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RESEARCH NOTE

Partisan responsiveness in real time: causal evidence from Trump's military attack on Iran

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Abstract
Elite cues signal citizens how to interpret political events through a partisan lens. Do these effects occur in real time? We exploit President Trump's unexpected announcement of US military strikes against Iran during a nationally representative survey to estimate the causal effect of elite cues on public opinion. Leveraging minute-level variation in exposure, we find that partisan divergence emerges almost immediately: support for military intervention increases among Republicans but declines among Democrats. These large effects remain robust across 28,672 model specifications and 57,344 partisan-specific treatment effects. Our quasi-experimental evidence demonstrates that elite cues can polarize public opinion within minutes, even before media framing and partisan commentary have diffused, highlighting the speed of partisan opinion formation and its implications for democratic accountability.

Keywords: elite cues; Iran; partisan reasoning; partisanship; polarization; quasi-experiment

> *"We have completed our very successful attack on the three Nuclear sites in Iran, including Fordow, Natanz, and Esfahan. All planes are now outside of Iran air space. A full payload of BOMBS was dropped on the primary site, Fordow. All planes are safely on their way home. Congratulations to our great American Warriors. There is not another military in the World that could have done this. NOW IS THE TIME FOR PEACE! Thank you for your attention to this matter."*
>
> Donald J. Trump
>
> *(Truth Social, 7.46 pm on June 21, 2025. The first public announcement of the US intervention in Iran.)*

Partisanship shapes how people understand the world. Social identities generally help humans process information effectively (Kunda, 1990), and in politics, partisanship is a key identity guiding this process (Campbell *et al.*, 1980; Huddy, 2001; Lodge and Taber, 2013; Mason, 2018). Partisanship shapes how people *genuinely* perceive the economy (Bartels, 2002; Graham, 2025), moderates their interpretation of anti-democratic acts (Graham and Svolik, 2020), influences who they are willing to date (Huber and Malhotra, 2017; Turnbull-Dugarte and López Ortega, 2025), socialize with (Engelhardt and Utych, 2020), hire (Gift and Gift, 2015), and biases the inferences partisans make

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about others' politics (Turnbull-Dugarte and Wagner, 2026). Partisan cues, statements, or acts that connect information to partisanship often trigger such reasoning (Zaller, 1992; Druckman *et al.*, 2018; Groenendyk and Krupnikov, 2021). By telling citizens how to interpret an issue through a partisan lens, a simple statement by a party or politician can suffice to shape people's attitudes (Bolsen *et al.*, 2014; Bisgaard and Slothuus, 2018; Barber and Pope, 2019; Slothuus and Bisgaard, 2021).

However, no prior study has provided causally identified evidence of partisan cue effects in a naturalistic setting with fine-grained temporal resolution. While survey experiments routinely measure cue effects within minutes of delivery (Bolsen *et al.*, 2014; Broockman and Butler, 2017; Barber and Pope, 2019; Graham, 2025), the external validity of these designs is not without limitations: stimuli are artificial, information environments are controlled, and exposure to research-administered stimuli is essentially guaranteed. The rare quasi-experimental designs that maximize external validity, like the pioneering work by Bisgaard and Slothuus (Bisgaard and Slothuus, 2018; Slothuus and Bisgaard, 2021), for example, demonstrate cue effects in externally valid settings, but their panel designs measure shifts in public opinion after weeks or months—time in which in- and out-group rhetorical nudges have proliferated via the media and social networks and have been internalized by group members, making it impossible to isolate the initial cue from its downstream amplification. Crucially, it remains unclear whether citizens apply partisan schemas in the *moment* they encounter a cue from a co-partisan leader, or whether opinion change requires the mediation of subsequent partisan debate and reinforcement. The competing view, implicit in observational designs with week- or month-long intervals, is that naturalistic cue uptake is not instantaneous but requires the amplification that follows an elite signal: news coverage, social media diffusion, peer discussion, and repeated partisan framing (Zaller, 1992; Bisgaard and Slothuus, 2018). On this account, what looks like a partisan response to the *cue itself* may in fact be a response to the ecosystem of partisan commentary that develops around it. If true, immediate partisan updating would be unlikely outside experimentally controlled conditions. We test whether this mediation is necessary, or whether the partisan screen is applied the moment citizens encounter information about a co-partisan leader's action.

