## Prominence over Proximity? The Effects of Terrorist Attacks on Party Preferences for Incumbent versus Populist Radical Right Parties

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2021

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# The Effects of Terrorist Attacks on Party Preferences for Incumbent versus Populist Radical Right Parties

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March 11, 2022

**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 to gain due to a radicalization of voters' preferences. These competing theories are tested with a unique data set of 413.175 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 populist radical right parties become more radicalized. This means that during crises, voters rally around the flag and prefer prominence over proximity.

***Keywords*—-** Terrorist attacks, Rally-effect, Populist Radical Right Parties, Immigration, VAA data

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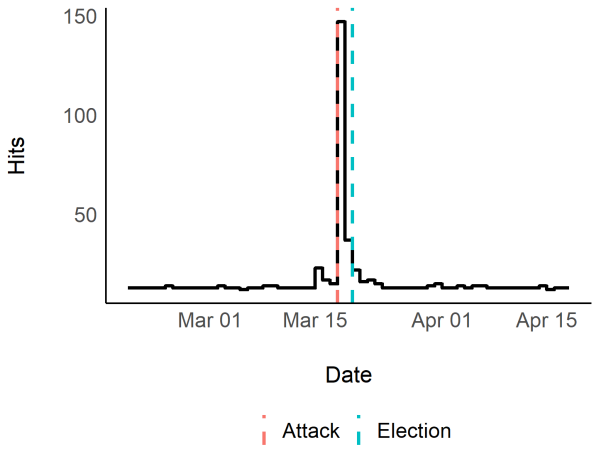
# 1 Introduction

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 (Muñoz, Falcó-Gimeno, & Hernández, 2020; Dinesen & Jæger, 2013; Larsen, Cutts, & Goodwin, 2020) and negative shifts on immigration attitudes and immigration policy preferences (Boomgaarden & de Vreese, 2007; Das, Bushman, Bezemer, Kerkhof, & Vermeulen, 2009; Ferrin, Mancosu, & Cappiali, 2019; Jakobsson & Blom, 2014; Legewie, 2013; Nägel & Lutter, 2020; Nussio, Bove, & Steele, 2019), to null-effects (Balcells & Torrats-Espinosa, 2018; Boydstun, Feezell, & Glazier, 2018; Castanho Silva, 2018; Finseraas, Jakobsson, & Kotsadam, 2011). 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 towards a rally-effect benefiting incumbent parties (Mueller, 1973), while more negative attitudes towards immigration may benefit populist radical right parties (PRRPs) (Van der Brug & Berkhout, 2015; Walgrave, Lefevere, & Nuytemans, 2009). 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 an open question.

One of the reasons for this is that existing studies relied on cases - such as on Spain (Balcells & Torrats-Espinosa, 2018) - which do not allow us to test whether a terrorist attack benefits the incumbent or PRRPs. 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. Moreover, while attitudinal effects may bare consequences deserving of scholarly attention, they may have most notable consequence around the time of an election. 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, Hagendoorn, & Hagendoorn, 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 trend data, shown in Figure 1, for 'Utrecht', 'attack', 'tram', 'shooting', and 'terrorist' clearly show that the attack was salience.

Figure 1: Salience of the event




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 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 (Bahara & Kranenberg, 2019). This assumption is in line with the idea that attacks that may create more negative attitudes towards immigration, aligning their policy preferences with those of PRRPs, leading to the success of such parties (Van der Brug & Berkhout, 2015; Walgrave et al., 2009; Van Kessel, 2011), 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 Voting Advice Application (VAA) data (n = 413.175) and employing an Unexpected Event during Survey Design (UESD) (Muñoz et al., 2020), the main results hint towards the rally-effect: the shooting increased the likelihood respondents would vote in favor of the main incumbent party (VVD) and one of their coalition partners (D66). This result is placed squarely in the middle of a relevant societal debate on the effects of this particular terrorist attack on the election outcome.

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 Voting Advice Applications 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

[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.

for an UESD design due to the high number of respondents, which allows for a tight bandwidth around the treatment event, ensuring that potential con-founders 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.

# 2 Theoretical Framework

## 2.1 The rally-effect

One of the first to introduce the 'rally-around-the-flag' phenomenon was John Mueller (1973). By focusing on the United States (US), 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 & Jæger, 2013).

Multiple studies have leveraged terrorist attacks to investigate a rally effect.[2] Most noteworthy, the 11/3 Madrid terrorist attack (Dinesen & 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 & 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 revealed the attacks were carried out by Al-Qaeda (Canel & Sanders, 2010; Canel, 2012). The incumbent party Partido Popular (PP) was, consequentially, accused of lying and no media outlets supported the government's response to the terrorist attack (Canel & 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

[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.

media's reaction to it?

