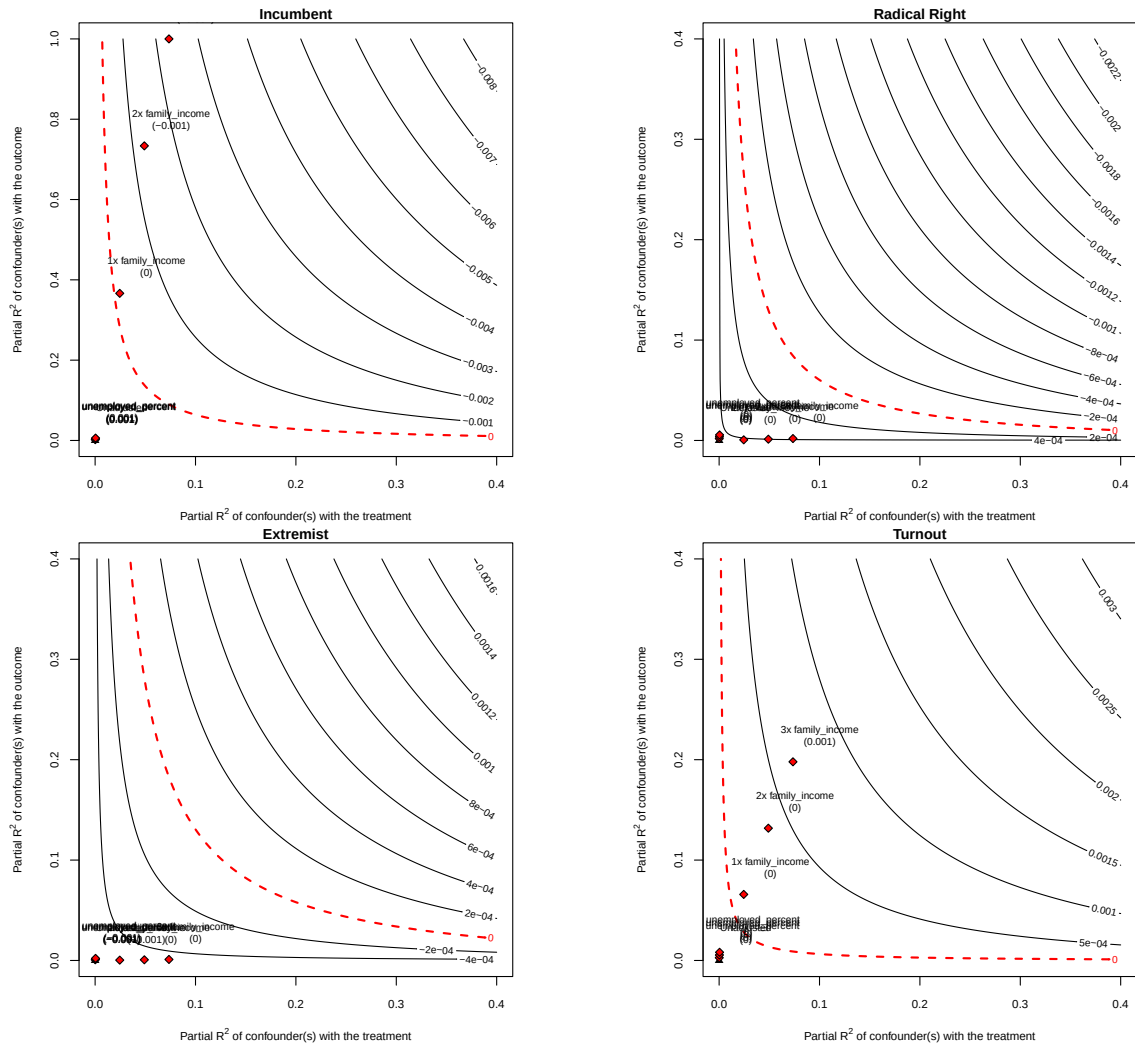


Figure F1: Sensitivity contour plots: party families and turnout. Contour lines show adjusted point estimates for hypothetical confounders with given partial R^2 values with the treatment (x-axis) and outcome (y-axis). Red dashed line: threshold where the estimate changes sign. Diamond markers: benchmarks based on observed covariates. Source: **sensemakr** (Cinelli and Hazlett, 2020).

