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RESEARCH ARTICLE

Prominence over proximity? Terror attacks' impact on party preferences

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Abstract
How does a terrorist attack affect party preferences? Based on existing theories, we would either expect incumbent parties to benefit because of a rally-effect, or populist radical right parties (PRRPs) to gain due to a radicalization of voters' preferences. These competing theories are tested with a unique dataset of a large sample of voters' responses on a Voting Advice Application. We do so using a novel way to leverage exogenous events using big public opinion data. We show that a terrorist attack has a positive effect for the main incumbent party, even when voters' positions on the issues owned by the PRRPs become more radicalized. This means that during crises, voters rally around the flag and prefer prominence over policy proximity.

Keywords: terrorist attacks; rally-effect; populist radical right parties; immigration; VAA data

Introduction

How does a terrorist attack affect voters' party preferences? The effects of a terrorist attack on attitudes have been shown to range from increasing trust in government institutions (Dinesen and Jæger, 2013; Larsen *et al.*, 2020; Muñoz *et al.*, 2020) and negative shifts on immigration attitudes and immigration policy preferences (Boomgaarden and Vreese, 2007; Das *et al.*, 2009; Legewie, 2013; Jakobsson and Blom, 2014; Ferrin *et al.*, 2019; Nussio *et al.*, 2019; Nägel and Lutter, 2020), to null-effects (Finseraas *et al.*, 2011; Balcells and Torrats-Espinosa, 2018; Boydstun *et al.*, 2018; Castanho Silva, 2018). When it comes to voting preferences rather than *attitudinal* shifts, however, our knowledge is sparse and findings are inconclusive. Previous results on attitudinal shifts imply two effects on voting preferences: an increased trust in government institutions points toward a rally-effect benefiting incumbent parties (Mueller, 1973), while more negative attitudes toward immigration may benefit populist radical right parties (PRRPs) (Walgrave *et al.*, 2009; Van der Brug and Berkhout, 2015). Nonetheless, whether changes in trust or a right-shift in policy preferences candidly translate into an advantage for either incumbents or the radical right remains largely an open question (Helbling and Meierrieks, 2020).

One of the reasons for this is that existing studies relied on cases – such as on Spain (Balcells and Torrats-Espinosa, 2018; Falcó-Gimeno *et al.*, 2022) or the USA (Baccini *et al.*, 2021) – which do not allow us to clearly test whether a terrorist attack benefits the incumbent or PRRPs.

[†]Emma Hoes and Jonne Kamphorst are shared lead authors. They designed and conducted the study. The data and reproduction files can be found on OSF here: https://osf.io/h6wjn/

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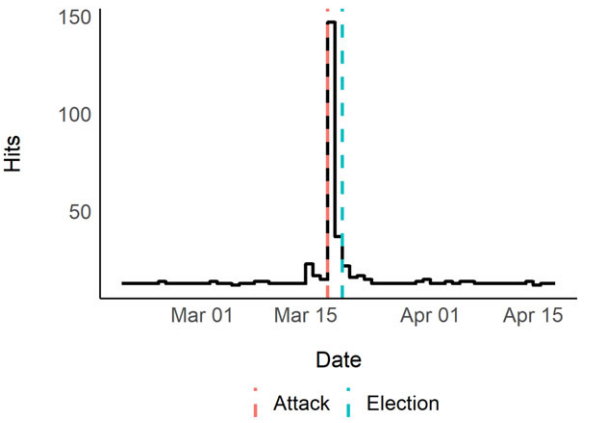


Figure 1. Salience of the event.

To explicitly test this hypothesis, two requirements need to be met: (1) The presence of a PRRP among all competing parties, and (2) The incumbent party should not be a PRRP. Whilst there are some studies that might meet these two conditions, they focus on the Israeli context (Berrebi and Klor, 2006; Berrebi and Klor, 2008; Gould and Klor, 2010; Getmansky and Zeitzoff, 2014). Israel's right wing parties, however, are notably different from those in Western-Europe: whilst Israel and Europe's right wing may both be considered populist and nationalistic, European right wing parties focus more on immigration whereas Israel's right wing parties are Zionist. It may therefore be that results from studies focusing on Israel do not travel to Europe. There is one study that does meet these conditions and focuses on Western-Europe, yet it does not use a design that allows for a more causally robust estimation of the effects of terrorist attacks (Gassebner *et al.*, 2008). More relevant, Kibris (2011) focus on terrorist attacks in Turkey, and find that they decrease support for the government. Whilst important evidence, it is only evidence from one country and the Turkish political context is, just like the Israeli one, different from Western-Europe. In this paper, we address the limitations of previous studies by addressing the electoral consequences – rather than attitudinal ones – of a terrorist attack right before an election in the Netherlands, a case that meets the requirements set out above (Sniderman *et al.*, 2007).

On the 18th of March, two days before the 2019 Dutch Provincial Elections, the lives of four people were taken and six more were left severely injured. At 10:42 that day, a shooting occurred inside a tram in Utrecht, the Netherlands, on its way to Utrecht Central Station. The shooting was discussed widely on all media platforms and, unsurprisingly, left the Netherlands in shock. With the regional threat level in Utrecht being raised to its highest and in the rest of the Netherlands to second highest, the other big cities such as Amsterdam, Rotterdam, and The Hague enhanced security near train stations and airports. Consequently, the occurrence of the terrorist attack was not only present in the media, but also physically for all people commuting via the most used public transport routes in the Netherlands. The salience of the attack can also be shown empirically, as Google Trends data, shown in Fig. 1, for 'Utrecht', 'attack', 'tram', 'shooting', and 'terrorist' clearly show that the attack was salient.

Two days after the attack, on the 20th of March 2019, the Dutch provincial elections took place.[1] With a total of 570 seats to divide, the new PRRP Forum for Democracy (FvD) won 86 seats. This made them the largest party of the 2019 Provincial Elections and most voted for in the

[1]In the Dutch provincial elections, eligible voters elect the members of the Provincial Parliaments in the twelve provinces of the Netherlands. The members of the Provincial Parliaments then elect the Dutch Senate via an electoral college. As such, the provincial elections thus indirectly determine the composition of the 75-seat Senate.

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megalopolis 'Randstad', which consists primarily of the four largest Dutch cities: Amsterdam, Rotterdam, The Hague, and Utrecht. Because of the ethnic-religious background of the perpetrator and the events that followed the incident, an assumption that occurred frequently in several media outlets was that the attack may have swayed the election outcome in favor of FvD. This assumption is in line with the idea that attacks may create more negative attitudes toward immigration, aligning voters' policy preferences with those of PRRPs, leading to the success of such parties (Walgrave *et al.*, 2009; Van Kessel, 2011; Van der Brug and Berkhout, 2015), but not with the rally-effect (Mueller, 1973).

In an attempt to further explore these two competing mechanisms, this study answers the following research question: *How does a terrorist attack affect party preferences?* By relying on a large Voting Advice Application (VAA) dataset and employing an Unexpected Event During Survey Design (UESD) (Muñoz *et al.*, 2020), the main results hint toward the rally-effect: the shooting increased the likelihood respondents would vote in favor of the main incumbent party (VVD).