While the electoral consequences of a terrorist attack thus remain unclear, there is some evidence pointing towards 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 towards 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 & 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:

> ***H1****: 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*

## 2.2 Populist Radical Right Parties

One of the explanations given for the successes and rise of populist radical right parties (PRRPs) is found on the supply-side, i.e. the parties from which the electorate can choose (Irwin & 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 & 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, Hobolt, & Hobolt, 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. As such, one could argue the success of PRRPs is dependent on the degree to which established parties represent sufficiently or are responsive to citizens' attitudes towards specific issues. In particular immigration, as immigration is an issue most commonly and extensively owned by PRRPs (Van der Brug & Berkhout, 2015).

In the case of a terrorist attack more specifically, a PRRP may have a strategic advantage due to its

[3] Our data set does not contain variables on trust or satisfaction with government

ownership of and its extreme political stance on immigration [4] (Van der Brug & Berkhout, 2015; Walgrave et al., 2009). For example, multiple studies found negative effects on attitudes towards immigration as a result of the Paris attacks in 2015 (Ferrin et al., 2019) and the murder of Theo van Gogh (Boomgaarden & de 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 of 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 towards 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[5], making established parties more vulnerable to PRRPs owning the issue (Van Kessel, 2011). To this end, a second, competing, hypothesis is formulated:

> ***H2**: A terrorist attack benefits Populist Radical Right Parties (PRRPs): after an attack the likelihood of voting for a PRRP increases.*

# 3 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 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 decentralisation. D66 is a progressive, social-liberal, pro European Union party. CDA and CU both are Christian-democratic parties, but the CU holds socially conservative

[4] 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 & Blom, 2014).

[5] Note that our data set does allow us to test this mechanism, as it contains a variable on immigration attitudes

positions on issues as same-sex marriage, abortion and euthanasia.

In the Netherlands there currently are two populist radical right parties, the newer Forum for Democracy (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[6]. 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[7], 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[8]. 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

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

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

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

looser ties and an exceptional high number of parties (Van Kessel, 2011). Lastly, the attack took place only two days before the elections, which is crucial for the design used in this paper, which will be discussed next.

# 4 Data & Design

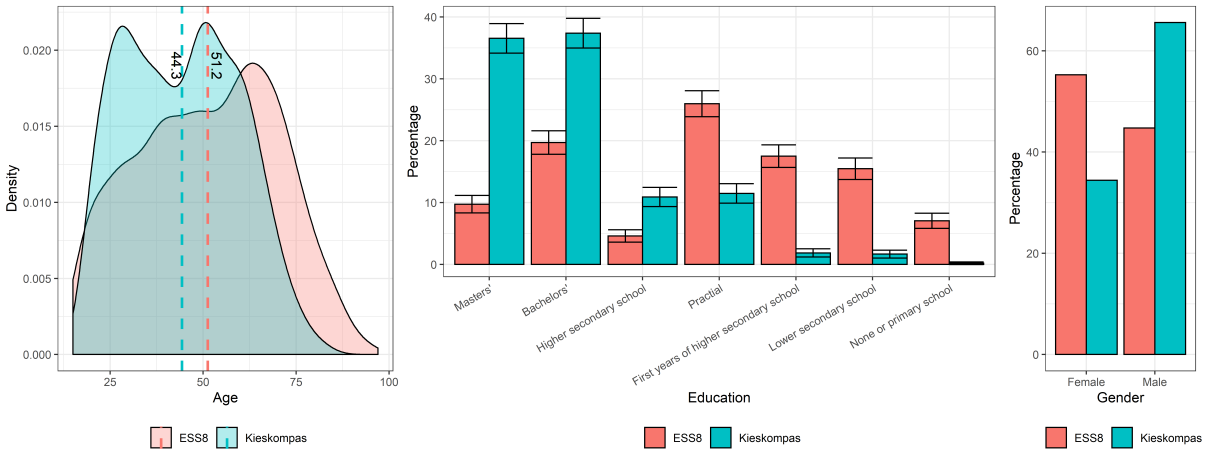
## 4.1 Data

The perfect test of our hypotheses is a world where it is randomized if people hear about the attack. Of course, such data does not exist, and therefore we will rely on causal inference using Voting Advice Application (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 parties (Garzia & Marschall, 2019; Krouwel, Vitiello, & Wall, 2012). 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 tool to get a voting advice before elections. Arguably they might thus be as influential as prime-time election debates, if not more.

We use VAA data from Kieskompas. 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 dimensions. 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. In total, the Kieskompas data contains a total of 413.175 respondents.

To provide some insight into the representativeness of the Kieskompas data, Figure 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. The VAA sample is quite similar to the ESS sample but slightly skewed towards younger cohorts, the higher educated, and has more men. This is as 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,

Figure 2: Sample comparisons of the VAA with the ESS8




Holleman, Kamoen, Krouwel, & De Vreese, 2014). As an opt-in sample, it is thus not representative of the general population but similar to it. 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 affiliation (Van de Pol et al., 2014). In addition, for the type of design we use in this study, a large numbers of respondents around the treatment helps exclude potential confounders by using a smaller bandwidth, whilst still allowing the detection of effects. All in all, it is a very useful data set for our purposes.

