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Violence Against Politicians Drives Support for Political Violence Among (Some) Voters: Evidence from a Natural Experiment

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Abstract We investigate whether a real-world episode of physical violence committed in October 2023 against Thierry Baudet, leader of the Dutch far-right FvD, conditioned a differential support for political violence among different voters: voters ideologically close to the target of the attack (in-group) and voters experiencing partisan Schadenfreude toward the party of the target (out-group). The unexpectedness of the attack makes it an excellent case of exogenous treatment in a natural experiment (“Unexpected Event during Survey Design”). The fact that the attack occurred in the midst of a survey unfolding a rolling cross-section design yields likely more robust estimations than usually found in similar natural experiments. Our results indicate that the attack against Baudet slightly normalized political violence in the short term. While no specific uptick of support for violence was measured among respondents close to the FvD, in the short and medium term the attack furthermore slightly increased support for violence among respondents who dislike the FvD and experience partisan Schadenfreude.

Introduction

From the storming of the US Capitol by an angry mob to the public assassination of leaders, political violence seems to be a defining feature of contemporary politics. Because of its potentially dramatic implications for the

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physical safety of the public and social cohesion, a deep understanding of the root causes of political violence is normatively urgent.

Recent studies highlighted the existence of a reservoir of citizens showcasing an attitudinal predisposition toward supporting violence committed against members of the out-party (e.g., [Kalmoe and Mason 2022a, 2022b](#)). And while supporting violence is not the same as committing it, the latter can hardly exist without the former. What explains such attitudinal support? At the voter level, existing research was able to highlight the central role played by the psychological and attitudinal profile of individuals, from sensation seeking ([Schumpe et al. 2020](#)) to trait aggressiveness ([Kalmoe 2014](#)), need for chaos ([Arceneaux et al. 2021](#)), personality traits ([Gøtzsche-Astrup 2019, 2021](#); [Nai and Young 2024](#)), and perceptions of violence support among political rivals ([Mernyk et al. 2022](#)).

Much less investigated are the contextual and situational sources of variation in support for political violence. While studies exist about the role of election campaigns in shaping radical partisanship (e.g., [Kalmoe 2014](#); [Martin and Nai 2024](#)), or the success of governmental communication initiatives to curb election violence ([Fafchamps and Vicente 2013](#)), evidence that the context shapes attitudes toward violence remains fragmentary. In particular, little is known about whether major and sudden episodes of political violence—say, a violent attack against a politician—shape how the public feels about political violence in general. Are people more likely to condemn it in the wake of such violent events? Are voters' reactions to such events conditional on who they are, most notably in terms of partisan proximity with victims and perpetrators of the violent attacks? In the wake of a seemingly increasing strike of high-profile assassinations (and attempts) in recent years, including toward major political figures—from the assassination of Shinzo Abe in July 2022 to attempts against Brazil's Jair Bolsonaro (September 2018), Argentina's Cristina Fernández de Kirchner (September 2022), Slovakia's PM Robert Fico (May 2024), Donald Trump (July 2024), as well as the seemingly spreading trend of physical attacks against representatives in Germany in 2024—the matter feels increasingly urgent.

In this article, we leverage a real-world episode of physical violence committed against a political figure—Thierry Baudet, leader of the far-right Forum for Democracy (FvD) party in the Netherlands, who was attacked in October 2023 and suffered a minor concussion—and investigate whether it affected public support for political violence. Specifically, we test the expectations that the attack resulted in an uptick in support for political violence among sympathizers of FvD, on the one hand, and among voters experiencing partisan Schadenfreude—that is, positive sentiments when misfortune befalls disliked (partisan) groups ([Webster, Glynn, and Motta 2024](#))—against FvD, on the other hand.

Because the violent attack against Baudet occurred in the midst of survey fieldwork, it is a good example of exogenous treatment in a natural

experiment (specifically, an “Unexpected Event during Survey Design”; UESD; Muñoz, Falcó-Gimeno, and Hernández 2020).

We are not the first to leverage unexpected violent episodes targeting politicians as a natural experiment. Krakowski, Morales, and Sandu (2022) show that the assassination of Polish opposition mayor Paweł Adamowicz in 2019 decreased support for the victim’s party—seemingly driven by societal backlash toward an excessively negative campaign the party used in the aftermath of the assassination. Closer to our case, Dinas et al. (2016) show that the murder of politician Pim Fortuyn in 2002 resulted in more positive feelings toward his party in the Dutch population.

Overall, these studies seem to indicate that real-world episodes of violence committed against political figures matter from an attitudinal standpoint. We build on this premise and investigate the effects of the attack against Baudet for individual support for political violence in the Netherlands.

On top of leveraging the unexpectedness of the event, the first wave of our data collection was using a rolling cross-section design (RCS; Johnston and Brady 2002). In an RCS, respondents are, prior to the start of data collection, randomly assigned to a specific date for their survey, instead of being able to take the survey whenever they want. This controlled and randomized distribution of respondents provides an ideal setting and potentially unbiased estimators for a natural experiment based on an exogenous treatment occurring during survey data collection (Muñoz, Falcó-Gimeno, and Hernández 2020). The specificities of the case investigated—the fact that the attack happened outside the country (Belgium), against the leader of a comparatively minor party,¹ was immediately condemned by politicians across the board, and is much less severe than other cases of violence against politicians leveraged in the literature (e.g., Dinas et al. 2016; Krakowski, Morales, and Sandu 2022)—likely make it a conservative test for our expectations. The fact that the Netherlands is traditionally known for its strong pluralist democracy based on compromise and tolerance, and low affective polarization (Gidron, Adams, and Horne 2020), furthermore, likely reduces the incidence of support for political violence in the first place.

The unexpectedness of the violent event occurring within an RCS survey design is a strong methodological contribution of this article with regard to the existing literature—in particular, contemporary research on political violence. We believe that our article is the first empirical investigation able to estimate the causal effect of exogenous real-world violent effects on radical partisan attitudes in a setting with high external validity (i.e., a real-world

1. Created in 2016, the FvD has known some electoral success in the 2021 national elections, obtaining approximately 5 percent of the votes (eight seats in the Lower House). Also due to the rise of other far-right parties, this success petered out in the 2023 election (2.3 percent, three seats).

unexpected event, versus an experimental simulation) and nonbiased estimators (due to the RCS component). Indeed, other existing natural experiments leveraging real-world incidents rely on less favourable research designs, such as pauses in longitudinal data collection due to violence outbursts (Jakiela and Ozier 2019), or traditional survey data (non-RCS) whose fieldwork coincided with a terrorist attack (e.g., Balcells and Torrats-Espinoza 2018; Castanho Silva 2018; Harding and Nwoko 2024).