Next to a substantive contribution, this study makes an important methodological contribution. We show how big public opinion data of the sort made available through VAAs can be leveraged in an USED design to causally research the effects of exogenous events. To the best of our knowledge, this is the first study of its type. We argue that VAA data is exceptionally well-suited for an UESD due to the high number of respondents, which allows for a tight bandwidth around the treatment event, ensuring that potential confounders caused by other events are not a concern. In addition, as VAAs are generally used right before elections, any treatment effects are likely to influence electoral outcomes.

Theoretical framework

The rally-effect

One of the first to introduce the 'rally-around-the-flag' phenomenon was John Mueller (1973). By focusing on the USA, he showed how people are inclined to rally – that is, 'to convene' – around the the flag in times of sudden, dramatic events. In the more modern interpretation of the rally-effect, it is often seen as rallying around government institutions or main political figures such as the President or Prime Minister. We define the rally-effect as follows: 'an increase in approval of government institutions in the face of specific, dramatic, and sudden events' (Dinesen and Jæger, 2013).

Multiple studies have leveraged terrorist attacks to investigate a rally-effect.[2] Most noteworthy, the 11/3 Madrid terrorist attack (Dinesen and Jæger, 2013) and the Charlie Hebdo terrorist attack (Muñoz *et al.*, 2020). Studies on both these events show an increase in trust in or satisfaction with government institutions after the attacks. When it comes to research on the political behavioral and electoral consequences in Western-European countries rather than attitudinal ones, however, insights are scarce and problematic. One study looking at the professed support for the incumbent party before and after several terrorist attacks conducted by Euskadi Ta Askatasuna (ETA), revealed no evidence for the rally-effect (Balcells and Torrats-Espinosa, 2018). A second study opposes the rally-effect by demonstrating how the actual votes cast before and after the the 2004 Madrid bombings shifted from the Spanish incumbent conservative to the Spanish opposition Socialist Party (PSOE) (Montalvo, 2011).

The latter finding, however, is remarkable and may be proof for the clever use of a political strategy rather than a lack of evidence for a rally-effect. That is, right after the 2004 Madrid bombings, the Spanish government attributed blame to ETA, even though the media soon

[2]Note that aside from reasons of case-selection (e.g., the two requirements) and timing (right before an election), we exclude studies on Israel in this review as these investigate support for a left-wing versus right wing bloc after a terrorist attack rather than support for a (populist) opposition versus the incumbent as is the case in Western-Europe.

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revealed the attacks were carried out by Al-Qaeda (Canel and Sanders, 2010; Canel, 2012). The incumbent party Partido Popular was, consequentially, accused of lying and no media outlets supported the government's response to the terrorist attack (Canel and Sanders, 2010; Canel, 2012). Due to these circumstances, the effect on actual votes before and after the 2004 Madrid bombings cannot be isolated (Montalvo, 2011). Was the increase in support for the socialist party an effect of the bombings, the governments' response, or the media's reaction to it?

While the electoral consequences of a terrorist attack thus remain unclear, there is some evidence pointing toward the rally-effect measured as increased trust in or satisfaction with government institutions. While we will not directly test an attack's consequences on (positive) attitudes toward the government,[3] we build our argument on the assumption that shifts in attitudes may translate into party preferences. Moreover, a rally-effect can be about incumbent parties in general or an incumbent figure (e.g., President in a presidential system or Prime Minister in a parliamentary system) (Dinesen and Jæger, 2013). Considering the highly fragmented nature of the Dutch party system and four parties in government at the time of the attack, we expect the party that is lead by the Dutch Prime Minister to gain most. On these grounds, we formulate our first hypothesis:

Hypothesis 1: *A terrorist attack causes a rally-effect: After an attack the likelihood of voting for incumbent parties increases – especially for the party that hosts the incumbent figure*

Populist radical right parties

One of the explanations given for the successes and rise of PRRPs is found on the supply-side, i.e., the parties from which the electorate can choose (Irwin and Van Holsteyn, 2003; Meguid, 2005). For PRRPs to thrive, other parties arguably must have not appealed sufficiently 'to those who held the most rightist positions on issues such as treatment of criminals and immigrants' (Irwin and Van Holsteyn, 2003). Indeed, in the more technical parlance of traditional spatial models, if the spatial distance between voters' policy preferences and existing parties is too big, then a (new) party can pop up and position itself to attract these voters (Downs, 1957). Political entrepreneurs may exploit this and make issues that are unaddressed by mainstream parties more important in the eyes of voters (De Vries *et al.*, 2020).

Similarly, in a case study on PRRPs in the Netherlands, Van Kessel (2011) argues that the performance of PRRPs depends on a combination of causal conditions: the availability of the electorate, the responsiveness of established parties, and the supply of credible PRRPs. The availability of the electorate relates to voters' ties with established parties: the stronger the ties a voter has with an established party, the less available this voter is. The weaker these ties are, the easier it becomes for (new) parties to sway voters. Therefore, one could argue the success of PRRPs is dependent on the degree to which established parties represent sufficiently or are responsive to citizens' attitudes toward specific issues. In particular immigration, as immigration is an issue most commonly and extensively owned by PRRPs (Van der Brug and Berkhout, 2015). Immigration itself (such as influx of refugees) is also found to increase PRRP support (Dinas *et al.*, 2019).

In the case of a terrorist attack[4] more specifically, a PRRP may have a strategic advantage due to its ownership of and its extreme political stance on immigration[5] (Walgrave *et al.*, 2009; Van der Brug and Berkhout, 2015). For example, multiple studies found negative effects on attitudes

[3]Our dataset does not contain variables on trust or satisfaction with government.

[4]In this study we focus on domestic Islamic terrorism. Other types of terrorism exist, which may have differential outcomes.

[5]Note that terrorist attacks may also change attitudes on another issue, depending on the characteristics of the terrorist attack and the circumstances it occurred in. For example, see: (Jakobsson and Blom, 2014).

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toward immigration as a result of the Paris attacks in 2015 (Ferrin *et al.*, 2019) and the murder of Theo van Gogh (Boomgaarden and Vreese, 2007; Das *et al.*, 2009). These studies all seem to have the same starting point: disastrous events like a terrorist attack may impact attitudes such that it could provoke political reactions with corresponding consequences, especially close to elections (Ferrin *et al.*, 2019). This could thus entail that an attack could bear the political consequence of increasing support for PRRPs if immigration attitudes are negatively affected or become more salient to voters, causing them to move closer to radical right parties or prefer them as they are issue owners of anti-immigration policies.

To date, research linking potential attitudinal shifts toward immigration in the aftermath of an attack to actual voting behavior is lacking. The arguments outlined above foster the idea that a terrorist attack has the potential to affect citizens' attitudes on immigration,[6] making established parties more vulnerable to PRRPs owning the issue (Van Kessel, 2011). To this end, a second hypothesis is formulated:

Hypothesis 2: *A terrorist attack benefits Populist Radical Right Parties (PRRPs): after an attack the likelihood of voting for a PRRP increases.*

Regarding our hypotheses we want to make two additional remarks. The first concerns the seeming competing nature of our hypotheses. While we test them side-by-side, it is important to note that both mechanisms may occur at the same time. On the one hand, finding support for the rally-effect does not exclude the possibility that voters' developed or enhanced negative attitudes toward immigration, nor does increased support for PRRPs rule out the option that voters enhanced other attitudes that would line up better with those of the incumbent party. On the other, some voters may increase their support for PRRPs as a response to the terrorist attack, while others rally around the flag.

