Sympathy for the Devil? Right-wing terrorism and political behavior in Hungary

by

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Submitted to

Central European University

Department of Political Science

In partial fulfilment of the requirements for the degree of

Master of Arts in Political Science

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Vienna, Austria

2022

Abstract

Despite the growing right-wing terrorist threat in Europe, the effects of extreme-right violence on political behavior remain understudied. Especially little is known about the impact of extremist terrorism or hate crime against minorities on the electoral performance of mostly non-violent radical right parties with a similar ideological outlook. This paper contributes to the literature by examining the electoral effect of the 2008-2009 terrorist attacks against Roma minority members in Hungary, the most significant case of domestic ethnic violence since WWII. The attacks preceded the rise of the radical right Jobbik party to national prominence in the European and national parliamentary elections of 2009 and 2010, where it ran on an explicitly anti-Roma platform. I utilize the spatial variation in targeted localities to estimate the effect of exposure to terrorism – proxied by geographical closeness – at the locality level compared to the 2006 elections. While previous research often failed to consider that electoral outcomes are endogenous to target selection, to identify more valid causal effects, I utilize a natural experiment in which the control group contains localities where perpetrators planned further attacks prior to their arrest. Difference-in-differences regression models with varying specifications and robustness checks demonstrate that the exposure to the attacks has on average increased support for Jobbik with approximately 3-5 percentage points by 2009. There is some evidence that this was accompanied by an increase in anti-Roma prejudice at the individual level, implying that the political change was mediated by shifting attitudes towards the outgroup in response to social identity threat.

Keywords: *right-wing terrorism, radical right, hate crime, ethnic violence, electoral behavior, Hungarian politics*

Word count: 14274

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I hereby declare that the present MA thesis is exclusively my own work based on my research and analysis. All external data sources and information is appropriately referenced with in-text citations and in the bibliography. An earlier version of this work limited to parts of the first six chapters, including the literature review and the research design, but excluding the analysis, was submitted as part of the assessment for the "Identity & Prejudice" course at CEU's Department of Political Science in the Fall term of 2021.

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5 June, 2022

Acknowledgements

This Master's thesis could have never been finished without indispensable advice and assistance by multiple people. First, I am particularly grateful for the insightful substantive and methodological feedback from my supervisor Gábor Simonovits, as well as his much-needed help with accessing some of my data sources. Furthermore, I am especially indebted to my other professors who taught me about quantitative political economy, applied econometrics, and causal inference during my MA studies at CEU, including, but not restricted to Michael Dorsch, Tímea Molnár, Arieda Muco, and Anand Murugesan. Besides, I am much obliged to those who helped me access crucial data and contextual information for my project: Balázs Lengyel from the Budapest Center for Economic and Regional Studies supported my request for data access from the Center's Databank; Gábor Miklósi from 444.hu, who covered the 2008-2009 attacks and the following trial as a journalist was kind enough to answer my questions about contextual details. I am grateful for the comments on earlier versions of this thesis made by participants of the MA thesis writing seminars and the "Identity & Prejudice" course at CEU's Department of Political Science. I am also thankful to all my friends and classmates with whom I could informally discuss my ideas and who answered my questions related to statistics and coding, including Bálint Cocchioni, Gergely Mónus, Ádám Víg, and many others. Finally, I would have never completed my work without the attentive emotional support of Júlia.

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Abbreviations

ATE	Average treatment effect
DiD	Difference-in-differences
EP	European Parliament
MIÉP	Party of Hungarian Truth & Life
NUTS	Nomenclature of Territorial Units for Statistics
OECD	Organization for Economic Cooperation & Development
OLS	Ordinary least squares
SE	Standard error
US	United States of America

1. Introduction

The electoral success of nativist radical right parties across OECD countries in the last two decades has been unprecedented in the post-war era. Their rise was paralleled by an increase in violent attacks by right-wing extremists, with whom they share important ideological tenets on issues such as the role of ethno-national identity, immigration, crime, and the rights of ethnic minorities (Mudde, 1995). While right-wing extremists tend to take up more radical positions, the main difference oftentimes lies in the means employed: while "mainstream" far-right parties take the traditional route of parliamentary politics, right-wing extremists, such as neo-Nazis, tend to pursue their goals through militancy and violent action against minorities and their supporters. Even though curtailing minority rights and immigration are usually highly salient on the parliamentary radical right's agenda too, this disagreement tends to lead them to at least nominally distance themselves from hate crime and violence against minorities (Pickard et al., 2022).

My paper asks the question of whether acts of violent terrorism by extreme right actors against ethnic minority members are capable of spurring electoral support for the parliamentary radical right. Insights from integrated threat and social identity theory suggest that combined with strong ingroup identification and prejudice against the outgroup, the collective guilt over ingroup misconduct may inhibit ingroup members from empathy with outgroup victims and spur more dehumanizing attitudes against them. The arising social identity threat could also lead ingroup voters to justify violence and turn to the nativist radical right, which promises to defend the relatively higher status of the ingroup (Castano and Giner-Sorolla, 2006; Riek, Mania and Gaertner, 2006; Charnysh and Finkel, 2017; Homola, Pereira and Tavits, 2020).

To answer the above research question, this study examines how a series of ethnically motivated terrorist attacks against members of the Roma ethnic community by right-wing extremists in Hungary in 2008-2009 have influenced the probability of voting for the radical right Jobbik party. While the perpetrators have not been charged with domestic terrorism, the attacks were premediated, had clear political motives, and strongly

resembled terrorist attacks in other European countries, with some commentators and academics arguing that they could rightfully be labeled as such (Verseck, 2011; Mareš, 2018; Tamás, 2019). Therefore, I will continue to refer to them this way throughout this paper. The attacks have taken place in parallel to the unprecedented politicization of the Roma issue in Hungary, which saw the rise of the anti-Roma radical right Jobbik party and just preceded its subsequent success in a series of elections in 2009-2010. The variation in the geographical distribution of the attacks allows me to estimate their effect on Jobbik's vote share in the European Parliamentary elections of 2009 and the Hungarian parliamentary elections of 2010. In addition, I also employ survey data to estimate the treatment effect on vote intentions and anti-Roma attitudes at the individual level.

This study makes some important contributions to the existing academic literature. Inquiry into interethnic violence, hate crime, and terrorism has tended to focus more on causes than consequences. Among studies about the effects of violence on public opinion and voting, the impact of right-wing extremist terrorism has received comparatively little attention, with most research focusing on the effects of Islamist and international terrorism instead. This imbalance leaves a crucial structural difference overlooked: while Islamist violence in OECD countries implies an attack committed by an outgroup minority member against the ingroup majority, radical right terrorism is usually committed by members of the in-group against the outgroup.[1] Furthermore, while it has often been argued that the parliamentary and media presence of populist and radical right parties normalizes and encourages hate crime and violence (Edwards and Rushin, 2018; Ang, 2021; Müller and Schwarz, 2021), very little research has been done to ascertain whether this effect also runs in the opposite direction: i.e. if hate crime and interethnic violence can contribute to the rise of the radical right.

The methodological novelty of this paper lies in offering the first causal quantitative analysis of the effects of right-wing extremist terrorism on the electoral fortunes of radical

[1]Right-wing extremist attacks against perceived ingroup allies of undesirable outgroup elements, such as left-wing party members in the case of Anders Breivik's 2011 Norway attack, constitute notable exceptions, but do not contradict the main argument.

right parties, underpinned by a novel identification strategy, which utilizes a natural experiment for post-treatment comparisons and a difference-in-differences (DiD) research design. After the perpetrators of the anti-Roma murders were captured, a list of locations for planned future attacks came to light during the investigation. Assuming that the terrorists selected their targets homogeneously over time, using locations of unrealized attacks as a control group allows me to estimate the treatment effects of right-wing terrorism on political behavior without the threat of selection bias due to unobserved spatial covariates. In addition, I also conduct a range of robustness checks, including comparing the locations of successful and unsuccessful attacks, and matching targeted localities with non-targeted ones based on pre-treatment outcomes and observables. Finally, to avoid the ecological fallacy, I also exploit survey data collected before and after the attacks to test their effect on individual-level attitudes.

The results suggest that the terror attacks have increased Jobbik's support in the 2009 EP elections by 3-5 percentage points. These estimates are consistent across specifications. The effect of the attacks has weakened by the 2010 parliamentary elections, albeit it still remained positive. Survey data analysis suggests that exposure to the attacks was associated with increased anti-Roma prejudice, which supplies some evidence for the social identity threat theory. On the contrary, the analysis found no evidence that the increased support for Jobbik was due to the heightened salience of interethnic conflict. If anything, increased salience of the anti-Roma terror attacks during the subsequent trial with heightened media attention was associated with a decrease in the intention to vote for Jobbik.

The paper proceeds as follows: first, I review the literature on the effects of right-wing terrorism and political behavior and identify some relevant gaps. Second, I justify the case selection and introduce the case in some detail. Third, I lay out the theoretical framework, grounded in intergroup threat, social identity, and issue salience theories, and operationalize my hypotheses. Fifth, I describe the data and include descriptive statistics. Then, I describe the research design and the identification strategy and highlight the results from the main analysis, followed by the robustness checks. Finally, I discuss the

results in light of the literature, and conclude the paper.

2. Literature review

It is often argued that in OECD countries, media and counter-terrorism policy exhibit a bias against Islamist terrorism, which is characterized by ethnic or religious outgroup members perpetrating violence against ingroup members. At the same time, right-wing extremist terrorism, usually consisting of acts of violence committed by majority ingroup members against the outgroup minority is much less salient in the public discourse and on the policy agenda. In the United States, despite the fact that the overwhelming majority of post-9/11 terrorist attacks were committed by right-wing extremists, the FBI focused most of its agents on international terrorism (Walters and Chang, 2021), while in Germany it was systematically overlooked by center-right political actors and police unions (Alizade, Dancygier and Homola, 2022). Right-wing extremism has remained a crucial threat and is currently on the rise in Europe, especially in Germany, too, where attacks by right-wing extremists, primarily against the Muslim and Jewish communities, have claimed more lives in recent years than Islamist terrorism. Nevertheless, recent events such as the Christchurch shooting have shifted more attention towards right-wing extremism (Auger, 2020; Assoudeh and Weinberg, 2021).

2.1. The effects of terrorism on voting

Social scientists so far have mostly focused on the causes rather than the consequences of violence and terrorism targeting minorities. On top of that, the subset of political science literature dealing with the consequences of terrorism on public opinion and political behavior suffers from a bias comparable to media and law enforcement. Most current research, including studies using natural experiments and causal inference methods to estimate the impact of terrorism on voting in OECD countries, has remained preoccupied with international, separatist, and Islamist terrorism. Overall, this literature delivers support for two major theories. On the one hand, many argue that violence increases intergroup threat, which drives people to support nativist parties in response. Work by Kibris (2011) shows that Kurdish terrorist attacks in Turkey result in an increased vote

share for the radical right. Examining data from the US, Hersh (2013) found that family members and neighbors of 9/11 victims have become significantly more supportive of Republicans over time compared to people with similar characteristics. According to Balcells and Torrats-Espinosa (2018), Basque ERA terrorist activity in Spain had a positive effect on the intention to participate in elections but has not impacted party preferences. In addition, terrorist attacks increase prejudice against minority members of the same ethnicity as the perpetrators (Legewie, 2013; Shin, 2021).

A prominent case for studying the electoral effects of terrorism has been Israel, where Berrebi and Klor (2006; 2008) found that terrorist attacks committed by Palestinians predict an increase in the vote share of Jewish right-wing parties, which is the strongest in the affected localities. However, they also find evidence for rising polarization, as terror attacks increase the share of the left-wing vote in left-leaning localities where no attack took place. Another study from Israel exploited the exogenous variation in the increasing range of Hamas missiles to estimate the effect of terrorist threat on voting outcomes in Southern Israeli territories (Getmansky and Zeitzoff, 2014). The authors found a significantly higher probability of voting for Jewish right-wing parties in the affected territories. According to Peffley, Hutchison and Shamir (2015), among Israelis identifying with the Jewish Right, support for anti-Palestinian exclusionary policies increases in the aftermath of terrorist attacks.

On the other hand, some delivered evidence that intergroup violence drives voters to take up positions more accommodating of minority demands, possibly out of fear of recurring attacks. In a more recent study, Gould and Klor (2010) have found that Palestinian terrorism, overall, has pushed the Israeli electorate and right-wing parties to the left on the Palestinian issue, making it more accommodating of Palestinian demands. According to Montalvo (2011), who exploits the fact that the deadline for sending mail-in-ballots preceded the 2004 Madrid bombings allegedly committed by Al-Qaeda members, found that the attacks have shifted the electorate towards the Socialist Party, which opposed the Iraq war and supported the accommodation of immigrants' rights. However, the shift may have also been driven by popular dissatisfaction with the Conservative government's

crisis management.