The lack of externally valid, temporally fine-grained evidence is problematic because the effects of partisan cues should theoretically occur *immediately*. Partisan cues are powerful because they are affectively charged (Lodge and Taber, 2013), allowing people to immediately evaluate information as positive or negative depending on whether it is congruent with their salient political group-based identity. While this theoretical problem implies that opinion shifts should ideally be measured right after a cue, longer gaps between cue and opinion measures also create empirical problems: outside experimentally controlled contexts, partisan cues usually prompt responses from opposing parties and media coverage (Bisgaard and Slothuus, 2018; Slothuus and Bisgaard, 2021) that complicates causally identifying the immediate effects of the initial cue. Moreover, cues are subsequently diffused through social networks, further complicating whether opinion shifts are a product of elite messaging or the social diffusion of it.

In this paper, we therefore ask whether we can observe immediate effects of a partisan cue in an externally valid setting. To answer this question, we employ the unexpected event during survey design (UESD) identification strategy (Muñoz *et al.*, 2020),[1] analyzing how US President Trump's unexpected announcement of the US bombing of Iranian nuclear sites shapes public opinion. These data provide a unique opportunity to examine partisan reasoning in real time: our data assess how specific policy support, in this case support for US military intervention in Iran, is shaped *en masse*, and in a partisan-oriented asymmetric direction, within the minutes and hours immediately after Trump announced US military intervention. This allows us to test the effect's immediacy that we would theoretically anticipate from a partisan cue, and it isolates the effect from subsequent partisan debates and media coverage.

[1]Also referred to as a Regression-Discontinuity in Time (RDiT) design.

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We find that partisanship instantly and strongly conditions how respondents interpret the bombing. Republicans surveyed after Trump's post are more than 100% more supportive of the military intervention, whereas support among Democrats falls by 68% following the cue. This divergence in public opinion is robust across a multiverse analysis of 28,672 specifications, cannot be explained by pre-treatment trends or Type I errors, and is unique to the issue discussed in the cue. These results contribute a crucial empirical extension to the literature on partisan reasoning: they demonstrate how partisan cues prompt immediate and strong opinion divergence outside an experimentally controlled setting.

These findings have important substantive implications. First, partisan reasoning is so ingrained in American voters that it happens powerfully and immediately. The cue created stark divisions on an issue that was neither important nor polarized for most Americans. While foreign politics is generally secondary to many Americans (Bennett *et al.*, 1996), it was overshadowed by discussions about the economy and democracy at the time of data collection in June 2025. Both Republicans and Democrats were united in their skepticism of military interventions in Iran (23 vs. 15%) and preferred diplomatic solutions (61 vs. 58%) days before the cue.

Yet the cue prompted strong opinion divergence, nonetheless. This suggests that partisan schemas are applied near-automatically at the moment of exposure. Citizens activated an ingrained partisan heuristic before any party elite commentary, news framing, or peer discussion had time to develop. The rapidity of the effect is consistent with affective intelligence accounts of partisan reasoning (Lodge and Taber, 2013), in which co-partisan cues trigger immediate evaluative responses rather than deliberative updating. We acknowledge, however, that our design cannot distinguish between several theoretically plausible mechanisms: automatic in-group/out-group heuristics, motivated reasoning to protect political identity, dissonance reduction given the prior anti-intervention stance of Republicans, or expressive responding. What our evidence establishes is the *speed and scale* of partisan divergence in a naturalistic setting, not its underlying mechanism.

Second, our findings speak to the established implication of strong partisan responsiveness as a threat to democratic accountability. While Trump repeatedly suggested his administration would refrain from military interventions should he be granted a second term,[2] his partisans immediately tolerated this violation of a campaign promise. This pattern is consistent with a top-down model of public opinion (Zaller, 1992; Achen and Bartels, 2016; Bartels, 2023): rather than holding the president accountable for a reversal of a campaign commitment, Republican identifiers updated their own positions to align with his action. In this policy domain at least, the president faced no bottom-up opinion constraint (from supporters). Given the opposing direction of opinion among Democrats, we can rule out the alternative explanation that the increase in support is purely a reflection of well-documented *rally-round-the-flag* effects (Mueller, 1970, 1973; Lai and Reiter, 2005) associated with military conflict (but see Seo and Horiuchi (2024) for an empirical rebuttal).

