Survey Response and Measurement Bias in Dynamic Political Settings

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Authorship Contribution

Table 1: Authorship contribution

Contribution	**Paper 1** How Snap Election Calls Shape Vote Intention Uncertainty	**Paper 2** Survey Nonresponse After Elections: Investigating the Role of Winner-Loser Effects in Panel Attrition	**Paper 3** Assessing Sample Imbalances in Event-Study Designs: Observable and Unobservable Sources of Bias
Conceptualization	KM, AFR	KM	KM
Theory	KM, AFR	KM	KM
Data management	KM	KM	KM
Data analysis	KM	KM	KM
Validation	KM, AFR	KM	KM
Visualization	KM	KM	KM
Writing – original draft	KM, AFR	KM	KM
Writing – review & editing	KM, AFR	KM	KM

Note: KM = Klara Müller, AFR = Alejandro Fernández-Roldán

1 Introduction

Empirical political science heavily relies on survey data to learn about the public's political attitudes, preferences, and behaviors. For survey data to be a reliable mirror of public opinion, it is decisive that both the people participating in a survey and the responses they provide accurately reflect the opinions of the underlying population. The reality of survey research is, however, that a significant portion of any sampled population is unable or unwilling to cooperate in the survey process. We know that some people are more likely to participate in (political) surveys than others: those with, for instance, high levels of political interest, sophistication, and education tend to be more cooperative (Groves et al., 2004; Keeter et al., 2006; Mellon & Prosser, 2017) as well as more loyal in longitudinal, repeated surveys (Olson & Witt, 2011). In addition to differential survey cooperativeness *across* people, it is also known that the willingness to respond in surveys may fluctuate *within* a person over time. Consider, for instance, phenomena like panel attrition, where once cooperative respondents turn to refuse participation at a later point; compliance at one moment does not imply steadfast participation and, conversely, refusing to respond at one point in time does not exclude future participation.

Knowing that it is dynamic *whether* and *how* people respond in surveys, the question arises what drives such shifts in cooperativeness and response behavior. Existing research shows that changes in an individual's narrow, *personal* context, such as in the socioeconomic status or sociopsychological setting, can affect survey participation (Lugtig, 2014; Trappmann et al., 2015). However, comparable effects of changes in the broader, *political* context are far less researched. At the same time, political behavior research demonstrates that this broader political context is decisive, as contextual change can induce shifts in people's political preferences and behavior (MacKuen & Brown, 1987; Valentino et al., 2009; Vráblíková & Císar, 2015).

In this thesis, I bridge these two strands of research. If we conceptualize survey participation not merely as a neutral administrative act, but as being functionally comparable to a political act, it follows that it should be sensitive to the same external stimuli that drive political behavior. Just as events like, e.g., elections or crises, reshape political behavior like, e.g., voting and other forms of political participation, they plausibly may also affect if and how people respond in surveys. Investigating this intersection, the overarching research question I address in this thesis is:

RQ 1: *How do political events and contextual changes affect survey participation and the way people express their political preferences in surveys?*

Contextual stimuli that influence survey participation can generate systematic differences in respondents' propensity to participate. If specific groups become more or less responsive, or systematically change how they answer particular survey items, post-event survey samples may overrepresent or underrepresent these groups' political stances. If left unaddressed, such patterns can distort measurements of political preferences and behavior and lead to inaccurate conclusions about public opinion (Cavari & Freedman, 2023; Sciarini & Goldberg, 2016).

At the same time, it is important to note that not every change in survey responsiveness or sample composition is consequential for survey-based measurements (Bailey, 2024; Groves, 2006; Jennings & Wlezien, 2018; Keeter, 2018). Measurement bias arises only when the decision to participate or the way a person responds is systematically related to the outcome that is being measured. Conversely, if event-induced response behavior is random and unrelated to the outcome variable of interest, the inferences drawn from that survey data should remain unbiased. This distinction is critical because when an event causes compositional disproportionalities (either in *who* responds or in *how* people respond), we risk observing a shift in who is responding rather than a true shift in political opinion. To fully understand the analytical and political consequences of event-induced shifts in survey response, it is therefore decisive to move from merely detecting such shifts toward assessing whether and how they produce measurement bias. This leads to the second overarching research question addressed in this thesis:

RQ 2: *When and how do event-induced disproportionalities in survey samples bias the measurement of political phenomena?*

This thesis addresses the two core research questions by three papers. Paper 1 analyzes how people shift their level of vote intention uncertainty and the survey-based expression thereof in response to (unexpected) early election announcements. Paper 2 focuses on how winner-loser dynamics affect nonresponse and panel attrition in post-election surveys. Paper 3 develops a methodological framework to detect compositional bias in quasi-experimental event-study designs and to disentangle events' causal effects from such compositional bias. While each paper stands on its own and makes contributions that partially go beyond the scope of the two overarching research questions, they jointly provide a comprehensive picture of the nexus of political context, survey response behavior and resulting measurement issues. In doing so, it yields implications that advance knowledge both for research and political practice:

From an analytical perspective, obtaining valid measurements is necessary for making sound survey-based inferences about the political world. If contextual stimuli systematically compromise how accurately survey data reflect the underlying population's stances, our ability to describe public opinion, identify true preference shifts, or isolate causal effects is constrained. By investigating which political events interfere with survey behavior in what way, this thesis provides valuable methodological insights. The tools it develops to detect and account for event-induced response patterns enable effective strategies for identifying and correcting resulting biases wherever possible. This helps to increase the reliability, validity and robustness of inferences drawn from event-driven and survey-based research.

Beyond the methodological domain, the findings of this thesis have important political and democratic implications. Policy-making and public discourse often rely on survey data to inform decisions (Cavari & Freedman, 2023), and survey and polling results themselves have political quality by becoming part of the political discourse (consider, e.g., "horse-race" journalism during election campaigns). As a result, uncorrected misrepresentation of specific groups and preferences in survey data can reinforce their marginalization and, for instance, translate into systematic underrepresentation in policy outcomes. Additionally, survey results can themselves shape political behavior. In electoral contexts, systematic distortions in pre-election polling data may directly affect the distribution of political power by influencing voting decisions (Brugarolas & Miller, 2021; Schmitt-Beck, 1996). For example, citizens who see low support for their preference may suppress their turnout (Fernández-Roldán & Barnfield, 2024; Lang & Lang, 1984), while perceived high support may encourage (electoral) participation (Valentim, 2025). Identifying and correcting event-induced sample imbalances and measurement biases is, thus, not only a methodological concern. It is decisive for ensuring that survey-based evidence accurately reflects public opinion without creating or reinforcing political cleavages in decision-making and public discourse.

In the following, I provide an overview of the existing survey methodology and political behavior research relevant to this thesis. Bridging these two strands of literature, I lay out the conceptual and theoretical notions on how political context affects survey response and how this links to measurement biases. Then, I introduce three perspectives from which I approach the overarching research questions and detail how each of the three papers fits into this framework. After outlining each paper, I summarize my key findings and make suggestions how future research can build upon this work and address its limitations.

1.1 What Is Known

1.1.1 Why and How People Respond in Surveys

To understand how political events can affect survey response behavior, it is decisive to know what determines *whether* and *how* people respond to surveys. Because both dimensions shape which opinions become observable in survey data and are therefore important for assessing potential event-induced measurement biases, this thesis focuses on both: the overall decision to participate in a survey, that is, *unit response*, as well as the way respondents answer specific questions once they do participate, that is, *item response*. This section provides an overview of extant literature on determinants and underlying theories of survey participation and specific responses.

Unit Response Behavior

Survey participation is not a spontaneous nor random, single choice, but is better conceptualized as a sequence of decisions made by the individual at several critical stages of the response process (Albaum & Smith, 2012; Green et al., 2004). A respondent must first be contacted, then successfully receive and process the survey request, and finally decide to participate. At best, this results in completing the survey. Survey participation is, thus, the outcome of passing through this multi-stage process; it depends on the interaction between the situation of the survey quest, the survey design and the respondent's individual predispositions (Albaum & Smith, 2012). Within this process, the literature distinguishes between three primary causes of nonresponse: nonresponse due to noncontact, nonresponse due to inability to participate (e.g., language barriers), and nonresponse due to refusal to cooperate (Tourangeau, 2004). This thesis predominantly focuses on the latter mechanism of refusal, as I expect external events to affect respondents' motivation to accept or decline a survey request.

An established framework to categorize the determinants of survey participation through this multi-stage process distinguishes three dimensions (Albaum & Smith, 2012; Groves et al., 1992; Keusch, 2015). These are (1) external factors, (2) attributes of the survey design, and (3) characteristics of the sampled person. This classification comprehensively applies to different survey modes (Albaum & Smith, 2012).[1] It provides the structural basis to identify where and how political events may intervene in the survey response process.

The *external determinants* of survey participation comprise the broader social and political environment in which a survey request takes place. They set the baseline probability of cooperation and are usually outside the control of the researcher. Key factors include attitudes toward the survey industry and the perceived level of trust in survey processes (Fan & Yan, 2010; Groves et al., 1992). Furthermore, societal factors include the prevailing political climate and the extent of "survey fatigue" within the population, which is the cumulative effect of increasing survey requests over time (Schleifer, 1986). These external factors constitute the societal-level context within which individual decisions of survey participation are made.

The *survey-related determinants* of survey response are the specific features of a survey's design that are under a researcher's direct control. These can be intentionally structured to push a sampled person's decision-making further toward participation. Key survey-related factors include the chosen mode of contact (e.g., telephone, mail, web) (Albaum & Smith, 2012; Ansolabehere & Schaffner, 2014; Schmidt & Hollensen, 2006), the structure and timing of incentives (pre-paid, post-paid, no monetary incentives at all) (Church, 1993; Mercer et al., 2015; Oscarsson & Arkhede, 2020; Singer & Ye, 2013), the reputation and trustworthiness of the sponsor sending the survey request (Fan & Yan, 2010; Fox et al., 1988), and the interest in and salience of the survey topic (Dillman, 2011; Groves et al., 1992). In the context of longitudinal studies, such design-related factors also affect panel attrition (i.e., the loss of participants over time) and cumulative survey burden (i.e., the time and (cognitive) effort required to respond to the survey) across multiple waves (Lugtig, 2014).

The third dimension focuses on the *individual-level factors* and person-specific traits that determine survey response. Beyond fixed attributes such as sociodemographic char-

[1] My empirical studies predominantly rely on self-administered web surveys and face-to-face interview data (more details on the respective data sources are provided in Section 1.4). While the specific stages of the survey response process differ across modes, the determinants and mechanisms presented in this section are found to hold for all modes utilized in this thesis (Albaum & Smith, 2012). At the same time, I see potential for future research to assess mode-specific effects of (political) events on the survey response process, which I discuss further in Section 1.5.2.

acteristics (e.g., age, education, gender) that are known to correlate with participation (Groves et al., 1992), existing research highlights the role of psychological predispositions. For instance, people with higher levels of conscientiousness, agreeableness and openness to experience tend to be more responsive (Fan & Yan, 2010; Marcus & Schütz, 2005; Rogelberg et al., 2003). More dynamic psychological states like emotions (Ashley & Shaughnessy, 2023), personal topic interest, personal attitudes toward surveys and one's sense of civic duty (Keusch, 2015) are also found to affect response-decisions. In the political context, specifically, political interest and satisfaction with the democratic system are strong, positive predictors of survey participation (Müller, 2025; Silber et al., 2022). This body of work frames survey participation as functionally equivalent to a form of political engagement. I will elaborate on this political dimension of survey response in greater detail in Section 1.1.2.

While the threefold classification identifies important drivers of participation, it does not fully explain the individual-level mechanisms underlying the decision to respond in a survey. Among existing theories of response decisions (see, e.g., Albaum & Smith, 2012; Fan & Yan, 2010), three are most central to understanding how (political) events shape survey response behavior: *social exchange* (Dillman, 2011; Thibaut & Kelly, 1959), *leverage-salience* (Groves et al., 2000, 2004), and *cognitive dissonance* theory (Festinger, 1957; Furse & Stewart, 1984).

The core feature of *social exchange theory* is the notion that any social (inter)action entails costs and benefits (Thibaut & Kelly, 1959). Against this backdrop, the survey response decision is a rational calculation of weighting the costs of participating (e.g., time, cognitive burden, emotional effort, conflicting interests) against the expected rewards of doing so (e.g., fulfilling a sense of civic duty, complying with perceived authority of the survey institution, altruism, survey incentives) (Dillman et al., 1978; Dillman, 2011; Groves et al., 1992). People are cooperative whenever their expected rewards of survey participation outweigh the anticipated costs (Singer, 2011).

This theory is helpful for explaining potential event-effects on survey response, because political events may alter this cost-benefit calculus for certain groups of the population. For instance, an external (political) stimulus can suddenly and significantly increase the costs of survey participation by inducing stress or making the topic emotionally painful. Conversely, it may boost the benefits of participating by giving respondents an opportunity to voice their preferences and opinions.

According to *leverage-salience theory* (Groves et al., 2000, 2004), the interplay of a survey's attributes and an individual's perceived importance of these features is decisive in participation decisions. Individuals tend to assign different importance (i.e., leverage) to a survey's attributes (e.g., its topic, the sponsor, incentives) and to the concept of survey participation more generally (Groves et al., 1999). This leverage can be either negative or positive. Whether this leverage influences the final decision depends on how salient that attribute is made during the request. It is very subjective which attributes of a survey quest are perceived as important; this theory thus helps in explaining different baseline levels of survey cooperativeness across the population.

In the context of event-specific survey participation, leverage-salience theory helps explain why, first, survey cooperativeness can shift after an incisive event and, second, why the strength and direction of theses event-induced shifts may differ between subgroups of the population. For instance, a sudden event can significantly increase the salience of "politics". Such political salience may boost participation for those who find politics engaging (and, thus, have a positive leverage) while simultaneously triggering nonresponse among those who are politically disengaged or even cynical (and, thus, have negative leverage).

Finally, *cognitive dissonance theory* (Festinger, 1957) states that people are fundamentally motivated to maintain or create internal consistency and avoid the discomfort that arises when actions conflict with their beliefs and attitudes. In the context of surveys, participation decisions can be understood as an act to reduce dissonance that is associated with nonresponse (Furse & Stewart, 1984; Hackler & Bourgette, 1973). For instance, refusing survey participation may conflict with the perception of being helpful and fulfilling a civic duty. To avoid potential discomfort arising from this conflict, people are incentivized to participate in a survey when queried (Furse & Stewart, 1984). Or, conversely, a decision to refuse survey response may reduce discomfort that is expected to come with survey participation.

In the context of event-specific survey response behavior, cognitive dissonance theory helps to explain differential reactions to incisive events in survey participation. For instance, if people hold a strong sense of commitment and have established an image of being a loyal respondent through past survey participation, they may be less prone to event-induced nonresponse than those for which nonresponse would create less or no such discomfort.

Item Response Behavior

The determinants and theories of survey response presented above predominantly focus on unit response, i.e. the overall participation decision. While the literature traditionally differentiates between the causes and consequences of unit response behavior and item response behavior, i.e. the way participants respond to specific survey questions once they do participate (Groves & Couper, 2012), many studies demonstrate an empirical and conceptual link between both (Burton et al., 1999; Loosveldt & Billiet, 2002; Mason et al., 2002; Yan & Curtin, 2010). Following Yan and Curtin (2010), both unit and item response can be understood as being part of the same "response continuum", in which similar external, survey-related and individual-level factors shape the decision to participate as well as the way how respondents engage with specific survey questions (compare, e.g., Albaum & Smith, 2012; Barth & Schmitz, 2018). In political contexts, factors such as political interest, partisanship, trust in political institutions, and the salience of political issues affect both the likelihood of participation and the quality of responses (Barth & Schmitz, 2018; Voogt & Saris, 2007). Depending on how these factors interact, respondents may entirely refuse to participate or remain in the survey but show certain item-specific behavior, such as item nonresponse, satisficing, reduced attentiveness, or socially desirable responding on sensitive items (DeMaio, 1984; Krosnick et al., 2002; Krumpal, 2013; Roberts et al., 2019; Wilcox & Wlezien, 1993).

Accordingly, unit and item response behavior in political surveys should be understood as related outcomes of shared underlying mechanisms rather than as independent processes. This means that when political events alter any of the external, survey-related, or, most importantly, individual-level psychological conditions, they can jointly affect *if* and *how* people respond in surveys.

1.1.2 Survey Response as a Political Act

At this point, it is important to detail how survey participation can be understood as a form of political participation, albeit perhaps a softer one than more "classical" forms of political participation, such as voting, protesting or contacting elected politicians (Conge, 1988). Prior research shows positive links between political participation and survey responsiveness (Bailey, 2024; Brehm, 1993; Tourangeau et al., 2010), and the motivations underlying survey response often overlap with those that drive more conventional acts of political participation (Voogt & Saris, 2007). For instance, politically interested people show higher survey responsiveness and are also more likely to participate politically than the less politically involved (Bailey, 2024; Brehm, 1993; Tourangeau et al., 2010). This suggests that survey participation carries political quality.

Surveys provide an indirect channel through which individuals can express their preferences, evaluations, and stances toward the political system. In this sense, survey participation can also reflect deeper political attitudes, including satisfaction with government performance or trust in institutions, such that both response and nonresponse may signal political engagement or disaffection (Harris-Kojetin & Tucker, 1999; Silber et al., 2022; Verba, 1996). Because the information generated by surveys feeds into scholarly analyses and policy debates (Cavari & Freedman, 2023), responding to surveys constitutes a way for citizens to voice their views and influence public discourse beyond more conventional moments of participation, such as, e.g., elections. This further supports that responding to surveys can likewise be understood as a meaningful form of political participation.

Because survey participation and political participation share motivational roots and are empirically associated (Peress, 2010), it follows that a person's survey responsiveness may correlate with a range of political outcomes. This connection is central to the argument of this thesis: if survey response carries political meaning, survey responsiveness should be sensitive to the same (political) events that influence other forms of political behavior. Besides individual-level determinants of political participation, such as, e.g., sociodemographics, political interest and efficacy or the sense of civic duty (Armingeon, 2007; Dalton, 2008; Verba et al., 1995), the political behavior literature commonly finds contextual influences to affect political participation. In particular, incisive events that trigger affective reactions and boost the salience of political issues or politics in general have been shown to increase political participation (Bateson, 2012; Groenendyk & Banks, 2014; Valentino et al., 2009; Vasilopoulos et al., 2018).

Against this backdrop, it stands to reason that such contextual stimuli can also affect survey response behavior. The notion of survey response as a political act is a theoretical precondition for assessing what (yet) remains insufficiently understood and is addressed by this thesis: how the political context shapes survey response, and under which conditions such effects become consequential for the accuracy of survey-based measurement and inference.

1.2 The Unknowns: How Political Context Affects Survey Response and Measurement Bias

The notion of survey participation as a political act links the two strands of literature this thesis builds upon: survey methodological findings on contextual determinants of survey response and participation, and political behavior research on how contextual events affect individual-level political behavior. In this section, I theorize how political events can alter individuals' willingness to participate in surveys and the way they respond to survey items, and under what conditions such changes translate into measurement bias. To this end, I first provide some intuition on the circumstances under which (non)response patterns introduce measurement biases and when they do not.

1.2.1 Ignorable Versus Nonignorable Sample and Response Disproportionalities

There is consensus in the literature that nonresponse potentially threatens the validity of survey-based inferences, because the people who choose to participate may differ from those who do not (Groves, 2006; Lin & Schaeffer, 1995; Marcus & Schütz, 2005). This challenge has become even more decisive in recent decades, as survey participation rates continue to decline (Berinsky, 2008, 2017; Cavari & Freedman, 2018, 2023; Curtin et al., 2005). At the same time, it is important to emphasize that not all nonresponse automatically produces consequential biases in the data (Groves, 2006; Jennings & Wlezien, 2018; Keeter, 2018). Thus, the crucial question is when nonresponse yields biases and under which circumstances it is analytically inconsequential. Here, survey methodology distinguishes between two patterns of missing data: *ignorable* and *non-ignorable* nonresponse (Bailey, 2024).[2]

Ignorable nonresponse occurs when the decision to respond is unrelated to the outcome variable that is being measured. In such cases, nonresponse should be inconsequential, at least regarding the distribution of the outcome variable that the sample should reflect (Bailey, 2024). The left panel of Figure 1.1 visualizes such ignorable nonresponse, closely based on Bailey (2024)'s formalization of these two types of nonresponse. Consider that each person has an underlying baseline willingness to respond in a survey. Let us call this latent responsiveness R^*, which is an unobserved continuous measure (depicted on the x-axis). The y-axis represents the outcome variable of interest (Y). Let us assume that R^* ranges from -1 (low responsiveness) to 1 (high responsiveness) and

[2] When I subsequently refer to *(non)response*, I subsume both unit and item nonresponse.

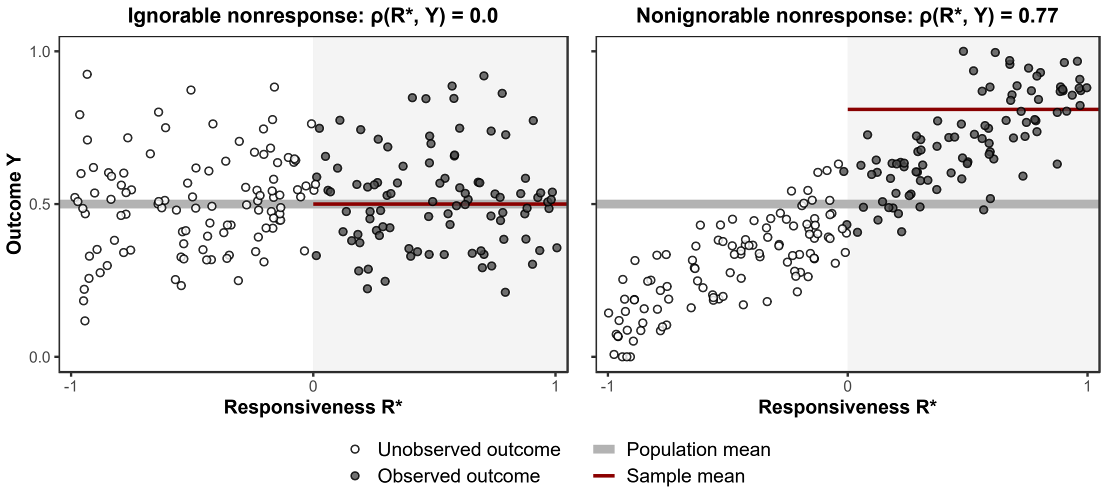

Figure 1.1: Ignorable and nonignorable nonresponse. The grey-shaded area represents the part of the population that would be observed in the sample.

that individuals participate whenever $R^* > 0$. This means that all outcomes located on the right half of the distribution of R^* are observed outcomes, while the less responsive individuals on the left half of this distribution are not observed in the sample. In this left panel of Figure 1.1, the correlation between the responsiveness R^* and the outcome Y is zero, that is, the levels of Y do not systematically vary across different levels of R^*. Under statistical independence between the two variables, the mean of Y (red line) among the actual respondents matches the population mean (grey line). Under such circumstances, surveys continue to yield reliable population estimates even with substantial levels of missing responses, because those people who do respond still constitute an adequately representative cross-section of the population under study. This type of nonresponse can be considered *ignorable* (Bailey, 2024)

When the decision to respond in a survey is correlated with the outcome variable of interest, however, nonresponse becomes *nonignorable* (Bailey, 2024; Cavari & Freedman, 2023; Lin & Schaeffer, 1995). This case is illustrated on the right hand panel of Figure 1.1. Here, R^* and Y are correlated, which produces a discrepancy between the sample and population means: Individuals with higher values of Y (in case of a positive correlation of R^* with Y, as depicted in the plot) are more likely to cross the participation threshold and respond (Bailey, 2024). Conversely, if the correlation was negative, the sample mean would underestimate the true population mean. In both cases, nonresponse yields a biased estimate of the quantity of interest.

This latent threshold framework provides an intuition on how the extent to which R^* is correlated with the outcome variable Y is the decisive distinction between ignorable and nonignorable nonresponse. Departing from this conceptualization, I subsequently detail different pathways by which I argue that (political) events can, first, alter the observed level of an outcome variable and, second, potentially introduce bias into the measurement of this very variable.

1.2.2 True Preference Shifts or Event-Induced Measurement Bias? Different Pathways of Event-Effects

If the same forces that influence political behavior and participation also shaped survey participation, the challenge arises which is at the core of this thesis: when we observe change in an outcome variable Y after a certain external event, this very change can arise from two different underlying processes. First, observed change in Y may reflect a genuine shift in underlying opinions that is adequately reflected by Y. In this case, the survey continues to capture the same individuals in the same way before and after the event (mechanism A). Second, observed changes in Y could also reflect event-induced shifts in survey response behavior. This involves changes in *who* participates in the survey or *how* respondents answer specific items (mechanism B). Below, I distinguish these two mechanisms and detail different ways in which both can play out to produce observed post-event changes in the outcome variable Y.[3]

Mechanism A: True Preference Shifts With Stable Response and Participation Behavior

The first mechanism involves a true shift in the preferences, evaluations, or attitudes that are measured by Y while the survey response process remains entirely stable. In this situation, the latent response propensity R^* maintains the same distribution and the same relationship to the outcome variable Y before and after the event. Individuals are neither more nor less likely to participate in the survey than they were (or would have been) prior to the event, and the way respondents translate their true opinions into survey answers does not change. Because both responsiveness and item response behavior

[3] Note here that the distinction between these mechanisms and their pathways is an analytical simplification. It should help to clarify how political events can affect observed outcomes, but real cases rarely follow only one pathway. In practice, several of these processes can operate at the same time and to different degrees. That is, events may simultaneously shift true opinions, change *who* participates in the survey, and alter *how* people respond. These combinations can produce varying levels of true change in Y and different amounts of bias in its measurement. Therefore, the presented mechanisms serve as a guide for identifying possible sources of observed change, not as a set of mutually exclusive patterns.

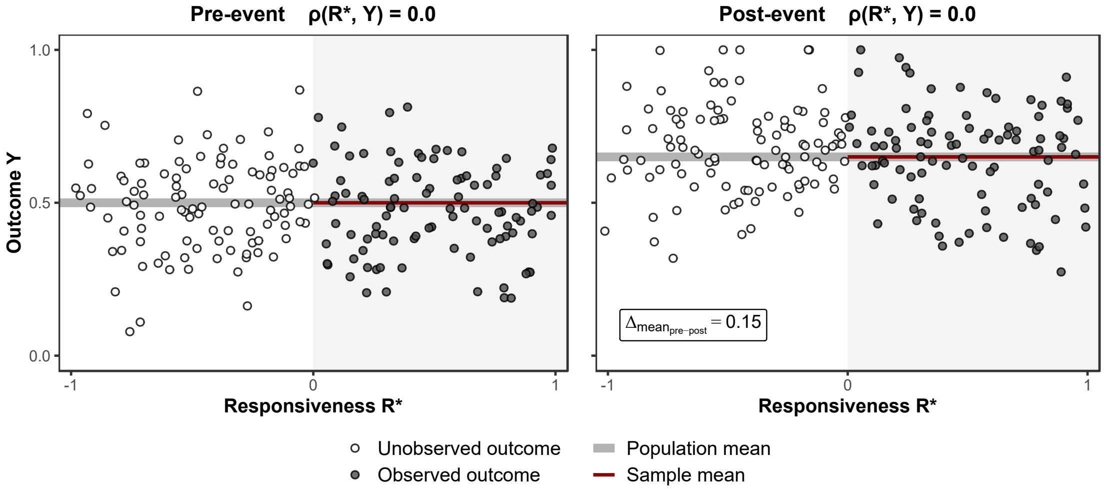

Figure 1.2: *Mechanism A* of event-induced shifts in the outcome variable. The grey-shaded area represents the part of the population that would be observed in the sample.

remain stable, the composition of the responding sample is unaffected by the event and the survey continues to capture the same cross-section of the population. When these conditions hold, any change in the distribution of Y can be attributed entirely to shifts in the underlying distribution of the measured opinion. Under this mechanism, the survey performs as intended, that is, it reliably mirrors substantive opinion shifts without interference from changes in *who* responds or *how* responses are expressed.

Figure 1.2 conceptualizes such real opinion shifts. In the left panel (pre-event), survey responsiveness is not correlated with the outcome variable; sampled respondents are equally likely to respond in the survey, irrespective of their level of Y. In the right panel (post-event), responsiveness remains uncorrelated with the outcome variable. However, both the population and sample mean of Y have increased (here, exemplary by 0.15 units). As R^* is equal for all levels of Y, the increase in Y reflects a true opinion shift.[4]

Mechanism B: Changes in Survey Response or Participation Behavior

While under mechanism A, observed changes in the outcome variable Y exclusively reflected true opinion shifts, change in Y under mechanism B reflects shifts in the survey response process. There are two branches of how an event can induce such shifts: first,

[4]Such true opinion shifts may also occur under a different degree of correlation between R^* and Y than illustrated in Figure 1.2; it is not necessary that this correlation is zero or that the sample mean perfectly reflects the population mean. The decisive factor for an event to not introduce *new* bias is that the correlation between responsiveness and the outcome does not change, i.e. that the difference between the population and sample mean remains equal before and after the event.

by altering *how* respondents express their true opinion while the sample composition remains essentially the same, or, second, by changing *who* ends up in the sample because individuals' overall willingness to participate is affected by the event. In both cases, the observed distribution of Y can shift even when the distribution of its underlying opinion remains unchanged.

The first branch of mechanism B involves changes in item response behavior under stable sample composition. Here, individuals are as likely to participate after the event as they were or would have been in absence of the event. However, their way of translating their opinions into survey answers changes. In principle, the shifted level of the outcome variable Y as illustrated in Figure 1.2 could also be produced by uniform event-effects on item response behavior where the entire sample overreports their true level of Y.

Such shifts in item response behavior could emerge because, e.g., political events can heighten social desirability concerns, shift perceived social norms, or increase reluctance to express views seen as contentious (Singh & Tir, 2023). As a result, respondents provide different answers after such an event even if their true opinions remained the same. In these situations, observed pre- to post-event change in Y reflect shifts in the *expression* of opinions rather than in the opinions themselves.

The second branch of mechanism B involves changes in unit response behavior. That is, the event changes who decides to participate in the survey and who does not. Thereby, the post-event sample captures different (types of) people than the pre-event sample. If responsiveness is correlated with the outcome variable, such compositional shifts can alter the observed distribution of Y in a way that does not necessarily mirror true change in the opinions reflected by Y. If, as a consequence, individuals with certain attitudes, preferences or traits that underlie Y become more or less likely to respond after the event, these characteristics are over- or underrepresented in the post-event sample.

Figure 1.3 illustrates how a shifted correlation of responsiveness with the outcome variable can produce shifts in the post-event observations of Y. If an event increased (or decreased) the association of responsiveness with Y[5], those who are willing to respond have higher (or lower) levels of Y than the less responsive. Thereby, the observed sam-

[5]Again, note here that the exemplary correlation $\rho(R^*, Y) = 0$ prior to the event (left panel) is chosen for consistency with mechanism A and for simplicity. The pre-event correlation of R^* and Y may also be different from zero. The decisive factor for an event to introduce *new* bias is that this correlation becomes stronger (in either positive or negative direction) in response to the event.

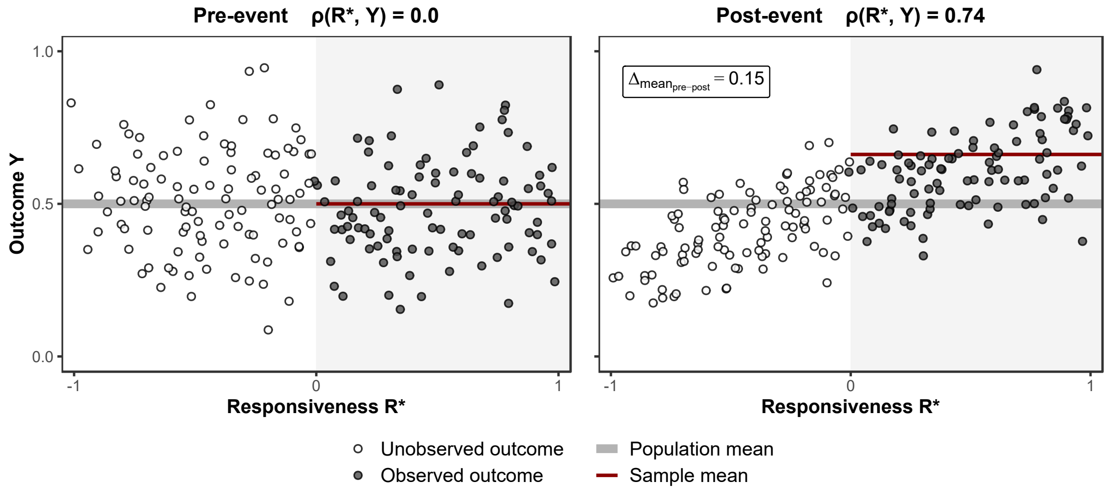

Figure 1.3: *Mechanism B* of event-induced shifts in the outcome variable. The grey-shaded area represents the part of the population that would be observed in the sample.

ple mean of Y is biased upwards (or downwards) in the post-event sample, even though the population mean of Y remains perfectly stable.[6] Just as we observed a 0.15 unit post-event increase in Y in Figure 1.2 that reflected true opinion shifts, we observe the same 0.15 unit increase in Figure 1.3, however now rooted in event-induced shifts in survey participation.

Such shifts in unit response behavior could occur because, e.g., political events can shape the salience of politics in general or certain political issues, individuals' levels of political engagement, or the perceived relevance of sharing political opinions. As outlined in the literature overview in Section 1.1.1, this may affect people's likelihood of survey participation (Furse & Stewart, 1984; Groves et al., 1999, 2004).

Importantly, the item and unit response branches of this survey-related mechanism B can operate simultaneously: an event may alter how respondents answer questions while also affecting who participates. Such survey-related changes (mechanism B) can occur at the same time as true opinion shifts (mechanism A). When combined with substantive opinion change, event-induced response patterns may amplify, offset, or even mask an event's observed effect on Y. Conversely, when operating on their own, the survey-related mechanism can create the appearance of opinion change where none exists.

[6]Mathematically, this mechanism would produce the same pattern if the general participation threshold of $R^* > 0$ would be increased due to the event, or, in other words, if only highly responsive people would agree to survey participation. However, for reasons of consistency, I illustrate this mechanism by an increased correlation of R^* and Y instead of an increased participation threshold.

It is also important to note that not all individuals are equally susceptible to such event-induced shifts. Some people are consistently highly responsive and attentive, such that even salient events are unlikely to alter their participation or item response behavior. Others are persistently uncooperative or unreachable, making it very unlikely that political events would induce them to start participating. Event-induced response effects are therefore most likely to arise among individuals who are not located at the margins of the responsiveness spectrum, but whose willingness to respond and way of answering is more dynamic and dependent on the specific context and situation of the survey request.

Taken together, event-induced changes in response and participation behavior introduce an additional source of variation in survey data that must be disentangled from genuine opinion change in order to draw valid inferences about how political events affect public opinion. In the following section, I outline how this thesis approaches this challenge.

1.3 Three Perspectives on How Political Events Affect Survey Response and Measurement Bias

This thesis addresses the overarching research questions from three analytical perspectives, which I argue are essential for a comprehensive and sound approach to understanding event-specific survey response and measurement bias. These are, first, the (political) *events* that may exert such effects, second, the individual-level *survey response* behavior and, third, the *data structures* through which potential event-specific survey behavior becomes observable. Figure 1.4 illustrates these perspectives and their components.

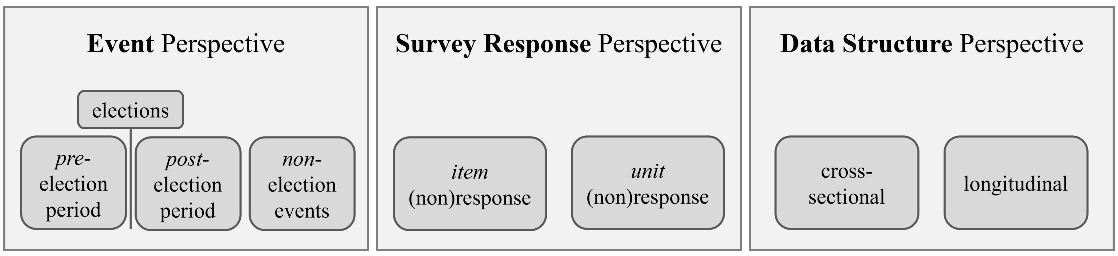

Figure 1.4: Overview of the three analytical perspectives through which event-induced survey response and participation are studied in this thesis.

1.3.1 The Event Perspective

The first perspective sets in focus the events that could trigger changes in survey response. Building on the idea that there are parallels between political participation and survey participation, it is a natural starting point to focus on election-related events: Elections are among the most salient and consequential political events and voting is a straightforward form of political participation (Barnes & H. Kaase, 1979; Campbell et al., 1954; Kriesi, 2013; Milbrath, 1981). Therefore, my research presented in this thesis analyzes, first, events in the *pre-election phase*, when anticipation and political mobilization increase (Wlezien & Erikson, 2001). Then, I shift the focus on elections themselves and the immediate *post-election phase* when individuals react to electoral outcomes. These are moments in which political motivation, attention, and emotional engagement are heightened (Banducci & Karp, 2003), making them fertile contexts for observing whether and how political stimuli spill over into the domain of survey participation. Further, moving beyond these inherently political events, I also analyze survey-related effects of *non-election events*. Here, I specifically focus on terrorist attacks that are no direct outcomes of political processes.

1.3.2 The Survey Response Perspective

The second perspective centers around the individual-level mechanisms of survey response and participation. Expectedly, external events can influence different points of the multi-stage decision process of survey response (Albaum & Smith, 2012; Green et al., 2004), as outlined in Section 1.1.1. They may thereby affect the overall willingness to participate, reflected in *unit (non)response*, or shape how individuals answer items, reflected in specific *item (non)response* patterns. Acknowledging that event-induced changes in both dimensions can introduce bias to the measurement of political phenomena, the papers in this thesis analyze both in different combinations.

1.3.3 The Data Structure Perspective

The third perspective concerns the structure of the data that is generated in the context of an event and used to assess event-specific survey response patterns. The type and structure of the data determines, first, what can(not) be known about respondents and, particularly, nonrespondents and, second, the implications of event-induced changes in survey response. My analyses focus on *cross-sectional* and *longitudinal* surveys.

Cross-sectional surveys, on the one hand, draw a fresh, independent sample for each data collection, whether through, e.g., random sampling or quotas. The main advantage of this type of data is that samples usually yield their initial intended representativeness and are free of distortion by, e.g., attrition effects that may be present in longitudinal data. However, the cross-sectional design poses a major challenge for research of nonresponse: there is a lack of baseline information on people who refuse to cooperate (Lin & Schaeffer, 1995). Consequently, it is difficult to identify the characteristics and motivations of those who change their survey response behavior after and event and, thereby, to detect and correct for resulting measurement biases.

Longitudinal surveys that follow the same individuals over time, on the other hand, overcome this constraint. When nonresponse occurs in a panel survey wave, there is, by design, some baseline data available that was collected in the recruitment wave. This allows to identify at least some characteristics of nonrespondents (e.g. sociodemographics and other time-invariant traits) and observe who becomes less responsive after certain events (Bianchi & Biffignandi, 2019; Lugtig, 2014). At the same time, panel data are constrained in other regards. Individuals who join a panel are already comparably cooperative, which means that event-induced changes occur within a selected subset of above-average responsive individuals. This bias toward highly cooperative respondents likely increases over time, as the burden of participation accumulates wave by wave and the likelihood of dropout or temporary nonresponse increases (Lemay, 2009; Lipps, 2009; Schoeni et al., 2013). To exploit the advantages of both data structures, I utilize both cross-sectional and longitudinal data in my analyses.

The papers of this thesis each address different components of these three analytical perspectives. Thereby, the papers collectively cover the full range of dimensions relevant to understanding how political events shape survey response and political measurement in dynamic political contexts. At the same time, each paper also stands on its own and makes a contribution that reaches beyond the scope of this thesis. In the next subsection, I give an overview over the three papers and their analytical emphases.

1.4 Overview of the Three Papers

Table 1.1 summarizes the components of the event perspective, the survey response perspective, and the data structure perspective that each paper addresses. In the following subsections, I briefly describe each paper, outline how it fits into this broader framework and summarize its objectives, research design, and key findings. Additionally, Table 1.2 provides a condensed overview of the three papers. The code and data to reproduce each paper's results, tables and plots are available in an OSF repository.[7]

[7] https://osf.io/6jeuk/overview?view_only=ae3143b2c3b0435e8d3ce854958383d9.

	Event Perspective	Survey Response Perspective	Data Structure Perspective
Paper 1	Pre-election period	Item (non)response	Cross-sectional & longitudinal
Paper 2	Post-election period	Unit (non)response	Longitudinal
Paper 3	Non-election events	Unit (non)response	Cross-sectional

Table 1.1: Overview of how each paper taps into the three analytical dimensions.

1.4.1 Paper 1: How Snap Election Calls Shape Vote Intention Uncertainty

Paper 1 (Section 2) examines how survey responses and the expression of uncertainty unfolds immediately after the announcement of early elections, addressing the specific research question:

How do snap election announcements affect people's short-term vote intention uncertainty, expressed through "don't know" survey responses?

From the event perspective, it focuses on the arguably most fundamental and salient political event in democratic systems: elections. Analyzing dynamics in the pre-election period immediately after an early election call, this paper marks a natural, almost chronological starting point for studying how political events shape survey response.

From the survey response perspective, the paper concentrates on item response behavior in vote intention questions, particularly on the likelihood of stating "don't know" (DK). This focus thereby closely bridges event-induced effects on political behavior with effects on survey response behavior: On the one hand, DK answers are substantively meaningful indicators of uncertainty in respondents' vote intentions (Elkjær & Wlezien, 2025; Graham, 2021; Purdam et al., 2024). Simultaneously, they represent a particular pattern of survey response behavior, that is, choosing to withhold a concrete preference when prompted to provide one (Durand & Lambert, 1988; Purdam et al., 2024). As such, shifts in the distribution of DK responses capture both changes in political preferences and changes in how respondents answer survey items.

With respect to the data structure perspective, Paper 1 relies on both cross-sectional and longitudinal data to capture short-term changes in item response and leverage the specific benefits each data structure comes with. Together, these elements position Paper 1 as the first step within the broader framework, showing how an electoral event can shape the expression of political preferences in surveys. The paper summarizes as follows:

Objective: This study investigates how snap election calls affect short-term uncertainty in vote intention, using "don't know" survey responses as a proxy for uncertainty. While prior work has examined how snap elections influence voters' attitudes and political behavior, this study focuses specifically on whether early election announcements reduce or increases voters' expressed uncertainty. This is a test of how contextual events may shape the way *how* people respond in political survey items.

Argument: Snap election calls disrupt the expected electoral timeline. Unlike scheduled elections, these announcements create temporal pressure and alter the information environment, arguably leading to an acceleration in decision-making and a shift in how voters form and express preferences. It is expected that these dynamics reduce vote intention uncertainty overall, though the size and distribution of the effect may vary depending on contextual factors, such as the political system, and individual-level factors, particularly partisan identification.

Data and Methods: The analysis focuses on two recent cases: the 2025 German Federal Election and the 2024 UK General Election. In Germany, the study applies a quasi-experimental design based on the "unexpected event during survey design" (UESD) framework using data from the German Longitudinal Election Study (GLES). In the UK, it employs a within-subject panel design using British Election Study Internet Panel (BESIP) data collected immediately before and after the election announcement.

Results: The results show that snap election calls significantly reduce expressed vote intention uncertainty. In Germany, the reduction of DK responses is concentrated among voters without a partisan identity. In the UK, the effect is broader, with declines in uncertainty observed across non-partisans, weak partisans, and even strong partisans. Exploratory analyses show that other factors like ideology, incumbent support, or political interest do not systematically account for the observed uncertainty reductions. Further, the consistency of effects across cases implies that the identified effect seems to be rooted in the early election announcement itself and less so in the case-specific political setting. Placebo tests with previous scheduled elections also reveal that the effect is exclusive to these two snap election calls and no regularity observable in any pre-election period.

1.4.2 Paper 2: Survey Nonresponse After Elections: Investigating the Role of Winner-Loser Effects in Panel Attrition

Paper 2 (Section 3) analyzes how survey participation shifts in response to election outcomes. Precisely, it addresses the question:

How do election outcomes and, specifically, winner-loser status affect the likelihood of post-election short- and longer-term panel attrition?

From the event perspective, it complements Paper 1 by analyzing elections as influential political context, however extending the focus from the pre-election phase to election outcomes themselves and the post-election period.

From the perspective of survey response behavior, Paper 2 marks a clearer step from political behavior into the domain of survey response behavior by shifting the focus from item response behavior to unit (non)response. By investigating how election outcomes and winner-loser status affect the decision to participate in an immediate post-election panel survey wave and in subsequent waves in the post-election period, Paper 2 provides a more direct test of how political context shapes the likelihood of (continued) survey participation.

In terms of data structure, Paper 2 relies on longitudinal data, leveraging its advantages of baseline information on future nonrespondents, repeated measures of the same individuals, and the possibility to trace the temporal evolution of survey participation after a political event. This allows to analyze both immediate drop-out and the durability of nonresponse across multiple post-election survey waves. Taken together, Paper 2 advances the overall dissertation argument by demonstrating how a major political event can shape who remains represented in survey data and for how long such effects and potential resulting measurement biases linger. The paper summarizes as follows:

Objective: This study examines whether election outcomes and, specifically, individuals' winner-loser status influence their likelihood of participating in post-election panel surveys. The aim is to assess if such winner-loser dynamics contribute to systematic nonresponse, thereby affecting the representativeness of public opinion in post-election survey data.

Argument: While electoral winners may be more inclined to continue participating due to, e.g., increased democratic satisfaction, electoral losers may be more likely to disengage, particularly those with lower support for democratic principles. However, not all losers behave the same: some may accept their defeat and remain involved and responsive ("graceful losers"), while others may withdraw or only selectively participate to express dissatisfaction ("sore losers").

Data and Methods: The analysis draws on individual-level GLES panel data, covering the 2009, 2013, 2017, and 2021 German federal elections. The study uses logistic regressions to estimate the immediate effects of election outcomes on post-election nonresponse and employs survival analyses to explore the duration of nonresponse over time. Electoral winners and losers are defined using an objective measure based on voting for parties that have gained (winners) or lost (losers) vote share from one election to another. Democratic satisfaction, support for democratic principles, and prior survey participation are included as key moderating variables.

Results: The findings show that, on average, winners and losers are equally likely to drop out of a panel in the post-election survey wave. However, substantial within-group differences exist: both winners and losers who express low democratic satisfaction and weak support for democratic principles are more prone to (durable) nonresponse. Additionally, prior survey loyalty is a strong predictor of continued participation and outweighs the effects of winner-loser status and political attitudes.

1.4.3 Paper 3: Assessing Sample Imbalances in Event-Study Designs: Observable and Unobservable Sources of Bias

Paper 3 (Section 4) addresses the methodological issue of how to identify and mitigate observable and unobservable sample imbalances that occur when events affect survey responsiveness and sample compositions. Specifically, it answers the question:

When and for whom do unexpected and salient events affect the likelihood of participating in a survey? And ultimately, do event-induced sample disproportionalities substantively bias causal conclusions we draw from survey data?

From the event perspective, this paper addresses non-election events, specifically terrorist attacks. These events are sudden, highly salient, and politically consequential, yet they are not embedded in a predictable political cycle. By focusing on such non-electoral events, this paper complements Paper 1 and 2 by moving beyond elections to examine external stimuli that are less of a direct outcome of political processes and after which political conditions change abruptly.

From the viewpoint of the survey response process, Paper 3 focuses on unit (non)response. Thereby, it adds to the insights generated by Paper 2 in understanding how political context can reshape the set of individuals whose opinions are measured in surveys.

Regarding the data structure, Paper 3 draws primarily on cross-sectional surveys. Even though parts of the analysis rest on single panel survey waves, all data are utilized in a cross-sectional manner. Taken together, Paper 3 complements the analytical dimensions of the other two papers by a stronger methodological focus to understand better when and how event-induced sample disproportionalities occur, and, crucially, how they can be detected and corrected for. The paper summarizes as follows:

Objective: This study examines how political events can distort survey-based causal estimates by not only shifting political preferences but also by altering who responds to surveys. The objective of this study is to develop and apply a framework that disentangles true causal effects of political events from compositional bias rooted in event-induced survey (non)response.

Argument: Many causal identification strategies based on pre-post designs, such as those leveraging events as natural experiments (e.g., UESD), assume that pre- and post-event samples are balanced. This paper argues that political events can threaten causal inference when they simultaneously affect political attitudes and survey participation. If an event systematically alters who responds, estimated effects reflect a combination of genuine opinion change and shifts in sample composition. While sample imbalances on observable characteristics can be adjusted for, unobserved differences (e.g., latent political preferences, psychological traits, or past experiences) may still be present and distort causal estimates. To identify causal effects based on such pre-post designs, potential observable and unobservable compositional biases need to be detected and accounted for.

Data and Methods: The paper focuses on identifying and mitigating bias in "unexpected event during survey design" analyses. It combines established UESD best-practices to detect bias rooted in observable factors with sensitivity analyses to gauge the likelihood of bias rooted in unobservable factors. This approach is demonstrated using the 2015 Charlie Hebdo terrorist attacks in Paris and their effect on government satisfaction (i.e., a typical rally-around-the-flag outcome), drawing on European Social Survey (ESS) data. To test the framework's robustness and generalizability, 14 published UESD analyses of terrorist attacks are replicated using data from the ESS, British Election Study, and Afrobarometer. These replications explicitly test the rally-around-the-flag hypothesis by focusing on typical rally outcomes (e.g., government satisfaction, trust in politicians, and prime minister approval).

Results: Across the Charlie Hebdo example and the 14 replications, observable differences between pre- and post-event samples can be effectively corrected for. The estimated event-effects remain largely unchanged after adjustment for observable sample imbalances. Sensitivity analyses indicate that an unobserved confounder would need to be very strong to overturn most findings. However, results derived from samples restricted to very narrow temporal windows around the respective event are more sensitive. Overall, both directional and null effects of rally-around-the-flag reactions to terrorist attacks established in the literature appear robust. This suggests that event-induced observable and unobservable shifts in survey participation are usually not large enough to threaten substantive causal conclusions.

1.5 Discussion and Conclusion

1.5.1 Summary and Discussion of the Findings

In this thesis, I address the questions of how political events and contextual changes affect survey response behavior (RQ 1) and when and how such event-induced shifts bias the measurement of political phenomena (RQ 2). Through the complementary perspectives of the three papers, I demonstrate that political events can affect both political and survey participation behavior, but that the analytical implications of these effects vary across contexts, events, and outcomes.

The first paper examines how unexpected snap election calls affect vote intention uncertainty as expressed through "don't know" survey responses. Across the German and UK cases, snap election announcements lead to immediate reductions in expressed vote uncertainty, particularly among politically less involved and non-partisan respondents. With respect to RQ 1, these findings show that political events can affect how respondents answer specific survey items. At the same time, a series of robustness checks,

Table 1.2: Summaries of the three papers

	Paper 1	Paper 2	Paper 3
Title	*How Snap Election Calls Shape Vote Intention Uncertainty*	*Survey Nonresponse After Elections: Investigating the Role of Winner-Loser Effects in Panel Attrition*	*Assessing Sample Imbalances in Event-Study Designs: Observable and Unobservable Sources of Bias Causal Inference*
Authorship	Klara Müller & Alejandro Fernández-Roldán	Solo authorship	Solo authorship
Research question	Do voters reduce or increase their expressed vote intention uncertainty in response to a snap election call?	When and for whom do election outcomes drive survey nonresponse? How long does post-election nonresponse persist?	When and how do political events alter survey participation? How does compositional bias distort causal estimates of such events?
Methods	UESD, individual-level panel regression	OLS and logistic regression, survival analysis, first-difference simulations	UESD, sensitivity analyses
Data	German Longitudinal Election Study Panel and Campaign Panels (GLES, 2015, 2016, 2023, 2025) British Election Study Internet Panel (BES, 2024)	German Longitudinal Election Study Panel and Campaign Panels (GLES, 2015, 2016, 2023)	European Social Survey (ESS, 2022) British Election Study Internet Panel (BES, 2024) and Afrobarometer data (Afrobarometer, 2015)
Core findings	Snap election calls immediately reduce DK responses in vote intention items in Germany (mainly among non-partisans) and across all partisan groups in the UK. Vote uncertainty reductions are moderated by strength of party identification.	Winning or losing an election does not by itself affect post-election survey nonresponse or its duration. Instead, negative views of democracy and prior survey habits are stronger drivers of nonresponse and attrition.	The paper introduces a framework to a) mitigate compositional bias rooted in observable factors and b) assess how likely unobservable factors are to substantively bias causal estimates.
Publication status	Under review at *Electoral Studies* Impact factor (IF): 2.3	Published in the *International Journal of Public Opinion Research*, IF: 1.3 DOI: 10.1093/ijpor/edaf031	In preparation for submission

including an application of the bias-mitigating framework developed in Paper 3, indicate that these shifts are not primarily driven by event-induced changes in survey participation or sample composition. Instead, the observed changes are most consistent with genuine preference adjustments to an altered electoral timeline and information environment. For RQ 2, this implies that event-induced changes in survey response behavior do not necessarily signal compositional or measurement bias, but rather reflect substantive preference shifts.

The second paper shifts the focus from item response to unit response behavior by analyzing post-election panel nonresponse. Contrary to expectations derived from winner–loser theories, winning or losing an election does not systematically affect the likelihood of post-election nonresponse or its persistence. Instead, heterogeneity within both winners and losers along the lines of democratic support and prior survey loyalty is more consequential. For RQ 1, this paper shows that elections can affect who remains represented in survey data, though primarily through attitudinal and behavioral predispositions rather than winner–loser status per se. For RQ 2, the results suggest that event-induced nonresponse does not generate uniform post-election measurement bias. However, it may introduce more selective compositional distortions tied to democratic attitudes, which can affect survey-based inference if left unaddressed.

Complementing the first two papers, Paper 3 focuses on the consequences of such compositional shifts for causal inference. It develops a framework to detect and mitigate compositional bias in event-based pre–post survey designs. The analyses show that political events can alter sample compositions even within narrow temporal windows. However, after adjusting for observable imbalances, estimated event-effects and their substantive implications remain largely stable. Sensitivity analyses suggest that unobserved compositional bias would generally need to be implausibly strong to overturn the substantive conclusions. With respect to RQ 1, this paper confirms that political events can affect who responds to surveys, which reinforces some of the unit-response dynamics identified in Paper 2. Regarding RQ 2, it shows that such event-induced compositional shifts do not necessarily bias measurements, which supports the implications of Paper 1 and 2. At the same time, it identifies specific conditions under which causal estimates become more sensitive to compositional bias. For instance, event-based designs are most vulnerable to observable and unobservable compositional bias when, e.g., the temporal window around the event is narrow or when the used data only includes a limited range of variables to account for (observable) pre-post sample imbalances.

Overall, in addressing RQ 1, the findings show that political events can affect survey response behavior along two distinct but related dimensions: *how* individuals express political attitudes in surveys and *who* remains represented in survey data by shaping willingness to participate. However, these effects are not uniform but depend on event characteristics and individual predispositions.

Regarding RQ 2, the combined evidence implies that event-induced shifts in survey response behavior do not automatically affect measurement accuracy. While political events can introduce compositional changes in survey samples, these shifts often remain small and/or addressable through established adjustment strategies, such as weighting. Measurement bias arises primarily under specific conditions, e.g., when the sample characteristics that are shifted by the event closely correlate with the outcome of interest or when data availability limits the ability to detect and correct imbalances. Consequently, event-based survey designs are not inherently prone to bias, but require careful assessments to distinguish true changes in the outcome of interest from artifacts of the survey response process.

1.5.2 Limitations and Pathways for Future Research

While the studies of this thesis provide important and original insights into context-specific survey response and measurement bias, they are naturally constrained in their conceptual, theoretical and empirical scope. In this section, I address the core limitations and outline how future research can address them to further advance our understanding of survey response and measurement bias in dynamic political settings.

A constraint faced by all studies is the absence of a direct measure of treatment reception. I deliberately chose to analyze highly salient events of national scope to justifiably assume a near-universal treatment reception across the population. Despite, I cannot empirically confirm whether every respondent was aware of the respective treatment event (i.e., the snap election call, the election outcome, or the terrorist attack) before completing the survey. Similarly, it is methodologically impossible to ensure treatment reception among nonrespondents. While treatment reception could effectively be approximated in some studies (e.g., in Paper 2 by indications of having voted), it is more limited for the unanticipated events of Paper 1 and 3. Thus, even though it is unlikely that differential treatment reception produced substantive noise in case of these high-saliency events, some uncertainty remains that may have affected the causal estimates.

Future research could resolve this uncertainty by investigating treatments with more clear-cut boundaries for reception, such as geographically constrained events (e.g., localized natural disasters or regional policy implementations) or policies targeted at specific groups (e.g., defined by age, profession or societal status).

Further, I see the potential for future research to investigate in greater detail the underlying mechanisms by which events drive shifts in response behavior. This involves both the more general reasons for (non)response (as outlined in Section 1.1.1) as well as the individual-level psychological mechanisms through which people change their survey response behavior:

First, concerning the general reasons for survey (non)response, the analyses of unit nonresponse (Papers 2 and 3) do not strictly differentiate between the three primary causes established in the literature: noncontact, inability to participate, and refusal to cooperate (Tourangeau, 2004). While I argue that event-induced nonresponse is predominantly rooted in the latter, i.e. in an active choice of refusing to cooperate, I cannot rule out that a portion of the observed effects stems from noncontact or inability. Even though the extent of any resulting measurement bias would remain unchanged irrespective of the exact reason of nonresponse, differentiating them could further advance our understanding of how the political context interferes with the survey process, and for developing strategies to minimize event-induced biases in future fieldwork.

Second, even if all observed effects were exclusively rooted in the individual-level determinants of survey response (i.e., refusal), some vagueness remains regarding the underlying psychological drivers for change in item- or unit-response behavior. For instance, while mechanisms like frustration or appraisal are discussed (Papers 1 and 2), my chosen research designs and utilized data naturally limit the analyses in isolating the precise psychological drivers (such as, e.g., anxiety, civic duty, or disconnect from politics) responsible for the behavioral shift. By incorporating this level of individual-level granularity through, e.g., experimental designs, more detailed theories about the interaction between politics and survey response could be established and tested.

Tapping into the survey methodological domain, this work is somewhat agnostic of the potential that event-effects may differ between survey modes. While the conceptual and theoretical basis underlying all studies is applicable across survey modes (Albaum & Smith, 2012), existing research highlights that the specifics of survey participation differ by mode (Bosnjak et al., 2005; Keusch, 2015). For instance, people can more easily deny participation online than when confronted by an interviewer at their doorstep. Also, response quality is affected by the survey mode (e.g., higher social desirability bias in face-to-face compared to online interviews (Kreuter et al., 2008; Tourangeau et al., 2000)). While most of my analyses utilize online surveys (Papers 1 and 2), Paper 3 incorporates data from different modes (e.g., computer-assisted web surveys in the BES versus face-to-face interviews in the ESS). I predominantly see strengths in this mixed design, as it highlights that the findings are consistent across modes. At the same time, it leaves room for future research to assess whether event-specific survey response and the direction and strength of related biases differ across modes.

Further, future research could expand the temporal dimension of the analyses. While Paper 1 assesses the short-term durability of event-effects over the survey field time and Paper 2 examines the persistence of election-induced effects on panel nonresponse, extending the temporal focus could further improve our understanding of such effects' longevity and consequences. This would yield two key insights:

First, we could learn more about when and for how long to expect measurement biases to occur in certain variables after certain events. Thereby, analytical tools and measurement validity could be improved. Second, we could learn about how event-induced response and participation behavior feeds back into political behavior and may, ultimately, shape political outcomes. If such effects persist until politically consequential moments, such as election day, they would have substantially stronger electoral implications than event-effects that fade quickly. I see several pathways for future research to address this. Paper 1 could be extended using available follow-up panel waves for both studied cases (GLES for the 2025 German federal election and BESIP for the 2024 UK general election) to assess the intra-individual durability of snap call effects until and beyond election day. Paper 2 could be expanded to incorporate more elections, testing for even longer-term durability as well as effect-consistency across more elections. Finally, complementing the cross-sectional analyses in Paper 3 with suitable case-specific panel data could reveal whether rally-type outcomes and event-induced nonresponse persist over longer periods.

Turning to issues of external validity and generalizability, another limitation of this work concerns the challenge of drawing inferences from non-random samples, particularly panel surveys. Panel respondents are above-average responsive and cooperative compared to the general public (Müller, 2025). Accordingly, the panel nonrespondents studied (predominantly in Paper 2) are individuals who were initially cooperative but became less so after an event. Learning about true nonrespondents who fundamentally refuse to participate in surveys remains difficult. Although this above-average responsive subset of the population represented in panel data still provides valuable insights, it is also known that these initially responsive nonrespondents differ from those who are genuinely reluctant or hard-to-reach (Lin & Schaeffer, 1995; Stoop, 2004). Consequently, while the event-effects identified in this thesis are robust within the panel context, they may underestimate the magnitude of nonresponse bias that occurs among the least cooperative segments of the population, where event-induced nonresponse is likely more severe.

Further tapping into questions of generalizability, the presented studies are, by design, limited to the specific cases, types of events and outcome variables under study. While all studies adopt a comparative perspective across time, across countries, or both, follow-up research could expand this scope to further test the consistency of my findings and to maximize generalizability.

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2 How Snap Election Calls Shape Vote Intention Uncertainty

*This chapter is based on an article currently under review at **Electoral Studies***:
Klara Müller and Alejandro Fernández-Roldán. "How Snap Election Calls Shape Vote Intention Uncertainty"

Abstract

While prior research has examined attitudinal and behavioral responses to snap elections, in this paper we study whether snap election calls influence vote intention uncertainty, measured through "don't know" (DK) responses in public opinion surveys. We find that snap election calls immediately and significantly reduce vote intention uncertainty. We demonstrate this through two cases: the 2025 German federal election and the 2024 UK general election. Leveraging a quasi-experimental unexpected event design in Germany and individual-level panel data in the UK, we show that snap election calls accelerate the timing of vote preference formation. Yet the distribution of these effects differ across contexts: in Germany, reductions are concentrated among non-partisans, whereas in the UK they are more broadly distributed across the electorate. Our core contribution is to show that snap election calls operate as a distinct political mechanism that triggers more rapid engagement and evaluation, thereby shifting the baseline level of vote uncertainty prior to the campaign. In doing so, we advance understanding of how disruptions to the expected electoral calendar shape the formation and expression of voters' preferences.

Keywords

Snap elections, unexpected event during survey design, electoral preferences, vote intention uncertainty, don't know responses, comparative politics, causal inference

2.1 Introduction

Pre-election periods are times of heightened political engagement and decision-making, during which voters must resolve their preferences under real-world time constraints. However, not all elections follow predictable calendars: unlike ordinary elections, snap elections often arrive unexpectedly and disrupt the anticipated political timeline, intensifying the pressure on voters to make up their minds and reshaping their expectations (Barnfield, 2023). These early election calls alter the expected course of political terms, the information voters are exposed to, how they process it, and consequently how they form their preferences. Recent scholarly work has examined the net electoral effects of snap election calls (Blais et al., 2004; Daoust & Péloquin-Skulski, 2021; Sevi et al., 2023) and their implications for voter attitudes (Sevi et al., 2023; Turnbull-Dugarte, 2023). Yet, despite the growing frequency of snap elections in parliamentary democracies, especially in Western Europe, we still know relatively little about how such sudden announcements shape voters' responses to vote intention questions in the immediate aftermath of a disrupted electoral timeline.

In this paper, we examine how snap election calls affect short-term vote intention uncertainty.[8] That is, do voters reduce or increase their expressed vote intention uncertainty immediately after a snap election is called? On the one hand, a shortened electoral calendar may prompt citizens to accelerate their preference formation process. On the other, the shift in expectations could heighten uncertainty by creating perceptions of information deficits in various domains. Crucially, we anticipate heterogeneous effects across key voter subgroups, particularly with respect to the strength of party identity. In the following sections, we develop these intuitions and draw on recent literature to outline expectations. Then, to address our research question, we use "don't know" (DK) responses in public opinion surveys as a measure of vote intention uncertainty (see Elkjær & Wlezien, 2025; Graham, 2021). Specifically, we examine whether the likelihood of respondents selecting DK when asked about their vote intention changes in the immediate aftermath of a snap election call. To account for relevant contextual differences and provide the necessary level of nuance, we leverage data from two recent cases: the German federal election held in February 2025 and the UK general election

[8] We specifically focus on the potential *immediate* effect, which is conceptually and practically distinct from any other election campaign-related effect (Wlezien & Erikson, 2001). If a snap election *call* produces an identifiable rapid change on vote uncertainty, the baseline for such vote uncertainty changes. Therefore, we argue that identifying this potential initial effect forms the necessary empirical basis for assessing later deviations rooted in pre-campaign or campaign effects.

of July 2024. We follow two distinct methodological approaches, each tailored to the characteristics of the available data.

For the German case, we rely on one wave from the German Longitudinal Election Study Panel (GLES, 2025). Although GLES is a longitudinal study, the wave in field during the snap election announcement was a large refresher wave which can effectively be treated as a cross-sectional survey. We leverage the "unexpected event during survey design" (UESD) framework (Muñoz et al., 2020) on this survey, treating the snap election call as an exogenous shock. For the UK case, we use two consecutive panel waves of the British Election Study Internet Panel (BESIP) (BES, 2024) that were conducted in close succession right before and immediately after the snap election call. Employing individual-level fixed-effects regression models, we trace within-respondent changes in short-term vote intention uncertainty. Both approaches allow for causal identification of a potential effect on vote intention uncertainty. Furthermore, by conducting this cross-country comparison, we can better isolate the effect of the snap election call itself from influences rooted in each specific political and institutional context. Therefore, these two cases allow us to shed light on whether the sudden shift from routine politics to an imminent electoral contest significantly changes the proportion of respondents who express uncertainty about their vote choice (Graham, 2021).

Our findings show that snap election calls rapidly and significantly reduce expressed vote intention uncertainty, though effects vary across cases and key voter subgroups: in Germany, the reduction is concentrated among voters with no partisan identification, whereas in the UK, the decline in uncertainty is more pronounced and affects the electorate more broadly. Despite contextual differences between elections, which we address in detail, the causal identification methods, the consistency of the observed effects across cases and the various robustness checks comparing these snap calls with ordinary elections provide solid evidence that the effects we find are primarily rooted in the phenomenon of the snap election call itself, rather than in the specific political context in which the early election is announced.