Our results indicate that the attack against Baudet marginally normalized political violence: it increased the share of respondents agreeing that violent acts against the out-group are frequent. While no specific uptick of support for violence was measured among respondents close to the attacked party (FvD), in the short and medium term the attack increased such support among respondents who dislike the FvD and experience partisan Schadenfreude. While the magnitude of these effects should not be overestimated, and likely exists only on the short run, the trend is relatively clear.

The expectations and analysis plan were preregistered prior to obtaining the data from the polling institute (but after the attack occurred). The preregistration can be found in the OSF repository² and in [Supplementary Material section D](#); [Supplementary Material section E](#) justifies deviations from the preregistration.

Conditioning Support for Political Violence

Political violence is beset by an overwhelmingly negative repute from a normative and analytical standpoint (Apter 1997), as it directly contravenes shared norms of cooperation and peaceful coexistence at the heart of the social contract (Hoffman 2020). Episodes of political violence thus tend to be harshly and swiftly condemned. For instance, in the aftermath of the violent attack against Paul Pelosi, husband of former Speaker of the House Nancy Pelosi, in October 2022, a *Time Magazine* article concluded that “democracy is best served when leaders of all kinds unmistakably denounce the words and deeds that target public officials for physical harm (Suri 2022).”

We have no reason to expect anything different at the general voters’ level. While news of the violent episode likely was experienced differently by different people, the expected baseline effect of the violent attack against Thierry Baudet should be a generalized drop in support for political violence in the Dutch population (H1). Violence is morally wrong, and sudden and unexpected acts of violence should remind us of this fact.

Yet, it is unlikely that this negative effect was homogeneous. Beyond this generalized condemnation against political violence, we argue that two types of individuals should be more susceptible to experience an *uptick* in their support

2. <https://osf.io/6cpbg/>.

for political violence in the aftermath of a real-world episode of partisan political violence: voters close to the party of the target of violence (in-party), and voters reporting high levels of partisan Schadenfreude (Webster, Glynn, and Motta 2024) toward such a target. We detail these two expectations below.

Threats Against the In-Group and Political Violence

Attachment to the (political) in-group tends to correlate with the desire to defend it against attacks from the outside. Strong personal in-group attachment has been shown as an important predictor of social vigilantism—a tendency in individuals to take upon themselves the defense of their community against real or perceived wrongdoings (Chen, Ok, and Aquino 2023)—sometimes violently. More broadly, threats against the in-group are associated with increased prejudice toward the out-group (e.g., Stephan, Ybarra, and Bachman 1999; Wilson 2001). According to the integrated threat theory (Stephan and Stephan 1996), two specific types of threats against the in-group—realistic threats (including threats against the physical well-being) and symbolic threats (resulting from conflict in values and beliefs)—are expected to lead to more negative out-group attitudes. The meta-analysis of Riek, Mania, and Gaertner (2006), which compares the results of 95 studies, provides strong support for this assumption.

Generally speaking, threat theories postulate that the (perceived) decreased well-being of the in-group is often seen as resulting from nefarious actions or prejudicial stances from the out-group (threats), with a resulting uptick of negative affect toward the latter (Croucher 2017), including the willingness to engage in collective action against it (Shepherd et al. 2018). In other terms, perceived attacks from the outside toward the in-group should naturally lead to upticks in negative feelings for—and willingness to engage against—the out-group. This is the case even when the threat toward the in-group comes not in a diffuse way from the out-group as a whole, but from specific members of the latter, as negative attitudes toward individual out-group members have been demonstrated to lead to general negative attitudes toward the whole out-group (Stark, Flache, and Veenstra 2013). Importantly, such threats result not only in more negative affect toward the out-group, but also to an increased support for violence against it (Obaidi, Thomsen, and Bergh 2018; Pauwels and Heylen 2020). This should be particularly the case when the threat toward the in-group is expressed in a violent form (see also Kalmoe and Mason 2022a). Evidence from Sudan's civil war in the early 2010s suggests, for instance, that episodes of violence against a specific group tend to “harden negative intergroup attitudes” (Beber, Roessler, and Scacco 2014). Negative attitudes stemming from violent threats against the in-group could lead, in turn, to a “cognitive opening” toward retaliatory violence (Ratelle and Souleimanov 2017) against the out-group. The retaliatory component of exposure to violence has been

widely investigated in studies about crime and delinquency (Farrell and Zimmerman 2018), which broadly suggest that exposure to violence, perceived as unjust, leads to “maladaptive coping strategies” that incentivize subsequent violence to compensate or retaliate (Agnew 1992). Overall, strong in-group attitudes should lead to upticks in support for (retaliatory) violence against the out-group when the former is attacked by the latter. With this in mind, we expect that the violent attack against Thierry Baudet should lead to an uptick of support for partisan violence against the out-group among FvD voters (H2).

Partisan Schadenfreude Against the Out-Group and Political Violence

A second group of voters should experience an uptick in support for political violence after the attack: voters experiencing *partisan Schadenfreude* toward the target of the attack. Generally speaking, Schadenfreude reflects experiencing “feel good” sentiments of elation and satisfaction when misfortune befalls disliked individuals or groups (Feather and Nairn 2005). In politics, Schadenfreude has been associated with lower sympathy for health concerns in political opponents (Peplak, Klemfuss, and Ditto 2022), spreading embarrassing news of political downfalls (Crysel and Webster 2018), and greater tolerance toward aggressive elite rhetoric (Nai and Otto 2021). Individuals high in Schadenfreude often feel that the targets of misfortune deserve it (Feather and Nairn 2005). Schadenfreude has been shown to correlate with relational aggression (Erzi 2020), and tends to be associated with more marked support for political violence toward out-group members (Cikara 2015).

Such positive association should be even greater in the aftermath of specific episodes of violence targeting the out-group—which likely make such a target more salient in the minds of voters and activate their attitudinal dispositions. Schadenfreude, elicited by misfortune befalling a member of the out-group, has been shown to “generalize to targets who have done nothing to provoke harm” (Cikara 2015, p. 12)—and should thus lead to an uptick in political violence toward members of the out-group in general. The attack against Thierry Baudet should lead to an uptick of support for partisan violence against the out-group among respondents who report high levels of partisan Schadenfreude toward FvD (H3).

Methods and Design

Data

Data comes from the Dutch Elections Monitor Survey (DEMoS), a high-quality multi-wave longitudinal survey developed to investigate attitudes of the Dutch electorate leading to the national election of November 22, 2023.

Unweighted data from the first wave in the DEMoS survey is leveraged here.³

Data collection on wave 1 started on October 2, 2023. It was set up following a rolling cross-section design (Johnston and Brady 2002). In an RCS, instead of inviting all participants in a sample to take part in the survey at once, participants are *randomly* assigned in a series of exclusive “batches,” whose members receive the invitation to participate in a staggered way across the duration of the survey wave—for instance, every other day a new batch receives the invitation. Because of the random assignment of respondents in batches, and the dispersion of those batches over time, an RCS design means that the invitation to participate for each respondent “is as much a product of random selection as the initial inclusion of that respondent in the sample” (Johnston and Brady 2002, p. 283). This design allows for a better causal estimation of effects by all time-related exogenous phenomena, such as the evolution of the campaign (Kenski et al. 2006).