2.2. Violence against the outgroup and voting behavior

Contrary to the richer set of evidence on the effect of ethnic violence and terrorism against majority members by minority groups, little research has tackled the question of how attacks by ethnic majority members against minorities impact political behavior. Integrated threat and social identity theory from social psychology postulate that since people tend to defend the status of their ingroup to protect their own self-esteem derived from group membership, they judge atrocities differently as a function of the group membership of perpetrators and victims. People are thus more likely to feel empathy towards the ingroup victims of violence by an outgroup member than vice versa (Riek, Mania and Gaertner, 2006; Molenberghs et al., 2016; James and Zagefka, 2017; Li, Leidner and Fernandez-Campos, 2020). In the presence of some moderating factors, however, ingroup members deviating from the norms may be punished more harshly than outgroup members. This so-called "Black Sheep Effect" is too motivated by the need to protect the ingroup's status (Otten and Gordijn, 2014). These theories thus imply that the effect of terror attacks on public opinion and political behavior diverges based on the perceived group membership of the perpetrators and the victims.

In case the wrongdoing was committed by an ingroup member against the outgroup, the feeling of collective guilt and the resulting identity threat can spur defensive attitudes justifying the violence, glorifying the ingroup, and dehumanizing the outgroup (Noor et al., 2012). This phenomenon has been confirmed empirically in multiple experimental studies. When the atrocities were committed by the US, Americans were more likely to justify the torture of Iraqi civilians, dehumanize the victims, and glorify their country, compared to when atrocities were committed by other countries (Leidner et al., 2010; Leidner and Castano, 2012; Tarrant et al., 2012); Westerners confronted with mass killings of their former colonial subjects showed increased prejudice against the victimized groups (Castano and Giner-Sorolla, 2006); Germans and Hungarians confronted with their respective nation's role in the Holocaust and ongoing Jewish suffering exhibited higher levels of anti-Semitism

and distancing (Imhoff and Banse, 2009; Peetz, Gunn and Wilson, 2010; Hirschberger, Kende and Weinstein, 2016). The justification of ingroup misconduct is often mediated by the perception of the ingroup as the real, but unrecognized victim of the outgroup. According to a study by De Guissmé and Licata (2017), a stronger sense of collective victimhood among Black African and Muslim immigrants in Belgium was associated with higher levels of anti-Semitism, based on an understanding of victimhood as a zero-sum game.

Despite the rich evidence from social psychology, several studies have failed to integrate insights from intergroup threat theory (Byrne et al., 2022), pooling together terror attacks committed by and against outgroup members (Baccini et al., 2021; Turkoglu and Chadefaux, 2022). Some authors delivered relevant evidence on the relationship between anti-minority communal violence and hate crime with extreme right voting. Koopmans (1996) reports an inverse relationship between the two in Germany, which supports his theory that racist violence compensates for the lack of political opportunities. On the contrary, Dancygier (2010) found that localities experiencing communal violence against immigrants subsequently saw a higher share of votes cast for the British National Party. These results, however, remain prone to endogeneity-induced bias.

However, localized, more or less spontaneous acts of hate crime against outgroup members are qualitatively different from right-wing terrorism, which remains understudied compared to the former. Most studies have delivered evidence for the Black Sheep Effect, with some heterogeneity across targets and voters. According to De la Calle and Sánchez-Cuenca (2013) – who utilize the geographic variation of separatist ETA attacks in the Basque region to examine their effect on support for the affiliated Batasuna party – the local treatment effect of terrorism in Basque-majority municipalities is heterogeneous: while the killings of informants and criminals increase party support, deadly attacks against security forces and moderate politicians result in a loss of votes. Jakobsson and Blom (2014), argue that due to the Black Sheep Effect in response to shameful behavior by an ingroup member, attitudes towards immigrants have changed in a positive direction after the 2011 Norway attacks by anti-immigrant terrorist Breivik. They employ

a regression discontinuity design on an interrupted survey to infer causal effects. Using panel data, Solheim (2020) showed that this change change was less pronounced among voters who previously supported the anti-immigrant right-wing populist Progress Party or disliked immigrants. Using another survey interrupted by the 2019 Christchurch attack in New Zealand, Byrne et al. (2022) reported an increased sense of community after the shooting. Using the same strategy, Shanaah et al. (2021) show increased warmth towards Muslims, which was, however, shorter-lived among conservatives who perceive a greater distance to the Muslim outgroup. According to an experiment set in Austria, (Knupfer and Matthes, 2021), Muslim victims of a fictitious right-wing terror attack elicited lower perceived similarity and compassion than Christians. Finally, Pickard et al. (2022) show that British voters distanced themselves from a range of culturally conservative political positions in the aftermath right-wing extremist attacks.

Papers examining the full universe of terror attacks in a given period also confirm the importance of group membership. A recent paper by Baccini et al. (2021) analyzes data on terrorist attacks perpetrated by all kinds of organizations in the United States to reveal a null effect on electoral outcomes. Given that attacks committed in the name of right-wing causes (i.e. racial animosity & anti-abortion) are among the most common ones in their dataset, this may indicate that public reactions to acts of violence perpetrated by members of the ethnic white ingroup may be weaker than to threats of outgroup violence. Turkoglu and Chadefaux (2022) too find a null effect of terror attacks in Europe – including right-wing ones – on political attitudes in a research design identifying causal effects through the inherent randomness of attackers' success.

There also exists some related work on media effects, which looks at how prejudiced anti-minority content incites nativist voting or violence. According to Selb and Munzert (2018), the effect of Hitler's speeches was mostly negligible on the Nazi vote in the localities he spoke at. However, unpublished work by Ang (2021) shows that screenings of the Birth of a Nation movie in the United States, which has been accused of promoting anti-Black racial prejudice, are linked to a higher probability of lynchings. Recently, a range of papers found that Donald Trump's election was associated with increases in racial prejudice and

hate crime (Edwards and Rushin, 2018; Giani and Méon, 2021; Feinberg, Branton and Martinez-Ebers, 2022).

Somewhat counterintuitively, some of the most compelling evidence of the effect of extreme-right violence on public opinion and voting comes from studies on the long-term effects of the persecution of Jews in the Third Reich. Charnysh and Finkel (2017) found that distance to the Treblinka death camp in contemporary Poland is a significant predictor of voting for the anti-Semitic League of Polish Families. Similar evidence has been delivered from Germany by Hoerner, Jaax and Rodon (2019) and Homola, Pereira and Tavits (2020). Both papers found that proximity to former concentration camps in Germany positively predicts voting for the radical right Alternativ für Deutschland party, political intolerance, and xenophobic attitudes. These articles have all attempted to explain their findings with theories rooted in the notions of collective guilt and social identity threat. First, Charnysh and Finkel uses a rich set of data to demonstrate how locals benefited from the presence of the camp, which presumably contributed to heightened feelings of collective guilt later on. Second, Hoerner, Jaax and Rodon speculate that locals cannot reconcile their national identity with their daily exposure to the historical memory of the genocide. Third, Homola, Pereira and Tavits argue that due to the direct exposure to the camps, locals have rationalized the inhumane treatment of outgroup members to avoid the identity threat resulting from admitting collective guilt. Had they not internalized the idea that Nazi victims deserved their fate, the evidence would have damaged the status of the ingroup and their identity. Subsequently, these attitudes were transmitted across generations.

3. Case selection & context

3.1. The anti-Roma attacks of 2008-2009

The anti-Roma attacks that took place in Hungary in 2008-2009 in many respects constitute an ideal case for identifying the causal effects of right-wing terrorism on voting behavior. First, terrorist violence has been an extremely rare phenomenon in Hungary, so the attacks can be considered the single most important case of anti-minority violence in the country since World War II, unlikely to be correlated with events of similar magnitude. Second, the attacks all committed by the same group of men who consistently employed the same methods and selected similar targets. Third, in contrast to much of the recent right-wing terrorist activity in Western Europe, which is often concentrated in cities populated by immigrants, these attacks took place in distant rural areas, with a more homogeneous, less mobile population. These factors allow for the more valid identification of causal effects due to the presumable lack of confounding by other violent activities and demographic factors. The uniqueness of the event may also imply a more profound behavioral effect. However, despite the advantages of this research design, the very same peculiarities of these attacks somewhat limit the generalizability of my findings to right-wing extremist violence in general.

The eight attacks in which terrorists attacked Roma-inhabited houes in villages across Eastern and Northern Hungary with arson and gunfire took place over the course of approximately one year from July 2008 to August 2009, leaving six people dead and many more injured. In the case of the attack at Tarnabod, however, they accidentally targeted houses of non-Roma residents without any casualties, which casts doubt on whether it could have influenced local voting behavior like at other localities.[2] Qualitative evidence

[2] The eight attacks took place at the villages of Galgagyörk, Piricse, Nyíradony, Tarnabod, Nagycsécs, Alsózsolca, Tatárszentgyörgy, Tiszalök, and Kisléta. Before the Galgagyörk attack, the terrorists also shot at a refugee camp in the city of Debrecen. Given that the attack was not directed against Roma, took place in a major city, did not have serious casualties, and did not receive widespread media coverage, I exclude it from the analysis.

indicates that by early 2009, the attacks exerted substantial behavioral effects among the Roma communities residing in the affected region. Journalistic accounts confirm widespread fear among Roma residents, with many of them responding to the terrorist threat with the organization of voluntary night patrols, resulting in some confrontative encounters with supposedly suspicious non-Roma (Tódor, 2017).

According to the trial, the motives of the neo-Nazi perpetrators who were dissatisfied with the parliamentary approach of Hungary's radical right Jobbik party were political in nature, including racial animosity and the goal of inciting further violence among and against Roma (Miklósi, 2011; Jászberényi, 2016; Tamás, 2019). Besides these motives, the attacks comply with definitions of terrorism in that they were carefully premeditated against civilians with whom the attackers had no previous relations. Furthermore, at the village of Kisléta, one of the targeted locations, the perpetrators also planned to assassinate the liberal pro-Roma mayor. These factors and the attackers' political stance shared by several 'officially' designated terrorists across Europe led both Hungarian and international commentators and academics to label the attacks as 'terrorism' even though the attackers were only sentenced for aggravated murder (Verseck, 2011; Jászberényi, 2016; Mareš, 2018; Tamás, 2019).[3] Correspondingly, the Tatárszentgyörgy attack, which has received the most media attention of all, is also included in the Global Terrorism Database (START, 2021).

The last attack at Kisléta in August 2009 was preceded by the European Parliamentary election in June, where the radical right Jobbik scored its first major electoral gains. While the connections between the eight attacks were widely assumed by that point, the extremist motives were not yet officially confirmed. In contrast, by the April 2010 parliamentary elections, when Jobbik emerged as the third-largest political force, the

[3] Ultimately, however, the designation of the attacks as "terrorism" does not influence the empirical strategy, the sample, or the results. The small number of attacks studied in this paper were selected according to a limited set of clear criteria: they were all committed by the same group against similar targets using similar methods, Therefore, I do not have to rely on competing definitions of terrorism to define my universe of cases. Nevertheless, employing the concept of "terrorism" eases the accommodation of the case in the political science literature.

terrorists were already captured, and their extreme-right motives were widely publicized. According to a widely-covered press conference by the Hungarian National Bureau of Investigation, and a later press release by the national news agency of Hungary, the attackers planned to target five additional locations across the same geographic area: Ipolytarnóc, Erdőkertes, Tura, Kisvárda, and Hajdúhadház (MTI, 2010; 2011). However, while the original press release indicates that Hajdúhadház was a location of a planned attack (MTI, 2011), others have argued that it was only targeted because of the presence of military barracks, which could have supplied the group with automatic rifles (Miklósi, 2011).[4].

3.2. The rise of Jobbik

In-between 2006 and 2010, Hungary saw not only unprecedented attacks against Roma but also experienced the rise of the radical right Jobbik party ("Jobbik - Movement for a Better Hungary"). In 2006, Jobbik was still a marginal party that ran in a coalition with MIÉP, the previously largest right-wing radical party, but remained below the 5% parliamentary threshold at 2.2%. In the following years, as the country was ravaged by a political crisis and the Great Recession (Schultz, 2021), Jobbik successfully grew into a sizable parliamentary force, capturing 15% of the national vote in the 2009 European Parliamentary elections, and 17% in the 2010 parliamentary elections.

Anti-Gypsism was at the center of Jobbik's political agenda, as the promise of putting an end to "Gypsy crime" and Roma welfare scrounging featured prominently in the party's platform. It allowed Jobbik to successfully mobilize voters in Hungary's Northern industrial rustbelt and left-behind rural areas on the Northern Great Plains, the regions with the highest Roma population share (Karácsony and Róna, 2011). In combination with its militaristic mobilization style and frequent anti-Roma demonstrations, this has led many to label Jobbik as a right-wing extremist party, ideologically standing to the

[4] This was also confirmed to me in personal correspondence with journalist Gábor Miklósi who had access to the original documentation of the case.

right of major Western European right-wing populist parties (Pirro, 2014). This implies an even greater ideological similarity between the parliamentary far right and the extra-parliamentary extremist right than in most Western European countries. Nevertheless, despite their shared animosity towards Roma, the perpetrators of the terror spree did not cultivate strong links with Jobbik. While briefly associated with Jobbik's nationalistic civil association, the Hungarian Guard, they were primarily affiliated with a more extremist, neo-Nazi subculture lacking high-profile political representation.