This project makes four main contributions. First, it provides strong evidence that snap election calls influence voters' decision-making and shape the timing of vote preference formation (Box-Steffensmeier et al., 2015; Bunting, 2024; Willocq, 2019). Second, it contributes to European comparative electoral research by leveraging different cases and causal identification methods to study how voters react to snap election calls in diverse contexts. The paper highlights the implications of such disruptions of the anticipated electoral calendar for electoral preferences and informs on what voter groups are more likely to remain undecided before the campaign starts. Third, it lays out why snap election calls constitute a truly singular formula that meaningfully differs from scheduled ordinary elections in both institutional logic and political consequences, with relevant implications for democratic accountability and strategic agency. And fourth, it highlights the methodological relevance of DK responses (Elkjær & Wlezien, 2025; Graham, 2021), which is a frequently overlooked yet crucial element in public opinion surveys. By demonstrating how changes to the regular electoral timeline can affect uncertainty we also provide lessons for public opinion practitioners, e.g., regarding the uncertainty surrounding vote intention estimates in pre-election polls and election forecasts (Barnfield & Johns, 2025).

2.2 Background on Snap Elections

Many democracies have constitutional provisions that allow presidents, prime ministers, or ministries to dissolve parliament early (Morgan-Jones & Loveless, 2023). Only Luxembourg, Norway, and Switzerland are the non-presidential OECD countries where legislative terms are fixed and must be completed in full (see Riera, 2015). These constitutional arrangements are broadly known as *snap elections*, and the electoral call is usually triggered when the incumbent promotes the dissolution of parliament and announces an early election. Although one could argue that there are normative reasons for calling early elections, the literature indicates that governments typically do so for strategic considerations tied to maximizing their prospects of remaining in power (Balke, 1990; Daoust & Péloquin-Skulski, 2021). Therefore, incumbents call early elections when they believe public opinion is in their favor (Sevi et al., 2023). Thereby, they aim to capitalize on electoral gains or minimize losses by leveraging the privileged information about the political and/or economic evaluations that comes with being in power (Schleiter & Tavits, 2016; Smith, 2003). Considering this "strategic" nature and incumbents' flexibility to call elections when conditions are most favorable for re-election (Walther & Hellström, 2019), literature often frames them as "opportunistic" (Schleiter & Tavits, 2016, 2018; Turnbull-Dugarte, 2023). From 1945 to 2013, around one in every seven elections worldwide was an opportunistic snap election (Schleiter & Tavits, 2016).

Typically, governments complete their full legislative terms and hold elections on the scheduled ordinary date (Schleiter & Tavits, 2016, 2018), but calling elections before that date is becoming more common. This has become particularly frequent in recent years, especially in multiparty systems, as increasing parliamentary fragmentation has often resulted in smaller and more unstable governmental arrangements (Best, 2013). Unsurprisingly then, snap elections have become more recurrent across the world. While particularly frequent in Western Europe, our focus in this paper, they are by no means limited to this region. Recent examples include Canada (2025), South Korea (2025), Ecuador (2023), or Israel (2021). Figure 2.1 illustrates the timing and distribution of snap elections held in European countries since 2010, which now account for a substantial share of all national electoral contests.

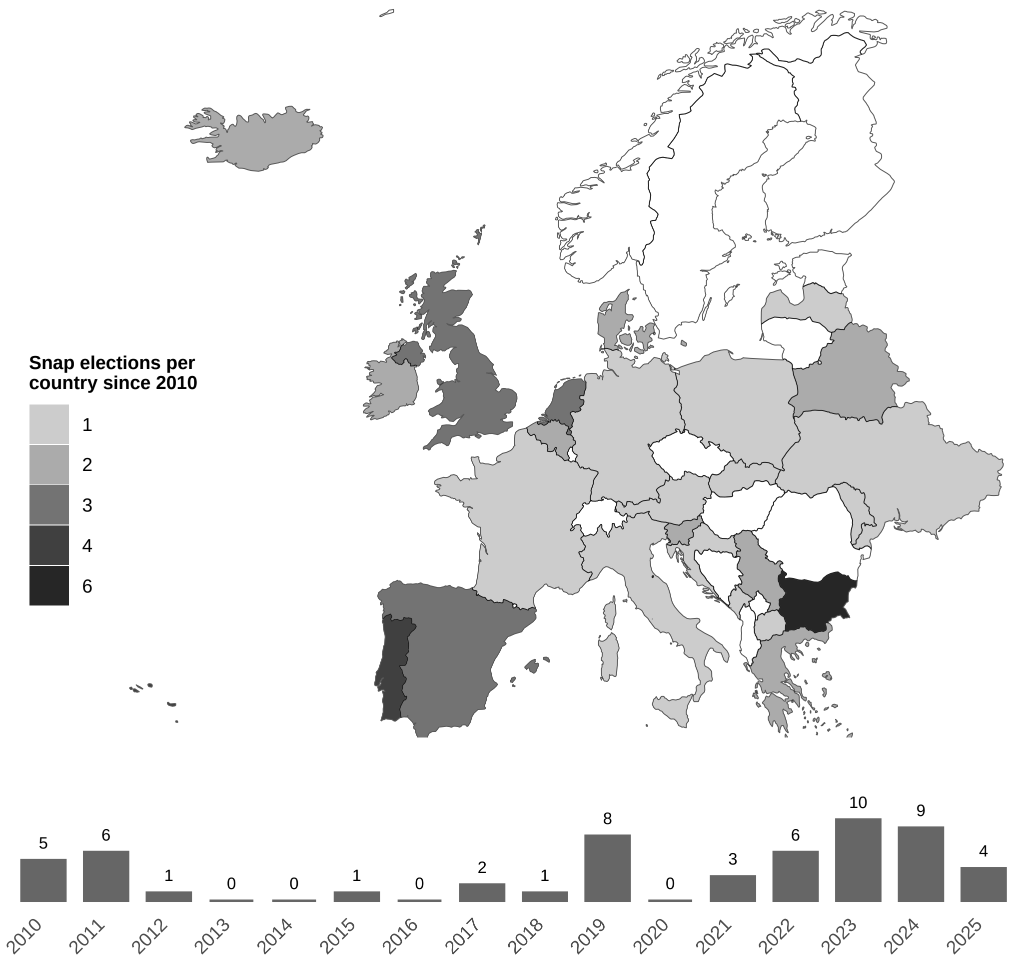

Figure 2.1: Snap elections in European countries from 2010-2025. Data: IFES Election Guide and own elaboration. Table 2.A.1 in the Appendix shows details on all cases.

2.2.1 Effects of Snap Elections

Net Electoral Effects

In addition to examining the arguments and motivations behind snap election calls (Kayser, 2006; Schleiter & Tavits, 2016) and the political contexts in which they are more likely to take place (Kayser, 2005; Riera, 2015; Schleiter & Morgan-Jones, 2009), existing research highlights various political consequences they may entail. Perhaps the most frequent question is whether incumbents do indeed benefit from calling a snap election. The evidence on this is mixed, and even if it may come as an underwhelming explanation, it can be summarized as follows: it depends on voters' perceptions to the election being called early (Sevi et al., 2023). A cross-country analysis of 27 European countries demonstrated that incumbents tend to gain a net electoral advantage by calling early elections (Schleiter & Tavits, 2016), while another study in Canada found that only a small proportion of voters directly penalizes them for such a move (Blais et al., 2004). However, other investigations point to a more conditional relationship. For instance, through cross-sectional data around a Quebec provincial election, Daoust and Péloquin-Skulski (2021) find that voter support for incumbents declines when early elections are perceived as opportunistic. This is consistent with experimental evidence by Schleiter and Tavits (2018), who also note that while snap elections perceived as opportunistic could harm support for an incumbent, this potential negative effect may be offset if the government is perceived to be managing well the economy.

Behavioral and Attitudinal Effects

Regarding the effects of snap elections on voters, some aspects are also worth noting. In terms of turnout, there is no final evidence that snap elections affect individuals' likelihood of casting a vote (Daoust & Péloquin-Skulski, 2021), despite compelling evidence that more frequent voting can be conducive to voter fatigue (see Kostelka et al., 2023). On political attitudes, there are also mixed findings: Turnbull-Dugarte (2023) leverages a quasi-natural experiment in the UK and finds that the 2017 snap election significantly increased citizens' levels of political trust. This result was nonetheless subject to partisan-like moderation (Turnbull-Dugarte, 2023). Perhaps in contrast to this notion, through observational data from European Social Survey 2002–2016, Morgan-Jones and Loveless (2023) investigated how the potential negative signal about incumbent competence associated with calling an early election influences satisfaction with democracy. Their analyzes across 26 European countries find that democratic satisfaction consistently declined in the aftermath of snap elections.

2.3 How Snap Election Calls Affect Vote Intention (Un)Certainty

2.3.1 Snap Election *Calls*

In the literature, snap elections are understood indistinctly as the early electoral contest itself and the announcement that triggers it. While it is obvious that the former follows from the latter, in this paper we explicitly focus on the potential effect derived from the snap election *call*. We conceptualize a snap election call as an institutional disruption to the anticipated electoral timeline, i.e., a shortening of the incumbent's term that challenges prior assumptions of governmental stability. Such an early call accelerates the timeline for potential shifts in both political power and electoral competition by crystallizing ongoing dynamics regarding the parties and candidates that will run, what coalition configurations are ultimately possible, etc.(Gschwend et al., 2017; Wagner & Harteveld, 2024; Wuttke, 2020). Furthermore, a snap election call also alters key issues and messages (Haselmayer et al., 2019), thereby reshuffling present and future public policy discussions too.

Therefore, we argue that snap election calls are not merely early election announcements; rather, they constitute a truly distinct political mechanism meaningfully different from scheduled ordinary elections in both institutional logic and political consequences. The crucial defining feature of a snap election call is the discretion over its timing, which carries implications for democratic accountability (Ashworth, 2012) and strategic agency (Turnbull-Dugarte, 2023). Because the legislature is called off before the full-term performance and legislation of the government can be assessed, incumbents can capitalize on favorable conditions (whether they are significantly ahead in the polls or merely narrowing a gap when they trail), thus trying to optimize the timing of the election to their advantage. But this temporal flexibility also reshapes the information environment, placing other political actors at a structural disadvantage, as the new electoral timeline may, among many other things, constrain public policy debates: the incumbent can attempt to set the initial agenda and shape early narratives already in the statement through which the election is announced. Of course, not always successfully. We argue that all these elements make snap election calls a distinct analytical category in political science.

2.3.2 Effects on Vote Intention (Un)Certainty

Although the extent to which a snap election call is completely unexpected or relatively anticipated depends on the concrete political context, the specific date of the election tends to remain unknown until the call is made official. In systems where election dates are typically fixed and snap elections are rare, such announcements are more likely to catch voters by surprise, particularly those less engaged in politics. Yet even in contexts marked by frequent political instability, the consequences of a sudden election call are always difficult to predict (MacKenzie, 2021). After all, uncertainty is intrinsic to democracy, which is inherently future-oriented (Barnfield & Johns, 2025). Therefore, uncertainty is a defining feature of elections and often lingers, albeit to a lesser extent, even after election results are made public (Bowler et al., 2022; Gschwend et al., 2017). Considering the special nature of snap elections and the distinct contextual uncertainty they induce, it is thus very likely that individuals' predispositions and judgments about this uncertainty may shape how they navigate the situation (Osman, 2010).

To be sure, elections are high-saliency events (Michelitch & Utych, 2018) that draw extensive media coverage and polling activity, often with near-live updates leading up to the election day (Fernández-Roldán & Barnfield, 2024). We argue that snap elections may generate an even greater surge in coverage compared to ordinary elections, for the media are especially drawn to conflict, unpredictability, and electoral competition (e.g., horse-race reporting) (Banducci & Hanretty, 2014; Bhatti & Pedersen, 2016; Fernández-Roldán & Barnfield, 2024). As such, snap election calls are particularly newsworthy. Once the early elections are announced, the flow of election-related information intensifies, raising the stakes and narrowing both media and public attention on the electoral dimension (Gelman & King, 1993; Norris, 2006; Wlezien & Soroka, 2024). While individual exposure varies by factors such as political interest or media diet, it is difficult to miss even basic information about such a prominent event. When an election is called, the thought of casting a vote should become less abstract: the call for an election imposes a reality constraint and resolves ambiguity about when the next election will take place. This may nudge voters to make up their minds soon and to (re)adjust their expectations (Barnfield, 2023). But when voters abruptly learn that an election will occur sooner than expected as a result of a snap election call, the time available to form or revise their preferences contracts. This compressed decision horizon can generate a sense of urgency, or at minimum, a recognition that the period of indecision has become shorter. In contrast, ordinary elections unfold within a relatively predictable calendar, perhaps allowing indecision to linger and preferences to remain unsettled for longer stretches.

As a result, we expect that voters will likely start to engage in political evaluation and preference formation processes right after the announcement (Brady & Johnston, 2009; Ohr & Schrott, 2001). Studies on campaign effects show that the onset of electoral campaigns trigger initial spikes in political attention, information seeking, and preference formation (Malet, 2025; Norris, 2006; Ohr & Schrott, 2001). But even outside formal campaign periods, major political events (e.g., leadership changes or policy crises) often shape public opinion and reduce respondent uncertainty on political issues in surveys (Lehrer et al., 2025; Ohme et al., 2018). We therefore expect that snap election calls enable similar strong effects by rapidly structuring public opinion, hence reducing short-term vote intention uncertainty.

2.3.3 Party Identity and Change in Uncertainty

With the rise of partisan dealignment and higher fragmentation in multi-party systems in the last decades, recent research has documented a growing tendency for voters to delay their decision-making in elections, making them more prone to express uncertainty in vote choice and ultimately less likely to cast a vote (Bunting, 2024; Willocq, 2019). In Western democracies, the proportion of voters postponing their vote choice until well into the campaign has steadily increased over time (Box-Steffensmeier et al., 2015), and this has prompted scholars to investigate the factors that shape the timing of vote decisions. While much of this literature focuses on the campaign period of ordinary elections (Preißinger & Schoen, 2016; Schoen & Müller, 2024), some studies have also explored recent snap elections (Bunting, 2024).

As posited in the previous section, we presume that by introducing a sudden and pressing deadline for preference formation, snap election calls may help accelerating the formation of vote intentions in a way that campaign ordinary elections do not. Of course, in the lead-up to any election voters also engage in candidate evaluation, and campaigns are precisely designed to encourage this. Thus, we do not argue that voters fail to form preferences during ordinary campaigns; rather, we suggest that the pace of preference formation may be distinctly faster following a snap election call.

If this were indeed the case, the snap election call may function as a swift and condensed temporal deadline for preference formation, by which voters may feel uniquely compelled to engage in political decision-making more quickly than they would under ordinary electoral timelines. While individuals vary in how they integrate information into their vote choice, such a high-stakes event could act as a "wake-up call", prompting even the less engaged to process political cues they might otherwise delay considering (Box-Steffensmeier et al., 2015).

At any rate, prior research has shown that political predispositions shape decision-making (Lachat, 2015; Lazarsfeld et al., 1968). Specifically, in the context of elections, partisan identity has been found to account for both uncertainty and volatility (Box-Steffensmeier et al., 2015; Bunting, 2024; Schoen & Müller, 2024). As such, despite a remarkable decline in party identity over recent decades (Willocq, 2019), it still remains a strong predictor of vote certainty, whereas the absence of such an identity is linked to greater aggregate voter volatility (Gunderson, 2025). Against this backdrop, we expect responses to a snap election call to vary substantially based on strength of party identity. Precisely because of their exceptional nature, snap election calls can serve as external cues that prompt voters to (re)engage with their partisan identities (Butler & Stoke, 1974), in ways similar to those observed during regular election campaigns (Farrell & Schmitt-Beck, 2002). This reactivation may strengthen alignment with pre-existing preferences. However, it is important to emphasize that our analysis captures change: since we rely on a relative measure, baseline levels of uncertainty must also be considered when interpreting the potential effects.

Therefore, strong partisans are likely to have formed their vote preferences prior to the call and to interpret new events through the lens of their established political beliefs (Miller et al., 1996). For them, the announcement serves as a powerful partisan cue, reinforcing identity-based loyalty and maintaining vote intentions with long-standing habits (Butler & Stoke, 1974; Clifford, 2017). Because these voters rely on partisanship as their main perceptual filter, they are likely to reaffirm their choices with minimal hesitation (Clifford, 2017; Foos & De Rooij, 2017; Green et al., 2004). Accordingly, we do not expect a substantial decline in expressed uncertainty among this group.

Yet, as electorates develop greater political skills and resources and more political parties compete, reliance on partisan cues is diminishing (Willocq, 2019). Voters with weaker or more fluid partisan ties may require additional effort or information to re-assess their preferences (Box-Steffensmeier et al., 2015). Yet, the strong salience of the electoral context, combined with a heightened perception of time pressure imposed by the snap call, may prompt many of them to default to prior choices. For this group of more dynamic partisans, we expect a moderate decrease in vote intention uncertainty.

The combination of an expanding party landscape, parliamentary fragmentation and declining partisan identity has led to an increased pool of non-partisans (Willocq, 2019). We suspect that the absence of party identity, combined with individual-level motivations and a high baseline-level of uncertainty, leaves greater room for snap election announcements to affect this group, resulting in the most pronounced reductions in immediate vote intention uncertainty (Blumenstiel & Plischke, 2015).

2.4 Overview of Studies

To assess the potential effect of snap election calls on vote intention uncertainty, we analyze two cases: the 2025 German federal election (Study 1) and the 2024 UK general election (Study 2). We selected these cases based on data availability and, relatedly, the potential for strong causal identification of the effects relevant to our research question (see the note to Table 2.A.1 in the Appendix for a detailed account of recent snap elections and data availability constraints). Crucially and luckily, the two cases differ markedly in political context and electoral system, in the extent to which the early elections were predictable, in the role of incumbents, and in the relative standings of political parties at the time of the announcement. These differences allow us to account for case-specific confounders and offer a more comprehensive examination of the potential effect of snap election calls on vote intention uncertainty.

Our first study addresses the recent 2025 German federal election. Germany's Basic Law mandates federal elections every four years, and after the 2021 election, the next regular election was expected in September 2025. Despite mounting tensions within the coalition government of the Social Democrats (SPD), Greens, and Liberal Democrats (FDP), very few anticipated an early election. This changed abruptly on November 6^{th}, 2024, when Chancellor Olaf Scholz (SPD) dismissed Finance Minister Christian Lindner (FDP) and announced his intention to seek a motion of confidence, widely recognized as a call for early elections (Von der Burchard et al., 2024). After the expected failure of the motion in December, elections were set for February 23^{rd}, 2025. Given the surprising nature of Scholz's announcement, the formal mechanisms involved in triggering early elections, and the high electoral competitiveness (amid the SPD's relatively weak polling), the German case constitutes a paramount example of an unexpected snap election.

Our second study examines the 2024 UK general election. Under current UK law passed in 2019, early elections can be called either through a two-thirds majority or following a lost vote of confidence in the House of Commons. In early 2024, weak polling numbers for the incumbent Conservative Party led to widespread speculation that Prime Minister Rishi Sunak would announce an early election in autumn. Nevertheless, Sunak unexpectedly called the snap election on May 22^{nd}, to be held on July 4^{th} (Zeffman, 2024). While the precise date came as a surprise, the general expectation that elections were going to be held relatively soon made the UK's snap call less abrupt than Germany's. Moreover, the broader political context is contrasting: at the time of the announcement, Labour party was consistently leading the Conservative party by a wide margin in the polls. That, compounded with UK's majoritarian electoral system offered voters a much clearer picture of likely electoral outcomes than in Germany.

Therefore, while both cases involve very salient snap election calls, they differ systematically in their level of expectedness, institutional procedures, and political context. Germany's snap election was largely unanticipated and occurred within a more fragmented partisan landscape, whereas the UK election was widely anticipated and took place against the backdrop of a clear Labour lead. This combination of shared features and contrasting conditions makes these cases particularly well-suited for examining how voters respond to snap election announcements under different circumstances. This comparative setup enables us to better isolate the immediate effect of the snap election call itself from broader, case-specific dynamics. We rely on two complementary methodologies tailored to the available data. For Germany, we draw on a refresher wave from the GLES Panel and apply the UESD framework (Muñoz et al., 2020), treating the snap election call as an exogenous shock. By comparing respondents surveyed before and after the November 6^{th} announcement, we exploit natural variation in survey timing to estimate causal effects on vote intention uncertainty. For the UK, we establish causality through panel data from the BESIP: we use two closely spaced waves conducted immediately before and after Sunak's snap election call. Here, we leverage the high retention rate between waves to employ individual-level fixed-effects regression models that trace within-respondent changes in vote intention uncertainty.

2.4.1 Outcome: *Don't Know* to Estimate Vote Intention Uncertainty

We measure vote intention uncertainty using respondents' selection of "don't know" (DK) when asked which party they would vote for if an election were held in the next day. This response option is available in both the GLES and BESIP datasets and is commonly employed in electoral and survey research (Elkjær & Wlezien, 2025). This operationalization is supported by growing evidence that DK responses typically reflect genuine uncertainty stemming from limited knowledge or weakly formed attitudes, rather than from satisficing or random responses (Elkjær & Wlezien, 2025; Graham, 2021; Purdam et al., 2024).[9] For instance, Elkjær and Wlezien (2025) show that without a DK option, researchers lose a useful signal about how confident people are in their answers: when respondents can select DK, those who do give an opinion report feeling more confident of it. Without this option, people give more uncertain answers.[10] Similarly, Graham (2021) analyzes ANES data and various original surveys from 1993 to 2019 and finds that DK responses account for nearly half of the variation in reported confidence, suggesting they are indeed a meaningful proxy for uncertainty. In line with this, Jessee (2017) demonstrates that in questions about political knowledge, people who select DK tend to actually know less than those who give either correct or incorrect answers, which lends support to the idea that DK responses really mean *I don't know*.

Although direct measures of certainty such as confidence scales are sometimes available (Mellon, 2017), we rely on DK responses for two main reasons. First, DK is typically less ambiguous than subjective scales, which can vary in interpretation across individuals and contexts (Jessee, 2017). Selecting DK is a deliberate act that signals uncertainty more reliably, allowing for better cross-national and individual-level comparability. Second, when respondents are truly unsure, they tend to prefer DK if it is available, rather than providing a directionally arbitrary answer (Gilljam & Granberg, 1993; Purdam et al., 2024). We thus argue that DK is a more valid indicator of uncertainty than a forced party choice or subjective scales, for it preserves the signal from respondents who genuinely lack a clear answer.

[9] Satisficing occurs when respondents avoid making the effort required to think through and state out an opinion or preference, which can lead to fewer meaningful responses and lower statistical power (Krosnick et al., 2002; Roberts et al., 2019). However, recent studies nuance this notion.

[10] Alternatively to explicitly stating DK, people might refuse to answer if the survey setup does not force response. Both GLES and BESIP allow respondents to skip the vote intention question. To check for potentially meaningful differences between DK answers and item nonresponse as expressions of uncertainty, we test an operationalization of vote intention uncertainty that accounts for both DK responses and item nonresponse. The results are robust: Table 2.C.1 and Table 2.G.1.

To sum up, the literature suggests that DK are most often selected by individuals who are genuinely uncertain about their vote intention. Once people develop even a slight leaning towards a party, they tend to report it (Graham, 2021). This means that using DK as our outcome likely underestimates the full extent of vote intention uncertainty. As a result, our estimates of how as snap election calls affect short-term voter uncertainty are probably conservative. We use this measure consistently throughout all empirical analyses in this paper. In the following sections, we further detail the data and methods employed for causal identification and present the results for both studies.

2.5 Study 1: The German Federal Election 2025

2.5.1 Data

The snap election call of November 6^{th} fell within the field time of a refresher wave of the German Longitudinal Election Study's (GLES) Panel (GLES, 2025). From October 23^{rd} to November 19^{th} 2024, $n = 9,934$ new respondents were recruited for the GLES panel.[11] All respondents were drawn from an opt-in online access panel with quotas for gender, age, and education. The target population comprised all German citizens with internet access who were eligible to vote in the 2025 German federal election. Surveys were conducted through computer-assisted web interviews (CAWI).

Research has shown that self-selection and quota-based sampling can introduce biases rarely present in random probability samples. This helps explain the higher levels of political involvement and partisanship observed among GLES panelists compared to the general population (Gärtner & Schoen, 2021; Steinbrecher & Schoen, 2013). Over time, panel fatigue may further amplify this over-representation, as politically engaged individuals are more likely to respond to surveys (Voogt & Saris, 2007) and to remain in longitudinal panels (Müller, 2025; Olson & Witt, 2011). However, since our analysis relies on data from the recruitment wave, we do not expect such biases arising from panel attrition to affect our results. Still, it is reasonable to assume that GLES respondents are more opinionated and possess more clearly formed political attitudes, including vote intentions, than the broader population.

[11] The GLES panel (GLES, 2023) is a large scale election panel administered by GESIS, spanning over more than 30 survey waves between 2016 and 2025 and closely monitoring a wealth of variables concerning respondents' voting behavior, political attitudes and personality traits.

2.5.2 Measures

We define the *treatment* as the snap election call on November 6^{th} when Chancellor Scholz publicly announced his intention to seek a motion of confidence. This declaration served as a clear and widely understood signal that early elections were extremely likely. While the formal German constitutional process required additional steps and time, the announcement itself was a decisive and widely recognized signal that voters would need to adjust their expectations about a new electoral timeline (Von der Burchard et al., 2024).

As additional explanatory factor and moderator, we account for respondents' *strength of party identity*.[12] We classify respondents indicating "very weak," "fairly weak," and "moderate" as *weak partisans*, while those selecting "fairly strong" or "very strong" are categorized as *strong partisans*. Respondents who report no party identity are classified as *non-partisans*. For transparency, approximately 23% ($n = 2,256$) of respondents fall into the non-partisan group, 26% ($n = 2,620$) are considered weak partisans and, around 51% ($n = 4,993$) are categorized as strong partisans.

We also control for socio-demographic characteristics, with particular attention to variables used for the sampling quotas. These include *age* (numerical), *gender* (binary indicator with female = 1) and *educational attainment*. The latter is operationalized in three levels: low education (= maximum of 9 years of schooling or no diploma), intermediate education (= 10 years of schooling), and high education (= university entry qualification). In Figure 2.2, we examine the distribution of these key socio-demographic characteristics across the survey fieldwork period.

By design, all respondents were contacted simultaneously and received identical survey invitations, regardless of their socio-demographic or geographical background. Quotas were filled on a rolling basis.

The figure reveals an imbalance in the composition of the sample before and after the treatment, a common occurrence in surveys based on quota samples (Muñoz et al., 2020). While gender and age are relatively stable across the fieldwork, the distribution

[12]To construct these groups, we rely on two variables: party identification and the strength of that identification. First, respondents are asked whether they identify with a political party. Those who indicate a party identity are then directed to a follow-up question measuring how strongly they identify with that party.

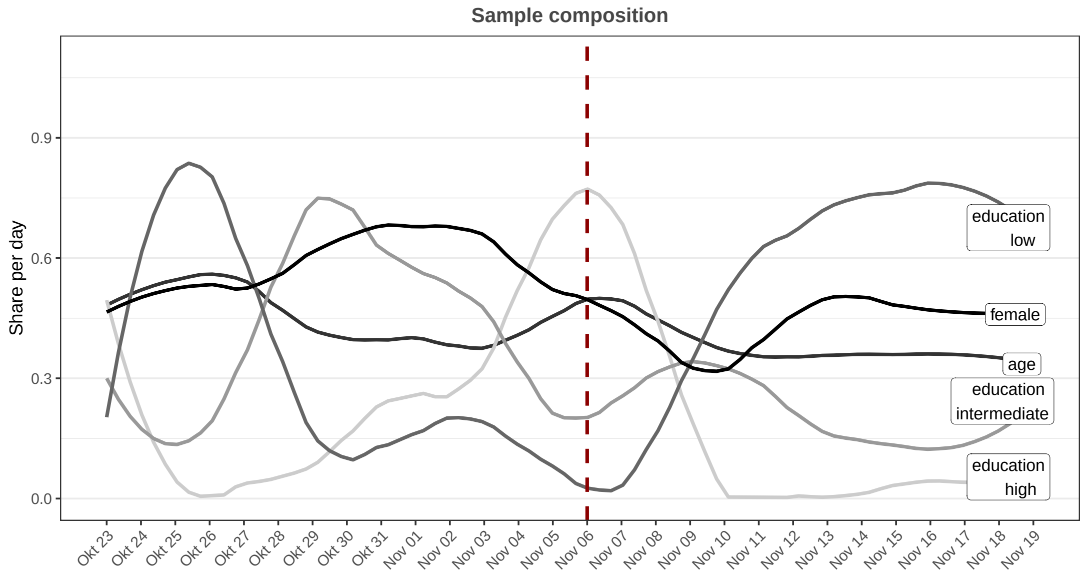

Figure 2.2: Distribution of socio-demographics across the field time of the GLES refresher wave 2024. Lines represent the share per day of respondents holding the respective characteristic (*age* shows average age divided by 100). Red line = day of the snap election call.

of education levels is more uneven, as we observe spikes in the share of respondents with lower education at both the beginning and end of the field period, while respondents with higher education levels are mostly concentrated around the middle of the fieldwork, coinciding with the snap election call.[13] In the following sections, we detail how we analytically account for these sample imbalances.

2.5.3 Analytical Approach

To identify the causal effects of the snap election call on the (un)certainty of vote intentions, we exploit the timing of the unexpected announcement during survey fieldwork. This identification strategy is known in the literature as "unexpected event during survey design" (UESD) (Muñoz et al., 2020), and has been applied to a wide variety of cases, including responses to acts of terrorism and war (e.g. Frese, 2025; Hernández & Ares, 2023; Nägel et al., 2024; Rogowski & Tucker, 2019; Unan & Klüver, 2025) natural disasters (e.g. Berger, 2010; Bol et al., 2021; De Vries, 2018) or political scandals (e.g. Ares & Hernández, 2017; Kim & Kim, 2019; Solaz et al., 2019). While the aforementioned events have different levels of unexpectedness, more foreseeable political events

[13]This also coincides with higher participation rates during these days compared to the average across the field time. For more details on the distribution of participants, see Figure 2.B.1 in the Appendix.

have also been leveraged with UESD. These include campaign events (Flores, 2018), cabinet reshuffles (Nemcok, 2020) and policy reforms (Burlacu et al., 2018; Larsen, 2018; Seimel, 2024). And while several studies analyze the effects of elections (Flores, 2018; Frye & Borisova, 2019; Minkus et al., 2019; Pierce et al., 2016; Schraff & Schimmelfennig, 2020), there is, to the best of our knowledge, only one study employing UESD in the context of snap elections (Turnbull-Dugarte, 2023).

The UESD approach leverages the division of a sample into two subsamples: respondents who participated before the unexpected event at t_e and those who have participated afterwards. The subsample of respondents with $t_i < t_e$ serves as the control group, while all respondents of the $t_i > t_e$ form the treatment group, having participated under the influence of the event. In the case of our study, the treatment T_i is operationalized as follows: $T_i = 0$ (control) if the respondent participated before November 6^{th} 2024 and $T_i = 1$ (treatment) if the respondent participated after November 6^{th} 2024. Respondents who participated on the treatment day are excluded from the analysis. This yields a control group $n = 6,043$ and a treatment group $n = 2,976$.

The UESD framework rests on the assumption that survey participation timing is as good as random. In the absence of the treatment, there should be no reason to expect systematic differences in the characteristics or response behavior of those interviewed immediately before versus after the event. Accordingly, comparing the treatment and control groups should provide an unbiased estimate of the event's average effect on vote intention uncertainty (see also Castanho Silva, 2018). To test our expectations, we thus treat Germany's 2024 snap election call as a natural experiment and follow the best-practices by Muñoz et al. (2020). At the core of our analysis, we estimate linear regression models to isolate the effect of the snap election call on vote uncertainty:

$$Y_i = \alpha + \beta_1 Treatment_i + \gamma X_i + \varepsilon_i \tag{1}$$

Y_i represents the likelihood of stating DK in the vote intention question. $\beta_1 Treatment_i$ captures the average effect of receiving the treatment, which we refer to as the intent-to-treat effect (ITT) (Turnbull-Dugarte, 2023). γX_i denotes a vector of covariates, which varies across our model specifications. In the results section, we report estimates obtained from this OLS model; these prove robust when compared against estimates from an alternative logit model (available in Table 2.C.3 and Table 2.C.4 in the Appendix).

For a causal interpretation under the UESD framework, several assumptions must hold, including *excludability* (no concurrent events or trends affecting the outcome), *ignorability* (as good as random assignment to treatment and control), and *noncompliance* (treatment group respondents are actually exposed to the event) (Muñoz et al., 2020; Nägel et al., 2024). During the fieldwork period of the November 2024 GLES wave, we identify no major political events in Germany that would threaten excludability.

Although we lack a direct measure of treatment reception, we expect noncompliance to be minimal, as the snap election call received remarkable media coverage. Furthermore, to address potential violations of these assumptions (particularly threats to ignorability due to non-random sampling and sample imbalances, as shown in Figure 2.2), we conduct a series of robustness and sensitivity checks. The next sections present our results and corresponding tests of these assumptions.

2.5.4 Results Study 1: Germany

In a first step, we estimate the ITT effect of the snap election call on vote intention uncertainty. Figure 2.3[14] illustrates this effect through three different model specifications: First, we estimate a naïve model that includes only the treatment indicator as explanatory variable. We have reasons to assume that the socio-demographic sample imbalances as shown in Figure 2.2 affect this model outcome and likely harm the UESD's ignorability assumption. To control for these imbalances, we, secondly, re-estimate this naïve model on entropy-balanced samples. That is, we reweigh the control group to match the covariate distributions of the treatment group (regarding age, gender and education) (Hainmueller, 2012).[15]

While the unbalanced naïve model shows a statistically significant negative effect of the snap election call on short-term vote intention uncertainty ($\beta = -0.017$, $SE = 0.006$), i.e., pointing to a reduction in uncertainty immediately following the call, this effect becomes non-significant once we control for socio-demographics in the entropy-balanced naïve model ($\beta = -0.001$, $SE = 0.005$). This underscores the importance of adjusting for covariate imbalances to mitigate potential violations of the ignorability assumption. We therefore estimate all subsequent models based on an entropy-balanced sample.

[14]Many visualizations in this section are inspired by Turnbull-Dugarte's (2023) work.

[15]To avoid post-treatment bias, we solely rest the balancing on these socio-demographic variables, i.e., variables that we can safely assume are unaffected by the snap election call.

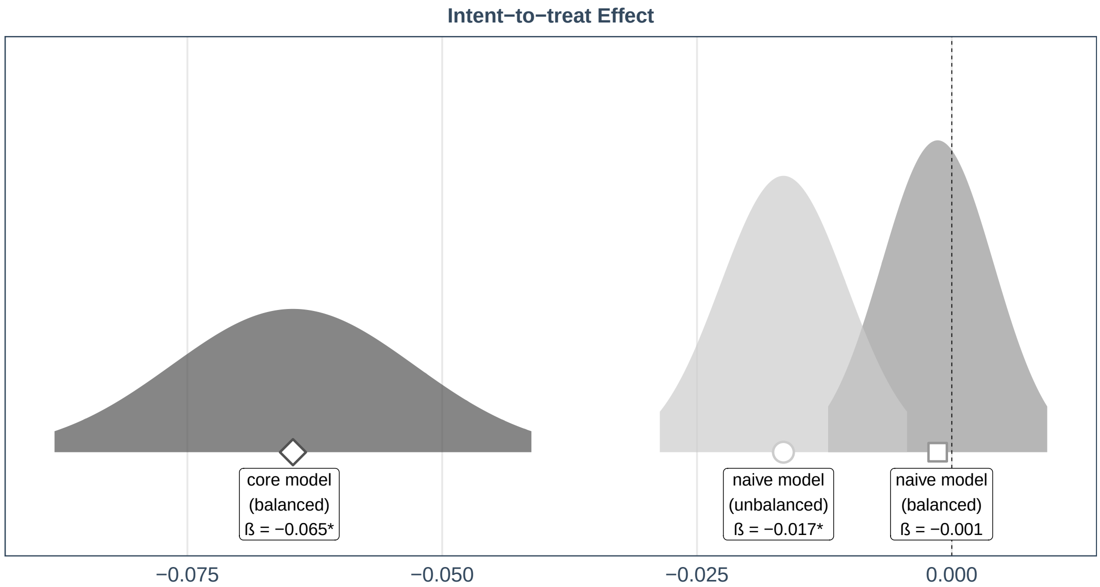

Figure 2.3: Estimates of the ITT effect of the snap election call on vote choice uncertainty based on naïve model (only including the treatment indicator as explanatory variable) in unbalanced and balanced form and our entropy-balanced core model including an interaction of the treatment with partisan strength. Shaded areas represent the 95% confidence intervals. A $p < 0.05$ is indicated by *. Full regression results are reported in Table 2.C.1 (entropy-balanced) and Table 2.C.2 (unbalanced) in the Appendix.

To assess whether the observed aggregate shift in uncertainty is shaped by subgroup-specific responses, particularly in line with our expectations regarding the moderating role of party identity, we estimate a third model that includes interaction terms between the treatment indicator and strength of party identity (Keele & Stevenson, 2021; Montgomery et al., 2018). We adopt this specification as our *core model* and use it as the basis for all subsequent analyses.[16] This specification reveals a substantial reduction in short-term vote intention uncertainty after the snap election call on non-partisans ($\beta = -0.065$, $SE = 0.012$). Yet the regression estimates show that the average treatment effect is indeed conditioned by partisan strength. This model offers a clear and nuanced understanding of how the snap election call affected the German electorate. Also, it implies that the decline in uncertainty is not uniform and thus varies across levels of partisan identity. We further address this heterogeneity in a following section.

[16]We obtain very similar results from a logistic regression (see Table 2.C.3 and Table 2.C.4 in the Appendix). Since cross-validation tests indicate no additional explanatory power from using a logit model, we use the OLS model as our specification for further analyses.

Table 2.C.1 and Table 2.C.2 in the Appendix report the full regression results of all models.[17]

In Figure 2.4, we zoom in on the ITT effect derived from our core model: the left panel displays the average difference in the predicted probability of reporting an uncertain vote intention between the control and treatment groups. On average, the likelihood of holding an uncertain vote intention is 6.4 percentage points lower among those surveyed after the snap election call compared to those interviewed beforehand. This constitutes a considerable effect size, particularly in light of the baseline probabilities of uncertainty: 27% in the control group versus 20.6% in the treated group. This corresponds to an approximate 24% relative reduction in the probability of being uncertain about one's vote intention following the snap election call, underscoring the substantial impact of the announcement on reducing vote intention uncertainty.

To assess whether this effect is unique to the 2024 snap election call or reflects a broader regularity across German electoral cycles, we conduct placebo tests using GLES panel survey waves from the 2009, 2013, 2017, and 2021 German federal elections (none of these was a snap election). For each wave, we focus on a time window approximately three months prior to election day, thereby mirroring the interval between the 2024 snap election call and the election day. A placebo treatment is assigned at the midpoint of each field period, which also corresponds to when the 2024 snap election call happened during the survey field time period. Results are summarized in the right panel of Figure 2.4. Across all placebo tests, we see no evidence of a comparable effect: ITT estimates are either statistically indistinguishable from zero or even show an increase in vote intention uncertainty.[18] These findings suggest that the observed reduction in vote intention uncertainty is not a fluke nor a general feature of German pre-electoral periods, but rather something deeply rooted in the snap election call of 2024. We now turn to provide more evidence on the robustness of this effect.

[17]There, we also include a model incorporating political variables as controls. While our results remain robust to their inclusion, we exclude political variables from the main specification to avoid post-treatment bias, as these may be directly affected by the snap election call. Additionally, although (strength of) party identity is relatively stable over time (Franklin & Jackson, 1983), it could be influenced by the treatment, as this was an extremely salient election announcement (Hernández et al., 2021). The interaction approach thus offers a more precise estimation to mitigate endogeneity and ensure a more valid inference as compared to, for instance, sub-setting on the variable (Keele & Stevenson, 2021; Montgomery et al., 2018).

[18]We also find no uncertainty-reducing placebo effects in GLES panel waves conducted during the year leading up to the 2024 snap election call (see Figure 2.D.1 in the Appendix).

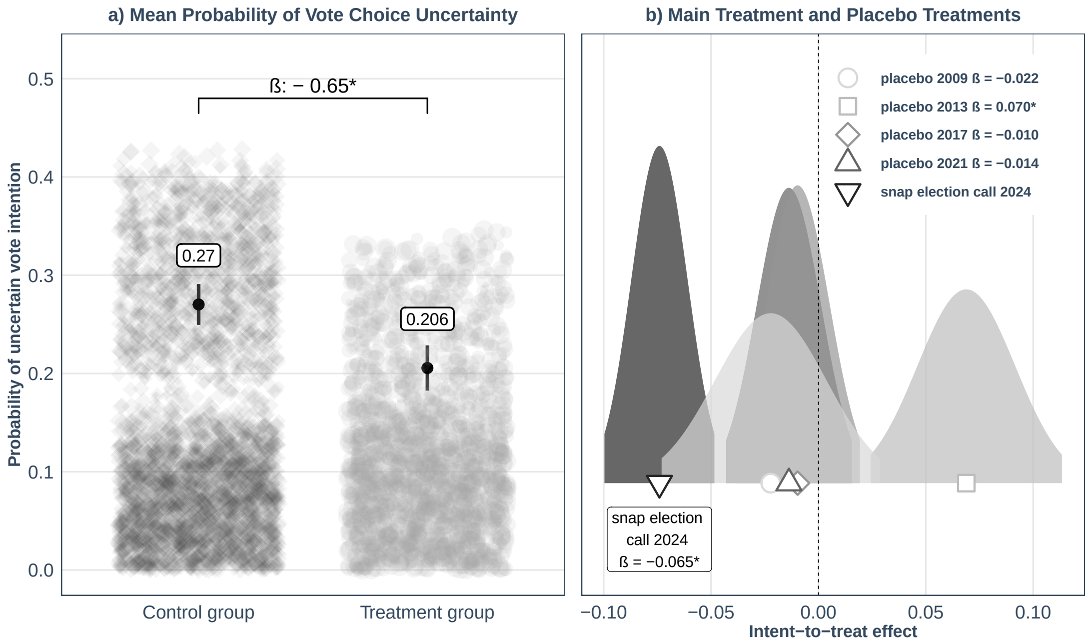

Figure 2.4: **Left:** Difference in mean predicted probability of vote choice uncertainty between the treatment and control group. Detailed results are reported in Table 2.C.1 in the Appendix. **Right:** Estimates of the ITT effect of the snap election call on vote intention uncertainty based on the core model specification. Additionally, placebo-treatment effects for previous German federal elections from 2009 to 2021 are displayed. Detailed results are reported in Table 2.C.5 in the Appendix. A $p < 0.05$ is indicated by *.

2.5.5 Sensitivity Tests Study 1: Germany

In order to demonstrate the rigour of these results and to ensure they are not biased by violations of the UESD assumptions, we deploy a range of sensitivity checks: Following the recommendations by Muñoz et al. (2020), we first test the sensitivity of our findings by narrowing the sample to respondents surveyed within different time windows around the treatment date. This shows whether the estimated ITT effect is robust or driven by specifics of early or late respondents. The left panel of Figure 2.5 shows the results based on our core model. Across the different bandwidths, the ITT effect remains both negative and statistically significant. This increases our confidence that the effect is not driven by specific characteristics of very early or late respondents. Moreover, the pattern is indicative that the uncertainty-reducing effect of the snap election call unfolded immediately after the announcement.

To delve further into the immediacy of the effect, we conduct inverse bandwidth reductions, gradually excluding respondents surveyed closest to the treatment date. As shown

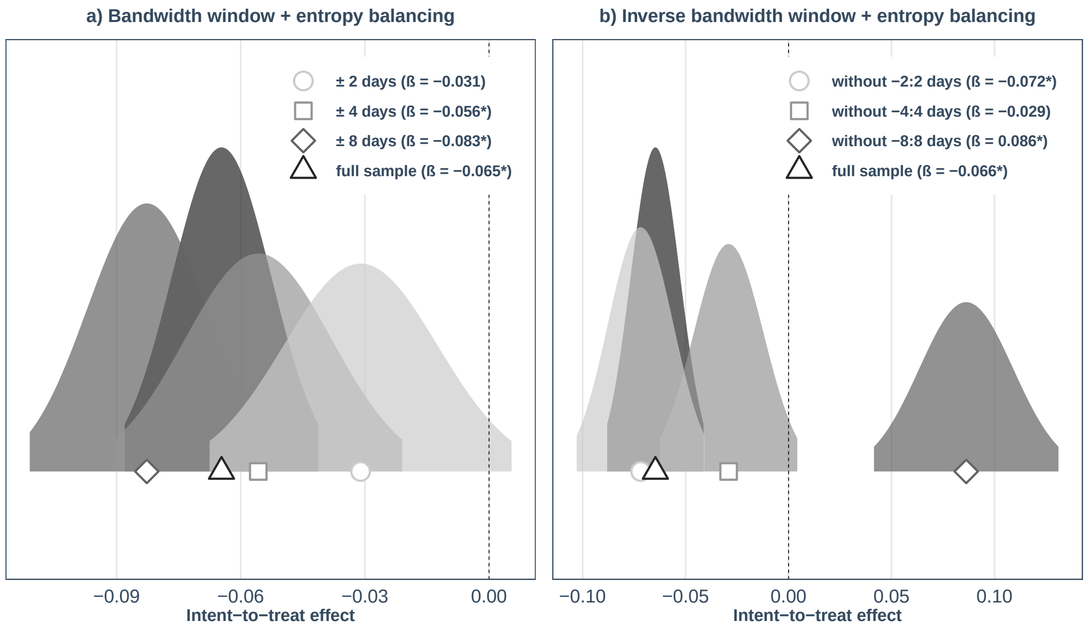

Figure 2.5: Sensitivity tests with a) stepwise sample restriction to different bandwidths of days around the snap election call and b) inverse bandwidth reductions restricting the sample towards the days at the beginning and end of the field time. A $p < 0.05$ is indicated by *. Detailed results are shown in Table 2.D.2 in the Appendix.

in the right panel of Figure 2.5, the negative effect persists until we exclude those surveyed within four days of the snap election call, but then it fades away. This further supports the notion that the effect materialised almost immediately following the snap election call and was primarily driven by respondents surveyed close to the event. An additional analysis of the effect's temporal dynamic confirms that it emerged rapidly and remained relatively stable during the whole post-treatment period (see Figure 2.C.1 in the Appendix).

Results for Subgroups Study 1: Germany

While we observe a reduction in vote intention uncertainty following the snap election call, this effect conceals important variation between subgroups. Specifically, we anticipated differences based on the strength of party identification. The results from our core model (Table 2.C.1) show that both weak and strong partisans report substantially lower levels of vote uncertainty to begin with (25 and 27 percentage points less than non-partisans, respectively). Moreover, the positive interaction coefficients for weak partisans and strong partisans indicate that the treatment effect is predominantly driven by non-partisans.

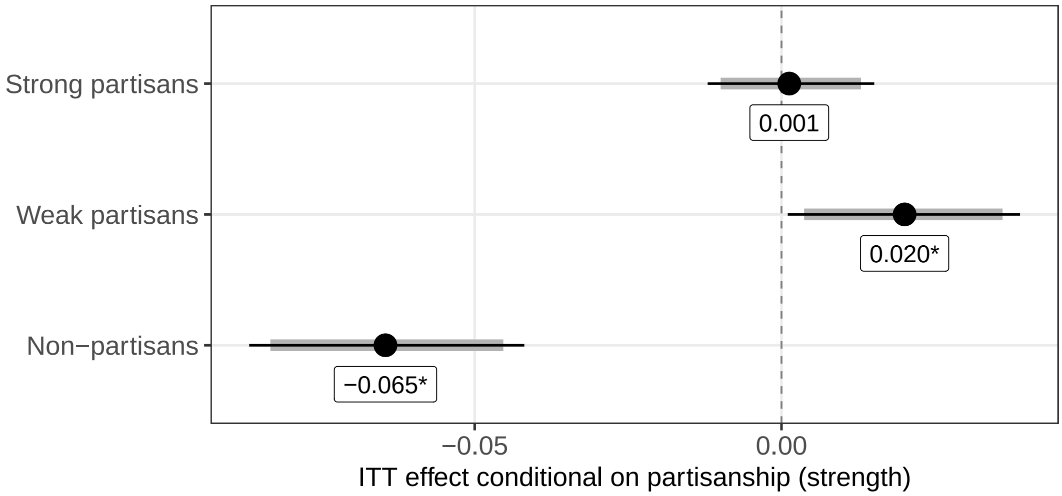

Figure 2.6: Subgroup-specific ITT effects on reported short-term vote intention uncertainty for non-partisans, weak partisans and strong partisans. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.C.1 in the Appendix.

In Figure 2.6, we present subgroup-specific ITT effects on short-term vote intention uncertainty derived from the interaction terms included in our core model. Among non-partisans, the reference category in the model, the treatment reduces the probability of expressing uncertain vote intentions by 6.5 percentage points ($SE = 0.012$, $p < 0.001$). In relative terms, and taking into account non-partisans' baseline likelihood of expressing vote intention uncertainty, this corresponds to a reduction of approximately 21%. For weak partisans, the interaction term is $\beta = 0.085$ ($SE = 0.015$, $p < 0.001$), which offsets the baseline effect and yields a net marginal effect of a 2 percentage points increase in vote intention uncertainty. For strong partisans, the interaction with the treatment of $\beta = 0.066$ ($SE = 0.014$, $p < 0.001$) results in a net marginal effect that is statistically indistinguishable from zero. The uncertainty-increasing effect or lack of a significant effect in these two groups implies that the substantial decline in short-term vote uncertainty after the snap call is concentrated almost exclusively among non-partisans. That is, the 6.4 percentage points drop in reported vote intention uncertainty observed at the aggregate level seems to be almost entirely driven by individuals without partisan attachment, whereas particularly the strong partisans remain virtually unaffected by the snap election call in that regard.

While in Figure 2.6 we do not take into account the *specific* party voters identify with, we acknowledge that perhaps one's partisan identification can also shape the observed effects. For instance, it may be relevant whether one identifies with one of the then governing "traffic light" coalition parties, as considerations of accountability and blame leading to the election call could play a role in vote intention formation. Similarly, it may make a difference whether someone has ties to a mainstream party or rather to one that is located at the ideological margins. We therefore estimate subgroup effects based on specific party identities; results are reported in Figure 2.E.1 and Table 2.E.1 in the Appendix. We do not find any significant difference conditional on party identification. Yet again, the results show that non-partisans are the only group that experiences a consistent reduction in uncertainty following the snap election call. This asymmetric responsiveness highlights the central role of the (lack of) partisan identity in structuring how citizens begin processing the sudden advancement of the electoral timeline.[19]

2.5.6 Exploratory Analyses Study 1: Germany

To understand better the drivers and nature of these subgroup effects, we also explore heterogeneity *within* the three subgroups. While these analyses are guided by our central expectation that political identity shapes the response to snap election calls, they remain exploratory in nature. Specifically, here we analyze variation within the three partisan strength groups by incorporating three additional moderators: incumbent support, political interest, and political media engagement. To do so, we extend our core model by estimating three-way interactions between treatment, partisan strength, and each of these variables. Details on the operationalization and regression results are reported in Figure 2.E.3 and Table 2.E.3 in the Appendix.

The general pattern of reduced vote intention uncertainty among non-partisans and unchanged levels among weak and strong partisans persists even when examining these groups in more detail. Within each subgroup, we observe limited additional variation.

[19]In a similar vein, we could expect that left-right ideology may moderate the treatment effect independently of party identity (or the absence thereof). To test this, we estimate ITT effects conditional on ideological self-placement using models that include three-way interactions with respondents' ideological position on the left-right dimension and strength of party identity (for details, see Figure 2.E.2 and Table 2.E.2 in the Appendix). Among strong partisans, we find no meaningful change in vote intention uncertainty across the ideological spectrum. However, among non-partisans (and even more so among weak partisans) uncertainty increases as individuals position themselves further to the right. These findings point to a more limited reduction in vote intention uncertainty among weak or non-partisans on the ideological right (as compared to those on the left), suggesting that the effect of this snap election call is not only shaped by partisan attachment but also on where individuals stand ideologically.

Across all non-partisan respondents, the uncertainty-reducing effect of the snap election call remains consistent. Only among those with high political media engagement is the effect non-distinguishable from zero. Weak partisans, by contrast, mostly exhibit no change in vote uncertainty. Exceptions are increased likelihoods of vote intention uncertainty among those who did not support the incumbent and who show low political interest. The latter we also find among strong partisans with low political interest. Apart from that, we do not find subgroup heterogeneity among strong partisans. This suggests that even among voters who hold an identity, a limited political engagement may weaken the role of partisan identity in the face of such an electoral disruption.[20] Taken together, these exploratory analyses reinforce the idea that the effect of the snap election call on vote intention uncertainty mainly varies by party identity or the lack of it, and only to a very limited extent by other dimensions of political engagement.

2.6 Study 2: The UK General Election 2024

To assess whether the effects observed above are unique to that particular case or do also emerge in other contexts, we now turn to investigate the 2024 UK general election.

2.6.1 Data

In order to study how the snap election call affected UK voters' short-term vote intention uncertainty, we use two closely timed waves of the British Election Study Internet Panel (BES, 2024), fielded immediately before and after Sunak's announcement on May 22nd. Wave 26 was in the field until the day of the snap election call, while the field for wave 27 began just two days later.[21] The one-day gap between waves offers an ideal setup for causal identification, as the panel data structure enables us to detect within-person change in vote intention uncertainty while minimizing the influence of collateral events that could confound this effect. Our sample consists of $N = 22,237$ respondents who completed both survey waves; the retention rate between wave 26 and wave 27 was 74%.

[20] To address concerns about sample imbalances during fieldwork, we also tested for heterogeneity by education, gender, and age in a similar manner. As shown in the Appendix (Figure 2.E.4, Table 2.E.4), conditional ITT effects remain small and are not statistically significant across these groups. This indicates that our main treatment effect is robust to demographic imbalances over the field period.

[21] Wave 26 was in field from May 3rd to May 22nd 2024. Wave 27 was fielded from May 24th to June 7th. All surveys were conducted as computer-assisted web interviews (CAWI) by YouGov using an online panel. Sampling quotas were employed to achieve representativeness of the UK general population. Daily participation trends are shown in Figure 2.F.1 in the Appendix.

2.6.2 Measures

In the UK case, we define May 22^{nd} as the *treatment*, for it marks the moment when then Prime Minister Rishi Sunak unexpectedly announced that a general election would be held on July 4^{th}, with Parliament dissolving on May 30^{th}(Zeffman, 2024).

As in the German case, we examine whether the effect of the snap election call in the UK is moderated by the strength of party identity. We adopt the same threefold categorization of non-partisans, weak partisans and strong partisans. To avoid any post-treatment bias in this grouping, we rely solely on party identity data from the pre-event wave. Respondents who indicate no party identity are defined as non-partisans. Those who consider their partisan strength as "not very strong" are categorized as weak partisans. Finally, we define those indicating a "fairly strong" or "very strong" party identity as strong partisans. Around 28% ($n = 5,769$) of respondents are categorized as non-partisans, 20% ($n = 4,257$) are categorized as weak partisans and, again comprising the largest group, 52% ($n = 10,812$) of respondents are considered strong partisans.[22]

2.6.3 Analytical Approach Study 2: UK

To account for the data's panel structure and trace within-person shifts in vote intention uncertainty, we estimate linear individual-level fixed-effects models using the pool of respondents who participated in both the pre-event (t_1) and post-event (t_2) waves.[23] This approach allows us to control for time-invariant individual characteristics (observed and unobserved) that might confound the relationship between the snap election call and shifts in vote intention uncertainty:

$$Y_{it} = \beta_1 Treatment_{it} + \gamma X_{it} + \alpha_i + \varepsilon_{it} \tag{2}$$

Y_{it} represents the likelihood to state DK in the vote intention question for person i at time t. $\beta_1 Treatment_t$ is the average effect of treatment reception. We define the treatment $T_t = 0$ for all observations from the pre-event wave and $T_t = 1$ for all post-event observations. γX_{it} is a vector of covariates that differs across our model specifications. The unobserved time-invariant individual effect is captured by α_i. In the results, we report estimates obtained from this OLS model; yet as in Study 1, we test all results against logistic regression models (see Table 2.G.1 in the Appendix).

[22]We do not explicitly include socio-demographic controls, as we already control for them through individual-level fixed-effects. We offer detail on this in the following subsection.

[23]We exclude the 44 respondents surveyed in the BESIP on May 22^{nd}.

2.6.4 Results Study 2: UK

We begin by examining whether the short-term vote intention uncertainty shifted from before to after the snap election call. As in Study 1, we estimate a core model that interacts the treatment indicator with the three defined levels of partisan strength: non-partisans, weak partisans, and strong partisans. The results of the individual-level fixed-effects model (see Table 2.G.1 in the Appendix) show that the snap election call in the UK also leads to a very sizable and statistically significant reduction in short-term vote intention uncertainty on the baseline (non-partisans) ($\beta = -0.042$, $SE = 0.005$).[24]

The estimates show that the snap election call had a clear uncertainty-reducing immediate effect across the electorate in the UK. On average, the probability of expressing vote intention uncertainty decreases from 17.3% prior to the announcement to 12.8% afterwards. The baseline of uncertainty here is lower than in Study 1, where it stood at around 27% before Scholz's snap call.[25] However, the absolute reduction of 4.5 percentage points in vote uncertainty here yields a relative reduction of 26%, a very similar relative difference to Study 1 (ca. 24%).

The temporal dynamics of the effect in the UK are also similar to the ones already observed in Germany: that is, the effect in the UK unfolded immediately after Sunak's snap call and remained very stable throughout the whole field period of the BESIP post-treatment wave (see Figure 2.G.1 in the Appendix). These dynamics are also reflected in Figure 2.7, where we can observe time trends of vote intention uncertainty for the entire sample as well as the three partisanship subgroups. In all four cases, we observe a sizable discontinuity by which the likelihood of respondents selecting DK in the vote intention question drops substantially after the snap election call. While there was a gradual trend of decreasing uncertainty for the whole sample, uncertainty levels were stable or, especially in the case of non-partisans, even increasing leading up to the call. The snap election call drastically changed the picture.

Considering our theoretical expectations and insights from Study 1, here we also explicitly address the variation of the treatment effect by different levels of strength of party

[24]This effect also holds when controlling for political variables potentially influenced by the announcement, such as political attention, government approval, or ideological self-placement. Further, see Table 2.G.1 in the Appendix for results from analogous logit models.

[25]This seems intuitive since UK's snap election call was semi-anticipated: unexpected on the date it was called, but expected during the year.

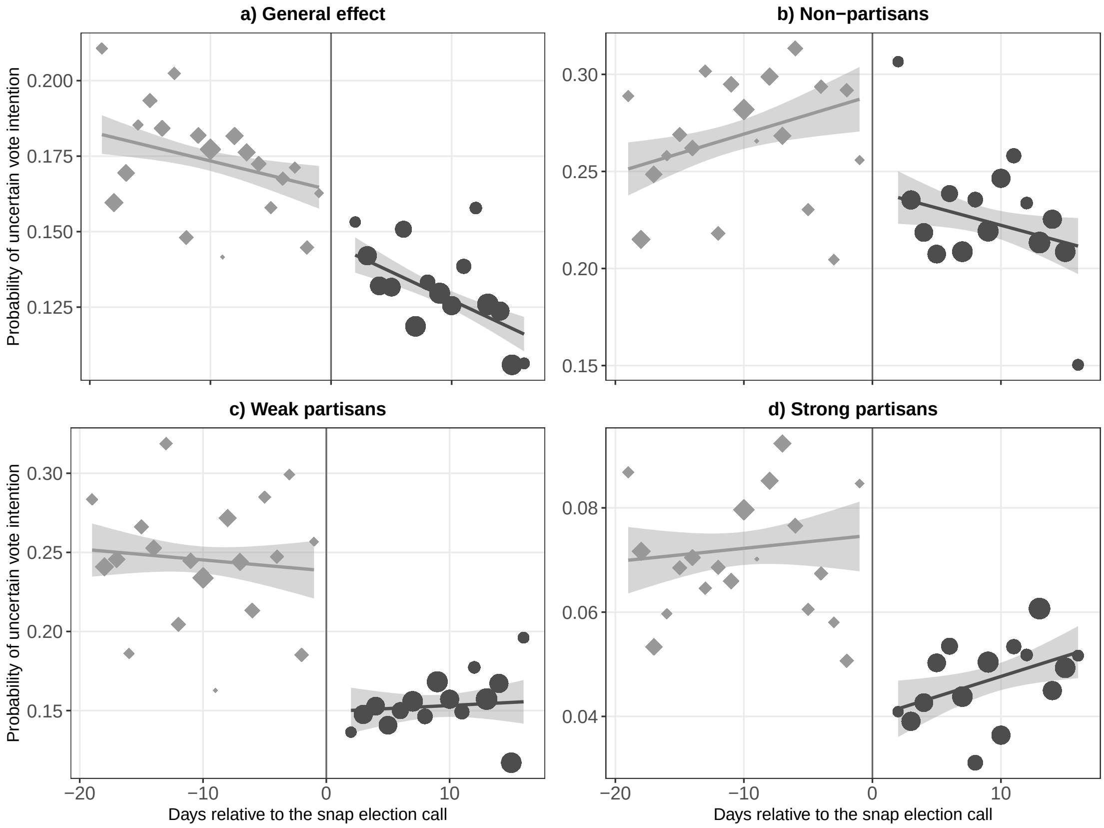

Figure 2.7: Trends in predicted probabilities of holding an uncertain vote intention before and after the snap election call for a) the general sample, b) non-partisans, c) weak partisans and d) strong partisans. Grey-shaded areas represent the 95% confidence intervals. Sizes of the dots vary relative to the number of survey respondents per day.

identity. Figure 2.8 presents the modeled coefficients stratified by these subgroups. The results from our core model in the left hand panel show that all three partisan groups experienced a reduction in short-term vote intention uncertainty following the snap election call. Among non-partisans, the likelihood of reporting an uncertain vote intention dropped by 4.2 percentage points. The effect was even stronger for weak partisans, who saw a 9.2 percentage points reduction ($\beta = -0.050$, $SE = 0.007$). Conversely, the positive interaction coefficient ($\beta = 0.018$, $SE = 0.006$) for strong partisans indicates that the aggregate effect is attenuated for these respondents, who present a 2.5 percentage point reduction in vote intention uncertainty between the two waves of BESIP. Crucially, when we consider each group's baseline uncertainty levels, it becomes even more clear that all groups were substantially affected by the early call of the general election: non-partisans' vote intention uncertainty fell from a pre-treatment level of 27% to a 22.3% after the call. That means a relative reduction in uncertainty of approx-

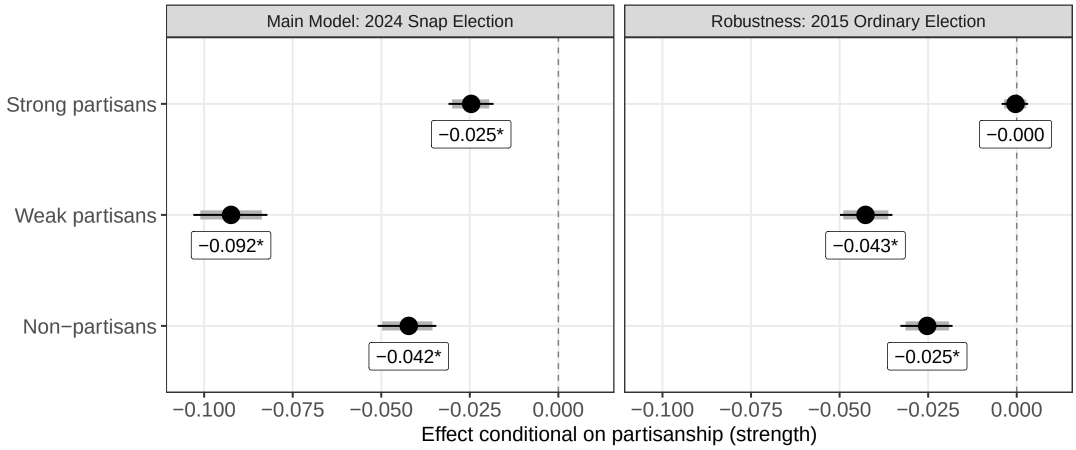

Figure 2.8: Subgroup-specific effects on reported short-term vote intention uncertainty for non-partisans, weak partisans and strong partisans for our core model and a robustness test for the 2015 UK general election. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.G.1.

imately 16%. While weak partisans had a 26% probability of holding an uncertain vote intention pre-treatment, this fell to 15% after it, constituting a relative reduction of 38%. Strong partisans' uncertainty probability shrunk from 7% before to around 5% after the call, implying a relative reduction of around 34%. These results show that even stable partisan groups were indeed responsive to the snap election call, depicting a broad and immediate effect across the three levels of partisan strength.

2.6.5 Robustness Test Study 2: UK

As in Study 1, we turn to examine whether this strong effect is specific to the 2024 snap election call or reflects a broader pre-election pattern that might also arise in the run-up to an ordinary UK election. To do so, we analyze the 2015 UK general election, the last contest held under the Fixed-term Parliaments Act (2011) and conducted on a scheduled timeline. We model changes in vote intention uncertainty between two BESIP waves fielded during that campaign (Wave 4: March 4^{th} to March 30^{th}; Wave 5: March 31^{st} to May 6^{th}.). The estimates, summarized in the right-hand panel of Figure 2.8, reveal that vote intention uncertainty declined across the electorate ahead of the 2015 election, though to a much lesser extent than after the 2024 snap election call.

Moreover, given that wave 5 of the BESIP ran until the day before election day, we also compute stepwise sample restrictions on this placebo in order to assess whether the effects observed in this election were gradual or rather concentrated close to election day, as it should be expected from textbook gradual campaign learning and other strategic considerations (Bunting, 2024; Claassen, 2011; Norris, 2006). Across restrictions displayed in Table 2.1, the treatment effect on non-partisans is small, and only significant excluding responses in the last week before the election. Weak partisans do exhibit lower uncertainty, but this effect also diminishes as we restrict the sample to observations closer to election day, consistent with well-established gradual learning during a standard campaign rather than accelerated decision-making. Strong partisans, if anything, become slightly more uncertain late in the campaign, which is the opposite of the pattern observed in the snap election call. We offer some plausible explanations of these patterns, other inferences and limitations in the Discussion section. However, taken together, these tests provide more evidence that the sharp reductions in uncertainty displayed in our main analyses seem to be tied to elements specifically rooted in the snap election call.

Table 2.1: Results of linear individual-level fixed effects regressions for the 2015 UK general election placebo test with stepwise sample restrictions to exclude observations closer to election day.

	Dependent variable:		
	Uncertain vote intention		
	excluding 30 days pre-election	excluding 15 days pre-election	excluding 7 days pre-election
Treatment (baseline: non-partisans)	−0.002	−0.004	-0.017^{***}
	(0.007)	(0.005)	(0.004)
Treatment × Weak partisanship	-0.028^{***}	-0.027^{***}	-0.017^{***}
	(0.010)	(0.007)	(0.006)
Treatment × Strong partisanship	0.003	0.007	0.018^{***}
	(0.008)	(0.005)	(0.005)
Observations	14,082	32,972	44,852
R^2	0.003	0.003	0.004
Adjusted R^2	−0.995	−0.995	−0.993
Note:			*p<0.1; **p<0.05; ***p<0.01

2.6.6 Exploratory Analyses Study 2: UK

To obtain a more nuanced insight into the nature of this effect and for consistency with the German case, we explore additional moderators and subgroup heterogeneity *within* the three partisan groups.