In our case, data was gathered from batches of respondents, launched every other day. Prior to the start of data collection, the polling institute (I&O) developed a sampling frame that accounted for likely response rate for all batches, eyeing a targeted 150 complete responses per two-day batch. See [Supplementary Material table B4](#) for response rates per batch. Respondents in this initial sampling frame, part of the larger I&O panel, were randomly assigned to one of the 20 batches, and received the invitation to participate in the survey on the first day of their batch. They were invited to answer the survey within the next couple of days. The attack against Thierry Baudet, which we leverage as treatment within a natural experiment, occurred on October 26, overlapping with batch #13 (which is excluded from the analysis).

After removing speeders (1 percent fastest, $N=45$) and respondents in batch #1, which was used as a soft launch, the final sample includes 3,403 respondents distributed in 18 batches,⁴ that is, approximately 190 respondents per batch on average. The final sample includes 46.2 percent female respondents (50.3 percent in the Dutch population), and has an average age of about 54 years ($SD=16.6$). 40.6 percent of respondents are highly educated (36.6 percent in the population), and the average respondent leans slightly toward the right, 5.5 ($SD=2.7$) of a 0–10 scale for self-reported left-right positioning. [Supplementary Material table B1](#) reports the composition of each of the batches for these four characteristics.

3. Data collection received full ethical approval from the University of Amsterdam on September 18, 2023.

4. Due to a programming mistake by the polling company, batch #15 (scheduled for October 30–November 1) was not fielded; the previous batch (#14) was left in the field for longer and collected a higher number of respondents.

Natural Experiment: Leveraging an Unexpected Event During Survey Data Collection

In the evening of October 26, 2023, Thierry Baudet, leader of the far-right Dutch Forum for Democracy (FvD),⁵ was physically attacked while in Ghent (Belgium) to give a lecture. In a clearly politically motivated incident, Baudet was violently hit on the head with an umbrella by an individual who shouted anti-fascist messages,⁶ and suffered a minor concussion. The violent act received widespread condemnation across the political spectrum, including by the leader of the main center-left coalition, Frans Timmermans (GroenLinks–PvdA), who qualified the attack as “completely unacceptable.”⁷ The attack received prominent attention by all news outlets in the Netherlands, with the public broadcaster NOS,⁸ for example, covering it at the top of their webpage/news app and in their live broadcasts. The attack captured the attention of the public. Data from GoogleTrends (figure 1) shows an evident uptick in searches for “Thierry Baudet” in the Netherlands corresponding with the date of the attack (and following day), both in general and when compared to two other major figures in the Dutch political landscape (former PM Mark Rutte, and the leader of the far-right PVV Geert Wilders).

While the Netherlands experienced occurrences of physical attacks against politicians in the past—most notably, the assassination of Pim Fortuyn in 2002,⁹ a mere nine days before that year’s general election—political violence is not a critically urgent concern. The latest official report of the Dutch National Coordinator for Counterterrorism and Security notes that political attacks—namely, jihadist terrorism, right-wing extremism, and anti-government extremism—remain a possibility, but recommends keeping the threat level at 3 on a 1–5 scale (NCTV 2022).

The fact that the attack happened in the midst of survey data collection turns it into a perfect “Unexpected Event during Survey Design” (UESD; Muñoz, Falcó-Gimeno, and Hernández 2020). UESDs leverage the unexpectedness of salient exogenous events that happen during data collections

5. Baudet is a well-known figure in Dutch politics. In our sample, only 26 respondents (0.8 percent) declared not knowing him. Out of the 12 leaders of the main parties, only one other leader had a name recognition greater than Baudet.

6. “Dutch Far-Right Leader Thierry Baudet Attacked with an Umbrella,” *POLITICO*, October 26, 2023, <https://www.politico.eu/article/dutch-far-right-leader-thierry-baudet-attacked-with-an-umbrella/> (accessed November 5, 2023).

7. “MPs Condemn Attack on Baudet, Far Right MP Hit by Umbrella,” *DutchNews*, October 27, 2023, <https://www.dutchnews.nl/2023/10/mps-condemn-attack-on-baudet-far-right-mp-hit-by-umbrella/> (accessed November 5, 2023).

8. The NOS is weekly used by 63 percent of the Dutch public.

9. “Dutch Politician Pim Fortuyn Assassinated,” *The Guardian*, May 6, 2002, <https://www.theguardian.com/world/2002/may/06/3> (accessed November 5, 2023).