1. Identification strategy

This study exploits an unexpected event during survey fieldwork design (Muñoz *et al.*, 2020) to estimate the causal effect of elite cues on public opinion. The UESD identification strategy has been leveraged to causally identify the effect of conflicts (Balcells and Torrats-Espinosa, 2018; Hernández and Ares, 2023; Balcells *et al.*, 2024), democratic backsliding (Chan, 2025), election results (Chan, 2024), political violence (Nai *et al.*, 2025), judicial decisions (Turnbull-Dugarte and

[2]See, for example, the following posts on the right-wing social media platform, Truth Social:https://truthsocial.com/@realDonaldTrump/posts/110034402803297281; https://truthsocial.com/@realDonaldTrump/posts/113426436990570801; https://truthsocial.com/@realDonaldTrump/posts/113612147757280297.

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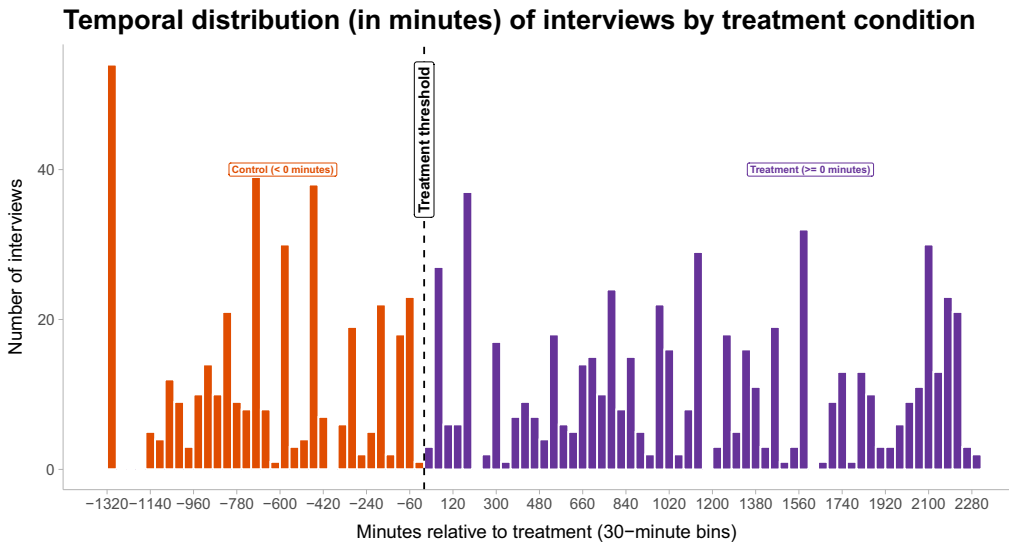


Figure 1. Distribution of survey respondents on either side of the treatment threshold.

Devine, 2022), and other political events (Unan and Klüver, 2025; Schulte and Bischof, 2026; Frese, 2026a).[3]

During the data collection for an online survey administered by YouGov among a representative sample of Americans, the United States conducted military strikes against Iranian nuclear facilities (June 21, 2025). The event, which received extensive and immediate media coverage globally, represents a real-world information shock: not only did it inform citizens of US military intervention in Iran, but it was also a *deviation* from the Trump 2.0 administration's campaign promise to limit and reduce the US military's involvement in other nations. Because the airstrikes occurred while interviews were actively underway, some respondents completed the survey immediately before the announcement, while others completed it within minutes afterward.

Formally, respondents interviewed before the public announcement serve as the *control group*, while those interviewed afterward serve as the *treated group*. The running variable is the precise interview start time, measured in minutes relative to the announcement. Under the assumption that, absent the event, potential outcomes would have evolved smoothly across the threshold, any discontinuous jump in attitudes observed at the cut-off can be interpreted as the causal effect of exposure to the airstrike announcement. The design is underpinned by the as-if random assignment of interview timing around the event (see Figure 1), which shows a continuous distribution of interviews before and after the threshold with no evidence of bunching or manipulation.

Figure 1 confirms the validity of the research design. Interviews are evenly distributed around the treatment threshold, with no discernible discontinuity in survey timing. This pattern is consistent with as-if random assignment of respondents into the pre- and post-announcement conditions, satisfying the central identifying assumption of the UESD framework. A balance test modeling treatment assignment as a function of observable covariates is reported in Appendix Table A4. In addition to observing partisan balance among the control and treatment groups, we observe no variation of note or significance between the two groups in terms of gender, race, sexuality, age, education level (and status), urban/rural location, income, or voter registration status. We can be confident that the

[3]For a more comprehensive overview of the case usage and estimation strategies most frequently applied with this quasi-experimental design, see Frese (2026b).