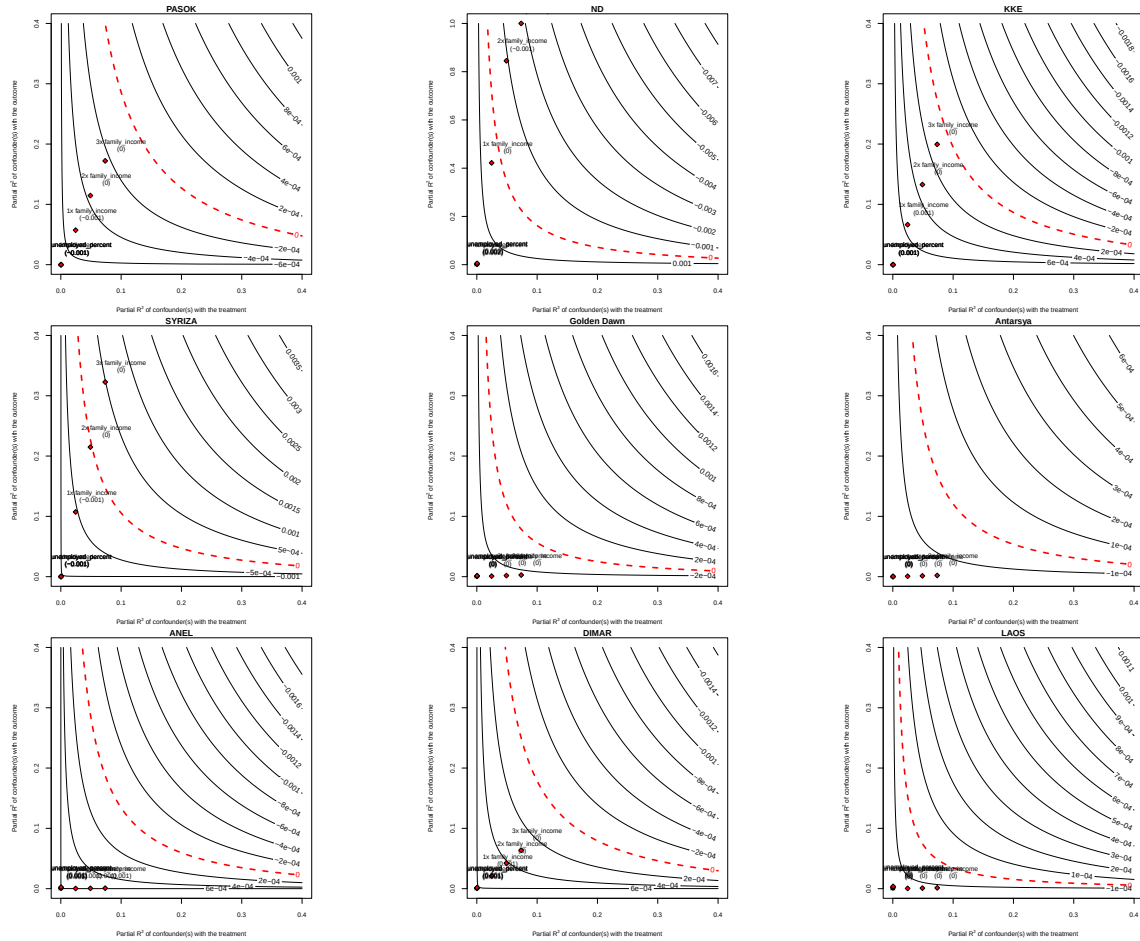


Figure F2: Sensitivity contour plots: individual parties. See notes to Figure F1.

G Dose-response analysis

Our main specification assumes that each additional incident of violence produces the same marginal effect on electoral outcomes. If the true relationship is nonlinear, with effects that diminish at higher dosage levels or accelerate beyond a threshold, the linear model could misrepresent the underlying pattern. We relax this assumption in two ways, by estimating categorical bin indicators and by including a quadratic term.

G.1 Categorical bins

We group voting centers into five dosage categories: 0 (reference), 1, 2–3, 4–5, and 6+ incidents ($n = 14$ and 8 in the two highest bins). Figure G1 plots the mean Δ vote share for each party family by dosage bin. The pattern is broadly monotonic for our core findings. The Incumbent vote share increases with dosage intensity, while the Extremist vote share decreases. The steepest changes occur between the 2–3 and 4–5 incident bins, suggesting that the electoral response to violence intensifies at higher dosage levels. The Radical Right pattern is non-monotonic at lower doses but shows a positive trajectory at higher levels.