The second and somewhat related remark is that not all voters may be similarly affected. Generally speaking, voters usually do not change their whole worldviews in response to a terror attack, but some voters may be more susceptible to the influence of such events. Berrebi and Klor (2006), for instance, find that while Israeli right-leaning voters increased their support for PRRPs in response to terrorism, left-leaning voters instead *de*creased their support. They also found that the effect depends on the place a voter lives. Left-leaning voters decreased their support *if* they lived in a place other than where the attack took place. This matters, because terrorism may thus have an important impact beyond increasing support for right-leaning parties by causing ideological polarization of the electorate. Indeed, '[. . .] terror fatalities appear to reinforce preexisting views of the electorate[. . .]' (Berrebi and Klor, 2008: 292). This study, however, is – to the best of our knowledge – the only one finding such a polarizing effect. In European contexts, little evidence is found that ideology shapes the response to an Islamic terrorist attack (Larsen *et al.*, 2020). Nonetheless, in this study we explore this role of ideology.

The Dutch case: incumbent and populist radical right parties and the 18/3 Utrecht Terrorist Attack

The electoral system of the Netherlands is a multi-party system with numerous political parties. As a single political party has little chance of winning a majority, parties work together and form coalitions. The Third Rutte cabinet has been the ruling coalition in the Netherlands since October 2017. It was formed by a coalition-government of four parties: the People's Party for Freedom and Democracy (VVD), Democrats 66 (D66), the Christian Democratic Appeal (CDA), and the Christian Union (CU). In this study, these parties are thus considered the incumbent parties. Of these four parties, VVD has been the biggest party for the past three election terms with currently

[6]Note that our dataset does allow us to test this mechanism, as it contains a variable on immigration attitudes.

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32 seats, followed by D66 and CDA with both 19 seats, and the CU with 5 seats in the House of Representatives (Second Chamber).

The leader of the VVD, Mark Rutte, is the Prime Minister of the Netherlands since 2010. The main causes VVD supports are private enterprise and economic liberalism, with a political platform combining support for tax reductions and decentralization. D66 is a progressive, social-liberal, pro European Union party. CDA and CU both are Christian-democratic parties, but the CU holds socially conservative positions on issues as same-sex marriage, abortion, and euthanasia. From these incumbent parties, only the VVD has a negative stance toward immigration. The CDA, CU, and D66 are generally favorably disposed toward immigration. This also counts for the largest parties in the opposition, The Green Left (GL), the Labor Party (PvdA), the Party for Animals (PvdD), and the Socialist Party (SP).

At the time of the attack, there were two PRRPs, the newer FvD and the older Freedom Party (PVV) that emerged in 2005. Since 2017, Geert Wilders' PVV is the second-largest party in Parliament with 20 seats. On immigration and culture, the party is nationalistic and wants to 'stop people from coming to the Netherlands', especially from non-Western countries and Muslim-majority countries such as Turkey and Morocco.

Founded in 2016, the FvD is a relatively new party. Ideologically, the FvD is a conservative, right wing party. Its main objectives are to 'break open the party cartel', leave the European Union, increase border security, introduce binding referendums and direct democracy, promote Dutch history and culture, and to implement a restrictive immigration policy.[7] Since March 23rd 2017, the party is represented in the House of Representatives with two seats, and with an amount of nearly 31.000 members, the FvD has become the fourth biggest since February 2019 in terms of membership.

Like the incumbent parties and the PVV, the FvD was running for the 2019 Dutch Provincial Elections. Unlike the other parties, however, the FvD continued campaigning after the 18/3 Utrecht terrorist attack whereas all other parties suspended their campaign activities. During a meeting right after the shooting took place,[8] Theo Hiddema – the right hand of the party leader Thiery Baudet – claimed that other parties dropping their campaign activities raised an opportunity to gain more votes. Moreover, Thiery Baudet made a direct link between the incident and the immigration and integration policies of the Netherlands because of the Turkish origins of the shooter. For the same reason, the FvD claimed to already know the terrorist motives of the shooter, despite the investigation still being ongoing.[9] This perspective, linking the migration background of the shooter to his motives and more generally the issue of immigration, was also largely present in the media.

The Turkish–Dutch are the second largest population group in the country, and often the object of Dutch political parties when discussing the immigration issue. The proclaimed Turkish roots of the shooter were therefore the perfect occasion for the FvD to increase the salience of the immigration issue. And, as Theo Hiddema had mentioned, the FvD seized that opportunity to continue their campaign regardless – or perhaps because – all the other parties dropped their activities.

In sum, the terrorist attack that occurred on the 18th of March 2019 in the Netherlands provides a unique opportunity to test the two competing theories for multiple reasons. First, it meets the requirements by having not only one but two PRRPs (FvD and the PVV). Second, none of the incumbent parties (VVD, D66, CDA, and CU) are PRRPs. Third, the Netherlands is an extremely proportional multi-party system, allowing the Dutch electorate to switch between parties more easily due to typical looser ties and an exceptional high number of parties

[7]https://forumvoordemocratie.nl/standpunten.

[8]https://www.metronieuws.nl/in-het-nieuws/2019/03/baudet-onder-vuur-vanwege-reactie-op-utrecht.

[9]The General Intelligence and Security Service (AIVD) of the Netherlands, later (after the elections) found evidence strongly suggesting terrorist motives of the shooter.

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(Van Kessel, 2011). Lastly, the attack took place only two days before the elections, which is crucial for the design used in this paper and will be discussed in the next section.

Still, it is important to note that even though the attack meets these requirements and was highly salient, it is not comparable to the scale of the previously discussed attacks in the USA, Spain, France, or Germany. This means that this study is a conservative test of our hypotheses, as more impactful attacks could have more severe consequences for incumbent or PRRP support.

Data and design

Data

The perfect test of our hypotheses is a world where it is randomized if people hear about a terrorist attack. This world does not exist, and therefore we will rely on causal inference using VAA data. VAAs are online tools that help citizens decide which party to vote for by comparing their personal policy preferences with the political stances of political (Krouwel *et al.*, 2012; Garzia and Marschall, 2019). VAA users then see an ordered list indicating how close parties are to them based on the congruence between the parties and voters. VAA data thus contains valuable information regarding users' stances on a variety of issues and often also includes general demographic information such as age, gender, and education. Examples of VAAs are the German 'Wahl-O-Mat', and the Dutch 'Stemwijzer' and 'Kieskompas'. Whilst relatively new tools, in many Western-European countries millions of people use these tools to obtain voting advice before elections. Arguably they might thus be as influential as prime-time election debates, if not more.