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difference between the two groups is exposure to the announcement of military action.

$$Y_i = \alpha + \delta_1 \text{Treatment}_i + \delta_2 \text{ Partisan ID}_i + \delta_3(\text{Treatment}_i^{\star} \text{ Partisan ID}_i) + \epsilon_i$$

To test whether partisans react differently to the same elite cue, the analysis models the outcome variable—support for US military intervention in Iran[4]—as a function of treatment exposure, partisanship, and their multiplicative interaction (Equation 1).[5] The estimands of interest are the conditional average treatment effect (CATE) of quasi-random exposure among each group of partisans (Democrats and Republicans), as well as the *difference* in the CATE between the two. Our modeling approach relies on linear regression and reports robust (HC2) standard errors.[6]

2. Results

Figure 2 plots mean levels of support for military intervention in 30-min intervals relative to the announcement, separately for Democrats and Republicans.[7] Among Democrats, support for intervention is, if anything, modestly increasing in the pre-treatment period—consistent with the small positive pre-treatment trend identified in the placebo analysis (Appendix Figure C.1)—but drops significantly, sharply and immediately afterwards. Among Republicans, support is flat before the announcement and spikes significantly, sharply and immediately after the announcement. The fitted local linear regressions on either side of the cut-off reveal a clear discontinuity at time $t = 0$, indicating a partisan divergence triggered by exposure to the airstrike news. In short, the same factual event is interpreted through a partisan lens, consistent with theories of motivated reasoning and elite cue-taking: Republicans *instantly* and dramatically update their support for US military intervention in Iran, whereas Democrats, reacting in direct opposition to the out-group party actions, *instantly* become less supportive of the same issue in response to the same information. This diversity in reactions does not increase as time passes (the post-treatment linear trends are flat), which indicates that the results do not emerge or compound as additional media reporting diffuses over time. In other words, the party gap that is observed 30 min after the news broke is statistically symmetrical to that which is observed 10 h or 40 h after it broke.

Figure 3 formalizes these differences using a regression model which estimates support for US intervention as a function of treatment allocation conditional on party identification using a global bandwidth (see Table B.1 for regression output and Figure C.2 for bandwidth sensitivity tests). Figure 3 presents predicted probabilities of support for intervention conditional on treatment and partisan identification and, inset, the (difference in) CATE. The predicted outcomes illustrate that

[4]The primary outcome is measured in response to the following question: *Do you think the U.S. military should or should not bomb Iranian nuclear facilities?* An additional outcome considered (and reported in Appendix B.2 alongside placebo dependent variables) is approval/disapproval of Donald Trump's handling of the Israel/Iran conflict more generally.

[5]The following specification, which considers pre- and post-treatment linear trends, is reported in Appendix Table B.3. The results are not conditioned by a control for linear time trends.

$$\begin{aligned} Y_i = \alpha &+ \delta_1 \text{Treatment}_i + \beta_1 \text{time}_i + \delta_2 \text{PartisanID}_i + \delta_3(\text{Treatment}_i^{*}\text{time}_i) + \delta_4(\text{Treatment}_i^{*}\text{PartisanID}_i) \\ &+ \delta_5(\text{time}_i^{*}\text{PartisanID}_i) + \delta_6(\text{Treatment}_i^{*}\text{time}_i^{*}\text{PartisanID}_i) + \epsilon_i \end{aligned}$$

[6]Statistical analysis was completed in R, relying on the *estimatr* package (Blair *et al.*, 2025) and the *marginaleffects* package (Arel-Bundock *et al.*, 2024) for estimation and the *modelsummary* package for reporting (Arel-Bundock, 2022).

[7]Our primary analysis restricts focus to self-identified Democrats and Republicans in order to examine the theoretically central question of how partisan identity conditions the interpretation of an in-/out-group cue. Independents, who constitute approximately 37% of the sample, are excluded from the main analysis because the directional prediction of the partisan cue model is ambiguous for this group. We report the treatment effect estimated among self-identified Independents in Appendix Figure D.1. The CATE for Independents is positive at around 14 percentage points. This effect is 50% smaller than that observed in the case of Republicans and directionally opposed to that observed in the case of Democrats.

