Partisan Responsiveness in Real Time

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Working paper pending peer review.
Last compiled November 26, 2025

Abstract

Elite cues signal citizens how to interpret political events through a partisan lens. Do these effects occur in real time? We exploit US president Trump's *unexpected* announcement of US military strikes against Iran–made during the fieldwork of a nationally representative survey–to estimate the causal effect of elite cues on opinion formation. Leveraging the temporal discontinuity (in minutes) in exposure to the event, we find that partisan divergence unfolds minutes after the announcement: support for military intervention surges among Republicans but drops among Democrats. These effects are immediate, large, and robust across more than 28,000 alternative model specifications. Our evidence demonstrates the immediacy and power with which elites cues have consequences. Our findings imply a threat to democratic accountability, such that a single post suffices to polarize US public opinion and make Republican voters tolerate a policy u-turn by their president.

Keywords: elite cues; Iran; partisanship; partisan reasoning; 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.)

Introduction

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 & Taber, 2013; Mason, 2018). Partisanship shapes how people *genuinely* perceive the economy (Bartels, 2002; Graham, 2025), moderates their interpretation of anti-democratic acts (Graham & Svolik, 2020), influences who they are willing to do date (Huber & Malhotra, 2017; Turnbull-Dugarte & López Ortega, 2025), socialize with (Engelhardt & Utych, 2020), hire (Gift & Gift, 2015), and biases the inferences partisans make about others' politics (Turnbull-Dugarte & Wagner, 2025). Partisan cues — statements or acts that connect information to partisanship — often trigger such reasoning (Druckman et al., 2018; Groenendyk & Krupnikov, 2021; Zaller, 1992). 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 (Barber & Pope, 2019; Bisgaard & Slothuus, 2018; Bolsen et al., 2014; Slothuus & Bisgaard, 2021).

However, no research so far observed partisan cue effects in real time. While most existing studies use experimental designs (but see Barber & Pope, 2019; Bolsen et al., 2014; Broockman & Butler, 2017), the rare quasi-experimental evidence lacks temporal granularity. The pioneering work by Bisgaard and Slothuus (2018) and Slothuus and Bisgaard (2021), for example, demonstrated cue effects in externally valid settings, but their panel designs measured 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. 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 & 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 & Slothuus, 2018; Slothuus & 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 research note, 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 assesses how specific policy support — in this case support for US military intervention in Iran — is shaped *en masse*, and in a partisan-orientated 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.

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

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

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, partisanship reasoning is so ingrained in American voters that it happens immediately and powerfully. The cue created stark divisions on a issue that was neither important nor polarized for most Americans. While foreign politics is generally secondary to many Americans (e.g., Bennett et al., 1996), it was overshadowed by discussions about the economy and democracy (e.g., YouGov, 2025a) 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 (YouGov, 2025b). The cue prompted strong opinion divergence, nonetheless. This implies that citizens *anticipated* how their partisan line would interpret the event, long before party elites told citizens how to understand it hours and days later. 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 be be granted a second term[2], his partisans immediately tolerated this violation of a campaign promise. This reaction leaves the president virtually unchecked, allowing explicit u-turns even on the bombing of another country.

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 identifica-

[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

tion strategy has been leveraged to causally identify the effect of conflicts (Balcells & Torrats-Espinosa, 2018; Balcells et al., 2024; Hernández & Ares, 2023; Unan & Klüver, 2024), democratic backsliding (Chan, 2025b), election results (Chan, 2024, 2025a), and other political shocks (Falcó-Gimeno et al., 2025).

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 (e.g., Al Jazeera, 2025; BBC, 2025; CNN, 2025), 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 afterwards.

Formally, respondents interviewed before the public announcement serve as the *control group*, while those interviewed afterwards 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