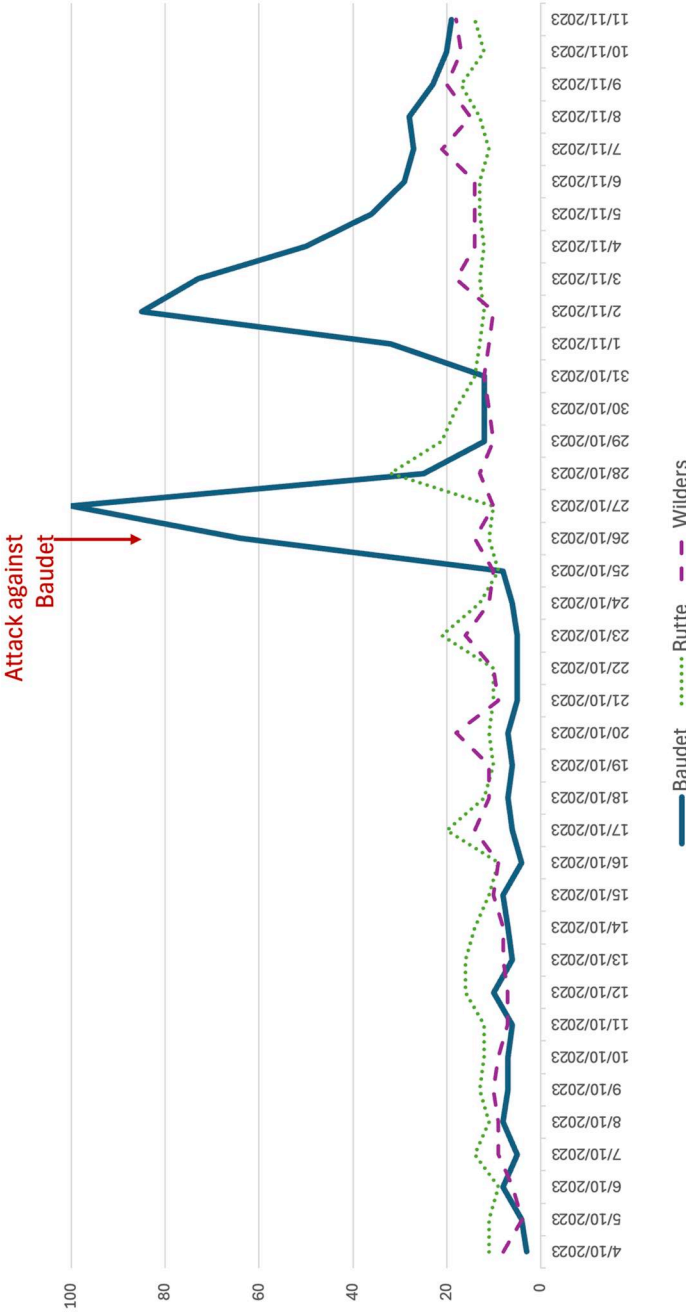


Figure 1. GoogleTrends for “Thierry Baudet,” “Mark Rutte,” and “Geert Wilders” search terms in the Netherlands during the survey window. GoogleTrends (<https://trends.google.com/trends>) reflect comparative search interest for specific terms during selected time windows; daily numbers are reported relative to the highest point on the chart (100) within the window across all compared search terms, reflecting peak popularity. Data collected on July 29, 2024.

that are underway prior to the event (and continue afterward), allowing for a before-after comparison of quantities of interest, in a “natural experiment” setting. Recent examples of UESD designs relying on survey data deal with topics as diverse as trust after corruption scandals (Hegewald and Schraff 2024), attitudes toward refugees after riots (Schwitter and Liebe 2024), government evaluation after internet outages in Africa (Strauch 2024), government support after welfare retrenchments (Larsen 2018), and preferences toward conflict after unexpected results in peace referenda (Ramirez-Ruiz 2024).

UESDs are not without empirical challenges. Most notably, because respondents are not randomly assigned to concurrent experimental groups but, rather, the sudden occurrence of an exogenous event creates such groups “naturally.” Therefore, determining causal effects in a natural experiment can be tricky—in particular, due to the potential violation of the temporal ignorability assumption.

In our case, the fact that the unexpected event happened during survey data collection with a Rolling Cross-Section (RCS) setup represents a “favorable survey design” (Muñoz, Falcó-Gimeno, and Hernández 2020, p. 191). Because, in an RCS, respondents are randomly assigned to batches, and batches are randomly distributed throughout the duration of the wave, the timing of participation in the survey is an independent function of the outcome of interest. In other terms, the RCS component should set the stage for the treatment being an unbiased estimator, drastically reducing issues related to selective exposure of participants into the treatment group that are a notable challenge in natural experiments (Craig et al. 2017). Overall, having had a RCS survey in place during the unexpected event (UESD) was particularly serendipitous. None of the articles listed above engaging with unexpected events leverage an RCS design.

To further reduce the incidence of possible ignorability violations, we will replicate all analyses using different pairs of experimental groups obtained by varying the “bandwidth” of the temporal boundaries of the control and treatment groups (Muñoz, Falcó-Gimeno, and Hernández 2020). Narrower bandwidths—that is, comparing respondents very close to the exogenous event—likely increase the precision of the estimate, but yield models run on fewer respondents and as such might face problems of statistical power; the reverse is true for broader bandwidths. With this in mind, we use three distinct cutoff points when it comes to the outer boundaries of the control and experimental group. A first comparison (the narrower, A, total $N = 610$) will be between respondents who participated during the last four days before the attack versus those who participated in the four days directly after it (short term); a second comparison (B, total $N = 1,323$) sets the cutoff point eight days before and after the attack; finally, a third comparison (the broader, C, total $N = 3,212$) simply leverages the full span of the survey wave—that is,

all batches until #12 in the control group, and all batches from #14 to the end in the treatment group. In all cases, batch #13 is excluded because it is concomitant with the exogenous event. Figure 2 summarizes the distribution of batches over time, the start and end dates of data collection, and the cutoff points for the three pairs of experimental groups, from the narrower bandwidth (A) to the broader (C). See also Supplementary Material table B2. Post-hoc power calculations suggest that the sample size is sufficient to pick up small effect sizes even in the narrowest bandwidth with power > 80 percent (see detailed power calculations, Supplementary Material section B).

Balance tests show that the control and experimental groups never significantly differ, across all three bandwidths, when it comes to gender and age. The two narrower bandwidths (± 4 and ± 8 days) also do not differ in terms of the proportion of respondents with a university degree, whereas in the broadest bandwidth (full span) this proportion is significantly higher in the treatment group, $\chi^2(1, N = 3,212) = 9.55, p = 0.002$. To further reduce the risk of violating the ignorability assumption, and in line with our preregistered analysis plan, we will then control for education in all analyses for the broader bandwidth. In the two narrower bandwidths, the average self-reported left-right scores are significantly lower in the treatment groups—respectively, $t(572) = 2.40, p = 0.017$ (± 4 days), and $t(1,234) = 2.55, p = 0.011$ (± 8 days). This being said, because we cannot exclude that left-right self-positioning could be affected by the treatment, and to avoid any chance of posttreatment bias (also as preregistered), we will refrain from using left-right as covariate.

Measures

Support for political violence

The dependent variable is support for political violence committed by a member of the partisan in-group against a member of the out-group. Instead of relying on attitudinal scales (Kalmoe 2014; Kalmoe and Mason 2022a, 2022b)—which perhaps overestimate support for violence because asking “about general support for violence without offering context, [thus] leaving the respondent to infer what ‘violence’ means” (Westwood et al. 2022a, p. 1)¹⁰—we measure in our survey *support for a specific violent political act*. Such measurement, grounded in a realistic scenario, likely offers a more conservative estimation (Westwood et al. 2022a).

Respondents were presented with a vignette in the form of a mock newspaper article, detailing a recent fictive violent incident that happened in the Netherlands. An in-party person is described as going to a gathering of out-

10. The original authors provided a rebuttal to this critique (Kalmoe and Mason 2022b); see also Westwood et al. (2022b).