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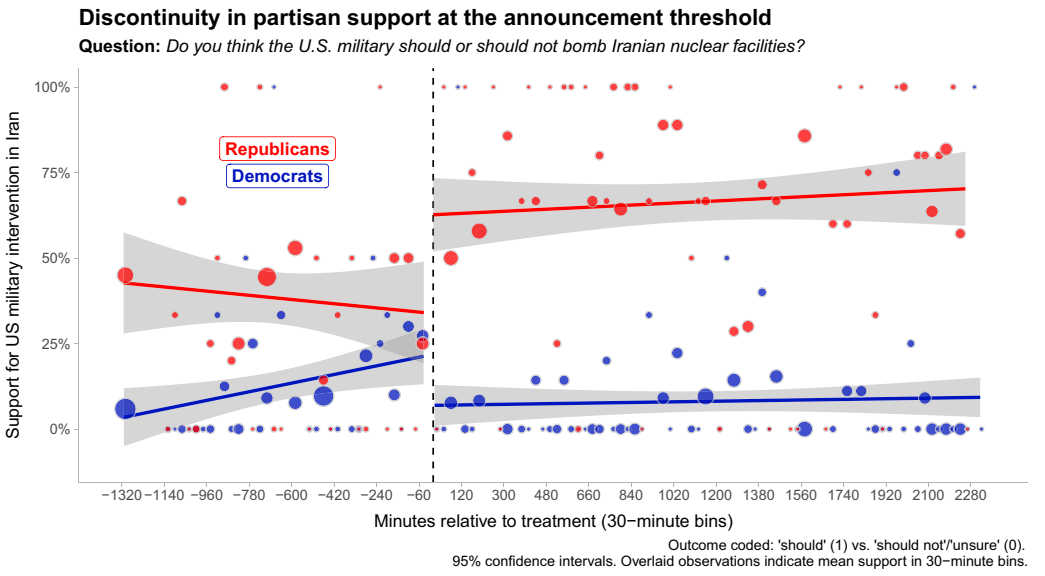


Figure 2. Descriptive illustration of discontinuity in time.

support for bombing Iran increases from 0.34 to 0.70 among Republicans following the announcement, while it decreases from 0.16 to 0.05 among Democrats. Note that, among the control group, the mean position among both Democrats *and* Republicans is opposition to intervention, indicated by predicted values below 0.5. Among the treated, there is a clear disparity between the exceptionally opposed position of Democrats (0.05) and the strong endorsement among Republicans (0.7). That is, Republicans (on average) become strongly supportive of intervention only after the announcement, while Democrats harden their opposition further. In magnitude terms, these are not small shifts in opinion. The estimated Democrat CATE is a decline of 10.9 percentage points, which corresponds to roughly 0.37 standard deviations ($d = -0.37$), while the Republican CATE is an increase of 36.0 percentage points, equivalent to $d = 0.77$. By conventional benchmarks, this implies a moderate effect among Democrats and a large effect among Republicans.

The estimated *difference* in CATE (0.47) indicates that exposure to the same information cue produces sharply divergent attitudinal shifts across partisan lines. The divergence is not small. In real terms, Republicans respond to news of the Republican president's action by increasing their support for the issue by more than 100% compared to the control group baseline, and Democrats depress their support for the same issue by 68%. This pattern illustrates real-time partisan updating in response to unexpected international events which, as a consequence, results in an increase in the disparity in public opinion upon polarized party lines.

To probe the stability of these findings, Figure 4 presents a multiverse specification curve (Simonsohn *et al.*, 2020) covering 57,344 identified conditional treatment effects (28,672 per partisan group) and 28,672 tests of the difference between these two CATE estimates. Each specification varies both the inclusion of covariates (i.e., age, gender, race, LGBT (lesbian, gay, bisexual or trans) identity, level of education, income, student status, urban/rural location, religiosity, voter registration status, ideology, and having a social media account on X[8]) as well as the temporal bandwidth

[8]We do not have an indicator that identifies respondents who are users of Truth Social. *X* remains, however, an alternative platform which, following the takeover of then-*Twitter* by Elon Musk, has become an important network where Trump's messages are shared and disseminated.