We reached out to Kieskompas and asked for access to their data for research purposes.[10] We received an anonymized version of the VAA data that we used in the analysis. The application runs before every major election in the Netherlands and is widely used. Users first answer some questions about personal characteristics (e.g., age, gender, and education), followed by their positions on a battery of policy questions. Subsequently, users are asked about their likelihood to vote for various parties, followed by the actual voting advice (calculated based on their positions and those of the parties on an economic left-right and cultural progressive–conservative dimension). Finally, respondents are asked which party they intend to vote for. The data also contains some technical variables such as the time users spent answering the survey, the number of times they re-opened the application (for instance because they did not finish or started over), the type of device they used, and their geographical location. The Kieskompas data contains a total of 413.175 respondents.

To provide some insight into the representativeness of the Kieskompas data, Fig. 2 shows how the Kieskompas VAA sample differs from round eight of the European Social Survey (ESS). The ESS is a useful reference point as it is Europe's most widely used survey and uses a representative sample (European Social Survey European Research Infrastructure (ESS ERIC), 2021). The VAA sample is skewed toward younger cohorts, the higher educated, and has more men. This is expected because younger people are more likely to use online applications and education and gender are known to be related to political interest (Van de Pol *et al.*, 2014). As an opt-in sample, it is thus not representative of the general population. This means that any sample-average-treatment effects that we estimate, even if completely causally identified, may differ from the true population-average-treatment effect. However, the amount of users makes the sample large enough to sway an election on its own, particularly as younger voters with lower levels of party attachment may be more easily influenced by events and voting advice than voters with long-standing party affiliations (Van de Pol *et al.*, 2014). In addition, for the type of design we use in this study, a large number of respondents around the treatment helps exclude potential confounders by using a smaller bandwidth, whilst still allowing the detection of effects. In all, it is a

[10]Their website can be found at www.kieskompas.nl. The data that we used can be found on OSF here: https://osf.io/h6wjn/.

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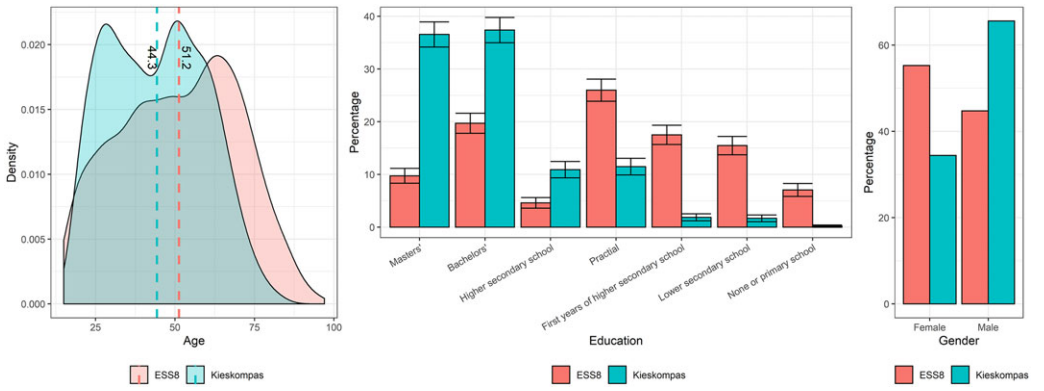


Figure 2. Sample comparisons of the VAA with the ESS8.

very useful dataset for our purposes, even if it is not a perfectly representative sample of the population.

Design

Unexpected event during survey design

We use an UESD. UESDs are an increasingly common identification strategy used to split a sample into treatment and control groups. In a UESD, the random occurrence of an unexpected and salient event, T, during the fieldwork of a survey is used to estimate the causal effect of that event on an outcome (Muñoz *et al.*, 2020). A respondent i receives a treatment D if she has been interviewed *after* an event T, whilst she is assigned as untreated if she has been interviewed *before*: $D = 1|t_i \rangle t_T$ and $D = 0|t_i < t_T$.

The UESD relies on two identification assumptions: the *excludability assumption* and the *temporal ignorability assumption* (Muñoz *et al.*, 2020). The excludability assumption states that any difference in the outcome between respondents before and after the event is only caused by the event. In other words, time should only affect the outcome through the occurrence of the event of interest. This assumption may be violated if there are other events post the treatment event or due to unrelated time trends. According to the temporal ignorability assumption, time should not be related to an individual's potential outcomes. The moment of the interview should thus be as good as random, meaning that respondents before and after the event are on average the same.

UESDs and VAA data

We argue that computational big data such as the kind VAAs produce is suited for causal inference using UESDs because the excludability assumption is easier to meet due to the high number of respondents. In conventional surveys, the fieldwork often takes up much more time, which means that the bandwidth before and after the treatment has to be larger in order to have enough power. For example, the UESD design used in Finseraas *et al.* (2011) and Castanho Silva (2018) use treatment windows of four weeks on both sides of the treatment event. As a result, the excludability assumption is not very likely to be met because the larger time span means that there is a higher likelihood of co-occurring events that correlate with the outcome. VAA data clearly does not have this problem as the larger number of respondents means that the windows can be much smaller.

However, the temporal ignorability assumption is *harder* to meet when using VAA data for two reasons. First, respondents choose the moment they do the survey themselves and different people do the survey at different moments. Respondent characteristics may therefore correlate with:

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(i) the moment of the day people choose to do the survey, and (ii) the number of days from an election they use the tool. For instance, Tables A2 and A1 in the Online Appendix show how respondent characteristics predict the moment of the day (from 0 to 24 hours) and the number of days from the election people do the survey (ranging from 0 to 5 days). People who are higher educated are more likely to do the survey earlier in the day, whilst older people do it later. Moreover, left-leaning participants do the survey further away from the elections, and so do progressives and women.

The second reason that makes it harder to meet the temporal ignorability assumption when using VAA data stems from the fact that respondents may self-select into the survey as a result of the treatment event. This situation may occur if, for instance, certain people are reminded about the election due to the event, which prompts them to do the survey. In all, both these factors imply that respondent covariates may correlate with the moment someone chooses to do the survey and therefore with the timing of the event. This correlation could pose a challenge to the temporal ignorability assumption.

We propose three solutions to these potential violations of the temporal ignorability assumption when using VAA data in a UESD. First, it is important to make sure that the moment of the day people do the VAA does not affect the outcome by setting the treatment and control windows in the same period of the day and as close to each other as possible. For instance, if the treatment window is from 20:00 to 8:00 the next day, then the control window should also be between 20:00 and 8:00 on a day as close to the event as possible. Doing this controls for the fact that at different moments of the day different people take the survey and thus makes sure that the moment of the day people do the survey is not creating an imbalance between treatment and control. Second, the effect should remain significant if the treatment window increases in size and be robust to the choice of treatment windows. This shows that the effect is robust further away from the event with different respondents in the sample. Doing so additionally indicates that the effect is not short-lived. Third, we propose to use placebo tests to show that any remaining violations of the temporal ignorability assumption do not drive the results. To be precise, it is important to conduct placebo-treatment tests by using other treatment windows. This shows that time trends do not drive the results. In addition, placebo outcome tests should show that the treatment does not influence respondent characteristics. This indicates that any results are not driven by a particular type of person self-selecting into the survey as a result of the treatment – note that in this case the placebo outcome test is the same as a balance check that predicts treatment status as a function of respondent covariates.