## 4.2 Design

### 4.2.1 Unexpected Event during Survey Design

We use an Unexpected Event During Survey Design (UESD). UESDs are an increasingly common identification strategy used to split a sample in to treatment and control groups. In an 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*: $D = 1|t_i > t_T$ and $D = 0|t_i < t_T$.

The UESD design relies on two identification assumptions: the *excludability assumption* and the *temporal ignorability assumption* (Muñoz et al., 2020). First, the excludability assumption states that any difference 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. Second, 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.

#### 4.2.2 UESD designs and VAA data

We argue that computational big data such as the kind VAAs produce is well suited for causal inference using UESD designs 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 USED design used in Finseraas, Jakobsson, and Kotsadam (2011) and Silva (2018) use treatment windows of four weeks on both sides of the 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 because respondents choose the the moment they do the survey themselves: different people do the survey at different moments. To be precise, respondent characteristics may correlate with: (i) the moment of the day people do the survey, and (ii) the number of days from an election they use the tool. For instance, tables 7 and 6 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). Generally speaking, 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 females. All in all, respondent covariates thus correlate with the time someone does the survey and therefore with the timing of the event, which is likely to violating the ignorability assumption.

We propose three solutions to these potential violations of the temporal ignorability assumption when using big data. 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 on 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 unbalance in 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.

#### 4.2.3 The design used in this paper

The best illustration of how VAA data in an USED design works is, of course, by explaining our own design. The main independent variable is a binary treatment status indicator. People before the attack are untreated and those after are treated. In our case, the event happened at 10:42 (Dutch winter time at GMT+1) on March the 18th, 2019. We use a nine-hour window after the event, thus ending at 20:00, in which all respondents are excluded from both treatment and control. We do so to ensure as many people as possible have heard about the event. At 20:00, after the 18:00 news, the whole working day, and dinner at home, it is very likely almost everyone in the country will know about the event and therefore there are fewer 'non-takers'. The nine-hour 'exclusion window' thus assures us that there is 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). To decide the optimal treatment and control windows, we run several co-variate 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 14hr 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.

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 by one hour in every model.[9] 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.

Table 1: Covariate balance tests

| | *Treatment windows:* | | | |
|---|---|---|---|---|
| | Same-day | 8hr-Window | 14hr-Window | 24hr-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) |
| Male | 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 |

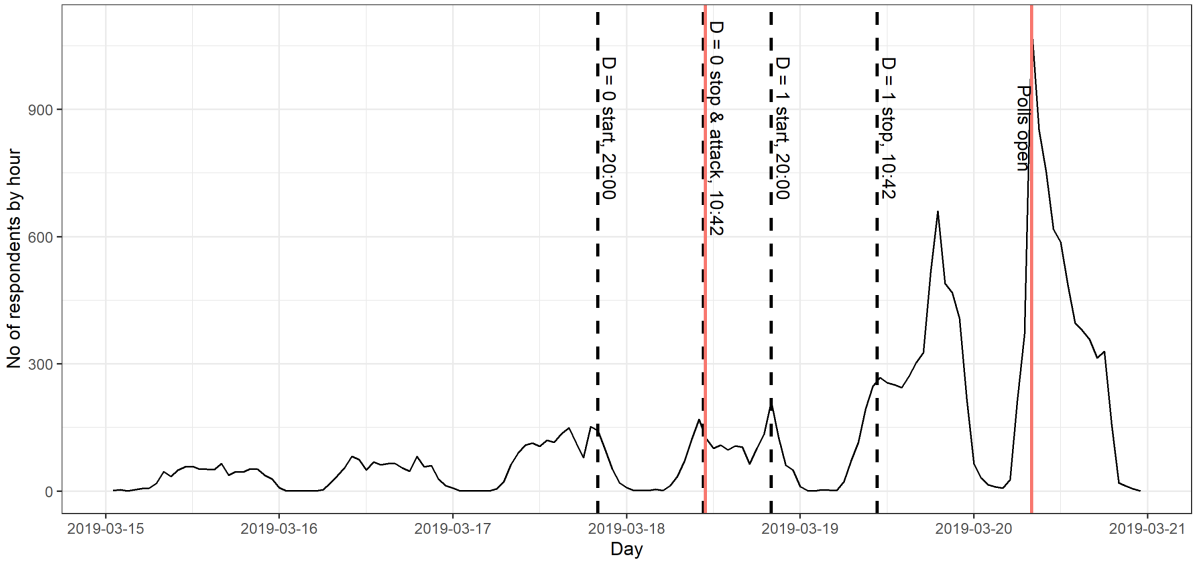
*p<0.05; **p<0.01

*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.

The main question in Kieskompas that we use as our dependent variable asks the likelihood that respondent will vote for a party. 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 Likert thermostats are a good indicator for actual voting behavior (Dalton & Klingemann, 2007, p. 82). To ensure balance between the treatment and control windows, we also control for a battery of other variables, specifically: age, education, province, 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).