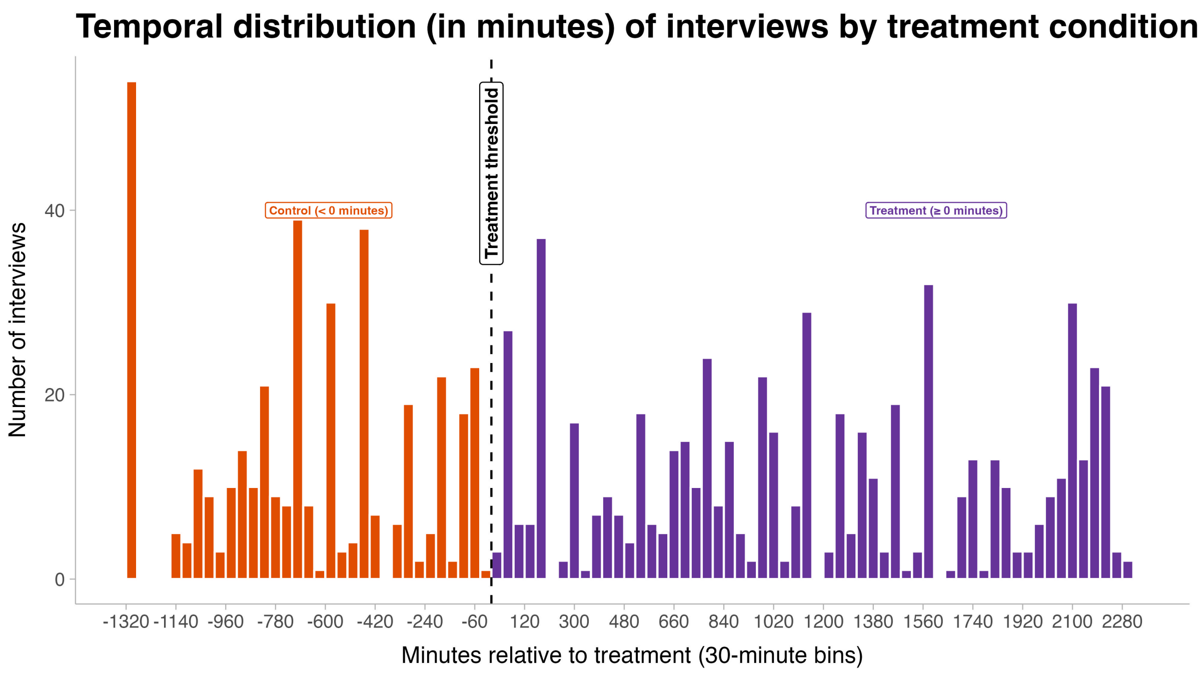

Figure 1: Distribution of survey respondents on either side of the treatment threshold

of observable covariates is reported in Appendix Table A.4. In addition to observing partisan balance among the control and treatment group, 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 difference between the two groups is exposure to the announcement of military action.

$$Y_i = \alpha + \delta_1 Treatment_i + \delta_2 PartisanID_i + \delta_3(Treatment_i * PartisanID_i) + \epsilon_i \quad (1)$$

To test whether partisans react differently to the same elite cue, the analysis models the outcome variable – support for US military intervention in Iran[3] – as a function of treatment exposure, partisanship, and their multiplicative interaction (Equation 1)[4].

[3]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.

[4]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 trends.

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 OLS regression and reports robust (HC2) standard errors.[5]

Results

Figure 2 plots mean levels of support for military intervention in 30-minute intervals relative to the announcement, separately for Democrats and Republicans. Among Democrats, support for intervention declines as the announcement approaches and becomes lower thereafter. Among Republicans, support rises 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.

Figure 3 formalizes these differences using an OLS model which estimates support for US intervention as a function of treatment allocation conditional on party identification (see Table B.1 for regression output). 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 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 con-

$$Y_i = \alpha + \delta_1 Treatment_i + \beta_1 time_i + \delta_2 PartisanID_i + \delta_3 (Treatment_i * time_i * PartisanID_i) + \epsilon_i \quad (2)$$

[5]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).

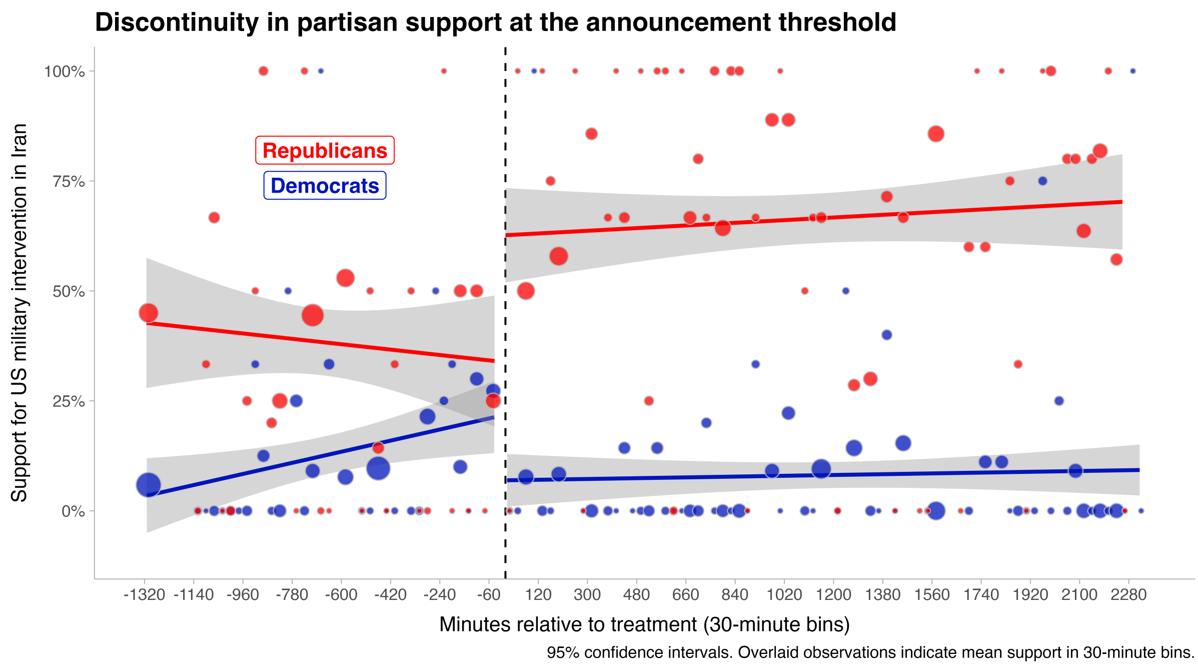

Figure 2: Descriptive illustration of discontinuity in time

trol 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, whilst Democrats harden their opposition further.