The design used in this paper

Our study provides a practical example of how VAAs data can be utilized in an UESD. The main independent variable in our design is a binary treatment status indicator: individuals surveyed before the attack are labeled as untreated, while those surveyed after are considered treated. The critical event in question occurred on March 18th, 2019, at 10:42 AM (Dutch Winter Time, GMT + 1). We then established a nine-hour 'exclusion window' following the event, lasting until 8:00 PM, during which we did not include any respondents in either the treatment or control group. We do so to ensure maximum exposure to news about the event. By the end of this window, after the work day, the evening news, and family dinners, we believe it highly probable that nearly all residents in the Netherlands would be aware of the event, thus minimizing the presence of 'non-takers.' Consequently, this nine-hour 'exclusion window' helps us to assure near-complete compliance with treatment assignment.

After the exclusion window, we use a treatment window that is at the same time of the day as the control window. The exact start and end times of these windows is hard to choose because we want to stay as close to the actual event as possible, while the end time of the control window is limited by the timing of the event (the control window should of course end before the attack).

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Table 1. Covariate balance tests for different treatment windows

	Same-day	8 hour window	14 hour window	24 hour window
	(1)	(2)	(3)	(4)
Age	−0.001	−0.003**	−0.003**	−0.005**
	(0.001)	(0.001)	(0.001)	(0.0004)
Education	−0.032*	−0.009	−0.010	0.011*
	(0.016)	(0.015)	(0.011)	(0.005)
Female	0.018	−0.023	0.025	0.015
	(0.039)	(0.036)	(0.027)	(0.013)
Times visited	1/12	1/14	0/14	2/20
Province	0/6	0/6	1/6	2/6
Constant	0.690**	0.615**	0.673**	0.929**
	(0.097)	(0.087)	(0.067)	(0.032)
Observations	756	894	1,582	5,066

*$P < 0.05$; **$P < 0.01$.

Note: Linear probability models predicting the treatment status (1 or 0) with different treatment windows as a function of covariates. Times visited and province show the number of significant dummies out of the total. Note that times visited captures how often an IP address shows up in the application. This may be multiple times if people open the application a second time at a later point to finish or re-do the application.

To decide the optimal treatment and control windows, we run several covariate balance tests, reported in Table 1. Column (1) reports the covariate balance by regressing treatment status on covariates using a linear probability model for a treatment window and control window that are on the same day. This means that the treatment window runs from 20:00 until 24:00. The accompanying control window starts at 00:01 and runs until the attack at 10:42. When using this window, education is significant, possibly because the moments of the day in treatment and control are not the same. In column (2), we use an eight hour window that starts on the 18th at 20:00 and stops at 2:00 on the 19th. The control window uses the same times but on the 17th and 18th. As we can see, the coviarate balance is better. The 14 hour window reported in column (3) uses the biggest treatment and control windows without relying on any correspondents on the 16th. That is, the control window starts at 20:00 on the 17th and stops at 10:42 on the 18th, whilst the treatment window runs from 20:00 on the 18th until 10:42 on the 19th. Note that in both models (2) and (3) the treatment and control windows are thus on the same part of the day, whilst at the same time the control window does not start earlier than the day before the event (i.e., the 17th). In model (4), we use a 24-hour window where the treatment runs from 20:00 on the 18th until the same time on the 19th, whilst the control window (which now has to start on the 16th to not end after the event) runs from 20:00 on the 16th until 20:00 on the 17th. Comparing these four windows, clearly the treatment and control windows reported in model (3) are superior because the covariate balance is better than in model (1) and (4), whilst the N is higher than in model (2). We thus choose the 14-hour window shown in model (3) because the covariate balance is best and show that the results are robust to the selection of other windows.[11]

In addition, we let the treatment window that uses the same time of the day as described above (model 3) grow to show that the results are robust for the end time of the treatment window and to demonstrate that the results are not short lived. To be precise, whilst keeping the control window constant, we increase the end time of the treatment window in steps of one hour until the afternoon on election day. Figure 3 shows the treatment window in model (3) that we will expand

[11]Note that the test reported in 1 can also be seen as a placebo-outcome test that checks whether a particular type of respondent is more likely to participate in the survey as a result of the treatment. The results indicate that this is unlikely to be the case, albeit there is a small significant effect for age. The robustness checks in the appendix show that controlling for age does not lead to any substantial changes in the treatment effect.

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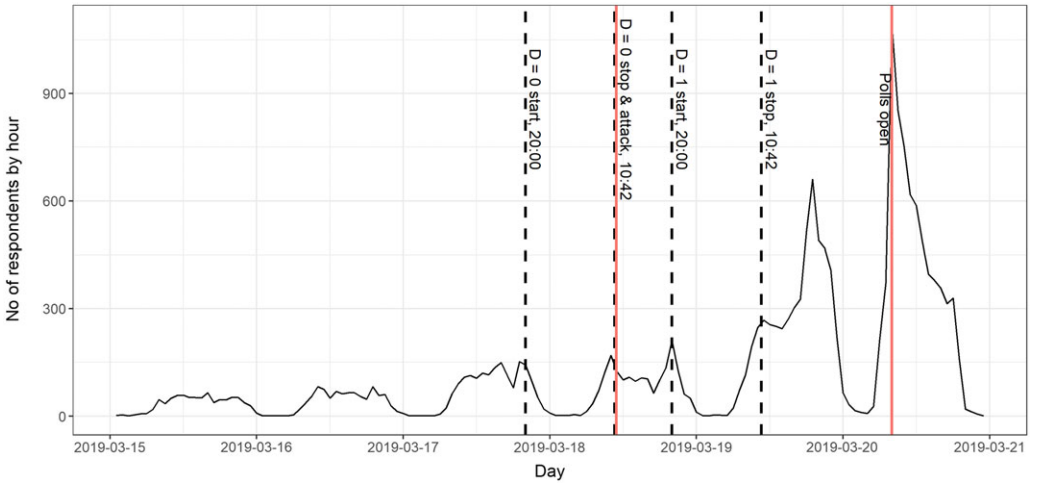


Figure 3. 14-hour treatment windows.

by one hour in every model.[12] As the treatment and control dashed v-lines show, we use the same moment of the day in both treatment and control windows (to meet the temporal ignorability assumption). The figure also shows how close our treatment windows are to the actual election.

The main question in Kieskompas that we use as our dependent variable asks 'how likely are you to ever vote for party X'. These questions use a 10-step thermostat and are asked before the respondent sees their voting advice and thus not contaminated by the VAA results. Research in the US context has shown that such Likelihood-to-vote Likert thermostats are a good indicator for actual voting behavior (Dalton and Klingemann, 2007: 82). One additional benefit of these variables, in our case, is that the additional variation in the outcome adds extra power to the design. In the Online Appendix we also use a question people see after the voting advice that asks directly which party a given respondent will vote for. To ensure balance between the treatment and control windows, we also control for a battery of other (pre-treatment) variables, specifically: age, education, province, and the number of times an IP address shows up in the data (due to people who enter the website multiple times or re-do the VAA). We present the results when controlling using regression, and in the Online Appendix we additionally control using matching and show the results without controls. The results are consistent using these different approaches.