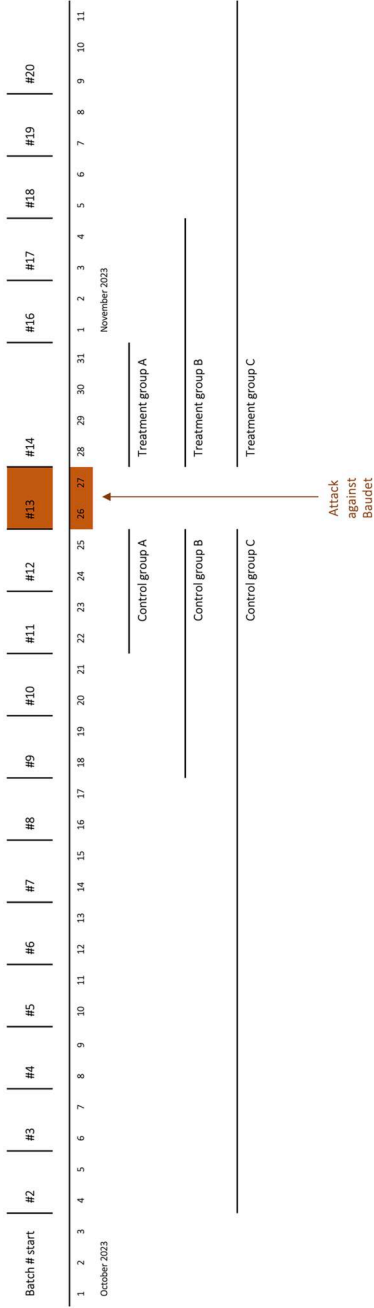


Figure 2. Timing of the RCS batches, exogenous treatment, and bandwidths of the experimental groups.

party voters, and severely hurting one attendee by punching them in the face, so that they had to be brought to the hospital. Both in- and out-party names in the article are automatically piped in based on answers to previous survey questions, in which respondents had to identify the party they like (in-party) and dislike the most (out-party) from closed-ended lists. The full text of the vignette is in [Supplementary Material section A \(table A1\)](#). Vignettes such as these ask for respondents to be focused on what they have in front of them; relying on high-quality professional samples, like in our case, is thus particularly important.

After reading the article, respondents were asked (from 1 = “Disagree strongly” to 7 = “Agree strongly”) whether (i) what the perpetrator did could under some circumstances be justified ($M = 1.8$, $SD = 1.4$), (ii) the perpetrator was likely provoked to react like that ($M = 3.2$, $SD = 1.5$), (iii) they support what the perpetrator did ($M = 1.5$, $SD = 1.1$), (iv) the perpetrator should face criminal charges ($M = 5.4$, $SD = 1.7$), and (v) these things happen all the time in the Netherlands ($M = 4.5$, $SD = 1.5$). Correlations are in [Supplementary Material table C1](#). We will present results for these five indicators separately, as well as for an additive index obtained by averaging responses to the five items after recoding the reversed one ($M = 2.7$, $SD = 0.9$, $\alpha = 0.59$).

[Figure 3](#) plots the evolution of the daily average scores on these five indicators for the whole duration of the survey wave. Trends in the figure support the excludability assumption ([Muñoz, Falcó-Gimeno, and Hernández 2020](#)); that is, the absence of important pretreatment trends on the main outcomes over time. The span before the occurrence of the exogenous event (the attack against Baudet), on the left of the vertical line in the graph, seemingly showcases temporally stable levels on all five outcome items. Regressing the scores on these five items on the day in which the responses were collected never yields a significant association, for any of the three experimental bandwidths—with one marginal exception: agreeing that violence is something that “happens all the time in the Netherlands” slightly decreases over time in the control group for the broadest bandwidth (full span), $b = -0.01$, $t(1,980) = -1.90$, $p = 0.057$, $\beta = 0.04$. The effect is, however, weak.

Robustness checks that employ a “placebo” treatment, splitting the control group at the empirical median (full span, October 15), as recommended in [Imbens and Lemieux \(2008\)](#), show again the absence of strong pretreatment trends. The placebo treatment does not have significant effects on any of the political violence items—except again on the last item (violence “happens all the time in the Netherlands”), $b = -0.19$, $t(1,776) = -2.67$, $p = 0.008$, $\beta = 0.06$. While the effect is not particularly strong, interpretation of our results should consider the possible presence of a (very slight) pre-

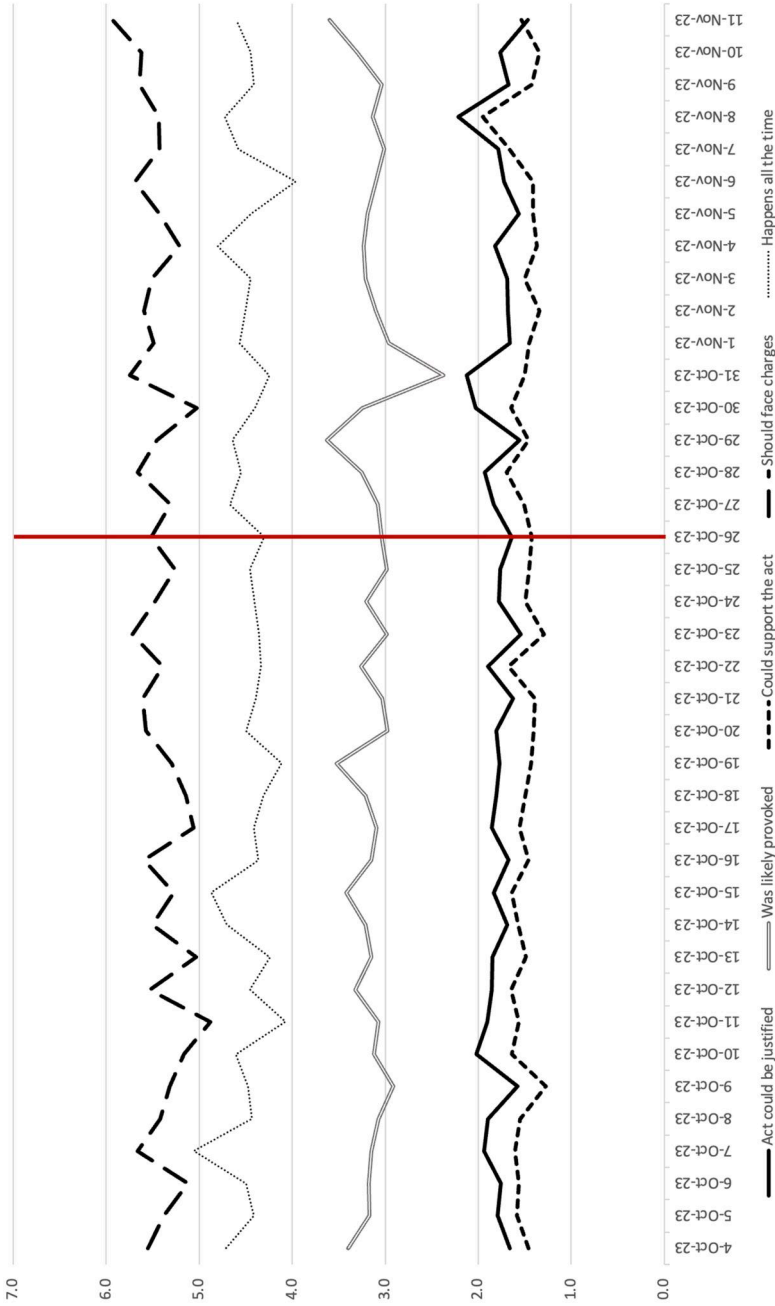


Figure 3. Evolution of support for the violent act over time in the sample. All variables range between 1 (strongly disagree) and 7 (strongly agree). Average scores. The vertical line indicates the exogenous treatment (attack against Thierry Baudet).

experimental downward trend in finding violence a usual occurrence in the Netherlands.