[9] 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.

Figure 3: 14-hour treatment windows




# 5 Results

## 5.1 Main findings: party preferences

To test our hypotheses, we focus particularly on the coalition (H1) and populist radical right parties (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 on different parties:

$$Y_i = \alpha + \rho D_i + \mathbf{X}_i\gamma + \epsilon_i \tag{1}$$

the likelihood to vote on 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 $\epsilon$.

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 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 0-10 scale. The effect size for the VVD is about 19% of the standard deviation of the variable, and thus substantial. As such, these results indicate that the terrorist attack caused a strong rally-effect effect for the main incumbent party (VVD) and one of their coalition partners (D66), confirming H1 and rejecting H2.

Table 2: likelihood to vote – 14hr window

| | *Party:* | | | | | |
|---|---|---|---|---|---|---|
| | 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, amounts visited, and results from the positional questions in the form of a left-right and progressive-conservative score. The results are similar without including controls (presented in the Appendix).

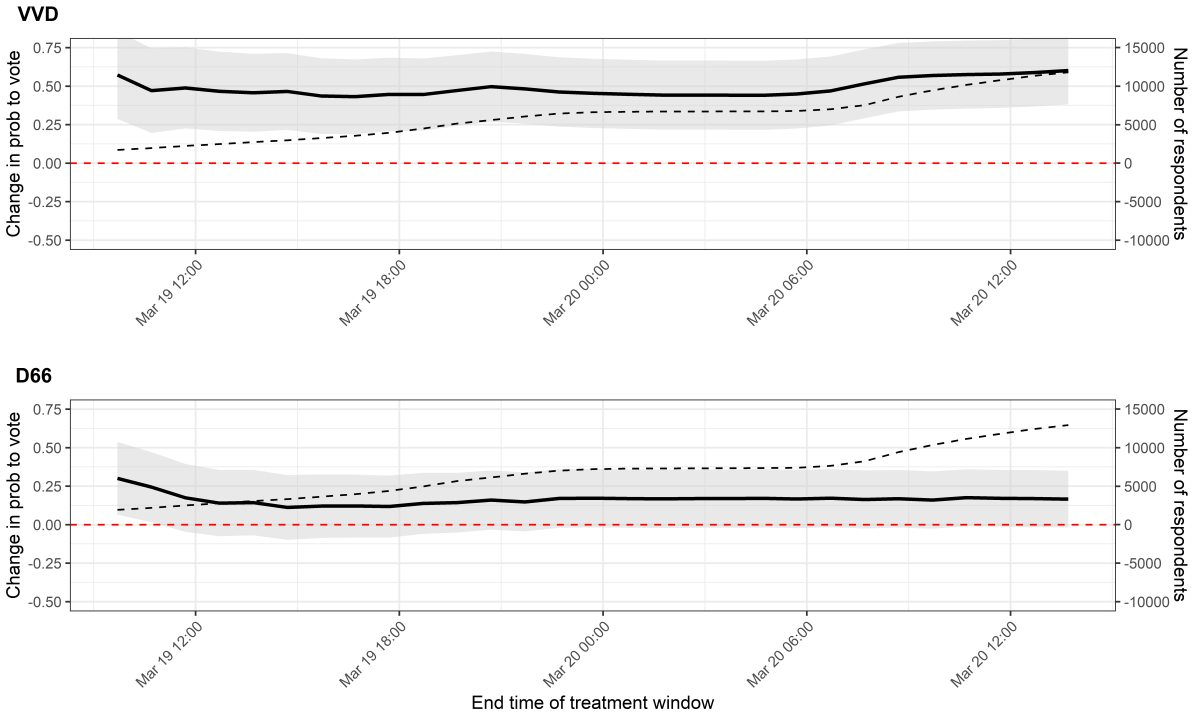
## 5.2 Robustness analyses

To show that the results are robust using different time windows and that the effects last up to election day, Figure 4 plots the results of the main model but with the treatment window increasing in size by an hour—the X-axis indicates the end time of the treatment window.[10] 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 plot 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.

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 online appendix table 10. 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 12, 13, and 11. 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 sample is not balanced, and the 24-hour window sees a strong result for the CU alongside a weaker effect for the VVD, which is likely caused by unbalance on 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.

[10] In the online appendix, we report these figures for all other parties.

Figure 4: Increasing size of treatment windows




## 5.3 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. In our case, there is one problem that might be unaccounted for: violations of the exclusion restriction through pre-existing time trends. This is of particular concern if there are more respondents who like the VVD in the sample closer to the election, or if respondents on average like the VVD more closer to the election. If such trends exist, they could explain the positive effect for the VVD that we find. Do note, however, that they would have to be very strong to explain our effect as the time windows that we use are extremely small compared to other UESD designs.

To check whether there is a more positive evaluation of the VVD closer to the election, we run a placebo 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 window. If there are pre-existing trends of the sort described above, then these tests should show an effect similar to our main results. Table 3 and 4 show the results of these placebo tests. As we find no significant effects, the placebo test excludes that time trends drive our results.