Results

Main findings: party preferences

To test our hypotheses, we focus particularly on the coalition (H1) and PRRPs (H2) running for office during the 2019 Dutch Provincial Elections. We use a linear model that predicts the self-reported likelihood of citizens to vote for different parties:

$$Y_i = \alpha + \rho D_i + \mathbf{X}_i\gamma + \varepsilon_i \quad (1)$$

the likelihood to vote for a party Y for a voter i is determined by her treatment status D, alongside a battery of individual-level control variables X_i and the error term ε.

Our main results are reported in Table 2. The results show a strong significant effect of the treatment on voting for the VVD and D66, the two most visible parties in the government

[12]In addition, the figure shows the patterns described earlier: more people do the survey closer to election day and there is a clear pattern in the time of the day when people do the survey.

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Table 2. Dependent variable: Likelihood to vote – 14 hour window, coalition, and populist parties

	PVV	FVD	CU	VVD	CDA	D66
Treatment	0.128	0.235	−0.204	0.573**	0.017	0.301*
	(0.113)	(0.138)	(0.134)	(0.145)	(0.129)	(0.120)
Controls	✓	✓	✓	✓	✓	✓
Observations	1,789	1,785	2,011	1,715	2,073	1,926

*$P < 0.05$; **$P < 0.01$.
Note: Linear models predicting voters self-reported likelihood that they would vote for a party on a 10 point Likert scale. The treatment uses the 14-hour window. Controls include: provincial fixed effects, education, age, gender, and amounts visited. The results are similar without including controls, with other treatment windows, and when matching (presented in the Appendix).

Table 3. Dependent variable: Likelihood to vote – 14 hour window, other parties

	PvdA	SP	GL	PvdD
Treatment	−0.197	−0.116	−0.165	−0.091
	(0.130)	(0.136)	(0.144)	(0.146)
Controls	✓	✓	✓	✓
Observations	1,774	1,711	1,817	1,737

*$P < 0.05$; **$P < 0.01$.
Note: Linear models predicting voters self-reported likelihood that they would vote for a party on a 10 point Likert scale. The treatment uses the 14-hour window. Controls include: provincial fixed effects, education, age, gender, and amounts visited.

coalition. More specifically, in the 14 hours after the attack, there is an, on average, 0.57 higher likelihood to vote for the VVD and 0.30 to vote D66 on a 0–10 scale. The effect size for the VVD is about 19% of the standard deviation of the variable, and thus substantial. These results thus indicate that the terrorist attack caused a strong rally-effect for the main incumbent party (VVD) and one of their coalition partners (D66), confirming H1. The attack did not spark more support for any of the largest four non-PRRP opposition parties (see Table 3).

Furthermore, we explore the role of voters' ideology (left- versus right-leaning) as well as the region ('locality') they live in. Following (Berrebi and Klor, 2008), it may be that the terror attack motivated left-leaning voters living in a region *other* than the one the terror attack occurred in (Utrecht) to decrease their support for right wing parties. We explore this by employing an additional analysis including an interaction term between the treatment and ideology, and run this analysis separately for all voters not living in Utrecht as well as including those that live in Utrecht. We divided voters into a left- and right-leaning block based on their stance on all issues in the VAA, and consider someone to be living in Utrecht when they did the VAA for the province of Utrecht.[13]

In Tables A11 and A12 as well as in Figure A4 in the Online Appendix, we show that right-leaning voters *not* living in Utrecht were more likely to vote for the FvD (one of the main PRRPs), whereas those from Utrecht were *less* likely to do so. In the sample as a whole (not splitting it up based on locality), we do not find a significant interaction effect with ideology. We address this result in more detail in the Discussion.

Robustness analyses

To show that the results are robust using different time windows and that the effects last up to election day, Fig. 4 plots the results of the main model but with the treatment window increasing

[13]While this is not a perfect indicator of where respondents live, this is the only variable available in our dataset that allows us to take into account locality, and we may assume that people take the VAA for the region they live as that is also the only one they are allowed to vote in (and thus most relevant).

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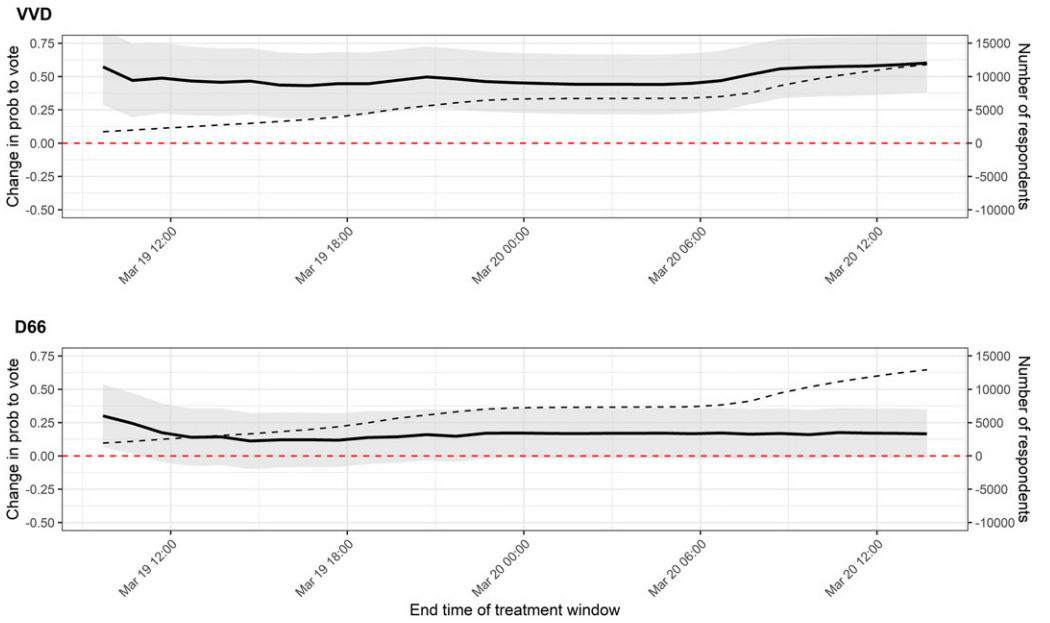


Figure 4. Increasing size of treatment windows.
Note: The figure plots what happens to the estimates (the solid black line) and the N (the dashed black line) if we expand the end time of the 14-hour treatment window for the VVD and D66. The x-axis indicates the end time of the treatment window. The results for all the parties can be found in the Online Appendix in Figure A1.

in size by an hour – the X-axis indicates the end time of the treatment window.[14] In both plots we see that, as expected, the number of observations (the dotted line) increase as we get closer to the actual election as more people do the VAA and the treatment windows are increasing in size. The figure shows that the effect (the black line) for the VVD remains consistent as the confidence interval (the gray band) does not overlap with 0. The effect for D66 dissipates as it is marginally not significant anymore at the 5% significance level as the end time of the treatment window increases, but briefly becomes significant again on election day. This is according to our expectations, as the VDD is the most visible party because they provide the Prime Minister. The initial significant result for D66 could be explained by the fact that their voters are more likely to live in urban areas and thus be directly impacted by the consequences of the attack.