First, and as already outlined in Study 1, we test how one's substantive partisan identification moderates the observed effects. Detailed results are shown in Figure 2.H.1 and Table 2.H.1 in the Appendix. We find that the greatest reduction in vote intention uncertainty by specific party identification is on voters who identify as Conservatives. By contrast, the reduction among voters who identify as Labour is still significant but way smaller, perhaps because many of those voters were already mobilized considering their standings in pre-election polls. As in Study 1, we also observe that lacking a specific party identity is a very strong predictor of a reduction in vote uncertainty once the election is called.[26]

To obtain a more nuanced insight into the nature of this effect and for consistency with the German case, we also explore how incumbent support, political interest, and political media engagement moderate the impact of the UK snap election call across levels of partisan strength. We report detailed results in Figure 2.H.3 and Table 2.H.3 in the Appendix.

The effect of the snap election call on short-term vote intention uncertainty is consistently negative in all subgroups. Across all three groups of partisan identity, we observe some differences by different levels of incumbent support, but barely anything relevant by political interest or political media engagement. Notably, individuals who supported the incumbent in the 2019 election show greater reductions in vote uncertainty. Weak partisans who reported voting for the incumbent in the 2019 election show a notably greater reduction in vote uncertainty than those who supported another party. A similar pattern emerges among strong partisans: incumbent supporters exhibit a much greater uncertainty reduction than their counterparts aligned with challenger parties. Although this is beyond the scope of our analyses, we speculate that this may hint at a mobiliza-

[26]Additionally, we again extend the analysis by incorporating respondents' ideological self-placement. In Figure 2.H.2 and Table 2.H.2 in the Appendix, we present subgroup-specific results by partisan strength across the left-right dimension. Among weak and strong partisans, right-leaning individuals exhibit a greater reduction in short-term vote intention uncertainty. This pattern does not hold for non-partisans, suggesting that in this case left-right placement does indeed exert an influence; it is however subject to partisan moderation.

tion effect among conservative voters immediately after the snap election call by their party in a moment where Labour led the polls by a great margin. Our results are perhaps unsurprising given that 2019 preferences on Brexit continue to shape party support, even though Brexit itself is a decreasingly salient issue (Griffiths et al., 2025; Prosser, 2025).

While political interest does not substantially drive the effect within partisan groups, lower political media engagement is associated with greater vote uncertainty reductions among the two partisan subgroups. For non-partisans, however, political media engagement appears to play a less influential role. Taken together, these analyses highlight that the effect of the snap election call on vote intention uncertainty mainly varies by strength of party identity or the absence of it, and to a much lesser extent by other political variables.[27]

2.7 Discussion and Conclusion

Snap elections have become an increasingly common feature of electoral cycles in democracies, especially in recent years across Europe. This paper examined how snap election calls affect vote intention uncertainty, estimated through DK responses in large public opinion surveys. Leveraging the UESD framework on an appropriate wave of the GLES panel, we analyzed the unexpected call for the German snap election of 2025, and using a within-subjects panel design on BESIP data, the semi-anticipated UK snap election call of 2024. In both cases, we demonstrate that the snap election calls led to a remarkable reduction in vote intention uncertainty, as voters became way more likely to express a preference.

In Germany, the relative reduction we observe in uncertainty reaches up to 24%, while it is around 26% in the UK.[28] We also find that strength of party identity explains much of the variation between voters. In Germany, the reduction in vote intention uncertainty is driven primarily by non-partisans, with weak and strong partisans largely unaffected. In contrast, the UK case shows declines across all three subgroups, with weak partisans experiencing the largest relative reduction. Notably, and contrary to both our expectations and the findings from Germany, here we find that partisans also show a meaningful

[27] Yet again, we test for heterogeneity across socio-demographic subgroups. Results are shown in Figure 2.H.4 and Table 2.H.4 in the Appendix.

[28] As we noted in the subsection *Outcome*, the use of DK as our dependent variable likely underestimates the full extent of vote intention uncertainty. Therefore, our estimates of how snap election calls affect short-term voter uncertainty are likely to be conservative.

uncertainty decrease. Our rationale was grounded in prior research showing that partisans tend to hold relatively stable vote intentions (Bunting, 2024; Willocq, 2019), and thus should be minimally affected by a snap election call. However, the findings from the UK challenge the consistency of this expectation. Contextual factors such as the incumbent Conservative Party's low favorability, Labour's strong lead in the polls, tactical voting in certain constituencies or relatedly, the majoritarian electoral system in the UK may help to account for this divergence (Bunting, 2024; Smith, 2024).

Exploratory analyses show that ideology, incumbent support, political interest, and political media engagement fail to consistently explain the observed reductions in vote intention uncertainty across countries. Rather, these seem to be subject to context-specific dynamics. In regards to the temporal dynamic of the observed effects, we also find consistent results across our two studies: the uncertainty-reducing effect in vote intention is mainly driven by the shock of the treatment and remains relatively stable over the field periods of both GLES and BESIP surveys.

The placebo tests based on previous scheduled ordinary elections in both studies leave us confident that ordinary campaigns do not generate the same sharp reductions in vote intention uncertainty that follow snap election calls. In Germany, none of the placebo tests for the 2009–2021 federal elections show any uncertainty-reducing effects. In the UK, by contrast, we do observe an uncertainty-reducing effect in the 2015 general election, though it is much smaller than that following the 2024 snap election call. Because the BESIP data for this placebo were collected over an extended campaign period almost reaching election day, this test effectively captures gradual campaign learning rather than abrupt responses to a new electoral timeline. The lack of such substantial reductions of uncertainty in these placebo tests gives strong support to our interpretation that the uncertainty reduction observed after snap election calls stems from something specifically tied to the institution of a snap election call. At the same time, we are cautious not to claim that the placebo tests conclusively demonstrate timing compression as the sole mechanism, as we do not have ideal placebo-case data to analyze everything with perfectly comparable timing granularity.

Yet to sum up, the causal identification strategies employed, the various robustness checks and the consistency of the strong effects across studies leave us very confident that these shifts are driven by the snap election calls themselves. However, the various differences between contexts and differences in subgroup and exploratory analyses are indicative that each case is also shaped by its own particular dynamics, probably both structural and contingent, as suggested by the analyses of party identification and incumbent support.

Our findings suggest that snap election calls are highly consequential political events that prompt individuals to consider options rapidly, and to form and express their preferences. Snap election calls therefore function as strong and consistent informational cues that enable a remarkable reduction in vote intention uncertainty. This is crucial for various reasons: first, because it demonstrates that some voters can make up their minds in a remarkable short time. Naturally, they may later go on to revise their preferences (potentially multiple times) during the pre-campaign or campaign period, or decide not to vote at all (see Bunting, 2024). Yet it is essential to distinguish these subsequent (campaign-rooted) changes from the initial structuring effect we identify in this paper: later shifts in voting intentions or turnout would arise from distinct dynamics linked to pre-campaign or campaign phenomena, with a new baseline level of uncertainty in the electorate defined by the snap election call itself.

Further, by altering the electoral calendar, an early call for elections can plausibly affect many variables; electoral turnout, polling, preference stability, or, from a party perspective, campaign strategies. These are outcomes of both normative importance (e.g., accountability) and practical relevance (e.g., polling dynamics and campaigning). Moreover, the uncertainty-reducing effect we find is far from negligible, and the magnitudes observed across our two cases are large enough to matter for election margins in many races and for seat outcomes under various electoral systems, particularly in the majoritarian system of the UK (see Crosson & Tsebelis, 2022).

We believe that future research could expand our contribution by addressing some of our limitations. The main constraint we encountered is data availability. However, given the increasing frequency of snap elections, we remain optimistic that future cases will offer suitable data for further nuance. Broadening the scope with more studies and exploring the effect of snap election calls across different institutional and political contexts would help to gauge better the generalizability of our results. Follow-up research incorporating more extensive cross-national comparisons could also provide a more comprehensive picture of how different voters react to these electoral advances.

Another limitation of our work is the impossibility to disentangle the psychological mechanisms driving the observed results. Future research could address this by drawing on alternative data sources and experimental designs that help to provide a more fine-grained understanding of the underlying motivations and psychological mechanisms at play. Additionally, other investigations could explore whether similar patterns arise in response to other events with the potential to reduce uncertainty beyond snap election calls. For example, the announcement of a major pre-electoral coalition agreement may offer a comparable cue to voters (see Barnfield et al., 2025).

While we provide evidence on the durability of the uncertainty-reducing effect triggered by the snap election call, thereby contributing to the literature on the timing of vote preference formation (see Box-Steffensmeier et al., 2015; Bunting, 2024; Willocq, 2019), we recognize that further research can help to deepen on the duration of this effect and to what extent it is offset by subsequent developments during the pre-campaign and campaign periods. Moreover, studies could also examine how these changes in vote intention uncertainty relate to eventual turnout. Given that non-partisans consistently exhibit a strong response to snap election calls, and that prior research associates the lack of party identity with greater electoral volatility and lower turnout (Bunting, 2024; Gunderson, 2025), it is essential to assess to what extent the observed reductions in uncertainty hold and eventually translate into casting concrete votes.

Addressing these gaps will allow our methodological contribution and valuable comparative insights into snap election calls to fit into a deeper and broader account of snap elections and vote choice formation.

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Appendix

2.A Overview of Snap Elections

Table 2.A.1: Overview of snap elections in European countries 2010-2025. Data source: electionguide.org and own elaboration.

Country	**Snap Election**
Austria	2019 (National Council)
Belarus	2019 (Chamber of Representatives), 2025 (Presidency)
Belgium	2010 (Senate & Chamber of Representatives)
Bulgaria	2021 (2x, National Assembly), 2022 (NA), 2023 (NA), 2024 (2x, NA)
Croatia	2024 (Assembly)
Denmark	2011 (Parliament), 2022 (Parliament)
France	2024 (National Assembly)
Germany	2025 (Federal Parliament)
Greece	2012 (Parliament), 2023 (Parliament)
Iceland	2017 (Parliament)
Ireland	2011 (Dáil), 2024 (Dáil)
Italy	2022 (Chamber of Deputies & Senate)
Latvia	2011 (Parliament)
Moldova	2021 (Parliament)
Montenegro	2023 (Assembly)
Netherlands	2010 (Second Chamber), 2023 (Second Chamber), 2025 (Second Chamber)
North Macedonia	2011 (Assembly)
Poland	2010 (Presidency)
Portugal	2011 (Assembly), 2022 (Assembly), 2024 (Assembly), 2025 (Assembly)
Serbia	2022 (National Assembly), 2023 (National Assembly)
Slovakia	2023 (National Assembly)
Slovenia	2011 (National Assembly), 2018 (National Assembly)
Spain	2019 (2x, Senate & Congress of Deputies), 2023 (Senate& Congress of Deputies)
Turkey	2015 (National Assembly, 2023 (National Assembly & Presidency)
Ukraine	2019 (Supreme Council)
United Kingdom	2017 (House of Commons), 2019 (House of Commons), 2024 (House of Commons)

Note: We explored data availability for all snap elections listed in the table above. However, aside from the two cases we analyze, none proved suitable. Common limitations included inadequate fieldwork timing (e.g., LISS panel for the Netherlands in 2023 or 2025), insufficient statistical power for UESD analysis (e.g., Wave 10 of the Austrian National Election Study for the 2019 election), panel waves spaced too far from the snap election call (e.g., British Election Study for the 2019 UK election or waves from CEVIPOF's Enquête Électorale Française 2023), missing vote intention items (e.g., European Social Survey or Eurobarometer), and confounding events (e.g., Spain's 2023 general election was called the day after local and regional elections).

2.B Descriptives Study 1 (Germany)

Figure 2.B.1 shows the distribution of respondents across the survey field time of the GLES panel refresher wave of October/November 2024. There were more daily participants prior to the snap election call as compared to afterwards. Pre-event participants, i.e. those constituting the control group, sum up to $n = 6,043$. With high participation on the day after the snap election call, the control group subsumes $n = 2,976$ respondents, leaving us with sufficient group sizes to employ statistically powerful analyses.

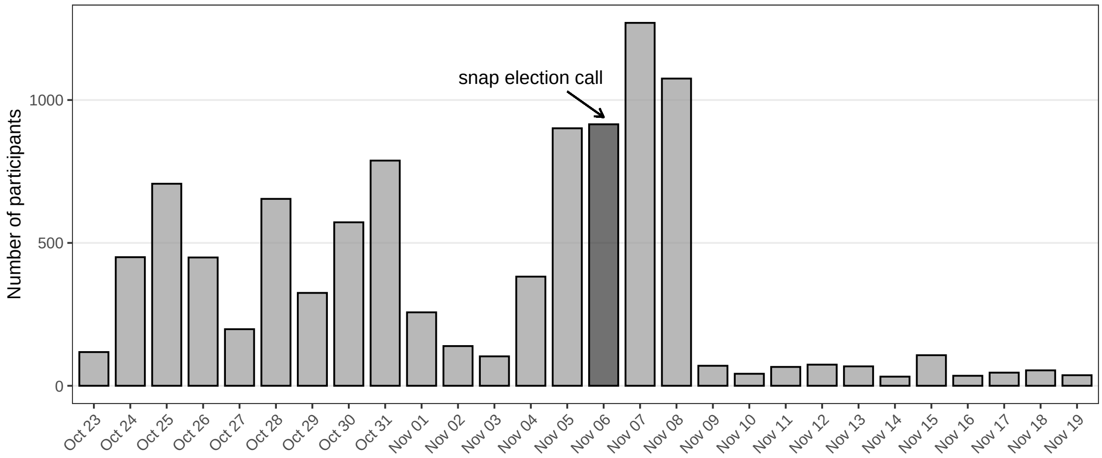

Figure 2.B.1: Number of participants per day in the GLES refresher survey wave 2024

Table 2.B.1: Comparison of means between control and treatment group

Variable	Mean (Control)	Mean (Treatment)	T-stat.
Female	0.57	0.46	9.92*
Age	46.35	44.91	4.491*
Education low	0.32	0.18	14.82*
Education intermediate	0.43	0.29	13.41*
Education high	0.25	0.50	−23.59*
Left-right self-assessment	0.48	0.48	0.211
Political interest	0.58	0.64	−10.03*
Democratic satisfaction	0.46	0.48	−3.17*
Observations	6043	2976	
Note:			*p<0.5

2.C Regression Results Study 1 (Germany)

Table 2.C.1: OLS regression results - entropy-balanced

	Dependent variable:					
	Uncertain vote intention					
	(1)	(2)	(3)	(core model)	(5)	(DK + nonresp.)
Treatment	−0.001	−0.002	−0.001	−0.065***	−0.070***	−0.065***
	(0.005)	(0.005)	(0.005)	(0.012)	(0.012)	(0.013)
Weak partisanship				−0.246***	−0.207***	−0.239***
				(0.011)	(0.011)	(0.012)
Strong partisanship				−0.270***	−0.219***	−0.268***
				(0.010)	(0.010)	(0.011)
Female		0.056***	0.022***	0.030***	0.016***	0.027***
		(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age		0.0003	0.0003*	0.001***	0.001***	0.001***
		(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Education interm.		−0.018**	−0.005	−0.018**	−0.010	−0.021***
		(0.008)	(0.008)	(0.007)	(0.007)	(0.008)
Education high		−0.041***	−0.004	−0.028***	−0.008	−0.031***
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Left-right			−0.012		0.003	
			(0.012)		(0.011)	
Political interest			−0.138***		−0.081***	
			(0.011)		(0.011)	
Democratic sat.			−0.001		0.017*	
			(0.009)		(0.009)	
Treat × Weak PID				0.085***	0.093***	0.081***
				(0.015)	(0.015)	(0.016)
Treat × Strong PID				0.066***	0.073***	0.065***
				(0.014)	(0.014)	(0.015)
Constant	0.068***	0.056***	0.132***	0.262***	0.255***	0.273***
	(0.004)	(0.011)	(0.015)	(0.013)	(0.016)	(0.014)
Observations	8,334	8,334	7,778	8,287	7,743	8,358
R^2	0.00001	0.017	0.030	0.140	0.112	0.124
Adjusted R^2	−0.0001	0.016	0.029	0.139	0.110	0.124
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 2.C.2: OLS regression results - unbalanced

	Dependent variable:					
	Uncertain vote intention					
	(1)	(2)	(3)	(core model)	(5)	(DK + nonresp.)
Treatment	-0.017^{***}	-0.002	-0.002	-0.074^{***}	-0.078^{***}	-0.076^{***}
	(0.006)	(0.006)	(0.006)	(0.013)	(0.013)	(0.014)
Weak partisanship				-0.250^{***}	-0.209^{***}	-0.246^{***}
				(0.010)	(0.010)	(0.010)
Strong partisanship				-0.288^{***}	-0.233^{***}	-0.285^{***}
				(0.009)	(0.009)	(0.009)
Female		0.060^{***}	0.024^{***}	0.029^{***}	0.016^{***}	0.025^{***}
		(0.006)	(0.006)	(0.006)	(0.005)	(0.006)
Age		0.0003	0.0003^{*}	0.001^{***}	0.001^{***}	0.001^{***}
		(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Education interm.		-0.020^{***}	-0.006	-0.021^{***}	-0.012^{*}	-0.024^{***}
		(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Education high		-0.043^{***}	-0.006	-0.028^{***}	-0.010	-0.032^{***}
		(0.008)	(0.007)	(0.007)	(0.007)	(0.008)
Left-right			-0.020		-0.002	
			(0.012)		(0.012)	
Political interest			-0.146^{***}		-0.075^{***}	
			(0.012)		(0.011)	
Democratic sat.			0.005		0.019^{**}	
			(0.010)		(0.010)	
Treat × Weak PID				0.089^{***}	0.095^{***}	0.088^{***}
				(0.017)	(0.016)	(0.018)
Treat × Strong PID				0.084^{***}	0.086^{***}	0.082^{***}
				(0.015)	(0.015)	(0.016)
Constant	0.083^{***}	0.055^{***}	0.138^{***}	0.268^{***}	0.260^{***}	0.284^{***}
	(0.004)	(0.012)	(0.015)	(0.013)	(0.016)	(0.014)
Observations	8,334	8,334	7,778	8,287	7,743	8,358
R^2	0.001	0.017	0.032	0.157	0.127	0.140
Adjusted R^2	0.001	0.017	0.031	0.156	0.126	0.140
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 2.C.3: Logistic regression results - entropy-balanced

	Dependent variable:				
	Uncertain vote intention				
	(1)	(2)	(3)	(4)	(5)
Treatment	−0.022	−0.006	0.014	−0.322**	−0.389**
	(0.107)	(0.108)	(0.125)	(0.147)	(0.175)
Weak partisanship				−2.267***	−2.166***
				(0.217)	(0.243)
Strong partisanship				−3.458***	−3.105***
				(0.260)	(0.279)
Female		0.925***	0.486***	0.591***	0.368***
		(0.113)	(0.135)	(0.122)	(0.142)
Age		0.005	0.008*	0.013***	0.015***
		(0.004)	(0.004)	(0.004)	(0.005)
Education interm.		−0.225	−0.062	−0.266*	−0.187
		(0.143)	(0.175)	(0.156)	(0.186)
Education high		−0.611***	−0.049	−0.474***	−0.138
		(0.132)	(0.167)	(0.144)	(0.176)
Left-right			−0.345		−0.069
			(0.310)		(0.374)
Political interest			−2.592***		−1.682***
			(0.265)		(0.290)
Democratic sat.			−0.040		0.289
			(0.252)		(0.277)
Treatment × Weak partisanship				0.779***	0.946***
				(0.286)	(0.317)
Treatment × Strong partisanship				0.559	0.668*
				(0.352)	(0.374)
Constant	−2.620***	−2.996***	−1.777***	−1.556***	−1.137***
	(0.075)	(0.220)	(0.345)	(0.248)	(0.373)
Observations	8,334	8,334	7,778	8,287	7,743
Log Likelihood	−991.209	−960.346	−750.253	−773.424	−646.327
Akaike Inf. Crit.	1,986.418	1,932.693	1,518.506	1,566.847	1,318.655

Note: *p<0.1; **p<0.05; ***p<0.01

Table 2.C.4: Logistic regression results - unbalanced

	Dependent variable:				
	Uncertain vote intention				
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.240^{***}	-0.013	0.001	-0.270^{**}	-0.343^{**}
	(0.090)	(0.095)	(0.112)	(0.129)	(0.155)
Weak partisanship				-2.059^{***}	-1.952^{***}
				(0.136)	(0.154)
Strong partisanship				-3.510^{***}	-3.179^{***}
				(0.182)	(0.199)
Female		0.908^{***}	0.496^{***}	0.543^{***}	0.366^{***}
		(0.092)	(0.110)	(0.099)	(0.116)
Age		0.004	0.008^{**}	0.013^{***}	0.014^{***}
		(0.003)	(0.003)	(0.003)	(0.004)
Education interm.		-0.246^{**}	-0.062	-0.307^{***}	-0.206
		(0.097)	(0.118)	(0.107)	(0.127)
Education high		-0.623^{***}	-0.106	-0.473^{***}	-0.184
		(0.112)	(0.136)	(0.122)	(0.146)
Left-right			-0.471^{*}		-0.196
			(0.244)		(0.298)
Political interest			-2.485^{***}		-1.447^{***}
			(0.205)		(0.227)
Democratic sat.			0.099		0.356
			(0.197)		(0.220)
Treatment $\times$ Weak partisanship				0.568^{**}	0.722^{***}
				(0.231)	(0.256)
Treatment $\times$ Strong partisanship				0.606^{**}	0.713^{**}
				(0.300)	(0.320)
Constant	-2.403^{***}	-2.961^{***}	-1.785^{***}	-1.586^{***}	-1.217^{***}
	(0.049)	(0.175)	(0.271)	(0.197)	(0.293)
Observations	8,334	8,334	7,778	8,287	7,743
Log Likelihood	$-2{,}266.183$	$-2{,}195.350$	$-1{,}644.024$	$-1{,}715.932$	$-1{,}372.683$
Akaike Inf. Crit.	4,536.365	4,402.700	3,306.048	3,451.864	2,771.367
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 2.C.5: Linear regression results - placebo models for the 2009, 2013, 2017 and 2021 German federal elections

	Dependent variable:			
	Uncertain vote intention			
	(2009)	(2013)	(2017)	(2021)
Treatment	−0.022	0.069***	−0.010	−0.014
	(0.026)	(0.023)	(0.015)	(0.015)
Weak partisanship	−0.248***	−0.236***	−0.322***	−0.316***
	(0.026)	(0.022)	(0.014)	(0.014)
Strong partisanship	−0.370***	−0.411***	−0.453***	−0.383***
	(0.024)	(0.020)	(0.013)	(0.013)
Female	0.054***	0.083***	0.052***	0.063***
	(0.015)	(0.011)	(0.007)	(0.007)
Age	0.001	−0.001	−0.0001	0.0002
	(0.001)	(0.0004)	(0.0003)	(0.0003)
Education intermediate	−0.022	0.010	−0.013	0.025***
	(0.022)	(0.018)	(0.009)	(0.009)
Education high	−0.039*	0.007	−0.023***	0.010
	(0.022)	(0.017)	(0.009)	(0.008)
Treatment × Weak partisanship	0.034	−0.065**	0.045**	0.034*
	(0.037)	(0.031)	(0.019)	(0.020)
Treatment × Strong partisanship	0.010	−0.072***	0.026	0.025
	(0.033)	(0.028)	(0.018)	(0.018)
Constant	0.441***	0.490***	0.490***	0.355***
	(0.037)	(0.028)	(0.017)	(0.018)
Observations	3,270	4,755	10,568	7,650
R^2	0.138	0.196	0.201	0.199
Adjusted R^2	0.136	0.195	0.200	0.198
Note:				*p<0.1; **p<0.05; ***p<0.01

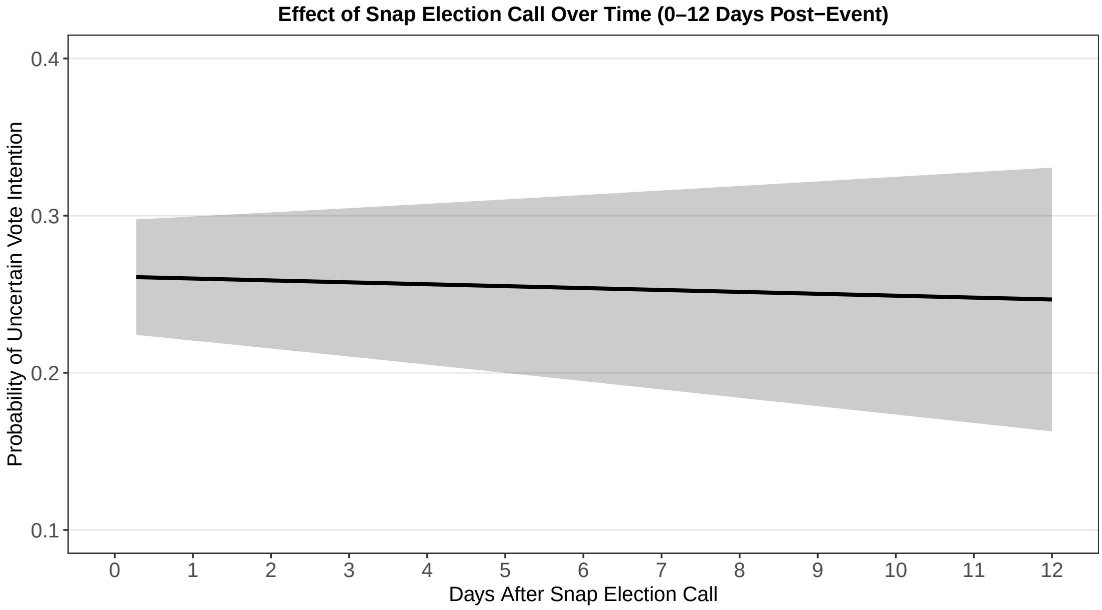

Figure 2.C.1: Time trends in the likelihood of holding an uncertain vote intention after the snap election call. Estimates are based on the core model including an interaction of the treatment variable with time and partisan strength. Grey-shaded area represents the 95% confidence interval.

2.D Sensitivity Tests Study 1 (Germany)

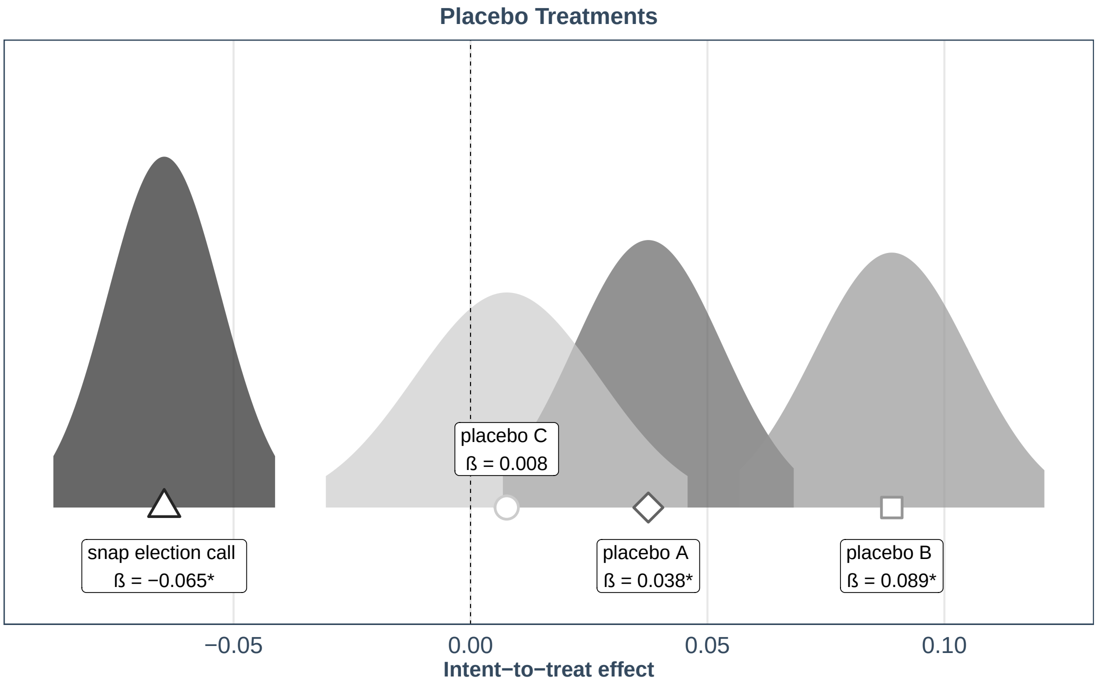

Figure 2.D.1: Sensitivity tests with placebo treatments assigned to three alternative GLES survey waves. Placebo A: October 2023, placebo B: June 2024, placebo C: September/October 2024. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.D.1.

Table 2.D.1: Placebo tests for three alternative GLES waves conducted in the year before the 2024 snap election call

	Dependent variable:		
	Uncertain vote intention		
	(A: October 2023)	(B: June 2024)	(C: Sept./Oct. 2024)
Treatment	0.038**	0.089***	0.008
	(0.016)	(0.016)	(0.019)
Weak partisanship	−0.294***	−0.216***	−0.334***
	(0.014)	(0.014)	(0.018)
Strong partisanship	−0.339***	−0.237***	−0.383***
	(0.013)	(0.013)	(0.017)
Female	0.035***	0.043***	0.050***
	(0.007)	(0.008)	(0.009)
Age	0.0001	−0.001**	0.0003
	(0.0003)	(0.0003)	(0.0004)
Education intermediate	−0.012	−0.010	−0.001
	(0.009)	(0.009)	(0.012)
Education high	0.010	−0.034***	0.016
	(0.008)	(0.009)	(0.011)
Treatment × Weak partisanship	−0.062***	−0.053**	0.047*
	(0.020)	(0.021)	(0.025)
Treatment × Strong partisanship	−0.043**	−0.087***	0.022
	(0.019)	(0.020)	(0.024)
Constant	0.324***	0.274***	0.350***
	(0.020)	(0.020)	(0.025)
Observations	5,144	4,565	4,435
R^2	0.242	0.176	0.197
Adjusted R^2	0.241	0.175	0.195
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 2.D.2: OLS regression results with bandwidth reduction (models 1-4) and inverse bandwidth reduction (models 5-7) and an entropy-balanced control group

	Dependent variable: Uncertain vote intention						
	Different bandwidths + entropy balancing				Different inverse bandwidths + entropy balancing		
	full sample	± 8 days	± 4 days	± 2 days	w/o ± 8 days	w/o ± 4 days	w/o ± 2 days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	−0.065***	−0.083***	−0.056***	−0.031*	0.086***	−0.029*	−0.072***
	(0.012)	(0.014)	(0.018)	(0.019)	(0.023)	(0.017)	(0.016)
Weak partisanship	−0.246***	−0.241***	−0.221***	−0.208***	−0.249***	−0.255***	−0.255***
	(0.011)	(0.013)	(0.016)	(0.017)	(0.022)	(0.016)	(0.015)
Strong partisanship	−0.270***	−0.268***	−0.243***	−0.225***	−0.263***	−0.291***	−0.295***
	(0.010)	(0.012)	(0.015)	(0.016)	(0.020)	(0.015)	(0.014)
Female	0.030***	0.034***	0.037***	0.032***	0.034***	0.025***	0.023***
	(0.005)	(0.006)	(0.008)	(0.008)	(0.011)	(0.008)	(0.008)
Age	0.001***	0.001***	0.001***	0.001***	0.00000	−0.0003	−0.0001
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0003)
Education intermediate	−0.018**	−0.021**	−0.024*	−0.055***	−0.009	−0.038***	−0.033***
	(0.007)	(0.010)	(0.015)	(0.018)	(0.015)	(0.011)	(0.009)
Education high	−0.028***	−0.030***	−0.029**	−0.056***	−0.026	−0.040	−0.031
	(0.007)	(0.009)	(0.014)	(0.017)	(0.028)	(0.026)	(0.025)
Treatment × Weak partisanship	0.085***	0.099***	0.078***	0.060**	−0.089***	0.002	0.039*
	(0.015)	(0.019)	(0.023)	(0.024)	(0.031)	(0.023)	(0.022)
Treatment × Strong partisanship	0.066***	0.081***	0.052**	0.026	−0.098***	0.035*	0.075***
	(0.014)	(0.017)	(0.020)	(0.021)	(0.027)	(0.020)	(0.019)
Constant	0.262***	0.253***	0.224***	0.235***	0.281***	0.324***	0.319***
	(0.013)	(0.016)	(0.022)	(0.024)	(0.024)	(0.018)	(0.017)
Observations	8,287	5,772	3,783	3,459	2,515	4,504	4,828
R^2	0.140	0.132	0.124	0.118	0.197	0.157	0.151
Adjusted R^2	0.139	0.131	0.121	0.116	0.195	0.155	0.150

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

2.E Subgroup Analyses Study 1 (Germany)

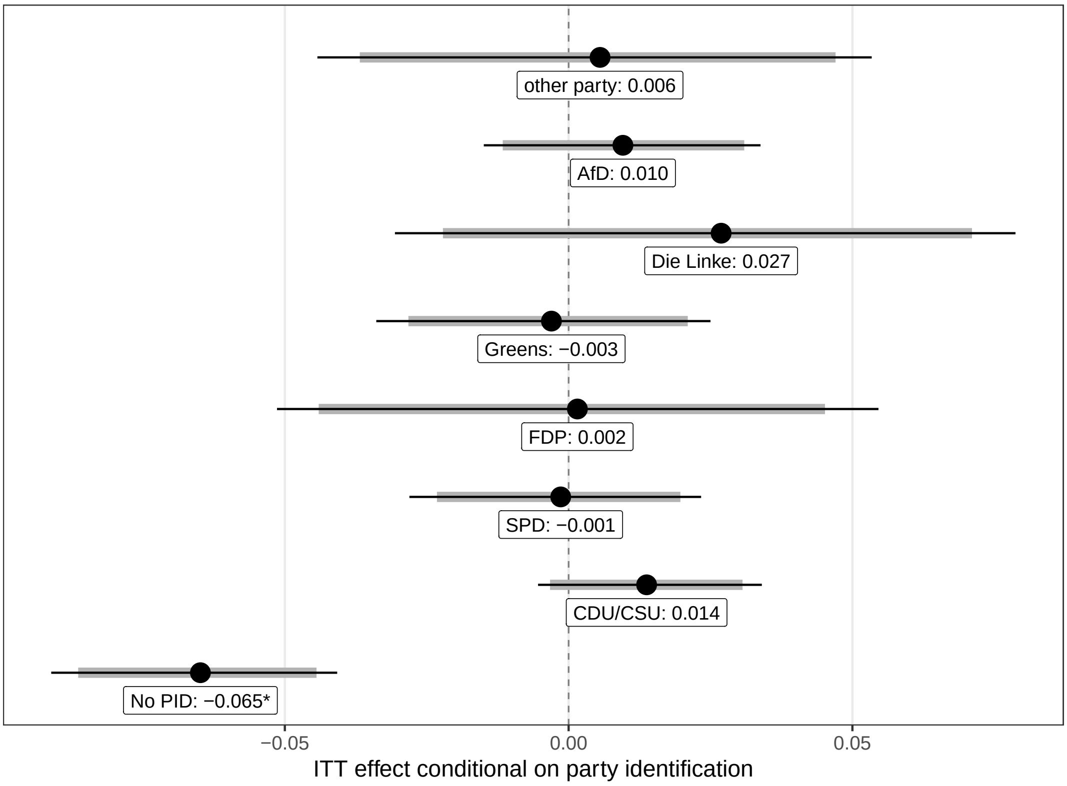

Figure 2.E.1: ITT effects conditional on specific identification with one of the six parties represented in the German Bundestag, plus those who do not identify with any party at all. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. For details, see Table 2.E.1.

Table 2.E.1: OLS regression results - interaction with specific partisanship (baseline category: no party identity)

	Dependent variable:
	Uncertain vote intention
Treatment	−0.064***
	(0.012)
CDU/CSU identification	−0.264***
	(0.012)
SPD identification	−0.249***
	(0.013)
FDP identification	−0.234***
	(0.020)
Green identification	−0.261***
	(0.014)
Die Linke identification	−0.259***
	(0.021)
AfD identification	−0.278***
	(0.013)
other party identification	−0.248***
	(0.019)
Female	0.034***
	(0.005)
Age	0.001***
	(0.0002)
Intermediate education	−0.018**
	(0.008)
High education	−0.030***
	(0.007)
Treatment × CDU/CSU identification	0.079***
	(0.016)
Treatment × SPD identification	0.063***
	(0.018)
Treatment × FDP identification	0.065**
	(0.029)
Treatment × Green identification	0.061***
	(0.020)
Treatment × Die Linke identification	0.091***
	(0.030)
Treatment × AfD identification	0.074***
	(0.018)
Treatment × no party identification	0.069**
	(0.028)
Constant	0.260***
	(0.014)
Observations	7,762
R^2	0.142
Adjusted R^2	0.139
Note:	*p<0.1; **p<0.05; ***p<0.01

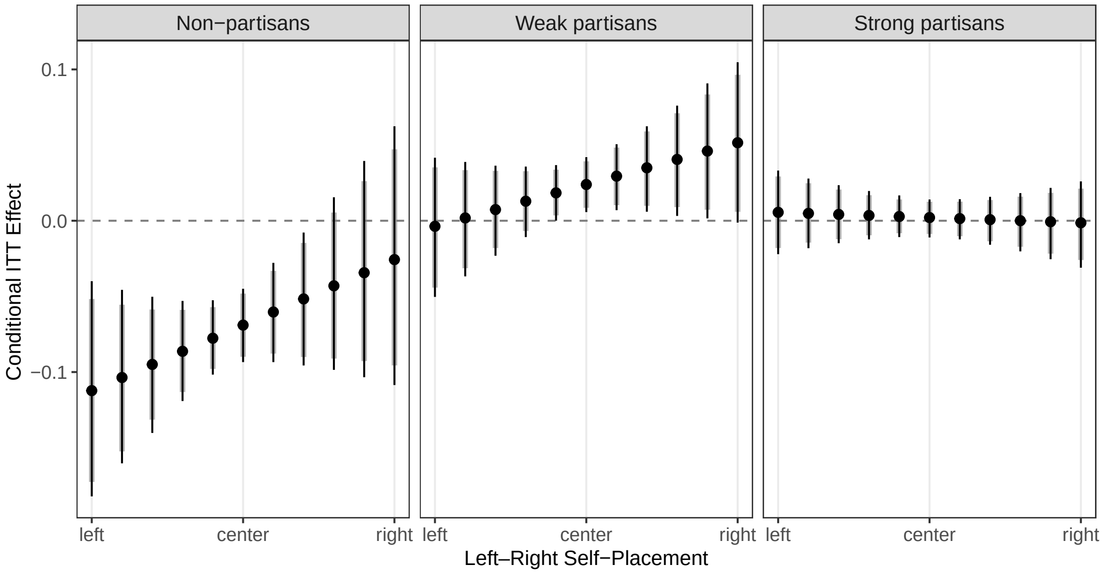

Figure 2.E.2: Subgroup-specific TT effects on reported short-term vote intention uncertainty for non-partisans, weak partisans and strong partisans across different levels of ideological left-right self-placement. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. Detailed regression results are reported in Table 2.E.2.

Table 2.E.2: OLS regression results - three-way interaction with partisanship and ideology

	Dependent variable:
	Uncertain vote intention
Treatment	-0.111^{***}
	(0.037)
Left-right self-placement	-0.036
	(0.054)
Weak partisans	-0.206^{***}
	(0.031)
Strong partisans	-0.248^{***}
	(0.028)
Female	0.024^{***}
	(0.005)
Age	0.001^{***}
	(0.0002)
Intermediate education	-0.013^{*}
	(0.007)
High education	-0.017^{**}
	(0.007)
Treatment × Left-right	0.083
	(0.076)
Treatment × Weak partisans	0.107^{**}
	(0.044)
Treatment × Strong partisans	0.117^{***}
	(0.039)
Left-right × Weak partisans	-0.009
	(0.064)
Left-right × Strong partisans	0.038
	(0.057)
Treatment × Left-right × Weak partisans	-0.029
	(0.090)
Treatment × Left-right × Strong partisans	-0.090
	(0.080)
Constant	0.239^{***}
	(0.028)
Observations	7,760
R^2	0.105
Adjusted R^2	0.104
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

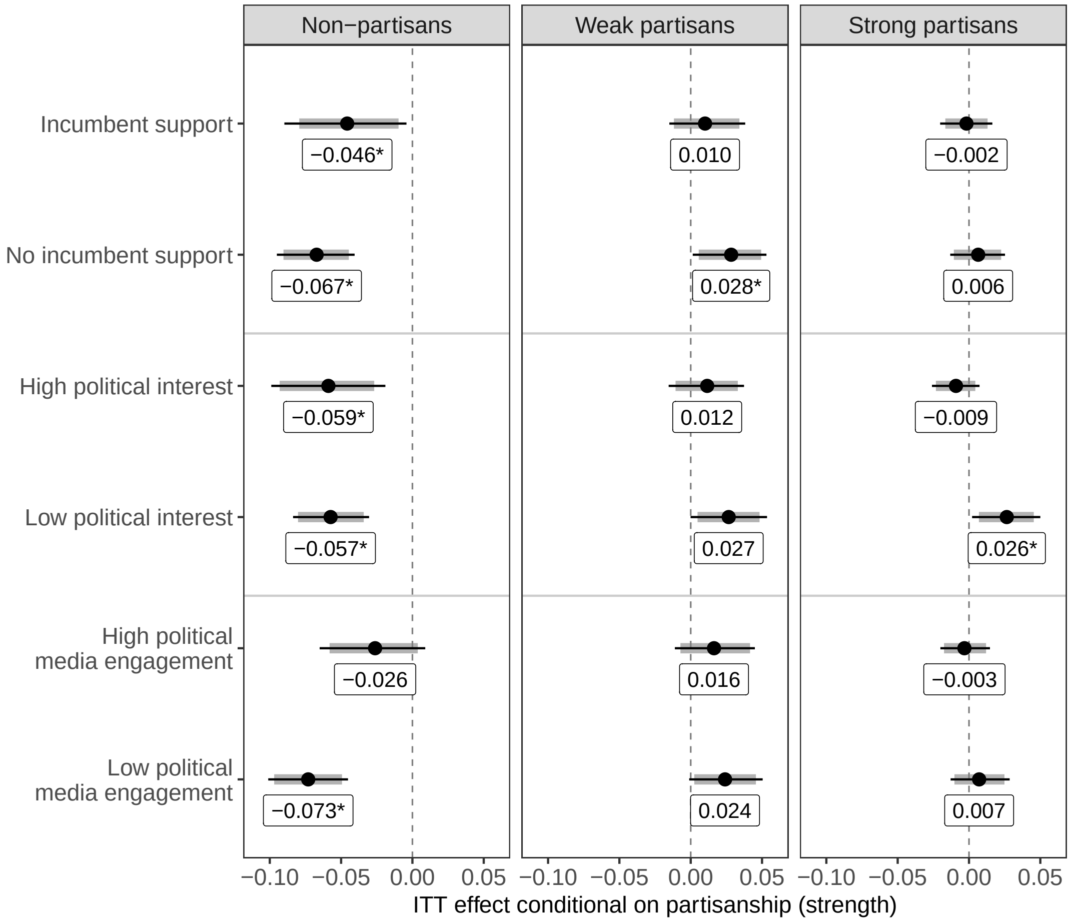

Figure 2.E.3: Subgroup-specific ITT effects on reported short-term vote intention uncertainty for non-partisans, weak partisans and strong partisans divided by incumbent support, political interest and political media engagement (low = median or below, high = above median). Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.E.3.

Note: We operationalize incumbent supporters as those who recall to have voted for one of the incumbent parties (CDU/CSU, Greens of FDP) in the 2021 German federal elections. Political interest (low = median or below, high = above median) is based on a self-assessment item. And political media engagement (low = median or below, high = above median) reflects the frequency with which respondents report having sought political information online.

Table 2.E.3: OLS regression results - three-way interactions with incumbent support, political interest and media engagement

	Dependent variable:		
	Uncertain vote intention		
	(1)	(2)	(3)
Treatment	-0.067^{***}	-0.058^{***}	-0.073^{***}
	(0.014)	(0.015)	(0.015)
Weak partisanship	-0.293^{***}	-0.281^{***}	-0.273^{***}
	(0.014)	(0.014)	(0.014)
Strong partisanship	-0.327^{***}	-0.324^{***}	-0.308^{***}
	(0.012)	(0.014)	(0.013)
Incumbent support	-0.198^{***}		
	(0.019)		
High political interest		-0.134^{***}	
		(0.019)	
High political media engagement			-0.104^{***}
			(0.019)
Female	0.029^{***}	0.022^{***}	0.028^{***}
	(0.005)	(0.005)	(0.005)
Age	0.001^{***}	0.001^{***}	0.001^{***}
	(0.0002)	(0.0002)	(0.0002)
Education intermediate	-0.013^{*}	-0.015^{**}	-0.017^{**}
	(0.007)	(0.007)	(0.008)
Education high	-0.017^{**}	-0.018^{***}	-0.024^{***}
	(0.007)	(0.007)	(0.007)
Treatment × Weak partisanship	0.095^{***}	0.084^{***}	0.098^{***}
	(0.020)	(0.020)	(0.020)
Treatment × Strong partisanship	0.073^{***}	0.084^{***}	0.081^{***}
	(0.017)	(0.019)	(0.018)
Treatment × Incumbent support	0.021		
	(0.026)		
Weak partisanship × Incumbent support	0.177^{***}		
	(0.023)		
Strong partisanship × Incumbent support	0.193^{***}		
	(0.021)		
Treatment × Weak partisanship × Incumbent support	-0.038		
	(0.032)		
Treatment × Strong partisanship × Incumbent support	-0.029		
	(0.029)		
Treatment × High political interest		-0.002	
		(0.025)	
Weak partisanship × High political interest		0.116^{***}	
		(0.023)	
Strong partisanship × High political interest		0.146^{***}	
		(0.021)	
Treatment × Weak partisanship × High political interest		-0.012	
		(0.032)	
Treatment × Strong partisanship × High political interest		-0.034	
		(0.029)	
Treatment × High political media engagement			0.048^{*}
			(0.025)
Weak partisanship × High political media engagement			0.094^{***}
			(0.024)
Strong partisanship × High political media engagement			0.115^{***}
			(0.021)
Treatment × Weak partisanship × High political media engagement			-0.056^{*}
			(0.032)
Treatment × Strong partisanship × High political media engagement			-0.059^{**}
			(0.029)
Constant	0.306^{***}	0.300^{***}	0.288^{***}
	(0.014)	(0.014)	(0.014)
Observations	8,287	8,287	8,287
R^2	0.162	0.153	0.144
Adjusted R^2	0.161	0.151	0.143
Note:			$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

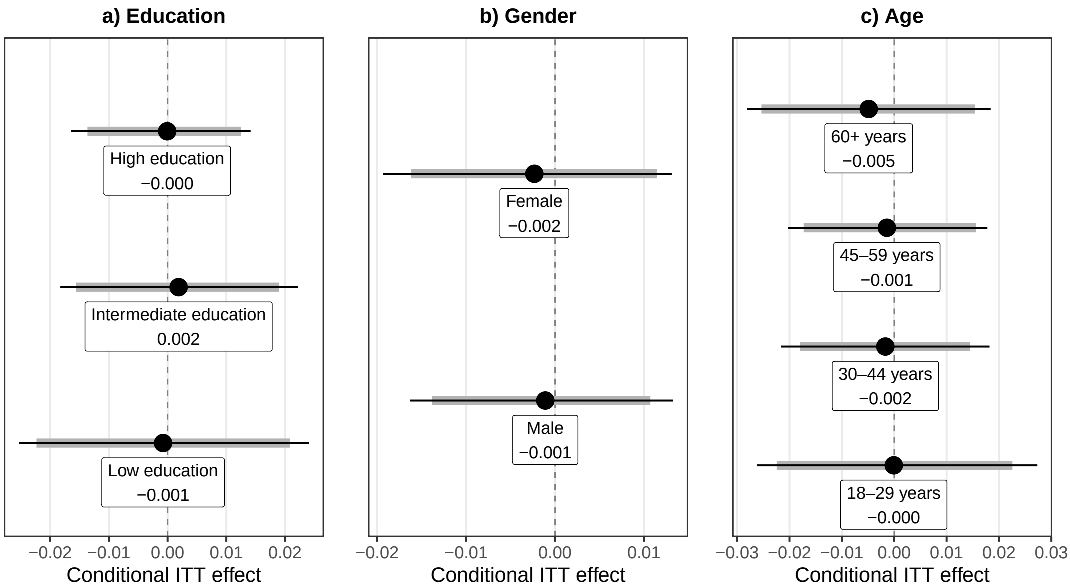

Figure 2.E.4: ITT effects isolated for a) different levels of education, b) gender and c) age. Low education = max. 9 years of school or no diploma, intermediate education = 10 years of school, high education = university entrance qualification. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.E.4.

Table 2.E.4: OLS regression results - three-way interactions with sociodemographics

	Dependent variable:		
	Uncertain vote intention		
	(1)	(2)	(3)
Treatment	0.000	−0.001	−0.001
	(0.013)	(0.009)	(0.014)
Education intermediate	-0.023^{***}	-0.021^{***}	-0.018^{***}
	(0.011)	(0.007)	(0.008)
Education high	-0.045^{***}	-0.043^{***}	-0.040^{***}
	(0.010)	(0.008)	(0.007)
Treatment × Intermediate education	0.002		
	(0.017)		
Treatment × High education	−0.000		
	(0.016)		
Female	0.056^{***}	0.062^{***}	0.056^{***}
	(0.006)	(0.007)	(0.006)
Treatment × Female		−0.006	
		(0.013)	
Age	0.000	0.000	
	(0.000)	(0.000)	
30-44 years			0.005
			(0.011)
45-59 years			0.004
			(0.011)
60+ years			0.011
			(0.011)
Treatment × 30-44 years			−0.001
			(0.018)
Treatment × 45-59 years			−0.001
			(0.018)
Treatment × 60+ years			−0.003
			(0.020)
Constant	0.058^{***}	0.054^{***}	0.063^{***}
	(0.014)	(0.012)	(0.011)
Observations	8,208	8,334	8,334
R^2	0.017	0.017	0.017
Adjusted R^2	0.016	0.017	0.015
Note:			*p<0.1; **p<0.05; ***p<0.01

2.F Descriptives Study 2 (UK)

Figure 2.F.1 shows the distribution of respondents across the survey field time of the BESIP waves 26 (pre snap election call) and 27 (post snap election call). As we only include repeated respondents into our analyses, the total number of pre- and post-event participants is equal ($n = 21,670$).

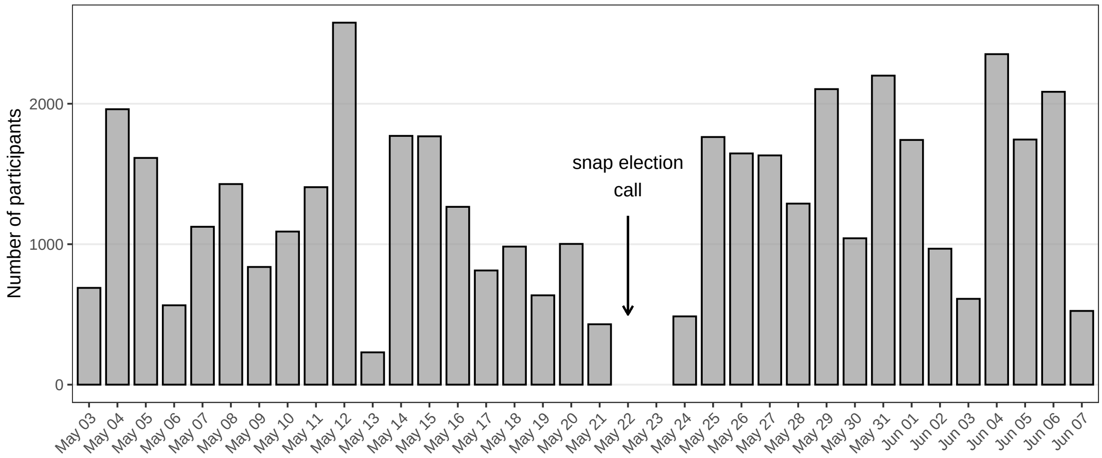

Figure 2.F.1: Number of participants per day in the BESIP survey waves 26 and 27

2.G Regression Results Study 2 (UK)

Note to Table 2.G.1: Because respondents without change in the outcome variable do not contribute to the likelihood-function, they are by design excluded in the estimation of the logit fixed-effects model (Model 5). This results in the much smaller sample size as compared to our core linear regression model (Model 2). To test the robustness of these results, we re-estimated our core linear model based on the same subsample of respondents. The linear model (Model 4) is estimated on the restricted sample of only those who switched from having a certain to an uncertain vote intention or vice versa between the two waves. All variables in Model 3 are recoded to the unit interval 0 – 1.

Table 2.G.1: Results of logit and linear fixed-effects regressions

	Dependent variable:						
	Uncertain vote intention						
	(1) linear	(2) linear (core)	(3) linear	(4) linear restricted	(5) logit	(6) linear 2015 GE	(7) lin. DK + nonresponse
Treatment	-0.045^{***}	-0.042^{***}	-0.046^{***}	-0.251^{***}	-1.505^{***}	-0.025^{***}	-0.043^{***}
	(0.002)	(0.005)	(0.006)	(0.030)	(0.083)	(0.004)	(0.005)
Political interest			0.009				
			(0.023)				
Left-right self-placement			0.001				
			(0.025)				
Government approval			-0.053^{***}				
			(0.014)				
Treat × Weak partisanship		-0.050^{***}	-0.048^{***}	-0.235^{***}		-0.017^{***}	-0.049^{***}
		(0.007)	(0.008)	(0.044)		(0.005)	(0.007)
Treat × Strong partisanship		0.018^{***}	0.021^{***}	-0.118^{***}		0.025^{***}	0.020^{***}
		(0.006)	(0.006)	(0.046)		(0.004)	(0.006)
Observations	43,252	41,590	34,150	5,000	5,370	53,772	42,506
R^2	0.016	0.021	0.027	0.140		0.006	0.020
Adjusted R^2	−0.968	−0.958	−1.043	−0.722		−0.987	−0.961
Log-Likelihood					-3,367.5		
Individual-level fixed-effects	✓	✓	✓	✓	✓	✓	✓
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$						

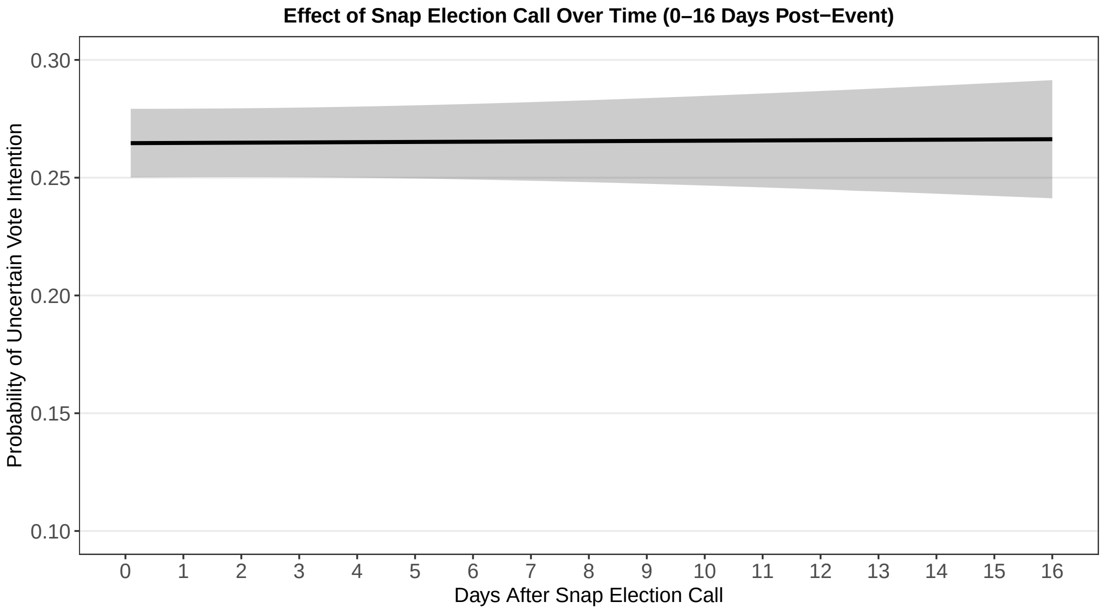

Figure 2.G.1: Time trends in the likelihood of holding an uncertain vote intention after the snap election call. Estimates are based on a model including an interaction of the treatment variable with time and partisan strength. Grey-shaded area represents the 95% confidence interval.

2.H Subgroup Analyses Study 2 (UK)

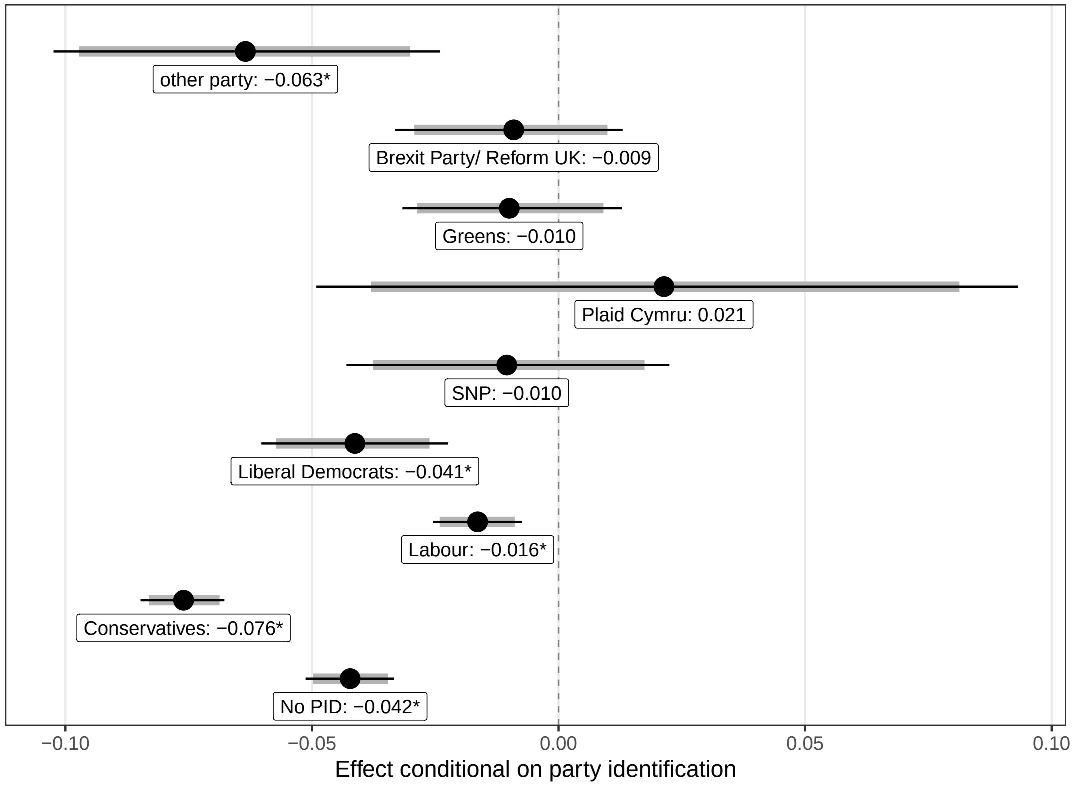

Figure 2.H.1: Effects conditional on specific party identification. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.H.1.

Table 2.H.1: Results of linear fixed-effects regression - interaction with specific party identity

	Dependent variable:
	Uncertain vote intention
Treatment (baseline: no PID)	-0.042^{***}
	(0.005)
Treatment × Conservatives	-0.034^{***}
	(0.006)
Treatment × Labour	0.025^{***}
	(0.007)
Treatment × Liberal Democrats	0.001
	(0.010)
Treatment × SNP	0.031^{*}
	(0.017)
Treatment × Plaid Cymru	0.064^{*}
	(0.036)
Treatment × Greens	0.032^{**}
	(0.013)
Treatment × Brexit Party/ Reform UK	0.033^{***}
	(0.013)
Treatment × other party	-0.021
	(0.020)
Observations	41,388
R^2	0.020
Adjusted R^2	-0.961
Note:	*p<0.1; **p<0.05; ***p<0.01

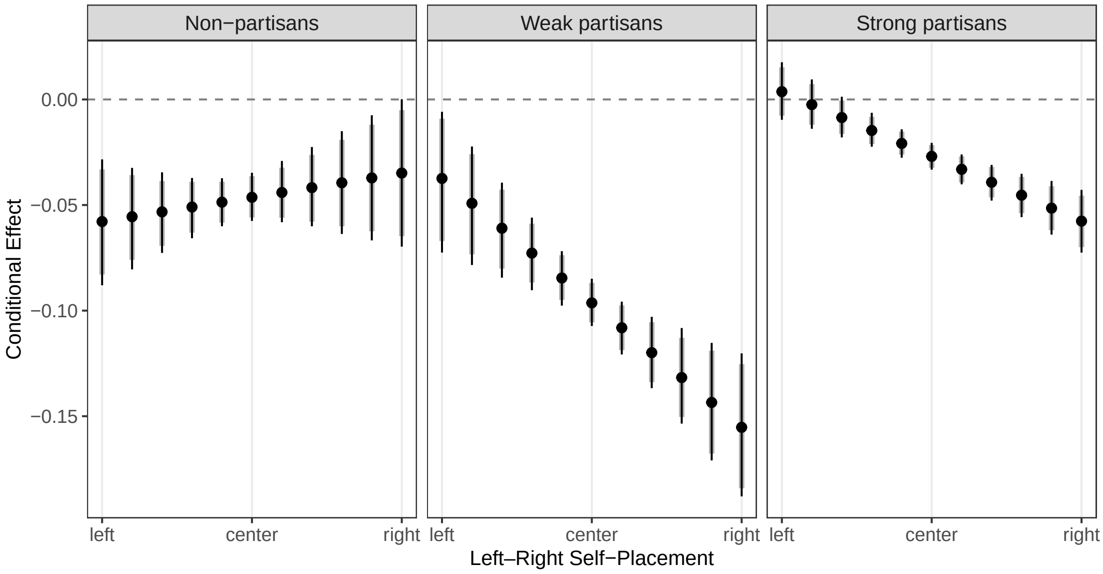

Figure 2.H.2: Effects on reported short-term vote intention uncertainty for non-partisans, weak partisans and strong partisans conditional on ideological left-right self-placement. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. Detailed regression results are reported in Table 2.H.2.

Table 2.H.2: Results of linear fixed-effects regression - three-way interactions with partisanship and ideology

	Dependent variable:
	Uncertain vote intention
Treatment	-0.058^{***}
	(0.015)
Left-right self-placement	0.054
	(0.065)
Treatment × Left-right	0.024
	(0.031)
Treatment × Weak partisans	0.021
	(0.023)
Treatment × Strong partisans	0.062^{***}
	(0.017)
Left-right × Weak partisans	-0.075
	(0.090)
Left-right × Strong partisans	0.003
	(0.073)
Treatment × Left-right × Weak partisans	-0.141^{***}
	(0.045)
Treatment × Left-right × Strong partisans	-0.085^{**}
	(0.034)
Observations	34,506
R^2	0.029
Adjusted R^2	-1.029
Note:	*p<0.1; **p<0.05; ***p<0.01

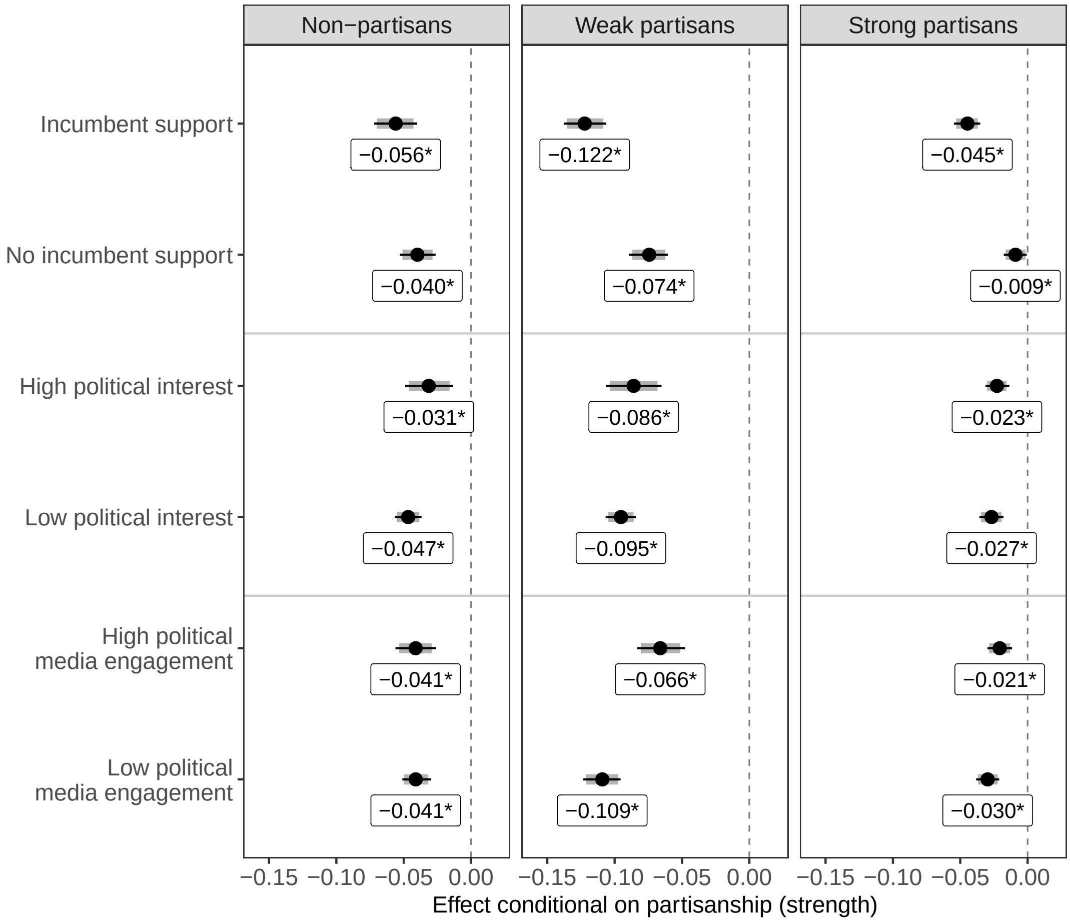

Figure 2.H.3: Subgroup-specific effects on reported vote intention uncertainty for non-partisans, weak partisans and strong partisans divided by incumbent support, political interest and political media engagement. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.H.3.