4. Theoretical framework

4.1. Social identity threat & issue salience

The increased support for nativist parties in the aftermath of right-wing extremist violence can be explained by two main theories: social identity threat and issue salience. Intergroup threat theory posits that radical right voters feel threatened by outgroup minorities, which explains their hostility towards them (Riek, Mania and Gaertner, 2006; Lucassen and Lubbers, 2012). The impact of social identity threat resulting from confrontation with ingroup-committed atrocities was shown to be associated with increased prejudice and justification for violence. Multiple political science papers dealing with the legacy of the Holocaust (Charnysh and Finkel, 2017; Hoerner, Jaax and Rodon, 2019; Homola, Pereira and Tavits, 2020) and contemporary extreme-right violence also delivered evidence for large perceived intergroup distance and social identity threat inhibiting compassion with outgroup victims and spurring support for nativists parties and positions. This claim is supported by recent research, which has indicated that even very transitory exposure can have a thorough impact on outgroup attitudes (Dinas et al., 2019; Hangartner et al., 2019; Gessler, Tóth and Wachs, 2021).

In this paper, I examine a case in which violence against ethnic minority members is committed by ethnic ingroup members who represent fringe extremism. Under such circumstances, some people may side with the victims of terrorism, even though they belong to an outgroup – in this case, the Roma – out of compassion or due to the Black Sheep Effect described by Jakobsson and Blom (2014). People may also desire violence to end due to their preference for stability, even though they do not feel compassion for the victims. In these two cases, one can expect that the probability of voting for radical right parties sharing the ideological tenets of extremists will decrease.

As the results below demonstrate, this was not the case after the Hungarian anti-Roma attacks, which can be explained through issue salience and identity threat. Existing evidence suggests that the social environment in Hungary is conducive to an increase in

anti-Roma prejudice in response to the attacks and the Black Sheep Effect type response to ingroup violence is highly unlikely. The psychological distancing of Roma is socially accepted and anti-Roma prejudice is so widespread that it can be considered the "expression of dominant social norms" (Kende, Hadarics and Lášticová, 2017). More frequent intergroup contact is also associated with higher prejudice (Kende, Hadarics and Lášticová, 2017), defying the contact theory of intergroup attitudes. Additionally, right-wing authoritarian attitudes among Hungarians are strongly related to justifications for anti-Roma violence (Faragó, Kende and Krekó, 2019). Thus, right-leaning voters are presumably more likely to justify the violent attacks and increase their prejudice in response to exposure to them. On the contrary, in Norway or New Zealand, where the social and media environment upholds stronger egalitarian norms, people may have been more supportive of the ostracization of norm-breaking ingroup members.

The explanatory relevance of social identity theory is corroborated by qualitative anthropological evidence collected right after the first murders. According to interviews with non-Roma locals by anthropologist Kristóf Szombathy *"in a village that was twenty kilometers from"* where in *"2008, the first two victims of the politically motivated "Roma murders" were killed"*, those residing in close geographic proximity to the murders exhibited reactions characteristic of social identity threat in response to ingroup atrocities against a stigmatized outgroup, including justification of violence, ingroup glorification, and a sense of competing, unrecognized ingroup victimhood. According to Szombathy, *"most disconcerting about these tragic episodes was the silence that surrounded them. Most of my non-Romani interlocutors were unwilling to discuss them at any length. The few who were sought to relativize the murders by juxtaposing them with criminal acts committed by Roma or by highlighting the putatively legitimate motivations of the perpetrators"* (Szombati, 2018, p. xiv).

The relative status of the in-group is a crucial component of intergroup threat: an increase in the status of the outgroup relative to the ingroup may be perceived as a symbolic threat and induce increased outgroup animosity that could increase support for the radical right (Riek, Mania and Gaertner, 2006). In Hungary, the relative status threat of non-Roma

exposed to the attacks could have been exacerbated by a perception that the Roma got the upper hand due to their officially recognized victimhood and the sympathy and pro-Roma gestures of political elites. Alternatively, perceived intergroup threat could have been amplified by the post-attack Roma mobilization as described above (e.g. voluntary patrols).

An alternative explanation for the association between exposure to violence and higher Jobbik vote share is related to issue salience theory. Issue salience theory posits that voters will put more weight on some high-salience issues when casting their ballots (RePass, 1971). In connection to issue ownership theory, this means that voters for whom an issue is salient will flock to parties exhibiting "ownership" of that issue through its prominence in their political platform and their perceived competence with regards to it (Bélanger and Meguid, 2008). By changing the salience of an issue through "priming", political campaigns and other events can thus result in changing political outcomes without a shift in attitudes, especially on issues where attitudes are already crystallized (Tesler, 2015). In the case of anti-Roma attacks, exposure to violence could have increased the salience of interethnic conflict and the Roma issue without shifting attitudes in favor of the victims or against the radical right associated with the perpetrators. Given Jobbik's clear ownership of the Roma issue around 2008-2010 (Karácsony and Róna, 2011), this could have shifted the Jobbik vote upwards in affected areas.

4.2. Hypotheses

Based on the theoretical framework above, I put forward the following two hypotheses.

H1: *Exposure to anti-Roma murders in 2008-2009 is associated with a higher vote share for the radical right in the 2009-2010 elections.*

H2A: *Exposure to anti-Roma murders in 2008-2009 is positively associated with anti-Roma prejudice & support for the radical right.*

H2B: *Increased salience of the anti-Roma murders of 2008-2009 is positively associated*

with support for the radical right.

Importantly, the following analysis can only deliver indirect evidence for one of the causal mechanisms described above. Another possible causal mechanism is rooted in bounded rationality: people exposed to the attacks with a preference for violence against Roma updated their beliefs about the efficacy of radical right and cast their vote for the anti-Roma Jobbik, possibly expecting more violent action. However, I believe that such a causal mechanism is highly unlikely to be prevalent in the Hungarian public at large.

5. Data

Data on electoral outcomes from 2002 to 2010 was accessed from the website of the National Election Office of Hungary and was aggregated at the level of localities based on the directory of all Hungarian localities from the Central Statistical Office of Hungary. Data on monthly Google search trends for establishing salience of the anti-Roma attacks over time were accessed from Google Trends. The Spring 2010 wave of the Hungarian Life Course Survey (Simonovits and Kézdi, 2016), which record vote intentions and attitudes towards the Roma in a large sample of more than 8000 Hungarian adolescents is used to test hypothesis 2A on changing anti-Roma prejudice and Jobbik support. Survey data to test hypothesis 2B on issue salience was provided by TARKI Research Institute's Omnibus surveys collected before and after relevant milestones in the attackers' trial in March-April 2011 and August-September 2013, with each wave containing a nationally representative sample of approximately 1000 respondents. Yearly sociodemographic and economic control variables at the locality level from 2000-2010 were accessed from the Central Statistical Office of Hungary through the databank of the Centre for Economic and Regional Studies in Budapest.

Given the large within-country East-West regional heterogeneity on a variety of factors and the way the attacks were clustered in a limited number of counties in Northern and Eastern Hungary, I will limit my analysis to eight counties and exclude Budapest, the capital city, which is an outlier on social, economic, and political variables. The eight counties are Bács-Kiskun, Borsod-Abaúj-Zemplén, Hajdú-Bihard, Heves, Jász-Nagykun-Szolnok, Nógrád, Pest, and Szabolcs-Szatmár-Bereg. Executed and planned attacks took place within or very close to these counties. Distance-based regressions additionally include Csongrád and Békés counties, so that the whole Eastern Hungary NUTS1 region is included. In addition, due to the aforementioned considerations, the localities of Tarnabod and Hajdúhadház cannot be considered proper terrorist targets with a high degree of confidence, so they are excluded from the group of treated and control localities. Coordinates for all localities in these counties used to calculate geographic distances from

attack locations were webscraped from the OpenStreetMap using the Nominatim API. For settlements whose borders have changed during the period of interest because they split off from each other, I am using the common denominator, pooling them into a single settlement for the years following the split-off.

6. Empirical strategy & identification

This paper estimates average spatial treatment effects to deliver evidence for the above hypotheses, particularly inspired by the research design of Selb and Munzert (2018), Homola, Pereira and Tavits (2020), and Gessler, Tóth and Wachs (2021). The seven attacks on Roma during the operation of the terrorists have taken place across a range of localities in the NUTS2 regions of Northern Hungary, the Northern Great Plains, and Central Hungary. I utilize this spatial variation to estimate whether exposure to the attacks, proxied as geographical distance to the affected localities, had a positive effect on Jobbik's vote share. Estimating spatial spillover effects is a customary practice in the literature on the political effects of violence and terrorism and have been employed among others by Berrebi and Klor (2008), Selb and Munzert (2018), Homola, Pereira and Tavits (2020), and Bove, Efthyvoulou and Pickard (2021). Nevertheless, with a few notable exceptions, such as Getmansky and Zeitzoff (2014) and Baccini et al. (2021), this literature has not addressed the endogeneity arising from the fact that incidents of terrorism and hate crime are endogenous to spatial covariates of electoral performance. Even with statistical controls, falsification tests (Berrebi and Klor, 2008), and propensity score matching on observables (Selb and Munzert, 2018; Gessler, Tóth and Wachs, 2021), some unobserved covariates, such as pre-existing ethnic tensions, are difficult to account for. In the case of Roma murders in Hungary, the culprits are known to have selected some of their targets based on news reports of crimes committed by Roma, which in some cases, such as in the village of Tatárszentgyörgy, have already spurred radical-right protest before the assault (MTI, 2010; Miklósi, 2011). Given that target selection was endogenous to locality characteristics, simply comparing targeted areas to all other localities would therefore only reflect omitted variable bias.

The Hungarian electoral system is a mixed one, where voters cast a double ballot on a proportional party list on the one hand, and a candidate in a single-member constituency on the other hand. Until 2010, the representatives from constituencies were elected in two rounds and the party lists were unique to each NUTS3 region (Benoit, 1996). To minimize

bias introduced from strategic voting and candidate characteristics in constituency-level first-past-the-post voting, this study only focuses on Jobbik's results obtained on the party lists. In addition, focusing on party lists allows comparisons with the 2009 European Parliamentary elections, which also follow a proportional, list-based procedure, albeit with a single national list per party, instead of multiple regional ones. In 2006, Jobbik was running in an electoral coalition with another radical right-wing party, MIÉP (Party of Hungarian Truth & Life). Therefore, vote shares both in 2006 and 2010 include votes that were cast for MIÉP. However, by 2009-2010, MIÉP was in utter demise: it did not run in the 2009 EP elections, and in the national elections it only nominated a candidate list in one of the regions included in our analysis and only captured 1300 votes nationally. This suggests a degree of exchangeability of Jobbik and MIÉP voters.

6.1. Locality-level analysis

In the quasi-first stage analysis, I run OLS regressions to estimate the interaction effect of distance to the attacks and a post-attack dummy on vote shares of Jobbik in the two elections at the level of localities. I have opted for an analysis conducted at the level of localities, given that the distribution of precincts changed substantially between the two elections so that precinct-level data cannot be considered a true panel, while the negligible number of changes in localities allow for a true panel estimation. I will estimate the following OLS regression,

$$Y_{i,t} = \beta_0 + \delta_0 Z_t + \beta_1 D_i + \delta_i Z_t D_i + \gamma_{i,t} + \phi_i + \epsilon_{i,t},$$

where Y_i is Jobbik vote share on the proportional party list at locality i at time t measured on a 0-100 scale, Z_t is a dummy indicator for post-treatment with $1 \mid 2009 - 2010; 0 \mid 2006$, D_i is the shortest geographic distance (Haversine distance) from any locality where an attack took place, $\gamma_{i,t}$ is a vector of time-variant control variables at the level of localities and ϕ_i is a locality-level fixed effect. The interaction term is supposed to capture the differential effect of distance on the Jobbik vote prior to and after the attacks.

Therefore, this specification is basically equivalent to a differences-in-differences estimation with a continuous treatment variable (Callaway, Goodman-Bacon and Sant'Anna, 2021). The analysis is limited to localities at most 100 km away from the nearest targeted village, situated with the NUTS1 regions of Eastern and Central Hungary, excluding the capital, Budapest. Nevertheless, statistical controls are crucial here, since the effect of the distance variable is most probably endogenous to other key predictors of Jobbik vote, such as Roma population share.

Given that the targeted locations were not selected randomly, locality-level fixed effects take care of the time-invariant economic and socio-demographic characteristics of each locality, standardized by resident or permanent population depending on how the unstandardized value was recorded. I control for time-varying factors too, including the share of the population in unemployment, the share of unemployed beyond 180 days, the share of unemployed with primary education or less, companies with legal personality per capita, retail businesses per capita, log personal income tax revenues per capita, log income and expenditure per capita of local governments, personal offenses per capita, crimes against property per capita, mortality rate, log population per capita, emigrations per capita, the share of residents above 60 and below 14, and voter turnout. While Roma population share is arguably a key predictor of Jobbik's success, I do not have access to time-variant data on ethnic composition at the locality level. However, given that population composition tends to change only slowly over longer periods, the locality-level fixed effects presumably controls for it.