As a second robustness test, we repeat the main model using the 14-hour treatment window yet using propensity score matching as opposed to regression. The results from this test are reported in Table A5 in the Online Appendix. Matching gives us similar results as a linear model with controls, yet the effect for the VVD is stronger using matching (0.716 vs 0.573). In addition, the matching test shows a significant effect for the FvD. As a third robustness test, we repeat the main analysis using the other three treatment windows reported in the Covariate Balance test in Table 1. The results of this test are shown in Appendix Tables A9, A10, and A8. The 8-hour window, which is the other most balanced window, shows very similar results. The same-day window fails to find a significant result, yet this model has the lowest N, and the 24-hour window sees a strong result for the CU alongside a weaker effect for the VVD, which is likely caused by an imbalance of age and education. The signs of the effects in all the different windows point in the expected direction. Overall, the robustness analyses confirm the rally-effect, especially for the VVD as the most visible party in the coalition.

[14]In the Online Appendix, we report these figures for all other parties.

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Table 4. Dependent variable: Likelihood to vote – 14 hour placebo window

	PVV	FVD	CU	VVD	CDA	D66
Placebo	−0.049	−0.206	0.253	−0.206	0.099	0.025
	(0.139)	(0.190)	(0.171)	(0.190)	(0.162)	(0.160)
Controls	✓	✓	✓	✓	✓	✓
Observations	1,181	1,127	1,359	1,127	1,408	1,296

*$P < 0.05$; **$P < 0.01$.
Note: Linear models predicting how likely voters self-report to vote for a party on a 10 point Likert scale. The placebo treatment uses the 14-hour window but with both treatment and control before the election. Controls include: provincial fixed effects, education, age, gender, and amounts visited.

Table 5. Dependent variable: Likelihood to vote – 24 hour placebo window

	PVV	FVD	CU	VVD	CDA	D66
Placebo	−0.024	−0.046	0.024	−0.046	0.063	0.027
	(0.088)	(0.118)	(0.107)	(0.118)	(0.102)	(0.099)
Controls	✓	✓	✓	✓	✓	✓
Observations	2,910	2,786	3,333	2,786	3,474	3,177

*$P < 0.05$; **$P < 0.01$.
Note: Linear models predicting how likely voters self-report to vote for a party on a 10 point Likert scale. The placebo treatment uses the 24-hour window but with both treatment and control before the election. Controls include: provincial fixed effects, education, age, gender, and amounts visited.

As a final robustness check, presented in the Online Appendix in Table A6 and Figure A2, we run the sensitivity analysis tools from (Cinelli and Hazlett, 2020). We focus on Age, which is the covariate that predicts which people self-select into taking the survey at a particular moment and on which we have the most imbalance. We find that even if any unobserved covariates are confounding three times as much as Age, they would still not render the results insignificant.

Placebo analysis

The purpose of placebo analysis is to check for endogeneity by showing that there is no effect when there should not be one. There is one problem that might be unaccounted for: pre-existing time trends. In our case, pre-existing time trends may be a violation of the excludability assumption or the ignorability assumption. Pre-existing time trends violate the excludability assumption if they occur because, for instance, the public 'mood' in the country becomes a little bit more conservative in the evening or closer to the actual election date. If this were the case, then time does not only influence the outcome through the event (the treatment) but also through such a change in mood. Time trends may also violate the temporal ignorability assumption if certain types of respondents self-select into taking the VAA at particular days and times. This is of particular concern if there are more respondents who like the VVD in the sample closer to the election. If this were the case, it could explain the positive effect for the VVD that we find. Do note, however, that time trends of this kind would have to be very strong to explain our effect as the time windows that we use are extremely small compared to other UESDs.

To check whether self-selection into the survey and generally more positive evaluations of the VVD closer to the election drive our effects, we run a placebo-treatment test that uses the same 14-hour window as in the main model, yet now placed completely *before* the event: the control window starts at 20:00 on the 16th and stops on the 17th at 10:42. We then run the same test using a 24-hour placebo-treatment window. If there are pre-existing trends of the sort described above, then these tests should show an effect similar to our main results. Tables 4 and 5 show the results of these placebo tests. As we find no significant effects, the placebo test excludes that time trends

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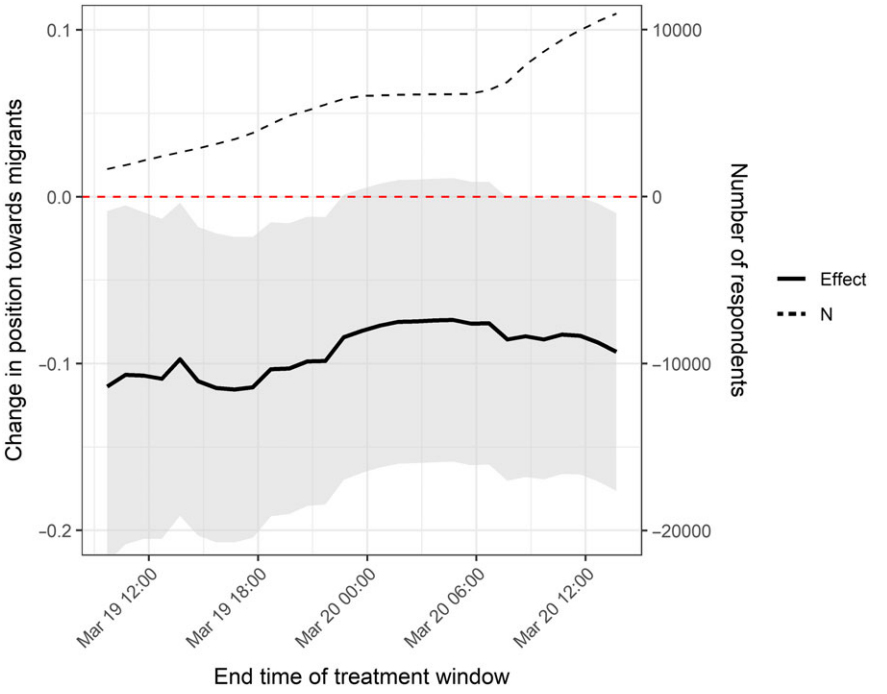


Figure 5. Positions toward migrants.

drive our results, therefore providing additional evidence that self-selection into participation in the VAA does not drive our effects.

Testing the causal mechanism: immigration attitudes

Our results lead us to reject H2: the terrorist attack did not increase citizens' likelihood to vote for PRRPs (PVV and FvD). However, the assumption underlying H2 is that such an event fuels a negative attitudinal shift toward immigration, arguably motivating citizens to vote for PRRPs since these are the most extreme on immigration (and citizens thus become closer to these parties). Although our results do not confirm an increase in the likelihood to vote for PRRPs, voters could still have become more anti-immigration. This would be an issue for our results if any of the other parties – especially the incumbents – are also anti-immigration, which is true only in the case of the VVD. To be precise, a shift on immigration attitudes means that the VVD could potentially have gained electorally because, out of the four incumbent parties, it is most anti-immigration. We test this possibility by looking at attitudinal shifts on immigration and using a mediation analysis to see whether any changes in immigration attitudes influence voting for the VVD.