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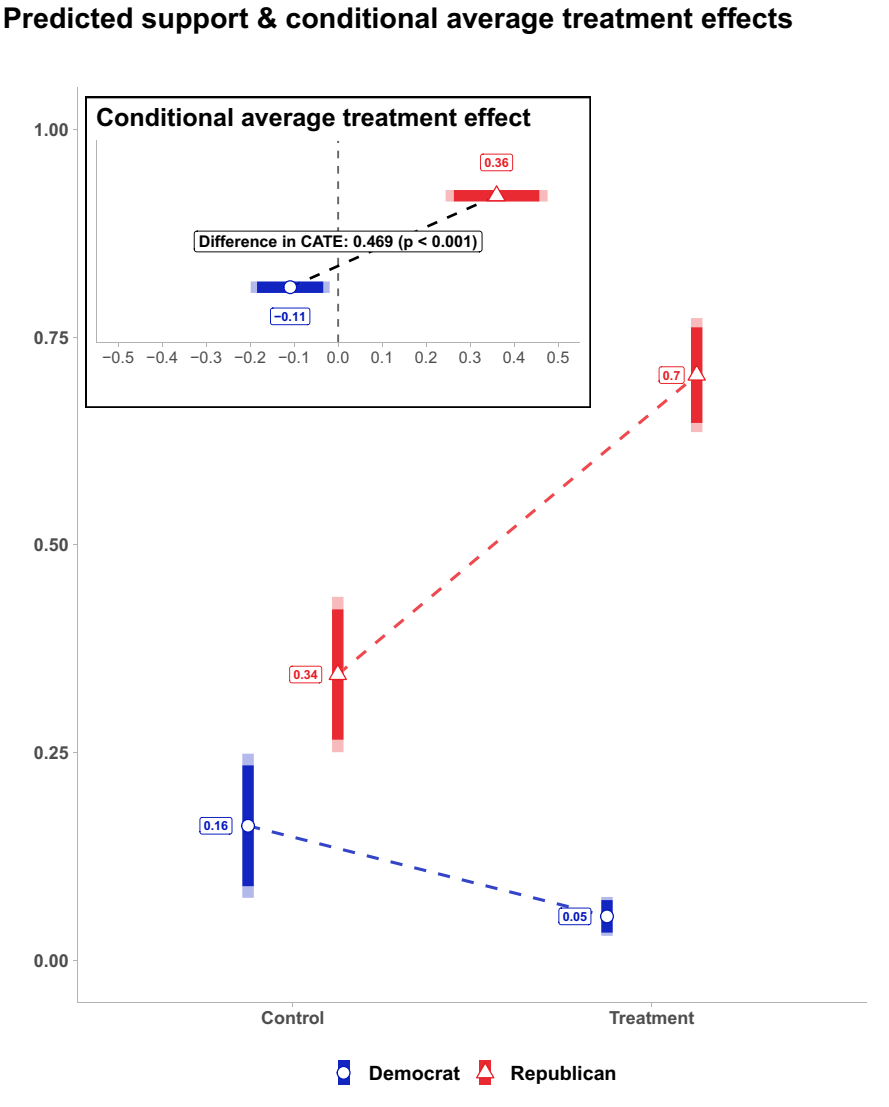


Figure 3. Causal evidence of partisan responsiveness in real-time (see Appendix Table B.1).

around the event window. The specification curves demonstrate remarkable robustness. Across virtually all model variants, estimated treatment effects remain negative among Democrats (median ATE $= -0.104$, $p = 0.032$) and positive among Republicans (median ATE $= 0.315$, $p < 0.001$). These results confirm that the observed partisan asymmetry is not an artifact of model specification or sample restriction.

As stated above, it is again worth emphasizing that the effects are far from trivial. Even under the small proportion of specifications among Democrat respondents where the p-value is above conventional thresholds (4.3% of estimations), an 11 percentage point estimate represents an effect that is statistically improbable under the null of no effect (see Appendix Figure C.3 for more detail). Updating among Republicans is more precisely estimated. That said, there are zero specifications