Note: We operationalize incumbent support by respondents' reported vote for the Conservative Party in the 2019 general election in the 2019 post-election survey wave.[29] Political interest is measured via a self-assessed item (low = median or below; high = above median). Political media engagement captures the frequency with which respondents report having sought political information online (low = median or below, high = above median). To avoid post-treatment bias, all moderators are taken from the pre-event wave.

[29]Using post-election vote recall from 2019 avoids potential biases by the 2024 snap election call. Because not all panel respondents from the core BESIP waves of May and June 2024 participated in the 2019 post-election wave, the sample for this subgroup analysis is reduced to approximately 36,000 observations, compared to roughly 42,000 in the core model. Estimating the core model on this smaller sample yields substantively similar results (the effect of the snap election call remains $\beta = -0.046$, $SE = 0.005$), suggesting the reduced sample does not bias our findings.

Table 2.H.3: Results of linear fixed-effects regression - three-way interactions with incumbent support, political interest and media engagement

	Dependent variable:		
	Uncertain vote intention		
	(1)	(2)	(3)
Treatment (baseline: no partisanship)	−0.040***	−0.047***	−0.041***
	(0.007)	(0.005)	(0.006)
Treatment × Weak partisanship	−0.034***	−0.049***	−0.068***
	(0.010)	(0.008)	(0.009)
Treatment × Strong partisanship	0.031***	0.020***	0.012
	(0.008)	(0.007)	(0.007)
Treatment × Incumbent support	−0.016		
	(0.011)		
Treatment × Weak partisanship × Incumbent support	−0.033**		
	(0.015)		
Treatment × Strong partisanship × Incumbent support	−0.020		
	(0.013)		
Treatment × High political interest		0.016	
		(0.011)	
Treatment × Weak partisanship × High political interest		−0.007	
		(0.016)	
Treatment × Strong partisanship × High political interest		−0.012	
		(0.013)	
Treatment × High media attention			0.001
			(0.009)
Treatment × Weak partisanship × High media attention			0.042***
			(0.014)
Treatment × Strong partisanship × High media attention			0.008
			(0.012)
Observations	35,598	41,490	40,798
R^2	0.027	0.022	0.023
Adjusted R^2	−0.947	−0.957	−0.955
Note:			*p<0.1; **p<0.05; ***p<0.01

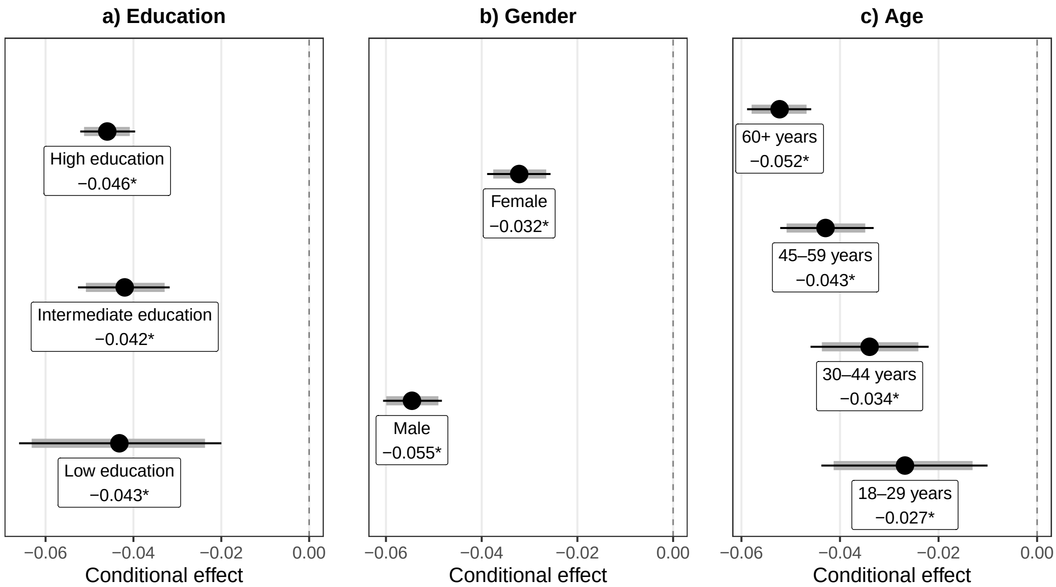

Figure 2.H.4: Effects of the snap election call on vote uncertainty conditional on a) different levels of education, b) gender and c) age. Low education = max. 9 years of school or no diploma, intermediate education = 10 years of school, high education = university entrance qualification. Thin lines represent the 95% confidence intervals, thicker lines represent the 90% confidence intervals. A $p < 0.05$ is indicated by *. Detailed regression results are reported in Table 2.H.4.

Table 2.H.4: Results of linear fixed-effects regression - three-way interactions with sociodemographics

	Dependent variable:		
	Uncertain vote intention		
	(1)	(2)	(3)
Treatment	-0.043^{***}	-0.032^{***}	-0.026^{***}
	(0.012)	(0.004)	(0.008)
Intermediate education	0.049		
	(0.036)		
High education	0.065		
	(0.041)		
Treatment × Intermediate education	0.001		
	(0.013)		
Treatment × High education	-0.002		
	(0.012)		
Treatment × Female		-0.023^{***}	
		(0.005)	
30-44 years			0.034
			(0.143)
45-59 years			0.161
			(0.160)
60+ years			0.329^{*}
			(0.175)
Treatment × 30-44 years			-0.008
			(0.010)
Treatment × 45-59 years			-0.016^{*}
			(0.010)
Treatment × 60+ years			-0.026^{***}
			(0.009)
Observations	34,994	43,252	43,252
R^2	0.016	0.017	0.017
Adjusted R^2	-0.992	-0.966	-0.967
Note:			*p<0.1; **p<0.05; ***p<0.01

3 Survey Nonresponse After Elections: Investigating the Role of Winner-Loser Effects in Panel Attrition

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Abstract

When and for whom do election outcomes drive survey nonresponse? This paper investigates whether belonging to the winners or losers of an election affects the likelihood of participating in post-election surveys. Systematic winner-loser effects on survey participation and resulting sample disproportionalities could yield decisive consequences for the accuracy of public opinion measurements. Through a series of logistic regressions and survival analyses on individual-level panel data from the German Longitudinal Election Study (GLES) after four German federal elections between 2009 and 2021, I find that winners and losers are equally prone to post-election survey nonresponse. Attitudes towards the democratic system and past survey loyalty outweigh winner-loser effects: For both winners and losers, more negative democratic views seem to make people more likely to exhibit (durable) post-election nonresponse. Moreover, past survey participation reduces post-election nonresponse and its durability. Crucially, my results show that election outcomes alone rarely change survey participation. However, post-election surveys tend to display underrepresentation (of, e.g., less educated or politically uninvolved people), if people who disapprove of the election outcome are already likely to have more cynical attitudes towards politics and the democratic system.

Keywords

Winner-loser effects, survey participation, survey nonresponse, panel attrition, survival analysis

3.1 Introduction

Time and again, research has shown that some people are more willing to participate in (political) surveys than others: for instance, those with high levels of political interest, sophistication and education tend to be more cooperative (Groves et al., 2004; Mellon & Prosser, 2017; Tourangeau et al., 2010) and more inclined to participate in longitudinal surveys (Olson & Witt, 2011). Considering widely researched phenomena like panel attrition, i.e., the loss of participants over time in a longitudinal study, it is well established that survey cooperativeness is not linear over time; participating in a survey once does not entail steadfast participation, and not participating once does not imply continued attrition (Lugtig, 2014). While research shows that changes in individuals' personal circumstances (e.g., socioeconomic or socio-psychological factors) affect survey participation (Lugtig, 2014; Trappmann et al., 2015), the impact of changes in the broader political context has been less studied (Camatarri et al., 2023).

I argue that elections represent a key example of contextual shifts, often resulting in new governments, shifts in legislative power, and policy changes that impact political behavior (Conway, 1989). Previous research compellingly demonstrates that being on the winning or losing side of an election matters, both for political actors and voters (Blais & Gélineau, 2007). While many studies focus on how these outcomes influence democratic satisfaction, trust, or political participation (Anderson et al., 2005; Easton, 1975; Gärtner et al., 2020; Nadeau & Blais, 1993), less is known about how they influence other underexplored (political) affordances, such as survey participation. In this paper, I explore how winning and losing influence survey cooperativeness, specifically examining who is most likely to become a nonrespondent in both the short and long term following an election.

This is important because if election outcomes influence survey participation, this effect is likely to drive systematic differences in respondents' propensity to participate in post-election surveys. If winners were more likely to respond than losers (or vice versa), post-election samples may overrepresent opinions of one of the two groups. If left unaddressed, such biases can distort measurements of political attitudes and behaviors, leading to inaccurate conclusions about public opinion (Bautista et al., 2007; Cavari & Freedman, 2023; Sciarini & Goldberg, 2016). Recognizing these potential patterns and identifying affected groups is therefore crucial for mitigating sample imbalances and improving the accuracy of survey-based research (Converse & Traugott, 1986). Survey results influence attitudes and can affect political behavior, such as vote choice (Brugarolas & Miller, 2021; Schmitt-Beck, 1996). Further, if people feel that there is little support for their viewpoint, surveys could discourage advocates of such a minority viewpoint, keeping that issue from being raised in political discourse (Fernández-Roldán & Barnfield, 2024; Lang & Lang, 1984). Against this backdrop, understanding how political events influence survey participation has valuable implications that extend beyond post-election surveys. Given the high saliency of elections, assessing participation biases in post-election surveys provides a strong starting point for broader investigations into survey representativeness and response dynamics.

I argue that winners are more likely than losers to continue participating in surveys after an election. However, I expect within-group variation, particularly among electoral losers: while some remain democratically supportive and accept their defeat, others experience increased frustration (e.g. Blais et al., 2017; Davis & Hitt, 2017), arguably yielding two outcomes: either a higher likelihood of post-election nonresponse, or a greater motivation to express dissatisfaction, thereby maintaining survey participation despite being on the losing side of the vote.

While the connection between political change and survey nonresponse is highlighted by extant literature (Harris-Kojetin & Tucker, 1999; Larsen et al., 2020; Silber et al., 2022), it is rarely tested on the individual-level, partly due to the challenge of obtaining data from nonrespondents. I address this by using panel data, which by design includes baseline information on respondents who later become nonrespondents. I draw on individual-level panel data from the German Longitudinal Election Study (GLES, 2015, 2016, 2023), covering four German federal elections (2009, 2013, 2017, and 2021). These cases provide a suitable setting due to the differing partisan outcomes across elections, where winners in one election may become losers in the next, and vice versa. This study employs a voter-level understanding of winning and losing: I classify "winners" as those who supported a party that gained vote share and "losers" as those whose party lost vote share since the last election. I employ logistic regressions to identify winner-loser effects on immediate nonresponse, and a simulation approach to estimate group-level differences in nonresponse likelihood. To gauge how long these effects might impact sample compositions, I conduct survival analyses to estimate the durability of post-election nonresponse.

My results show that after elections winners and losers are, on average, equally prone to nonresponse, without distinct differences in the durability of post-election nonresponse. Notable differences emerge among electoral losers: while democratically supportive and satisfied losers remain relatively responsive, those who score lower in these democratic outcomes are more prone to (durable) post-election nonresponse. Similar patterns prevail among winners: those who are more satisfied with democracy and show higher support for democratic principles are more responsive than the less satisfied and supportive. Moreover, past survey loyalty has greater effects on post-election participation and the durability of nonresponse than winner-loser effects. My findings highlight that democratic values are more predictive of post-election nonresponse than election outcomes themselves. While no substantial shifts in survey participation arise directly from election results, existing biases – like the underrepresentation of less educated or politically disengaged individuals (Billiet et al., 2007) – may be emphasized when for large numbers of individuals electoral loss coincides with negative democratic attitudes.

3.2 The Nexus of Politics and Survey Nonresponse

The willingness to participate in surveys is not uniformly distributed across the population (Groves et al., 2004; Mellon & Prosser, 2017; Tourangeau et al., 2010), and some people are more responsive than others. Considering phenomena like panel attrition, i.e. the loss of participants over time in a longitudinal survey, we also know that participating in a survey once does not imply subsequent participation. In panel studies, respondents face an accumulative participation burden from wave to wave (Lemay, 2009; Lipps, 2009), leading to an increasing likelihood of refusing to participate over time.

In this paper, I refer to the refusal to participate in a survey as survey nonresponse. While this term and the mechanisms outlined in the following section should, in general, apply to nonresponse in any kind of survey design, i.e. cross-sectional as well as panel designs, I focus on nonresponse in panel studies. Strictly speaking, survey nonresponse resembles attrition in this case, i.e. the switch from being responsive at least once (the panel recruitment wave) to becoming unresponsive.

There are various reasons for survey nonresponse in panel studies (Lugtig, 2014): a lack of commitment, the breaking of the habit of survey participation, and panel fatigue, i.e. the burden of future participation catering to the perception of having already fulfilled one's duty. Additionally, contextual "shocks" (Lemay, 2009) can lead to drop-out. These can be, e.g., geographical mobility or changes in the household composition (Lugtig, 2014).

While existing research has almost exclusively focused on contextual shocks located in people's narrower, personal environment (Berinsky, 2008; Lugtig, 2014), I argue that also broader, far-reaching political events can affect survey nonresponse. Due to their unique characteristics, I argue that elections meet this condition: First, elections are of broad scope with most people being aware of them as election day nears, given an intensified media coverage, widespread campaign events, televised debates and the reception of voting materials. Second, elections have a distinct temporal dimension, allowing for a clear division between pre- and post-treatment time-periods. Ultimately, election outcomes affect people as they likely yield a redistribution of decision-making power and policy shifts. One phenomenon that links an election outcome to the individual level is voters' perception of belonging to the electoral winners or losers.

3.2.1 Who Are Election Winners and Losers?

Considering the variety of electoral systems, government compositions and party systems, there are many notions of who are electoral winners and losers. Both parties and voters can be winners or losers. In this study, I focus on voters' winner- and losership. Literature in this field has identified winners and losers by subjective and objective factors (Plescia, 2019). While the former are based on voters' perceptions of feeling electoral winner or loser, the latter are linked to vote choice and its relationship to parties' objective performance, such as vote share or government participation.

For the subjective and perceived winner- and losership, the individual interpretation of election results in interplay with one's party preferences is decisive (Plescia, 2019). Perceived winning and losing has been operationalized, e.g., by direct questions of self-perceived winning (Gärtner et al., 2020). Alternatively, Wolak (2014) employs people's evaluation of the election being fair or unfair as proxy for, respectively, winner- and losership, pointing towards an association of winning with higher and losing with lower levels of democratic support.

An objective identifier for individual-level winner- and losership is, for instance, voting for a party that enters the government (Anderson & Guillory, 1997; Plescia, 2019). Voters of the party gaining the largest share of votes and seats are also likely to consider themselves winners (Stiers et al., 2018). Other studies demonstrate that voters tend to feel electoral winner if their party of choice gained vote share compared to the previous election (Plescia, 2019; Stiers et al., 2018). Contrasting winner-loser operationalizations that rest on vote choice, other studies rely on party preferences and identifications for the classification (Jou, 2009; Moehler, 2009).

In this study, I rely on objective winner-loser indicators, as only these allow to approximate winner- and losership also for nonrespondents. Specifically, I employ an indicator of voting for a party that has gained (winner) or lost (loser) vote share as compared to the previous election. The Research Design section provides a more thorough account of the used operationalization.

3.2.2 How Can Winning or Losing an Election Affect Survey Nonresponse?

The literature on winner-loser effects consistently demonstrates that winners and losers differ in their behaviors, experiences, and emotional responses to election outcomes. Electoral winners exhibit higher levels of external political efficacy (Davis & Hitt, 2017) and more favorable attitudes toward the government compared to those perceiving themselves as electoral losers (Anderson et al., 2005; Esaiasson, 2011; Nadeau & Blais, 1993). Moreover, winning is strongly associated with increased democratic satisfaction (Blais & Gélineau, 2007; Blais et al., 2017; Singh, 2014), higher levels of political support and approval of electoral rules (Davis & Hitt, 2017; Kernell & Mullinix, 2019). While research on the nexus of political behavior and survey methodology has analyzed how, e.g., political efficacy and democratic satisfaction relate to survey participation (Silber et al., 2022), this is the first study to directly link winning or losing an election to survey nonresponse. This section explores the underlying mechanisms of winner-loser effects on post-election survey participation.

Electoral *winners* typically experience a boost in democratic satisfaction, political support and incentives for political participation after their preferred candidate or party succeeds (Singh et al., 2012). This is driven by policy-related and emotional mechanisms (Gärtner et al., 2020).

According to the *policy-related* mechanism, winners expect their preferred policies to be implemented, as their chosen party gains representation in parliament or government

(Singh et al., 2012). This expectation of prospective policy gains reinforces winners' satisfaction with democracy, particularly when their policy preferences align closely with those of the elected party and government (Singh, 2014).

On the *emotional* side, winners experience positive feelings, such as the psychological thrill of victory, which can further enhance their satisfaction with the political system (Bol et al., 2018; Davis & Hitt, 2017). As a result, winners are likely to view the political system as responsive and legitimate, increasing their likelihood of engaging in political activities.

At this point it is important to consider how survey participation can be seen as a form of political participation. Building on research that identifies shared drivers of survey and political participation (Voogt & Saris, 2007), I argue that survey participation carries a political dimension: it enables individuals to express political views, preferences, and attitudes, contributing to the democratic process. Political participation is traditionally understood as actions like voting, protesting, or contacting representatives (Conge, 1988). However, survey participation also provides a means of indirect engagement with the political system. Feedback from surveys is often used by researchers and policymakers to gauge public opinion and shape policy priorities. Thus, survey participation allows citizens to assert their political presence and potentially influence public discourse, even outside formal electoral processes.

This indirect form of political engagement is especially relevant in the electoral context, where public attitudes towards the outcome and the functioning of democracy can be critically assessed. By participating in surveys, individuals validate their views and contribute to a dialogue on the state of the political system (Silber et al., 2022). As such, survey participation can be seen not only as an expression of civic duty but also as a response to one's (dis)satisfaction with political outcomes, framing it as a form of political participation where the decision to engage or abstain reflects broader political sentiments (Harris-Kojetin & Tucker, 1999; Silber et al., 2022; Verba, 1996).

Given the policy-related and emotional mechanisms that boost their democratic satisfaction and thus likelihood to politically participate, winners expectedly maintain or even decrease their likelihood of post-election survey nonresponse. Contrasting, *losers* may not experience the same boost in democratic satisfaction and political participation, as their preferred policies are less likely to be implemented. Therefore, I expect:

Hypothesis 1: *Electoral losers are more prone to nonresponse in post-election surveys than electoral winners.*

However, losers' ways of dealing with electoral defeat are, arguably, not uniform. This likely yields heterogeneous effects of losing on post-election survey nonresponse. Relying on existing winner-loser literature, I argue that losers can be divided into two groups based on predispositions that determine their acceptance of the electoral outcome: graceful and sore losers (Nadeau et al., 2021). Each group has different implications for post-election survey participation.

Graceful losers accept the election outcome, even when it does not favor their preferred candidate or party. They are characterized by a positive stance towards the democratic system which renders them likely to view the electoral process as legitimate and fair, despite their disappointment with the outcome (Esaiasson, 2011; Nadeau et al., 2021). This acceptance often reflects a broader commitment to democratic principles and an understanding that losing is a natural part of competitive elections (Nadeau et al., 2021). Graceful losers are likely to retain their pre-election levels of external efficacy and satisfaction with democracy also after the election (Nadeau et al., 2021), which can sustain their participation in post-election surveys. Thereby, the election outcome should not affect the degree to which they perceive survey participation as an act of political engagement. Thus, their likelihood of survey participation remains stable; graceful losers are expected to participate in post-election surveys at levels like those before the election.

Sore losers, on the other hand, struggle to accept the election outcome and often experience a subsequent decline in democratic satisfaction. They generally hold more pessimistic stances towards the democratic system. When experiencing electoral defeat, these predispositions render them more likely to perceive the political system as unresponsive and unfair (Nadeau et al., 2021). This negative perception can generate feelings of frustration, anger, and disillusionment, which may discourage political participation, including survey response. Negative emotions such as distress and anxiety are, further, associated with higher nonresponse rates (Ashley & Shaughnessy, 2023). Sore losers' negative view on politics makes them generally more prone to survey nonresponse. Experiencing electoral defeat likely increases their feeling of disconnect from politics and their disillusionment by political outcomes (Quaranta et al., 2020). Thus, they may be less willing to engage in post-election surveys. For sore losers, the experience of defeat might be, consequently, the decisive factor driving a switch from responsiveness prior to election day to post-election nonresponse. Based on this reasoning, I hypothesize:

Hypothesis 2a: *Electoral losers who accept their defeat ("graceful losers") are less prone to post-election survey nonresponse than democratically dissatisfied and less supportive losers ("sore losers").*

However, it is important to recognize that not all sore losers will withdraw from participation. Some may be motivated to voice their dissatisfaction and seek to influence future political outcomes, which can drive them to participate in surveys as a form of political expression (Voogt & Saris, 2007). This subgroup of sore losers, despite their dissatisfaction, may view survey participation as an opportunity to communicate their discontent and advocate for change; participating in surveys could become a channel to express their opposition and potentially impact public discourse. Thus, I hypothesize:

Hypothesis 2b: *If "sore losers" translate their defeat and dissatisfaction into a motivation to voice their stances and politically participate, they are equally prone to post-election survey nonresponse than "graceful losers".*

3.2.3 How Durable Are Winner-Loser Effects on Survey Nonresponse?

An election outcome initially serves as a reliable indicator of public sentiment, but over time, surveys become the primary reflection of public opinion. Understanding how long winner-loser effects on survey nonresponse persist is thus crucial, as it helps identify potential biases that arise from their effects on survey participation as the election outcome fades as a benchmark. In a more exploratory manner, I thus question how durable winner-loser effects on survey nonresponse are. Building on past research that explores the temporal dynamics of winner-loser effects on democratic satisfaction (Chang et al., 2014), I expect that their effects on survey nonresponse may also vary in duration.

From an emotional perspective, the initial euphoria of winning or disappointment of losing is likely to fade over time. Accompanied by decreasing media coverage of election outcomes and blurring memories of the event, the impact on survey nonresponse may analogously diminish early in the electoral cycle.

From a policy perspective, different outcomes are likely: Once a new government takes office, citizens can evaluate its performance, providing a "reality check" against their expectations (Clarke & Acock, 1989). Since these evaluations vary based on individual expectations and policy outcomes (influenced by factors like coalition politics or unforeseen crises), there is no clear expectation of how and when such evaluations may alter initial winner-loser effects on survey participation, as both winners and losers might shift in satisfaction with government policies.

In sum, while the same factors that drive winner-loser effects on post-election survey participation may also influence their duration, this analysis remains exploratory given the broad theoretical implications involved.

3.3 The Case: German Federal Elections 2009 to 2021

I test my theoretical arguments using the German federal elections of 2009, 2013, 2017, and 2021, which provide suitable cases due to both data availability and Germany's electoral system. While Germany is not unique as a multi-party parliamentary democracy, the selected elections vary in political dynamics, enabling an assessment of whether post-election nonresponse is driven by winner-loser status or other election-specific factors.

In 2009, both "people's parties" the Christian Democrats (CDU/CSU)[30] and Social Demo-crats (SPD) lost votes. While the CDU/CSU suffered minor losses only, the SPD lost 11.2 pp of votes, being the only party losing parliamentary seats. To a different extent each, the Liberal Democrats (FDP), the leftist party Die Linke and the Greens gained votes. One month after the election, the coalition government of CDU/CSU and FDP formed.

In 2013, the fate of the two coalition partners was far apart: While CDU/CSU managed to regain almost 8 pp in vote share, the FDP clearly lost this election. With an almost 10 pp vote share loss compared to 2009 they did not surpass the 5%-threshold to secure seats in the Bundestag. With the Greens and Die Linke, two other well-performing

[30]There are two Christian Democratic parties in Germany, the Christian Democratic Union (CDU) and the Christian Social Union (CSU). They are independent parties, with the CSU being present in Bavaria and the CDU in all remaining German states. Historically, they campaign together, nominate a common chancellor candidate and form a joint parliamentary group in the Bundestag. As they are politically and in media communication routinely presented as one, for example, by reporting a joint vote share as election result, I treat them as one cohesive party.

parties of 2009 lost votes. The SPD gained around 3 pp, however falling short of the expectations held before election day. A grand coalition of CDU/CSU and SPD formed three months after election day under chancellor Merkel (CDU).

The 2017 election shook up the balance of power. CDU/CSU and SPD lost votes, achieving one of their worst results. Although CDU/CSU remained the largest party, many voters perceived these parties as losers (Gärtner et al., 2020). Besides the Greens and Die Linke, FDP and the right-wing party Alternative für Deutschland (AfD) both gained votes and (re-)entered parliament. Following lengthy negotiations of almost six months, another grand coalition under chancellor Merkel formed.

In 2021, incumbent chancellor Merkel did not run again for the first time in 16 years, promoting a dynamic campaign phase (Jesse, 2021). CDU/CSU achieved their historically worst result, while the SPD managed to overcome their historic defeat of 2017, overtaking CDU/CSU as strongest party. While Die Linke and AfD lost vote share, Greens and FDP gained votes. Even though the Greens achieved their historically best result, they did not live up to the expectations that polls and generated during the campaign phase (Jesse, 2021). In December 2021, a coalition government of SPD, Greens and FDP formed under chancellor Olaf Scholz (SPD).

Germany's proportional representation system likely moderates the winner-loser gap in democratic satisfaction compared to majoritarian systems, where gaps tend to be larger (Anderson & Guillory, 1997). Many prior studies focus on referenda (Nadeau et al., 2021) or majoritarian elections (Nadeau & Blais, 1993), where winner-loser distinctions are clearer. As a result, I argue that Germany presents a conservative test case with likely smaller effect sizes as compared to most existing studies.

3.4 Research Design

To test determinants of post-election survey nonresponse and its durability, I first focus on winner-loser effects on nonresponse immediately after election day (Study 1). Then, I assess determinants of the durability of post-election nonresponse (Study 2). In the following, I detail the analytical setup.

3.4.1 Data

I leverage individual-level panel data from the German Longitudinal Election Study, offering a unique opportunity to track winner-loser effects on post-election survey participation across multiple elections, for its high-frequency post-election waves allow for an analysis of both immediate and long-term effects. For the 2009 and 2013 cases, I use data from the respective campaign panels (GLES, 2015, 2016). From the 2017 election onwards, I use data from the GLES panel (GLES, 2023) which includes re-contacted respondents from these previous campaign panels as well as newly recruited participants. For details on the wave- and recruitment structure, see Figure 3.A.1 in the Appendix. All respondents are drawn from an opt-in online access panel with quotas for gender, age, and education. The target population comprises all German citizens with internet access who were eligible to vote in the respective German federal election. Surveys were conducted by computer-assisted web interviews (CAWI). The criterion for inclusion into the respective analysis dataset is participation in the pre-election survey wave, yielding sample sizes of $n = 2,774$ (2009), $n = 3,619$ (2013), $n = 11,122$ (2017) and $n = 12,504$ (2021).[31]

Panel attrition and self-selection introduce biases that would not occur in random probability samples, yielding higher levels of political involvement and partisanship among GLES panelists as compared to the general population (Gärtner & Schoen, 2021; Steinbrecher & Schoen, 2013). Political involvement is positively correlated with survey response (Voogt & Saris, 2007) and survey loyalty (Olson & Witt, 2011). Thus, GLES-respondents are likely more responsive than the general population and less likely to switch towards post-election nonresponse, even under similar levels of affectedness by the election outcome. This additionally contributes to my results representing a conservative measure of how winning or losing can affect post-election nonresponse.

[31] Given the re-contacting of participants of the 2009 and 2013 campaign panels for the GLES panel used from the 2017 elections onwards, some respondents are present in the analysis sub-samples for more than one election. For instance, around 700 respondents of the 2013 sample had already participated in 2009. Of the sample used for the 2017 analysis, around 2000 respondents had participated in 2013. Given the circumstance that from 2016 onwards all respondents of the GLES panel were invited to participate in all subsequent waves, almost all respondents included in the 2021 analysis sample are also represented in the 2017 sample. Overall, this means that the majority of respondents is included in at least two of the four analysis samples. Given the relatively high temporal distance between elections and the comparability of sample compositions across the four cases, I do not expect repeated inclusion of the same respondents to bias my results.

3.4.2 Operationalization

In Study 1, the outcome is a binary variable indicating whether someone switched from participation in the pre-election survey wave to nonresponse in the post-election wave.[32]

For the key explanatory factors of being electoral winner or loser, I rely on an objective proxy (Plescia, 2019) based on voting for a party that has gained (= winner) or lost (= loser) vote share compared to the previous election. An overview of winning and losing parties based on vote share gains and losses is provided in Table 1.

Table 3.1: Winner and loser parties based on vote share gains (winner) or losses (loser) compared to the previous election.

	2009	**2013**	**2017**	**2021**
Winner parties	FDP Greens Die Linke	CDU/CSU SPD AfD	FDP Greens Die Linke	SPD FDP Greens
Loser parties	CDU/CSU SPD	FDP Greens Die Linke	CDU/CSU SPD	CDU/CSU AfD Die Linke

Arguably, subjective indicators of perceived winning and losing would be the most expressive proxies. However, given that reporting such perceptions requires post-election survey participation, missing data for post-election nonrespondents does not allow me to employ such a subjective operationalization. Extensive tests (see Section 3.B in the Appendix) demonstrate that among different objective winner-loser indicators, the operationalization based on gains and losses compared to the previous elections is the closest proxy of subjective measures of perceived winning and losing. Further, this operationalization proves most robust to cross-electoral differences (e.g., the election outcome or duration of government formation).[33] To determine if respondents are classified as win-

[32]In 2009, the federal election was held on 27 September. The pre-election wave was in field from 18-27 September, the post-election one from 29 September until 7 October. In 2013, the federal election was held on 22 September. The pre-election wave was in field from 16-21 September, the post-election one from 24 September until 4 October. In 2017, the federal election was held on 24 September. The pre-election wave fielded from 18-23 September, the post-election one from 27 September until 10 October. In 2021, election day was on 26 September. The pre-election wave fielded from 16-25 September, the post-election one from 29 September until 12 October.

[33]All analyses are additionally conducted based on the two alternative operationalizations; results are

ners or losers, I use their vote intention stated in the pre-election wave, as post-election nonrespondents lack vote choice data from the post-election wave.[34]

To differentiate *graceful* from *sore* losers, I include interaction terms of the loser variables with respondents' pre-electoral level of democratic satisfaction. The expectation is that higher initial levels of democratic satisfaction attenuate the expected positive effect of losership on post-election survey nonresponse. Democratic satisfaction is measured on a 5-point scale and recoded such that low values indicate low, high values indicate high levels of satisfaction. Considering the stark association of democratic satisfaction with survey participation (Silber et al., 2022), I include it as an independent variable too. Further, I construct an additive index of three items querying support of democracy principles.[35] High values indicate high support for principles of liberal democracy. Hypothesizing that graceful losers (and winners) will score higher in this variable than sore ones, I interact this index with losership (and winnership).

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reported in Section 3.C in the Appendix. Given that the employed core operationalization does not always follow an intuitive labeling of winners and losers (e.g., CDU/CSU and SPD in 2017 are both labeled as losers despite forming a coalition government together), these robustness checks demonstrate how the results differ when alternative notions of winning and losing (e.g. based on voting for a party that entered vs. did not enter the coalition government) are employed.

[34]For those who had decided by the pre-election wave, e.g., by casting a postal vote, their pre-election vote intention is replaced by their postal vote and thus matches their final vote choice. This is the case for 25.5 % (2009), 20.0 % (2013), 24.8 % (2017) and 29.5 % (2021) of respondents. For those who were not yet decided, the pre-election vote intention is the best reliable proxy for vote choice. Between 13 % (2017) and 30 % (2009) of respondents who participated in both waves switched their vote choice from pre- to post-election (these shifts are distributed evenly across the ideological space, i.e. there is no pattern that voters of a specific party show significantly more switches in vote intention). To minimize noise from these voting proxies, I re-run all estimations with winner-loser variables based on the more stable construct of party identification instead of vote intention as robustness test. Party identification is generally more stable than vote intention (Franklin and Jackson, 1983), with fewer respondents switching their party identification from pre- to post-election (17 % in 2021 and 25 % in 2009). These results (detailed in Section 3.C in the Appendix) show that the substantive findings remain consistent, suggesting that unobserved shifts from pre-election vote intention likely do not skew the results.

[35]Queried principles are 'In general, each democratic party should have the chance to assume government responsibilities', 'Everybody should have the right to defend his own view even if the majority dissents on that aspect', and 'A democracy will not work without a political opposition'. Including both democracy-related variables as moderators entails a more fine-grained differentiation of graceful and sore losers. Satisfaction with democracy reflects most recent perceptions of the state of democracy and is a relatively volatile concept with a clear link to the current political reality. Support for democracy principles, on the other hand, reflects values that are more deeply ingrained within the individual and more stable over. Especially considering that by design of the analytical setup, both variables can only be measured at the pre-electoral level, I expect the index of support for democracy principles to be more robust against shifts from pre- to post-election as compared to satisfaction with democracy. Thus, including both variables increases the validity and robustness of my results. The model results prove robust against only including one of the two moderators into the models. Results are reported in Table 3.D.3 in the Appendix.

All models incorporate additional explanatory variables that likely impact winner-loser status or survey nonresponse. Given that political interest generally boosts survey participation (Groves et al., 2004; Mellon & Prosser, 2017; Voogt & Saris, 2007), I control for respondents' self-reported political interest (0 = low to 1 = high). To ensure that winner-loser effects do not mask the impact of ideological stances, respondents' ideological self-placement is also included (0 = left to 1 = right). Respondents who consistently participate in survey waves before the election are more likely to participate in the post-election wave, so I control for the number of survey waves completed during the election year. Additionally, research indicates a positive link between education and survey response (Fitzgerald et al., 1998). I therefore control for high education levels (Abitur or equivalent qualification). All explanatory variables are measured pre-election to capture data on nonrespondents and to avoid bias from reactions to the election results (Plescia, 2019).

3.4.3 Study 1: Analytical Setup

In the first step, I test how winner-loser effects determine post-election survey nonresponse immediately after election day by estimating a set of binary logit regression models with post-election nonresponse as dependent variable. These models are estimated on the subsample of pre-electoral respondents, yielding sample sizes of $n = 2,774$ (2009), $n = 3,619$ (2013), $n = 11,122$ (2017) and $n = 12,504$ (2021).

To directly test for systematic differences in post-election nonresponse probabilities between winners and losers and to identify different types of losers (and winners), I additionally simulate so called *first differences* in predicted nonresponse probabilities. This is done through Monte Carlo simulations (with 1,000 draws) to obtain predicted probabilities while accounting for estimation uncertainty (King et al., 2000). To do so, I first construct scenarios for winners and losers as well as for subgroups of winners and losers. These scenarios utilize the observed value approach (Hanmer & Kalkan, 2013), where specific variables are held constant while all others reflect their observed values. Averages of the explanatory variables are computed post-simulation rather than pre-simulation. This method ensures that simulations are based on realistic scenarios represented in the data. Based on these scenarios, I simulate predicted nonresponse probabilities and calculate their differences between the groups. I detail the construction of scenarios in section "Study 1: Results".

3.4.4 Study 2: Analytical Setup

Expanding this cross-sectional perspective, I leverage the panel structure of the data and assess the durability of winner-loser effects on post-election nonresponse. Due to limited data availability for post-election periods in 2009 and 2013, I solely focus on the 2017 and 2021 elections. Within the employed survival analysis framework, a post-election nonrespondent can have two different states at each point of observation: (1) having returned to or (2) not having returned to participation. The likelihood of returning to participation at time t for the specified set of explanatory variables denoted by X is modeled by a Cox proportional hazard model:

$$h(t,X) = h_0(t)e^{\sum_{i=1}^{p} \beta_i X_i} \tag{3}$$

with $h_0(t)$ being the baseline hazard involving the factor of time. The baseline hazard is combined with the exponent of the linear sum $\beta_i X_i$ over the sum of p explanatory variables in X. While the baseline hazard involves time t but no other explanatory factors, the function's second quantity involves all time-invariant explanatory factors, i.e. X but not t. This semi-parametric approach allows me to assess the same explanatory variables that I already included in Study 1. Thereby, I can test whether immediate post- election nonresponse and its duration share similar determinants. All models are estimated on the subgroup of those who switched from pre-election response to post-election nonresponse, yielding sample sizes of $n = 663$ (2017) and $n = 849$ (2021).

3.5 Results

Figure 3.1 shows the number of participants across all survey waves per election year. The overview is constructed for all respondents who participated in the first wave fielded in the respective year.[36] As expected, participation always decreased over time (Lemay, 2009; Lipps, 2009; Lugtig, 2014). In all years except 2009, participation peaks after election day, which is most evident in 2017.[37] This increased aggregate-level survey participation in the post-election waves contrasts with the general attrition trend. My analysis should therefore, on average, yield conservative estimates of winner- and losership's effects on post-election nonresponse.

[36] The patterns are robust to earlier and later cut-offs.

[37] It has to be noted here that for each wave, panelists were incentivized to participate: every invitation included a reminder that respondents receive points (exchangeable for money) if they complete a minimum number of waves. This incentive was also given in the post-election wave. As this incentive did not differ compared to the few previous survey waves, I do not control for it in my analyses.

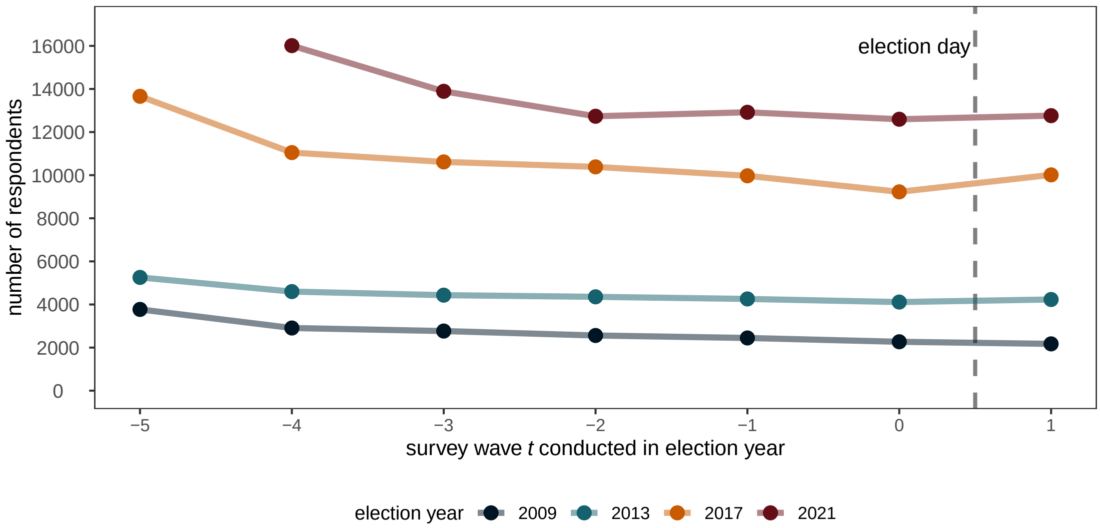

Figure 3.1: Survey respondents per election year and survey wave.

The left hand panel of Figure 3.2 displays the percentages of respondents identified as winners, losers, or non-voters, with non-voters excluded from winner-loser classifications. Across all four elections, winners represent the largest group, followed by losers and then non-voters.[38] The gap between voters and non-voters widens over time, which is partly due to the rising share of voters in the GLES Panel over these years. The right hand panel of Figure 3.2 shows post-election nonresponse rates by group. Non-voters consistently have the highest dropout rates, while winners and losers display, at least on this aggregate level, similar rates of post-election nonresponse. Notably, nonresponse rates were highest in 2009, dropping substantially in 2013 but gradually rising to about 5-6% per group by 2021.

To assess on a descriptive level whether these patterns of post-election nonresponse are accompanied by significant sample distortions, I examine the differences in pre- and post-electoral sample compositions by calculating the difference in means between post-election respondents (R) and nonrespondents (NR) for various variables per election in Figure 3.3. Note that all factors were observed in the closest pre-election wave, serving as proxies for (non)respondents' true post-election views, given the lack of information from nonrespondents. Statistically significant differences ($p < 0.05$) in means are highlighted by bolder colors.

[38]These patterns prove robust to the other two winner-loser operationalizations as well as to alternative categorizations relying on party identification instead of vote intention.

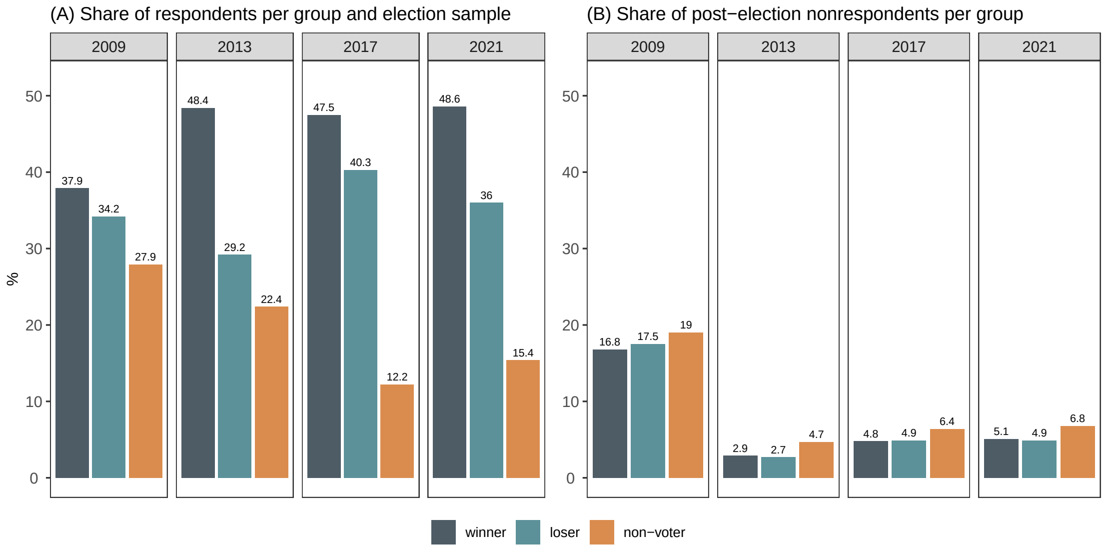

Figure 3.2: Distribution of winners, losers and non-voters across the four elections (A) and the share of post-election nonrespondents per group (B).

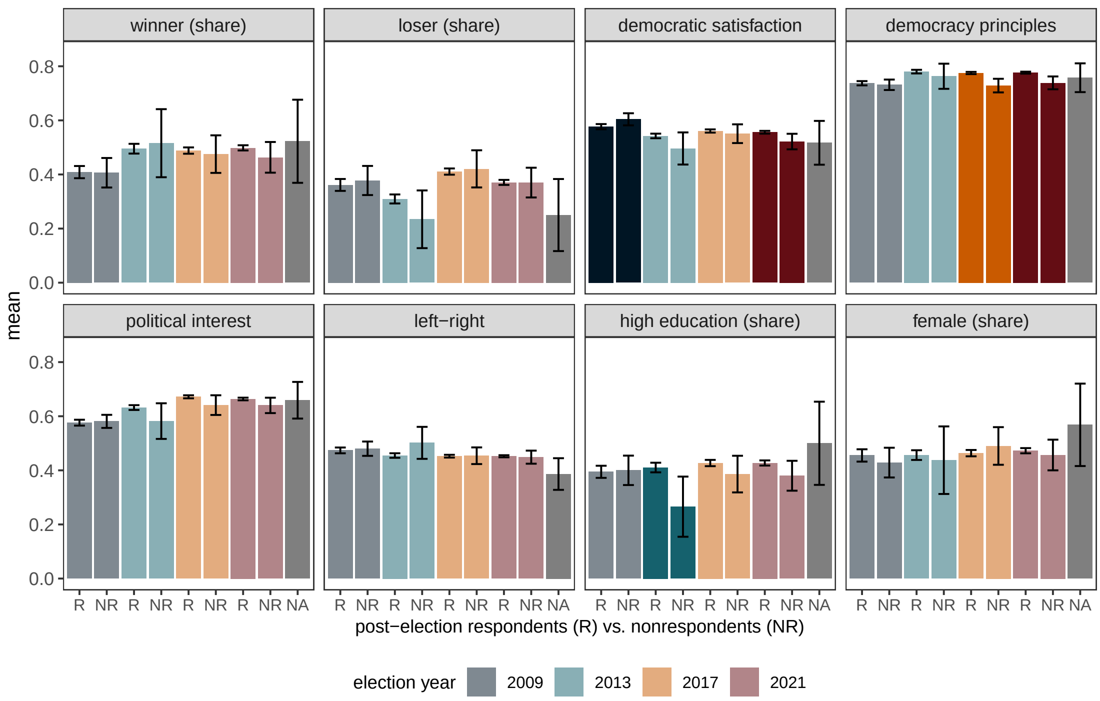

Figure 3.3: Differences between post-election respondents (R) and nonrespondents (NR) per election year. Statistically significant differences ($p < 0.05$) are highlighted by bolder colors.

I observe few systematic differences between respondents and nonrespondents. In 2021, post-election respondents have significantly higher levels of democratic satisfaction and support for democratic principles. A similar trend appears in 2017 for support of democratic principles, while respondents after the 2013 election are more educated than nonrespondents. In contrast, nonrespondents in 2009 show higher pre-election democratic satisfaction than respondents. Other comparisons reveal no statistically significant differences.

These findings suggest that differences in pre- and post-election samples are systematic only for specific years and variables, primarily related to factors already known to influence survey participation, such as educational attainment (Groves et al., 2004; Tourangeau et al., 2010), democratic satisfaction, and support for democratic principles (Silber et al., 2022). Thus, while no new patterns of sample distortion arise from post-election nonresponse, existing cleavages may be amplified, as post-election samples tend to include more democratically satisfied, supportive, and educated respondents. In the following section, I analyze how these differences may be rooted in winner-loser effects on post-election survey nonresponse.

3.5.1 Study 1: Results

I first identify the factors likely driving pre-election participants to decline participation in the post-election survey wave. Results of the core logistic regression models are reported in Figure 3.4.[39]

Figure 3.4 shows that most of the explanatory variables have no significant impact on post-election nonresponse. Also, there are no systematic effects of winning or losing on nonresponse for most elections; none of the estimates of winning or losing across the four elections reach the 5% level of statistical significance.

My results suggest that the pre-election the level of democratic satisfaction, all else being equal, has a nonresponse-promoting effect only in the 2009 election. Support for democratic principles reduces the likelihood of post-election nonresponse in the 2017

[39] The alternative visualization employing party identification instead of vote choice as the reference to identify winners and losers is presented in Figure 3.C.1 in the Appendix. Here, detailed regressions results for the models employing vote intention-based winner-loser proxies are reported in Table 3.C.1 (2009), Table 3.C.3 (2013), Table 3.C.5 (2017) and Table 3.C.7 (2021). The alternative model output based on party identification-based winner-loser proxies is reported in Table 3.C.2 (2009), Table 3.C.4 (2013), Table 3.C.6 (2017) and Table 3.C.8 (2021).

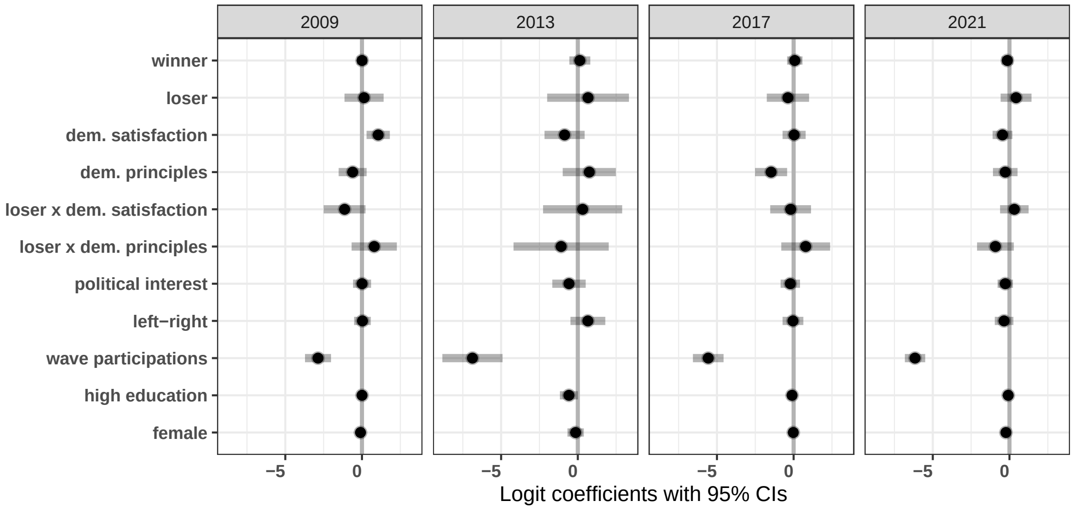

Figure 3.4: Coefficient plot of the logistic regression results per election. Depicted are the respective effects on post-election survey nonresponse with 95% confidence intervals.

election, however not reaching statistical significance in the other cases. Across all cases, I find that democratic satisfaction and support, by tendency and with exceptions, mitigate nonresponse, although not being significantly different from zero. When controlling for the number of survey participations during the election year, the effects of democratic satisfaction and support for democratic principles shrink nearly to zero, suggesting they are largely outweighed by consistent survey participation. The impact of prior wave participation is larger than other coefficients, indicating that the more consistently respondents participate throughout the election year, the more likely they are to also join the post-election survey wave.

Interacting losing with two pro-democracy variables shows mixed effects. In 2009, high democratic satisfaction appears to reduce the effect of losing on post-election nonresponse, but this effect is near zero in subsequent elections. Support for democratic principles also varies as a moderating factor: it tends to reduce the impact of losing in 2013 and 2021 but seems to amplify it in 2009 and 2017. However, these interactions are not statistically significant and should be interpreted cautiously.[40]

[40] As with losing, I test whether democratic satisfaction and support for democratic principles moderate the effect of winning on post-election nonresponse. The patterns are similar to the ones I find on the effect of losing: results indicate no systematic moderating effect of democratic satisfaction or support on winning. Detailed results of this robustness test are shown in Table 3.D.1 and Table 3.D.2 in the Appendix.

Overall, this implies that democratic satisfaction and support for democratic principles have a stronger influence on post-election nonresponse than winning or losing. These results are robust against different winner-loser operationalizations and model specifications.[41]

To directly test the hypothesized differences in post-election nonresponse probabilities between winners and losers and to identify different types of losers (and winners), I simulate *first differences* in predicted nonresponse probabilities. I derive group differences by simulating various scenarios for selected subgroups and calculating differences in their predicted nonresponse probabilities. For the scenario distinguishing between graceful and sore losers (and winners), I define graceful respondents as those exhibiting high levels of democratic satisfaction, support for democratic principles, and political interest. To avoid too extreme scenarios, these variables are fixed at 90% of their maximum (i.e. at 0.9). For sore winners and losers, they are fixed at 0.1. The simulation results are shown in Figure 3.5.[42]

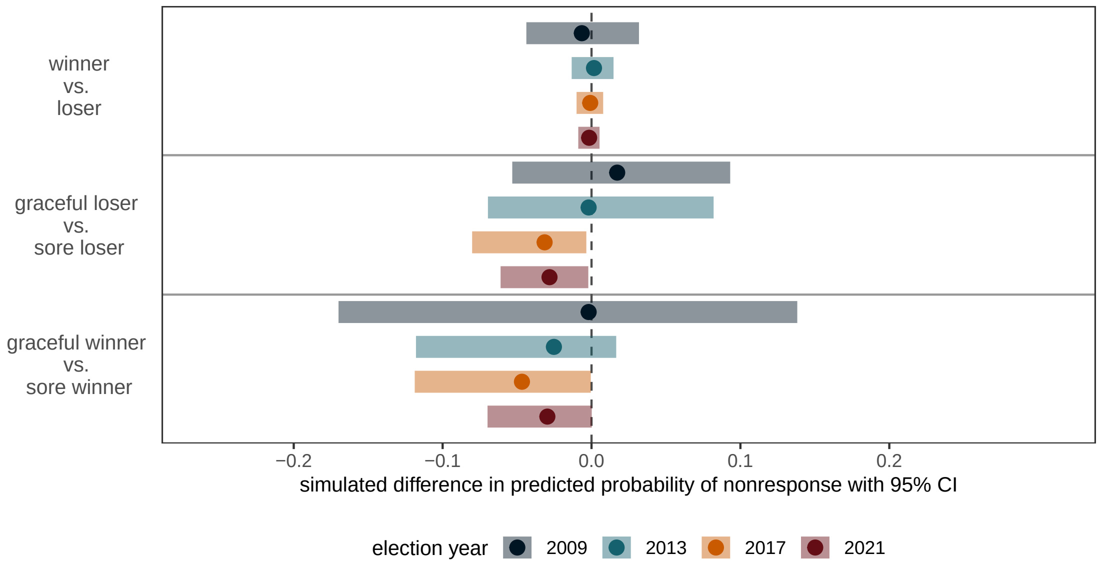

Figure 3.5: Simulated first differences in predicted probabilities of post-election nonresponse for different subgroups. Estimations are based on the winner-loser operationalization relying on vote choice.

[41]Detailed results of these robustness checks are included in Table 3.C.1 - Table 3.C.8 in the Appendix.
[42]Results employing party identification as reference for the winner-loser indicators are shown in Figure 3.E.1 in the Appendix. Further results employing the alternative winner-loser operationalizations are presented in Figure 3.E.2 of that section.

Figure 3.5 shows differences in nonresponse probabilities, resembling direct tests of H1 (top) as well as H2a and H2b (middle) hypothesis. As robustness test, the equivalent of Hypotheses 2a and 2b are tested for different types of winners (bottom). The point estimates represent differences in predicted probability of post-election nonresponse of the first group compared to the second.

Across all elections, winners and losers tend to be, on average, equally likely to refuse post-election participation. Crucially, these results imply that winning or losing do not make decisive differences for the immediate post-election likelihood of nonresponse when all other factors are held constant.

Proceeding from the assessment of between-group heterogeneity to within-group heterogeneity among losers and winners, I find differences in post-election nonresponse propensity: In 2017 and 2021, graceful losers are systematically less likely to refuse participation in the post-election wave compared to their sore counterparts. A similar difference prevails between democratically supportive and less supportive winners, although less systematic than in the losers' case. These differences range from -0.03 to -0.05, indicating that individuals with high democratic satisfaction and support for democratic principles have a 3-5 pp lower probability of post-election nonresponse compared to those with lower levels of these characteristics. In the 2009 and 2013 elections, no systematic within-group heterogeneity of this kind is found, neither for losers nor winners.

Substantively, this indicates that low levels of democratic satisfaction and support contribute to nonresponse among both losers and winners in more recent elections. Although the differences are relatively small, these effects could lead to sample disproportionalities, which would be more significant if these isolated effects persisted longer after election day. To shed light on this, I assess the durability of the nonresponse-promoting effects.

3.5.2 Study 2: Results

The results of Cox proportional hazard models reveal how winning, losing and other selected factors affect the duration of post-election nonresponse before returning to participation again.[43] Based on these models, I calculate per post-election survey wave the

[43] Detailed estimation results are shown in Table 3.F.1 and, for the party identification-based winner-loser operationalization, in Table 3.F.2 in the Appendix.

predicted probabilities of observing durable nonresponse until this respective wave for the subgroups already adopted in Figure 3.5, facilitating the direct assessment of the three hypotheses. The same variables as included in the simulations are varied to construct these cases (for details, see Section 3.5.1); all other factors are held at their mean. Figure 3.6 visualizes the predicted probabilities of durable nonresponse.

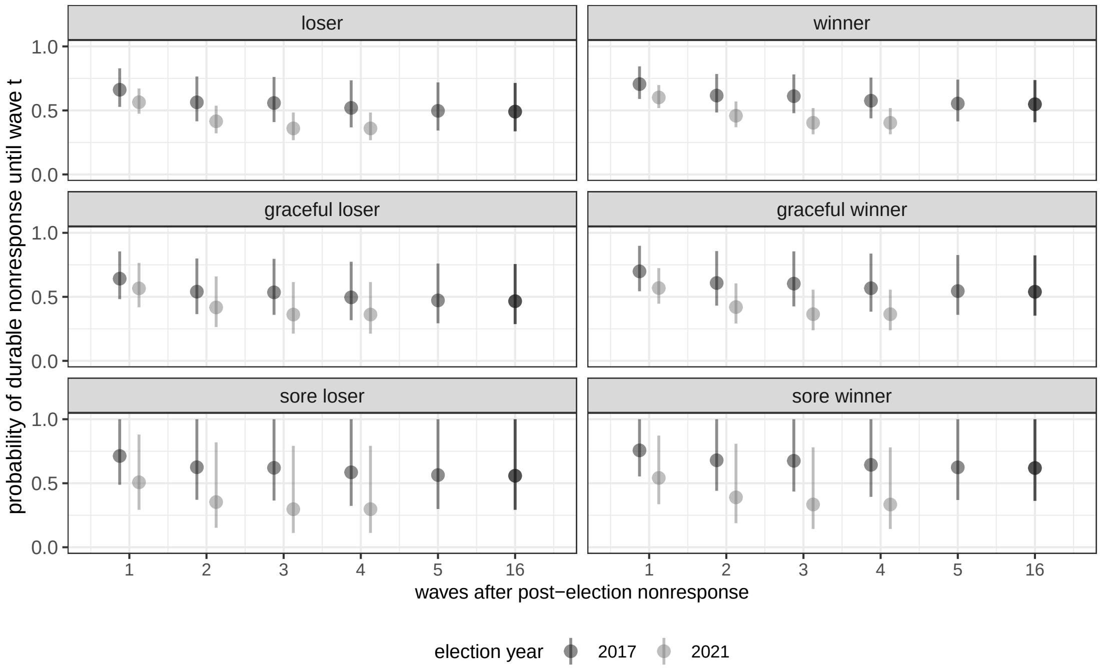

Figure 3.6: Predicted probabilities of durable nonresponse until each survey wave after dropping out of the panel in the 2017 and 2021 post-election survey wave. Note here that no changes in survival probability occurred between wave 5 and 16 in 2017, therefore no probabilities for these waves are included in the plot.

In 2017, I can trace nonresponse over 16 survey waves. For 2021, four post-election survey waves were available. The probability of remaining unresponsive decreases over waves after both elections and for all subgroups. Considering that it is common for panel respondents to skip individual waves without fully dropping out of the panel, it is unsurprising that post-election nonrespondents tend to rejoin the panel after some time.

For both elections, the results for winners and losers are nearly identical, suggesting that their similar propensity for immediate post-election nonresponse persists over time. More notable differences emerge between graceful and sore nonrespondents: In 2017, sore losers (and winners) are more likely to remain unresponsive across all survey waves, though this difference is less pronounced in the 2021 election. However, the upper limits of the confidence intervals still include comparably high probabilities for durable nonresponse.

Although not consistently statistically significant, these findings suggest that sore losers or winners tend to experience more durable nonresponse than their graceful counterparts, indicating that democratically satisfied and supportive nonrespondents are more likely to return to participation sooner than those who are less satisfied. This tendency is more evident in 2017 than in 2021. Thus, winning or losing has a smaller impact on the durability of post-election nonresponse compared to democratic satisfaction and support for democratic principles.

3.6 Discussion and Conclusion

When do political events like elections affect survey participation, and for whom? What does this imply for post-election sample composition? In this paper, I examined these questions from various perspectives. Focusing on four German federal elections between 2009 and 2021 as ideal test cases, I leveraged panel data from the German Longitudinal Election Study (GLES) to test three hypotheses. First, I hypothesized that electoral winners would have lower post-election nonresponse rates than losers due to their receipt of participation-motivating cues from being on the winning side (H1). Second, I explored heterogeneity within the group of losers, proposing that attitudes toward the democratic system offer more explanatory power than simply losing. Specifically, I hypothesized that *sore* losers – those dissatisfied with democracy – would be more likely to exhibit post- election nonresponse than *graceful* losers (H2a). Alternatively, I considered that both types of losers might exhibit similar rates of post-election nonresponse, as frustration could motivate some to express their views (H2b).

The findings show that, on average, winners and losers are equally likely to switch to post-election nonresponse, without clear differences in the occurrence and persistence of this nonresponse. These results do not support the relationship predicted by Hypothesis 1.

However, I observed more pronounced differences within the group of electoral losers. While those with high satisfaction with democracy and strong support for democratic principles remain comparatively responsive, those who reject democratic outcomes are more prone to (durable) post-election nonresponse. A similar pattern is observed among winners, suggesting that democratic principles explain post-election survey nonresponse more effectively than simply winning or losing. This implies that intra-group differences among both winners and losers have a greater impact on survey participation than inter-group differences.

Nevertheless, this within-group heterogeneity is not consistently observable across all four elections. Systematic differences in post- election nonresponse between graceful and sore losers appear in 2017 and 2021, but not in 2009 and 2013. This suggests that, in earlier years, the mechanisms at play may align more closely with the alternative outcome proposed by Hypothesis 2b while for 2017 and 2021, the results lend support for Hypothesis 2a. One possible explanation, although speculative, is that the nature of electoral competition and, e.g., the surprise element of the election outcome may have played a stronger role in shaping nonresponse patterns in the two more recent elections compared to earlier ones. Providing partial support for both hypotheses, these findings highlight the need for further research on the underlying mechanisms.

Notably, past survey participation behavior has the largest effect on post-election nonresponse and its persistence, outweighing winner-loser dynamics. This suggests that general attitudes toward surveys and past participation patterns are more influential than election outcomes in determining post-election participation.

These findings suggest that election outcomes alone are unlikely to significantly influence survey participation. This is reassuring for the quality of post-election survey data, as there is no increased risk of sample distortions from winner-loser dynamics; also considering that the number of post-election nonrespondents remains small, averaging 5% of pre-election respondents.[44] However, existing patterns of sample imbalances, such as the overrepresentation of educated and politically engaged individuals, could be more pronounced in post-election samples if nonresponse-prone predispositions align with certain electoral outcomes.

Several factors may explain the limited impact of winner-loser dynamics on post-election survey participation. Stronger determinants, like political attitudes and past survey participation, may overshadow these effects. Additionally, the subgroup that is neither

[44] For details, see Figure 3.2. The 2009 election marks an outlier by amounting to approximately 20 % of respondents becoming unresponsive in the post-election wave.

consistently responsive nor unresponsive but influenced by election outcomes is likely small. Most people's decision to participate in post-election surveys is only marginally affected by an election outcome. This likely causes these effects to diminish when averaged across respondents. The use of panel data, where all respondents demonstrated at some point a baseline level of responsiveness as well as the GLES-sample's overrepresentation of political involvement further yield conservative estimates of winner-loser effects. As a result, this analysis focuses on generally responsive individuals, excluding those for whom winning or losing may more severely impact participation. Collectively, these factors likely contribute to the observed effect sizes.

By linking the analysis of attitudinal responses to political outcomes with methodological questions surrounding nonresponse bias, this paper contributes to the literature on electoral behavior and survey methodology, paving the way for further research at the intersection of politics and survey participation.

Future research could extend this contribution by addressing several limitations. First, obtaining information about nonrespondents is inherently challenging. To address this, I relied on the best available approximations of vote choice and winner-loser status. However, these proxies introduce uncertainty, particularly compared to post-election measures such as stated vote choice. Additionally, my use of objective measures of winning and losing may not capture individuals' subjective perceptions of electoral outcomes. Developing alternative measures to better reflect perceived winner-loser status could improve the analyses given current data limitations. Furthermore, the GLES panel respondents are not fully representative of the German population; they tend to be more educated and politically engaged. This may lead to more conservative estimates of winner-loser effects on post-election nonresponse. A replication based on a random sample would yield valuable insights and increase the generalizability of the results. Lastly, this study focuses on just four German elections. Future research could expand the temporal and geographical scope. For the German case, additional elections and, e.g., district-level differences in winning and losing could be analyzed. Exploring whether these patterns hold similarly in other countries with, e.g., majoritarian systems, different political and partisan landscapes, and potentially larger winner-loser gaps could further test the generalizability of the findings across different contexts.

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Appendix

3.A Overview of GLES Data Structure

This section provides an overview of the data and recruitment structure of the GLES Panel data used in the analyses. For the 2009 and 2013 cases, I use data from the respective campaign panels. From the 2017 election onwards, I use data from the GLES panel 2016-2021. In Figure 3.A.1, the structure of the three panel datasets and their connection is displayed. The blue highlighted boxes indicate the waves used for the analyses of Study 1, assessing switches from pre-election response to post-election nonresponse. The boxes highlighted in green indicate all survey waves that are additionally included in the survival analysis of Study 2.

The 2009 campaign panel comprises six survey waves in total and is based on a sample drawn from an opt-in online access panel (provided by Respondi AG) with quotas for gender, age, and education. The target population are all German citizens who were eligible to vote in the 2009 German federal elections.

The 2013 campaign panel consists of six survey waves and is based on a newly recruited sample drawn from an opt-in online access panel (provided by Respondi AG) with quotas for gender, age, and education as well as re-invited respondents of the 2009 campaign panel. The target population comprises all German citizens who were eligible to vote in the 2013 German federal elections.

From 2016 onwards, all GLES panel waves are subsumed by the GLES panel. This is based on re-invited respondents of the 2013 campaign panel as well as newly recruited participants. Again, these are drawn from an opt-in online access panel (provided by Respondi AG) with quotas for gender, age, and education with a target population of people eligible to vote in the 2017 German federal election. From wave 5 and wave 14 onwards, refresher samples (sampled by the same criteria as applied for the initial recruitment) are included. For additional details on all datasets, see the respective study descriptions provided by GESIS (2009, 2013, 2017 and 2021).