Partisan Schadenfreude

The measure of partisan Schadenfreude is inspired by the protocol discussed in Webster, Glynn, and Motta (2024) and Nai and Otto (2021). Respondents were presented with the hypothetical situation that a new neighbor of theirs, who voted in the most recent election for the respondents' most disliked party, "recently lost their job and is experiencing financial difficulty." They were asked afterward to what extent they agree with a series of four statements, such as "I would be a little amused by what happened to them" and "I would find it difficult to resist a smile" (from 1 = "Disagree strongly" to 7 = "Agree strongly"). Schadenfreude is simply measured as the average support for these four statements (after reversing the fourth one, "I would feel sorry for them"). The full text of the vignette is in [Supplementary Material section A \(table A2\)](#). Reliability of the additive scale is satisfactory ($\alpha = 0.83$; $M = 2.3$, $SD = 1.2$). We should note that our measure of partisan Schadenfreude does not capture feelings of joy related to the treatment—the attack against Thierry Baudet. The measure is general, concerning a non-descript member of the partisan out-group, not specific to any real-world event.

Results

The attack against Thierry Baudet did not have any significant or substantial electoral effects, even in the short term. Regardless of the amplitude of the bandwidth considered, no significant differences exist before and after the attack with regard to the share of respondents that indicate FvD to be their preferred party, the share of respondents that identify that party as their most disliked one, the average propensity to vote for FvD, or the average evaluation of Thierry Baudet as a suitable¹¹ leader ([Supplementary Material tables C2–C5](#)).

The attack also does not seem to have led to a widespread depression of attitudes toward political violence in general. Models in [figure 4](#) estimate support for political violence—both the general index, and the five separate indicators—as a function of the treatment (plus selected unbalanced covariates). As the figure shows, support for political violence is never significantly or substantively lower after the attack. Even more so, against our expectations, we find some (limited) evidence that the attack against Baudet seems to have somewhat normalized the presence of political violence as a whole; in the medium term (± 8 days), after the attack, a significantly higher

11. In Dutch, "geschikt."

share of respondents agreed with the statement that episodes of intraparty violence like the one described in the fictive vignette “happen all the time in the Netherlands,” $b = 0.16$, $t(1,321) = 1.93$, $p = 0.053$. The effect remains relatively marginal (and only significant at $p < 0.1$) and should thus not be overestimated.

We expected stronger support for political violence among respondents close to the party of the attacked politician (FvD). This does not seem to be the case in any systematic way in our data. Regardless of the bandwidth chosen, our models fail to pick up any substantial increase (or decrease) in support for political violence among FvD copartisans, compared to respondents who do not indicate that the FvD is their most preferred party, after the attack (Supplementary Material tables C9–C11). In all models, the only significant effect is with regard to estimating the attack against Baudet as being likely provoked. In the very short term (± 4 days), respondents close to the FvD are significantly *less* likely to agree that the attack in the vignette was provoked; $b = -1.23$, $t(606) = -1.70$, $p = 0.091$ —perhaps an indirect condemnation of the attack their leader was a victim of (Supplementary Material table C9, model M3). The effect is, however, only significant at $p < 0.1$. These models are rather suboptimal, due to the extreme skewness of the variable identifying supporters of the FvD, with only a few respondents indicating FvD as their preferred party. Additional models using instead a continuous 0–10 variable measuring respondents’ propensity to vote (PTV) for the FvD equally fail to yield any significant or substantial results (Supplementary Material tables C12–C14). It has to be noted that these models could potentially be biased by posttreatment effects (Montgomery, Nyhan, and Torres 2018), even if neither the share of respondents that identify the FvD as their preferred party nor the propensity to vote for the FvD are significantly affected by the treatment. Even with this in mind, we fail to find any evidence that physical attacks toward a member of one’s party trigger any direct retaliatory attitudes.

Finally, we expected an uptick in support for violence after the attack among voters who oppose the targeted party (FvD) and experience higher levels of partisan Schadenfreude. Figure 5 shows supporting evidence on the short and medium term. Looking at respondents who declare particularly disliking the FvD, higher levels of partisan Schadenfreude are associated, in the direct aftermath of the attack (± 4 days) and compared to the control group, with an uptick on the general index of support for violence, $b = 0.16$, $t(309) = 2.12$, $p = 0.035$, $\beta = 0.26$, with stronger support for the idea that acts of partisan violence toward the out-group could be justified, $b = 0.36$, $t(309) = 2.66$, $p = 0.008$, $\beta = 0.34$, and with stronger support for the idea that the act was likely provoked, $b = 0.26$, $t(309) = 1.80$, $p = 0.073$, $\beta = 0.23$. This trend is confirmed when increasing the bandwidth to eight days; for voters who dislike the FvD higher levels of Schadenfreude are associated