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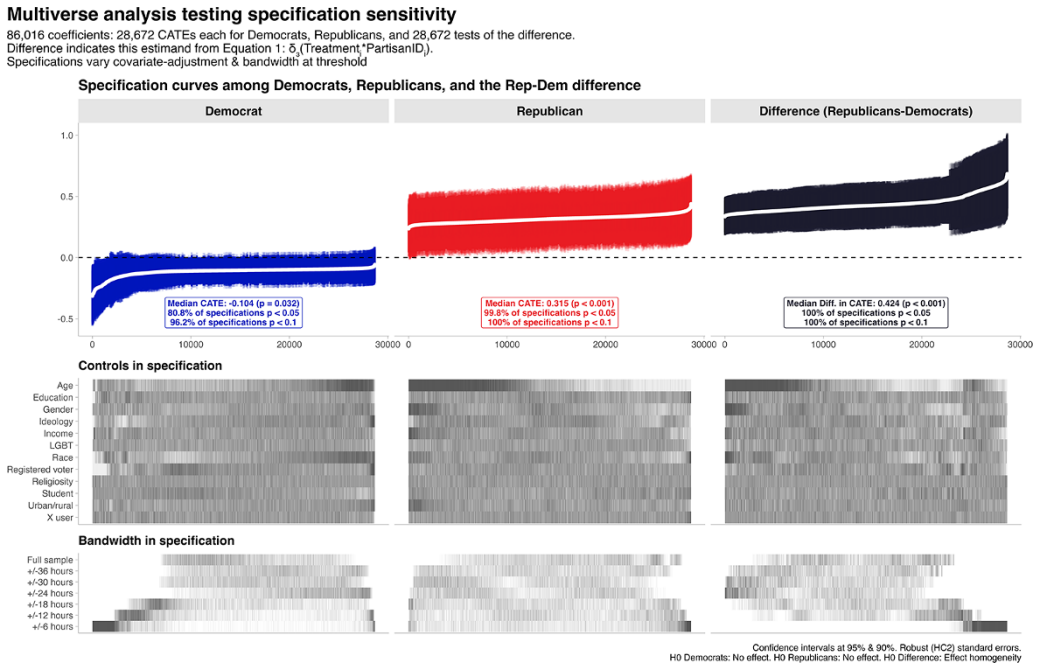


Figure 4. Multiverse analysis across alternative specifications.

among those considered where the *difference* between how Republicans and Democrats react to the news is not statistically significant. In other words, we can confidently and precisely reject the null of effect *homogeneity* between the two partisan camps. In terms of the magnitude of the partisan divergence, the median difference in the CATE from 28,672 comparisons is 0.42 ($p < 0.001$).

In addition to the multiverse analysis reported here, which demonstrates that our causal estimates are not sensitive (in magnitude or significance) to alternative specifications, we also demonstrate that the identified effects are not the result of pre-treatment trends, nor are they of a comparable size to those that might be observed by chance (type I error).

First, as shown in Appendix Figure C.1, we re-estimate the analysis using the median interview start time as a faux treatment threshold, as recommended by Muñoz *et al.* (2020), to test for the presence of pre-treatment linear trends. The results of this analysis demonstrate that the identified treatment effects are not the result of pre-existing time trends in public opinion.[9] Second, given the potential increased risk of Type I errors in UESD applications (Frese and Riaz, 2025), we assess whether the observed effects are distinct from those which might be identified by chance. We rely on randomization inference (Gerber and Green, 2012) and identify *zero instances* in 10,000 where effects comparable to those causally identified would be observed under the null of no treatment effect and the null of effect homogeneity between partisans.[10] Third, we test for the conditional effect of treatment on distinct placebo items (evaluations of inflation and immigration) which, theoretically, should

[9]We identify small linear trends in the direction *opposite* to that induced by treatment. Before treatment, Democratic respondents were, if anything, becoming more supportive of US intervention in Iran. In the case of the Republicans, the linear trends are negative (the reverse of treatment) but not significantly distinct from zero.

[10]The results of the randomization inference approach are reported in Appendix Figure C.3. Empirically, we execute 10,000 permutations where observations are resampled and assigned to different treatment conditions. We do this three times: once for the individual CATE for each partisan block (Democrats and Republicans) independently, and a third time comparing the difference in the CATE between each group.

https://doi.org/10.1017/psrm.2026.10135

not be asymmetrically impacted by treatment. As shown in Table B.2, the overall partisan responsiveness we observe in the case of support for US military intervention in Iran is not a product of broader general partisan division that emerges in response to treatment. There is treatment-induced partisan responsiveness in evaluations of Trump's handling of the Israel-Iran conflict but no effects on evaluations of inflation or immigration.