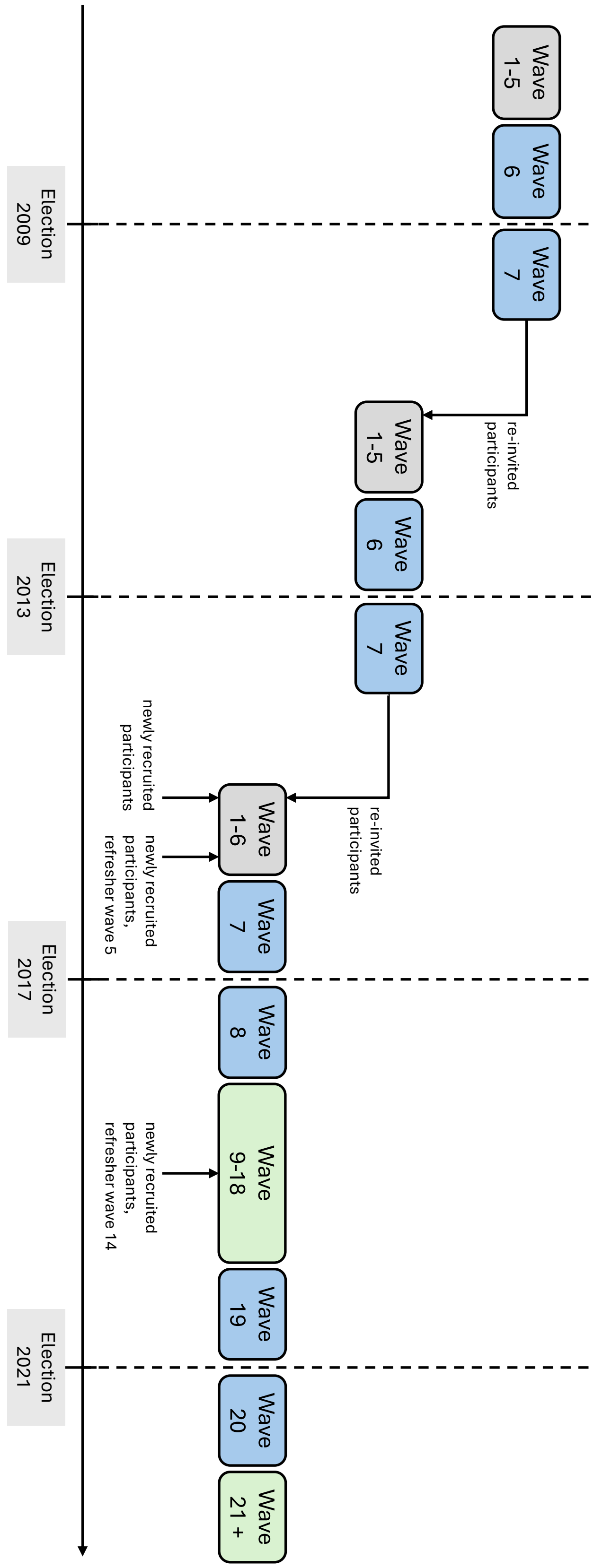

Figure 3.A.1: Overview of GLES panel data and recruitment structure.

3.B Winner-Loser Operationalizations

Table 3.B.1: Winner and loser parties according to each operationalization per election.

	Winner-loser operationalization		
	Vote share gains compared to last election	**Party in government**	**Vote share gains compared to pre-election polls**
		2009	
winner	FDP, Greens, Die Linke	CDU/CSU, FDP	FDP, Die Linke
loser	CDU/CSU, SPD	SPD, Greens, Die Linke, others	CDU/CSU, SPD, Greens
		2013	
winner	CDU/CSU, SPD, AfD	CDU/CSU, SPD	CDU/CSU, AfD
loser	FDP, Greens, Die Linke	FDP, Greens, Die Linke, AfD, others	SPD, FDP, Greens, Die Linke
		2017	
winner	FDP, Greens, Die Linke, AfD	CDU/CSU, SPD	Greens, FDP, AfD
loser	CDU/CSU, SPD	FDP, Greens, Die Linke, AfD, others	CDU/CSU, SPD, Die Linke
		2021	
winner	SPD, FDP, Greens	SPD, Greens, FDP	CDU/CSU, SPD, FDP
loser	CDU/CSU, Die Linke, AfD	CDU/CSU, Die Linke, AfD, others	Greens, Die Linke, AfD

Considering the variety of established winner-loser definitions and the differences in election outcomes and government compositions in the cases analyzed, I test the main operationalization employed in the paper against two alternatives: First, voting for a party that enters government (= winner) or ends up in opposition (= loser). Second, given that voters might be more aware of a party's electoral performance in contrast to their most recent polling results instead of their performance in the previous election, I include the operationalization of voting for a party that has gained (= winner) or lost (= loser) vote share compared to the 20-day polling average prior to election day. Here, polling averages are calculated based on election polls scraped from the website wahlrecht.de. Included pollsters are Allensbach, Kantar, Forsa, Forschungsgruppe Wahlen, GMS, Infratest dimap, and YouGov (only for 2017 and 2021). In the 20-day period prior to the 2009 election, 24 polls are available. For 2013, 28 polls are averaged. 21 polls are available for 2017, and 18 for 2021. An overview of winner and loser parties per election according to these three operationalizations is shown in Table 3.B.1. Note here that the category "others" subsumes all smaller parties that are commonly presented as one group in election results and media communication. This category is only included for the operationalization relying on parties (not) entering government.

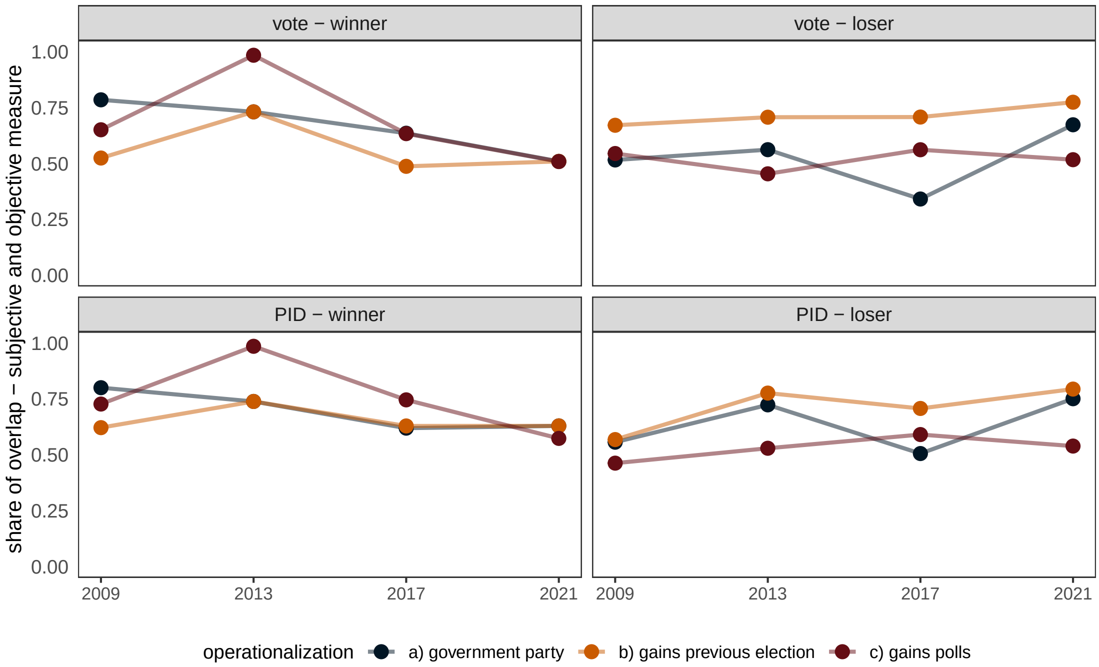

Figure 3.B.1: Overlap of the three objective winner-loser operationalizations with subjective measures.

When comparing all three objective indicators of winning and losing to the subjective indicators of perceived winner- and losership, the core operationalization based on voting for a party that has gained (= winner) or lost (= loser) vote share compared to the previous election has the largest overlap. Such an overlap is desirable, as, arguably, subjective indicators of perceived winning and losing would be the most expressive proxies. However, given that reporting such perceptions requires post-election survey participation, the data availability gap for post-election nonrespondents does not allow me to employ such a subjective operationalization. Extensive tests demonstrate that among the three different objective winner-loser indicators presented above, the operationalization based on gains and losses compared to PID - winner PID - loser the previous elections is the closest available proxy of subjective measures of perceived winning and losing. For an overview of alternative objective winner-loser operationalizations and their overlap with subjective measures of winning and losing, see Figure 3.B.1. Note here that quantities are estimated for the group of post-election respondents only, as subjective measures of winning and losing are by design not available for post-election nonrespondents. Also note that there are no subjective measures available for the AfD in 2009 and 2013, hence voting for or identifying with the AfD is not accounted for in these two cases.

For the perceived winner- and losership, I rely on items included in the GLES panel that directly query respondents whether they perceive selected parties as winners (rather winners) or losers (rather losers). I define subjective winners as voters of a party they rated as winners of the election (indicating that this party belongs "rather to the winners" or "to the winners") in either the immediate post-election survey wave or, if they did not participate in that wave, in the second wave after the election. Subjective losership is coded analogously. Even though these measures may be reliable indicators of self-perceived winner- and losership (Gärtner et al., 2020), they are only surveyed in the two waves post-election. Obtaining information on subjective, self-perceived winner- and losership hence requires post-election survey participation, which contradicts my analytical aim. The results in Figure 1 demonstrate that among pre- and post-election participants, there is, in general, large overlap of the applied objective measures and their subjective perception of winners and losers of the election. This is especially true for the objective winner-loser operationalizations relying on gains and losses in vote share. Larger deviations prevail when comparing the winner-loser operationalization relying on government participation compared to subjective perceptions of winning and losing.

3.C Tables and Figures Study 1

This section provides detailed regression results and robustness test for Study 1. In all regression tables, the operationalization (a) refers to the winner-loser operationalization referring to vote share gains or losses compared to the previous election. Operationalization (b) defines winners as those who voted for or identified with a party that entered the government, losers as those who voted for or identified with an opposition party. Operationalization (c) employs an analogous definition as in (a), however using the 20-day polling average prior to election day as the benchmark for vote share gains or losses.

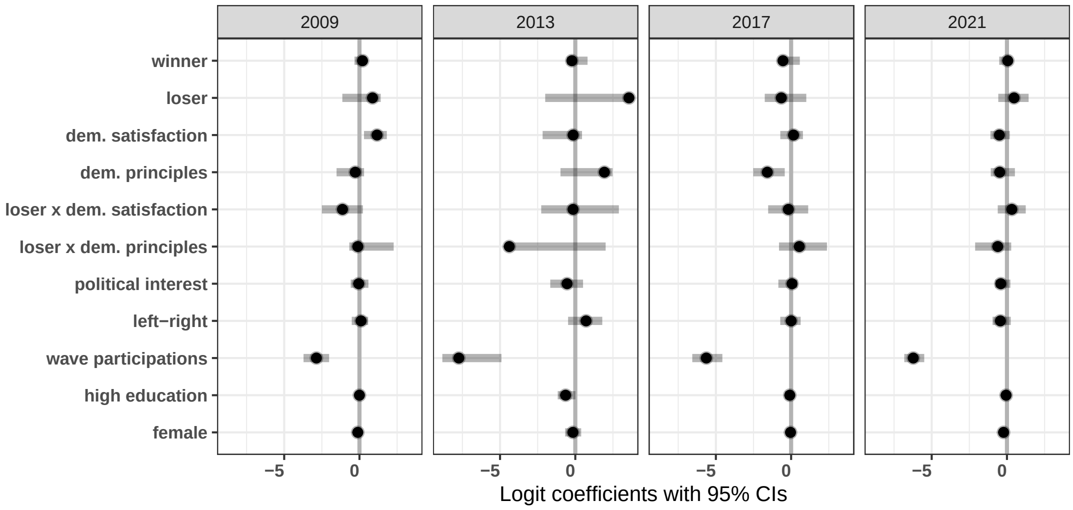

Figure 3.C.1: Logistic regression results: Post-election nonresponse employing a winner-loser operationalization based on party identification.

Table 3.C.1: Logistic regression: Post-election survey nonresponse after the German federal election 2009 — vote intention as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.150	-0.003	0.008	-0.360***	-0.350**	-0.268	-0.237*	-0.123	-0.040
	(0.123)	(0.019)	(0.174)	(0.137)	(0.175)	(0.207)	(0.139)	(0.167)	(0.188)
Loser	-0.099	0.001	0.137	-0.233*	0.188	0.317	-0.069	0.059	0.080
	(0.125)	(0.078)	(0.651)	(0.124)	(0.567)	(0.622)	(0.117)	(0.572)	(0.624)
Dem. satisfaction		0.106**	1.059***		0.993**	1.054**		0.895**	1.067**
		(0.042)	(0.387)		(0.448)	(0.492)		(0.396)	(0.433)
Dem. principles		-0.068	-0.608		-0.325	-0.327		-0.584	-0.651
		(0.052)	(0.466)		(0.485)	(0.536)		(0.458)	(0.518)
Political interest			0.007			0.015			0.008
			(0.300)			(0.298)			(0.300)
Left-right			0.032			0.253			0.029
			(0.278)			(0.318)			(0.274)
Wave parts.			-2.866***			-2.863***			-2.869***
			(0.433)			(0.434)			(0.433)
High education			0.004			0.005			0.004
			(0.130)			(0.130)			(0.130)
Female			-0.096			-0.091			-0.099
			(0.130)			(0.130)			(0.130)
Loser × dem. sat.		-0.124	-1.133		-0.465	-0.386		-0.862	-0.897
		(0.083)	(0.696)		(0.591)	(0.635)		(0.607)	(0.649)
Loser × dem. princ.		0.098	0.799		-0.054	-0.138		0.667	0.694
		(0.091)	(0.752)		(0.681)	(0.730)		(0.670)	(0.718)
Constant	-1.452***	0.143***	0.679	-1.324***	-1.915***	0.450	-1.452***	-1.803***	0.710
	(0.092)	(0.043)	(0.584)	(0.101)	(0.377)	(0.604)	(0.092)	(0.378)	(0.610)
Observations	2,774	2,395	2,150	2,774	2,395	2,150	2,774	2,395	2,150
Log Likelihood	-1,291.115	-928.284	-870.727	-1,288.402	-1,007.175	-870.952	-1,290.329	-1,007.595	-870.959
AIC	2,588.231	1,870.569	1,765.454	2,582.804	2,028.350	1,765.905	2,586.659	2,029.190	1,765.918

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.C.2: Logistic regression: Post-election survey nonresponse after the German federal election 2009 – party identification as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	0.198	0.024	0.202	0.064	-0.004	-0.012	0.205	0.221	0.237
	(0.152)	(0.020)	(0.183)	(0.153)	(0.166)	(0.194)	(0.173)	(0.180)	(0.201)
Loser	0.118	0.094	0.867	0.156	0.499	1.080*	0.132	0.459	0.627
	(0.138)	(0.073)	(0.624)	(0.142)	(0.572)	(0.632)	(0.133)	(0.568)	(0.626)
Dem. satisfaction		0.115**	1.167***		0.878**	1.101**		1.164***	1.374***
		(0.045)	(0.419)		(0.425)	(0.475)		(0.423)	(0.469)
Dem. principles		-0.038	-0.279		-0.404	-0.123		-0.603	-0.513
		(0.055)	(0.498)		(0.450)	(0.503)		(0.483)	(0.548)
Political interest			-0.051			-0.081			-0.035
			(0.295)			(0.295)			(0.295)
Left-right			0.104			0.241			0.076
			(0.281)			(0.328)			(0.275)
Wave parts.			-2.860***			-2.913***			-2.871***
			(0.433)			(0.435)			(0.433)
High education			-0.003			0.008			0.012
			(0.131)			(0.130)			(0.130)
Female			-0.112			-0.093			-0.099
			(0.130)			(0.130)			(0.130)
Loser $\times$ dem. sat.		-0.128*	-1.128*		-0.538	-0.632		-1.271**	-1.309**
		(0.076)	(0.646)		(0.581)	(0.628)		(0.598)	(0.643)
Loser $\times$ dem. princ.		-0.012	-0.109		-0.041	-0.657		0.552	0.369
		(0.087)	(0.727)		(0.680)	(0.739)		(0.668)	(0.721)
Constant	-1.839***	0.103**	0.279	-1.819***	-2.024***	0.190	-1.839***	-2.051***	0.339
	(0.109)	(0.046)	(0.600)	(0.114)	(0.372)	(0.594)	(0.109)	(0.404)	(0.625)
Observations	2,472	2,395	2,150	2,472	2,395	2,150	2,472	2,395	2,150
Log Likelihood	-1,046.146	-927.287	-869.903	-1,046.36	-1,008.135	-870.125	-1,046.21	-1,006.135	-869.422
Akaike Inf. Crit.	2,098.292	1,868.575	1,763.807	2,098.715	2,030.269	1,764.251	2,098.418	2,026.271	1,762.843
Note:									*p<0.1; **p<0.05; ***p<0.01

Table 3.C.3: Logistic regression: Post-election survey nonresponse after the German federal election 2013 - vote intention as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.491**	-0.004	0.131	-1.018***	-0.679*	-0.340	-0.743***	-0.305	-0.079
	(0.219)	(0.007)	(0.348)	(0.263)	(0.356)	(0.512)	(0.285)	(0.375)	(0.434)
Loser	-0.551**	-0.021	0.668	-1.042***	-0.503	0.236	-0.429**	-0.696	1.198
	(0.251)	(0.026)	(1.360)	(0.269)	(0.981)	(1.203)	(0.211)	(0.938)	(1.178)
Dem. satisfaction		-0.027*	-0.862		-1.084*	-0.913		-1.362*	-0.917
		(0.014)	(0.668)		(0.626)	(0.737)		(0.724)	(0.861)
Dem. principles		-0.010	0.748		0.126	0.993		-0.458	1.344
		(0.016)	(0.884)		(0.757)	(0.979)		(0.822)	(1.156)
Political interest			-0.574			-0.355			-0.617
			(0.556)			(0.581)			(0.557)
Left-right			0.657			0.623			0.866
			(0.579)			(0.560)			(0.623)
Wave parts.			-6.870***			-6.786***			-6.955***
			(1.002)			(1.005)			(1.002)
High education			-0.580*			-0.560*			-0.591**
			(0.300)			(0.300)			(0.299)
Female			-0.143			-0.126			-0.152
			(0.273)			(0.272)			(0.273)
Loser × dem. sat.		0.012	0.315		0.193	0.388		1.009	0.436
		(0.025)	(1.316)		(1.035)	(1.166)		(1.012)	(1.155)
Loser × dem. princ.		0.001	-1.087		-0.823	-1.275		-0.031	-1.598
		(0.030)	(1.584)		(1.238)	(1.449)		(1.164)	(1.446)
Constant	-3.015***	0.051***	2.880**	-2.489***	-2.546***	2.978**	-3.015***	-2.550***	2.444*
	(0.166)	(0.013)	(1.188)	(0.222)	(0.556)	(1.252)	(0.166)	(0.607)	(1.285)
Observations	3,619	3,408	3,020	3,619	3,408	3,020	3,619	3,408	3,020
Log Likelihood	-516.913	1,623.018	-286.778	-512.747	-368.174	-286.089	-516.212	-370.940	-286.140
Akaike Inf. Crit.	1,039.826	-3,232.037	597.557	1,031.494	750.347	596.177	1,038.423	755.881	596.281

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.C.4: Logistic regression: Post-election survey nonresponse after the German federal election 2013 – party identification as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.083	-0.003	-0.409	-0.204	-0.294	-0.579*	-0.441	-0.363	-0.646
	(0.242)	(0.006)	(0.326)	(0.247)	(0.297)	(0.338)	(0.327)	(0.394)	(0.425)
Loser	-0.240	0.051**	2.949**	-0.465	1.718	2.310*	-0.010	1.019	1.923*
	(0.307)	(0.026)	(1.294)	(0.301)	(1.128)	(1.228)	(0.239)	(1.024)	(1.168)
Dem. satisfaction		-0.010	-0.107		-0.509	-0.061		-0.967	-0.020
		(0.012)	(0.663)		(0.613)	(0.683)		(0.742)	(0.809)
Dem. principles		0.010	1.281		0.575	1.285		0.776	1.520
		(0.014)	(0.896)		(0.755)	(0.921)		(0.907)	(1.079)
Political interest			-0.436			-0.300			-0.427
			(0.564)			(0.570)			(0.560)
Left-right			0.454			0.389			0.588
			(0.600)			(0.594)			(0.649)
Wave parts.			-8.140***			-8.152***			-8.145***
			(1.450)			(1.453)			(1.454)
High education			-0.586*			-0.581*			-0.578*
			(0.301)			(0.301)			(0.299)
Female			0.030			0.031			0.023
			(0.273)			(0.273)			(0.273)
Loser $\times$ dem. sat.		0.008	-0.039		0.232	-0.066		1.301	-0.048
		(0.024)	(1.408)		(1.257)	(1.302)		(1.089)	(1.172)
Loser $\times$ dem. princ.		-0.074**	-4.034***		-3.063**	-3.553**		-2.293*	-2.685*
		(0.030)	(1.495)		(1.390)	(1.492)		(1.235)	(1.408)
Constant	-3.666***	0.020*	3.557**	-3.545***	-3.862***	3.652**	-3.666***	-3.942***	3.272**
	(0.185)	(0.011)	(1.577)	(0.192)	(0.590)	(1.575)	(0.185)	(0.700)	(1.627)
Observations	3,891	3,702	3,218	3,891	3,702	3,218	3,891	3,702	3,218
Log Likelihood	-423.884	2,130.546	-287.856	-422.974	-343.304	-288.101	-422.992	-343.457	-288.925
Akaike Inf. Crit.	853.768	-4,247.092	599.713	851.948	700.608	600.203	851.985	700.914	601.850
Note:									*p<0.1; **p<0.05; ***p<0.01

Table 3.C.5: Logistic regression: Post-election survey nonresponse after the German federal election 2017 - vote intention as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.313**	0.003	0.076	-0.411**	-0.202	-0.181	-0.238**	-0.049	-0.083
	(0.128)	(0.007)	(0.256)	(0.182)	(0.319)	(0.406)	(0.108)	(0.165)	(0.180)
Loser	-0.293**	-0.025	-0.372	-0.397**	0.148	0.027	-0.358***	-0.168	0.012
	(0.131)	(0.020)	(0.704)	(0.178)	(0.585)	(0.695)	(0.101)	(0.609)	(0.677)
Dem. satisfaction		-0.004	0.031		-0.084	-0.161		0.327	0.443
		(0.010)	(0.384)		(0.471)	(0.528)		(0.395)	(0.439)
Dem. principles		-0.066***	-1.470***		-1.440***	-0.844		-1.865***	-1.401**
		(0.014)	(0.534)		(0.550)	(0.603)		(0.517)	(0.606)
Political interest			-0.216			-0.163			-0.104
			(0.323)			(0.325)			(0.325)
Left-right			-0.044			-0.042			0.028
			(0.345)			(0.345)			(0.370)
Wave parts.			-5.568***			-5.585***			-5.551***
			(0.510)			(0.511)			(0.517)
High education			-0.103			-0.100			-0.101
			(0.154)			(0.154)			(0.156)
Female			-0.022			-0.020			-0.011
			(0.151)			(0.151)			(0.152)
Loser × dem. sat.		-0.002	-0.196		-0.071	0.242		-0.720	-0.764
		(0.018)	(0.679)		(0.590)	(0.656)		(0.575)	(0.626)
Loser × dem. princ.		0.035	0.787		-0.332	-0.525		0.474	0.299
		(0.023)	(0.813)		(0.726)	(0.800)		(0.743)	(0.801)
Constant	-2.677***	0.082***	2.977***	-2.559***	-2.192***	2.877***	-2.769***	-2.185***	2.784***
	(0.111)	(0.012)	(0.677)	(0.168)	(0.436)	(0.744)	(0.085)	(0.402)	(0.722)
Observations	11,122	7,691	7,211	11,122	7,691	7,211	10,959	7,605	7,173
Log Likelihood	-2,213.408	2,647.810	-868.592	-2,213.987	-1,036.462	-868.658	-2,028.542	-963.074	-853.850
Akaike Inf. Crit.	4,432.817	-5,281.619	1,761.185	4,433.975	2,086.923	1,761.316	4,063.084	1,940.148	1,731.701

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.C.6: Logistic regression: Post-election survey nonresponse after the German federal election 2017 – party identification as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.372***	-0.014***	-0.544***	-0.424***	-0.420**	-0.375*	-0.355***	-0.490**	-0.585***
	(0.107)	(0.005)	(0.194)	(0.108)	(0.174)	(0.198)	(0.121)	(0.199)	(0.221)
Loser	-0.437***	-0.034*	-0.651	-0.306***	0.001	0.016	-0.430***	-0.391	-0.256
	(0.103)	(0.018)	(0.671)	(0.108)	(0.606)	(0.665)	(0.097)	(0.571)	(0.644)
Dem. satisfaction		0.0004	0.160		0.342	0.318		0.406	0.568
		(0.010)	(0.388)		(0.370)	(0.419)		(0.375)	(0.431)
Dem. principles		-0.062***	-1.581***		-1.707***	-1.278***		-1.939***	-1.583***
		(0.013)	(0.521)		(0.427)	(0.490)		(0.478)	(0.573)
Political interest			0.061			-0.013			0.064
			(0.314)			(0.315)			(0.315)
Left-right			0.005			-0.015			0.211
			(0.352)			(0.349)			(0.361)
Wave parts.			-5.633***			-5.618***			-5.638***
			(0.490)			(0.490)			(0.490)
High education			-0.089			-0.092			-0.091
			(0.150)			(0.150)			(0.150)
Female			-0.041			-0.037			-0.039
			(0.146)			(0.146)			(0.146)
Loser × dem. sat.		-0.0001	-0.183		-0.830	-0.532		-0.767	-0.842
		(0.017)	(0.662)		(0.580)	(0.634)		(0.554)	(0.613)
Loser × dem. princ.		0.029	0.543		-0.025	-0.266		0.565	0.438
		(0.021)	(0.782)		(0.757)	(0.808)		(0.705)	(0.771)
Constant	-2.706***	0.086***	3.218***	-2.719***	-2.141***	2.937***	-2.706***	-1.994***	2.930***
	(0.075)	(0.011)	(0.647)	(0.082)	(0.330)	(0.637)	(0.075)	(0.361)	(0.678)
Observations	12,205	8,445	7,837	12,205	8,445	7,837	12,205	8,445	7,837
Log Likelihood	-2,323.260	3,109.292	-924.087	-2,325.298	-1,090.406	-925.731	-2,323.244	-1,088.756	-923.432
Akaike Inf. Crit.	4,652.519	-6,204.585	1,872.174	4,656.596	2,194.811	1,875.463	4,652.488	2,191.512	1,870.863

Note: *p<0.1; **p<0.05; ***p<0.01

Table 3.C.7: Logistic regression: Post-election survey nonresponse after the German federal election 2021 - vote intention as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.313***	-0.009*	-0.139	-0.334**	-0.339	0.146	-0.171*	-0.106	-0.029
	(0.108)	(0.005)	(0.192)	(0.164)	(0.228)	(0.339)	(0.096)	(0.138)	(0.154)
Loser	-0.365***	0.005	0.431	-0.299*	-0.331	0.365	-0.154	-0.292	-0.077
	(0.114)	(0.015)	(0.514)	(0.164)	(0.435)	(0.549)	(0.104)	(0.498)	(0.539)
Dem. satisfaction		-0.013	-0.454		-0.306	-0.346		-0.512*	-0.441
		(0.009)	(0.329)		(0.338)	(0.388)		(0.284)	(0.315)
Dem. principles		-0.028**	-0.274		-1.170***	-0.570		-1.165***	-0.696*
		(0.011)	(0.409)		(0.385)	(0.465)		(0.330)	(0.392)
Political interest			-0.273			-0.334			-0.291
			(0.256)			(0.257)			(0.258)
Left-right			-0.351			-0.379			-0.259
			(0.309)			(0.307)			(0.320)
Wave parts.			-6.158***			-6.140***			-6.101***
			(0.339)			(0.338)			(0.341)
High education			-0.081			-0.081			-0.087
			(0.130)			(0.129)			(0.131)
Female			-0.231*			-0.233*			-0.273**
			(0.127)			(0.127)			(0.129)
Loser × dem. sat.		0.001	0.318		-0.158	0.063		0.446	0.485
		(0.013)	(0.475)		(0.438)	(0.489)		(0.446)	(0.484)
Loser × dem. princ.		-0.018	-0.916		0.181	-0.205		0.133	-0.114
		(0.017)	(0.612)		(0.536)	(0.609)		(0.600)	(0.638)
Constant	-2.611***	0.066***	2.014***	-2.590***	-2.145***	1.930***	-2.817***	-2.346***	2.138***
	(0.091)	(0.009)	(0.458)	(0.153)	(0.303)	(0.528)	(0.079)	(0.264)	(0.453)
Observations	12,504	11,415	10,644	12,504	11,415	10,644	12,337	11,312	10,605
Log Likelihood	-2,578.360	3,931.857	-1,228.159	-2,581.763	-1,540.039	-1,229.201	-2,441.825	-1,454.903	-1,203.714
Akaike Inf. Crit.	5,162.717	-7,849.710	2,480.318	5,169.525	3,094.077	2,482.402	4,889.650	2,923.805	2,431.429
Note:									*p<0.1; **p<0.05; ***p<0.01

Table 3.C.8: Logistic regression: Post-election survey nonresponse after the German federal election 2021 – party identification as winner-loser reference

	post-election survey nonresponse								
	(a) gains previous election			*(b) government party*			*(c) gains polling average*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner	-0.056	-0.0001	0.054	-0.069	0.024	0.118	-0.134	0.039	0.134
	(0.096)	(0.004)	(0.176)	(0.105)	(0.156)	(0.190)	(0.095)	(0.141)	(0.169)
Loser	-0.138	0.002	0.470	-0.043	0.027	0.536	-0.154	-0.450	-0.138
	(0.099)	(0.014)	(0.502)	(0.103)	(0.430)	(0.497)	(0.104)	(0.487)	(0.533)
Dem. satisfaction		-0.020**	-0.504		-0.607**	-0.381		-0.827***	-0.614**
		(0.008)	(0.332)		(0.305)	(0.357)		(0.269)	(0.309)
Dem. principles		-0.033***	-0.480		-1.176***	-0.561		-1.219***	-0.737*
		(0.010)	(0.412)		(0.355)	(0.436)		(0.311)	(0.380)
Political interest			-0.408			-0.441*			-0.391
			(0.252)			(0.253)			(0.253)
Left-right			-0.435			-0.434			-0.293
			(0.308)			(0.307)			(0.315)
Wave parts.			-6.224***			-6.218***			-6.189***
			(0.333)			(0.333)			(0.332)
High education			-0.057			-0.052			-0.069
			(0.128)			(0.128)			(0.128)
Female			-0.226*			-0.226*			-0.236*
			(0.125)			(0.125)			(0.125)
Loser × dem. sat.		0.009	0.322		0.137	0.087		0.785*	0.683
		(0.012)	(0.465)		(0.419)	(0.469)		(0.439)	(0.483)
Loser × dem. princ.		-0.008	-0.611		-0.012	-0.419		0.166	-0.048
		(0.017)	(0.600)		(0.531)	(0.597)		(0.589)	(0.631)
Constant	-2.878***	0.064***	2.132***	-2.880***	-2.361***	2.070***	-2.820***	-2.207***	2.271***
	(0.073)	(0.008)	(0.450)	(0.082)	(0.274)	(0.462)	(0.073)	(0.245)	(0.438)
Observations	12,337	12,088	11,191	13,218	12,088	11,191	13,218	12,088	11,191
Log Likelihood	-2,442.660	4,414.764	-1,262.787	-2,665.118	-1,576.785	-1,262.566	-2,664.001	-1,575.315	-1,262.938
Akaike Inf. Crit.	4,891.310	-8,815.530	2,549.575	5,336.237	3,167.570	2,549.131	5,334.002	3,164.629	2,549.876
Note:									*p<0.1; **p<0.05; ***p<0.01

3.D Robustness Tests Study 1

This section presents robustness tests for Study 1, testing differences in nonresponse propensities among democratically satisfied or dissatisfied winners as well as between the two democracy variables as moderators of losing's effect on nonresponse.

Table 3.D.1: Logistic regression: Post-election survey nonresponse after the German federal election 2009 and 2013 – robustness: Graceful and sore winners

	post-election survey nonresponse					
	2009			2013		
	(a)	(b)	(c)	(a)	(b)	(c)
Winner	0.473	-0.481	0.630	0.337	0.060	-0.501
	(0.645)	(0.688)	(0.730)	(1.175)	(1.196)	(1.546)
Loser	0.028	0.009	0.050	-0.049	-0.570	0.181
	(0.182)	(0.190)	(0.171)	(0.408)	(0.513)	(0.355)
Democratic satisfaction	0.650	0.749**	0.665*	-0.650	-0.577	-0.570
	(0.429)	(0.360)	(0.389)	(0.802)	(0.791)	(0.637)
Democratic principles	-0.072	-0.404	-0.115	0.528	0.635	0.243
	(0.469)	(0.446)	(0.428)	(1.064)	(1.073)	(0.830)
Political interest	-0.018	0.017	-0.013	-0.560	-0.364	-0.563
	(0.298)	(0.298)	(0.299)	(0.558)	(0.583)	(0.556)
Left-right	0.020	0.252	0.026	0.655	0.618	0.922
	(0.278)	(0.318)	(0.275)	(0.582)	(0.562)	(0.621)
Wave participations	-2.870***	-2.864***	-2.866***	-6.848***	-6.798***	-6.931***
	(0.434)	(0.435)	(0.434)	(1.001)	(1.005)	(1.003)
High education	0.003	0.006	0.004	-0.581*	-0.571*	-0.585*
	(0.130)	(0.130)	(0.130)	(0.301)	(0.300)	(0.300)
Female	-0.092	-0.092	-0.098	-0.133	-0.121	-0.139
	(0.130)	(0.130)	(0.130)	(0.272)	(0.272)	(0.273)
Winner × dem. satisfaction	0.168	0.340	0.031	-0.231	-0.325	-0.497
	(0.639)	(0.728)	(0.691)	(1.149)	(1.143)	(1.473)
Winner × dem. principles	-0.734	0.043	-0.918	-0.141	-0.318	0.871
	(0.743)	(0.800)	(0.832)	(1.444)	(1.450)	(1.883)
Constant	0.528	0.663	0.546	2.931**	3.119**	3.034**
	(0.584)	(0.586)	(0.567)	(1.273)	(1.289)	(1.188)
Observations	2,150	2,150	2,150	3,020	3,020	3,020
Log Likelihood	-871.876	-871.057	-871.629	-286.992	-286.416	-286.630
Akaike Inf. Crit.	1,767.751	1,766.113	1,767.258	597.983	596.833	597.259

Note: *p<0.1; **p<0.05; ***p<0.01
(a) gains compared to previous election
(b) government party
(c) gains compared to polling average

Table 3.D.2: Logistic regression: Post-election survey nonresponse after the German federal election 2017 and 2021 – robustness: Graceful and sore winners

	post-election survey nonresponse					
	2017			2021		
	(a)	(b)	(c)	(a)	(b)	(c)
Winner	0.155	-0.572	0.283	-0.272	-0.032	0.036
	(0.669)	(0.740)	(0.713)	(0.521)	(0.576)	(0.511)
Loser	0.063	-0.219	0.084	-0.096	0.262	-0.096
	(0.278)	(0.378)	(0.260)	(0.187)	(0.327)	(0.188)
Democratic satisfaction	0.008	0.070	-0.182	-0.178	-0.216	-0.175
	(0.492)	(0.385)	(0.401)	(0.294)	(0.293)	(0.304)
Democratic principles	-1.101*	-1.430***	-0.954*	-0.811**	-0.859**	-0.625
	(0.566)	(0.534)	(0.504)	(0.397)	(0.397)	(0.413)
Political interest	-0.239	-0.157	-0.217	-0.257	-0.328	-0.254
	(0.322)	(0.326)	(0.322)	(0.257)	(0.258)	(0.257)
Left-right	-0.059	-0.041	0.005	-0.358	-0.371	-0.298
	(0.344)	(0.346)	(0.362)	(0.309)	(0.307)	(0.312)
Wave participations	-5.568***	-5.584***	-5.568***	-6.140***	-6.147***	-6.121***
	(0.510)	(0.511)	(0.510)	(0.338)	(0.338)	(0.337)
High education	-0.106	-0.101	-0.106	-0.079	-0.082	-0.082
	(0.154)	(0.154)	(0.154)	(0.130)	(0.130)	(0.129)
Female	-0.021	-0.019	-0.023	-0.233*	-0.231*	-0.240*
	(0.151)	(0.151)	(0.150)	(0.127)	(0.127)	(0.127)
Winner × dem. satisfaction	-0.042	-0.250	0.407	-0.337	-0.296	-0.225
	(0.638)	(0.679)	(0.630)	(0.500)	(0.500)	(0.496)
Winner × dem. principles	-0.104	0.733	-0.565	0.409	0.480	-0.122
	(0.797)	(0.813)	(0.838)	(0.621)	(0.622)	(0.614)
Constant	2.753***	3.169***	2.692***	2.260***	2.060***	2.094***
	(0.698)	(0.713)	(0.675)	(0.455)	(0.508)	(0.468)
Observations	7,211	7,211	7,211	10,644	10,644	10,644
Log Likelihood	-869.065	-868.476	-868.660	-1,229.052	-1,228.845	-1,229.213
Akaike Inf. Crit.	1,762.130	1,760.952	1,761.320	2,482.105	2,481.689	2,482.426

Note: *p<0.1; **p<0.05; ***p<0.01
(a) gains compared to previous election
(b) government party
(c) gains compared to polling average

Table 3.D.3: Logistic regression: Post-election survey nonresponse after the German federal election 2009, 2013, 2017 and 2021 – robustness: Democracy variables (Note: Models are based on the winner-loser operationalization based on voting for a party that has gained/lost vote share compared to the previous election)

	post-election survey nonresponse							
	2009		2013		2017		2021	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner	-0.025	0.033	-0.051	-0.019	-0.050	0.098	-0.129	-0.227
	(0.171)	(0.152)	(0.309)	(0.328)	(0.192)	(0.222)	(0.190)	(0.181)
Loser	0.520	-0.404	-0.673	0.626	0.138	-0.464	-0.260	0.495
	(0.444)	(0.507)	(0.675)	(1.266)	(0.353)	(0.556)	(0.276)	(0.479)
Democratic satisfaction	0.935**		-0.482		-0.382		-0.475	
	(0.377)		(0.619)		(0.301)		(0.323)	
Democratic principles		-0.612		0.589		-1.227***		-0.345
		(0.416)		(0.871)		(0.469)		(0.407)
Political interest	-0.050	-0.108	-0.600	-0.614	-0.417*	-0.247	-0.418*	-0.267
	(0.287)	(0.265)	(0.475)	(0.558)	(0.249)	(0.283)	(0.249)	(0.257)
Left-right	0.115	0.158	0.523	0.600	-0.338	-0.304	-0.397	-0.306
	(0.272)	(0.250)	(0.510)	(0.582)	(0.267)	(0.299)	(0.308)	(0.308)
Wave participations	-2.764***	-3.213***	-5.390***	-6.838***	-1.682***	-5.614***	-6.176***	-6.168***
	(0.430)	(0.283)	(0.495)	(0.999)	(0.196)	(0.286)	(0.334)	(0.338)
High education	-0.013	0.064	-0.415*	-0.620**	-0.125	-0.205	-0.128	-0.093
	(0.129)	(0.117)	(0.252)	(0.297)	(0.120)	(0.135)	(0.128)	(0.129)
Female	-0.113	-0.149	-0.132	-0.120	0.117	-0.121	-0.191	-0.224*
	(0.128)	(0.117)	(0.236)	(0.272)	(0.118)	(0.132)	(0.126)	(0.127)
Loser × dem. satisfaction	-0.906		1.179		-0.078		0.351	
	(0.676)		(1.137)		(0.527)		(0.465)	
Loser × dem. principles		0.636		-0.940		0.778		-0.841
		(0.678)		(1.571)		(0.711)		(0.611)
Constant	0.250	1.613***	2.011***	2.677**	-1.449***	3.091***	1.937***	1.861***
	(0.486)	(0.426)	(0.656)	(1.176)	(0.322)	(0.482)	(0.385)	(0.444)
Observations	2,190	2,416	3,176	3,026	9,705	7,643	10,705	10,654
Log Likelihood	-888.118	-1,039.320	-351.486	-287.810	-1,364.846	-1,055.512	-1,241.451	-1,229.413
Akaike Inf. Crit.	1,796.235	2,098.641	722.973	595.620	2,749.692	2,131.024	2,502.902	2,478.825

Note: *p<0.1; **p<0.05; ***p<0.01

3.E Simulation Results Study 1

This section provides detailed results and robustness tests of the simulation analysis presented in Study 1.

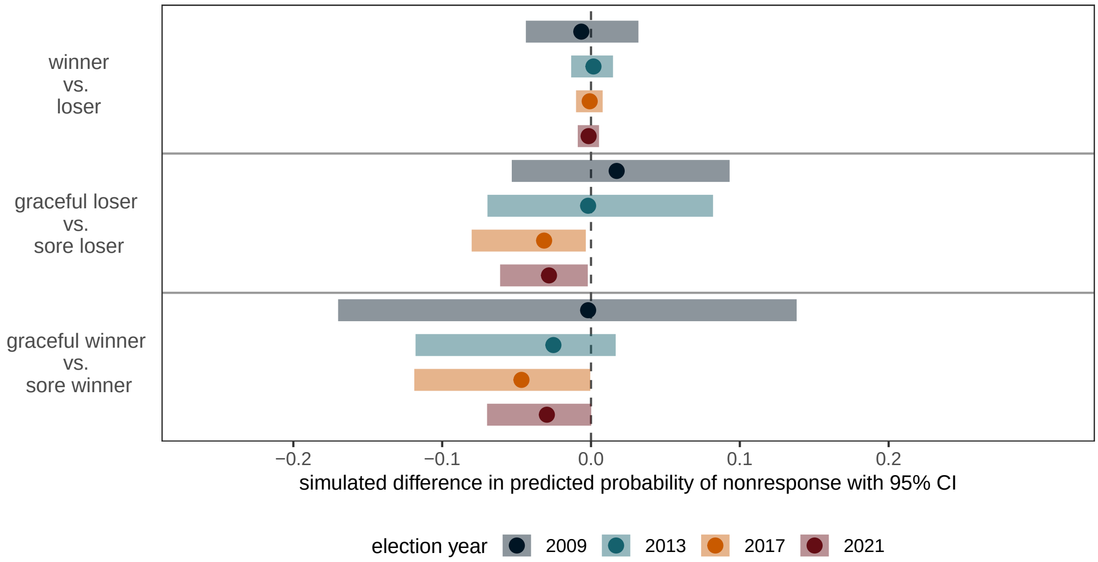

Figure 3.E.1: Simulated first differences in predicted probabilities of post-election nonresponse for different subgroups. Estimations are based on the winner-loser operationalization relying on party identification.

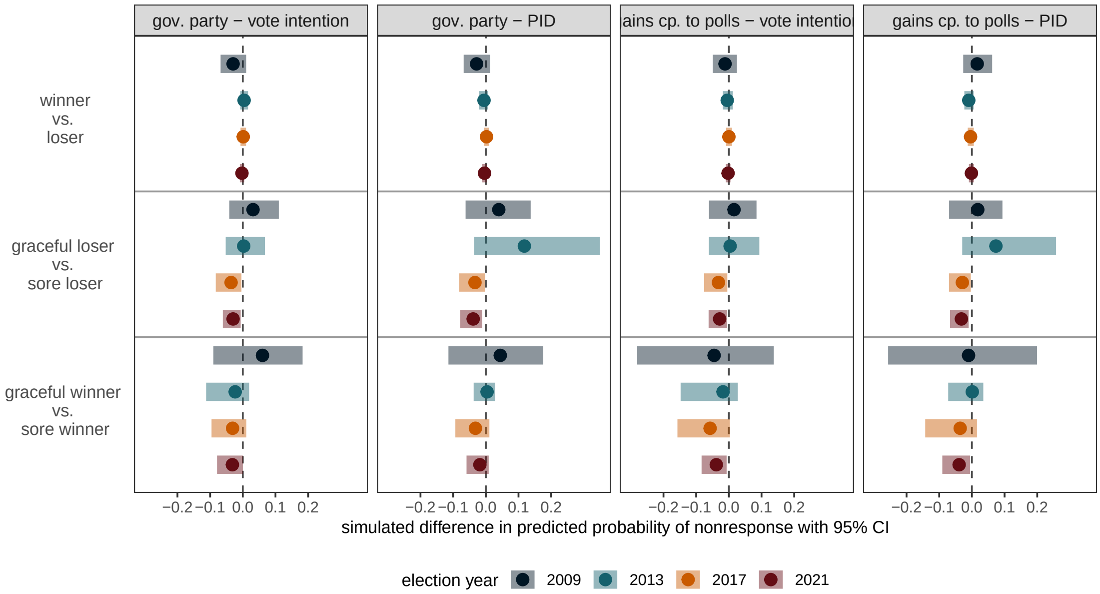

Figure 3.E.2: Simulated first differences in predicted probabilities of post-election nonresponse for different subgroups. Estimations are based on the winner-loser operationalization of (a) voting for or (b) identifying with a party that did (not) enter government (left two panels) or that gained (lost) vote share compared to the 20-day pre-election polling average (right two panels).

3.F Tables Study 2

Table 3.F.1: Cox proportional hazard regression: Durability of post-election nonresponse after the German federal election 2017 and 2021 – winner-loser categorization based on vote intention

	returning to participation after post-election nonresponse					
	2017			2021		
	(a)	(b)	(c)	(a)	(b)	(c)
Winner	-0.267	-0.460	-0.286	-0.100	0.326	-0.008
	(0.316)	(0.471)	(0.334)	(0.227)	(0.419)	(0.226)
Loser	0.516	-1.012	0.205	0.469	0.469	0.335
	(0.854)	(0.814)	(0.832)	(0.554)	(0.600)	(0.568)
Democratic satisfaction	0.048	-0.085	0.027	0.559	0.176	0.380
	(0.440)	(0.638)	(0.509)	(0.427)	(0.497)	(0.405)
Democratic principles	0.736	0.310	0.673	-0.234	-0.259	-0.192
	(0.679)	(0.746)	(0.776)	(0.416)	(0.488)	(0.412)
Political interest	0.241	0.315	0.253	0.256	0.267	0.242
	(0.381)	(0.381)	(0.385)	(0.303)	(0.297)	(0.303)
Left-right	0.919**	0.901**	1.013**	-0.168	-0.225	-0.197
	(0.393)	(0.391)	(0.438)	(0.337)	(0.339)	(0.343)
Participation waves elec year	2.172***	2.148***	2.137***	1.239***	1.232***	1.240***
	(0.691)	(0.693)	(0.695)	(0.390)	(0.392)	(0.391)
High education	-0.016	-0.025	-0.017	-0.065	-0.062	-0.057
	(0.183)	(0.182)	(0.182)	(0.152)	(0.152)	(0.153)
Female	-0.097	-0.076	-0.078	0.002	0.016	-0.001
	(0.179)	(0.179)	(0.178)	(0.149)	(0.149)	(0.148)
Loser × democratic satisfaction	-0.288	0.166	-0.048	-0.565	0.180	-0.324
	(0.800)	(0.780)	(0.745)	(0.598)	(0.611)	(0.612)
Loser × democratic principles	-0.573	0.375	-0.383	-0.236	-0.185	-0.266
	(1.042)	(1.008)	(1.024)	(0.672)	(0.678)	(0.685)
Observations	202	202	202	300	300	300
R^2	0.084	0.093	0.082	0.051	0.052	0.048
Max. Possible R^2	0.999	0.999	0.999	0.999	0.999	0.999
Log Likelihood	-692.591	-691.681	-692.819	-1,102.335	-1,102.212	-1,102.773
Wald Test (df = 11)	16.800	18.900*	16.460	14.460	14.530	13.640
LR Test (df = 11)	17.800*	19.620*	17.344*	15.767	16.014	14.893
Score (Logrank) Test (df = 11)	16.966	19.186*	16.688	14.687	14.729	13.836

Note: *p<0.1; **p<0.05; ***p<0.01
(a) gains compared to previous election
(b) government party
(c) gains compared to polling average

Table 3.F.2: Cox proportional hazard regression: Durability of post-election nonresponse after the German federal election 2017 and 2021 – winner-loser categorization based on party identification

	returning to participation after post-election nonresponse					
	2017			2021		
	(a)	(b)	(c)	(a)	(b)	(c)
Winner	0.147	0.225	0.316	0.112	0.107	0.308
	(0.235)	(0.227)	(0.262)	(0.218)	(0.234)	(0.207)
Loser	0.481	-1.371	0.383	0.397	0.338	-0.012
	(0.808)	(0.847)	(0.819)	(0.536)	(0.527)	(0.568)
Democratic satisfaction	0.194	-0.310	0.452	0.080	-0.098	-0.050
	(0.506)	(0.486)	(0.584)	(0.433)	(0.462)	(0.393)
Democratic principles	0.242	-0.165	0.157	-0.187	-0.064	-0.305
	(0.668)	(0.554)	(0.761)	(0.408)	(0.429)	(0.381)
Political interest	0.074	0.029	0.059	0.094	0.122	0.079
	(0.364)	(0.367)	(0.363)	(0.289)	(0.289)	(0.288)
Left-right	0.856**	0.952**	0.890**	-0.285	-0.276	-0.285
	(0.386)	(0.390)	(0.406)	(0.332)	(0.334)	(0.343)
Participation waves elec year	1.764***	1.800***	1.838***	1.130***	1.151***	1.131***
	(0.621)	(0.622)	(0.627)	(0.379)	(0.376)	(0.378)
High education	0.049	0.019	0.019	-0.060	-0.051	-0.039
	(0.176)	(0.179)	(0.180)	(0.149)	(0.149)	(0.150)
Female	-0.114	-0.098	-0.096	0.034	0.034	0.044
	(0.168)	(0.169)	(0.166)	(0.147)	(0.148)	(0.145)
Loser × democratic satisfaction	-0.622	0.678	-0.690	0.002	0.388	-0.063
	(0.794)	(0.783)	(0.785)	(0.592)	(0.591)	(0.619)
Loser × democratic principles	0.067	1.597	0.153	-0.138	-0.403	0.270
	(0.927)	(1.047)	(0.944)	(0.658)	(0.654)	(0.702)
Observations	216	216	216	308	308	308
R^2	0.063	0.074	0.067	0.048	0.047	0.047
Max. Possible R^2	0.999	0.999	0.999	0.999	0.999	0.999
Log Likelihood	-765.198	-763.964	-764.832	-1,152.792	-1,153.081	-1,153.002
Wald Test (df = 11)	13.600	15.990	14.260	14.120	13.610	13.700
LR Test (df = 11)	14.146	16.615	14.879	15.285	14.706	14.863
Score (Logrank) Test (df = 11)	13.663	15.966	14.306	14.338	13.799	13.884

Note: *p<0.1; **p<0.05; ***p<0.01

(a) gains compared to previous election
(b) government party
(c) gains compared to polling average

4 Assessing Sample Imbalances in Event-Study Designs: Observable and Unobservable Sources of Bias

Abstract

Incisive political events can shape political attitudes and behavior but may also affect who responds to surveys. This paper brings together insights from political psychology and survey methodology to argue that such events can bias causal estimates if shifts in participation, especially through unobserved factors, are related to outcomes of interest. Using the increasingly popular design of unexpected events during survey fieldwork (UESD), I propose a framework to disentangle the genuine causal effect of an event from compositional bias, i.e. bias from changes in sample compositions after the event. I outline strategies to adjust for observable imbalances and extend sensitivity analyses to assess how strong unobserved confounders would need to be to threaten substantive (causal) conclusions. I demonstrate this approach using the rally-around-the-flag effect after the 2015 Charlie Hebdo attacks in France, then apply it to replicate 14 published UESD studies on terrorist events and rally-style outcomes like government approval and trust in political institutions. By addressing both observable and unobservable forms of bias, this framework improves the robustness of causal inference in event-driven research and strengthens the assessment of public opinion measurements' credibility in dynamic political settings.

Keywords

Measurement error, unexpected event during survey design, sample imbalances, compositional bias, sensitivity analysis, rally-around-the-flag effect

4.1 Introduction

This paper addresses the question of how political events affect people's willingness to participate in surveys and, thereby, the accuracy of public opinion measurements. There is large consensus that political events affect people and their political behavior (e.g. Dalton & Klingemann, 2007; Huddy & Feldman, 2011). To study how political contextual change affects political behavior, event-focused analyses and quasi-experimental designs have become increasingly common in recent years (Dunning, 2008; Müller & Kneafsey, 2023; Muñoz et al., 2020; Robinson et al., 2009). One popular approach in this domain is the "unexpected event during survey design" (UESD) (Dunning, 2008; Müller & Kneafsey, 2023; Muñoz et al., 2020; Robinson et al., 2009), which leverages the quasi-random timing of survey interviews around unforeseen events. The causal effect of an unexpected event on political attitudes is estimated by comparing respondents interviewed before and after this event.

This approach rests on the critical assumption that the unexpected event affects only the outcome of interest, and not the composition of the sample itself. This paper questions this assumption: Previous literature has shown that some individuals are more willing to participate in political surveys than others. For instance, those with higher levels of political interest, sophistication, and education tend to be more cooperative (Groves et al., 2004; Keeter et al., 2006; Mellon & Prosser, 2017; Tourangeau et al., 2010). Additionally, phenomena such as panel attrition, where participants drop out over time in longitudinal studies, have shown that survey participation can change over time. While research shows that changes in individuals' *personal* circumstances (e.g., socioeconomic or sociopsychological factors) affect survey participation (Lugtig, 2014; Olson & Witt, 2011; Trappmann et al., 2015), we know little about the impact of *political* events on survey response. I therefore question: When and for whom do unexpected and salient events affect the likelihood of participating in a survey? And ultimately, do potential event-induced sample disproportionalities substantively bias causal conclusions we draw from survey data?

Understanding how political events affect survey participation is essential for accurately interpreting public opinion data. If an event changes who responds to a survey, the composition of the pre- and post-event samples may differ in ways that bias estimates of the event's impact. This is particularly problematic when compositional shifts are correlated with the outcome variable of interest, as the observed effect may conflate true attitudinal change with differences in who is represented in the data. While *observable* differences, such as age, gender, or education, can be adjusted for, others may stem from *unobserved* or *unobservable* factors like political preferences, psychological traits, or prior experiences. These unobservables are difficult, if not impossible, to fully account for analytically. Understanding and quantifying their potential for biasing the estimate of an event's effect is thus crucial to gauge the robustness of causal conclusions.

Conceptually, in this paper I develop a formal framework that distinguishes between the true causal effect of an event and compositional effects that arise from post-event shifts in the sample composition. Methodologically, I propose a workflow comprising three core steps to identify and mitigate such bias. After defining a baseline model setup, the approach evaluates imbalance on *observable* covariates and uses entropy balancing to re-estimate treatment effects. To assess the likelihood of substantive compositional bias based on *unobserved* or even *unobservable* factors, I propose to employ sensitivity analyses to quantify how influential an unobserved confounder would need to be to substantively bias the causal estimate of an event's effect.

I demonstrate the framework along the well-studied case of the 2015 Charlie Hebdo terrorist attacks in Paris and their effect on satisfaction with government. This event is a textbook example of a rally-around-the-flag effect (Nägel et al., 2024; Vasilopoulos et al., 2018; Vlandas & Halikiopoulou, 2025), i.e. a sudden surge in government approval in the aftermath of a political shock. Therefore, this application is not only a demonstration of my proposed analytical framework, but also a robustness test of a phenomenon that is well-established in the literature. I then extend the analysis to a broader set of cases by replicating 14 published studies that apply the UESD design to assess the impact of terrorist events on rally-type outcomes, such as government approval, trust in political institutions, and leadership satisfaction (Harding & Nwokolo, 2024; Holman et al., 2022; Nägel et al., 2024; Vlandas & Halikiopoulou, 2025). This broader application enables a systematic assessment of the extent to which, first, this instrument is applicable to different cases and, second, compositional bias may distort findings in this area of research.

Overall, my findings imply that the influence of compositional bias tends to be limited. Estimated event-effects remain largely stable after adjusting for observable compositional bias, and unobserved shifts in the pre- and post-sample compositions would in most cases need to be implausibly strong to overturn the substantive conclusions. This indicates that despite the occurrence of event-induced nonresponse, it does not automatically undermine causal conclusions that are drawn from event-focused survey designs. By showing how political events, generally, and terrorist attacks, specifically, might skew survey samples and bias measurements of political phenomena, this paper provides valuable insights into the complexities of public opinion research in dynamic political environments.

4.2 The *Unexpected Event During Survey Design* and Potential Bias

The effect of (political) events on political attitudes, behaviors and beliefs is often studied through pre-post survey approaches where participants from before the occurrence of an event are compared to people responding afterwards (Bertoli et al., 2024). Such approaches can rest on random samples, quota samples, rolling cross-sections or panel designs (Bertoli et al., 2024). One increasingly prominent approach to identify causal effects of (unexpected) events is the so called "unexpected event during survey design" (UESD) (Muñoz et al., 2020). This design has been applied to a plethora of cases, including responses to terror events or war (e.g. Bove et al., 2025; Epifanio et al., 2023; Frese, 2025; Harding & Nwokolo, 2024; Hernández & Ares, 2023; Holman et al., 2022; Nägel et al., 2024; Unan & Klüver, 2025), violence (e.g. Laniyonu, 2022; Rogowski & Tucker, 2019; Roman & Thompson, 2024; Singh & Tir, 2023) natural disasters (e.g. Berger, 2010; Bol et al., 2021; De Vries, 2018) or political scandals (e.g. Ares & Hernández, 2017; Kim & Kim, 2019; Solaz et al., 2019). Slightly more foreseeable political events have also been studied through the UESD, such as campaign events (e.g. Flores, 2018), cabinet reshuffles (e.g. Nemcok, 2020), policy reforms (e.g. Burlacu et al., 2018; Larsen, 2018; Seimel, 2024) and elections (e.g. Flores, 2018; Frye & Borisova, 2019; Giani & Méon, 2021; Minkus et al., 2019; Pierce et al., 2016; Schraff & Schimmelfennig, 2020; Turnbull-Dugarte, 2023).

UESD leverages the division of a single survey sample into a subsample of respondents who participated before an unexpected event at t_e and those who participated afterwards. The subsample of $t_i < t_e$ respondents resembles the control group, while all respondents of the $t_i > t_e$ sample form the treatment group participating under the presence of the event's effect. The timing of participation within the period of survey fieldwork is assumed to be random. Under the absence of the event, it should therefore not be expected that respondents from right before or after these dates substantively differ in their characteristics and attitudes. Thus, a comparison of the treatment and control group should yield an unbiased estimate of the events' average effect on opinion shifts (see also Castanho Silva, 2018). Due to its increasing popularity and because UESD relies on the assumption that survey participation is unaffected by the event, it is a particularly demanding test case for evaluating how event-induced compositional shifts can bias estimates. This approach will therefore be in focus of my study.

Despite UESD's and other event-focused approaches' widespread application and overall validity for causal identification (Muñoz et al., 2020), different factors can introduce bias into the estimate of an event's effect on outcome variables (Frese & Riaz, 2025). Bertoli et al. (2024) identify four different types of bias in this context. First, there may be *temporal bias* if factors unrelated to the event, such as weather, mood, or other events happening close to the treatment event alter the way individuals respond to survey questions (Bertoli et al., 2024). Second, it may be that respondents' answers are influenced by their anticipations of and expectations about the event. Such *anticipation bias* is particularly relevant when respondents are aware of an upcoming event and already adjust their opinions before it happens (Bertoli et al., 2024). Third, respondents may not always truthfully answer survey questions. Such *differential misreporting bias* can occur due to different social desirability pressures or differences in how individuals answer questions at different times. This can lead to response inconsistencies that may distort effect estimates. Finally, there may be *demographic bias* if differences in the demographic composition of pre- and post-event respondents affect estimates. Avoiding such bias is particularly challenging when compositional differences are rooted in unobserved or unobservable variables.

To address and circumvent these biases and to provide valid causal inference employing the UESD approach, several assumptions need to be met (Muñoz et al., 2020). Two are most focal to address the above mentioned biases: The *excludability* assumption subsumes the absence of collateral or simultaneous events to the one event in focus, the absence of time trends in the outcome variable of interest that is unrelated to the event, and the exogenous and unexpected timing of the event. If this assumption was strictly met, temporal bias and anticipation bias should be negligible. The *ignorability* assumption refers to the independence of respondents' treatment status from outcome variables of interest, i.e. the random assignment of respondents into treatment or control group. This assumption can likely be violated by sampling procedures involving quota or multistage sampling as well as different degrees of reachability and cooperativeness for different groups of the population. Under ideal conditions and when the ignorability assumption is met, compositional differences should not bias estimates, as the random nature of survey participation would ensure that the pre- and post-event samples are comparable regarding both observable and unobservable characteristics.

In practice, however, these conditions are rarely fully met. Even if the excludability assumption is strictly fulfilled, i.e. there is no anticipation of the event, no collateral stimuli and no time trends in the outcome variable, it is likely that the ignorability assumption is at least partially violated and influential differences between the pre- and post-event sample prevail. For this reason, this study focuses on the causes and consequences of such *compositional bias*. There are two most-likely reasons for this bias to arise, one rooted in the sampling design, another one rooted in an event's potential effect on survey participation and response behavior.

First, data used in UESD analyses are, by design, almost never collected with the aim of studying effects of the specific event under analysis. This is a fundamental difference from randomized controlled experiments or studies of planned interventions, where researchers can predefine treatment and control groups (Sekhon & Titiunik, 2012) and apply strategies to mitigate compositional bias ex ante (such as, e.g., dual randomized surveys (Bertoli et al., 2024)). In the case of UESD, such strategies are typically infeasible, precisely because the event occurs *unexpectedly* during ongoing fieldwork. As a result, researchers must rely on survey data whose collection procedures were determined independently of the event. Non-random features of the data, such as quota sampling or geographically staggered multistage designs, can produce by-design compositional differences between the pre- and post-event samples. Even if the event itself is plausibly exogenous, these design-induced differences can violate the ignorability assumption and produce compositional bias in estimates of the event's causal effect.

A second source of bias arises when the event under study does not only affect political attitudes or behavior, but also who chooses to participate in the survey. That is, even if the timing of survey participation is effectively random, compositional differences between the pre- and post-event samples may emerge if the event itself changes who is willing or able to respond. Existing research shows that survey participation is not politically neutral: the likelihood of responding to a survey is systematically related to political engagement and participation (Peress, 2010), both of which may be influenced by high-salience political events. That is, individuals who are more politically interested, trusting of institutions, or mobilized by political events are generally more likely to respond to surveys (Billiet et al., 2007; Groves et al., 2004; Mellon & Prosser, 2017; Silber et al., 2022; Tourangeau et al., 2010; Voogt & Saris, 2007), just as they are more likely to vote or engage in other forms of political behavior (Voogt & Saris, 2007). Conversely, individuals who are politically disengaged, alienated, or overwhelmed by an event may opt out of participation altogether (Müller, 2025). As such, events that increase or decrease public attention, trust, fear, or motivation and likely affect political behavior may also affect who responds to surveys.

If these shifts in survey responsiveness affect characteristics that are *observable*, such as, e.g., age, gender, education, or income, they can be diagnosed and adjusted for using standard techniques like entropy balancing (Muñoz et al., 2020). However, if the shifts are rooted in *unobserved* characteristics, such as, e.g., political attitudes, psychological traits, prior experiences, or any latent variables not covered in the data, the resulting compositional differences are more challenging, if not impossible, to detect and address (Bertoli et al., 2024). This becomes particularly problematic and biasing when unobserved characteristics are correlated with the outcome of interest.

Consider an example: a typical rally-around-the-flag effect, as used later in this paper to demonstrate my proposed analytical framework, where government approval increases following a terrorist attack (Edwards III & Swenson, 1997; Huddy & Feldman, 2011). While the true causal effect may be a genuine rise in government approval, this estimate can be distorted if the event also affects survey responsiveness. If individuals who already support the government become more likely to participate, the observed effect may be artificially inflated, reflecting not only the true attitudinal shift but also a disproportionately increased share of satisfied respondents in the post-event sample. Conversely, if disapproving individuals become more responsive, the true effect could be underestimated. In either case, what appears to be a genuine shift may (partially) be driven by changes in sample composition. In this way, even when the formal assumptions of the UESD framework are met, causal estimates may still be biased due to the event's effect on survey response and, thus, sample composition.

Such compositional bias can have serious consequences for both empirical accuracy and the substantive claims we draw from survey data. Methodologically, there is suggestive evidence that it can inflate false positive rates in designs like the UESD, overstating the effects of political events (Frese & Riaz, 2025). Substantively, conflating true opinion change with shifts in who is represented in the data risks distorting our understanding of how citizens actually respond to political stimuli. This problem is especially consequential in dynamic political environments, where individuals are continually exposed to influential events that may shape their likelihood of survey response. Addressing compositional bias by mitigating *observable* imbalances and assessing the potential influence of *unobservables* is therefore not merely a technical concern but a necessary step to ensure that survey data are a credible basis for understanding public opinion and political behavior.

In the following section, I formalize the interplay of the *true causal effect* and *compositional effect* of an event on an outcome variable. I aim to disentangle both effects to propose different steps to, first, identify and, second, account for compositional bias to obtain an as close as possible quantification of an event's true causal effect.

4.2.1 Formalizing Compositional Bias

In quasi-experimental designs where an incisive event occurs during the fieldwork of a single survey wave, individuals are interviewed either before or after the event. This splits the sample into a pre- and post-event group. The observed estimate of the event's effect on an outcome of interest is a combination of the true causal effect of the event and any bias introduced by differences in the composition of those two sub-samples.

I formalize this idea using the potential outcomes framework (Rubin, 1974, 1991, 2005). As point of departure under this framework, assume that each individual i has a potential outcome Y_i^1 if surveyed after the event and Y_i^0 if surveyed beforehand. The true causal effect for an individual i is given by:

$$\text{True Causal Effect}_i = Y_i^1 - Y_i^0 \tag{4}$$

However, in practice, each respondent is only observed *either* before or after the event, never in both states. This makes it impossible to directly observe this true individual-level causal effect. To nevertheless approximate this effect, the difference in mean outcomes between the post-event and pre-event sample can be estimated, which yields the average treatment effect (ATE):

$$\hat{\tau} = \frac{1}{n_1} \sum_{i \in \text{Post}} Y_i^1 - \frac{1}{n_0} \sum_{i \in \text{Pre}} Y_i^0 \tag{5}$$

The core difficulty here is that the pre-event sample may differ systematically from the post-event one, both regarding observable or unobservable characteristics. Such differences in the sample compositions can, as mentioned in the previous section, occur for two reasons: First, they may reflect underlying features of the survey process that are unrelated to the event, such as fieldwork logistics or regionally staggered sampling. Second, and most central to this study, they may be caused by the event itself if it affects people's likelihood of survey participation (Müller, 2025). These compositional differences can bias the estimate of the event's causal effect if the characteristics that influence post-event participation are also related to the outcome variable of interest. I refer to this source of distortion as the *compositional bias*. Consequently, the estimate $\hat{\tau}$ does not solely reflect the causal effect of the treatment event. Instead, it combines both the causal effect and the compositional bias:

$$\hat{\tau} = \text{Average Treatment Effect} + \text{Compositional Bias} \tag{6}$$

This compositional bias reflects the difference in the outcome variable between a treatment group sample after the occurrence of the event and a hypothetical treatment group sample in a counterfactual world where the event did not happen:

$$\text{Compositional Bias} = \frac{1}{n_1} \sum_{i:S_i(1)=1} Y_i(0) - \frac{1}{n_1^*} \sum_{i:S_i(0)=1} Y_i(0) \tag{7}$$

where $S_i(1)$ indicates being in the sample after the event and $S_i(0) =$ being in the sample in a counterfactual world where the event did not happen. For both $S_i(1)$ and $S_i(0)$ the treatment status is $Y_i(0)$ to indicate that the compositional bias only captures change in Y that is not rooted in the direct event-effect on this outcome variable.