Importantly, the specific context of the 2008-2009 attacks also allows for a causal identification strategy in a quasi-experimental framework beyond the simple distance-based analysis. After the terrorists were identified and captured, the police uncovered evidence that they were planning to attack Roma at additional localities across the same geographic area. Given that the localities to attack were presumably not selected randomly, under the assumption that the terrorists selected their targets homogeneously over time, these additional four localities of unrealized attacks should share the same unobserved characteristics with those where the attacks actually took place. Thus, they can be used

as the control group in the natural experiment. Following Selb and Munzert (2018) and Gessler, Tóth and Wachs (2021), to create control and treatment groups, I selected treated and control localities that are found within 15 ± 5 km from the localities of the planned attacks. To ensure that my control zone does not include localities that could have been affected by their proximity to the attacks, localities in overlapping areas are relegated to treatment zones. In addition, inspired by Selb and Munzert (2018), I also designate buffer zones in a 5 km radius around the treatment zones. Control zone localities found in the buffer belts are excluded from the analysis (Figure 2.). Then, I conduct a post-treatment comparison of group means 2009 and 2010 with control variables with OLS regressions, where $D_{i,s} = 1 \mid treated; 0 \mid control$.

$$Y_{i,s} = \beta_0 + \beta_1 D_{i,s} + \gamma_i + \epsilon_{i,s}$$

Variation in exposure to the attacks in control and treatment zones is not completely independent of some observable covariates of Jobbik's success. Descriptive statistics for the pre-treatment election year of 2006 with F-tests of the mean differences between treatment and control groups are found in the Appendix. Table A2 demonstrates that there are pre-existing differences of some variables including measures of crime and unemployment. Therefore, the controls for observables are remain important in the post-treatment comparisons.

To account for pre-existing differences between localities, I also conduct a difference-in-differences analysis of the treatment effect of attacks on the locality-level Jobbik vote in treated vs untreated localities between 2006 and 2009-2010 based on the following regression specification:

$$Y_{i,s,t} = \beta_0 + \delta_0 Z_t + \beta_1 D_{i,s} + \delta_1 Z_t D_{i,s} + \gamma_{i,t} + \phi_i + _{i,s,t},$$

where $Y_{i,s,t}$ is the Jobbik vote share on the party list in locality i, at treatment area s, at election t, $Z_t = 1 \mid 2009 - 2010; 0 \mid 2006,$ and ϕ_i is the locality-level fixed effect. These

analyses are also conducted without the assumption of a spatial spillover effect, with a very limited sample of treated and control localities, excluding the respective treatment and control zones in the radii around them. Robust standard errors across these models are clustered at the level of localities, except for the cross-sectional-only post-treatment comparison models. Since only very few localities received the treatment after the 2009 parliamentary elections, it makes no sense to create an event study with two treatment periods. Therefore, I will stick to estimating separate models for the 2009 and 2010 election results.

6.2. Individual-level analysis & potential mechanisms

To test for the presence of an individual-level effect, as well as for the heterogeneous effect of exposure to the attacks based on ethnicity grounded in social identity and issue salience theories, I run additional regressions. I use cross-sectional and repeated-sample survey data collected before and after the murders or trial milestones. These analyses estimate the effect of the distance from the attacks and treatment status on prejudice towards Roma and voting for the radical right with OLS and binary probit regressions, while being able to control for the ethnicity of respondents, among others. Changing prejudice against Roma would indicate that the causal mechanism behind the changes observed at the locality level is linked to social identity threat, spurring either the Black Sheep Effect against the misbehaving ingroup or increased justification for violence against the outgroup. This approach also allows me to clear away the threat of ecological fallacy, since aggregate locality-level results may reflect opposing trends among ethnic groups. Therefore, the following model is only tested on a subset of the Life Course Survey data containing non-Roma adolescents. To decide whether a respondent is Roma, instead of the less reliable self-identification, I classify adolescents Roma if any of their parents identified as Roma in the initial, 2006 wave of the panel. For the survey data analysis of the Jobbik vote intention, I estimate the following probit regression models with a continuous explanatory variable, as the post-treatment comparison of means without an

interaction term:

$$P(Y_{j,i} = 1) = \beta_0 + \beta_1 D_i + C_j + \gamma_i + \epsilon_{j,i},$$

where $Y_{j,i}$ denotes the probability of supporting Jobbik if "yes" = 1, D_i is the distance of the respondent's residence from the nearest attack location, C_j is a vector of time-invariant individual-level controls, and γ_i is the vector of locality-level controls. Standard errors across these models are robust to heteroscedasticity. A similar OLS regression is run on post-treatment Life Course survey data with anti-Roma prejudice as the continuous dependent variable, again only on non-Roma individuals. Anti-Roma prejudice is the standardized mean of multiple questions on attitudes towards Roma. Beyond the locality-level controls described above, I also control for the binary variable of intention to participate in the elections.

Finally, to test for the alternative hypothesis entailing that the attacks increased the salience of intergroup threat and thus shifted voters towards Jobbik – the party having the strongest ownership claim for the Roma issue – I make use of the variation in public and media attention to the anti-Roma attacks over time. I select two time points in the aftermath of the attacks which are associated with increased media attention and thus presumably with heightened salience nationally. As Google search engine data demonstrates, three events related to the anti-Roma terror spree are associated with a sudden spike in Google searches (Figure 3.), which presumably signals the increased salience of the murders in a subset of the general public (Mellon, 2013). Importantly, previous research conducted on data from the US shows that weekly Google search frequency concerning terrorism and ethnic relations correlates strongly with representative survey results about the public salience of the same issues (Mellon, 2014).

The dates of these three sudden increases in interest in the anti-Roma murders correspond to the capture of the attackers in August 2009, the beginning of their trial in March 2011, and the end of their trial and subsequent sentencing in August 2013. Given that the August 2009 capture happened shortly after the last attack, the independent effect of salience apart from the murders is not identifiable there, especially because I do not have

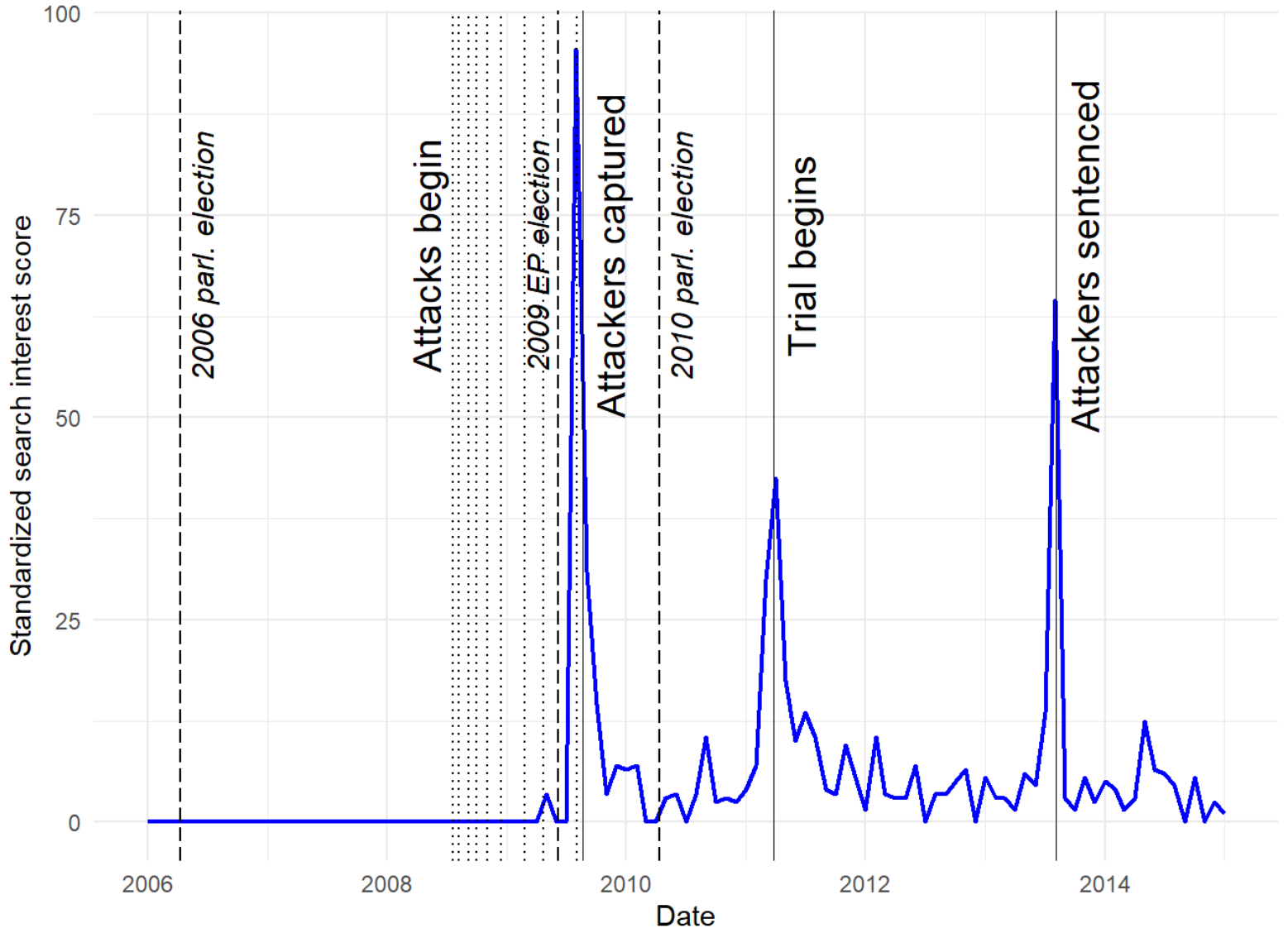

Figure 1: Hungarians Google search trends for keyword "Roma murder" ["Romagyilkosság"] in singular and plural form. The y axis displays the mean value of Google Trends' standardized search interest score for the two forms.

access to survey data that was collected before the capture, but already succeeding the last attack (Figure 1). Therefore, I will focus on the latter two presumed increases in salience, relying on the analysis of the Omnibus survey data. These events are assumed to be exogenous to trends in Jobbik's popularity. In contrast to the previous regression models using survey data, for this analysis, I am making use of the full samples instead of just subsets.

To estimate the effect of increasing salience, I use data from four survey waves overall, preceding and following the milestones in the court case. To estimate the effect of increases in salience on the intention to vote Jobbik, I estimate the following probit regression:

$$P(Y_{i,j,t,g,k} = 1) = \beta_0 + \delta_0 Z_t + C_j + \gamma_i + \phi_k + \theta_g + \epsilon_{i,j,t,g,k},$$

whereby $\delta_0 Z_t$ captures the effect of the binary dummy for post-treatment increased salience. It is an indicator for the survey waves collected after the beginning and the end of the trial in April 2011 and September 2013, respectively. ϕ_k is the county-level fixed effect, θ_g is a binary dummy included to capture year-specific effects in 2011 and 2013. Due to the fact that different sets of localities are included in each wave, I do not include locality-level fixed effects in these models.

6.3. Robustness checks

I also conduct robustness tests of the initial results in several ways. First, I run the same DiD regressions with treatment and control zones delineated in 10 and 20 km radii around included localities. Second, to account for the lower sample size in the quasi-experimental control group based on unrealized target locations, an alternative difference-in-differences analysis will compare Jobbik vote shares to a control group of untreated localities found outside the treatment and buffer zones within NUTS3 regions where attacks were planned or took place. This control group was selected through optimal pairwise matching on the Mahalanobis distance on pre-treatment Jobbik vote share and a range of other covariates using the MatchIt R package (Stuart et al., 2011). Control zones are constituted of the

localities in 15 km radii around them (Figure 3.). Matching was conducted on the same variables used as statistical controls.

Moreover, I also conduct a robustness test by comparing localities based on whether the attack was successful, i.e. if it resulted in deadly casualties. This quasi-experimental strategy is a standard way to estimate the causal effect of terror attacks and assassinations in the economics and political science literature since the deadly attacks are expected to be more salient and provoke a stronger public reaction (Jones and Olken, 2009; Brodeur, 2018; Baccini et al., 2021; Turkoglu and Chadefaux, 2022). The Global Terrorism Database also defines an attack against people as successful, when at least one person died as a result (START, 2021). However, it requires the assumption that the success of an attack is quasi-random, depending on unpredictable factors (e.g. if a bomb explodes at the right time or a bullet hits the target), and is independent from the outcome of interest. While in some cases this may be a reasonable assumption, in the case of the Hungarian anti-Roma attacks, there is some reason to doubt so. While the initial attacks in 2008 did not have any deadly casualties, the attacks resulting in murders, except for one, were the final ones in the series of assaults, having taken place from February 2009 until the capture of the attackers in August 2009. This could imply that initially, the terrorists were not planning to kill their targets, and only wanted to cause confusion, but switched strategies halfway through their campaign. This could have influenced target selection. In addition, given the clear time-trend in the style of attacks, the estimated coefficients may be biased towards a lower magnitude, since behavioral effects usually wane away over time. Finally, this model is based on the lowest sample size of all locality-level ones given the small number of targets included. Therefore, I find these estimates less trustworthy, and only indicative of the overall robustness of the main estimation strategy. To estimate causal effects, control and treatment zones are established around the localities where attacks resulted in deadly casualties (Figure 4).