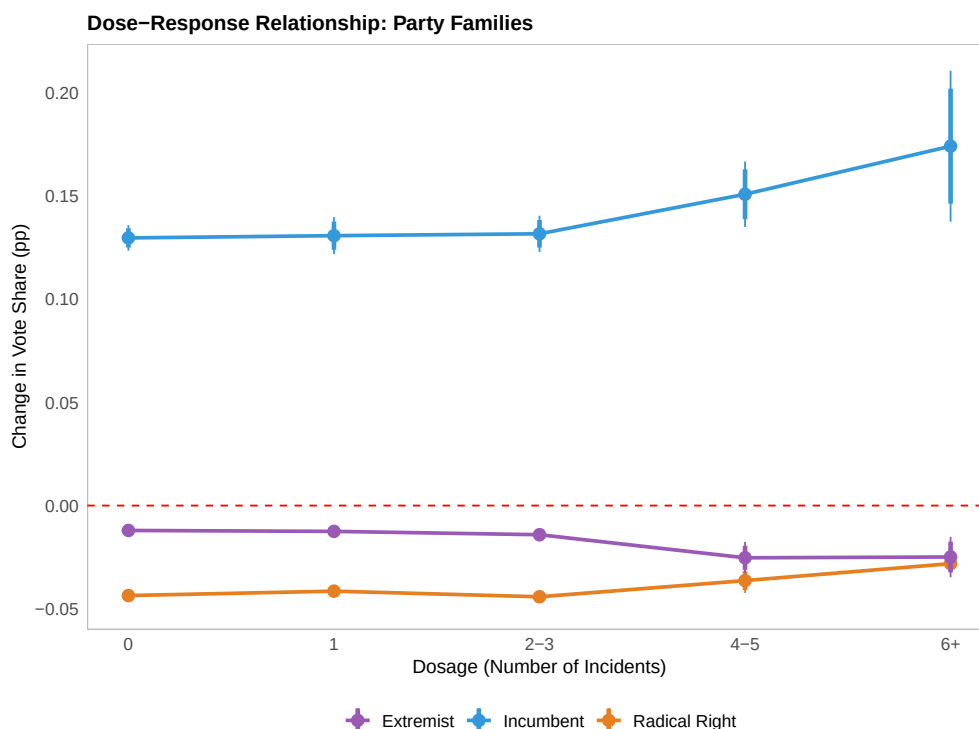


Figure G1: Dose-response relationship: party families. Mean Δ vote share (percentage points) by dosage bin with 95% confidence intervals. Dosage bins: 0 (control), 1, 2–3, 4–5, 6+ incidents of political violence within 750m.

Dose-Response Relationship: Individual Parties

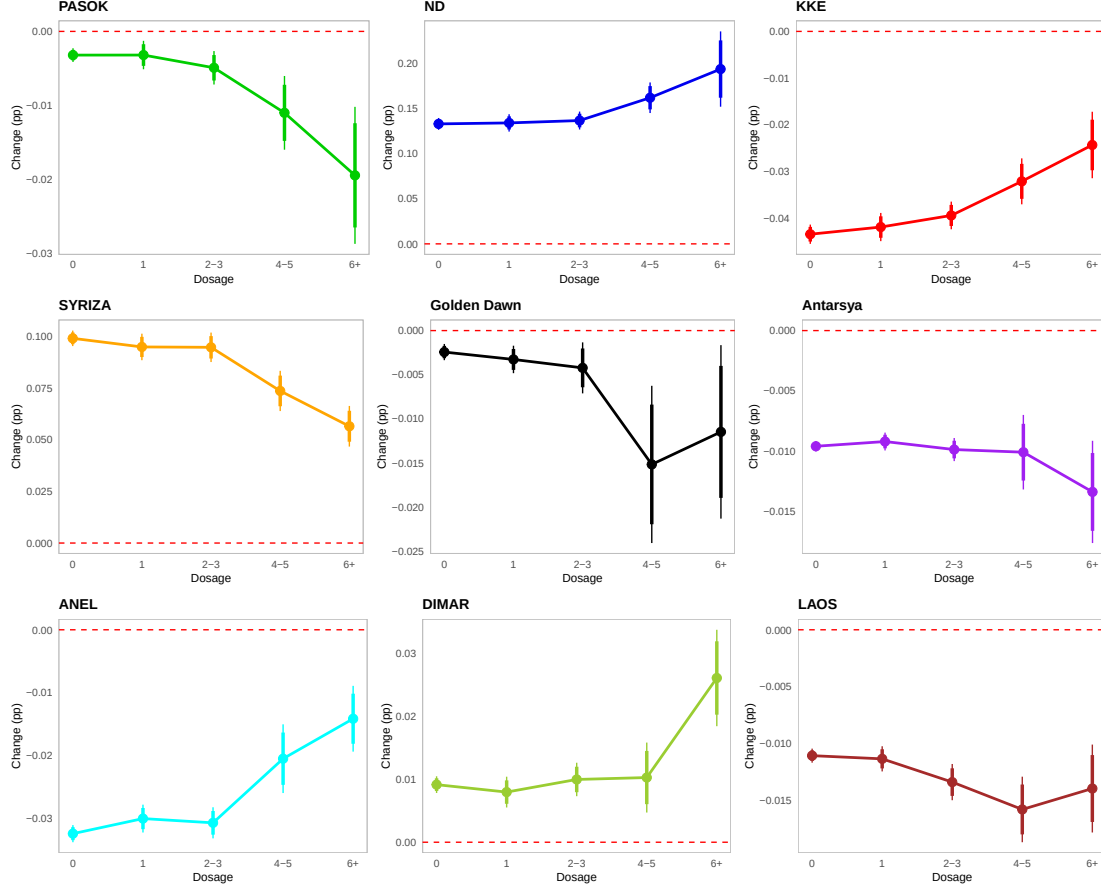


Figure G2: Dose-response relationship: individual parties. Mean Δ vote share (percentage points) by dosage bin. See notes to Figure G1.