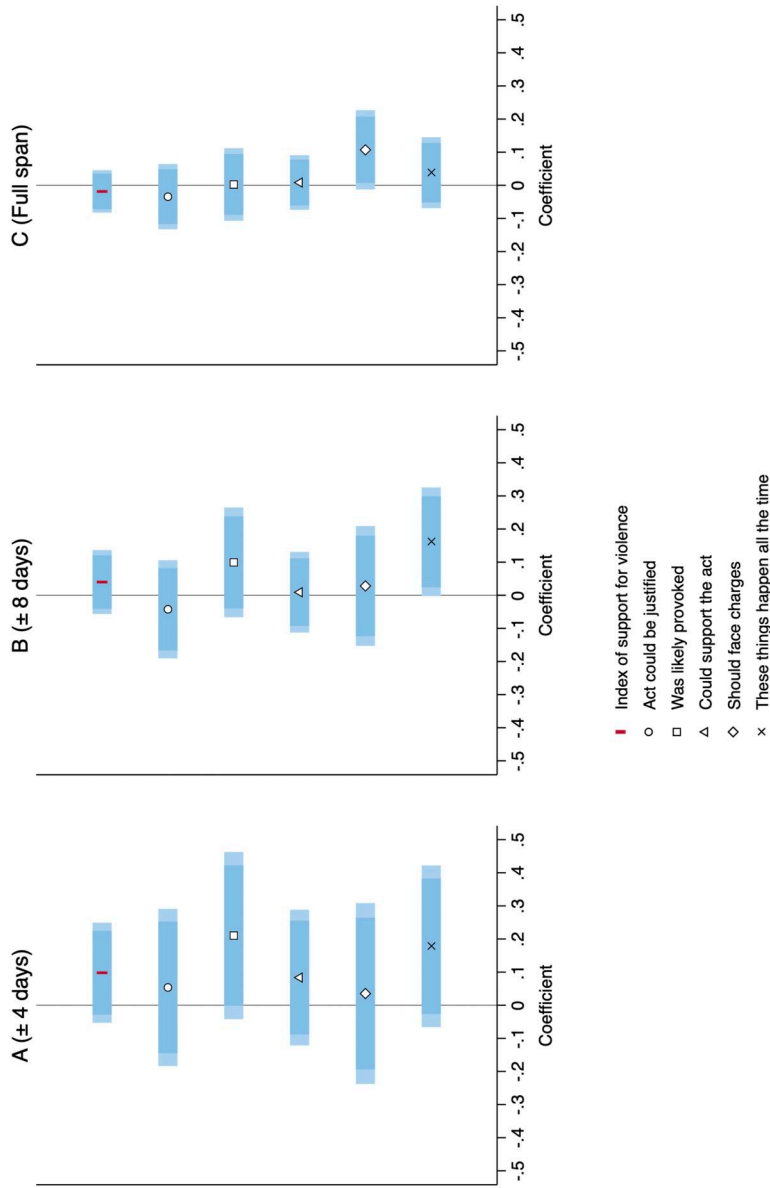


Figure 4. Effect of the attack on support for partisan violence (full sample). Coefficient plots with 95 percent (outer bounds, lighter color) and 90 percent (inner bounds, darker color) confidence intervals. Coefficients reflect the effect of exposure to the attack (before/after comparison). Positive significant coefficients indicate that support for violence (e.g., agreeing that the violent act in the vignette could be justified) increases after the attack. One separate model for each dependent variable. Model in panel C controls for respondents' education (university, 0/1). Full results in [Supplementary Material Tables C6–C8](#).

with an uptick in the general index of support for violence, $b=0.11$, $t(656)=2.11$, $p=0.035$, $\beta=0.19$, with the idea that political violence toward the out-group is likely provoked, $b=0.23$, $t(656)=2.27$, $p=0.023$, $\beta=0.21$, and with the fact that the violent act could be supported, $b=0.11$, $t(656)=1.72$, $p=0.086$, $\beta=0.15$. Higher levels of Schadenfreude are furthermore negatively associated, among respondents exposed to the attack and compared to the control group, with the idea that the perpetrator should face criminal charges, $b=-0.22$, $t(656)=-1.97$, $p=0.050$, $\beta=-0.18$. The magnitude of these effects is not very large, but still two to three times as great as the direct effects for the whole sample discussed above.

Figure 6 substantiates the magnitude of the moderating role of Schadenfreude for the average bandwidth (± 8 days) with marginal effects with 95 percent confidence intervals. While these effects do not appear when running the models on the full span (panel C in figure 4)—suggesting indirectly that the persistence in time of these effects is likely short-lived—in the shorter term, they seem to indicate that real-world acts of violence toward disliked political figures can trigger small but meaningful upticks in support for violence against the out-group in some segments of the population.

Conclusion and Discussion

This study set out to investigate how real-world violent episodes against political figures affect support of political violence among different voters, leveraging the exogenous unexpected event of an attack on Thierry Baudet, leader of the far-right FvD, during an ongoing rolling cross-sectional study conducted in the Netherlands prior to the November 2023 national election. Taking stock of our results leads to contrasting conclusions.

On the one hand, against expectations, we did not find that support for violence is higher after the attack, among voters sympathizing with the party of the victim (FvD). Evidence about the evolution of social conflict, civil wars, and crime suggests that retaliating against violence experienced is a powerful driver of further violence (Ratelle and Souleimanov 2017; Topalli, Wright, and Fornango 2002)—easily showing how spirals of violence are triggered. The absence of clear upticks in support for political violence among sympathizers of the attacked politician, against what we expected, is reassuring.

On the other hand, our results suggest that the effects of the attack on support for political violence were particularly prevalent among people who *dislike* the FvD and who experience partisan Schadenfreude with regard to this party. These voters were about two to three times as likely to agree that political violence can at times be justified, is likely provoked, and should not be charged, at least in the short term. This trend is quite worrying, in

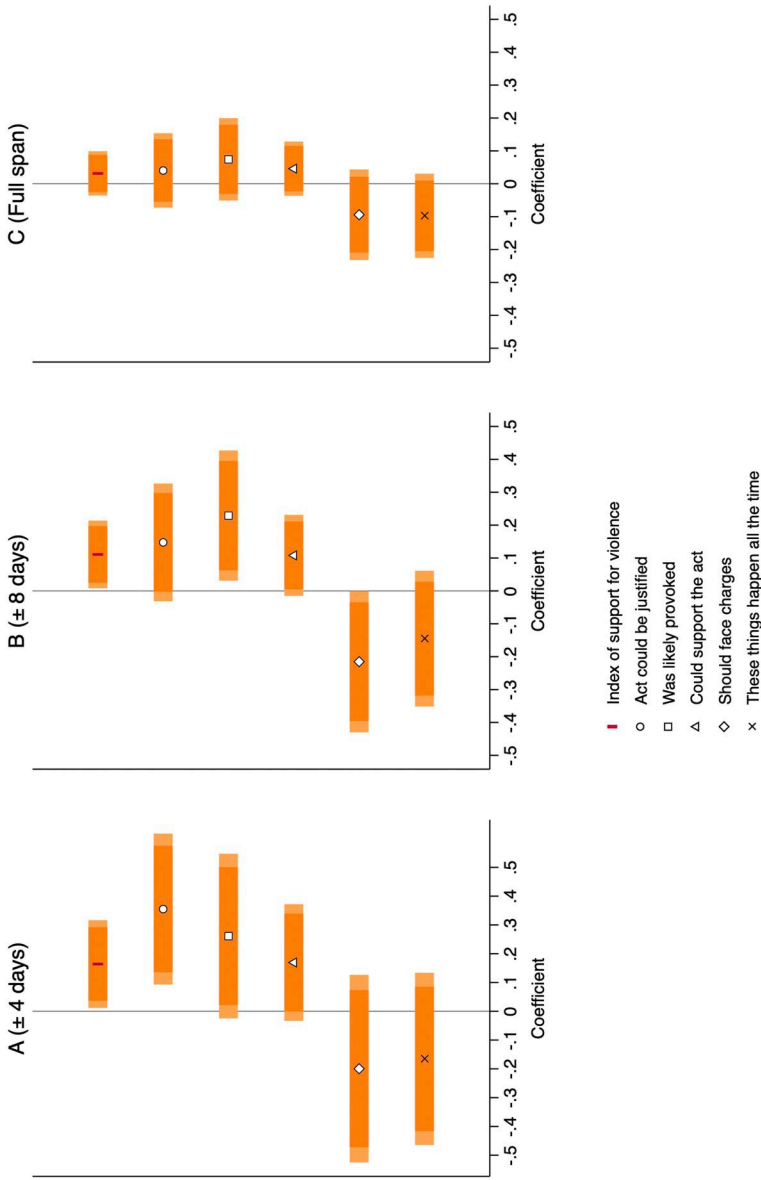


Figure 5. Effect of the attack on support for partisan violence (moderated by partisan Schadenfreude). Coefficient plots with 95 percent (outer bounds, lighter color) and 90 percent (inner bounds, darker color) confidence intervals. Coefficients are interaction terms between exposure to the attack (before/after comparison) and respondents' level of partisan Schadenfreude. Positive significant coefficients indicate that support for violence (e.g., agreeing that the violent act in the vignette could be justified) increases after the attack with increasing levels of Schadenfreude. One separate model for each dependent variable. Model in panel C controls for respondents' education (university, 0/1). Full results in [Supplementary Material tables C15–C17](#).