Unfortunately, the parallel trends assumption of the differences-in-differences analysis cannot be properly tested, as Jobbik did not emerge as an electoral contender before 2006. Matching on pre-treatment outcomes as described above constitutes one of the attempts

to introduce parallel trends that may not be present in the control group selected through the natural experiment (Ham and Miratrix, 2022). To examine whether the assumption of parallel trends holds, I also run falsification tests on the results of MIÉP in the 2002 parliamentary election and 2004 European Parliamentary election. While the two parties diverged on many issues, and anti-Roma rhetoric was more central to Jobbik's program, the two parties shared a similar ethnonationalist ideology. While somewhat more focused on anti-Semitism, MIÉP has made use of anti-Roma rhetoric more often than other parties in the early 2000s (MTI, 2002; Félix and Vásárhelyi, 2021), while its supporters were the most prejudiced against Roma according to survey data (Enyedi, Fábián and Sik, 2005).

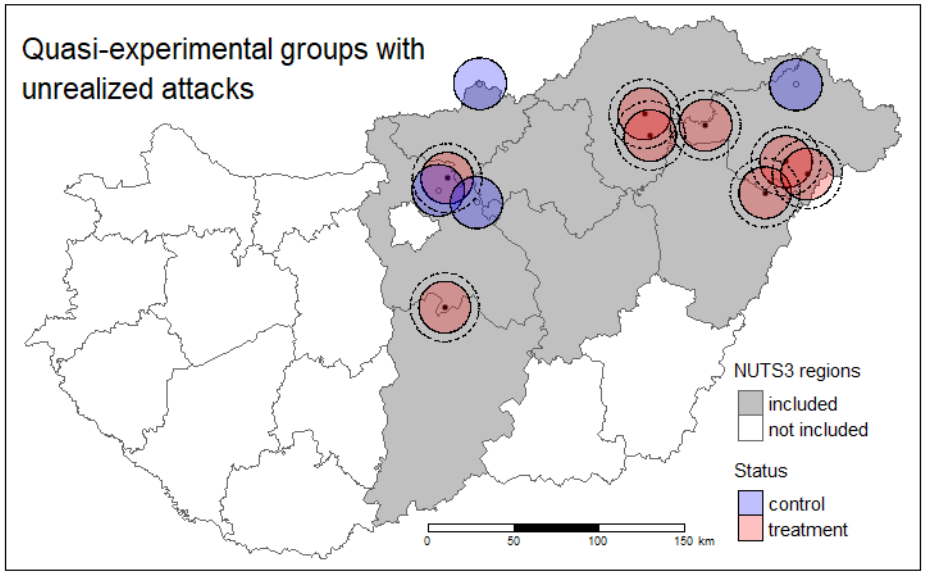

Figure 2: Map of Hungary with treatment and control groups based on the natural experiment with unrealized attacks. Dots designate attack locations and colored circles the treatment and control zones. Dashed circles represent the buffer zones around the treatment areas.

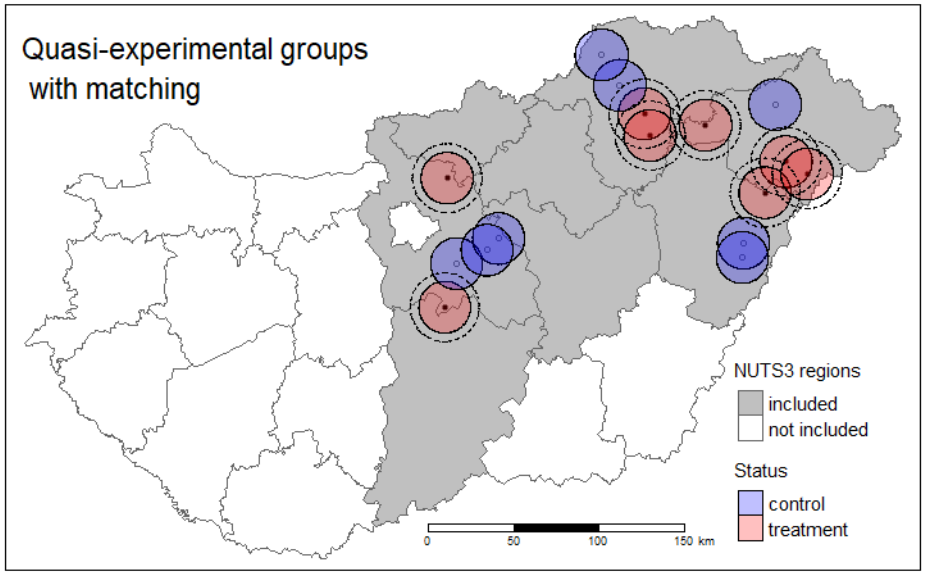

Figure 3: Map of Hungary with treatment and control groups based on optimal pairwise Mahalanobis distance matching. Dots designate attack locations and colored circles the treatment and control zones. Dashed circles represent the buffer zones around the treatment areas.

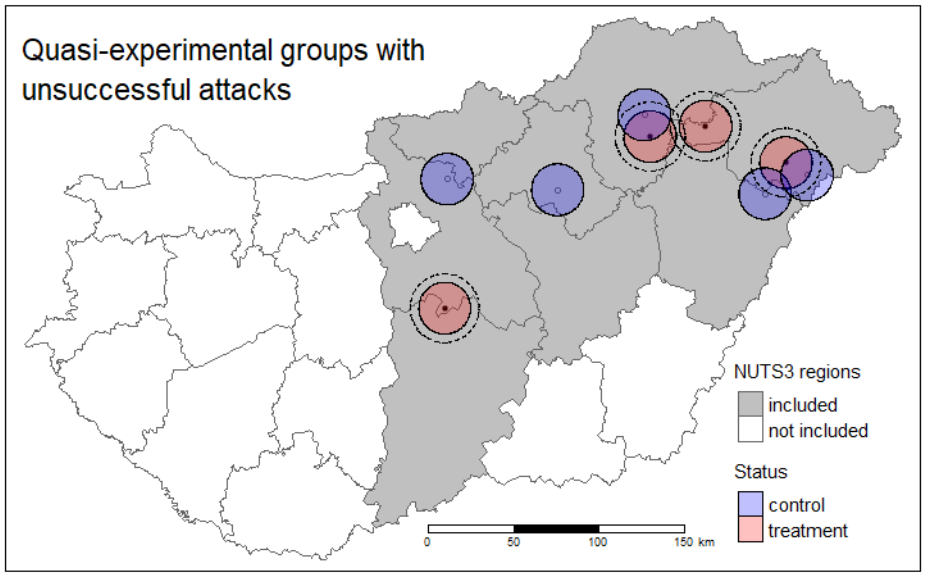

Figure 4: Map of Hungary with treatment and control groups based on the natural experiment with terrorist success. Dots designate attack locations and colored circles the treatment and control zones. Dashed circles represent the buffer zones around the treatment areas.

7. Main results

7.1. Continuous treatment

Regression results with a continuous treatment variable – geographic distance to the closest attack location – are presented in Table 1. The consistently negative and statistically significant interaction terms demonstrate that the regression slope of the distance from attack locations is significantly steeper in 2009 and 2010 compared to 2006. In 2009-2010, localities lying closer to the attack locations were more likely to support Jobbik than in 2006, all else held equal. Given that Jobbik rose to national prominence between 2006 and 2009-2010, the on average better results in the latter years are unsurprising. The distance to these localities is presumably not independent of the Jobbik vote, but the estimates are fairly consistent with the inclusion of controls.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment	17.944***	14.341***	22.315***	22.937***
(2009 / 2010)	(0.482)	(1.358)	(0.491)	(0.655)
Distance from nearest attack *	−0.062***	−0.073***	−0.058***	−0.077***
Post-treatment	(0.010)	(0.012)	(0.010)	(0.010)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.848	0.856	0.884	0.904
Adj. R^2	0.695	0.706	0.768	0.800
nobs	2656	2528	2658	2572
Log Likelihood	−7264.116	−6851.571	−7408.827	−6901.596

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 1: OLS regressions with continuous distance measure from closest attack location. The sample is limited to localities within maximum 100 km distance, located within Eastern Hungary. The main effect of distance is omitted from the equation due to collinearity with the locality fixed effects

The results from Table 1 are confirmed by a simple visual analysis as well. Figure 5 shows that the association of Jobbik's vote share with the shortest geographic distance was more negative in 2009 and 2010 than in 2006. The two slopes are almost parallel,

with a lower intercept in 2009. However, these models were fitted without controls or fixed effects.

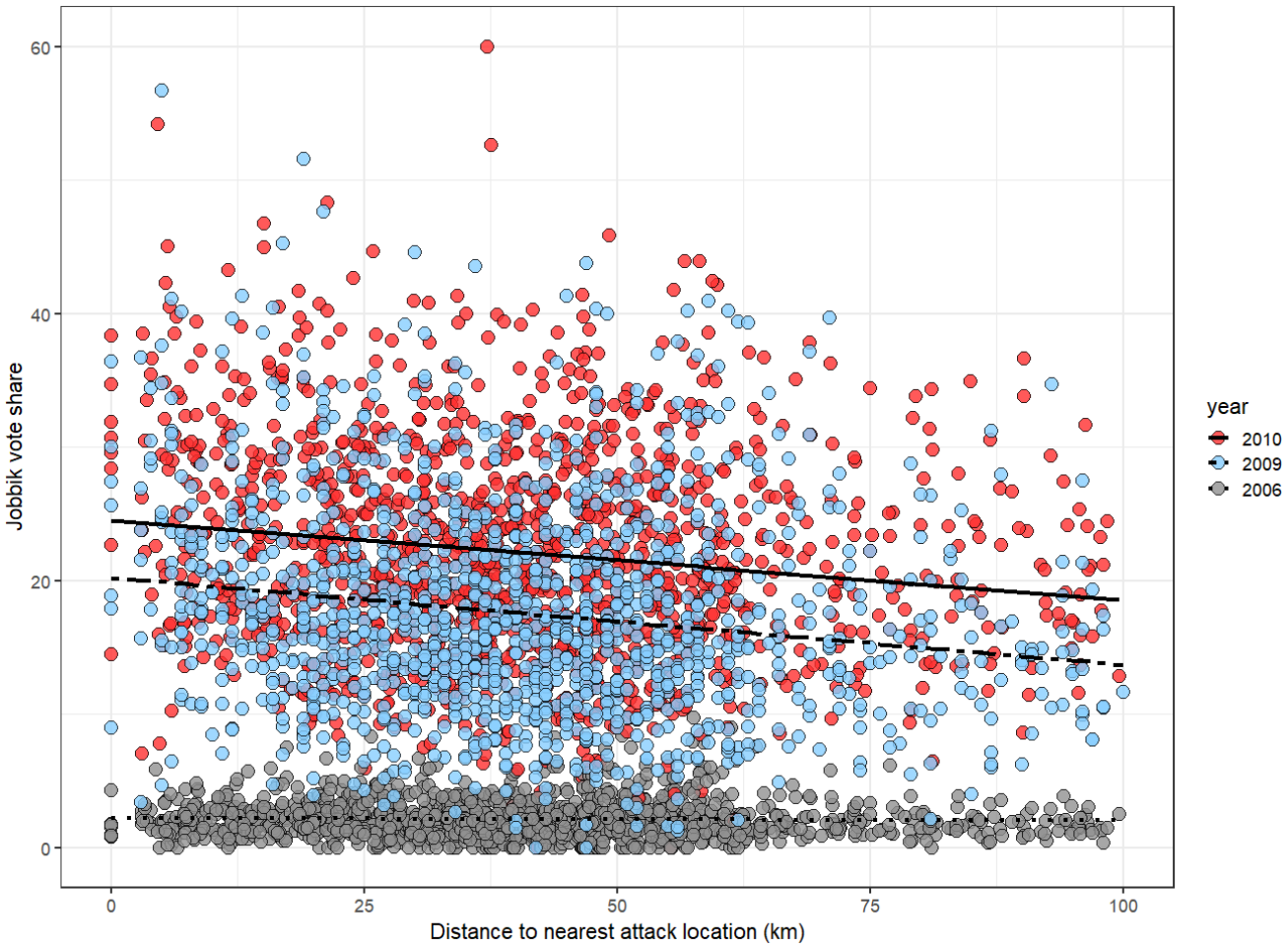

Figure 5: Association of the Jobbik vote in different elections with distance from locations of attacks

7.2. Post-treatment comparison & differences-in-differences

The differences-in-differences regressions utilize the inherent randomness of the capture of the terrorists, who could thus not proceed with attacks on some localities. In contrast to the previous estimation, this introduces exogenous variation, allowing for the estimation of average treatment effects.The results confirm the positive effect exposure to the attacks has had on voting behavior.