G.2 Quadratic specification

We add a quadratic dosage term to detect diminishing or accelerating returns. For most outcomes, the quadratic term is insignificant (Incumbent: $p = 0.60$; Radical Right: $p = 0.15$; Turnout: $p = 0.80$), supporting the linear specification. Two exceptions are Golden Dawn ($p = 0.031$) and LAOS ($p < 0.001$), for which the positive quadratic term suggests that the negative effect of violence on these parties' vote shares levels off at higher dosage. For the Extremist family, the quadratic term is statistically significant ($p = 0.036$), indicating modest saturation at the highest dosage levels.

H Far-left and far-right violence

Our pooled specification treats all incidents of political violence as equivalent regardless of perpetrator ideology. Yet far-left and far-right violence may produce distinct electoral responses if voters attribute responsibility differently depending on the ideological source. Section 7.1 of the main text presents results from a single joint model that includes both far-left and far-right dosage. Figure H1 provides the full coefficient plot for all parties and turnout, and Table H1 reports the coefficients together with Wald tests of the equality of the two poles' effects.

We discuss the results in four steps. We first describe the estimates for far-left (FL) and far-right (FR) violence. We then consider which parties gain where others lose, test whether the FL and FR coefficients differ from each other, and check whether the pattern holds under the mean reversion and nearest-window adjustments.