Table 3: Likelihood to vote – 14hr placebo window

| | *Party:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Placebo | −0.038 | −0.198 | 0.259 | −0.198 | 0.121 | −0.010 |
| | (0.131) | (0.165) | (0.171) | (0.165) | (0.153) | (0.156) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 1,181 | 1,127 | 1,359 | 1,127 | 1,408 | 1,296 |

*p<0.05; **p<0.01

Table 4: Likelihood to vote – 24hr placebo window

| | *Party:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Placebo | −0.009 | −0.034 | 0.029 | −0.034 | 0.080 | 0.003 |
| | (0.083) | (0.103) | (0.107) | (0.103) | (0.095) | (0.097) |
| 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, amounts visited, and results from the positional questions in the form of a left-right and progressive-conservative score.

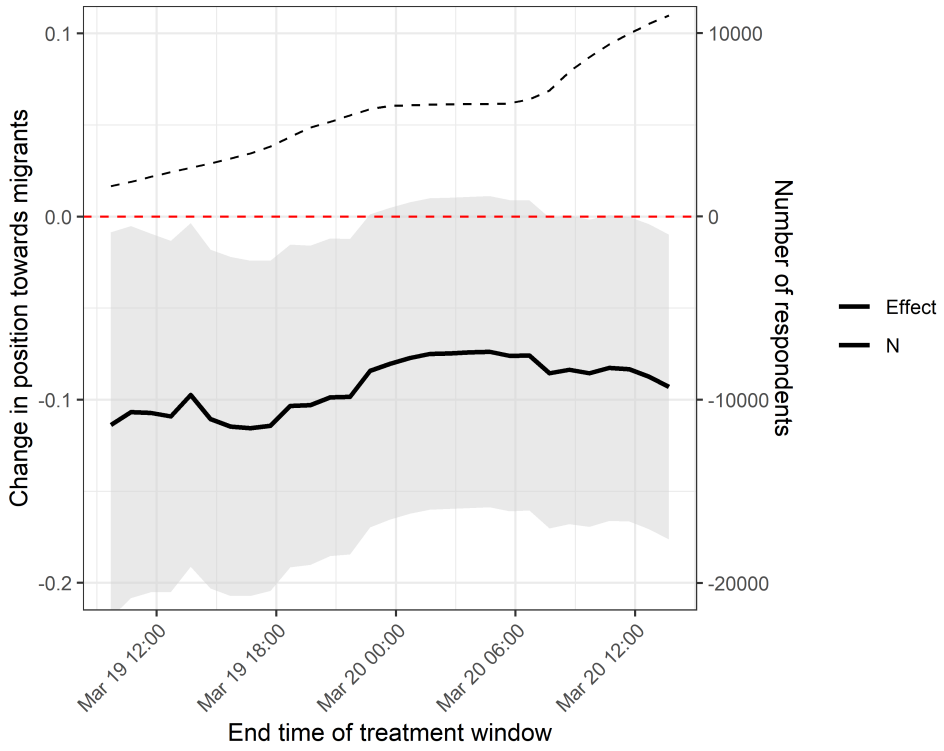
## 5.4 Testing the causal mechanism: immigration attitudes

Our results lead us to reject H2: the terrorist attack did not increase citizens' likelihood to vote for populist radical right parties (PVV & FvD). However, the assumption underlying H2 is that such an event fuels a negative attitudinal shift towards immigration, arguably motivating citizens to vote for populist radical right parties 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 the incumbents are also anti-immigration, which in the case of the VVD is true. 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 towards 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 spend on asylum seekers. In Figure 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 towards immigration than those before.[11]

[11] Note that the sample size is much bigger in this test because this question is earlier in the survey than the voting

Figure 5: Positions towards migrants




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, Keele, Tingley, & Yamamoto, 2011). A mediation model seeks to identify a third variable through which the effect of $X$ on $Y$ runs. The results, reported in Table 5, 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 these findings may seem ambiguous. Why did citizens favor the coalition parties more than populist radical right parties who are issue-owners of immigration and have a more negative position towards 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 crises, 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 seem 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.*

intention questions and less respondents have thus dropped out

Table 5: Mediation analysis – 14hr 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, amounts visited, and results from the positional questions in the form of a left-right and progressive-conservative score.