3. Discussion

Our design, leveraging an unanticipated natural experiment in survey timing within minutes, provides unique causal insight into a classic debate: the importance of elite (partisan) cues for people's policy support. Partisan reasoning is most often tested via experimental manipulations within (online) surveys (Bolsen *et al.*, 2014; Broockman and Butler, 2017; Barber and Pope, 2019), which may not reflect how voters receive political information and have well-covered issues of external validity. Instead, our design maximizes external validity by taking advantage of random and natural treatment which is temporally unique: there are minutes and hours separating the "treatment" and "control" groups, minimizing the threat of confounding. Moreover, the treatment more likely signals the "pure" effect of the announcement before it is heavily mediated through media coverage, public discussion, and challenged by countervailing narratives.

One nuance we acknowledge, however, is that the pathway from the original cue to respondent exposure may well differ across partisan groups. Republicans, and in particular MAGA-aligned Republicans, are comparatively more likely to follow Trump on Truth Social directly or be exposed to Truth Social content via their immediate social networks, meaning that their initial exposure plausibly occurred via the original post. Democratic respondents and less politically interested Republicans, by contrast, are far less likely to be Truth Social users and would more commonly have encountered the announcement through a secondary source such as a breaking news alert that may have added minimal framing alongside the factual report of the strikes. This asymmetry implies that the "pure cue" interpretation is most credible for certain Republican respondents, while other responses in the immediate post-treatment window reflect the cue *plus* the earliest media coverage of it. Overall, our evidence suggests that partisanship in the US is an almost-immediate perceptual screen that can condition attitudes even to the extent of turning moderate opposition to military action to strong support. Given the longstanding debate about how people form opinions—whether it is the cue, how and whether it is mediated, or countervailing opinions (Zaller, 1992; Newton, 2006; Paluck and Green, 2009)—it is remarkable how quickly people polarize, even given the context of the United States.

One methodological contribution of our paper rests in demonstrating how granular time-based survey data can be leveraged for inferential purposes. The overwhelming majority of UESD applications use days as the running variable (Frese, 2026b). Daily measures are often required by data availability rather than chosen for theoretical reasons. Publicly available data from large-N surveys frequently record only the date of interview (if any interview time data at all) rather than the precise start time. Where fine-grained temporal data exist, however, a running variable based on smaller temporal windows, when statistical power permits it, offers meaningful advantages. First, and as inferentially useful for the purpose of our research question, it allows researchers to narrow the bandwidth to a window in which confounding information flows like media coverage, peer discussion, and social-media diffusion have not yet had time to accumulate. Second, it reduces heterogeneity within the post-treatment group: respondents interviewed 30 min after an announcement and those interviewed 2 days later may have encountered qualitatively different information environments, a source of treatment-effect heterogeneity that daily designs cannot detect.

Substantively, our paper, in addition to contributing clean causal estimates from a real-world setting to an important body of work on the power of partisan cues, also speaks to the context of contemporary US political developments. How did American voters react in response to Donald Trump's decision to bomb Iran in 2025? They reacted in a way congruent with their partisan loyalties. This

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behavior, if by no means surprising, is troubling in terms of accountability in a polarized political system. Trump generally signaled opposition to direct involvement in foreign wars to date, and support for involvement in Iran was low among Republicans before action was announced; yet when posed with confirmation of military action in Iran, Republicans became strongly *supportive* of such action. Trump was not held to account for a U-turn to an unpopular policy, but in fact the public updated their positions. We cannot say how this generalizes to other, more salient policies. An open question is whether the observed partisan divergence persists, grows, or attenuates as a conflict evolves from a single-night airstrike into a sustained military engagement. The results shown in Figure 2 suggest that there is immediate partisan polarization in the hours following the announcement that is stable until 40 h after.

At the time of writing, the US is once again engaged in military action in Iran under the second presidency of Donald Trump. Consistent with the immediate partisan responses to military intervention we identify here, Republicans are more supportive of Trump's actions in Iran than opposed, whereas the reverse is true for Democrats.[11] As the war, and its economic aftermath, continue with no clear identifiable measure of success, the partisan gulf in mass opinion may well shrink (Gelpi *et al.*, 2009). Republican support may attenuate slowly if elite cues from the president remain uniformly positive, while Democratic opposition may harden as the partisan signal accumulates through repeated out-group framing. Given the subsequent trajectory of US–Iran hostilities in the months following our data collection, our snapshot should be understood as a baseline estimate of *initial* cue responsiveness rather than a durable description of opinion.

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[11] Polling from YouGov USA from April 2026 (Montgomery, 2026) indicates that 67% of Republicans support war with Iran, and this value is higher among MAGA Republicans (77%) and dramatically lower among Democrats (9%).

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