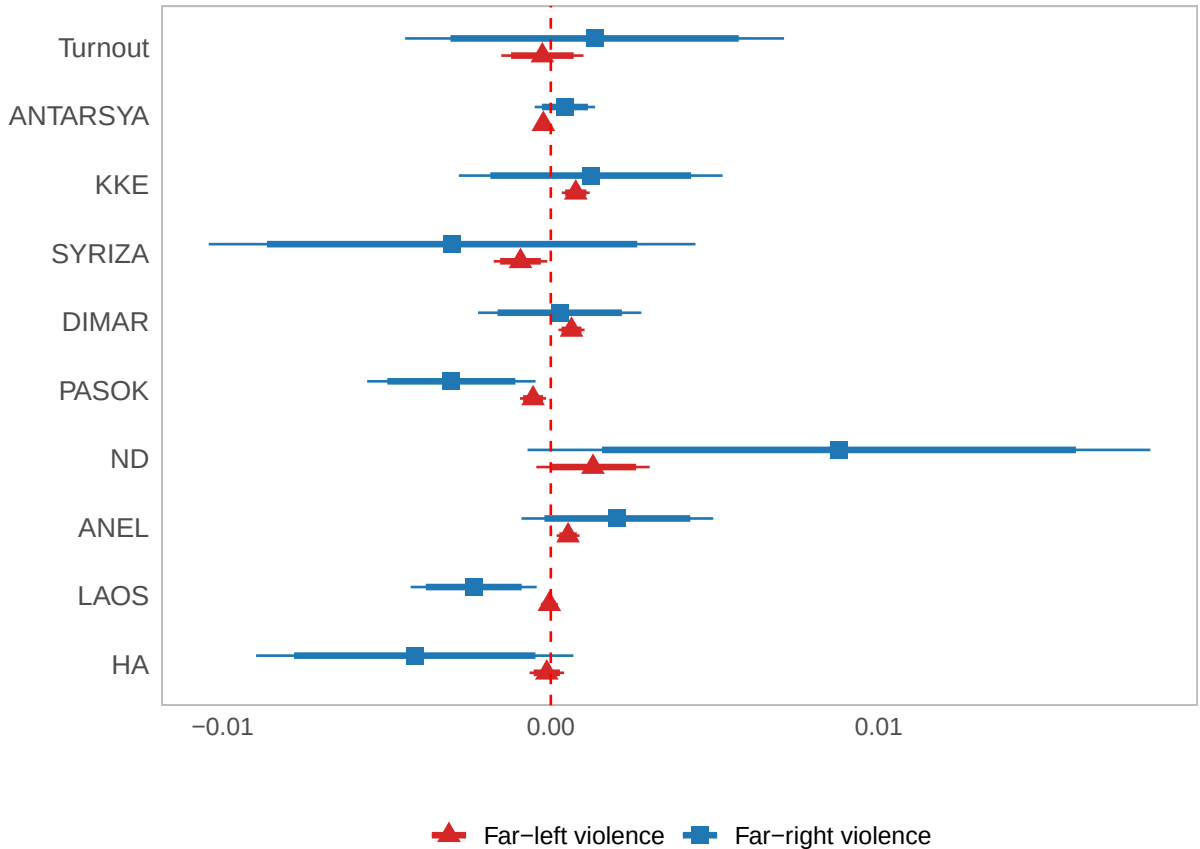


Figure H1: Disaggregated effects of far-left and far-right violence on party vote shares. Coefficients on FL dosage (red triangles) and FR dosage (blue squares) from a single joint model including both poles, with covariates and municipality fixed effects. 95% (thick) and 99% (thin) confidence intervals from Conley spatial-HAC (1 km) standard errors.

FL violence accounts for most incidents (120 of 148) and therefore drives much of the pooled pattern. It is associated with lower vote shares for the FL extremist ANTARSYA ($\beta = -0.0002$, $p = 0.007$), SYRIZA, and PASOK, and with higher vote shares for KKE,

Table H1: Joint-model far-left and far-right dosage coefficients and Wald tests of equality. Each row reports the FL and FR dosage coefficients from a single model including both poles, with covariates and municipality fixed effects. Conley spatial-HAC (1 km) standard errors in parentheses; significance stars reflect the Conley (1 km) p -value. The middle columns report the FL–FR difference with its 95% confidence interval and the Wald statistic $W(1)$ at the primary 1 km cutoff; the final three columns report the p -value of the Wald test of $\beta_{FL} = \beta_{FR}$ at Conley cutoffs of 0.5, 1, and 2 km. ** $p < 0.01$; * $p < 0.05$.

Outcome	FL dosage		FR dosage		Diff.	$\beta_{FL} - \beta_{FR}$ (1 km)		$W(1)$	Wald p : $\beta_{FL} = \beta_{FR}$		
	β	(SE)	β	(SE)		95% CI			0.5 km	1 km	2 km
Incumbent	0.0007	(0.0006)	0.0058	(0.0036)	−0.0050	[−0.0124, 0.0024]	1.75	0.170	0.185	0.243	
Radical Right	0.0005**	(0.0002)	−0.0003	(0.0010)	0.0008	[−0.0012, 0.0028]	0.64	0.480	0.423	0.298	
Extremist	−0.0003	(0.0002)	−0.0037	(0.0019)	0.0034	[−0.0006, 0.0073]	2.84	0.043	0.092	0.144	
Turnout	−0.0003	(0.0005)	0.0013	(0.0022)	−0.0016	[−0.0064, 0.0032]	0.42	0.451	0.516	0.622	
ANTARSYA	−0.0002**	(0.0001)	0.0004	(0.0004)	−0.0007	[−0.0014, 0.0001]	2.87	0.132	0.090	0.027	
KKE	0.0008**	(0.0002)	0.0012	(0.0016)	−0.0004	[−0.0036, 0.0027]	0.08	0.749	0.775	0.825	
SYRIZA	−0.0009**	(0.0003)	−0.0030	(0.0029)	0.0021	[−0.0037, 0.0078]	0.51	0.425	0.476	0.538	
DIMAR	0.0006**	(0.0002)	0.0003	(0.0010)	0.0004	[−0.0016, 0.0023]	0.13	0.723	0.721	0.722	
PASOK	−0.0005**	(0.0002)	−0.0030**	(0.0010)	0.0025	[0.0005, 0.0045]	5.78	0.014	0.016	0.022	
ND	0.0013	(0.0007)	0.0088*	(0.0037)	−0.0075	[−0.0150, 0.0000]	3.81	0.040	0.051	0.106	
ANEL	0.0005**	(0.0001)	0.0020	(0.0011)	−0.0015	[−0.0038, 0.0008]	1.68	0.193	0.195	0.212	
LAOS	−0.0000	(0.0001)	−0.0024**	(0.0007)	0.0023	[0.0008, 0.0038]	8.90	0.001	0.003	0.007	
Golden Dawn	−0.0001	(0.0002)	−0.0041*	(0.0019)	0.0040	[0.0002, 0.0078]	4.30	0.015	0.038	0.062	

DIMAR, and ANEL. It has no significant association with the vote share of the FR extremist Golden Dawn ($p = 0.55$) or of ND ($p = 0.056$).

FR violence is rarer (24 incidents), so its estimates are larger but less precise. It is associated with lower vote shares for the FR extremist Golden Dawn ($\beta = -0.0041$, $p = 0.028$), LAOS ($\beta = -0.0024$, $p = 0.002$, approximately 7.9% of LAOS’s baseline per incident), and PASOK ($\beta = -0.0030$, $p = 0.002$), and with a higher vote share for ND ($\beta = 0.0088$, $p = 0.017$). It has no significant association with the vote share of the FL extremist ANTARSYA ($p = 0.23$).