# 6 Conclusion & Discussion

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

By relying on VAA data in an UESD, evidence was found for the rally-effect: after an attack the likelihood of voting for incumbent parties increases (Hypothesis 1). No evidence was found for the notion that a terrorist attack benefits Populist Radical Right Parties (Hypothesis 2). Interestingly, the attack nonetheless negatively affected respondents' attitudes towards immigration—as hypothesis 2 would predict. That is, after the attack respondents were generally less favorable towards asylum seekers. Noteworthy, as the mediation analysis has shown, this negative shift towards 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 Populist Radical Right Parties. In addition, we find a significant effect for the D66 who are pro immigration. Therefore, our results lead us to the conclusion that during times of crises citizens' rally around the flag, and that attitudinal shifts are less important. The stronger 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, during uncertain episodes citizens' familiarity with coalition parties matters more than political attitudes. *In times of crisis voters rally around the flag, and they prefer prominence over proximity.*

In addition, our paper made a methodological contribution by describing the use of big public opinion data of the sort made available by VAAs in an UESD design. 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 on 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, there are two alternative interpretations worth discussing. 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, there might be certain campaign effects that we have not taken into consideration. For instance, the fact that the FvD was the only party purposefully continuing their campaign activities might have swayed its potential voters towards other parties. Although no significant effects were found for this interpretation (since we did not find a change in likelihood to vote for the FvD), one could argue that the increased likelihood to vote for the VVD is caused by appreciation from voters for stopping their campaign—as opposed to their prominence as coalition party. As the data used in this paper is not well-suited to test these alternative explanations, a follow-up study using an online-experiment could address the range of alternative interpretations above, as well as provide additional causal evidence for the mechanisms we propose.

The main findings in this paper have far-reaching implications for understanding how terrorist attacks may shape election outcomes. We show that voting behavior work differently in a time of crisis as voters' policy positions matter less. Since this is only an exploratory study, however, future research should explore the the mechanisms between the importance of prominence over proximity in the aftermath of a crisis in more detail.

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# 7 Online Appendix

## 7.1 Correlations between time and respondent characteristics

Table 6: Predicting hour of the day

| | *Dependent variable:* |
|---|---|
| | Hour of the day (0-24) |
| Age | 0.010** |
| | (0.002) |
| Education | −0.132** |
| | (0.024) |
| Male | 0.035 |
| | (0.059) |
| Constant | 13.609** |
| | (0.141) |
| Observations | 30,803 |
| Adjusted $R^2$ | 0.012 |

*p<0.05; **p<0.01

*note:* Linear model predicting the hour of the day (0-24). Not all dummies shown.

Table 7: Predicting days from the election

| | *Dependent variable:* |
|---|---|
| | Days from election |
| Age | 0.014** |
| | (0.001) |
| Education | −0.019 |
| | (0.011) |
| Male | −0.023 |
| | (0.026) |
| Constant | 0.487** |
| | (0.065) |
| Observations | 15,570 |
| Adjusted $R^2$ | 0.043 |

*p<0.05; **p<0.01

*note:* Linear model predicting days from the election (0-5). Not all dummies shown.

## 7.2 Further robustness and placebo analyses

In Table 8 we present the main model without control variables. As we would expect with a balanced sample, the estimates do not change substantially.

Table 8: likelihood to vote – 14hr window, without controls

| | *Party:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | 0.032 | 0.189 | −0.182 | 0.519** | 0.038 | 0.411** |
| | (0.108) | (0.134) | (0.122) | (0.136) | (0.118) | (0.120) |
| Controls | No | No | No | No | No | No |
| Observations | 2,119 | 2,121 | 2,399 | 2,020 | 2,475 | 2,285 |

*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. The model does not include the control variables

We show the results when increasing the size of the treatment windows for all different parties in Figure 6. The plot indicates that the overall results are consistent with what we have reported before.

A reader familiar with VAAs might wonder why we do not use a question on the exact party a respondent indicates she will vote for. Such questions are common in VAAs, including ours, and might serve as a robustness check. However, in our specific VAA this question comes *after* people see the voting advice, which means that the results are contaminated by the recommendations. Furthermore, since the question asks for voting on a specific party—as opposed to feelings about a party—it is much less sensitive to smaller changes in attitudes. That being said, in Table 9 and Figure 7 we show the results for these question. Overall, the main result holds; there is a positive effect for the VVD, albeit at a lower level of confidence. All in all, we believe these additional results also provide evidence for our argument because the signs are all in the expected direction. Generally, the results are less significant for four reasons, none which we believe are reasons for concern. First, the sample is smaller as the question is later in the VAA. Second, there is less variation because these are binary outcomes as opposed to 10-step Likert scales. Third, direct changes in vote behavior are a more extreme outcome than liking a party more, thus the effect is bound to be smaller (and therefore less significant). Fourth, these questions come after the voting advice and are thus influenced by the voting advice.