While the theoretical framework provides a clear conceptual distinction between an event's causal effect (ATE) and compositional bias introduced by changes in the survey sample, this separation is far more difficult to quantify in practice. Since compositional bias may stem from unobserved or even unobservable characteristics, it is factually impossible to directly measure or fully analytically adjust for its influence. It is, however, possible to assess the likelihood by which (unobserved) compositional shifts distort causal estimates and their substantive implications. In the following section, I outline a framework that follows two aims: first, detecting and adjusting for compositional bias from *observables*, and, second, assessing if and to what extent bias from *unobservables* threatens the robustness and validity of the estimated causal effect.

4.3 Research Design

I present the underlying logic and implementation of each proposed step along the example of the Charlie Hebdo terrorist attacks of 2015 in Paris as unexpected event and satisfaction with government as outcome variable. Previous research renders this a textbook case of a *rally-around-the-flag effect* (Nägel et al., 2024; Vasilopoulos et al., 2018; Vlandas & Halikiopoulou, 2025).

The rally-around-the-flag effect describes a well-documented pattern in which citizens express increased support for political leaders and institutions in response to national or international crises. Events such as terrorist attacks, wars or natural disasters can trigger short-term boosts in government approval, leadership satisfaction, and institutional trust (Bol et al., 2021; Edwards III & Swenson, 1997; Huddy & Feldman, 2011; Huddy et al., 2005; Lehrer et al., 2025; Yam et al., 2020). Literature has attributed these effects to mechanisms such as *opinion leadership*, where elite consensus reduces criticism and boosts government credibility (Brody, 1991; Lee, 1977) and *patriotic reflexes*, where crises strengthen national identity and in-group loyalty (Chowanietz, 2011; Mueller, 1970; Tajfel & Turner, 2004). Regardless of the mechanism, the result is often a temporary surge in support for incumbents and political institutions.

4.3.1 The Running Example: The *Charlie Hebdo* Terrorist Attacks

At 11:30 am on January 7^{th}, 2015, two Al-Qaeda terrorists entered the offices of the Charlie Hebdo satirical magazine in Paris. The duo killed twelve people and injured another eleven. The following day, four other people were taken hostage and killed by an accomplice of the Charlie Hebdo terrorists. On January 9^{th}, police identified and found the perpetrators, who were shot and killed during an exchange of fire. The days after the attacks were characterized by large-scale demonstrations, meetings of international politicians and leaders and a global display of solidarity.

This attack falls into the field time of the 7th round of the European Social Survey (ESS, 2023), which fielded in France between October 31^{st}, 2014 and March 3^{rd}, 2015. ESS data is considered among the highest quality cross-country data as they follow rigorous methodology and protocols for data collection and fieldwork (Schnaudt et al., 2014). Existing studies clearly demonstrate the suitability of this case and the data for the UESD design (Castanho Silva, 2018; Muñoz et al., 2020; Nägel et al., 2024). They identify a rally-around-the-flag effect in the form of significantly increased levels of satisfaction with government among the treated (Muñoz et al., 2020; Nägel et al., 2024). This case is particularly suitable due to the attack's unexpectedness and its far-reaching impact on public attention.

During this survey wave, $n = 1917$ interviews were conducted in France. Respondents participating after January 7^{th} are assigned to the treatment group ($n = 303$), respondents participating beforehand constitute the control group ($n = 1594$). Those who participated on January 7^{th} are omitted from the analysis ($n = 20$).

To approximate the rally-around-the-flag effect, I rely on a measure of government support as dependent variable. The ESS data includes a variable on satisfaction with government. Originally ranging from 0 (= extremely dissatisfied) to 10 (= extremely satisfied)[45], I rescale it to the unit interval.

Along this example, I demonstrate the analytical procedure to isolate the causal estimate of this event's effect on satisfaction with government within the UESD framework. These steps aim to obtain a causal estimate that is as free as possible of compositional bias rooted in observables and to gauge how likely bias due to unobservable compositional differences is in the context of this specific example.

4.4 A Framework to Disentangle Causal and Compositional Effects

Table 4.1 details the steps I propose to follow to mitigate compositional bias based on *observable* factors and to gauge the likelihood by which *unobserved* confounders may substantially bias the causal estimate of an event's effect. Subsequently, I demonstrate each step's analytical components and objectives along the Charlie Hebdo example.

[45]English question wording: "Now thinking about the [French] government, how satisfied are you with the way it is doing its job?".

Table 4.1: Overview of the analytical framework to address compositional bias

	Analytical Procedure	Objective
Step 1: Baseline setup	Estimate the UESD model, find appropriate model specification (e.g., fixed-effects, selection of covariates)	Establish a baseline estimate of the event-effect.
Step 2:[46] Address *observable* compositional bias	Power analysis	Inform the selection of bandwidth sizes around the treatment event to ensure sufficient sample sizes and statistical power.
	Descriptive assessment of imbalances between pre- and post-event samples	Identify variables for which sample compositions differ to inform the selection of covariates for entropy balancing.
	Entropy balancing	Obtain entropy weights to weigh the control group observations in a way that they match the treatment group distributions.
	Model re-estimation on balanced samples	Control for bias in the causal estimate that is rooted in observable sample imbalances.
Step 3:[47] Address *unobserved* compositional bias	Sensitivity analysis: Robustness values	Quantify how much variation in the entropy weights and outcome variable would need to be explained by an unobserved confounder to bias the causal estimate to a specified threshold.
	Sensitivity analysis: Contour plots and Maximum Relative Confounding Strength (MRCS)	Benchmark the effect of unobserved confounders against that of observed covariates to assess the likelihood of an unobserved confounder substantively biasing the causal estimate.

4.4.1 Step 1: The Baseline Setup

The initial step is to set up the baseline model of the event's effect on an outcome variable of interest. The stochastic model component should be adapted to the structure of this outcome variable. The systematic model component may range from a "naïve" model including only the treatment indicator as explanatory variable to models with

[46]The workflow for this step is mainly based on best-practices as suggested by Muñoz et al. (2020).
[47]The workflow for this step is informed by the sensitivity analyses as established by Hartman and Huang (2024).

more complex covariate structures and, e.g., interactions. Additionally, one can already account for survey- and data-specific characteristics by including fixed-effects (e.g., on the region-level to account for multistage sampling). This first step should be theory-driven to obtain a selection of models that is most suitable for testing the case-specific hypothesis as well as the data structure and sampling design.

Figure 4.1 displays the intent-to-treat (ITT) effects of the Charlie Hebdo terrorist attacks on satisfaction with government from two different models. Both models include region-level fixed-effects to control for ESS' multistage sampling. The *naïve model* includes only the treatment variable as explanatory factor. The *time model* additionally interacts the treatment with a time variable indicating the days to the attacks (with 0 marking the day after the treatment). In the latter case, the ITT estimate resembles the effect on government satisfaction on the day after the attacks. Detailed regression results are reported in Table 4.A.1 in the Appendix. Both models show a significant increase in satisfaction with government in response to the attacks. While this is a sizable finding and allows for first conclusions that a typical rally-around-the-flag effect occurred, it cannot be ruled out that the observed surge is (partially) rooted in systematic differences between control and treatment group that are not solely linked to government satisfaction. The next steps therefore focus on detecting and mitigating compositional bias from, first, observable and, second, unobserved or unobservable factors.

4.4.2 Step 2: Addressing *Observable* Compositional Bias

This second step is targeted at identifying and controlling for compositional bias rooted in *observable* factors. In doing so, the suggested analytical procedures largely follow the best-practices suggested by Muñoz et al. (2020). Departing from the baseline model(s) established previously, this second step includes conducting power analyses to choose appropriate bandwidths around the treatment day for sample restrictions, descriptive assessments of sample imbalances, entropy balancing and, ultimately, the re-estimation of the baseline model(s) on balanced samples to mitigate observable compositional disproportionalities.

Power Analysis

Very early respondents may systematically differ from very late respondents (Brehm, 1993; Muñoz et al., 2020). Despite the assumption of a random timing of survey participation, reasons for people responding very early or rather late may be related to the

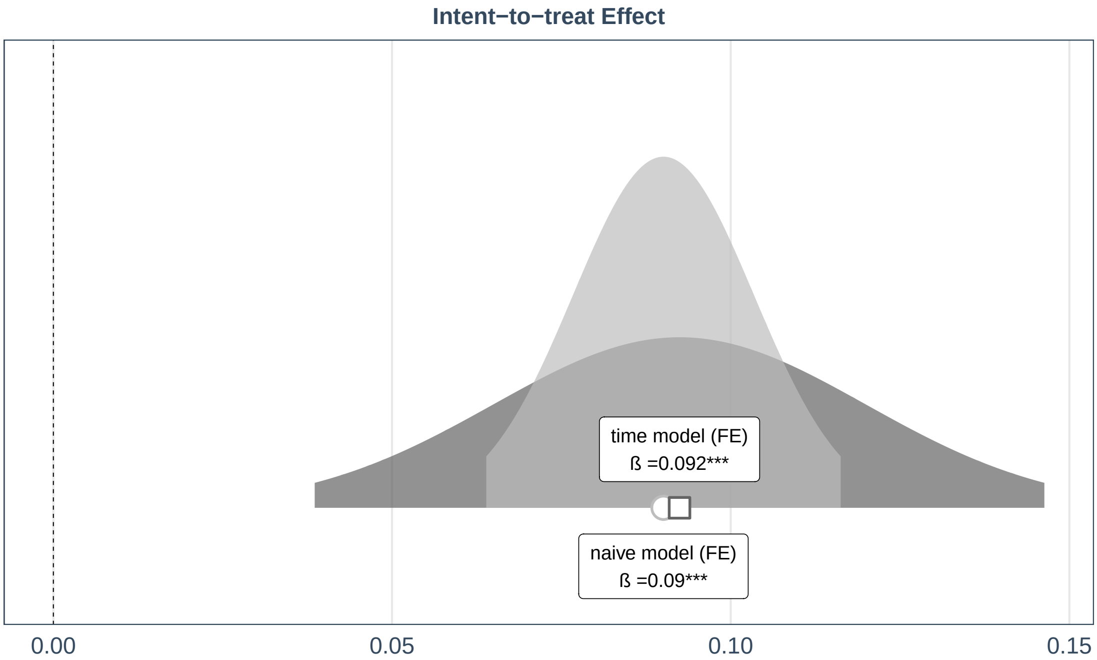

Figure 4.1: ITT effects of the Charlie Hebdo terrorist attacks on government satisfaction in France from two base models. The *naïve model* only includes the treatment indicator as explanatory variable. The *time model* additionally includes an interaction of the treatment variable with a variable of running days around the treatment, i.e. the depicted estimate shows the ITT on the day after the attacks. Both models include region-level fixed-effects.

outcome variable of interest. To reduce such noise, the bandwidth of days around the treatment can be narrowed. This reveals in how far the ITT effect is driven by specifics of very early or late respondents and additionally decreases the influence of collateral events and other contextual influences. There is a trade-off between reducing such noise and compromising on statistical power due to smaller sample sizes. Which bandwidth reduction to choose is thus very specific to the case under study; it depends on the contextual setting the event happened in, the sample size, the field time period as well as potential time-trends in the outcome variable. Besides these case-specific considerations, power analyses can help in selecting appropriate bandwidths by quantifying the trade-off of noise reduction and statistical power.

Results of the power analysis for the Charlie Hebdo case are shown in Figure 4.2. The smallest possible bandwidth that maintains sufficient statistical power to identify an effect as large as one half of government satisfaction's standard deviation ($\sigma_{1/2} = 0.1$) lies at ± 25 days around the attacks. To isolate change in government satisfaction as small

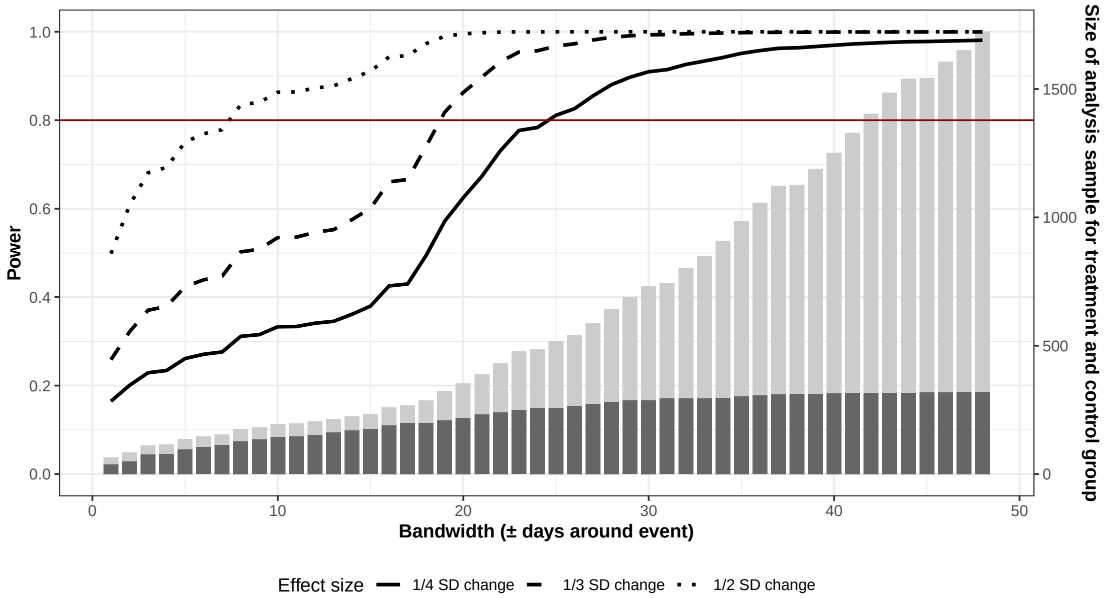

Figure 4.2: Power analysis for different bandwidths between ± 1 to ± 48 days around the event. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of government satisfaction in response to the event. Dark bars indicate the treatment group sample size, lighter bars indicate the control group sample size.

as one quarter of its standard deviation ($\sigma_{1/4} = 0.05$), the bandwidth could be narrowed down ± 8 days. Thus, the next steps are conducted for different bandwidths ranging from the full sample to the narrowest time frame of ± 8 days around the treatment.

Descriptive Assessment of Observable Sample Imbalances

Once there is an informed selection of bandwidths, it is helpful to assess descriptive differences between the pre- and post-event sample. This is another step to gauge which bandwidth is the most appropriate one in terms of having as balanced as possible treatment and control groups. Further, identifying for which characteristics the samples are imbalanced informs the selection of variables to consider when estimating the entropy weights in the next step.

Figure 4.3 shows the differences in means between the treatment and control group for a selection of variables across bandwidths. Overall, the absolute differences do not change much with decreasing bandwidths, however the uncertainty surrounding the differences in means increases. While the samples do not systematically differ regarding the distribution of gender, employment status, and having voted in the last election, there

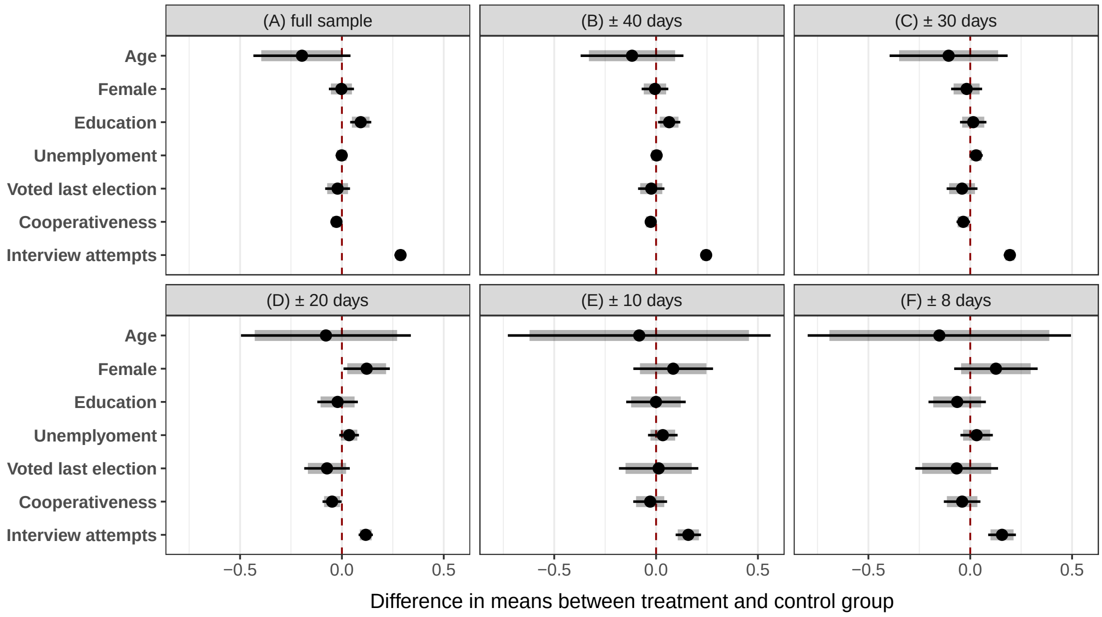

Figure 4.3: Differences in means between pre- and post-attack samples for the Charlie Hebdo case and different bandwidths of days around this treatment. Bold lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. *Age* and *education* are years divided by 10. *Interview attempts* are also divided by 10. For sample sizes, see the descriptive summary in Table 4.A.2 in the Appendix.

are some more pronounced differences: Treatment group respondents are, by tendency, slightly younger than their control group counterparts and have higher educational attainment. Unsurprisingly, treatment group respondents participated after more interview attempts and were slightly less cooperative during the interview itself. Besides the number of interview attempts, there are no variables for which systematic differences occur across the whole range of bandwidths. Nevertheless, this overview prompts to control for these differences in the modeling procedure, as it cannot be ruled out that they have an impact on the causal estimate. Summary statistics of the means of treatment and control groups across all bandwidths are reported in Table 4.A.2.

Entropy Balancing and Model Re-Estimation

The aim of this step is to re-estimate the baseline model(s) on balanced treatment and control samples. This way, any compositional bias rooted in *observable* differences between these two sub-samples, as identified above, should be mitigated.

First, it is necessary to obtain appropriate balancing weights. This is achieved through *entropy balancing*, a procedure of weighting control group observations to match the covariate distributions of the treatment group for a selection of variables (Hainmueller, 2012). The finite choice of balancing-covariates is case-specific. Appropriate weighting variables are those that arguably affect both the response propensity and the outcome variable of interest (Bailey, 2024). Common choices are socio-demographics, given the consensus in the literature that such characteristics affect both political outcome variables as well as survey response behavior (Groves et al., 2004; Keeter et al., 2006; Mellon & Prosser, 2017; Tourangeau et al., 2010). Depending on the specific case, it might also be sensible to include time-variant variables for entropy balancing. To avoid post-treatment bias, there should however be valid reasons to assume that these variables are neither directly affected by the treatment nor strongly correlated with the outcome variable (Muñoz et al., 2020).

While the power analysis should have indicated the narrowest plausible bandwidth, it may be the case that the convergence of the entropy balancing limits the range of bandwidths. For instance, the smallest possible bandwidth for the Charlie Hebdo example as indicated by the power analysis was $\pm$ 8 days around the attacks. However, the entropy balancing model faces convergence problems due to collinearity for bandwidths smaller than $\pm$16 days. The models are therefore re-estimated only for the full sample, the $\pm$40, $\pm$20 and $\pm$16 days bandwidths. Results are presented in Figure 4.4. Detailed regression results are reported in Table 4.A.3 in the Appendix.[48] Overall, the substantive effect from the baseline models remain unchanged under entropy balancing and across bandwidths. That is, government satisfaction significantly increased in the immediate aftermath of the Charlie Hebdo terrorist attacks. Under the wider bandwidths (full sample and $\pm$40 days), the balanced estimate even exceeds the size of the baseline estimate. Overall, this re-estimation implies that the differences in time-invariant characteristics between treatment and control group do not systematically bias the causal estimate of the rally-around-the-flag effect.

Conducting Step 1 and Step 2 has established a sound assessment of an event's effect on an outcome variable of interest. Most importantly, the most influential compositional biases rooted in *observable* differences between the treatment and control group should have been eliminated. Nevertheless, compositional bias rooted in *unobservable* factors may still remain. The following step, thus, gauges the potential impact of such unobserved confounders on the causal estimate of the event's effect.

[48] In Table 4.A.4, I additionally report results of regression models that include the variables used for entropy balancing as explanatory factors.

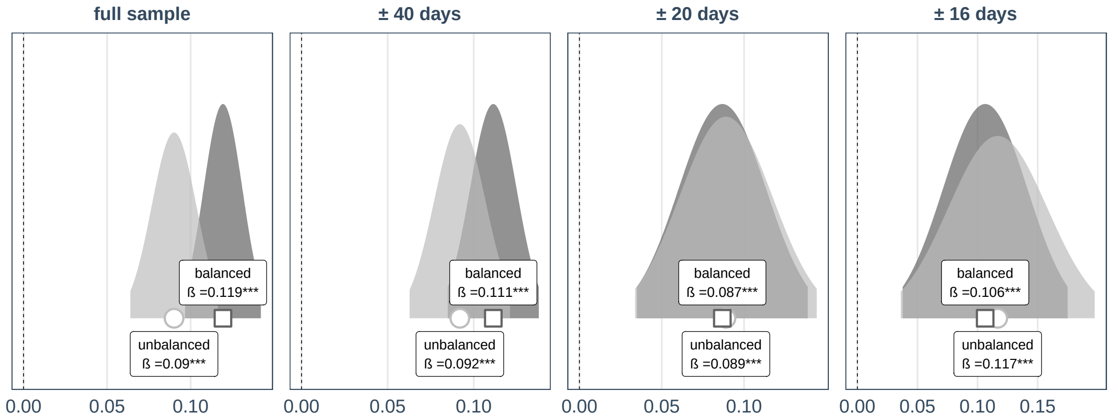

Figure 4.4: ITT effects of the Charlie Hebdo terrorist attacks on government satisfaction in France for different bandwidths and based on non-balanced as well as entropy-balanced samples. All models include the treatment indicator as explanatory variable and region-level fixed-effects. The sample sizes are $n = 1865$ (full sample), $n = 1181$ ($\pm$40 days bandwidth), $n = 306$ ($\pm$20 days bandwidth), $n = 213$ ($\pm$16 days bandwidth).

4.4.3 Step 3: Addressing *Unobserved* Compositional Bias

Unobserved confounders can influence two different components of the analysis. First, they may directly bias the outcome variable of interest. This is the case if an unobserved confounder is linked to the outcome variable but varies between the pre- and post-event sample. Second, they may bias the weights used to adjust for observable differences between the two sub-samples, such as those obtained through entropy balancing. This is the case if unobserved characteristics affect the likelihood of survey participation in ways that are not captured by the chosen balancing-covariates. Either pathway could bias the causal estimate of the event's effect.

Sensitivity Analysis: Obtaining the Robustness Value

To understand the potential impact of such unobserved confounders, I conduct a sensitivity analysis following the approach by Hartman and Huang (2024). This analysis quantifies how strong an unobserved confounder would need to be, in terms of its association with the outcome variable as well as the (entropy) weights, to meaningfully alter the estimated effect. One expressive quantity in this context is the *robustness value* (RV). The RV is defined as the minimum proportion of variation that an omitted variable must simultaneously explain in the ideal weights $w*$ and the outcome Y for the resulting bias to reach a pre-specified threshold b^* (for example, $b^* = 0$ for reducing the causal estimate to zero) (Cinelli & Hazlett, 2020; Hartman & Huang, 2024; Huang, 2024).

Intuitively, the RV provides a benchmark for how influential an unobserved variable would need to be to substantively change a causal estimate and its implications. The RV is bounded between 0 and 1: values close to 1 indicate that an implausibly strong confounder would be required to overturn the findings, suggesting robustness. Values close to 0 indicate that only a weak confounder would suffice, suggesting sensitivity to unobserved confounding.

Applied to the Charlie Hebdo example, Figure 4.5 presents the RVs for varying levels of bias across bandwidths. The x-axis displays different levels of bias as proportions of the ITT estimate (q). The y-axis displays the respective RV, i.e. the minimum necessary explained variation in weights and the outcome variable by an unobserved confounder to bias the ITT by the respective portion. Wider bandwidths, particularly the full sample and ± 40 day window, show high robustness: driving the ITT estimate to zero would require an unobserved confounder to explain over 80% of the relevant variation. This is implausibly large. By contrast, for the narrower bandwidth of ± 16 days, even a moderate amount of confounding could meaningfully reduce the estimate. This indicates greater sensitivity. These results highlight a trade-off between reducing noise through smaller bandwidths and maintaining robustness to unobserved confounding.

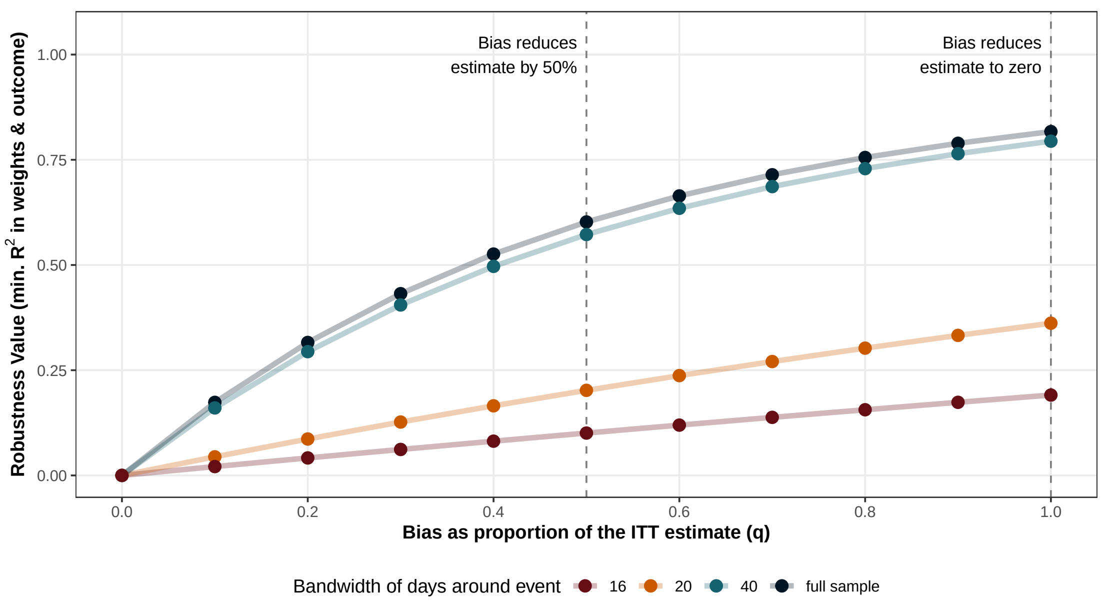

Figure 4.5: *Robustness values*: Minimum explained variation in the outcome variable and entropy balancing weights needed for an unobserved confounder to substantively change the ITT estimate.

Sensitivity Analysis: Contour Plots and Minimum Relative Confounding Strength

While the RV is a useful benchmark for the necessary impact of an unobserved confounder to substantively bias the causal estimate, it can be difficult to interpret in the context of a specific empirical setting. To assess the actual likelihood of such bias, it is essential to consider how strongly observed covariates relate to both the weights and the outcome variable. The contour plot in Figure 4.6 provides this context by mapping the effects of observed covariates alongside the threshold combinations of explained variation in weights and the outcome variable that an unobserved confounder would need to reach to alter conclusions. At the same time, this graphical illustration acknowledges that an unobserved confounder may not bias the weights and the outcome equally. It thereby yields additional information to the RV, which does not distinguish between these two different dimensions of bias.

A contour plot visualizes how different combinations of influence on the weights and the outcome variable lead to the same level of bias: the proportion of variation in the ideal (entropy) weights explained by a hypothetical unobserved confounder ($R^2_{\varepsilon,W}$) on the y-axis. The x-axis represents the correlation of the error in the entropy weights with the outcome variable (the *alignment* $\rho_{\varepsilon,Y}$), i.e. to what extent the unobserved confounder causes misweighting that favors units with higher or lower outcomes (Hartman & Huang, 2024). The shaded area indicates the so-called "killer confounder" region, that is, combinations of these parameters strong enough to drive the ITT effect to zero (or another specified threshold b^*, such as halving the effect).[49] Moving upward in the plot reflects a confounder explaining more of the variation in the weights, while moving horizontally indicates stronger positive or negative alignment with the outcome variable.

Observed covariates effects' can be depicted in the contour plot to improve interpretability. By positioning the hypothetical killer confounder relative to these observed covariates, it becomes assessable whether the magnitude of bias required to overturn the result is plausible. If the killer confounder's influence on the weights and outcome would need to be much stronger than any observed covariate's, the estimate can be considered relatively robust. Conversely, if the required strength is comparable to that of existing covariates, the estimate is more vulnerable to bias from unobserved confounding.

[49] An alternative specification, in which the hypothetical confounder would reduce the ITT effect by half rather than drive it to zero, is presented in Figure 4.A.1 in the Appendix.

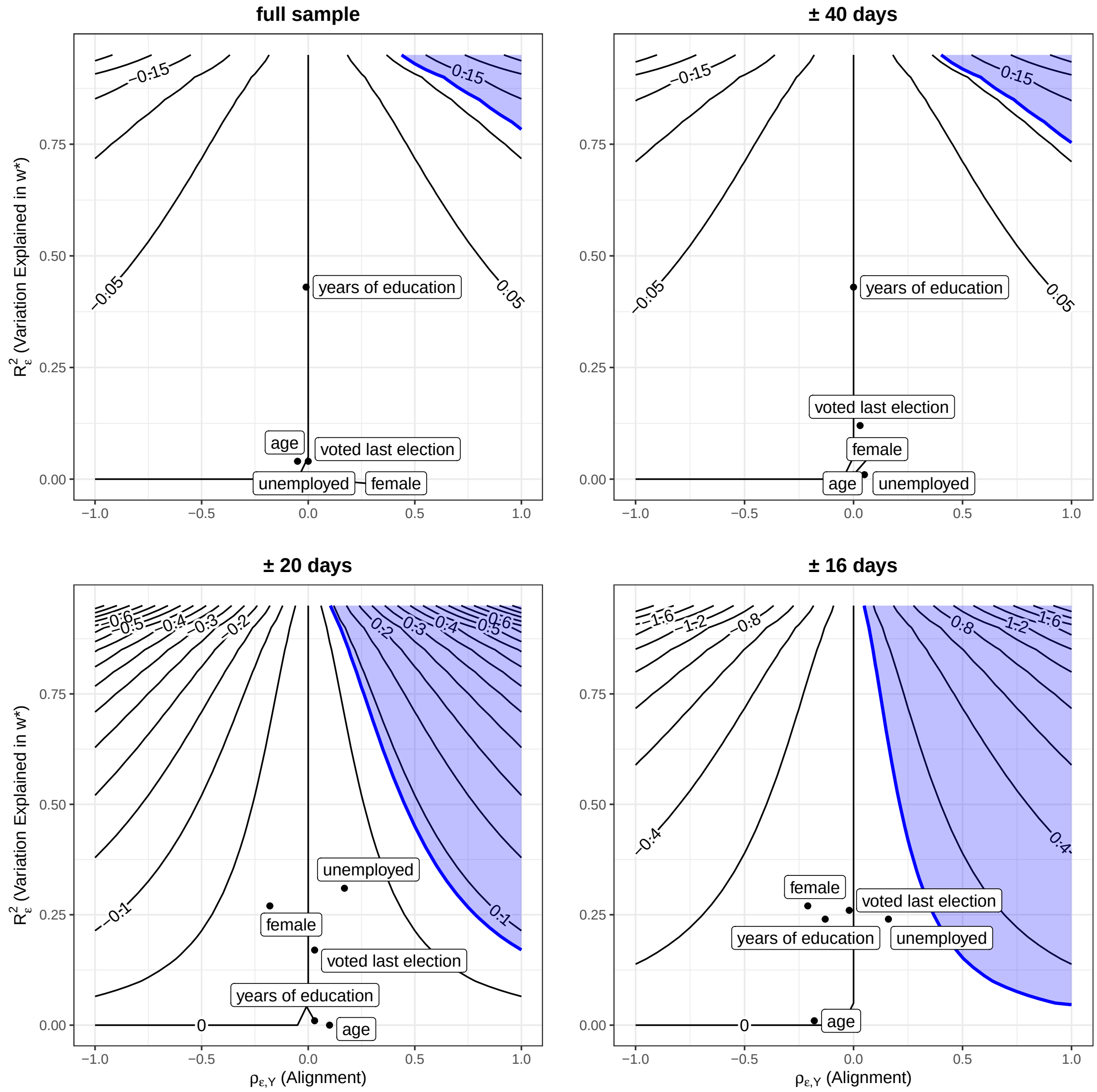

Figure 4.6: *Contour plots* for the *Charlie Hebdo* case. The plots visualize the robustness of the estimated ITT effect to potential unobserved confounding. The x-axis shows the required correlation between a hypothetical unobserved confounder and the outcome variable (i.e., $\rho_{\varepsilon,Y}$, *alignment*), while the y-axis represents the proportion of variation in the entropy balancing weights that would need to be explained by such a confounder (i.e., $R^2_{\varepsilon,W}$, *variation explained in weights*) to drive the ITT effect to zero.

Importantly, this sensitivity analysis does not eliminate bias. However, both the RV and contour plots provide an intuitive way to assess the likelihood of unobserved confounding substantively biasing the estimated causal effect. This helps to benchmark results and clearly communicate how robust the conclusions are in the presence of potential unobserved compositional effects.

Applied to the running example, Figure 4.6 indicates that across bandwidths, the estimated increase in government satisfaction after the Charlie Hebdo attacks is quite robust to unobserved confounding. In the larger bandwidths, even a confounder that explains a substantial share of variation in the weights and is moderately aligned with the outcome would not suffice to eliminate the estimated effect. Also under the narrower bandwidths of ± 20 and ± 16 days, observed covariates such as *education*, *employment*, or *voting behavior* fall well outside the "killer confounder" region. This indicates that an unobserved confounder would need to be substantially more influential than the observed covariates to drive the effect to zero. This strengthens the interpretation that the increase in government satisfaction reflects a genuine rally-around-the-flag effect rather than artifacts of compositional shifts in the survey sample.[50]

To further contextualize the potential influence of an unobserved confounder, I utilize the Minimum Relative Confounding Strength (MRCS) as introduced by Hartman and Huang (2024). The MRCS quantifies how strong the influence of an unobserved variable on the weights and outcome would need to be in direct comparison to the influence of an observed covariate. While this information is essentially already depicted in the contour plot, the MRCS breaks it down into a single metric. To make this benchmark more interpretable, I identify the MRCS of the *strongest observed influence*, which I denote as $MRCS_{max}$. The strongest observed influence is the covariate whose omission produces the largest bias, i.e. the greatest combination of imbalance in the entropy weights and alignment with the outcome variable.[51] The $MRCS_{max}$ represents how much stronger (or weaker) the influence of an unobserved confounder would need to be relative to the most influential observed factor. An $|MRCS_{max}| > 1$ indicates that the effect of an unobserved confounder would need to exceed that of the observed covariate. Conversely, an $|MRCS_{max}| < 1$ implies that already a portion of the most influential observed covariate's effect would be sufficient to make this unobserved variable a killer confounder.

In the Charlie Hebdo example, the $MRCS_{max}$ signals high robustness to unobserved confounding across all bandwidths. For the full sample and ± 40 days bandwidth, edu-

[50] As an additional test of this framework and for internal consistency in this thesis, I applied these steps to the analysis of the German snap election call of 2024 from Section 2. Results are detailed in Appendix Section 4.B. The findings prove robust against observable and unobserved compositional biases.

[51] While selecting the empirically most influential covariate as a default benchmark is a broadly applicable approach, other covariates may be more deterministic for the weights or outcomes in a specific case study. It is therefore also worth considering alternative benchmarks that are informed by case-specific knowledge and theoretical expectations to ensure that the chosen benchmark is both empirically informative and substantively meaningful.

cational attainment is the most influential covariate. An unobserved confounder would need to explain around 205-times (full sample) to 688-times (± 40 days bandwidth) as much variation in the weights and outcome like educational. For the ± 20 days bandwidth, unemployment is the most influential covariate. A killer confounder would need to explain around 7-times as much combined variation in weights and the outcome variable than unemployment. When narrowing down the bandwidth to ± 16 days, gender is most influential. With an $MRCS_{max} = -8.45$, a killer confounder would need to have an around 8-times (inverted) stronger influence than identifying as female.

To assess the broader applicability of my proposed framework, I extend the analysis beyond the Charlie Hebdo case and employ this framework to replicate a set of published UESD studies in the following section.

4.5 Extended Application of the Framework: Replication Analyses

By applying my proposed framework to cases that differ in context, data sources, and sampling procedures, I evaluate its consistency and generalizability. For this purpose, I stick to the case of rally-around-the-flag effects in response to terror events. Focusing also the broader application on rally effects following terrorist attacks has two key advantages. First, terrorist events are inherently unexpected, which keeps alternative sources of bias (e.g., the anticipation effects) constant. Second, rally effects among the most well-studied outcomes studied in connection with terror events (Edwards III & Swenson, 1997; Huddy & Feldman, 2011; Huddy et al., 2005) and are frequently analyzed through UESD (Nägel et al., 2024; Vlandas & Halikiopoulou, 2025). Thereby, this comparative application of the framework also resembles a substantive assessment of how established findings like the rally-around-the-flag effect, often framed as having law-like character (Lehrer et al., 2025), are vulnerable to hidden compositional bias.

4.5.1 Replicated Studies and Cases

For the replication analysis, I selected studies based on the following criteria: First, the analyzed treatment has to involve a terrorist event, and the outcome variable needs to capture a typical rally effect, such as government approval or leadership satisfaction. Second, studies had to be published within the past five years (2021–2025) and, broadly, follow the UESD best-practices outlined by Muñoz et al. (2020). Third, I focused on studies published in what are widely considered top-tier political science journals.[52] I excluded all studies without available replication materials.

A literature search[53] led to four papers meeting these criteria (Harding & Nwokolo, 2024; Holman et al., 2022; Nägel et al., 2024; Vlandas & Halikiopoulou, 2025). The analyses presented in these papers jointly cover eleven different terror events and four types of outcome variables (satisfaction with government (Vlandas & Halikiopoulou, 2025), trust in politicians (Nägel et al., 2024), trust in politics (Harding & Nwokolo, 2024) and prime minister approval (Holman et al., 2022)). Altogether, this yields 14 unique combinations of terror events and outcome variables to which I apply the proposed framework. Table 4.2 provides an overview of all replicated cases, the available data, outcome variables and effects identified in the original studies.

Nägel et al. (2024) analyze eight different terror attacks that happened while an ESS survey was in field. They question in how far the rally effect, which was mostly established based on US-focused analyses, translates into the European context. They find that the notion of rally effects' law-like character is mainly driven by the strong effect after the Charlie Hebdo 2015 attacks but is less consistently found after other attacks. While they analyze various dimensions of institutional trust (trust in politicians, parliament, police and the legal system), I replicate their sub-analysis focusing on *trust in politicians*.

Vlandas and Halikiopoulou (2025) examine how jihadist terrorist attacks influence public support for far-right parties in four cases happening during ESS field time. Their main focus lies on party preferences. As part of their broader analysis, they assess how these attacks affect *satisfaction with government*, which is the sub-analysis I replicate. While they find no significant effects of terror attacks on far-right party preferences, they observe post-attack surges in satisfaction with government.

Harding and Nwokolo (2024) examine how terrorist violence by Boko Haram in Nigeria affects political trust and national identity. My replication focuses on their analyses of effects of terrorist attacks on *political trust*, which they find to increase in response to a series of Boko Haram terror attacks.

[52] Included journals are the American Political Science Review, the American Journal of Political Science, the British Journal of Political Science, the Journal of Politics, Political Science Research and Methods, the European Journal of Political Research and Perspectives on Politics.

[53] Web of Science (search link: WoS search) complemented by a GoogleScholar search.

Table 4.2: Overview of replicated analyses

Place	**Date**	**Event summary**	**Data**	**Outcome**	**Effect**	**Paper**
Israel, Tel Aviv	05.01.2003	Suicide bombings near Central bus station	ESS W1	trust in politicians	null	Nägel et al. (2024)
Netherlands, Amsterdam	02.11.2004	Islamist extremist kills filmmaker van Gogh	ESS W2	gov. satisfaction trust in politicians	increase null	Vlandas and Halikiopoulou (2025) Nägel et al. (2024)
Russia, Chechnya	03.12.2008	Caucasus Emirate militants kill family	ESS W4	trust in politicians	null	Nägel et al. (2024)
Sweden, Stockholm	11.12.2010	Islamist motivated suicide bombing	ESS W5	gov. satisfaction	increase	Vlandas and Halikiopoulou (2025)
Russia, Moscow	24.01.2011	Suicide bombing at airport	ESS W5	trust in politicians	null	Nägel et al. (2024)
Israel, Kiryat Malachi	15.11.2012	Hamas rocket attack	ESS W6	trust in politicians	null	Nägel et al. (2024)
Nigeria, several cities	10.12.2014	Multi-site attacks by Boko Haram	Afrobarometer W6	political trust	increase	Harding and Nwokolo (2024)
France, Paris	07.01.2015	Islamist attack at *Charlie Hebdo*	ESS W7	gov. satisfaction trust in politicians	increase increase	Vlandas and Halikiopoulou (2025) Nägel et al. (2024)
Germany, Berlin	19.12.2016	Islamist attack at Christmas market	ESS W8	gov. satisfaction trust in politicians	increase null	Vlandas and Halikiopoulou (2025) Nägel et al. (2024)
UK, Manchester	22.05.2017	Suicide bombing at Ariana Grande concert	BES W12	prime minister approval	decrease	Holman et al. (2022)
France, Strasbourg	11.12.2018	Islamist shooting at Christmas market	ESS W9	trust in politicians	null	Nägel et al. (2024)

Holman et al. (2022) investigate gendered patterns in public opinion responses to the 2017 terrorist attacks in Manchester during Theresa May's tenure as Prime Minister. They find that, contrary to traditional rally effects, public support for May declined following the attacks. While the study explores several outcome variables, I replicate their analysis of *prime minister approval*.

4.5.2 Replication Results

As a starting point, I follow each paper's original analytical setup and model specifications as closely as possible. For reasons of consistency, I however exclude (if not already done so in the original design) all treatment-day observations from the analysis. Detailed case descriptions and stepwise replication results are included in Section 4.C in the Appendix. Figure 4.7 presents the ITT estimates from the baseline and entropy-balanced models for both the full sample and case-specific narrowest bandwidth. The bandwidths are informed by power analyses. For entropy balancing, all time-invariant covariates that are included in the respective model are considered. All dependent variables and continuous explanatory variables were z-standardized before estimation.

The results shown in Figure 4.7 represent a test of the original findings' sensitivity to *observed* compositional bias. Overall, the estimates change only marginally between the full sample models and the bandwidth-restricted ones (left vs. right panel) as well as with or without entropy balancing (dark vs. light blue coefficients). Only in few cases does the result change when controlling for observable compositional bias: Most pronounced, the effect of the 2003 Tel Aviv terror attack on trust in politicians shifts from an insignificant positive effect to a significantly negative one when entropy balancing on the full sample. Both estimates move towards zero under the narrow bandwidth. In a second Israeli case (Kiryat Malachi 2012), the positive effect of trust in politicians also becomes statistically insignificant under entropy balancing on the full sample and when narrowing down the bandwidth. Further, the negative effect of the 2017 Manchester attacks becomes insignificant when entropy balancing and restricting the bandwidth.[54]

Proceeding with the sensitivity analyses to assess potential bias through *unobserved* confounders, Table 4.3 shows the RVs and $MRCS_{max}$ values with the respective bench-

[54]Due to the narrow confidence intervals, they are not visible in the plot. For the full sample, the estimate is $\beta_{base} = -0.13[-0.15; -0.11]$, the entropy-balanced one is $\beta_{balanced} = -0.11[-0.20; -0.01]$. Under bandwidth-restricted samples, these effects are $\beta_{base} = -0.09[-0.12; -0.07]$ and $\beta_{balanced} = -0.05[-0.15; 0.05]$.

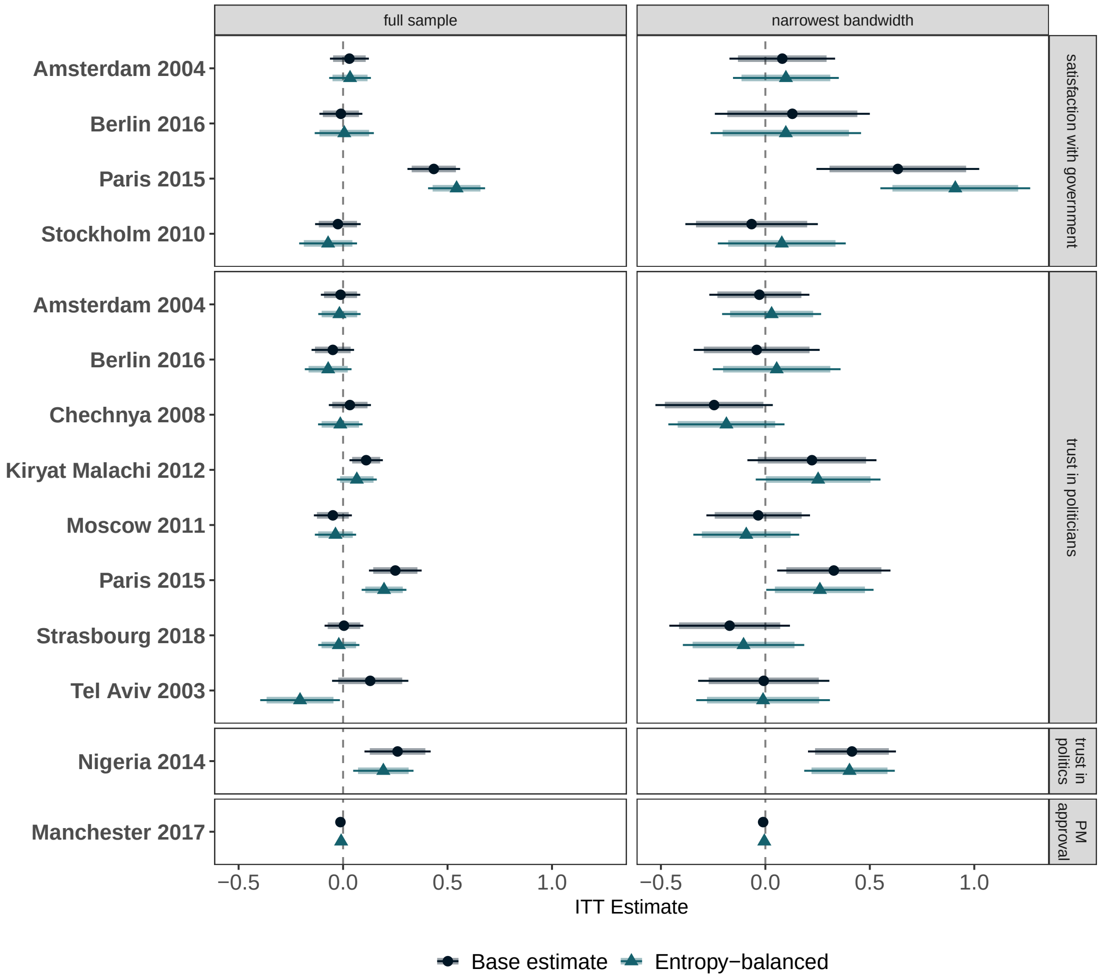

Figure 4.7: ITT estimates per case for both the full sample and the respective narrowest bandwidth. The base estimates for the unbalanced model are contrasted against estimates based on the entropy-balanced sample. Thin lines represent the 95% confidence intervals, thicker lines the 90% confidence intervals. All continuous variables were z-standardized before estimation.

mark covariate. Consistent with the Charlie Hebdo analysis, the bias threshold is set to $b^* = 0$, i.e. defining that an unobserved confounder would bias the estimate to zero. With one of the highest RVs both for the full sample and narrowest bandwidth, the Charlie Hebdo case proves highly robust, both when satisfaction with government (B) or trust in politicians (A) is considered as outcome variable. In the former, having children in the household is the most influential observed covariate; an unobserved confounder similar to this variable would need to exert an (inverted) effect around 85-times as strong as having children does. Similarly, in case of the analysis of satisfaction with government, a potential killer confounder would need to influence the entropy weights and outcome

Table 4.3: Robustness values and MRCS values for the full sample and narrowest bandwidth per case. (A) indicates cases with *trust in politicians* as outcome variable, (B) with *satisfaction with government*, (C) with *prime minister approval* and (D) with *trust in politics*.

	full sample			*narrowest bandwidth*		
	$RV_{b^*=0}$	$MRCS_{max}$	**benchmark variable**	$RV_{b^*=0}$	$MRCS_{max}$	**benchmark variable**
Paris 2015 (B)	0.79	-85.2	children	0.57	32.4	age
Manchester 2017 (C)	0.64	8.3	labour party ID	0.37	4.5	labour party ID
Tel Aviv 2003 (A)	0.60	8.3	age	0.01	0.1	age
Paris 2015 (A)	0.48	-24.4	education	0.32	49.4	age
Nigeria 2014 (D)	0.27	-2.3	urban	0.56	-3.1	urban
Berlin 2016 (A)	0.21	7.8	age	0.17	-1.8	age
Stockholm 2010 (B)	0.21	-13.8	rural area	0.22	4.3	rural area
Amsterdam 2004 (B)	0.20	5.3	female	0.32	-3.7	education
Strasbourg 2018 (A)	0.14	-2.0	age	0.36	-7.2	age
Kiryat Malachi 2012 (A)	0.12	-3.2	HH income	0.40	-31.5	female
Amsterdam 2004 (A)	0.11	22.7	age	0.13	-0.8	education
Chechnya 2008 (A)	0.04	-1.1	female	0.44	15.4	female
Moscow 2011 (A)	0.04	-26.0	age	0.35	12.3	age
Berlin 2016 (B)	0.02	-0.5	age	0.25	-7.2	children

variable around 24-times as strongly (inverted) as educational attainment. In both cases, this indicates high robustness to unobserved confounding. Another case very robust to unobserved compositional bias is the Manchester attack of 2017. Across all bandwidths, the most influential variable is identifying with the Labour party. Considering that the outcome variable is approval of Tory Prime Minister May, a Labour party identity is also highly deterministic on theoretical level. An unobserved confounder would need to be around four- (narrowest bandwidth) to eight-times (full sample) as influential as identifying with the Labour party to overturn the negative effect of the Manchester attacks on May approval.

Some less robust cases are worth highlighting. In case of the Tel Aviv 2003 attacks, it seems like the effect on trust in politicians is mainly driven by differences between early and late respondents. This is indicated by the much lower RV and $MRCS_{max}$ under the narrow bandwidth than the full sample. Further, while the change of the estimate through entropy balancing (see Figure 4.7) already pointed towards observable compositional bias, the low $MRCS_{max} = 0.1$ under the narrow bandwidth shows that unobserved compositional bias also likely diminishes the observed effect. Similar sensitivity to potential killer confounders is also observable for the Berlin 2015 case. Under the full sample, an unobserved confounder similar to the variable age would only need to exert half of the age-effect to bias the estimate to zero. Contrary to the Tel Aviv 2003 case, the result is more robust under the narrowest bandwidth, which indicates that the pre- and post-event samples are more balanced closer to the day of the attack. Overall, such sensitive cases in which a fraction of the strongest observed influence would suffice to overturn substantive conclusions are rare. For the majority of replicated cases, the conclusions would not change under unobserved compositional bias.

4.6 Discussion and Conclusion

This paper examined a key vulnerability of event-focused and survey-based research designs, namely that incisive events may change not only attitudes, preferences and political behavior, but also who responds to surveys. To separate events' effects on political opinion from effects on survey responsiveness and sample compositions, I developed a framework building upon best-practices for the UESD approach. This framework combines established adjustments for *observable* imbalances in pre- and post-event sample compositions with sensitivity analyses to assess how strong *unobserved* imbalances would need to be to alter substantive conclusions. I illustrated the framework using the case of the 2015 Charlie Hebdo terrorist attacks in Paris. By additionally replicating 14 published UESD studies on terrorist events and rally-type outcomes, I evaluated this framework's broader applicability and the robustness of the studies' original effects.

In doing so, this paper produces both methodological and substantive insights. First, it demonstrates that political events can indeed reshape survey samples, even within very narrow temporal windows around an event. This implies that survey response behavior is not static but reactive to political context in ways that matter for measurement and causal inference. Second, across both the Charlie Hebdo case and the replication analyses, these observable and unobserved compositional shifts and resulting measurement biases tend to be limited in magnitude. After adjusting for observable imbalances, estimated event-effects remain largely stable. Assessing the potential of unobserved compositional bias, sensitivity analyses suggest that unobserved shifts in the pre- and post-sample compositions would, more often than not, need to be implausibly strong to overturn substantive conclusions. Taken together, these findings indicate that while event-induced nonresponse is a real phenomenon, it does not automatically undermine causal conclusions that are drawn from event-focused survey designs.

At the same time, it is important to highlight that while the framework is deliberately designed to be broadly applicable and to offer standardized diagnostic tools, it becomes most expressive when tailored to the specific case-context. This holds both for the steps to mitigate observable and to gauge the impact of unobservable compositional bias: With respect to observable compositional bias, the framework relies on entropy balancing to reweigh samples along relevant covariates. In this context, the choice of balancing variables is not merely a technical decision but a substantively consequential one. Selecting sensible covariates for entropy balancing should be informed by the characteristics of the case under study and by theoretical expectations regarding which time-invariant factors are plausibly imbalanced between samples. In practice, this implies that instead of mechanically including all available pre-treatment covariates, it should be reflected on which characteristics most likely differ across groups in ways that matter for the outcome variable. These considerations may stem from prior literature, institutional knowledge, or contextual features of the setting. The descriptive assessments of sample imbalances are crucial in this process and should inform the covariate selection for the entropy balancing model.

Moreover, while I propose to prioritize time-invariant covariates for balancing to avoid post-treatment bias, there may be settings in which including time-varying variables is justifiable or even desirable. If there are strong theoretical or empirical reasons to assume that time-varying characteristics are, first, related to survey responsiveness and the outcome variable and, second, clearly unaffected by the treatment event, incorporating them may reduce compositional differences.

Relatedly, the assessment of unobservable compositional bias also highly depends on researcher-defined assumptions. As this step relies on hypothetical confounders, the quantified likelihood of unobserved bias is, by design, sensitive to, e.g., the specification of the baseline model, the bias threshold, and the selection of benchmarking variables:

The bias threshold is quite determinate of how likely compositional bias is estimated to be by the sensitivity analyses. The default bias threshold of $b^* = 0$ is deliberately conservative and suited for general-purpose applications. However, depending on the case and outcome variable under analysis, this threshold may not always be the most expressive one; there are contexts where smaller amounts of bias could already threaten key causal conclusions. Consider, for example, binary outcomes. Here, even modest unobserved bias could be sufficient to reverse the predicted outcome, such as changing the winner in a closely contested electoral race. Similarly, when outcomes are continuous but linked to meaningful cutoffs (such as, e.g., vote shares thresholds for entering parliament) bias thresholds should be defined relative to these critical values rather than the null effect.

Further, the choice of benchmarking variables for the contour plots and the MRCS value need not be solely data-driven. While my framework is broadly applicable by benchmarking against the empirically most influential variable (identified via the $MRCS_{max}$), alternative benchmarks may be more informative. Variables that are empirically less influential may still be theoretically decisive for the treatment, the outcome, or both. The benchmark selection should therefore be guided by case-specific knowledge and theoretical expectations. Such adaptations can yield a more fine-grained and sound understanding of the potential for an unobserved confounder to bias results.

While such case sensitivity is essential for meaningful insights into compositional bias, it limits the framework's direct applicability across empirical contexts. Its performance may thus vary across different research contexts. Future research could therefore apply and adapt the framework to a broader range of cases and outcomes. Doing so would test the robustness of the approach itself and improve our understanding of how often compositional bias arises and how consequential it is across different events and outcome phenomena.

This links to another constraint more substantive in nature. While terror events are clear-cut treatment and a useful testing ground for the framework, other political events may differ in their susceptibility to compositional bias. Future research could extend the framework to events more directly embedded in political processes, such as scandals or election-related events. These could exert more immediate effects on political outcomes while simultaneously affecting responsiveness (e.g., through frustration or disengagement following unfavorable political developments). Studying such cases would clarify how compositional bias varies across event types, the conditions under which it arises, and the outcomes for which it most likely threatens causal inference. Insights from such applications could also facilitate extending the framework to other event-based research designs, including regression discontinuity and difference-in-differences approaches.

Overall, this paper advances the central argument of the thesis by showing that political events can affect not only what people think but also who is observed in survey data, and that both processes matter for valid causal inference. By providing a practical framework to account for observable compositional bias and to assess the robustness to unobservable compositional bias, it helps to understand the conditions under which survey-based estimates can be interpreted as reflecting genuine opinion change rather than artifacts of shifting survey participation.

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Appendix

4.A Supplementary Material for the *Charlie Hebdo* Running Example

Table 4.A.1: OLS regression results for baseline models of the effect of the Charlie Hebdo terrorist attacks on satisfaction with government

	Dependent variable:			
	Satisfaction with government			
	(1)	(2)	(3)	(4)
ITT effect	0.088^{***}	0.085^{***}	0.090^{***}	0.092^{***}
	(0.013)	(0.027)	(0.013)	(0.027)
Running days around event		0.001		0.0005
		(0.0005)		(0.0005)
Treatment × running days		-0.002^{*}		-0.002
		(0.001)		(0.001)
Constant	0.274^{***}	0.297^{***}		
	(0.005)	(0.019)		
Observations	1,873	1,873	1,873	1,873
R^2	0.024	0.026	0.024	0.025
Adjusted R^2	0.024	0.024	0.013	0.013
Region fixed-effects			✓	✓
Note:	*p<0.1; **p<0.05; ***p<0.01			

Table 4.A.2: Descriptive means and sample sizes by bandwidth and treatment status

	(A) full sample		(B) ±40 days		(C) ±30 days		(D) ±20 days		(E) ±10 days		(F) ±8 days	
Variable	control	treatment	control	treatment	control	treatment	control	treatment	control	treatment	control	treatment
Sample size	1574	291	895	286	415	258	111	195	35	128	31	110
Satisfaction with government	0.27	0.36	0.27	0.36	0.29	0.36	0.29	0.38	0.31	0.38	0.32	0.37
Voted last election	0.62	0.60	0.62	0.60	0.62	0.58	0.64	0.57	0.56	0.57	0.61	0.55
Age (years/10)	5.02	4.82	4.96	4.85	4.97	4.87	4.93	4.85	4.88	4.79	4.86	4.71
Female	0.52	0.52	0.53	0.52	0.55	0.54	0.46	0.58	0.47	0.55	0.42	0.55
Education (years/10)	1.27	1.36	1.30	1.36	1.33	1.35	1.36	1.33	1.32	1.32	1.37	1.31
Unemployed	0.06	0.06	0.05	0.05	0.03	0.06	0.03	0.07	0.03	0.06	0.03	0.06
Cooperativeness	0.91	0.88	0.91	0.88	0.91	0.88	0.92	0.87	0.91	0.88	0.92	0.88
Interview attempts (div. by 10)	0.35	0.64	0.40	0.64	0.44	0.64	0.49	0.61	0.42	0.58	0.42	0.58

Table 4.A.3: OLS regression results for bandwidth-restricted and entropy-balanced models of the effect of the Charlie Hebdo terrorist attacks on satisfaction with government

	Dependent variable:							
	Satisfaction with government							
	full sample		± 40 days		± 20 days		± 16 days	
ITT effect	0.090***	0.119***	0.092***	0.111***	0.089***	0.087***	0.117***	0.106***
	(0.013)	(0.012)	(0.015)	(0.013)	(0.028)	(0.026)	(0.041)	(0.035)
Observations	1,873	1,832	1,181	1,181	305	305	213	213
R^2	0.024	0.025	0.032	0.032	0.034	0.034	0.040	0.040
Adjusted R^2	0.013	0.014	0.014	0.014	−0.034	−0.034	−0.060	−0.060
Entropy-balanced		✓		✓		✓		✓

Note: *p<0.1; **p<0.05; ***p<0.01

Table 4.A.4: OLS regression results for different bandwidths including all explanatory variables that were used for entropy balancing.

	Dependent variable: Satisfaction with government							
	full sample		± 40 days		± 20 days		± 16 days	
ITT effect	0.091***	0.149	0.092***	0.106	0.083***	0.237	0.114***	0.832*
	(0.014)	(0.093)	(0.015)	(0.102)	(0.029)	(0.245)	(0.043)	(0.496)
Age	−0.006*	−0.007*	−0.005	−0.007	−0.015*	−0.009	−0.022**	−0.034
	(0.003)	(0.003)	(0.004)	(0.005)	(0.009)	(0.015)	(0.011)	(0.026)
Female	0.012	0.021**	−0.003	0.008	0.016	0.094**	0.022	0.129*
	(0.010)	(0.011)	(0.012)	(0.014)	(0.027)	(0.044)	(0.033)	(0.075)
Education (years)	0.002	0.0001	−0.005	−0.011	−0.015	0.001	−0.020	0.097
	(0.013)	(0.014)	(0.017)	(0.020)	(0.033)	(0.055)	(0.041)	(0.118)
Unemployed	−0.018	−0.035	−0.022	−0.049	−0.050	−0.245*	−0.060	−0.210
	(0.021)	(0.023)	(0.028)	(0.033)	(0.058)	(0.133)	(0.066)	(0.178)
Voted last election	−0.006	−0.003	0.001	0.009	0.012	−0.009	0.027	−0.030
	(0.011)	(0.012)	(0.014)	(0.017)	(0.031)	(0.053)	(0.039)	(0.089)
Cooperativeness	0.045*	0.062**	0.029	0.045	−0.087	−0.048	−0.020	0.577
	(0.027)	(0.030)	(0.034)	(0.040)	(0.071)	(0.169)	(0.090)	(0.430)
Treatment × age		0.006		0.007		−0.007		0.018
		(0.008)		(0.009)		(0.018)		(0.029)
Treatment × female		−0.056**		−0.046		−0.123**		−0.145*
		(0.026)		(0.029)		(0.056)		(0.083)
Treatment × education		0.009		0.019		−0.024		−0.132
		(0.034)		(0.038)		(0.068)		(0.125)
Treatment × voted		0.107*		0.106		0.236		0.196
		(0.058)		(0.065)		(0.148)		(0.194)
Treatment × cooperativeness		−0.020		−0.032		0.025		0.055
		(0.030)		(0.033)		(0.064)		(0.098)
Constant		−0.068		−0.042		−0.053		−0.631
		(0.067)		(0.074)		(0.187)		(0.440)
Observations	1,832	1,832	1,181	1,181	305	305	213	213
R^2	0.031	0.037	0.035	0.041	0.050	0.078	0.063	0.114
Adjusted R^2	0.017	0.019	0.012	0.013	−0.039	−0.030	−0.067	−0.044

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

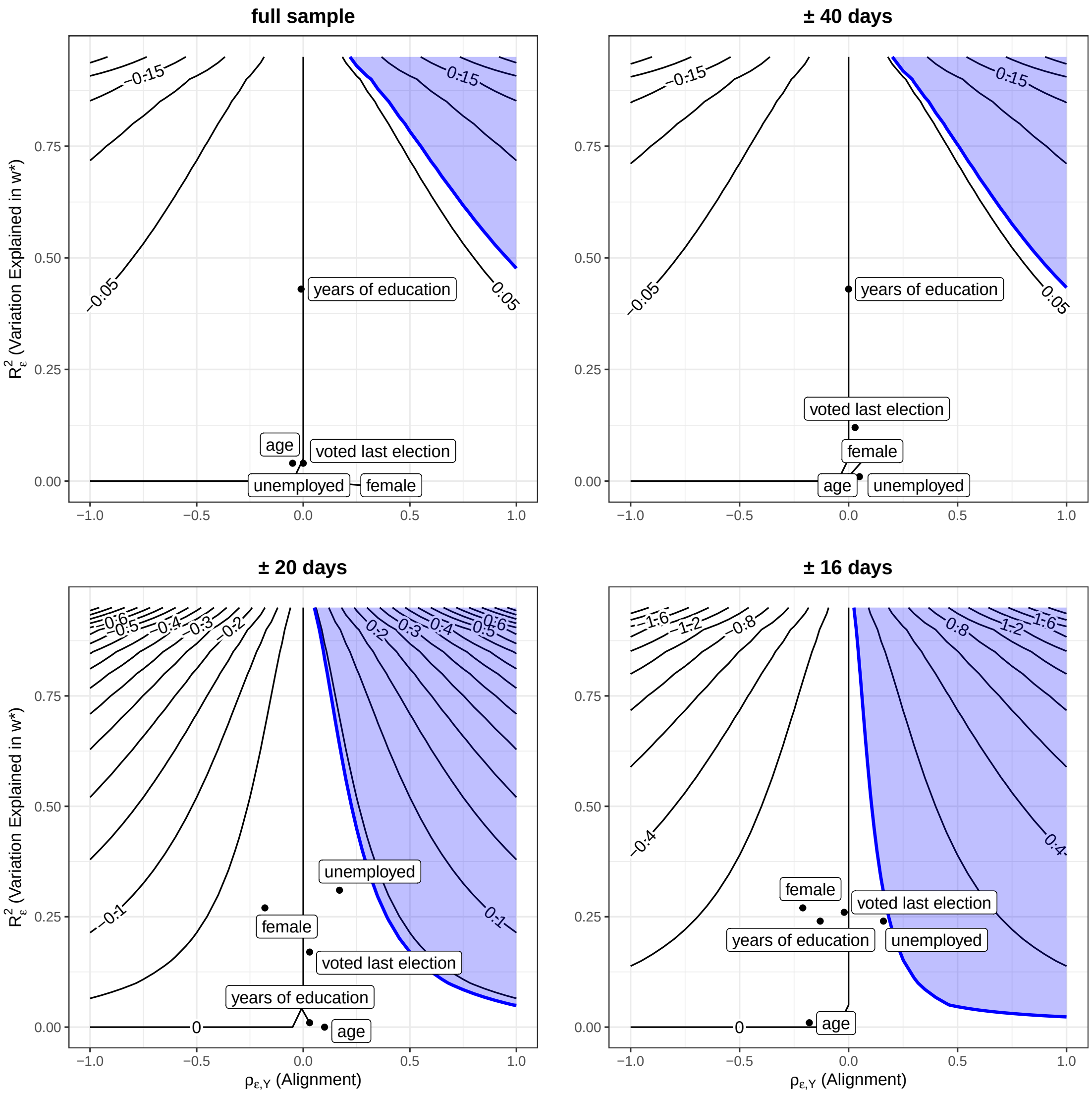

Figure 4.A.1: *Contour plots* for the *Charlie Hebdo* case. The plots visualize the robustness of the estimated ITT effect to potential unobserved confounding. The x-axis shows the required correlation between a hypothetical unobserved confounder and the outcome variable (i.e., $\rho_{\varepsilon,Y}$, *alignment*), while the y-axis represents the proportion of variation in the entropy balancing weights that would need to be explained by such a confounder (i.e., $R^2_{\varepsilon,W}$, *variation explained in weights*) to half the ITT effect.