Each extremist party’s estimate is therefore negative and significant for violence from its own side, and not significant for violence from the other side. We refer to this as the same-side pattern. Whether the two estimates differ from each other is a separate question, which we take up with the Wald tests below.

We next consider which parties gain where others lose. The gains go mostly, though not only, to parties in the same ideological camp as the violence. On the right, FR violence is associated with losses for both Golden Dawn and LAOS and with a large gain for ND. Golden Dawn has documented organizational ties to political violence, whereas LAOS had none. On the left, FL violence is associated with losses for ANTARSYA, SYRIZA, and PASOK and with gains for KKE and DIMAR, the established left-wing alternatives. Its association with ND is not significant ($p = 0.056$).

The exception is ANEL, a radical right party that gains significantly under FL violence. Its estimate under FR violence is also positive but not significant ($p = 0.074$). ANEL’s 2012 manifesto contains no security emphasis (Section A.2.3), which does not fit an issue-ownership reading, and its gain is not robust to our identification checks. We therefore do not rely on it. Our aggregate design cannot show which voters moved, and the conclusion of the main text weighs these patterns against competing interpretations.

The same-side pattern compares an estimate that is significant with one that is not, which does not by itself show that the two differ. We therefore test directly whether the FL and FR

coefficients are equal, using the Wald tests in Table H1. For Golden Dawn the test rejects equality at the primary 1 km cutoff. Its far-right penalty exceeds its far-left penalty by 0.0040 (FL–FR = 0.0040, 95% CI [0.0002, 0.0078]; $W(1) = 4.30$, $p = 0.038$). The rejection also holds at 0.5 km ($p = 0.015$) but not at 2 km ($p = 0.062$), and under the leverage-adjusted estimator the p -value is 0.051. The result therefore depends on the Conley bandwidth and on the estimator. For ANTARSYA the test does not reject at the primary cutoff. The difference is -0.0007 , with a confidence interval that includes zero ($[-0.0014, 0.0001]$; $W(1) = 2.87$, $p = 0.090$), and equality is rejected only at the 2 km cutoff ($p = 0.027$).

The comparison between the two poles has little statistical power, because only 24 FR incidents (43 exposed voting centers) inform the FR coefficient. We therefore read the same-side pattern as descriptive. The differences that hold at every cutoff and under the leverage-adjusted estimator concern LAOS ($W(1) = 8.90$, $p \leq 0.007$ at every cutoff) and PASOK ($W(1) = 5.78$, $p \leq 0.022$). For both parties the estimate for FR violence is far larger than the estimate for FL violence.

Table H2: The joint far-left and far-right model under the mean reversion and nearest-window adjustments. Each panel re-estimates the joint model (both poles, covariates and municipality fixed effects) for the two extremist parties and their family: Panel A as in Table H1, Panel B adding the outcome party’s May 2012 vote share (Y_{May} ; Appendix C.7), Panel C adding the closest pre-treatment window’s dosage (Appendix D). Conley spatial-HAC (1 km) standard errors in parentheses, with significance stars on that p -value; “ p adj.” is the leverage-adjusted Conley p -value (Appendix B.1). The last two columns report the Wald test of $\beta_{\text{FL}} = \beta_{\text{FR}}$ at 1 km under the two estimators. ** $p < 0.01$; * $p < 0.05$.

Outcome	FL dosage			FR dosage			Wald p : $\beta_{\text{FL}} = \beta_{\text{FR}}$	
	β	(SE)	p adj.	β	(SE)	p adj.	Conley	adj.
<i>Panel A. Baseline</i>								
Golden Dawn	−0.0001	(0.0002)	0.601	−0.0041*	(0.0019)	0.038	0.038	0.051
ANTARSYA	−0.0002**	(0.0001)	0.017	0.0004	(0.0004)	0.266	0.090	0.121
Extremist	−0.0003	(0.0002)	0.103	−0.0037	(0.0019)	0.072	0.092	0.114
<i>Panel B. Adding the party’s May 2012 share (Y_{May})</i>								
Golden Dawn	−0.0002	(0.0002)	0.176	−0.0034*	(0.0015)	0.036	0.041	0.058
ANTARSYA	0.0000	(0.0000)	0.443	0.0002	(0.0002)	0.287	0.338	0.370
Extremist	−0.0004**	(0.0002)	0.018	−0.0030	(0.0016)	0.080	0.113	0.142
<i>Panel C. Adding the closest pre-treatment window’s dosage</i>								
Golden Dawn	−0.0005	(0.0003)	0.070	−0.0043*	(0.0019)	0.031	0.049	0.064
ANTARSYA	−0.0003**	(0.0001)	0.009	0.0004	(0.0004)	0.325	0.067	0.095
Extremist	−0.0008**	(0.0003)	0.003	−0.0039*	(0.0019)	0.057	0.121	0.146