Figure 6: Increasing size of treatment windows: all parties

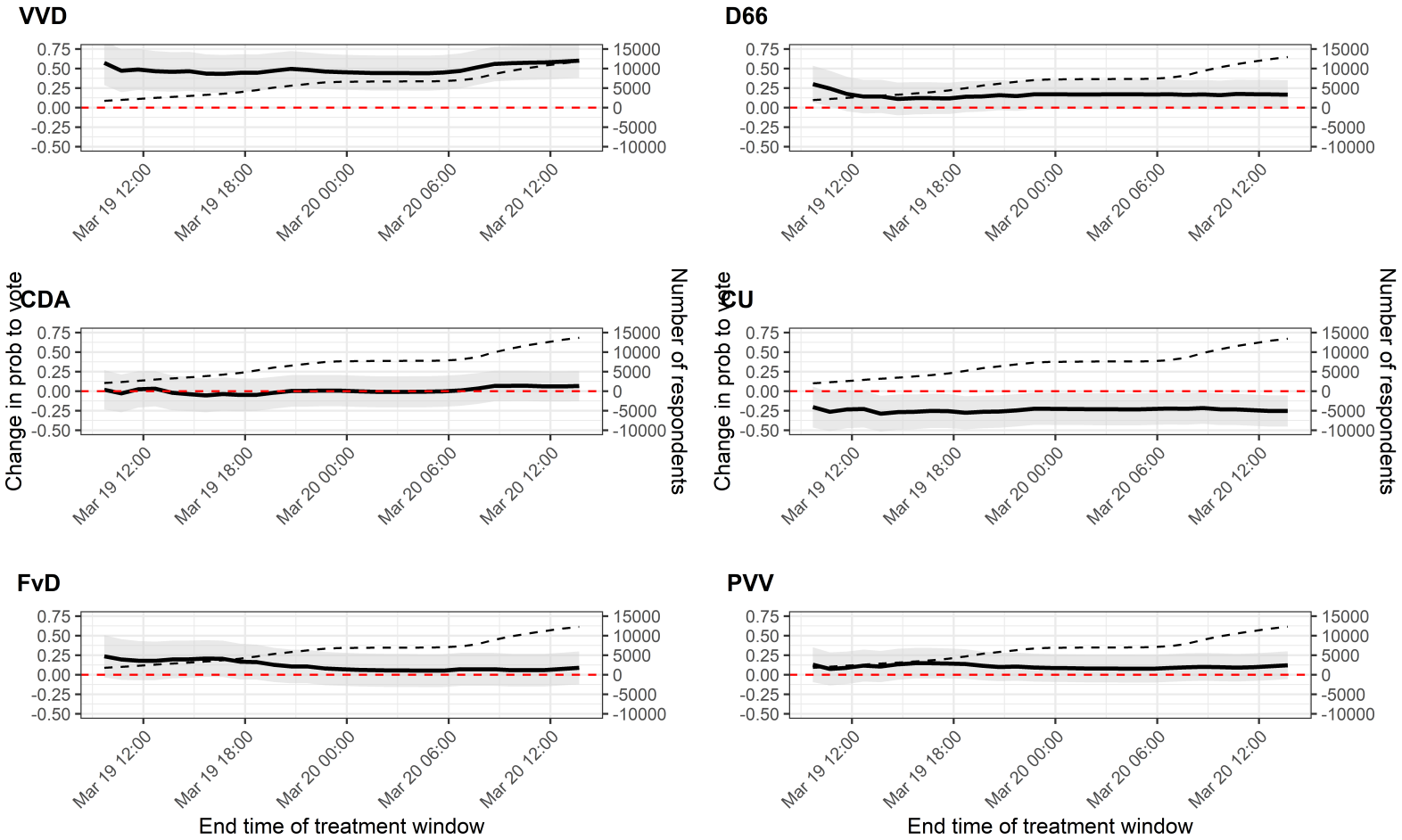

Table 9: Which party will you vote for – 14hr treatment window

| | *Party:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | 0.011 | −0.017 | −0.003 | 0.020 | −0.007 | −0.005 |
| | (0.007) | (0.012) | (0.010) | (0.012) | (0.011) | (0.013) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 1,682 | 1,682 | 1,682 | 1,682 | 1,682 | 1,682 |

*p<0.05; **p<0.01

*Note:* linear probability models predicting the party voters self-report they will vote for. The treatment treatment uses the 14-hour window. Controls include: provincial fixed effects, education, age, gender, amounts visited, and results from the positional questions in the form of a left-right and progressive-conservative score.