4.B Assessing Potential Bias in the Analysis of Snap Election Calls' Effects on Vote Intention Uncertainty (from Section 2)

In this appendix section, I apply the proposed framework to the study of Paper 1, that examines how snap election announcements affect voter uncertainty (Section 2). This serves as an additional robustness check and ensures consistency of the analytical approaches employed throughout this thesis. The analysis in Section 2 includes the case of the German snap election call in 2024 and that of the UK General election of 2024. While the former is analyzed by the UESD approach, the latter relies on a panel design and is not suited for this framework. I therefore focus this robustness check of Paper 1 on the German snap election call of 2024.

The majority of the proposed framework's steps to address compositional bias are already conducted in Section 2, particularly those to identify and control for *observable* compositional bias. Figure 2.3 displays the estimates of the effect of the snap election call on vote choice uncertainty based on a naïve baseline model (only including the treatment indicator as explanatory variable) in unbalanced and balanced form and the entropy-balanced core model including an interaction of the treatment with partisan strength. This already indicates that controlling for observable compositional bias through entropy balancing drives the effect of the naïve model to zero, however does not invalidate the effect obtained from the core model. To assess how these estimates shift under narrower bandwidths of days around the snap election call, models are re-estimated for bandwidths of ± 8, ± 4 and ± 2 days. This choice of bandwidths is supported by results of a power analysis (for details, see Figure 4.B.1): Due to the large sample size of the GLES refresher survey wave (GLES, 2025), there is sufficient statistical power even under the narrowest possible bandwidth.

Figure 4.B.2 displays the differences in means between the treatment and control groups for selected variables across all bandwidths. By tendency, differences shrink with narrower bandwidths. For instance, differences in high and intermediate educational attainment that are still present up until a bandwidth of ± 8 days around the event shrink markedly with decreasing bandwidth sizes. Nevertheless, even under a ± 2 day bandwidth do systematic differences occur regarding age and education. These results support what is also implied by Figure 2.2: there are observable imbalances between the treatment and control group that should be analytically accounted for. Further summary statistics for treatment and control groups across bandwidths are shown in Table 4.B.1.

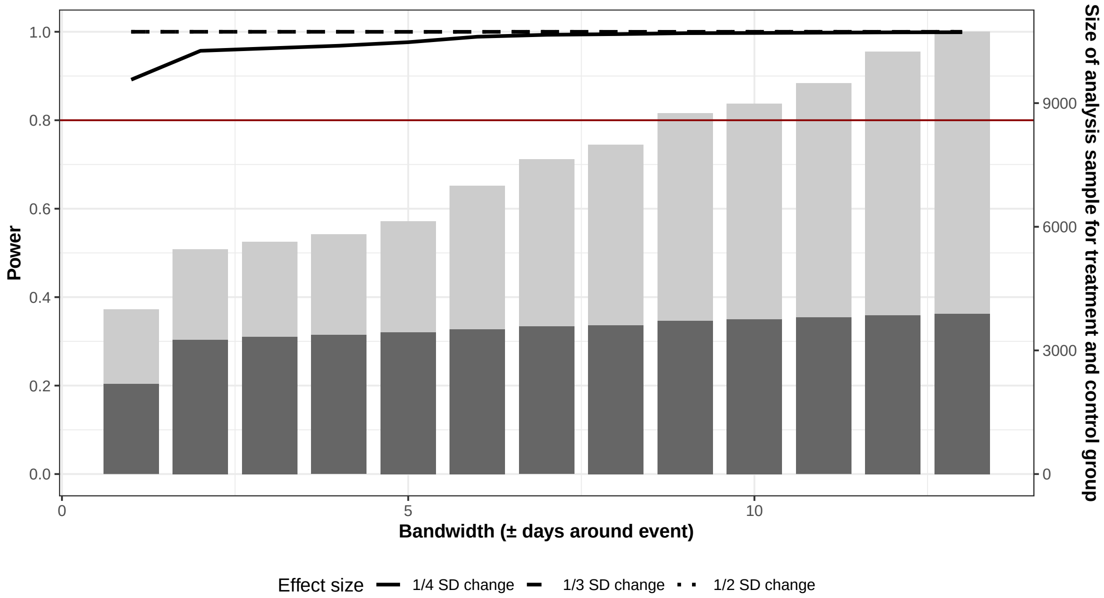

Figure 4.B.1: Power analysis for different bandwidths between ± 1 to ± 13 days around the German snap election call of November 2024. Different lines indicate the estimated power for observing change in vote intention uncertainty between 1/10 to 1/2 of the standard deviation of government satisfaction in response to the event. Dark bars indicate the treatment group sample size, lighter bars indicate the control group sample size.

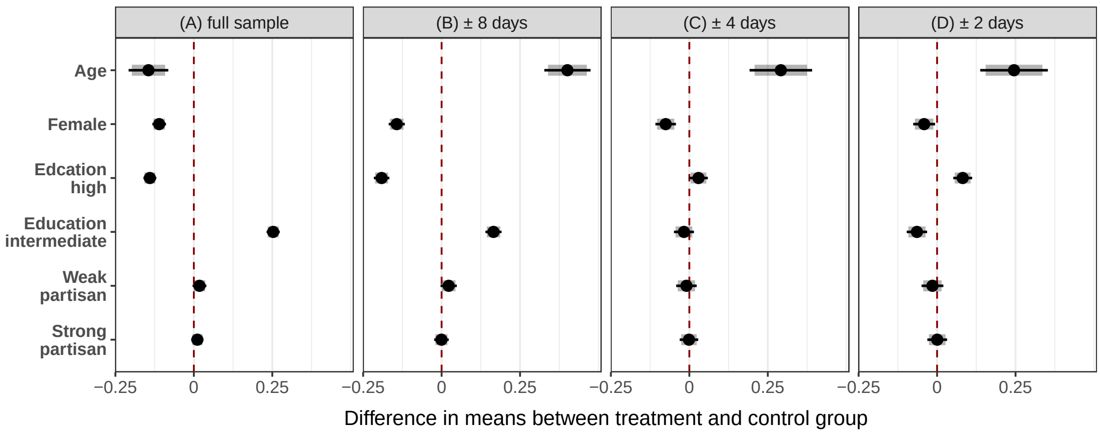

Figure 4.B.2: Difference in means between pre- and post snap election call samples and difference bandwidth of days around this treatment. *Age* are years divided by 10. Bold lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the treatment and control group per bandwidth are included in Table 4.B.1.

Table 4.B.1: Descriptive means and sample sizes by bandwidth and treatment status

	(A) full sample		(B) ±8 days		(C) ±4 days		(D) ±2 days	
Variable	control	treatment	control	treatment	control	treatment	control	treatment
Sample size	5490	2797	3217	2555	1440	2343	1215	2244
Uncertain vote intention	0.08	0.07	0.08	0.06	0.07	0.06	0.06	0.06
Weak partisan	0.27	0.28	0.28	0.28	0.28	0.28	0.28	0.29
Strong partisan	0.53	0.53	0.51	0.54	0.54	0.53	0.55	0.53
Age	4.65	4.53	4.21	4.61	4.41	4.70	4.49	4.74
Female	0.56	0.46	0.60	0.46	0.54	0.46	0.51	0.47
Education intermediate	0.43	0.29	0.49	0.30	0.28	0.31	0.22	0.30
Education high	0.26	0.52	0.40	0.56	0.63	0.61	0.70	0.64

Informed by these descriptive assessments, re-estimating the models under entropy balancing and for different bandwidths reveals that the identified effect of the snap election call proves robust against observable compositional bias. Besides the results already displayed in Figure 2.5 in Section 2, a direct comparison of unbalanced and balanced[55] results across bandwidths is shown in Figure 4.B.3. Only in the narrowest bandwidth is the effect not strictly indifferent from zero. Overall, the results imply that compositional bias is very unlikely to overturn or artificially create these substantive findings.

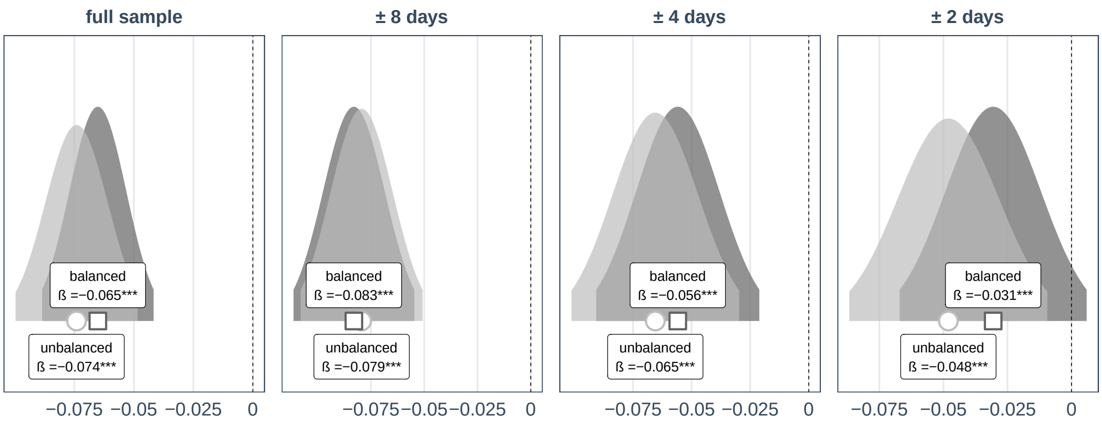

Figure 4.B.3: ITT effect of the snap election call on vote intention uncertainty for different bandwidths and based on non-balanced vs. entropy-balanced samples. All models include the treatment indicator as explanatory variable and an interaction of the treatment with partisan strength. The sample sizes are $n = 8287$ (full sample), $n = 5772$ (±8 days), $n = 3783$ (±4 days), $n = 3459$ (±2 days).

[55] Weights are obtained through entropy balancing (Hainmueller, 2012), for which the variables age, gender and educational attainment are taken into account.

As the final step, I assess these findings' sensitivity by estimating robustness values. Figure 4.B.4 shows these RVs for the four different bandwidths and across a range of potential proportions of the ITT effect that would be biased by an unobserved confounder. The plot shows that the robustness of the ITT estimate, by tendency, increases with wider bandwidths around the snap election call. For the full sample, an unobserved confounder would need to explain a substantial portion (up to 50%) of the variation in both the outcome and the weights to reduce or eliminate the estimated effect, indicating high robustness. Narrower bandwidths (especially ±2 and ±4 days) require less explanatory power from a confounder to alter the results. This suggests that estimates based on very narrow time windows are more vulnerable to unobserved compositional bias. Nevertheless, to reduce the ITT effect to zero, an unobserved confounder would need to explain around 25% of variation in the outcome variable and weights even under the narrowest bandwidth. To allow for an assessment of the actual likelihood of such bias, these robustness values are put into context of observed effects in the contour plot in Figure 4.B.5.

Figure 4.B.5 shows that across all bandwidths, the estimated ITT effect of the snap election call on vote intention uncertainty demonstrates a high degree of robustness to potential unobserved confounding. In the full sample and the ±8 day window, an un-

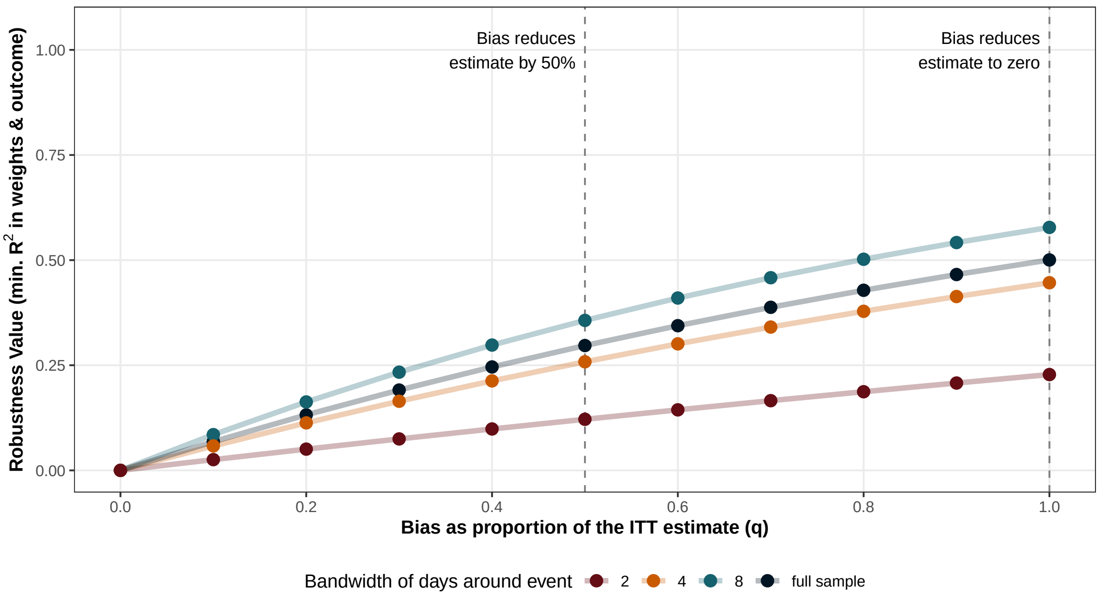

Figure 4.B.4: *Robustness values*: Minimum explained variation in the outcome variable and entropy balancing weights needed for an unobserved confounder to substantively change the ITT estimate.

observed confounder would need to be substantially stronger than any of the observed covariates age, gender, or educational attainment in explaining both the survey weights and the outcome variable to overturn the effect. Even in the narrower windows (± 4 and ± 2 days), where sensitivity increases slightly, the hypothetical confounder would still need to exceed the explanatory power of most observed variables to pose a serious threat to causal conclusions.

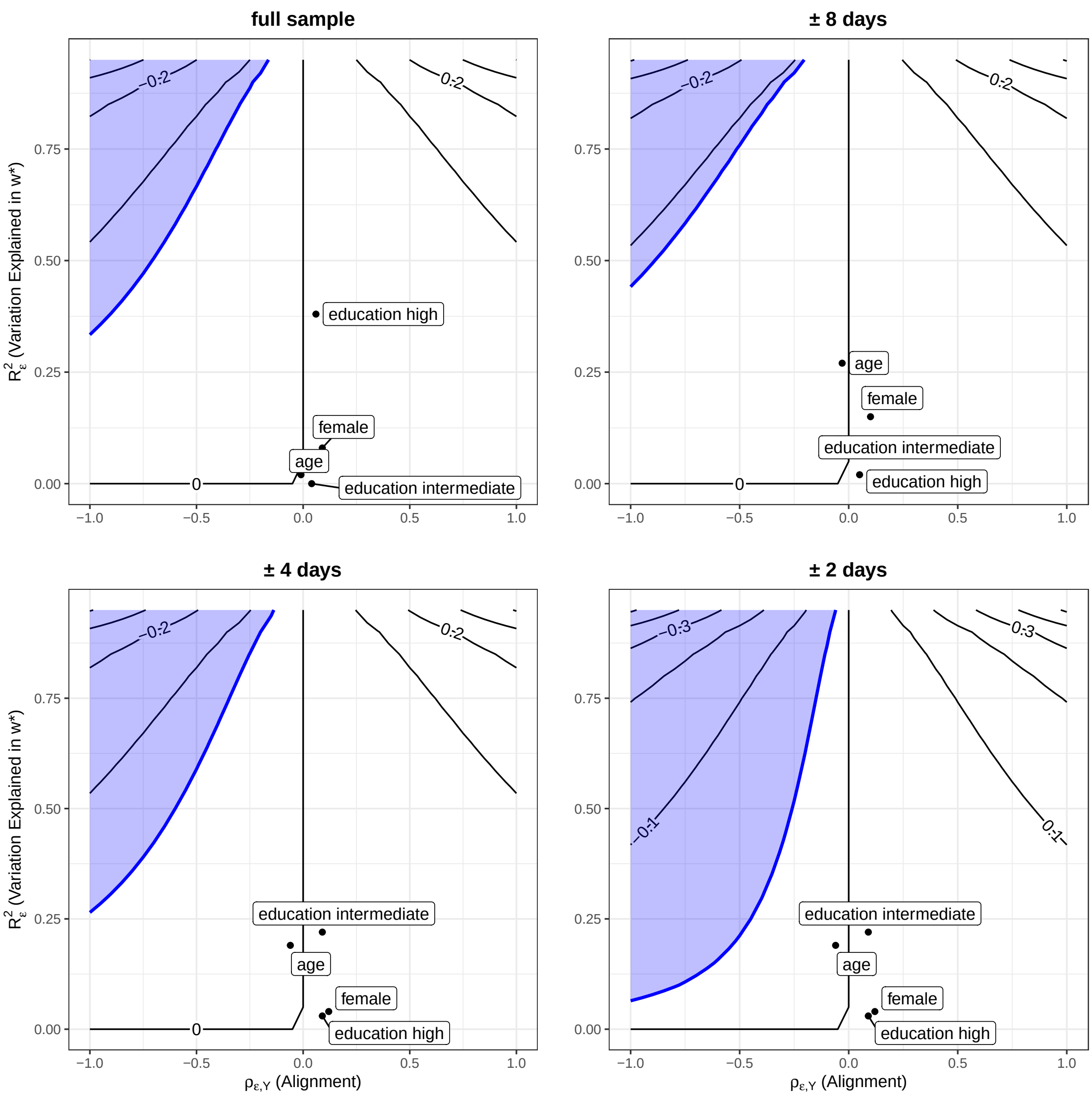

Figure 4.B.5: Contour plot for the snap election case from Section 2 for different bandwidths around the snap election call. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Assessing the $MRCS_{max}$ supports what the contour plots already demonstrate: the results seem to be highly robust to unobserved confounding across all four bandwidths. For the full sample, high levels of education is the most influential covariate. An unobserved confounder would need to explain around 8-times (inverted) as much variation in the entropy weights and outcome variable compared to high education to drive the causal estimate to zero. Identifying as female is most influential when considering the ± 8 days bandwidth, with an $MRCS_{max} = -12.1$. Under a bandwidth of ± 4 days, age is most influential ($MRCS_{max} = 26.9$. Finally, when narrowing down the bandwidth to ± 2 days, the most influential observed covariate is intermediate education. For an unobserved confounder to be a killer confounder, it would need to explain around 11-times (inverted) as much combined variation in weights and the outcome variable than intermediate education. Having already assessed that *observable* compositional bias has not threatened the core findings, these sensitivity results suggest that the identified effect is also unlikely to be driven by compositional bias from *unobserved* factors.

4.C Replication Analyses

Case 1: Israel, Tel Aviv, January 2003 (Nägel et al., 2024)

On January 5, 2003, two suicide bombers carried out coordinated attacks near Tel Aviv's Central Bus Station during evening rush hour, killing 23 civilians and injuring more than 100. Both the Al-Aqsa Martyrs Brigade and Islamic Jihad briefly claimed responsibility but later retracted their statements, leaving the perpetrators unclear. The attack was Israel's deadliest since March 2002 and highlighted ongoing instability in the Israeli–Palestinian conflict.

Table 4.C.1.1: Regression results for the Tel Aviv 2003 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±14 days)	(full)	(±14 days)	(full)	(±14 days)
ITT effect	0.130	−0.008	0.060	−0.122	-0.206^{**}	−0.011
	(0.093)	(0.160)	(0.108)	(0.163)	(0.097)	(0.163)
Age (years/10)			0.090^{***}	0.295^{***}		
			(0.025)	(0.086)		
Female			0.039	0.061		
			(0.049)	(0.156)		
Net HH income			−0.016	−0.040		
			(0.027)	(0.101)		
Education (years/10)			-0.043^{*}	0.026		
			(0.025)	(0.088)		
Minority group member			0.074	−0.322		
			(0.072)	(0.256)		
Observations	2,400	169	1,720	169	1,720	169
R^2	0.001	0.00001	0.013	0.087	0.0004	0.00001
Adjusted R^2	−0.002	−0.043	0.006	0.016	−0.004	−0.043
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01

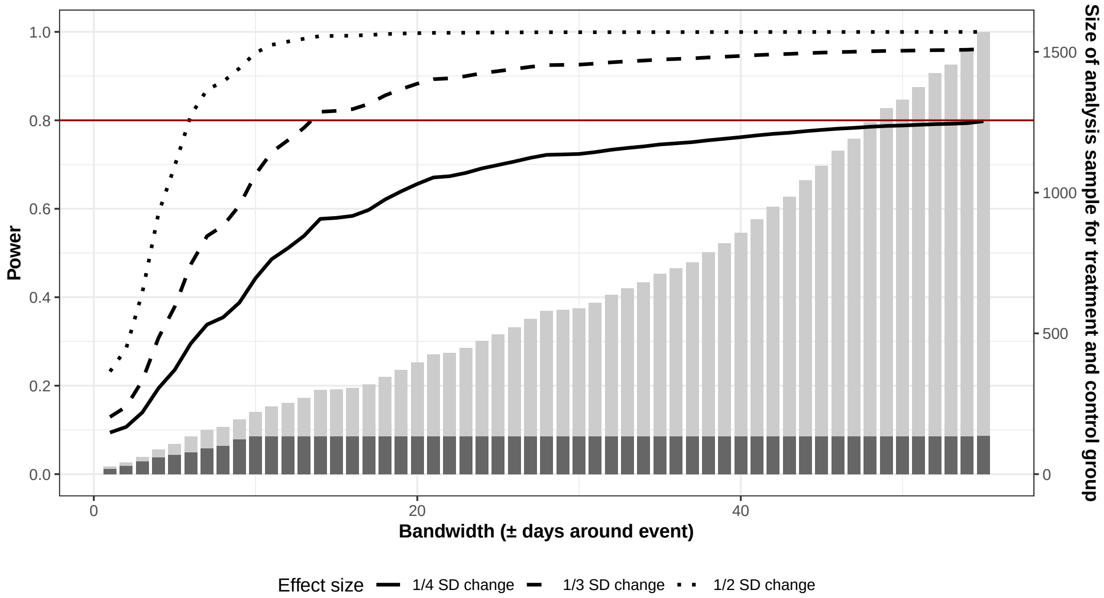

Figure 4.C.1.1: Power analysis for different bandwidths around the Tel Aviv terrorist attack of January 2003. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

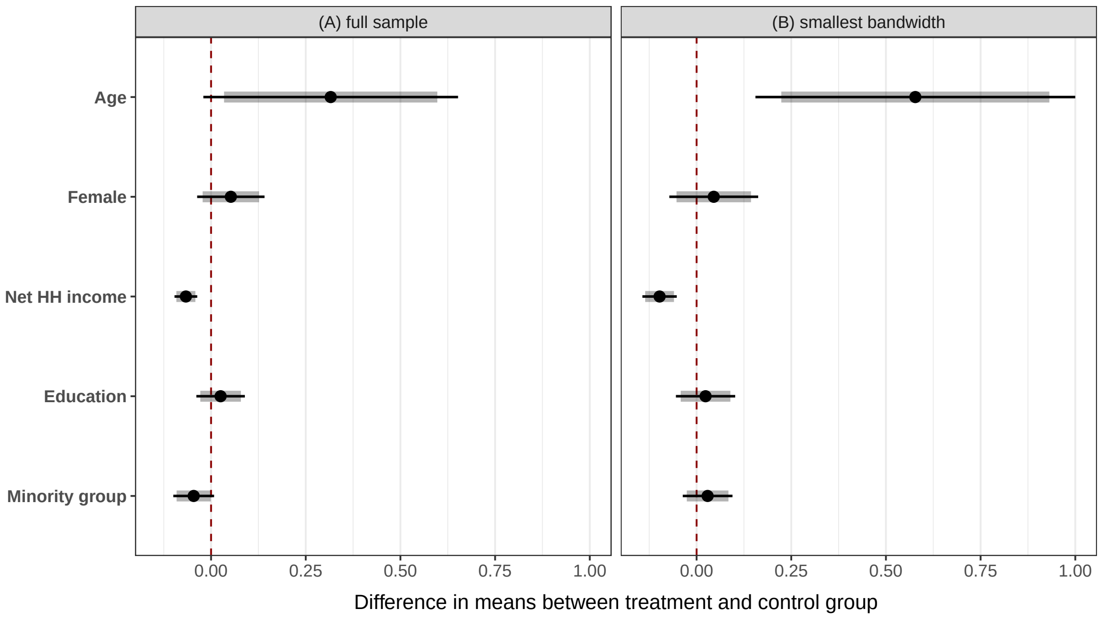

Figure 4.C.1.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 1627$ and $n_{treat} = 93$. For the narrowest bandwidth of ± 14 days, these are $n_{control} = 77$ and $n_{treat} = 92$.

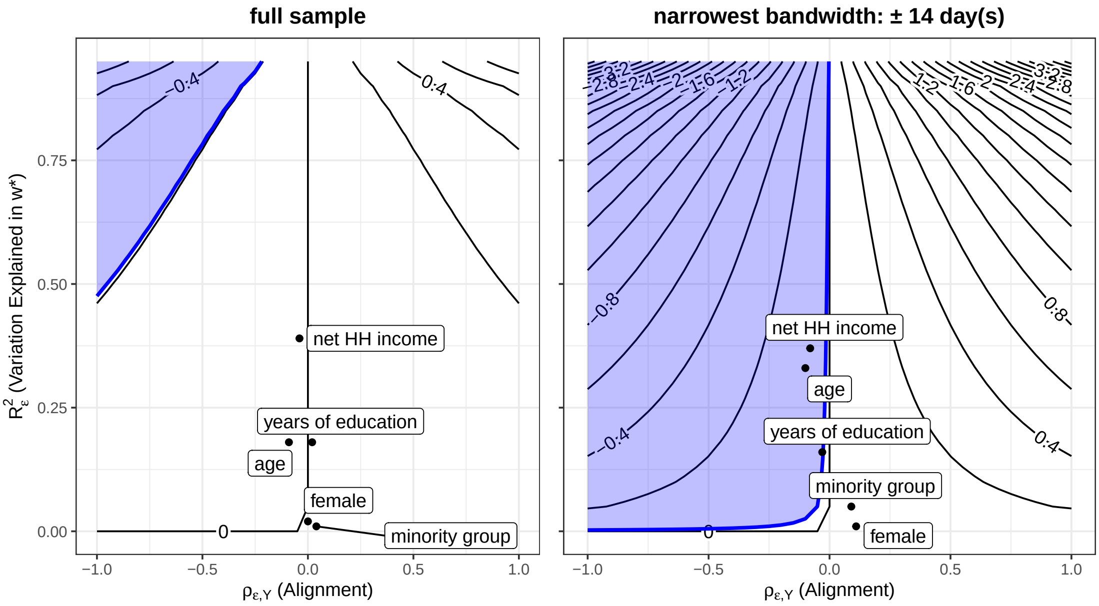

Figure 4.C.1.3: Contour plot for the Tel Aviv 2003 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 2: Netherlands, Amsterdam, November 2004 (Nägel et al., 2024)

On November 2, 2004, Dutch filmmaker Theo van Gogh was murdered in Amsterdam by Mohammed Bouyeri, a Dutch-Moroccan extremist, while cycling to work. Bouyeri left a letter threatening Ayaan Hirsi Ali, Western societies, and Jews, reflecting radical Islamist ideology. The attack, linked to the Hofstad Network, shocked the Netherlands and reignited debates over extremism and free expression.

Table 4.C.2.1: Regression results for the Amsterdam 2004 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±9 days)	(full)	(±9 days)	(full)	(±9 days)
ITT effect	−0.012	−0.029	−0.024	−0.062	−0.018	0.030
	(0.048)	(0.122)	(0.051)	(0.122)	(0.052)	(0.121)
Age (years/10)			0.020	0.112^{*}		
			(0.028)	(0.068)		
Female			−0.029	0.039		
			(0.051)	(0.127)		
Net HH income			0.087^{***}	0.024		
			(0.027)	(0.064)		
Education (years/10)			0.159^{***}	0.205^{***}		
			(0.028)	(0.070)		
Minority group member			0.022	0.398		
			(0.120)	(0.304)		
Observations	1,829	245	1,589	245	1,589	245
R^2	0.00004	0.0003	0.040	0.053	0.0002	0.0003
Adjusted R^2	−0.022	−0.167	0.012	−0.133	−0.026	−0.167
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01

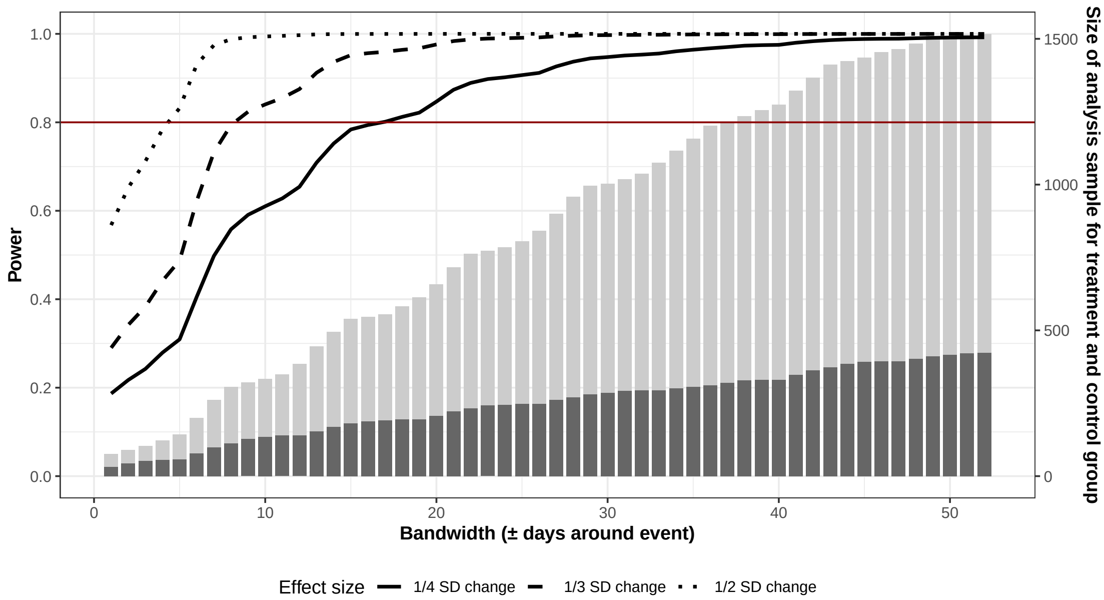

Figure 4.C.2.1: Power analysis for different bandwidths around the Amsterdam 2004 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

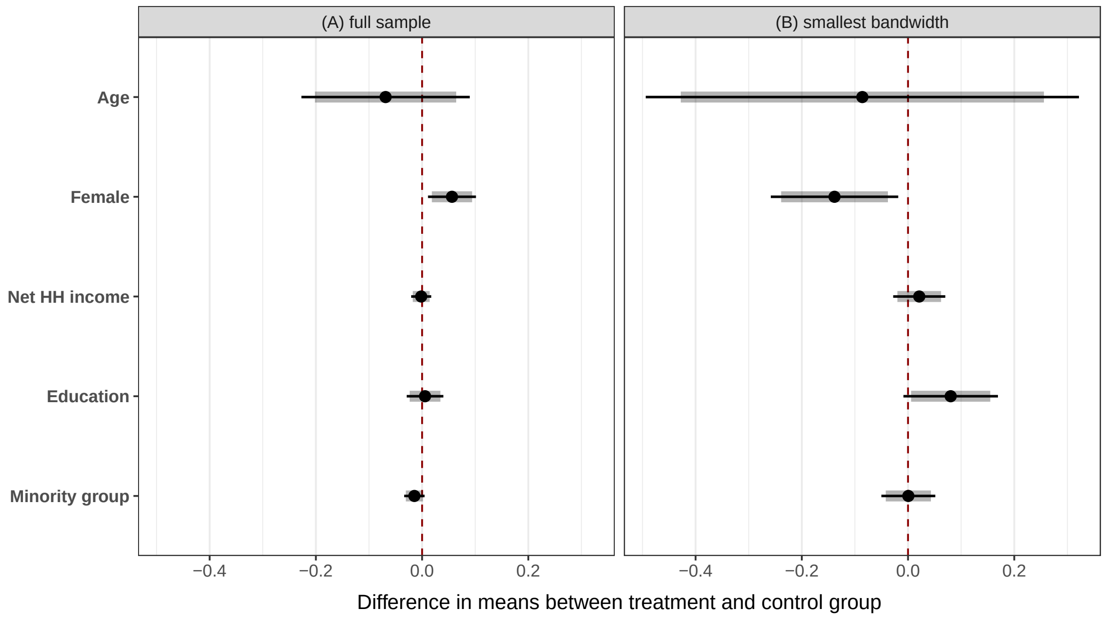

Figure 4.C.2.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 907$ and $n_{treat} = 679$. For the narrowest bandwidth of ±9 days, these are $n_{control} = 146$ and $n_{treat} = 87$.

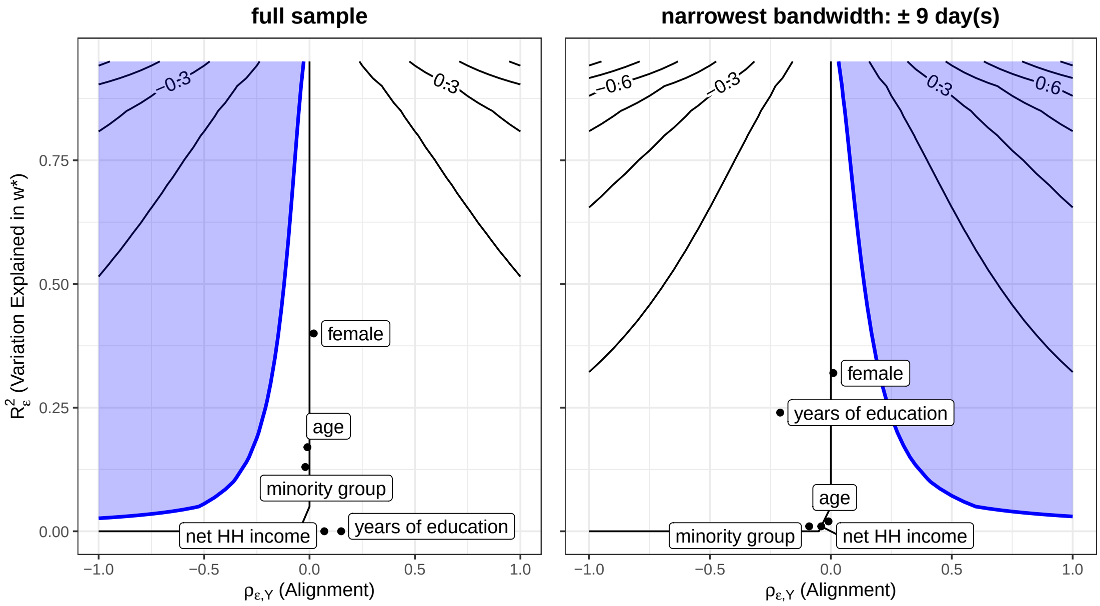

Figure 4.C.2.3: Contour plot for the Amsterdam 2004 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 3: Russia, Chechnya, December 2008 (Nägel et al., 2024)

On the night of December 3–4, 2008, armed members of the Caucasus Emirate killed three members of the Sadulayev family in Chechnya before setting their home on fire. Rebel sources claimed the attack was retaliation for the family's alleged role in exposing insurgents to Russian forces in 2006. Limited information surrounds the incident, and uncertainties about its background remain.

Table 4.C.3.1: Regression results for the Chechnya 2008 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±7 days)	(full)	(±7 days)	(full)	(±7 days)
ITT effect	0.033	−0.245*	−0.010	−0.228	−0.013	−0.186
	(0.051)	(0.143)	(0.059)	(0.142)	(0.054)	(0.142)
Age (years/10)			0.044*	−0.031		
			(0.026)	(0.064)		
Female			0.238***	0.325***		
			(0.047)	(0.116)		
Net HH income			0.062**	0.019		
			(0.026)	(0.066)		
Education (years/10)			0.019	−0.075		
			(0.026)	(0.062)		
Minority group member			−0.117*	−0.244		
			(0.068)	(0.168)		
Observations	2,302	320	1,963	320	1,963	320
R^2	0.0002	0.009	0.018	0.045	0.00002	0.009
Adjusted R^2	−0.004	−0.023	0.010	−0.002	−0.005	−0.023
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

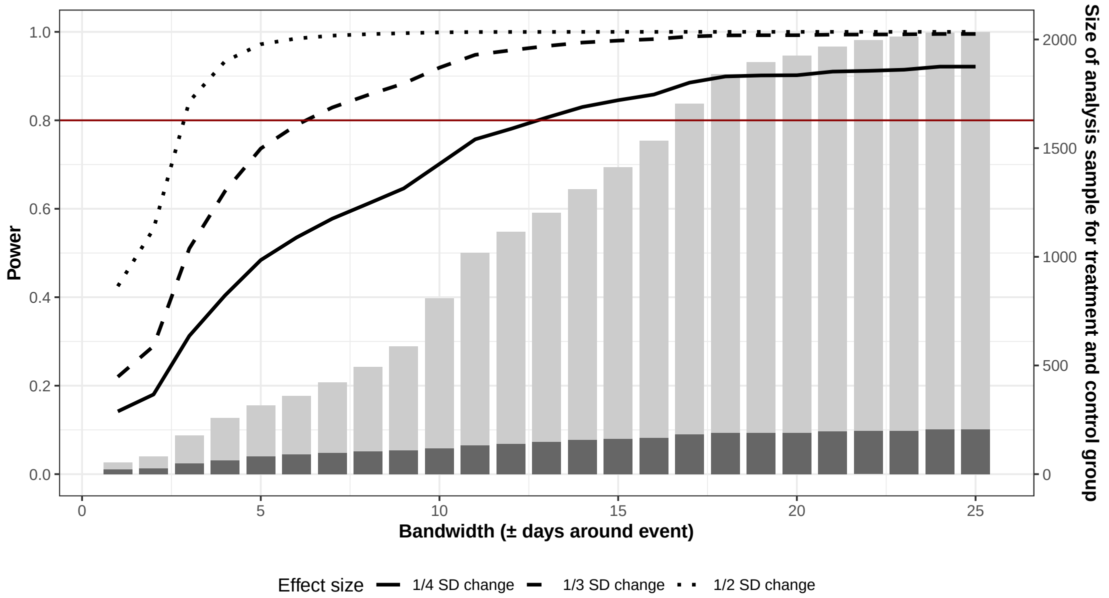

Figure 4.C.3.1: Power analysis for different bandwidths around the Chechnya 2008 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

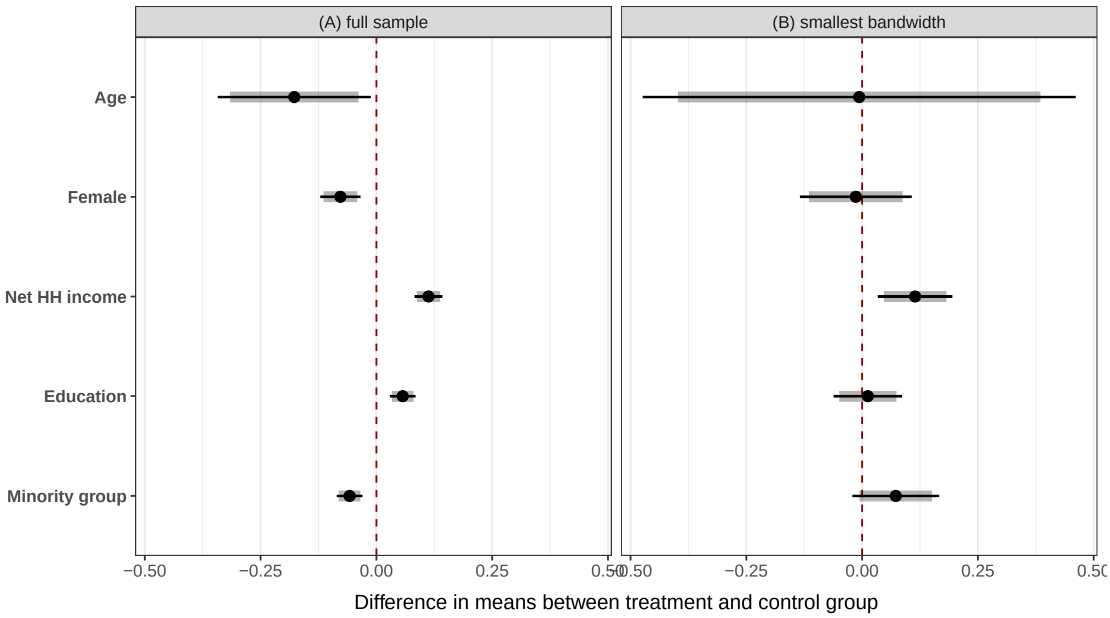

Figure 4.C.3.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 1501$ and $n_{treat} = 462$. For the narrowest bandwidth of ± 7 days, these are $n_{control} = 255$ and $n_{treat} = 65$.

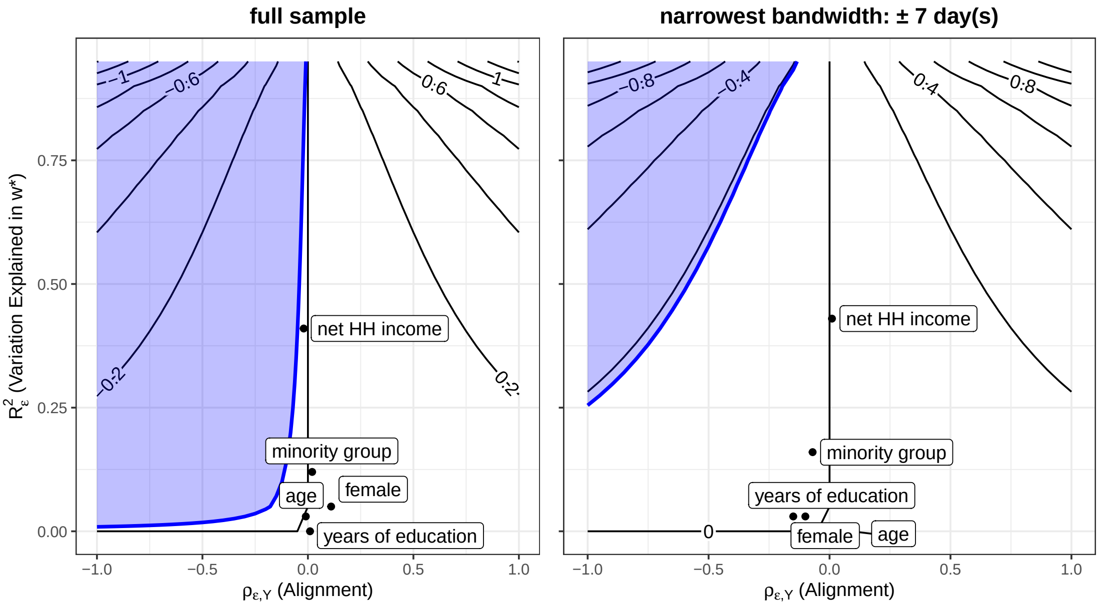

Figure 4.C.3.3: Contour plot for the Chechnya 2008 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 4: Russia, Moscow, January 2011 (Nägel et al., 2024)

On January 24, 2011, a suicide bomber detonated explosives in the international arrivals hall of Moscow's Domodedovo Airport, killing 36 people and injuring over 150. Chechen militant leader Doku Umarov later claimed responsibility, describing the attack as targeting foreign nationals. The bombing marked one of Russia's deadliest terrorist incidents since the 2010 Moscow metro attacks.

Table 4.C.4.1: Regression results for the Moscow 2011 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±6 days)	(full)	(±6 days)	(full)	(±6 days)
ITT effect	−0.049	−0.034	−0.050	−0.018	−0.036	−0.091
	(0.046)	(0.127)	(0.051)	(0.128)	(0.050)	(0.129)
Age (years/10)			0.085***	0.069		
			(0.025)	(0.075)		
Female			0.139***	0.075		
			(0.046)	(0.135)		
Net HH income			0.069***	0.024		
			(0.026)	(0.078)		
Education (years/10)			−0.077***	−0.129**		
			(0.025)	(0.065)		
Minority group member			0.124*	0.146		
			(0.065)	(0.195)		
Observations	2,397	223	1,993	223	1,993	223
R^2	0.0005	0.0003	0.021	0.041	0.001	0.0003
Adjusted R^2	−0.004	−0.047	0.013	−0.028	−0.005	−0.047
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

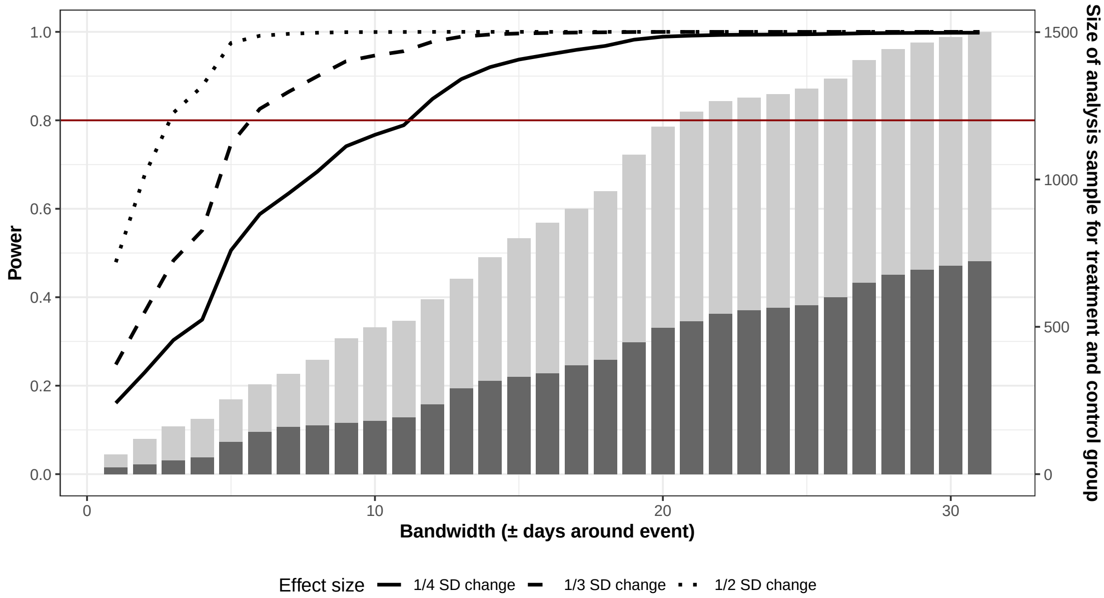

Figure 4.C.4.1: Power analysis for different bandwidths around the Moscow 2011 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

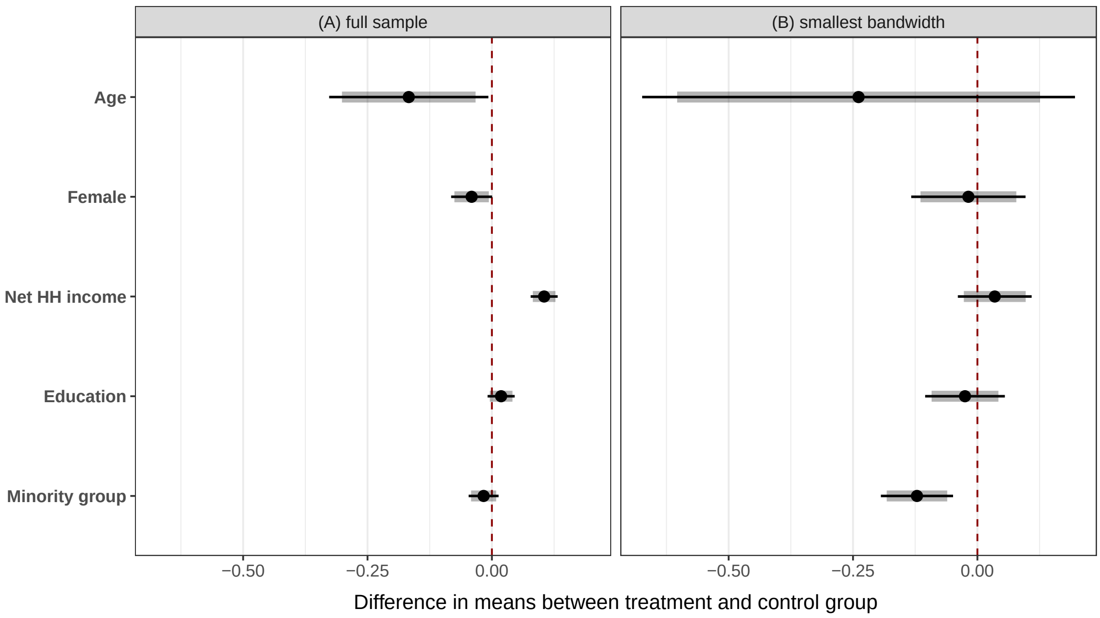

Figure 4.C.4.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 593$ and $n_{treat} = 1400$. For the narrowest bandwidth of ±6 days, these are $n_{control} = 117$ and $n_{treat} = 106$.

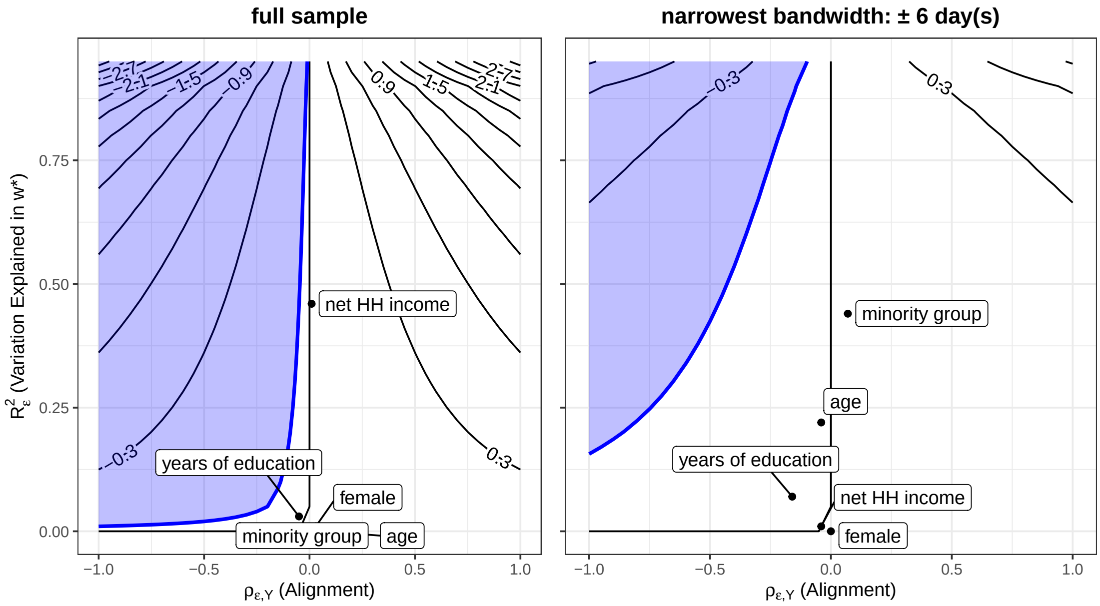

Figure 4.C.4.3: Contour plot for the Moscow 2011 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 5: Israel, Kiryat Malachi City, November 2012 (Nägel et al., 2024)

On November 15, 2012, a rocket launched from Gaza struck the Israeli city of Kiryat Malachi, killing three civilians and injuring two others. Hamas claimed responsibility, describing the attack as retaliation for Israel's earlier assassination of its military chief. The incident intensified hostilities, prompting Israeli airstrikes on over 100 targets in Gaza within 24 hours, resulting in additional civilian casualties.

Table 4.C.5.1: Regression results for the Kiryat Malachi City 2012 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±5 days)	(full)	(±5 days)	(full)	(±5 days)
ITT effect	0.110***	0.223	0.110**	0.270	0.066	0.253*
	(0.041)	(0.158)	(0.050)	(0.167)	(0.049)	(0.152)
Age (years/10)			−0.022	0.115		
			(0.026)	(0.087)		
Female			−0.031	−0.261*		
			(0.049)	(0.158)		
Net HH income			0.067**	0.026		
			(0.027)	(0.093)		
Education (years/10)			−0.016	−0.042		
			(0.025)	(0.080)		
Minority group member			−0.379***	−0.162		
			(0.071)	(0.199)		
Observations	2,411	166	1,563	166	1,563	166
R^2	0.003	0.012	0.031	0.053	0.001	0.012
Adjusted R^2	0.003	0.006	0.027	0.017	0.001	0.006
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

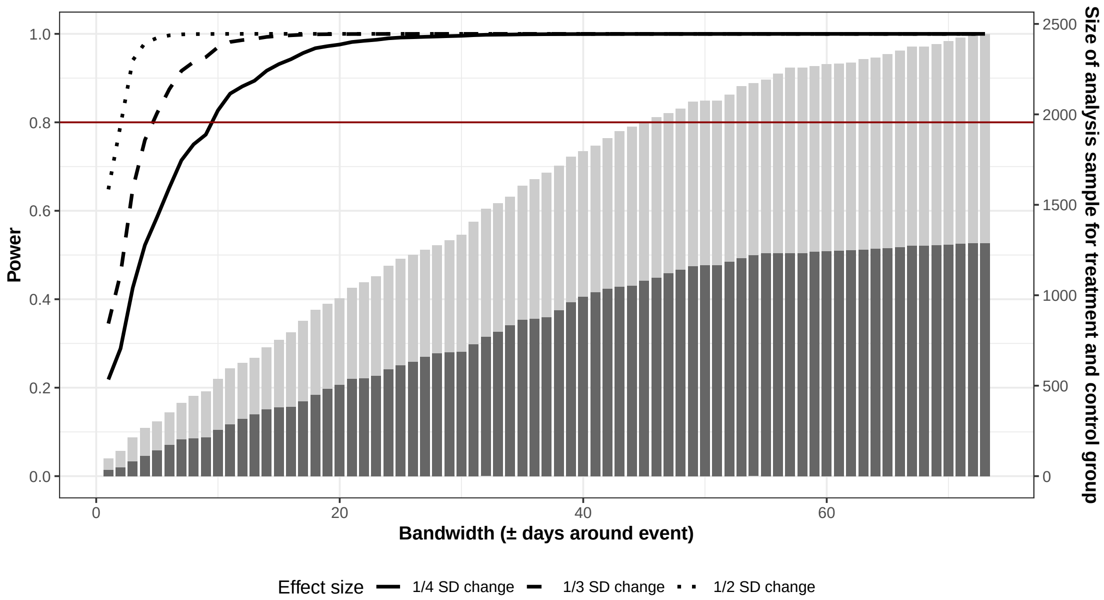

Figure 4.C.5.1: Power analysis for different bandwidths around the Kiryat Malachi City 2012 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

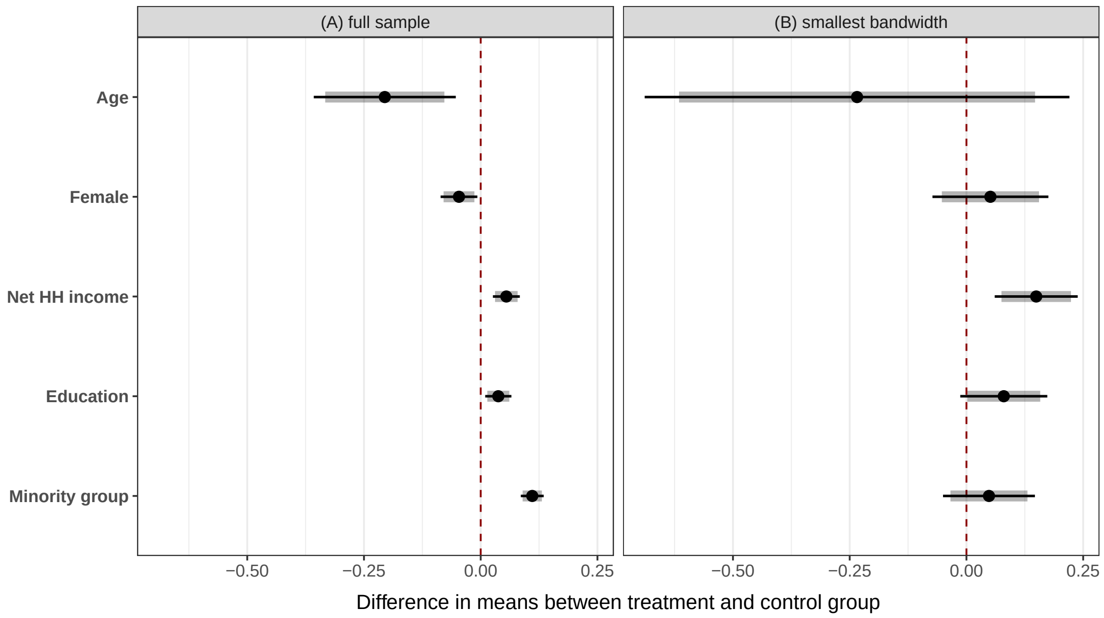

Figure 4.C.5.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 695$ and $n_{treat} = 868$. For the narrowest bandwidth of ±5 days, these are $n_{control} = 99$ and $n_{treat} = 67$.

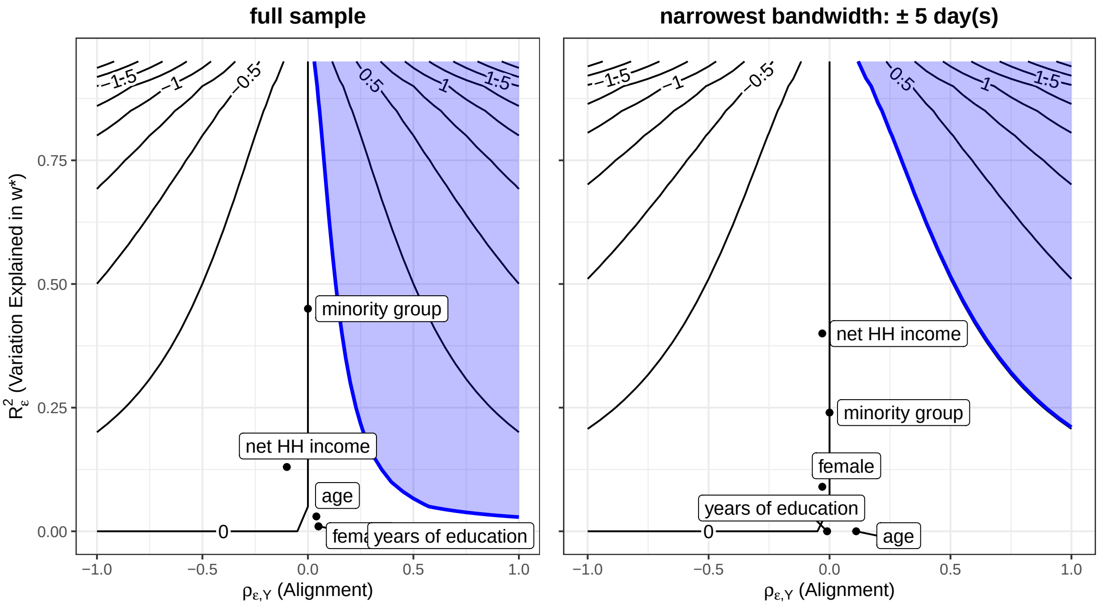

Figure 4.C.5.3: Contour plot for the Kiryat Malachi City 2012 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 6: France, Paris, January 2015 (Nägel et al., 2024)

For a description of this case, see Section 4.3.1.

Table 4.C.6.1: Regression results for the Paris 2015 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±19 days)	(full)	(±19 days)	(full)	(±19 days)
ITT effect	0.250***	0.328**	0.218***	0.298**	0.196***	0.261**
	(0.064)	(0.138)	(0.067)	(0.141)	(0.055)	(0.131)
Age (years/10)			0.079***	0.014		
			(0.026)	(0.069)		
Female			0.029	0.187		
			(0.048)	(0.130)		
Net HH income			0.019	0.050		
			(0.026)	(0.069)		
Education (years/10)			0.063**	0.047		
			(0.027)	(0.067)		
Minority group member			0.152	0.213		
			(0.112)	(0.253)		
Observations	1,889	259	1,744	259	1,744	259
R^2	0.008	0.023	0.015	0.039	0.007	0.023
Adjusted R^2	−0.003	−0.059	0.001	−0.064	−0.006	−0.059
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

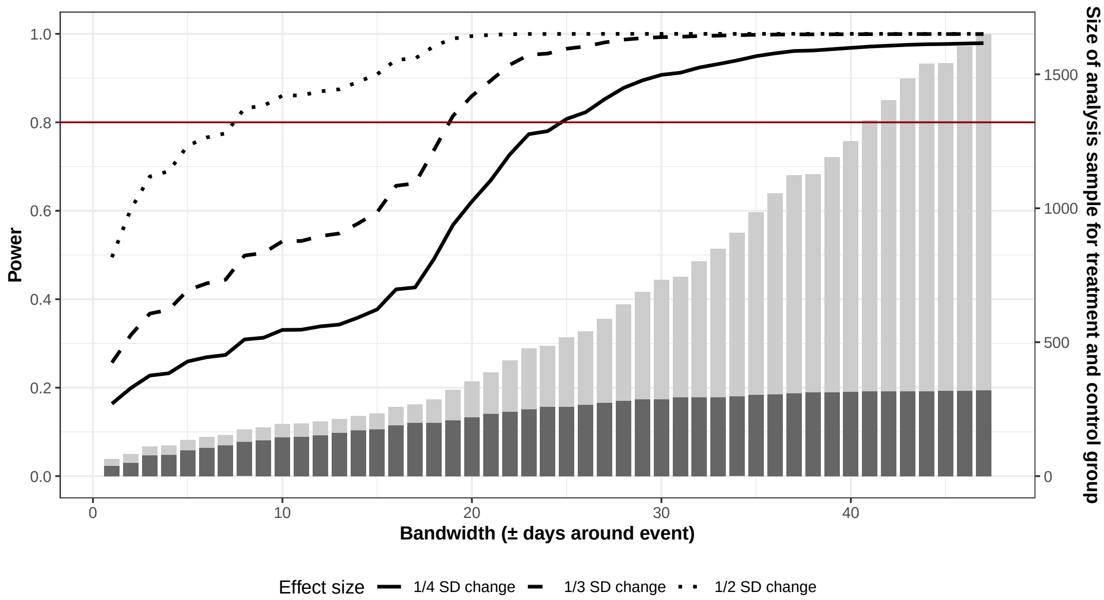

Figure 4.C.6.1: Power analysis for different bandwidths around the Paris 2015 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

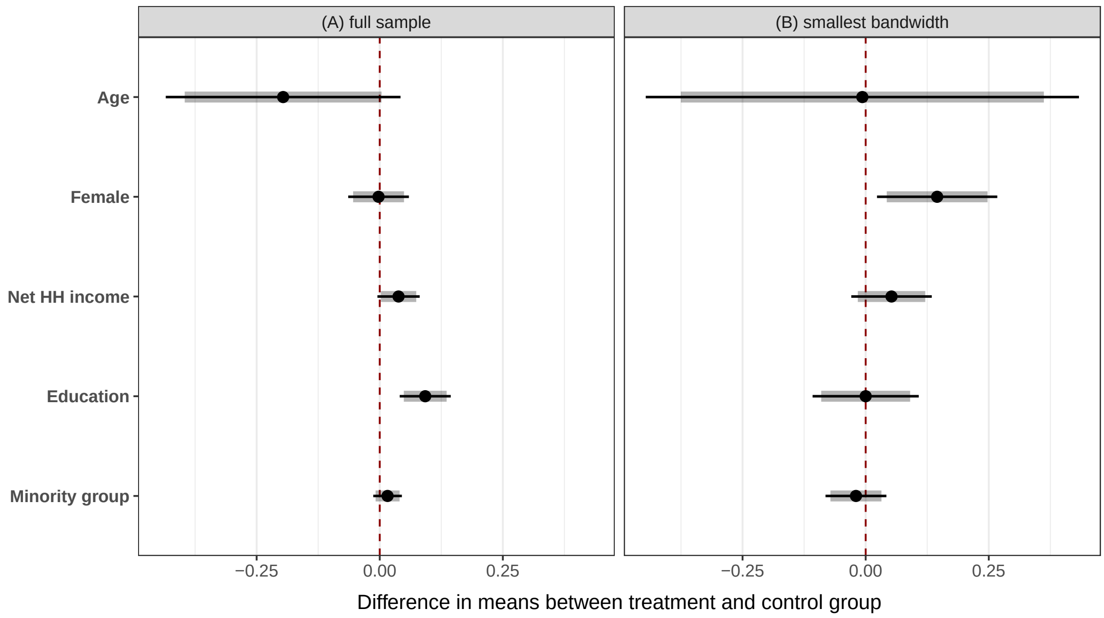

Figure 4.C.6.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 1471$ and $n_{treat} = 273$. For the narrowest bandwidth of ± 19 days, these are $n_{control} = 87$ and $n_{treat} = 172$.

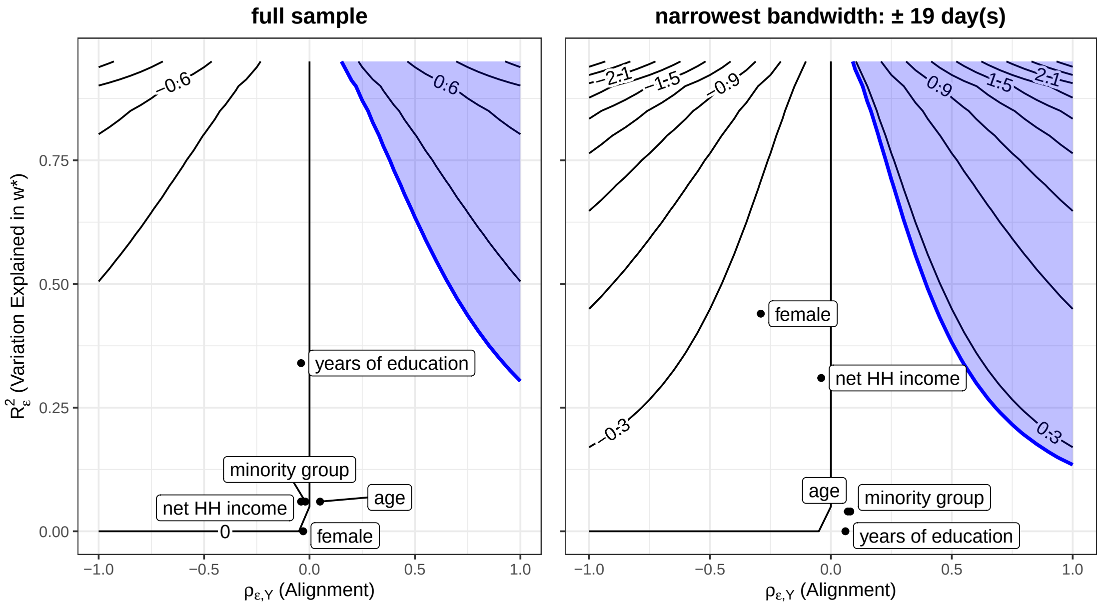

Figure 4.C.6.3: Contour plot for the Paris 2015 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 7: Germany, Berlin, December 2016 (Nägel et al., 2024)

On December 19, 2016, Tunisian national Anis Amri carried out a terrorist attack at Berlin's Christmas market by driving a stolen truck into the crowd, killing 12 people and injuring 55. Among the dead was the Polish truck driver, whom Amri had shot before the assault. The Islamic State later claimed responsibility, and Amri was killed by Italian police four days later while fleeing through Europe.