Finally, we check whether the same-side pattern holds under the two adjustments that we apply to the pooled model. Table H2 re-estimates the joint model for Golden Dawn, ANTARSYA, and the extremist family, adding first the party’s own May 2012 vote share (Appendix C.7) and then the dosage of the closest pre-treatment window (Appendix D.3). The far-right penalty on Golden Dawn holds in all three specifications ($p = 0.028$, 0.024, and 0.021 under Conley; 0.038, 0.036, and 0.031 under the leverage-adjusted estimator). The far-left penalty on ANTARSYA does not. It holds at baseline ($p = 0.007$) and under the nearest-window control ($p = 0.005$), but it disappears once ANTARSYA’s May share is added ($\beta = 0.00003$, $p = 0.38$). Appendix C.7 reports the same sensitivity to mean reversion for ANTARSYA’s pooled estimate. The same-side pattern therefore holds in two of the three

specifications, and its left half does not hold under the mean reversion adjustment. For this reason the main text treats the pattern as descriptive rather than as a finding.

The extremist family behaves differently. Its far-left coefficient is not significant at baseline ($p = 0.067$) and becomes significant under both adjustments ($p = 0.007$ and 0.001), which is a further reason to state the extremist claim for the family as a whole. The Wald tests in Table H2 lead to the same reading as those in Table H1. At the 1 km cutoff, equality is not rejected in any specification for ANTARSYA or for the family. For Golden Dawn it is rejected under Conley inference in all three specifications (p between 0.038 and 0.049) but not under the leverage-adjusted estimator (p between 0.051 and 0.064).

I Leave-one-out municipality analysis

To assess whether our results are driven by any single municipality, we iteratively exclude each municipality in our sample and re-estimate the main models. Figure I1 displays the resulting dosage coefficients for the three party families. The red diamond marks the full-sample estimate, grey points show leave-one-out coefficients, and the grey band spans the interquartile range.

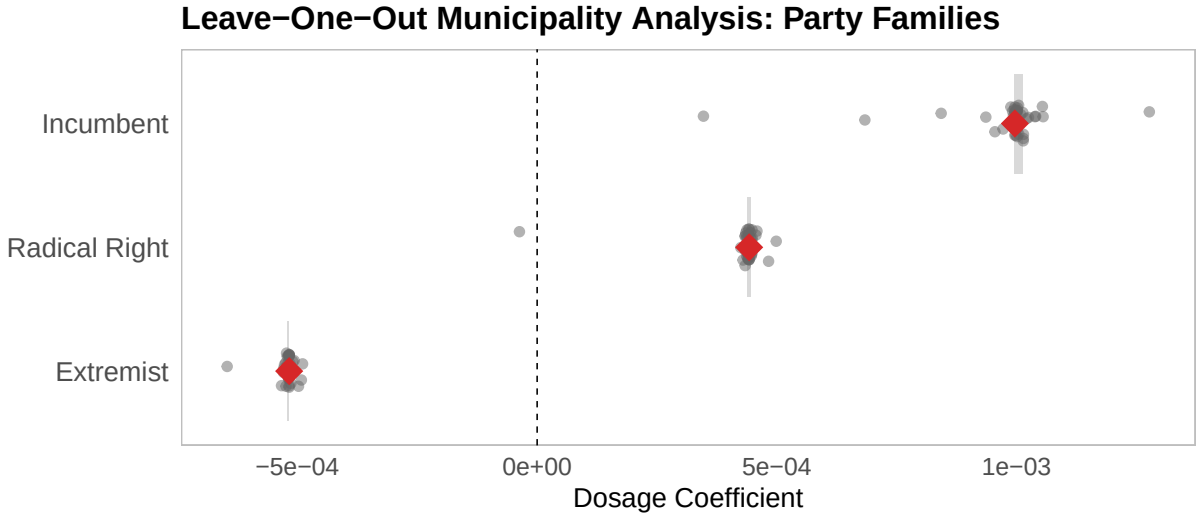


Figure I1: Leave-one-out municipality analysis: party families. Each grey point represents the dosage coefficient from a model excluding one municipality. Red diamond: full-sample estimate. Grey band: interquartile range of leave-one-out coefficients. Results are stable across all three party families.