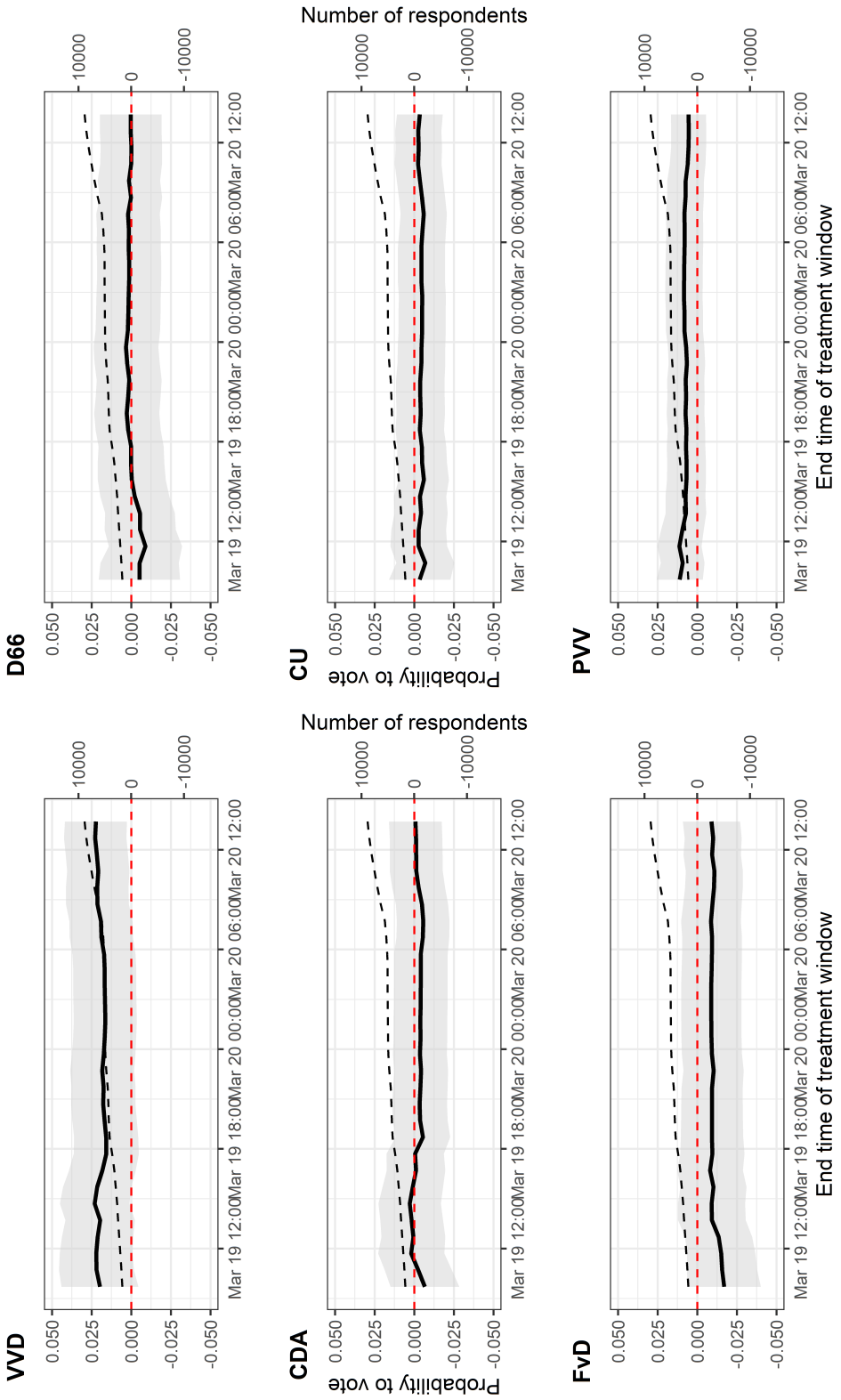


Figure 7: Increasing size of treatment windows: all parties, which party question

Table 10: likelihood to vote – 14hr window using matching

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | 0.175 | 0.340* | −0.244 | 0.716** | 0.089 | 0.219 |
| | (0.121) | (0.149) | (0.145) | (0.156) | (0.139) | (0.130) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 1,632 | 1,620 | 1,818 | 1,569 | 1,870 | 1,747 |

*Note:* *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 based on Nearest Neighbor matching using the Propensity Score. Respondents were matched using the R-package Matchit. The treatment uses the 14-hour window. Controls include: provincial fixed effects, education, age, gender, amounts visited, and results from the positional questions in the form of a left-right and progressive-conservative score.

Table 11: Probability to vote - Same-day Window

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | −0.169 | −0.098 | 0.191 | 0.178 | 0.105 | 0.521*** |
| | (0.170) | (0.196) | (0.198) | (0.213) | (0.192) | (0.174) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 857 | 856 | 962 | 820 | 1,001 | 927 |

*Note:* *p<0.05; **p<0.01

Table 12: Probability to vote - 8-Hour Window

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | 0.131 | 0.152 | −0.224 | 0.770*** | 0.076 | 0.401** |
| | (0.142) | (0.179) | (0.180) | (0.191) | (0.171) | (0.158) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 1,006 | 1,010 | 1,143 | 968 | 1,179 | 1,089 |

*Note:* *p<0.05; **p<0.01

Table 13: Probability to vote - 24-Hour Window

| | *Dependent variable:* | | | | | |
|---|---|---|---|---|---|---|
| | PVV | FVD | CU | VVD | CDA | D66 |
| Treatment | 0.116 | −0.083 | −0.271*** | 0.263*** | −0.0005 | 0.119 |
| | (0.072) | (0.087) | (0.082) | (0.090) | (0.080) | (0.077) |
| Controls | √ | √ | √ | √ | √ | √ |
| Observations | 5,706 | 5,731 | 6,338 | 5,480 | 6,520 | 6,073 |

*Note:* *p<0.05; **p<0.01