First, Tables 2 and 3 below show simple post-treatment comparisons of treated and control localities in 2009 and 2010, with and without spillover effects. In the latter, the sample is

restricted to the eight localities where attacks took place and four others where attacks were planned. In all models regardless of the specification, the coefficient of exposure to attacks is positive. While thanks to the low sample size, standard errors are very large in Table 2, all coefficients are statistically significant in Table 3. The estimates are stable across models, with an estimated increase in the Jobbik vote at around 2-4 percentage points.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Attacked	3.024	17.847	7.287	17.136
	(3.069)	(35.330)	(3.958)	(16.291)
locality FE	no	no	no	no
controls	no	yes	no	yes
clustered SE	no	no	no	no
R^2	0.034	0.353	0.173	0.637
Adj. R^2	−0.062	−2.557	0.090	−0.998
nobs	12	12	12	12
Log Likelihood	−40.258	−37.854	−40.197	−35.263

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 2: Post-treatment comparison with OLS regressions without spillover effects. The sample only contains (planned) attack locations.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Attacked	3.478***	4.201***	2.260*	3.250**
	(1.003)	(1.202)	(1.089)	(1.075)
locality FE	no	no	no	no
controls	no	yes	no	yes
clustered SE	no	no	no	no
R^2	0.045	0.107	0.018	0.124
Adj. R^2	0.041	0.065	0.013	0.084
nobs	203	203	207	207
Log Likelihood	−696.872	−690.101	−716.207	−704.301

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 3: Post-treatment comparison with OLS regressions with spillover effects within 15 km radii around treatment & control localities. Control group selected from localities around unrealized attack locations. Standard errors are robust to heteroscedasticity

Table 4. displays the results from a baseline estimation without spillover effects. The interaction term capturing the treatment effect is positive for voting across specifications, which implies that the attacks have increased Jobbik's support in affected localities by 4-11 percentage points. Nevertheless, these highly variant estimates are not statistically significant, presumably also owing to the small sample size unsuitable for probabilistic inference.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	16.631***	−78.403*	17.821***	26.899*
	(0.834)	(26.580)	(2.979)	(11.592)
Attacked * Post-treatment	3.514	10.576	7.777	10.541
	(2.955)	(6.037)	(4.131)	(5.719)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.903	0.981	0.923	0.989
Adj. R^2	0.777	0.782	0.824	0.878
nobs	24	24	24	24
Log Likelihood	−63.322	−43.752	−64.794	−41.065

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 4: Difference-in-differences OLS regressions without spillover effects. The sample only contains (planned) attack locations. The main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

Presumably, however, the effect of exposure to the attacks was not limited to municipalities where the attacks took place. Therefore, I also estimate difference-in-differences models with the assumption of spatial spillover effects, since people residing in areas in the vicinity of attacks were presumably also impacted through less immediate exposure. The inclusion of localities within a 15 km radius around treatment and control settlements allows for larger sample size and thus a more valid probabilistic inference too. Localities included in these regressions are those assigned to treatment and control zones as shown in Figure 2. Detailed descriptive statistics and F-values are found in the Appendix.

Table 5 contains the main estimates of this paper from the DiD analysis with spillovers assumed in a 15 km radius around each affected locality. The results demonstrate that the

interaction coefficient capturing the treatment effect is positive. While the post-treatment comparison of means displayed above may suffer from omitted variable bias, the DiD analysis provides more conservative estimates of the positive causal effect. Compared to the control zone around localities where attacks were planned but never realized, localities in the treatment group saw on average approximately 3-5 percentage point higher increase in Jobbik's vote share in the examined period, all else held equal. These estimates are significant at a 99% confidence interval even when time-variant controls are included. The inclusion of controls is associated with a lower treatment effect, but the estimates are still remarkably stable compared to the analysis of the limited sample of (planned) attack locations. The validity of the identification strategy is supported by the similarity of the comparison-of-means in Table 3 and the DiD estimates in Table 5. In both cases, the estimated treatment effect is lower in 2010 than in 2009, implying a decreasing effect over time.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	15.295***	12.101***	20.579***	25.458***
	(0.671)	(2.437)	(0.844)	(1.375)
Attacked * Post-treatment	3.985***	4.992***	2.941**	3.278**
	(0.969)	(1.046)	(1.088)	(1.041)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.878	0.898	0.905	0.929
Adj. R^2	0.755	0.778	0.809	0.845
nobs	406	406	414	413
Log Likelihood	−1104.141	−1068.724	−1149.701	−1085.286

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 5: Difference-in-differences OLS regressions with spillover effects within 15 km radii around treatment & control localities. Control group selected from localities around unrealized attack locations. The time-invariant main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

7.3. Individual-level analysis

The main analysis was conducted at the level of localities, with a full population of localities that were included in any given specification. However, macro-level analyses like this one are typically prone to the ecological fallacy, whereby aggregated results mask contradictory individual-level developments. In this case, we do not know anything about the heterogeneous effect of exposure to the attacks on voter behavior across ethnic groups. Presumably, ethnic Roma and non-Roma Hungarians reacted to the attacks differently. It is especially unlikely that exposed Roma would increase their support for the radical right after their co-ethnics were harmed by right-wing extremists.

The following section contains estimates from the Hungarian Life Course survey. Although the Life Course survey has only a single wave from before the 2010 elections which contains political questions, its large sample size allows for more credible estimates with a geographically restricted sample. Given that the results from the above locality-level post-treatment comparisons in Tables 2 & 3 closely align with the results of the more conservative DiD analyses in Tables 4 & 5, it can be assumed that omitted variables do not constitute a grave threat here. However, since the data is time-invariant, locality-level fixed effects are not possible here.

	Jobbik vote intention	Jobbik vote intention	Jobbik vote intention	Jobbik vote intention
Attacked			0.085	0.163
			(0.253)	(0.239)
Distance from nearest attack	−0.005*	−0.005		
	(0.002)	(0.003)		
controls	no	yes	no	yes
pseudo.r.squared	0.010	0.154	0.007	0.730
nobs	3071	3029	546	545
Log Likelihood	−179.397	−153.300	−11.211	−3.053

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 6: Individual-level post-treatment probit regressions with distance from attacks and comparison of group means For the latter models, the control group was selected from localities around unrealized attack locations. Data comes from the Life Course survey, with a sample limited to non-Roma adolescents.

Table 6 shows the results from the post-treatment only comparisons from the Life Course survey. The survey analysis could only confirm that distance from the attacks is inversely related to the intention to vote for Jobbik among non-Roma adolescents. However, the estimates are not statistically significant with the inclusion of controls. The survey did not deliver strong evidence for the increased support for Jobbik in treated localities compared to those in the control group: the coefficients are positive but insignificant. However, given the limited sample size of 600 observations for this subset, only limited conclusions can be drawn from these models. The survey was not supposed to be representative at the locality level, and the variation in the main explanatory variable in the continuous distance models is relatively small, as multiple respondents are selected into the survey from the a small number of localities.

8. Mechanisms

I am testing for two potential mechanisms behind the causal effect from the locality-level analysis. First, I test for the post-treatment association of anti-Roma prejudice and exposure to the attacks with data from the Life Course survey. In Table 7, the results align with those from the results with the locality-level analysis. Living further away from the murders is associated with lower prejudice against the Roma, while living in a treatment zone, is associated with increased prejudice. This latter estimate exceeds the standard error with the inclusion of controls. This implies that the attacks could have increased prejudice among the non-Roma.

	Anti-Roma prejudice	Anti-Roma prejudice	Anti-Roma prejudice	Anti-Roma prejudice
Attacked			0.127	0.260
			(0.139)	(0.161)
Distance from nearest attack	−0.002	−0.003		
	(0.001)	(0.002)		
controls	no	yes	no	yes
R^2	0.003	0.065	0.004	0.093
Adj. R^2	0.003	0.060	0.002	0.065
nobs	3072	3011	545	539
Log Likelihood	−4201.591	−4104.662	−714.487	−702.113

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 7: Individual-level post-treatment probit regressions with distance from attacks and comparison of group means. For the latter models, the control group was selected from localities around unrealized attack locations. Data comes from the Life Course survey, with a sample limited to non-Roma adolescents.

Table 8. displays the results from estimations of the effect of increased salience on Jobbik's popularity among respondents. Regardless of specification, the effect of increased salience on Jobbik's success was negative and not significant at a 95% confidence interval, albeit exceeding standard errors with more stringent controls. Due to the sparse data from these years, I did not include all locality-level controls in these models. The same trend is displayed on Figure 6, just showing the sample averages of Jobbik support before and after the start and the conclusion of the murderers' trial.

Overall, the results of this chapter suggest that increased salience of the attacks was not associated with increased support for Jobbik, but the attacks did increase anti-Roma prejudice among exposed people. This may imply that the majority population attributed blame to the victimized Roma, possibly justifying violence against them through reference to anti-Roma stereotypes in response to social identity threat.

	Jobbik vote intention	Jobbik vote intention	Jobbik vote intention
Post-treatment	−0.054	−0.074	−0.104
	(0.071)	(0.064)	(0.106)
county FE	yes	yes	yes
year FE	yes	yes	yes
controls	no	individual	individual & locality-level
clustered SE	county-level	county-level	county-level
pseudo.r.squared	0.017	0.062	0.094
nobs	3554	3450	1818
Log Likelihood	−1085.497	−1003.831	−522.571

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 8: OLS regression comparisons before after the start of the trial the sentencing of perpetrators with fixed effects for years and post-treatment.

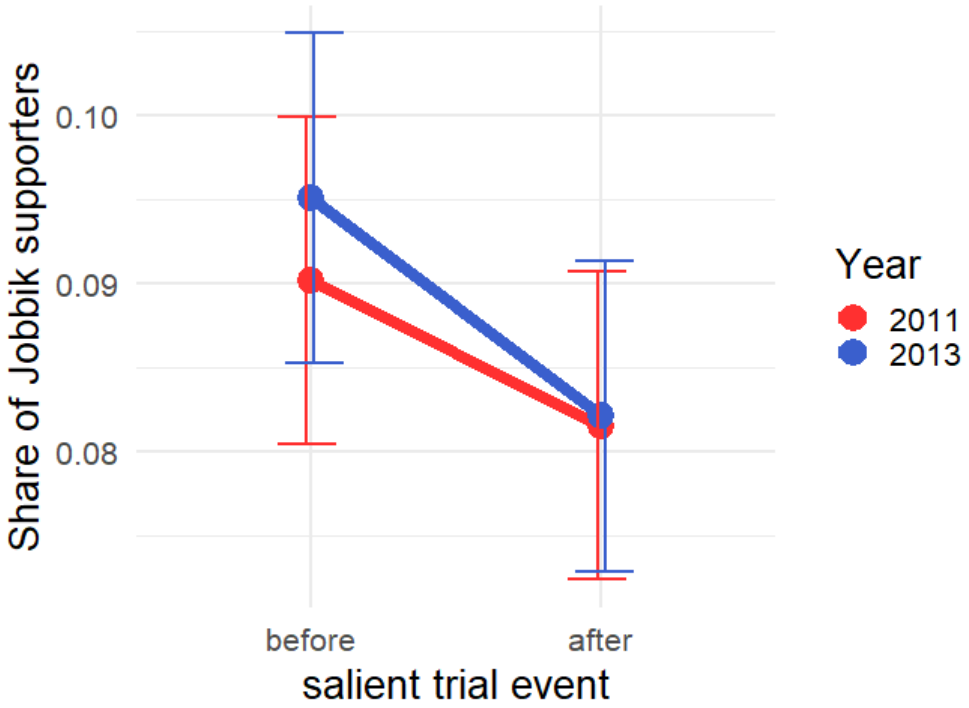

Figure 6: Jobbik vote share in the Omnibus surveys before and after salient trial events

9. Robustness checks

The main estimates obtained above are valuable, but prone to certain biases. The natural experiment utilized above is based on information from police press statements, which cannot be confirmed independently, since the documentation of the court case and the evidence is not declassified for researchers yet. Moreover, these models depend on the assumption that the target selection over time was independent of potential confounders. Finally, some of the control localities lie very close to treated ones even if they fall outside the somewhat arbitrary radii. With the estimation of spillover effects around the localities involved, this means that they could have been impacted by the exposure too, which may bias our results.

Thus, the locality-level DiD models of Table 5 were also estimated with alternative bandwidths, with treatment and control zone radiuses set at 10 or 20 km. The results, displayed in the Appendix, suggest that the main estimates hold under these alternative specifications: the treatment effect of exposure on the Jobbik vote is around 3-6 percentage points in 2009, and 2-4 percentage points in 2010, all else held equal. Nevertheless, the interaction terms of models without controls are not statistically significant in 2010, even though they are positive and exceed the standard errors. In line with expectations, the coefficients are of greater magnitude in the 10 km specifications than in the 20 km specifications: this could be explained by more immediate exposure to the attacks at locations closer to them.

Moreover, I selected an alternative control group of localities based on optimal pairwise matching on the Mahalanobis distance. I selected 8 control localities from all localities within the NUTS3 regions where attacks took place or were intended to take place, leaving me with the counties of Pest, Heves, Borsod-Abaúj-Zemplén, Hajdú-Bihar, and Szabolcs Szatmár-Bereg (see Figure 3.). The localities were selected based on pre-treatment outcomes and the same covariates used as controls in the regression models. I used standardized sociodemographic and economic variables, as well as Jobbik's pre-treatment vote share and turnout rate. The localities selected through matching were then used to assign

localities in 15 km radii around them into the control group, except for those found in the overlaps with the treatment zones or their buffers.