Table 4.C.7.1: Regression results for the Berlin 2016 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±10 days)	(full)	(±10 days)	(full)	(±10 days)
ITT effect	−0.049	−0.042	−0.059	−0.053	−0.071	0.054
	(0.052)	(0.154)	(0.055)	(0.155)	(0.057)	(0.156)
Age (years/10)			−0.024	−0.186**		
			(0.021)	(0.077)		
Female			0.037	0.216		
			(0.039)	(0.145)		
Net HH income			0.109***	0.004		
			(0.022)	(0.072)		
Education (years/10)			0.044**	−0.046		
			(0.021)	(0.074)		
Minority group member			0.284***	0.615*		
			(0.083)	(0.325)		
Observations	2,810	236	2,512	236	2,512	236
R^2	0.0003	0.0003	0.022	0.051	0.0003	0.0003
Adjusted R^2	−0.005	−0.073	0.013	−0.043	−0.006	−0.073
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

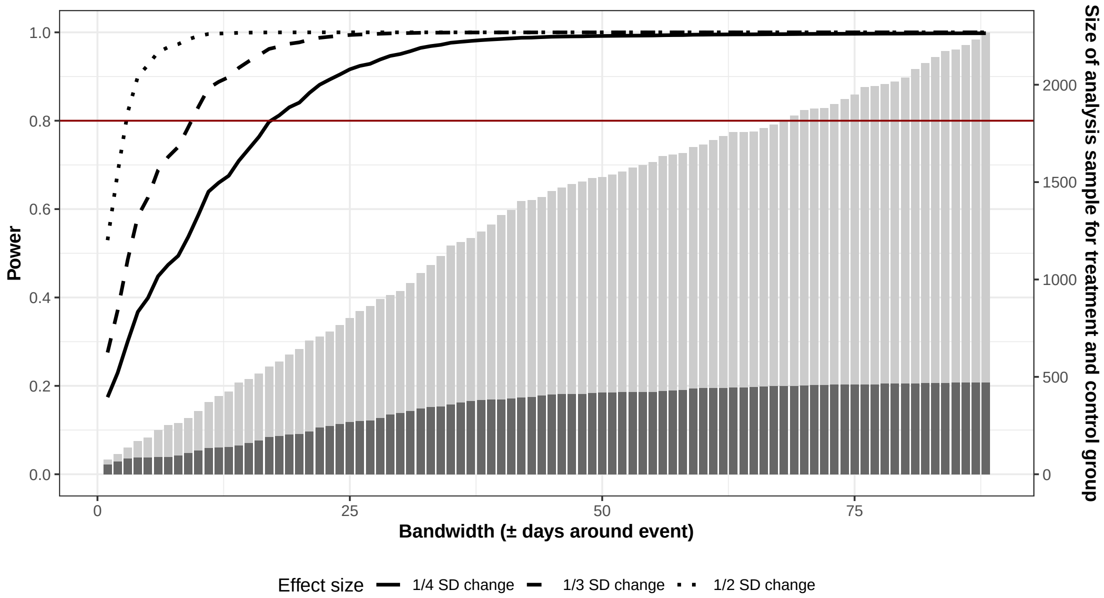

Figure 4.C.7.1: Power analysis for different bandwidths around the Berlin 2016 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

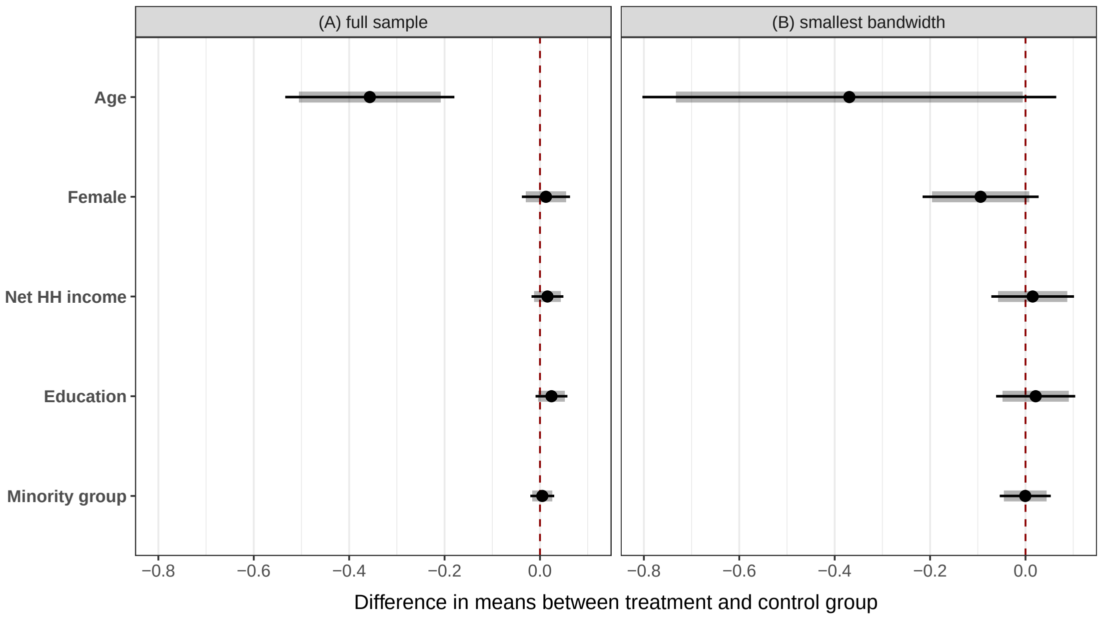

Figure 4.C.7.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 2122$ and $n_{treat} = 390$. For the narrowest bandwidth of ± 10 days, these are $n_{control} = 154$ and $n_{treat} = 82$.

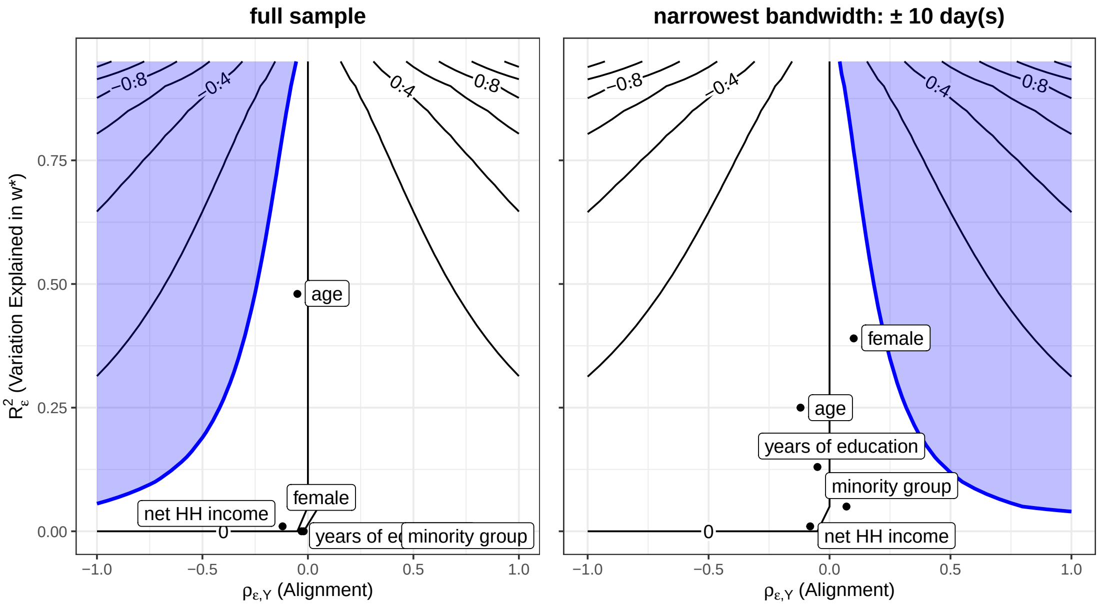

Figure 4.C.7.3: Contour plot for the Berlin 2016 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 8: France, Strasbourg, December 2018 (Nägel et al., 2024)

On December 11, 2018, Chérif Chekatt carried out a shooting near the Strasbourg Christmas market, killing five people and injuring eleven others. A French national of Moroccan descent, Chekatt had a long criminal record and claimed allegiance to the Islamic State. He was shot dead by police two days later, and investigations confirmed his Islamist extremist motives.

Table 4.C.8.1: Regression results for the Strasbourg 2018 terrorist attacks on trust in politicians

	Dependent variable:					
	Trust in politicians					
	(full)	(±8 days)	(full)	(±8 days)	(full)	(±8 days)
ITT effect	0.004	−0.171	−0.021	−0.200	−0.021	−0.104
	(0.047)	(0.147)	(0.050)	(0.148)	(0.050)	(0.148)
Age (years/10)			0.102***	0.162**		
			(0.026)	(0.077)		
Female			−0.039	−0.115		
			(0.049)	(0.142)		
Net HH income			0.002	0.058		
			(0.027)	(0.080)		
Education (years/10)			0.102***	0.086		
			(0.027)	(0.079)		
Minority group member			−0.121	−0.247		
			(0.116)	(0.264)		
Observations	1,942	237	1,704	237	1,704	237
R^2	0.00000	0.006	0.016	0.046	0.0002	0.006
Adjusted R^2	−0.011	−0.091	0.001	−0.072	−0.012	−0.091
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01

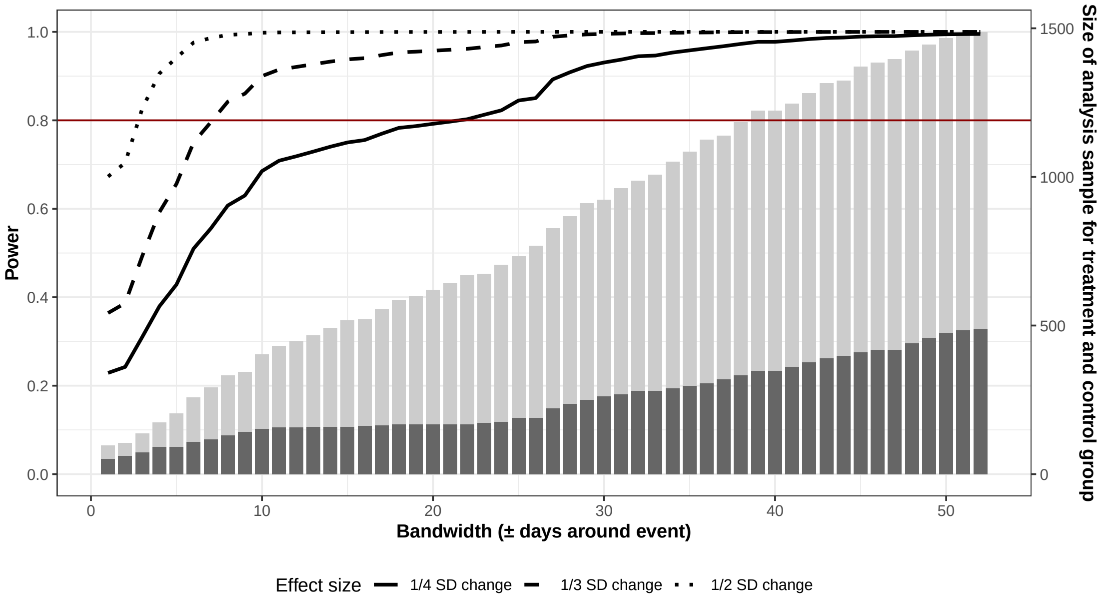

Figure 4.C.8.1: Power analysis for different bandwidths around the Strasbourg 2018 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

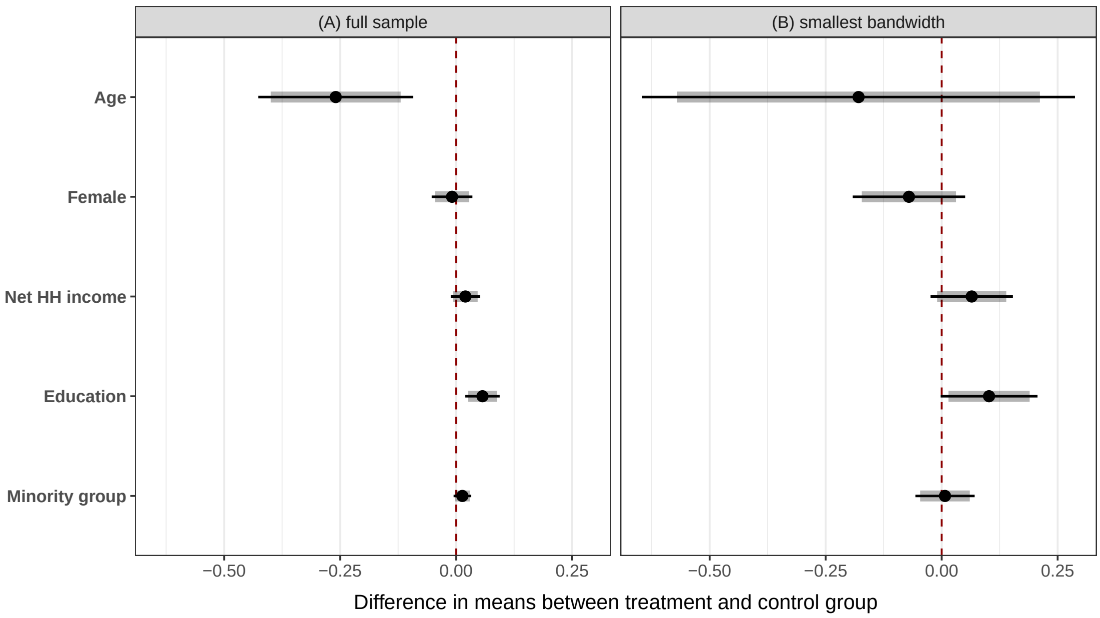

Figure 4.C.8.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 849$ and $n_{treat} = 855$. For the narrowest bandwidth of ± 8 days, these are $n_{control} = 146$ and $n_{treat} = 91$.

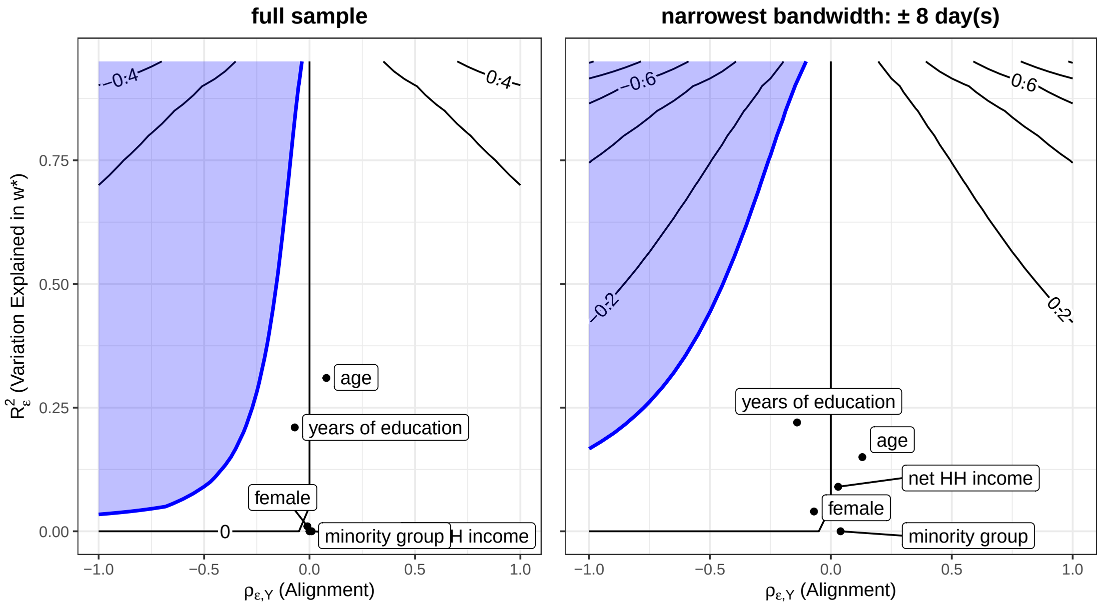

Figure 4.C.8.3: Contour plot for the Strasbourg 2016 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 9: Netherlands, Amsterdam, November 2004 (Vlandas & Halikiopoulou, 2025)

For a description of the event, see Case 2.

Table 4.C.9.1: Regression results for the Amsterdam 2004 terrorist attacks on satisfaction with government

	Dependent variable:					
	Satisfaction with government					
	(full)	(±8 days)	(full)	(±8 days)	(full)	(±8 days)
ITT effect	0.030	0.081	0.049	0.005	0.033	0.098
	(0.048)	(0.129)	(0.049)	(0.125)	(0.051)	(0.129)
Age (years/10)			0.075**	0.093		
			(0.032)	(0.080)		
Female			−0.127**	−0.017		
			(0.049)	(0.131)		
Children			−0.027	−0.090		
			(0.062)	(0.154)		
Urban vs. rural area			0.082***	−0.055		
			(0.027)	(0.071)		
Education (years/10)			0.147***	0.144**		
			(0.027)	(0.072)		
Net HH income			0.095***	0.086		
			(0.026)	(0.070)		
Unemployed			−0.313**	−0.315		
			(0.136)	(0.355)		
Religiosity			0.228***	0.287***		
			(0.025)	(0.067)		
Observations	1,835	233	1,586	233	1,586	233
R^2	0.0002	0.002	0.103	0.135	0.0003	0.002
Adjusted R^2	−0.022	−0.175	0.075	−0.062	−0.026	−0.175
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

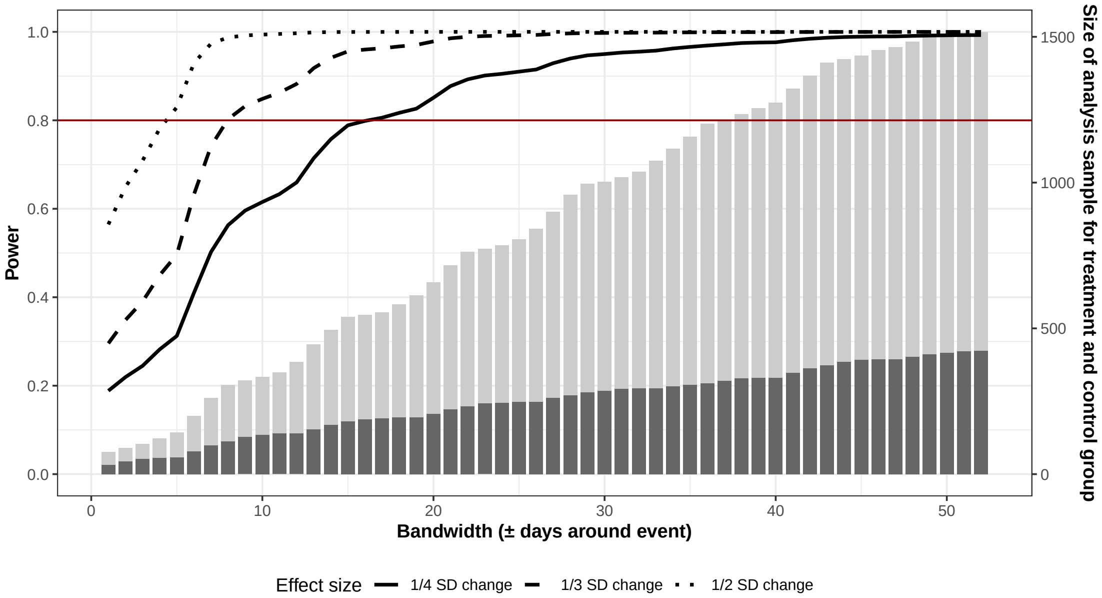

Figure 4.C.9.1: Power analysis for different bandwidths around the Amsterdam 2004 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

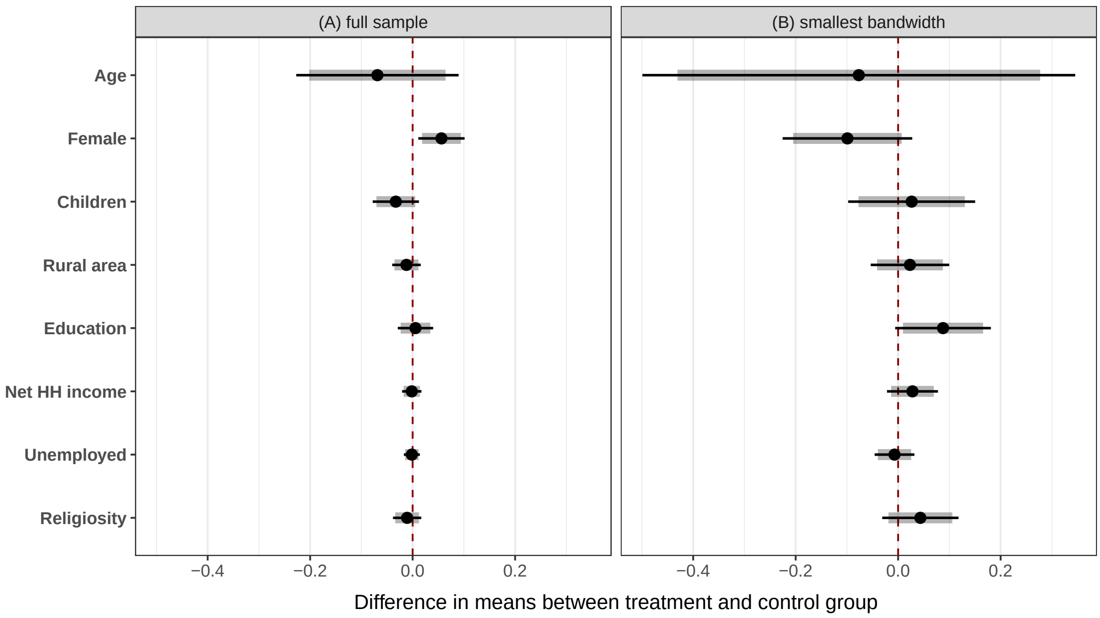

Figure 4.C.9.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 907$ and $n_{treat} = 679$. For the narrowest bandwidth of ±8 days, these are $n_{control} = 146$ and $n_{treat} = 87$.

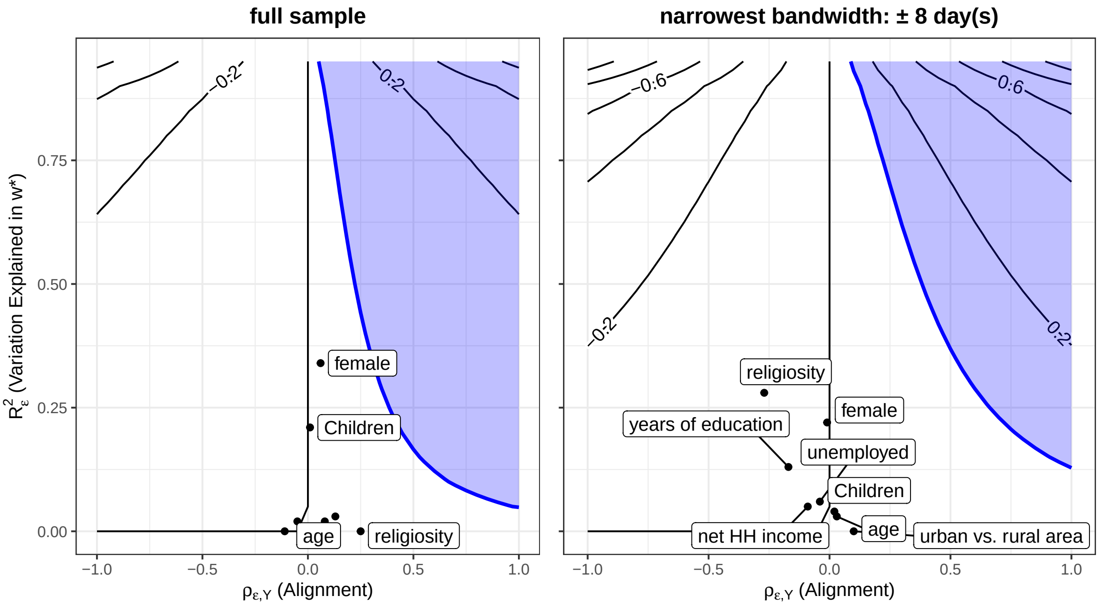

Figure 4.C.9.3: Contour plot for the Amsterdam 2004 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 10: Sweden, Stockholm, December 2010 (Vlandas & Halikiopoulou, 2025)

On December 11, 2010, a suicide bomber detonated an explosive device on Drottninggatan in central Stockholm, killing himself and injuring two civilians. The attacker, identified as Taimour Abdulwahab, was a Swedish citizen of Iraqi origin. Authorities later confirmed that he had expressed extremist motives in messages sent before the attack.

Table 4.C.10.1: Regression results for the Stockholm 2010 terrorist attacks on satisfaction with government

	Dependent variable:					
	Satisfaction with government					
	(full)	(±24 days)	(full)	(±24 days)	(full)	(±24 days)
ITT effect	−0.025	−0.066	−0.088	−0.034	−0.072	0.079
	(0.056)	(0.162)	(0.069)	(0.160)	(0.071)	(0.156)
Age (years/10)			0.160***	0.116		
			(0.048)	(0.090)		
Female			−0.167**	−0.078		
			(0.067)	(0.129)		
Children			0.169	0.027		
			(0.104)	(0.197)		
Urban vs. rural area			0.050	0.091		
			(0.037)	(0.068)		
Education (years/10)			0.094***	0.049		
			(0.035)	(0.063)		
Net HH income			0.164***	0.134**		
			(0.035)	(0.060)		
Unemployed			−0.386*	−0.373		
			(0.212)	(0.461)		
Religiosity			0.145***	0.141**		
			(0.033)	(0.060)		
Observations	1,445	288	921	288	921	288
R^2	0.0001	0.001	0.084	0.075	0.002	0.001
Adjusted R^2	−0.015	−0.078	0.054	−0.029	−0.021	−0.078
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

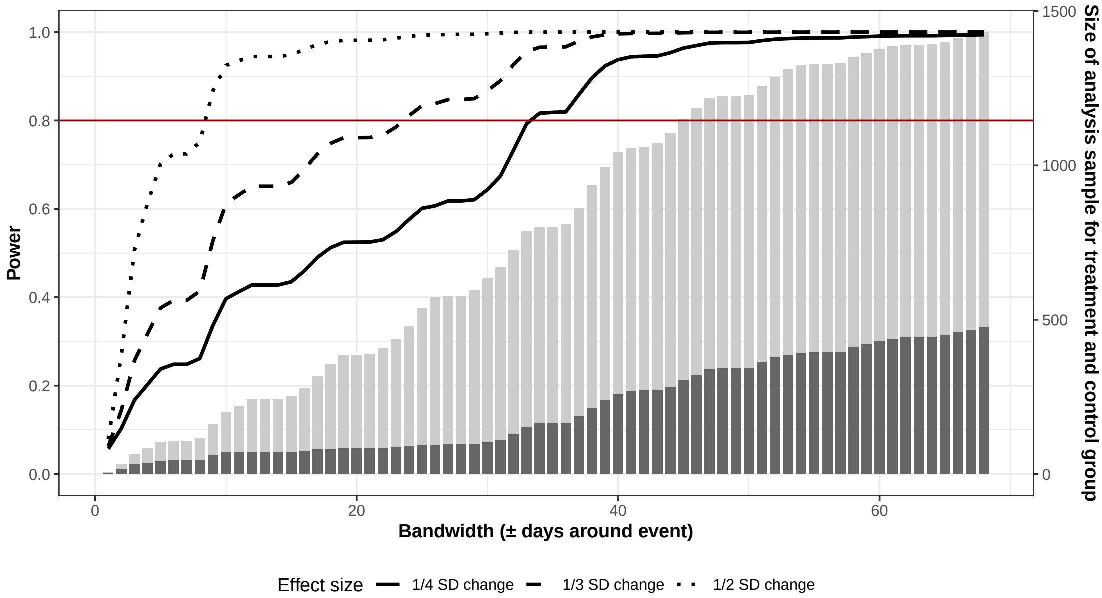

Figure 4.C.10.1: Power analysis for different bandwidths around the Stockholm 2010 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

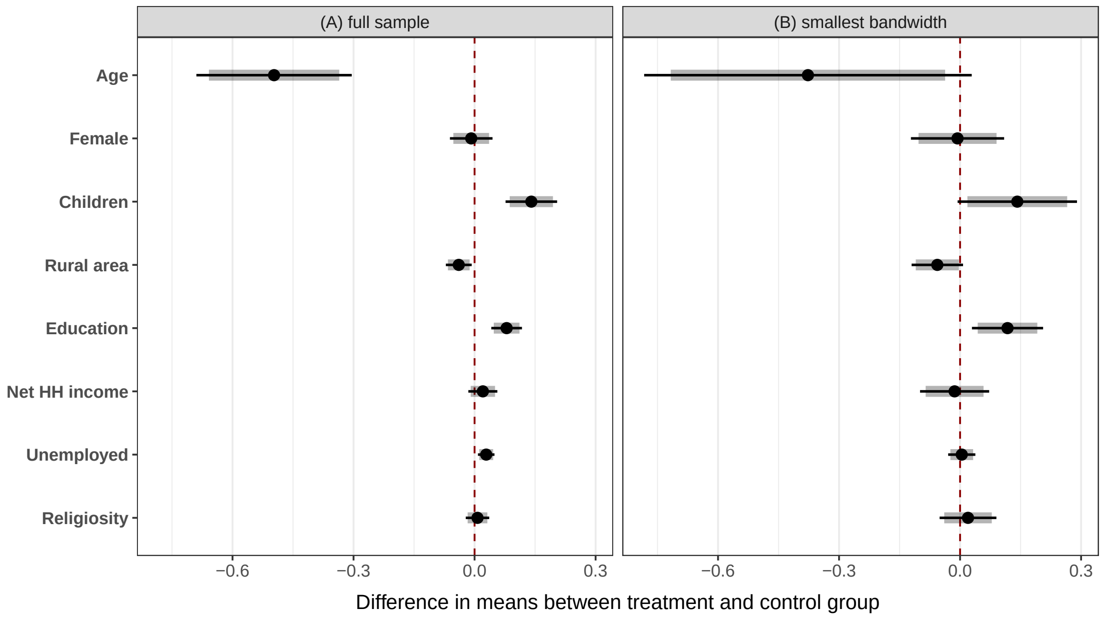

Figure 4.C.10.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 608$ and $n_{treat} = 313$. For the narrowest bandwidth of ± 10 days, these are $n_{control} = 238$ and $n_{treat} = 50$.

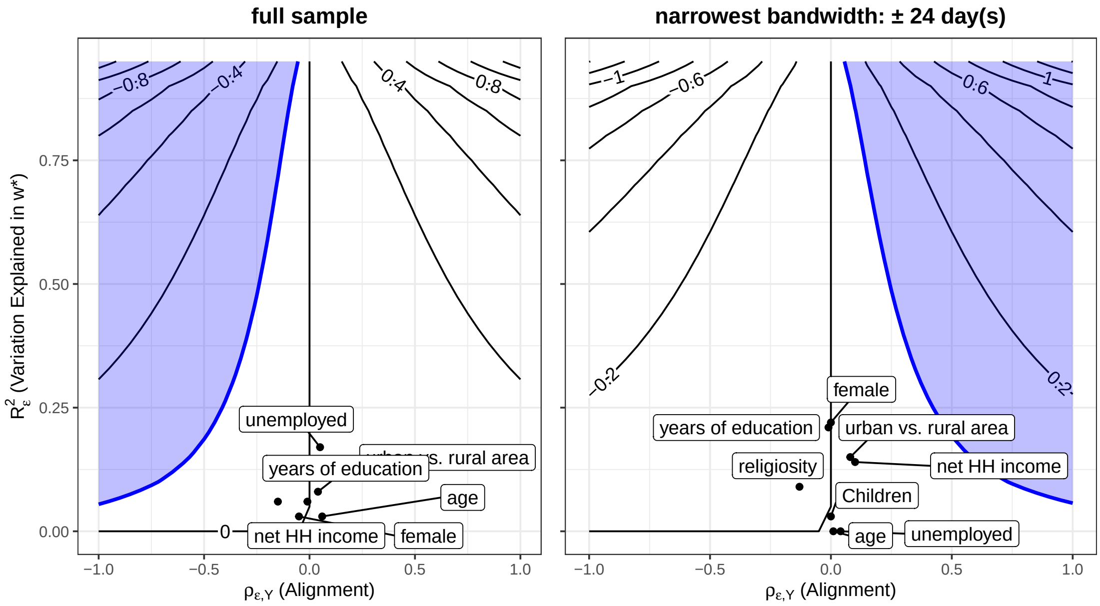

Figure 4.C.10.3: Contour plot for the Stockholm 2010 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 11: France, Paris, January 2015 (Vlandas & Halikiopoulou, 2025)

For a description of the event, see Case 6.

Table 4.C.11.1: Regression results for the Paris 2015 terrorist attacks on satisfaction with government

	Dependent variable:					
	Satisfaction with government					
	(full)	(±19 days)	(full)	(±19 days)	(full)	(±19 days)
ITT effect	0.434***	0.634***	0.451***	0.761***	0.543***	0.909***
	(0.064)	(0.199)	(0.086)	(0.211)	(0.070)	(0.183)
Age (years/10)			0.022	−0.207		
			(0.039)	(0.126)		
Female			0.012	−0.066		
			(0.061)	(0.191)		
Children			0.111	−0.146		
			(0.082)	(0.261)		
Urban vs. rural area			−0.055	−0.097		
			(0.034)	(0.125)		
Education (years/10)			0.018	−0.059		
			(0.033)	(0.096)		
Net HH income			0.012	0.024		
			(0.034)	(0.100)		
Unemployed			−0.067	−0.523		
			(0.148)	(0.384)		
Religiosity			−0.048	−0.070		
			(0.031)	(0.091)		
Observations	1,873	159	1,135	159	1,135	159
R^2	0.024	0.068	0.035	0.108	0.028	0.068
Adjusted R^2	0.013	−0.052	0.010	−0.068	0.009	−0.052
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓

Note: *p<0.1; **p<0.05; ***p<0.01

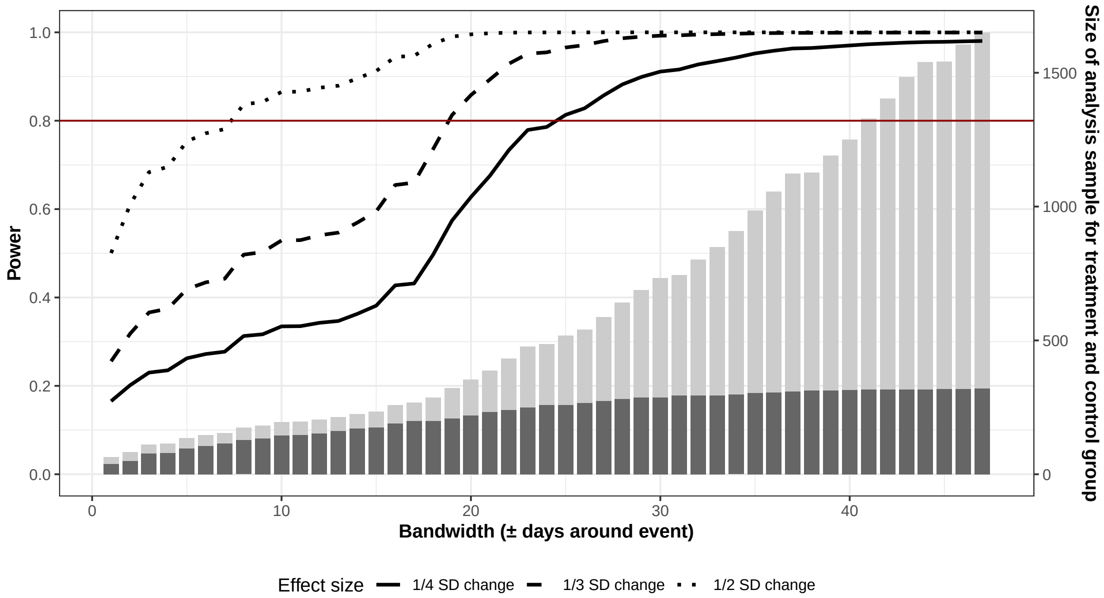

Figure 4.C.11.1: Power analysis for different bandwidths around the Paris 2015 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

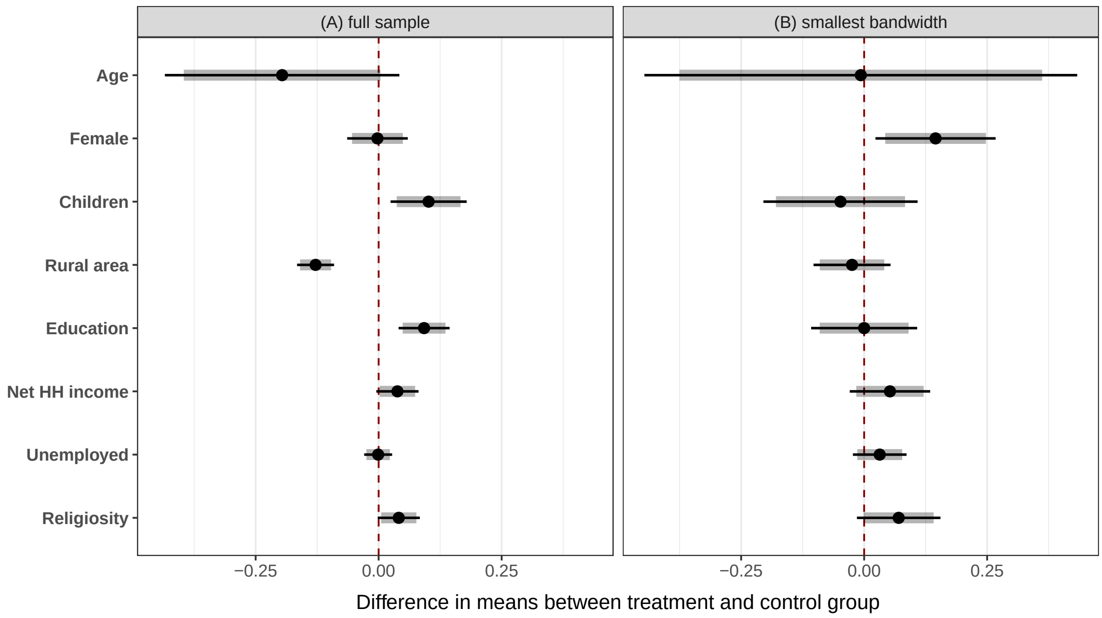

Figure 4.C.11.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 963$ and $n_{treat} = 172$. For the narrowest bandwidth of ± 19 days, these are $n_{control} = 53$ and $n_{treat} = 106$.

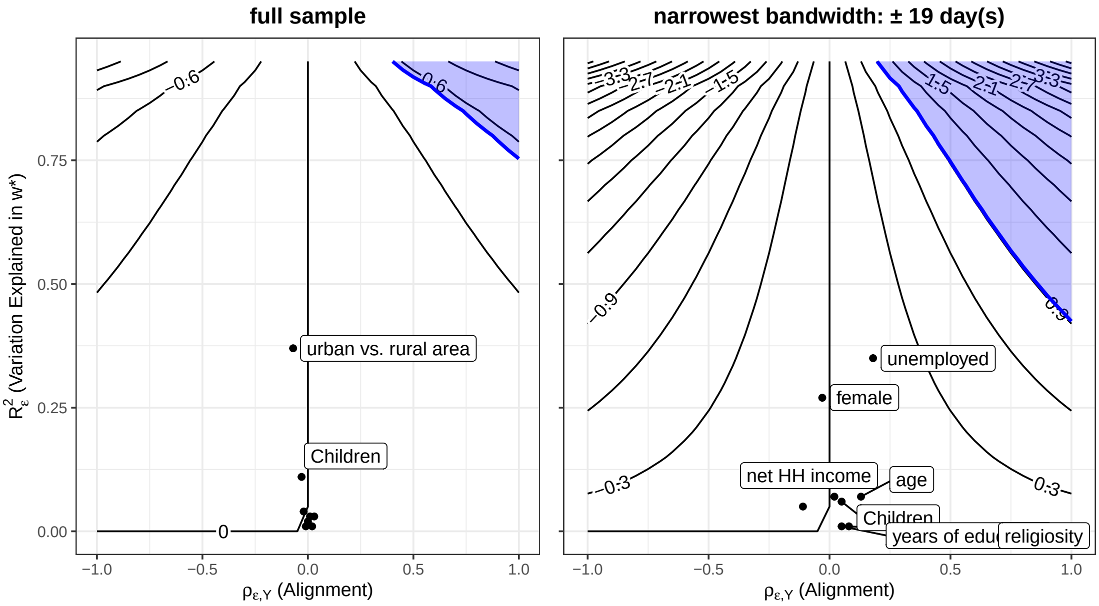

Figure 4.C.11.3: Contour plot for the Paris 2015 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 12: Germany, Berlin, December 2016 (Vlandas & Halikiopoulou, 2025)

For a description of the event, see Case 7.

Table 4.C.12.1: Regression results for the Berlin 2016 terrorist attacks on satisfaction with government

	Dependent variable:					
	Satisfaction with government					
	(full)	(±10 days)	(full)	(±10 days)	(full)	(±10 days)
ITT effect	−0.011	0.129	−0.040	0.040	0.006	0.098
	(0.052)	(0.189)	(0.068)	(0.193)	(0.072)	(0.184)
Age (years/10)			-0.055^{**}	−0.020		
			(0.028)	(0.119)		
Female			0.031	0.204		
			(0.047)	(0.178)		
Children			0.096	0.688^{***}		
			(0.060)	(0.258)		
Urban vs. rural area			-0.050^{*}	−0.050		
			(0.027)	(0.117)		
Education (years/10)			0.010	−0.045		
			(0.025)	(0.092)		
Net HH income			0.113^{***}	−0.044		
			(0.026)	(0.087)		
Unemployed			-0.382^{***}	−0.300		
			(0.145)	(0.414)		
Religiosity			0.206^{***}	0.131		
			(0.025)	(0.097)		
Observations	2,771	144	1,696	144	1,696	144
R^2	0.00001	0.004	0.064	0.130	0.00002	0.004
Adjusted R^2	−0.006	−0.122	0.050	−0.045	−0.010	−0.122
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:					*p<0.1; **p<0.05; ***p<0.01	

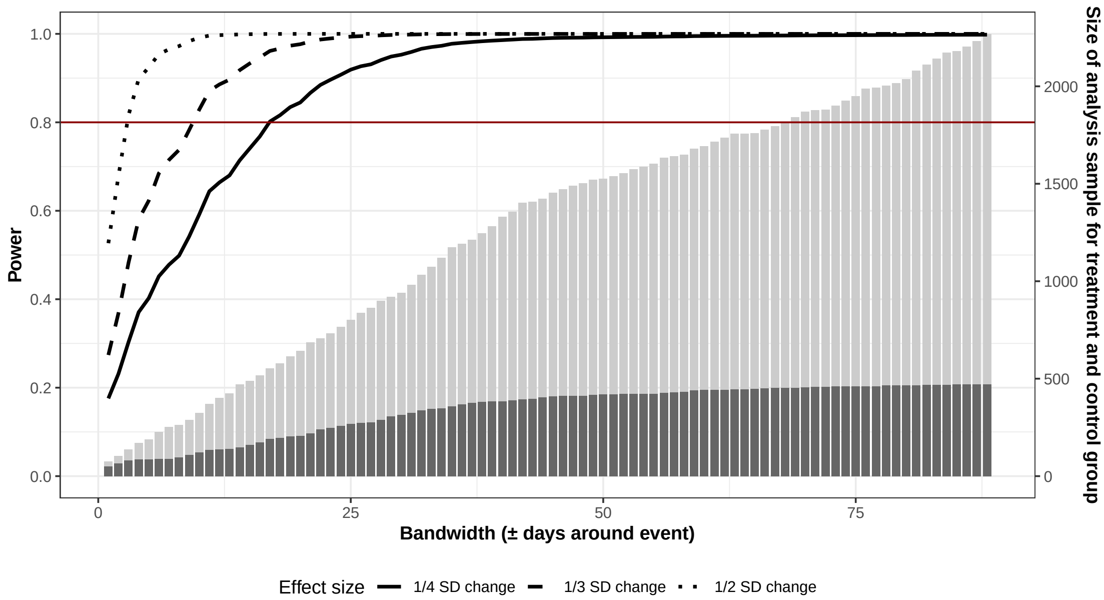

Figure 4.C.12.1: Power analysis for different bandwidths around the Berlin 2016 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

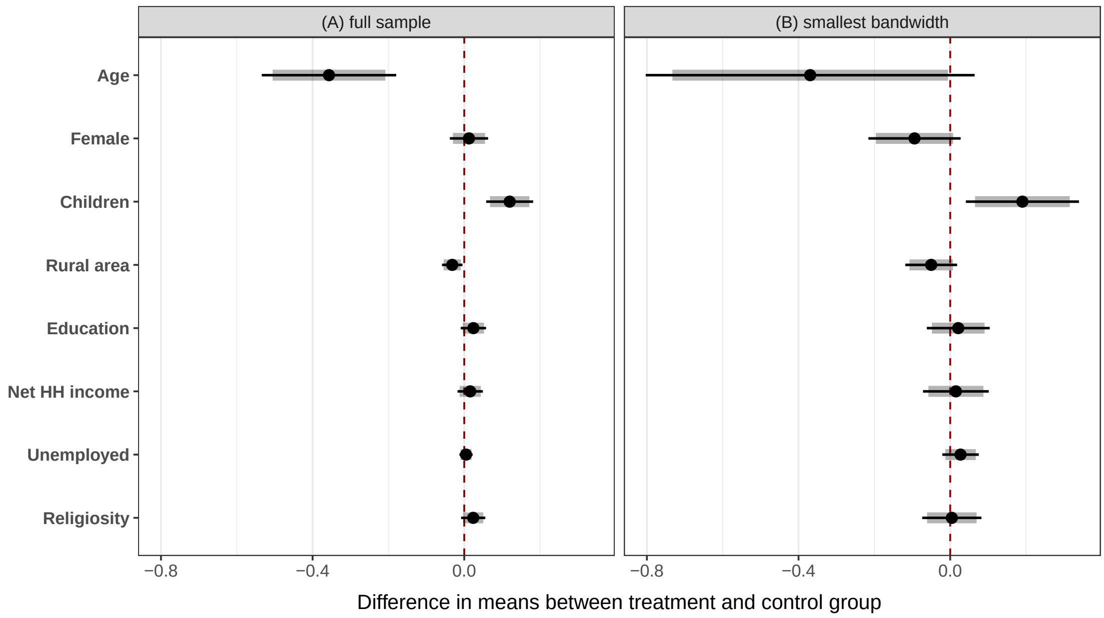

Figure 4.C.12.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. *Age* and *education* are years divided by 10. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 1457$ and $n_{treat} = 239$. For the narrowest bandwidth of ± 10 days, these are $n_{control} = 93$ and $n_{treat} = 51$.

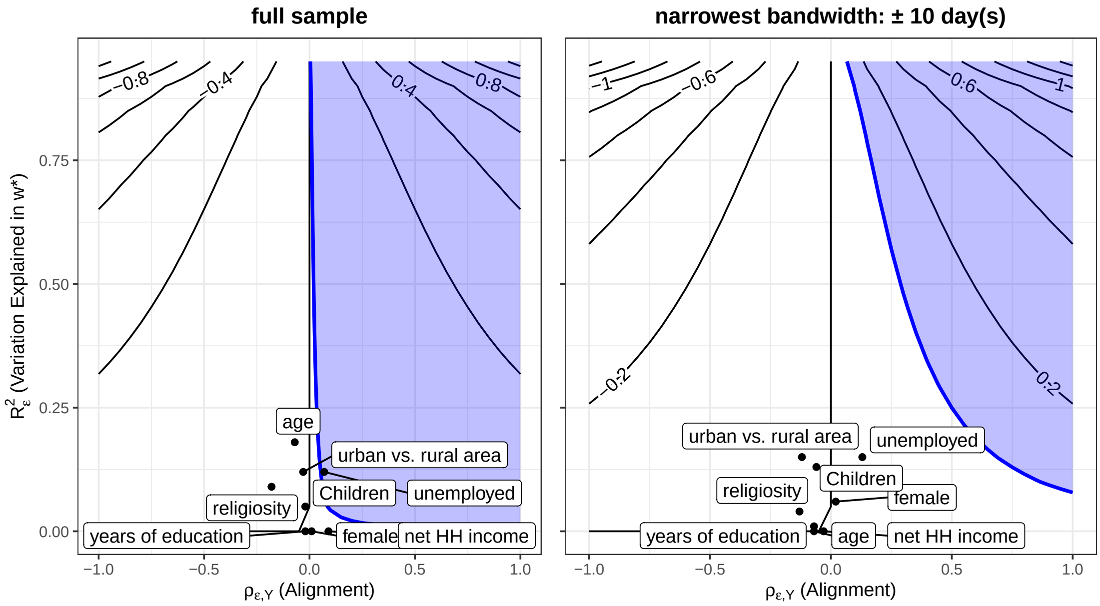

Figure 4.C.12.3: Contour plot for the Berlin 2016 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 13: UK, Manchester, May 2017 (Holman et al., 2022)

On May 22, 2017, a suicide bomber detonated an explosive device at the Manchester Arena during an Ariana Grande concert, killing 22 victims and injuring hundreds. The attacker also died in the explosion. The incident was one of the deadliest terrorist attacks in the United Kingdom in recent decades.

Table 4.C.13.1: Regression results for the Manchester 2017 terrorist attacks on approval ratings of prime minister Theresa May

	Dependent variable:					
	Approval rating of Theresa May					
	(full)	(±1 day)	(full)	(±1 day)	(full)	(±1 day)
ITT effect	−0.127***	−0.106**	−0.107***	−0.093***	−0.094***	−0.050
	(0.011)	(0.047)	(0.009)	(0.036)	(0.012)	(0.049)
Labour party ID			−0.859***	−0.823***		
			(0.015)	(0.057)		
Ethnically British			0.123***	0.063		
			(0.014)	(0.058)		
Gender			0.128***	0.148***		
			(0.009)	(0.036)		
Other party ID			−0.692***	−0.733***		
			(0.012)	(0.048)		
Income			−0.012***	−0.024		
			(0.005)	(0.019)		
Ideology			0.393***	0.395***		
			(0.006)	(0.023)		
Constant	0.068***	0.074**	0.425***	0.459***	0.044***	0.042
	(0.008)	(0.033)	(0.018)	(0.072)	(0.009)	(0.035)
Observations	31,646	1,587	25,658	1,587	25,658	1,587
R^2	0.004	0.003	0.441	0.442	0.002	0.001
Adjusted R^2	0.004	0.003	0.441	0.439	0.002	0.00003
Entropy-balanced					✓	✓
Note:					*p<0.1; **p<0.05; ***p<0.01	

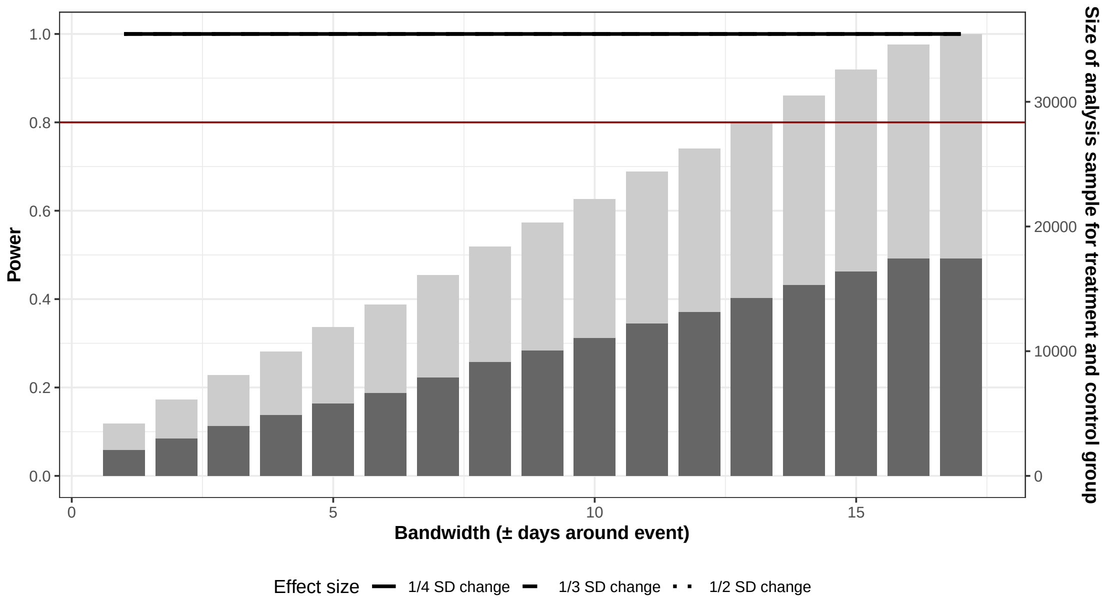

Figure 4.C.13.1: Power analysis for different bandwidths around the Manchester 2017 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

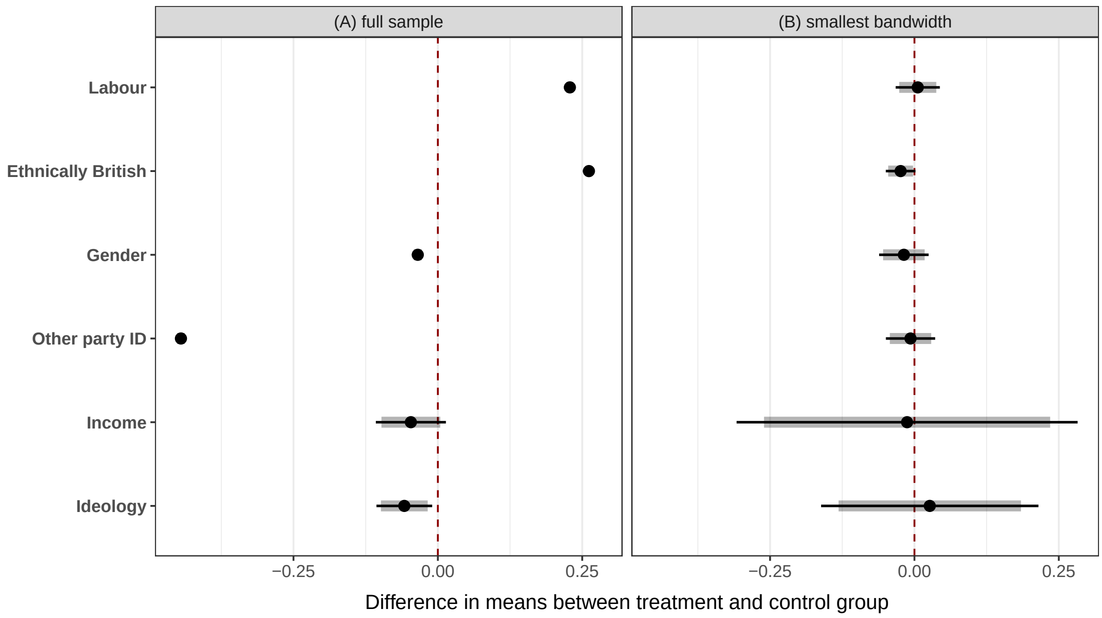

Figure 4.C.13.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 12,984$ and $n_{treat} = 12,674$. For the narrowest bandwidth of ± 1 day, these are $n_{control} = 826$ and $n_{treat} = 761$.

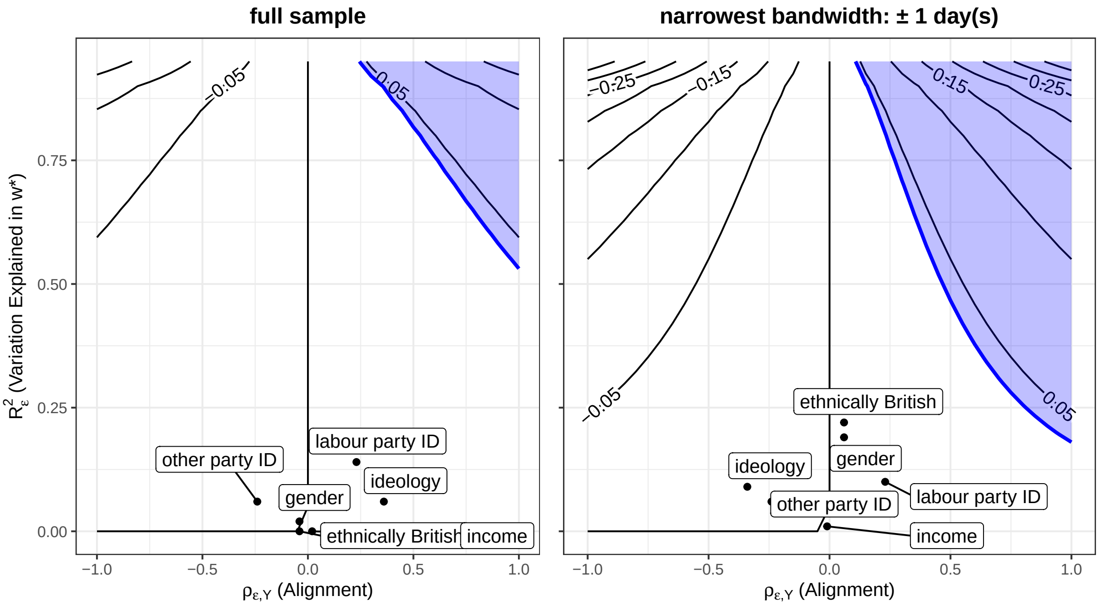

Figure 4.C.13.3: Contour plot for the Manchester 2017 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.

Case 14: Nigeria, December 2014 (Harding & Nwokolo, 2024)

On December 10–11, 2014, Boko Haram carried out a series of coordinated attacks across Nigeria, killing more than 50 people and injuring many others. The assaults included suicide bombings in Kano and Jos, an armed raid in Gajiganna.

Table 4.C.14.1: Regression results for Nigeria 2014 terrorist attacks on trust in politics

	Dependent variable:					
	Trust in politics					
	(full)	(±1 day)	(full)	(±1 day)	(full)	(±1 day)
ITT effect	0.261***	0.415***	0.228***	0.279**	0.193***	0.403***
	(0.081)	(0.107)	(0.081)	(0.129)	(0.074)	(0.111)
Age 16 to 25			−0.309	−0.045		
			(0.269)	(0.531)		
Age 26 to 35			−0.287	0.118		
			(0.267)	(0.529)		
Age 36 to 45			−0.206	0.132		
			(0.270)	(0.533)		
Age 45 to 55			−0.232	0.151		
			(0.278)	(0.549)		
Age 56 to 65			−0.452	−0.436		
			(0.293)	(0.569)		
No formal education			−0.202*	0.017		
			(0.110)	(0.164)		
Primary education			0.026	0.089		
			(0.084)	(0.129)		
Secondary education			0.028	0.047		
			(0.060)	(0.096)		
Female			0.060	0.023		
			(0.050)	(0.076)		
Muslim			0.007	0.212		
			(0.074)	(0.141)		
Urban			−0.214***	−0.286*		
			(0.062)	(0.156)		
Observations	1,467	592	1,466	592	1,466	592
R^2	0.007	0.026	0.023	0.054	0.007	0.026
Adjusted R^2	−0.016	−0.030	−0.008	−0.021	−0.016	−0.030
Region-level fixed-effects	✓	✓	✓	✓	✓	✓
Entropy-balanced					✓	✓
Note:						*p<0.1; **p<0.05; ***p<0.01

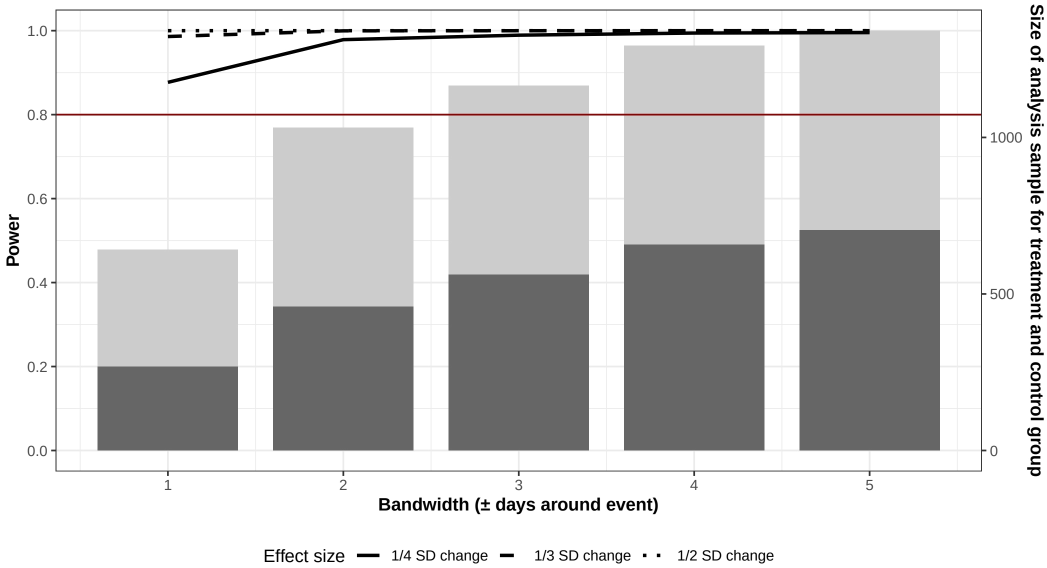

Figure 4.C.14.1: Power analysis for different bandwidths around the Nigeria 2014 terrorist attacks. Different lines indicate the estimated power for observing change between 1/4 to 1/2 of the standard deviation of the outcome variable in response to the event. Dark bars = treatment group, lighter bars = control group.

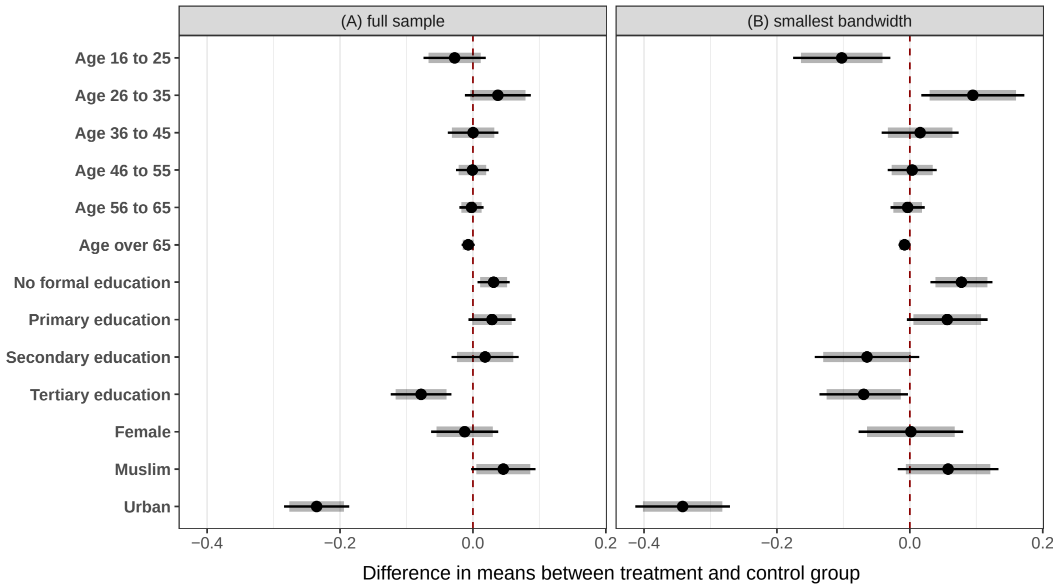

Figure 4.C.14.2: Difference in means between pre- and post attack samples and difference bandwidth of days around this treatment. Bold shaded lines represent the 90% confidence intervals, thin lines represent the 95% confidence intervals. The sample sizes for the full sample are $n_{control} = 577$ and $n_{treat} = 889$. For the narrowest bandwidth of ± 1 day, these are $n_{control} = 333$ and $n_{treat} = 259$.

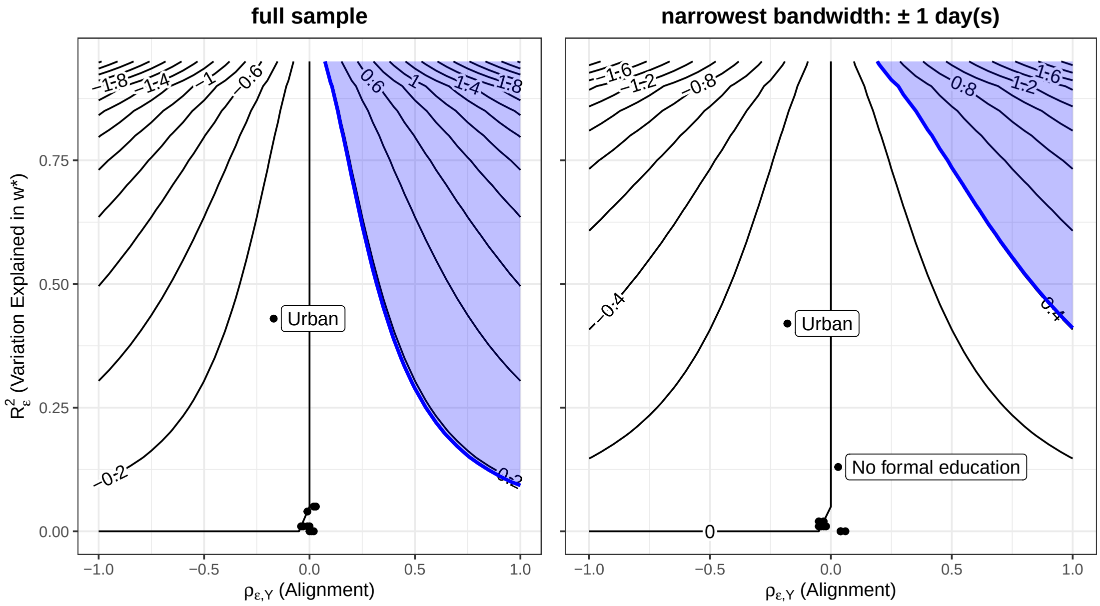

Figure 4.C.14.3: Contour plot for the Nigeria 2014 case for the full sample and the narrowest bandwidth. Blue shaded area represents the "killer confounder" area, i.e. the proportion of variation in the weights and outcome variable that would need to be explained by an unobserved confounder to drive the ITT estimate to zero.