The party family results are broadly stable. The Extremist coefficient clusters tightly around the full-sample estimate, and the Incumbent and Radical Right estimates show somewhat more variation but remain qualitatively consistent across most exclusions. The individual-party results (Figure I2) similarly demonstrate stability. The one visible outlier in each panel corresponds to the Municipality of Athens, which contributes 103 of the 603 voting centers and contains the highest concentration of violence incidents. Dropping Athens shifts the Radical Right coefficient from 0.0004 ($p = 0.003$) to approximately zero ($p = 0.94$), and the Extremist coefficient from -0.0005 ($p = 0.025$) to -0.0006 ($p = 0.12$). No other municipality's exclusion changes the significance of any family-level result under Conley inference; under the leverage-adjusted estimator of Appendix B.1, four exclusions (Glyfada, Thessaloniki, Kallithea, Piraeus) put the Extremist family just above the 5% threshold. That Athens exerts this leverage is unsurprising, as it is the primary locus of both political violence and electoral competition in the sample, so removing it eliminates much of the identifying variation. The key point is that outside Athens, no single municipality drives the results, and even with Athens excluded, coefficients retain their relative magnitudes.

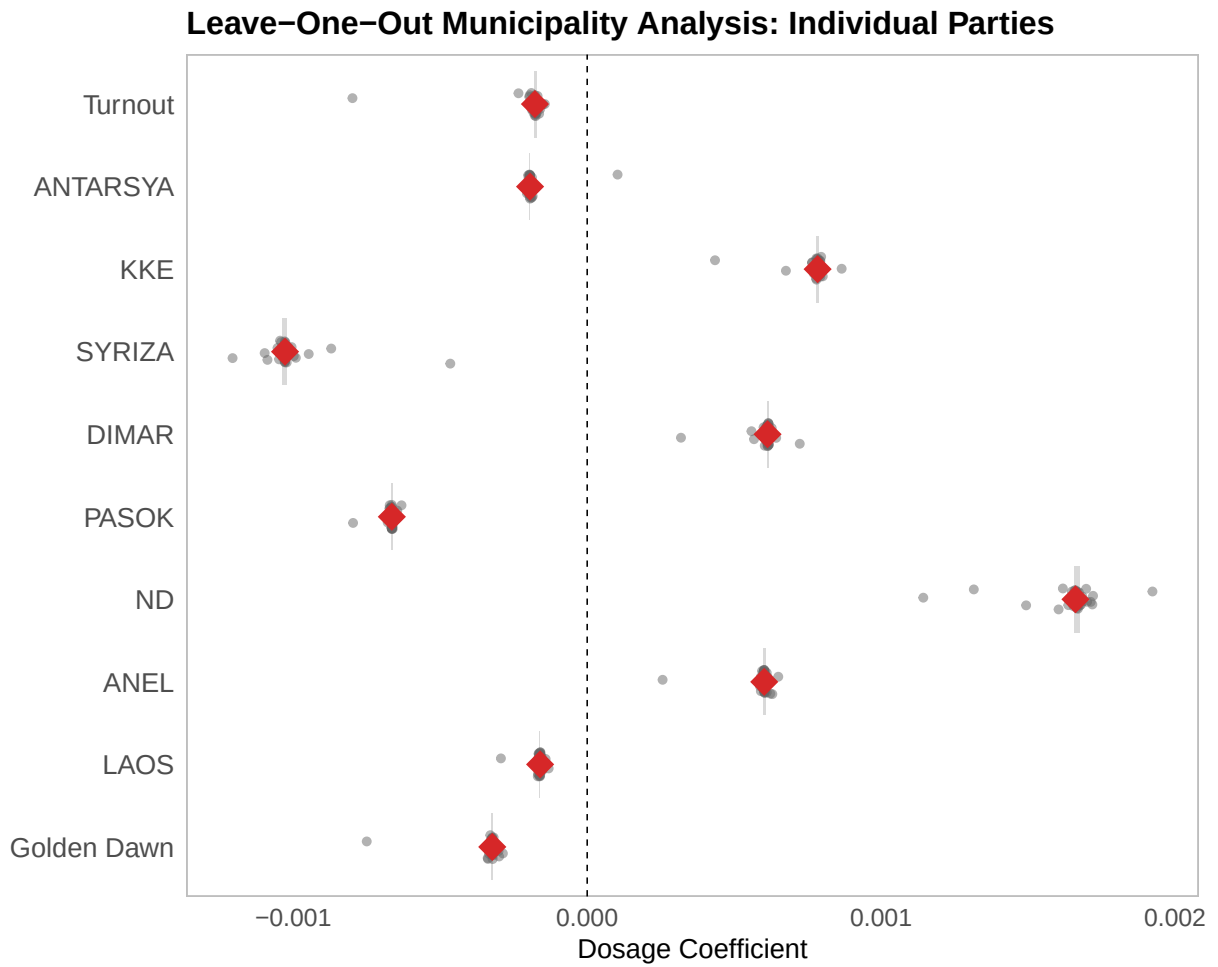


Figure I2: Leave-one-out municipality analysis: individual parties. See notes to Figure I1.

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