The dataset created through matching was used for the estimation of treatment effects in a DiD analysis with specifications akin to the ones in Table 5. The results are shown in Table 9. The estimated treatment effect on Jobbik's vote share in 2009 is remarkably similar to the estimates obtained from the natural experiment: without controls, the increase is 4 percentage points, whereas with the inclusion of controls, the coefficient is somewhat higher at 4.3 percentage points (instead of 5). Similarly, to the original estimations, the coefficients for 2009 are all significant. In the case of the 2010 interaction coefficients, however, the matched estimates are substantially lower with higher standard errors. The estimated treatment effect, however, still exceeds the standard error when controls are included. Similarly, to the estimates from Table 5, Table A3, and Table A4, this implies that the behavioral effect of attacks wanes away over time – the 2010 elections took place 8 months after the last attack, while the EP elections were immediately preceded by them.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	15.346***	13.502***	22.957***	25.631***
	(0.592)	(2.766)	(0.617)	(1.226)
Attacked * Post-treatment	3.933***	4.380***	0.563	1.141
	(0.915)	(0.913)	(0.922)	(0.895)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.869	0.884	0.910	0.928
Adj. R^2	0.736	0.752	0.819	0.843
nobs	548	547	566	553
Log Likelihood	−1504.732	−1464.860	−1569.782	−1469.454

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 9: Difference-in-differences OLS regressions with spillover effects within 15 km radii around treatment & control localities. Control group selected with optimal Mahalanobis distance matching of localities with realized attack locations. The main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

The robustness of the initial estimates was also checked through the locality-level comparison of treatment and control zones around locations of successful and unsuccessful attacks, respectively, based on the assumption that the deadliness of an assault is independent of electoral outcomes. Given the apparent time-trend in the attacks' success, with most deadly ones taking place in 2009, the coefficients are probably biased towards larger magnitude, since they compare attacks that happened closer to the elections with ones that happened as far as almost two years prior to the 2010 election. As Table 11 shows, the estimated treatment effects are indeed quite high around 5 percentage points.

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	17.346***	8.247	21.190***	24.214***
	(0.717)	(4.279)	(0.790)	(2.185)
Attacked * Post-treatment	5.188***	5.468***	5.247***	5.175***
	(1.391)	(1.267)	(1.214)	(1.207)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.879	0.905	0.916	0.933
Adj. R^2	0.756	0.788	0.830	0.849
nobs	314	314	322	321
Log Likelihood	−877.430	−839.511	−888.848	−849.767

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 10: Difference-in-differences OLS regressions with spillover effects within 15 km radii around treatment & control localities. Treatment and control groups selected from localities around attack locations based on whether the attack was successful (i.e. if it resulted in deadly casualties).The main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

Finally, I also test for parallel pre-trends of treatment and control zones as employed in the main specifications of Table 5. To do so, I conduct two falsification tests estimating the difference in the change of the vote shares of MIÉP, Hungary's largest far-right party before the appearance of Jobbik. In the following set of models, I estimate the effect of the placebo treatment on MIÉP's vote share in the 2002 parliamentary and the 2004 European parliamentary elections, followed by the comparison of the vote share in the 2004 European Parliamentary and the 2006 parliamentary elections. Due to the sparse

data from this period, these models are estimated with fewer controls. The results in Tables 11-12 show that the effect of the placebo is negative and much smaller than the treatment effects estimated after 2006. igures A1 and A2 in the Appendix display these interaction coefficients that capture the treatment effect over pairs of years. In terms of 2009 treatment groups, the placebo is significant for 2002-2004, as shown in Table 11. In Table 12 displaying models with 2010 treatment boundaries, treatment effects are not significant with a 95% confidence interval, except for the 2004 model with controls. Overall, this amounts to a violation of the parallel trends assumption. Nevertheless, given the opposite sign of the effect compared to the period of interest, assuming that the same trends continued with Jobbik too, a violation of the assumption should have only increased the chance of a Type II error by decreasing the magnitude of the positive coefficients of Table 5. Given that the hypothesis was still confirmed, the divergence from parallel trends was too low to pose a serious threat to internal validity. Figures 7 and 8 demonstrate the same time trend in the form of an event study with two-way fixed effects, merging MIÉP and Jobbik vote share data from 2002-2010. The clear time trend shows how far right parties benefited from exposure after 2006. The trends are not perfectly parallel given significant differences in 2002, but its direction direction is the opposite of the 2009-2010 effect. This implies that the positive effect on the Jobbik vote might have in fact been underestimated.

	MIÉP vote share 2004	MIÉP vote share 2004	MIÉP vote share 2006	MIÉP vote share 2006
Post-treatment (2004 / 2006)	−0.804***	0.132	−0.075	1.595
	(0.221)	(1.283)	(0.164)	(0.824)
Placebo treatment * Post-treatment	−0.711*	−1.020**	−0.139	−0.167
	(0.278)	(0.343)	(0.211)	(0.223)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.800	0.838	0.777	0.796
Adj. R^2	0.596	0.646	0.551	0.562
within.r.squared	0.327	0.455	0.015	0.104
nobs	402	396	402	401
Log Likelihood	−548.411	−499.987	−446.888	−427.217

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 11: Difference-in-differences OLS regressions with spillover effects within 15 km radii around placebo treatment & control localities. Control group selected from localities around unrealized attack locations by the 2009 parliamentary elections. The time-invariant main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

	MIÉP vote share 2004	MIÉP vote share 2004	MIÉP vote share 2006	MIÉP vote share 2006
Post-treatment (2004 / 2006)	−0.864***	0.173	−0.042	1.557
	(0.234)	(1.273)	(0.175)	(0.793)
Placebo treatment * Post-treatment	−0.558	−0.825*	−0.191	−0.171
	(0.284)	(0.341)	(0.215)	(0.225)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.798	0.834	0.779	0.797
Adj. R^2	0.592	0.639	0.554	0.565
within.r.squared	0.313	0.437	0.018	0.105
nobs	410	404	410	409
Log Likelihood	−559.657	−512.702	−452.998	−433.237

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table 12: Difference-in-differences OLS regressions with spillover effects within 15 km radii around placebo treatment & control localities. Control group selected from localities around unrealized attack locations by the 2010 parliamentary elections. The time-invariant main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

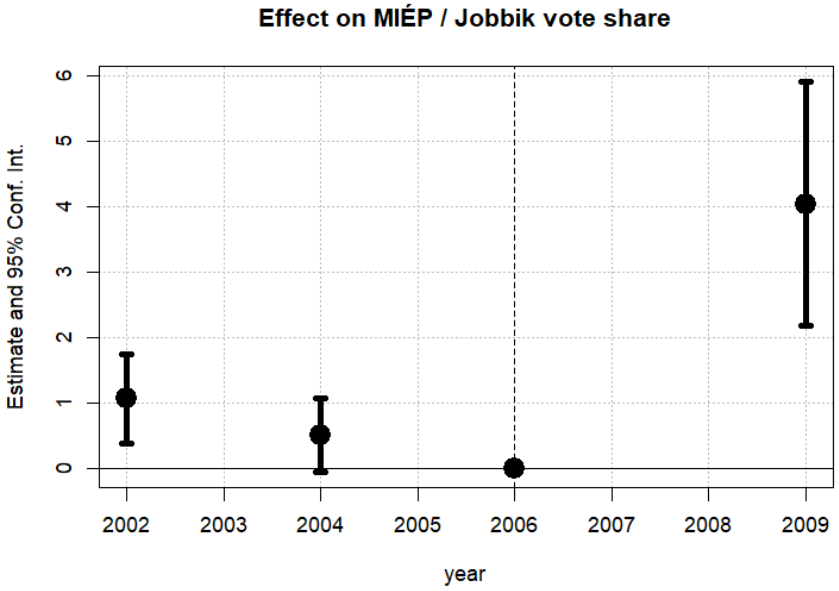

Figure 7: Coefficient plot of the interaction terms of an event study with MIÉP and Jobbik vote shares. The model was estimated without controls. 2006 is the reference year, treatment and control zones reflect the 2009 EP elections.

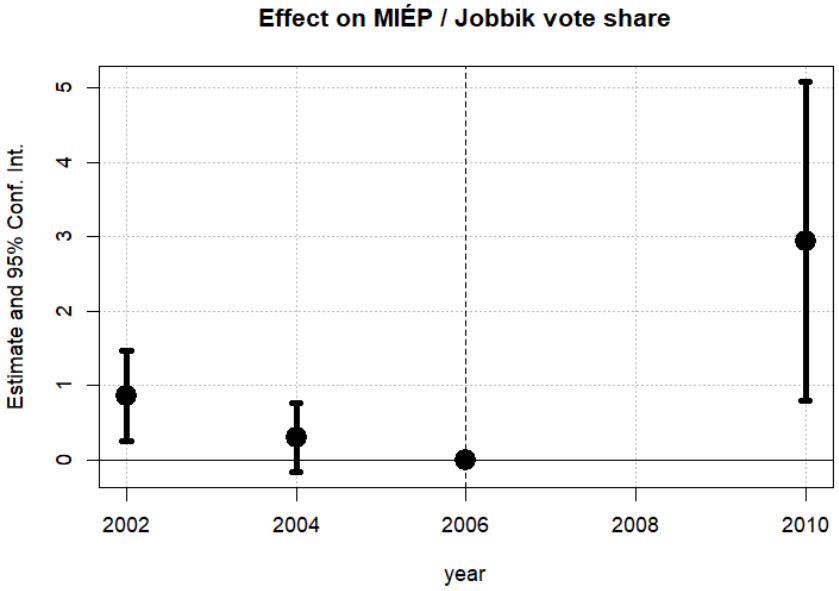

Figure 8: Coefficient plot of the interaction terms of an event study with MIÉP and Jobbik vote shares. The model was estimated without controls. 2006 is the reference year, treatment and control zones reflect the 2010 parliamentary elections.

Overall, the various robustness checks demonstrate that even if the estimated treatment effect is not perfectly consistent across models, the main results hold. Across various

alternative specifications, identification strategies, and falsification tests, the treatment effects are consistently positive around 1-6 percentage points, always significant for 2009, and somewhat lower and sometimes not significant for 2010.

10. Discussion

The regression models above estimated the impact of the anti-Roma terror attacks on the electoral performance of Jobbik in the European parliamentary election of 2009 and the parliamentary election of 2010. All models delivered support for the hypothesis that exposure to the attacks increased Jobbik's vote share, using different measures of exposure. These findings are robust to the inclusion of a range of time-variant locality level statistical controls. Distance from the localities that were attacked is significantly and negatively associated with Jobbik's vote share, with coefficients of interest ranging between -0.06 and -0.09. This means that between 2006 and 2009/2010, the further a locality is found from the locations of the attacks, the lower was the increase in the share of votes cast on the proportional party list for Jobbik. This estimate, however, cannot be considered causal. Localities lying closer to affected settlements presumably share similar characteristics with them, which could have been related to Jobbik's popularity. For example, in the Northeastern Hungary region, the counties of Borsod-Abaúj-Zemplén, Heves, and Szabolcs-Szatmár-Bereg with a dearth of economic opportunities and high Roma population share were Jobbik strongholds in both 2009 and 2010. Even though models in Table 1 control for observable covariates, including those usually seen as associated with Jobbik's success, such as unemployment and crime, the variation in the location of attacks is not truly exogenous.

To estimate a valid average treatment effect of the attacks, I, therefore, turned to the natural experiment offered by the quasi-random variation in which localities were attacked and which ones were targeted for later attacks that remained unrealized due to the capture of the attackers. Depending on the plausible assumptions that the National Bureau for Investigation's work was independent of the attackers' schedule and that the attackers selected their targets homogeneously over time, the post-treatment comparison of localities in "treated" and "control" zones offers the opportunity to estimate the ATE. However, given that there are statistically significant differences in group means on multiple covariates (see Appendix), I also estimated more conservative treatment effects

with differences-in-differences regressions. Both the post-treatment comparisons and the DiD regressions capture positive and statistically significant treatment effects. Remarkably, estimates from DiD and post-treatment regressions are fairly similar: exposure to the attacks increased Jobbik's popularity by 3-5 percentage points in 2009, and by 2-4 percentage points by 2010, depending on the set of controls included in the estimation. When limiting the sample to the much smaller set of locations of realized and unrealized attacks, the estimates are positive but mostly larger and less reliable with high standard errors. Generally, the inclusion of controls only increases the coefficients. The similarly positive, and mostly significant interaction terms from the DiD regressions based on different delineations of the treatment-control zones, as well as the regressions making use of a control group based on matching confirm these conclusions.

Overall, the results imply that the positive effect of the attacks on Jobbik's vote share was around 1-2 percentage points lower in 2010 than in 2009. This implies that the magnitude of the effect decreased over time since the 2009 election took place right after the attacks. This is in line with previous findings that similarly show quickly fading effects of terrorism on public opinion and voting. (Berrebi and Klor, 2008; Turkoglu and Chadefaux, 2022).

At the individual level, I find confirmation of the locality-level causal effect through the post-treatment comparison of means in the Life Course survey wave collected before the 2010 elections. While the Life Course survey did not allow for a DiD analysis, it is quite representative due to its large sample size, whereby the relevant subset of a single wave has more than 3000 observations.