To test whether people change their positions toward immigration we run the same model as before, but now with a question asking people about immigration on a five-point Likert scale as the dependent variable. Higher values mean that respondents think more money should be spent on asylum seekers. In Fig. 5, we plot how their attitudes increase if we use the 14-hour treatment window and increase it by the hour. Whilst a very small effect size, the results indicate that voters post-event have significantly more negative toward immigration than those before.[15] Even though

[15]Note that the sample size is much bigger in this test because this question is earlier in the survey than the voting intention questions and fewer respondents have thus dropped out.

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Table 6. Likelihood to vote, mediation analysis – 14 hour window

	VVD	D66	CDA	CU	PVV	FvD
ACME	0.059*	−0.052	0.026	−0.007	0.106*	0.136*
ADE	0.506**	0.380**	−0.015	−0.193	0.008	0.089
Total effect	0.565**	0.328**	0.011	−0.201	0.114	0.226
Prop. mediated	0.103*	−0.153	0.073	0.03	0.689	0.566
Controls	✓	✓	✓	✓	✓	✓
Observations	1668	1874	2016	1957	1739	1739
Simulations	1,000	1,000	1,000	1,000	1,000	1,000

*$P < 0.05$; **$P < 0.01$.

Note: Mediation analysis predicting how much of the treatment effect runs through changes in immigration positions (on a 5-point Likert scale). ACME, average combined mediation effect; ADE, average direct effect. The treatment uses the 14-hour window. Controls include: provincial fixed effects, education, age, gender, and amounts visited.

the evidence is weak, we can thus not completely rule out the idea that terrorist attack may increase PRRP support.

To explore whether these changes in immigration attitudes drove the effect for the VVD we employ mediation analysis, using the appropriate software developed by Kosuke Imai and collaborators (Imai *et al.*, 2011). A mediation model seeks to identify a third variable through which the effect of *X* on *Y* runs. The results, reported in Table 6, indicate that the effect for the VVD was only for 10% caused by changes in immigration attitudes, whilst the effect for D66 has nothing to do with immigration attitudes.

The substantive interpretation of all our findings may seem ambiguous. Why did citizens favor the coalition parties more than PRRPs who are issue owners of immigration and have a more negative position toward it? And why is the effect, from all four coalition parties, most prominent for the VVD? Perhaps the answer lies in the nature of the event: in times of crisis, citizens' familiarity with coalition parties – especially with those coalition parties that are most visible – causes them to choose familiarity over proximity. In our case, the main effect for the VVD thus seems to be explained by a rally-effect: in comparison to the other incumbent parties, the VVD is most visible as this party provides the Prime Minister. To conclude, during uncertain episodes citizens choose to rally behind the known and proximity becomes less important: *after terrorist attacks, citizens prefer prominence over proximity.*

Conclusion and discussion

Do terrorist attacks have the capacity to sway voters in favor of incumbent parties over PRRPs, or is it rather the other way around? This paper aimed to test these mechanisms side-by-side on the potential answer to this question. Using the 18/3 Utrecht terrorist attack as a case study, we asked how a terrorist attack affects party preferences.

By relying on VAA data in an UESD, we find evidence for the rally-effect: after an attack the likelihood of voting for incumbent parties increases (Hypothesis 1). No strong evidence was found for the notion that a terrorist attack benefits PRRPs (Hypothesis 2). Interestingly, however, the attack did negatively affected respondents' attitudes toward immigration – as Hypothesis 2 would predict. That is, after the attack respondents were generally less favorable toward asylum seekers. Noteworthy, as the mediation analysis has shown, this negative shift toward immigration only explained a small part of the effect for the VVD, and does not translate in a significant increase in the vote share of PRRPs. In addition, we find a significant effect for the D66 who are pro-immigration. While we cannot say there is no evidence for increased PRRP support as a response to a terrorist attack at all, our main body of evidence leads us to the conclusion that during times of crisis citizens rally around the flag, and that attitudinal shifts may be less important. The stronger

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effect for the VVD we explain by its visibility: from the four parties, the VVD is most visible as it provides the Prime Minister. To conclude, in the face of specific, dramatic and sudden events, voters are more approving of the incumbent party and thus rally around the flag.

We also explored the role of voters' ideology and the region they live in. Following Berrebi and Klor (2008), it may be the terror attack motivated left-leaning voters living in a region *other* than the one the terror attack occurred in (Utrecht) to decrease their support for right wing parties. The authors explain this by the nature of the conflict in Israel: left-leaning voters living in a different locality decreased their support for the right-bloc because they themselves prefer non-violent solutions to the larger conflict with Palestine, such as territorial concessions. Although this explanation is unlikely in our case (the Netherlands is not involved in such a deeply-rooted conflict), we do find that right-leaning voters *not* living in Utrecht were more likely to support the FvD, and right-leaning voters from Utrecht were *less* likely to support the FvD. Although we do not find such effects for left-leaning voters nor in the sample as a whole (not splitting it up based on locality), this finding is still in contrast with other studies which did not find that ideology shapes the response to a terrorist attack (Larsen *et al.*, 2020). We explain our result by the FvD's unique reaction to the terrorist attack: it was the only party to continue their campaign activities, which may have sparked stronger reactions among those less close to the 18/3 attack in Utrecht. With our data, however, we cannot test this assumption and thus encourage future studies to examine (local) campaign effects in response to a crisis such as a terrorist attack. In the sample as a whole (not splitting it up based on locality), we do not find a significant interaction effect with ideology.

In addition, our paper makes a methodological contribution by describing the use of big public opinion data of the sort made available by VAAs in a UESD. Due to VAAs proximity to elections, it is easier to meet the excludability assumption: because the treatment and control window can be placed merely hours from the event, other events are more easily ruled out. However, as we discussed, the ignorability assumption is harder to meet because of self-selection into doing the application. We discussed several solutions to this issue, predominantly by making sure treated and control respondents did the VAA at the same time of the day and by showing that the effects do not depend on the choice of treatment windows. Big public opinion data, such as VAA data, thus opens up interesting possibilities for causal inference.

Although our conclusion seems plausible, it is worth discussing alternative interpretations and explanations that we could not test with our data. First, whilst we control for changing positions of voters, we have no measures for issue salience. It may be that the attack changed which issues voters find important, consequentially changing their party preferences not because they look for the prominence of government parties, but because they weigh their positional priorities differently. Second, other attitudes may matter too, as terrorist attack could also affect voters' security concerns or their attitudes toward religious minorities which in turn may benefit certain (right wing) parties. Third, it is possible that terrorist attacks different in nature (i.e., other than domestic-Islamic terrorism) come with different effects, and that attacks larger in scale would have yielded stronger negative effects on people's attitudes toward immigration, and thereby increase support for PRRPs. Regardless, with our conservative test of our hypotheses we show that even a 'smaller' attack can influence voters' party preferences.

The main findings in this paper therefore have far-reaching implications for understanding how terrorist attacks – even one smaller in size – may shape election outcomes. Our results hint toward the idea that voting behavior works differently in a time of crisis as voters' policy positions matter less. To be able to fully substantiate this claim, future research should further explore the mechanisms between the importance of prominence over proximity in the aftermath of a crisis in more detail.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S175577392300019X.

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