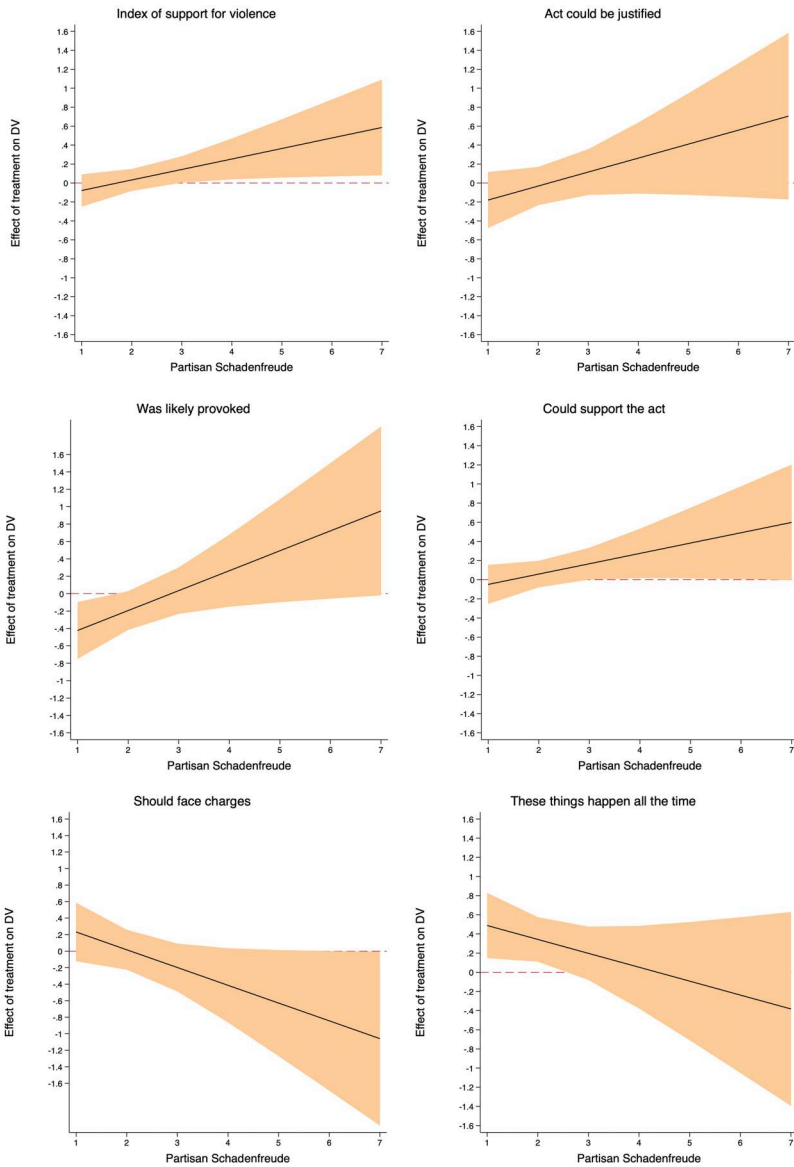


Figure 6. The moderating role of partisan Schadenfreude (± 8 days; marginal effects). Marginal effects with 95 percent confidence intervals, based on coefficients in [Supplementary Material table C16](#).

particular in light of increasing levels of affective polarization worldwide, which tends to deepen partisan hostility and aggravate negative political out-group feelings (Iyengar, Sood, and Lelkes 2012). Our results thus seem to add to the existing evidence linking affective polarization and general support for political violence (Kalmoe and Mason 2022a; but see Berntzen, Kelsall, and Hartevelde 2024).

Equally worrying is the result that the attack seems to have normalized the presence of political violence as a whole (in the short term), with a higher share of respondents holding the idea that episodes of violence against out-group members “happen all the time in the Netherlands.” While the magnitude of this effect is rather weak, its mere presence should be cause for alarm, and not only in the Netherlands. A strong pluralist democracy based on compromise and tolerance, and where people’s party identification is relatively limited due to the fragmented multiparty system and high volatility (Thomassen et al. 2009), the Netherlands likely represents a rather conservative test case for the effect of violence against politicians. Whether the effects of such real-world acts of violence would be stronger in countries with higher levels of affective polarization, and where negative out-party feelings are more developed than in the Netherlands, such as the United States (Gidron, Adams, and Horne 2020), remains an open question. Similarly, the fact that the candidate involved in the incident under investigation in our article was the representative of a “fringe” political party, and the fact that the attack itself was much less severe than other episodes of political violence we have witnessed in recent times, suggest that the results shown here are likely conservative.

Finally, further research should strive to expand on the specific mechanisms associating violence against politicians and radical attitudes in the public. Is it simply by the mere presence of these violent acts that the public becomes more attitudinally disposed toward more violence? Our personal profile as communication scholars leads us to think otherwise. A case can certainly be made that the effects of unexpected events also unfold by the way in which they are framed in the public discourse—for instance, by media reports or election campaigns. This is precisely what emerges from the study by Krakowski, Morales, and Sandu (2022) leveraging public opinion and campaigning data surrounding the assassination of a Polish opposition mayor: it is likely because the party of the victim engaged in negative campaigning against the government in the aftermath of the attack, that public opinion turned sour toward that party, when compared to sentiments for the government. Such mediated effects are certainly not rare, as cross-cutting influences between changes in public opinion and content of election campaigns are frequent in the pre-election period (Blackwell 2013). Additionally, shifting attitudes toward violence in the public likely stem from perceived violations of norms of peaceful coexistence—which likely

have both a group and an individual component, in line with recent research adopting a constructionist approach to norm violation and attitudes toward politics (e.g., Vargiu 2022). Further research should strive to include this perceptive component of norm violation into (causal) models investigating attitudes toward political violence.

Supplementary Material

Supplementary Material may be found in the online version of this article: <https://doi.org/10.1093/poq/nfaf010>.

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Data Availability

Replication data and documentation are available at <https://osf.io/6cpbg/>.

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