Although the behavioral effect of extremist violence against minority outgroups has remained understudied, the results contradict the limited evidence that exists and shed doubt on the applicability of such findings beyond Western liberal democracies. Previous research on right-wing terror attacks in New Zealand (Shanaah et al., 2021; Byrne et al., 2022), Norway (Jakobsson and Blom, 2014), and the United Kingdom (Pickard et al., 2022) found that they decreased prejudice against immigrant minorities and diminished

support for right-wing authoritarian positions. Albeit some studies have shown milder or shorter effects for conservative voters who perceive a greater distance from minorities, no study so far has delivered evidence in favor of increased prejudice and support for radical right parties. Nonetheless, given the widespread anti-Roma prejudice and the connection between right-wing political positions and justification for violence against the outgroup that characterizes the Hungarian population, the positive treatment effect on Jobbik's popularity is not implausible at all (Kende, Hadarics and Lášticová, 2017; Faragó, Kende and Krekó, 2019). On the contrary, the analysis indicates that increased salience of the Roma issue was not the factor behind Jobbik's icreased popularity.

11. Conclusion

This paper attempted to fill a gap in the literature by estimating the effect of a series of deadly anti-Roma attacks by a group of right-wing extremists in Hungary on their effect on the 2009-2010 elections. The regressions results utilizing various causal inference methods suggest that the effect of the attacks on Jobbik's vote was positive, causing a 1-5 percentage point increase in its vote share in party-list-proportional voting depending on the election and the regression specification. Survey data analysis shows that exposure to the attacks was also associated with higher anti-Roma prejudice, implying that increased perceptions of social identity threat due to ingroup collective guilt could be the underlying causal mechanism. This theory is also supported by qualitative evidence from the anthropological literature (Szombati, 2018).

Methodologically, the paper offered a novel strategy to identify the causal effects of terrorism on electoral results. Previous research utilizing geographic variation in attacks mostly ignored the possibility that target selection by terrorists is endogenous to political outcomes, and the popular "interrupted survey"-style studies are limited to pre- and post-election representative surveys prone to respondents' biases, and sampling and measurement errors Muñoz, Falcó-Gimeno and Hernández (2020). The utilization of the presumably random variation in realized and unrealized attacks introduced by the exogenously induced capture of the attackers is a novel identification strategy that allows for the internally more valid estimation of spatial treatment effects. This strategy bears some resemblance to the one based on the utilization of the success of terrorists, used as a robustness check above. However, both identification techniques depend on important assumptions: either the selection of targets over time, or the success of terror attacks should be independent of local political outcomes. Given that police and intelligence agencies around the world often publish information about planned attacks after the capture of terrorists, the "unexecuted attacks" strategy might prove less labor-intensive than determining which attacks count as successful based on detailed contextual information, especially because there may be a bias towards reporting successful attacks.

Despite its important contribution, the external validity of this analysis and its applicability to right-wing extremist terrorism in Western Europe and the United States is limited. The Hungarian anti-Roma attacks were unique in some respects: the attackers targeted national minorities instead of immigrants and attacked people in rural areas instead of cities with a large immigrant population. Characteristics of the Hungarian population most probably also influenced the underlying causal mechanism: social norms and the media environment in Hungary are more conducive to anti-Roma prejudice than in Western European democracies with stronger liberal norms, which can explain why the Black Sheep Effect and decreased outgroup prejudice were not observed. Correspondingly, in 2009-2010 Jobbik's political position was in many respects more extremist than that of most Western European right-wing populist parties.

Further research on this specific case of the anti-Roma attacks is hindered by the lacking availability of alternative data sources. However, further tests can contribute to confirming the internal validity of the findings and the speculations about the underlying causal mechanism. Given the inherently unpredictable nature of violent terrorism, this would be a challenging undertaking. A possible solution could entail a survey experiment in which people are retrospectively asked about their support for radical right positions and anti-Roma prejudice after being exposed to information about the attacks or similar cases of anti-minority violence by right-wing extremists. To test for causal mechanisms, alternative treatments should include text vignettes that on the one hand attribute responsibility to the ingroup attackers to spur feelings of collective guilt, while on the other hand emphasize Roma criminality. In line with the results of (Hirschberger, Kende and Weinstein, 2016), it can be expected that respondents agree with more extremist and prejudiced positions when exposed to the defensive narrative that minimizes ingroup responsibility.

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Appendix

Table A1: Locality-level descriptive statistics for quasi-experimental groups. Control group contains localities within 15 km radii of unrealized attack locations. Unstandardized values are displayed.

Group		Control			Treatment		
Variable	N	Mean	SD	N	Mean	SD	Test
Companies	62	49.887	162.929	145	64.421	374.785	F= 0.086
Log Municipal income	62	10.179	1.525	145	10.466	1.462	F= 1.635
Log Municipal expenditure	62	12.791	1.059	145	12.86	1.12	F= 0.17
Retail businesses	62	45.29	106.978	145	53.641	251.307	F= 0.063
Log Personal income tax	62	11.668	1.285	145	11.725	1.297	F= 0.084
All crimes	62	88.177	199.704	145	184.669	1091.97	F= 0.476
Personal offences	62	5.79	8.526	145	8.021	28.65	F= 0.361
Crime against property	62	57.161	136.05	145	118.621	635.345	F= 0.568
Registered offenders	62	39.532	56.257	145	59.414	232.64	F= 0.441
Unemployed	62	180.645	178.593	145	230.352	725.958	F= 0.283
Unemployed 180+ days	62	101.113	104.751	145	130.366	447.834	F= 0.258
Unemployed max primary education	62	15.5	14.37	145	22.221	49.72	F= 1.091
Unemployed manual workers	62	155.339	142.484	145	194.49	551.234	F= 0.303
Roma population 2001	62	122.758	160.745	143	173.937	425.031	F= 0.845
Log Population	62	7.573	0.926	145	7.565	0.975	F= 0.003
Population 60+	62	630.855	1014.168	145	756.51	3198.124	F= 0.092
Population 0-14	62	599.097	846.647	145	703.8	2049.872	F= 0.15
Deaths	62	42.71	58.831	145	50.655	199.98	F= 0.094
Out-migrants	62	168.645	268.565	145	203.8	635.518	F= 0.176
Pensioners	62	436.5	872.494	145	558.952	2670.837	F= 0.124

Statistical significance markers: * p<0.1; ** p<0.05; *** p<0.01

Table A2: Locality-level descriptive statistics for quasi-experimental groups. Control group contains localities within 15 km radii of unrealized attack locations. Values displayed are standardized by population (except from full population).

Group		Control			Treatment		
Variable	N	Mean	SD	N	Mean	SD	Test
Jobbik vote share	62	0.028	0.014	145	0.021	0.012	F= 12.831***
Voter turnout	62	0.62	0.067	145	0.636	0.065	F= 2.587
Companies	62	0.008	0.007	145	0.009	0.007	F= 2.163
Log Municipal income	62	0.007	0.004	145	0.007	0.006	F= 0.732
Log Municipal expenditure	62	0.009	0.006	145	0.009	0.008	F= 0.268
Retail businesses	62	0.01	0.004	145	0.009	0.004	F= 0.343
Log Personal income tax	62	0.008	0.005	145	0.008	0.007	F= 0.26
All crimes	62	0.022	0.012	145	0.027	0.018	F= 4.815**
Personal offences	62	0.002	0.002	145	0.002	0.003	F= 0.094
Crime against property	62	0.014	0.009	145	0.019	0.015	F= 5.354**
Registered offenders	62	0.014	0.009	145	0.014	0.008	F= 0.037
Unemployed	62	0.085	0.042	145	0.069	0.041	F= 6.755**
Unemployed 180+ days	62	0.05	0.031	145	0.037	0.026	F= 9.665***
Unemployed max primary education	62	0.009	0.009	145	0.008	0.01	F= 0.535
Unemployed manual workers	62	0.077	0.041	145	0.062	0.04	F= 6.004**
Roma population 2001	62	0.054	0.063	143	0.055	0.076	F= 0.011
Population 60+	62	0.19	0.04	145	0.18	0.037	F= 2.586
Population 0-14	62	0.187	0.035	145	0.189	0.045	F= 0.141
Deaths	62	0.015	0.006	145	0.013	0.004	F= 4.991**
Out-migrants	62	0.054	0.017	145	0.056	0.016	F= 0.529
Pensioners	62	0.119	0.042	145	0.122	0.037	F= 0.379

Statistical significance markers: * p<0.1; ** p<0.05; *** p<0.01

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	15.738***	12.593***	21.314***	25.478***
	(0.589)	(2.528)	(0.685)	(1.350)
Attacked * Post-treatment	2.602**	3.122***	1.687	2.161*
	(0.836)	(0.875)	(0.897)	(0.987)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.861	0.875	0.898	0.920
Adj. R^2	0.721	0.733	0.795	0.817
nobs	632	632	644	609
Log Likelihood	−1759.537	−1726.196	−1814.173	−1640.316

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A3: Difference-in-differences OLS regressions with spillover effects within 20 km radii around treatment control localities. Control group selected from localities around unrealized attack locations. The time-invariant main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

	Jobbik vote share 2009	Jobbik vote share 2009	Jobbik vote share 2010	Jobbik vote share 2010
Post-treatment (2009 / 2010)	14.905***	13.871**	21.440***	27.961***
	(0.789)	(4.997)	(1.276)	(2.234)
Attacked * Post-treatment	5.330***	6.600***	2.930	3.544*
	(1.294)	(1.530)	(1.612)	(1.528)
locality FE	yes	yes	yes	yes
controls	no	yes	no	yes
clustered SE	locality-level	locality-level	locality-level	locality-level
R^2	0.876	0.907	0.898	0.936
Adj. R^2	0.749	0.774	0.794	0.834
nobs	226	226	232	220
Log Likelihood	−625.307	−593.487	−663.350	−579.209

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table A4: Difference-in-differences OLS regressions with spillover effects within 10 km radii around treatment control localities. Control group selected from localities around unrealized attack locations. The time-invariant main treatment effect is omitted from the equations with the locality fixed effects due to collinearity

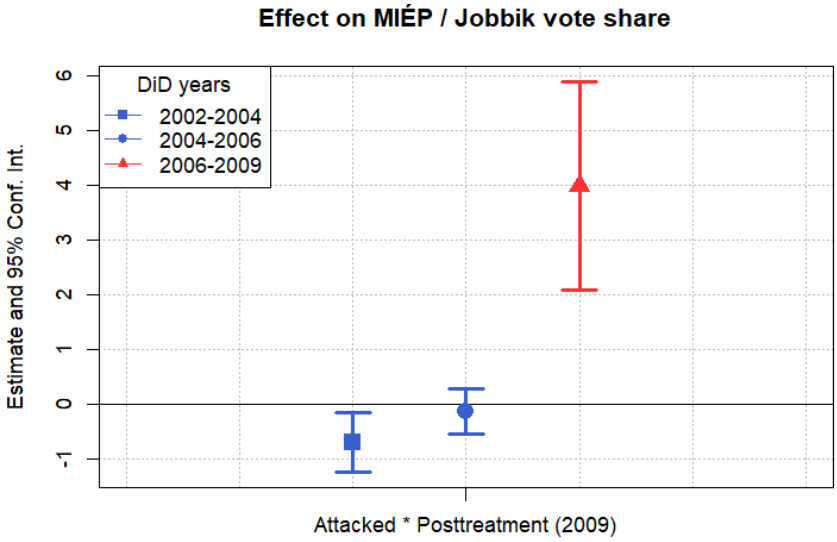

Figure A1: Coefficient plot of the interaction terms based on Table 11 and Table 5, models without controls

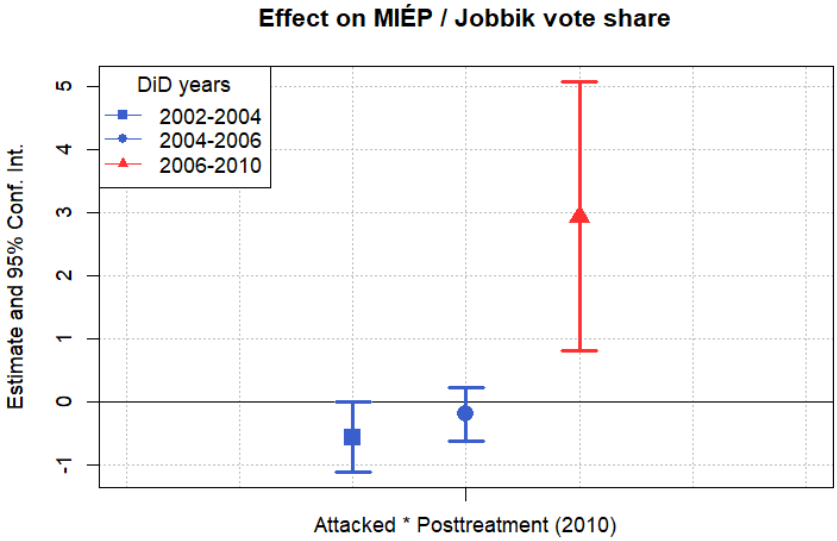

Figure A2: Coefficient plot of the interaction terms based on Table 12 and Table 5, models without controls