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 increasing 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 28,672 model specifications (14,336 per partisan group). Each specification varies both the inclusion of covariates (e.g., age, gender,

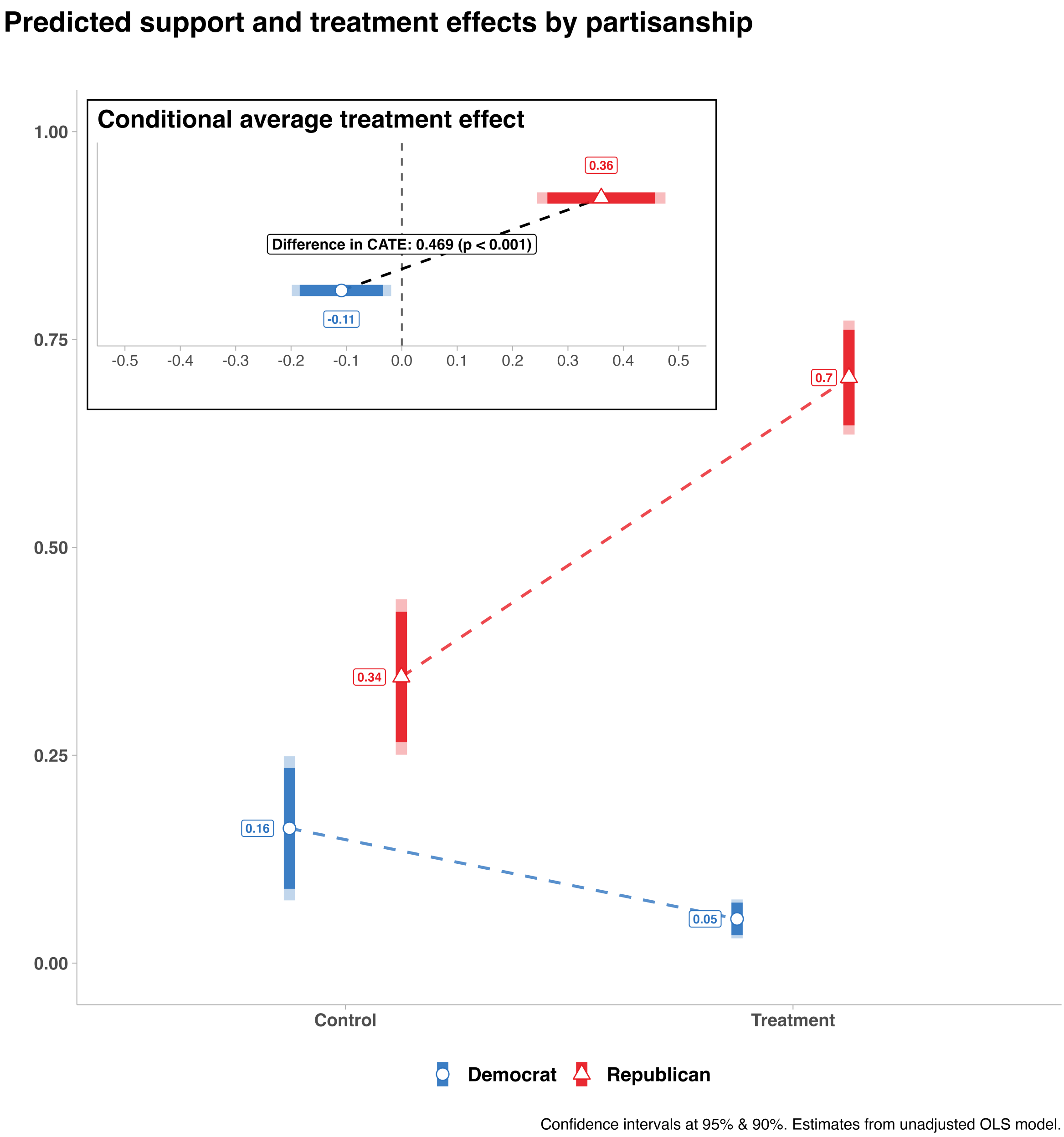

Figure 3: Causal evidence of partisan responsiveness in real-time

race, LGBT identity, level of education, income, student status, urban/rural location, religiosity, voter registration status and ideology) as well as the temporal bandwidth around the event window. The specification curves demonstrate the remarkable robustness of our causal evidence. Across virtually all model variants, estimated treatment effects remain negative among Democrats (median ATE $= -0.11$, $p = 0.032$) and positive among Republicans (median ATE $= 0.32$, $p < 0.001$). These results confirm that the observed partisan asymmetry is not an artifact of model specification or sample restriction. The effects are also large: even under the small proportion of specifications among Democrat respondents where the p-value is above conventional thresholds (4.3% of estimations), an eleven percentage-point point estimate represents an effect that is statistically improbable under the null of no effect (see Appendix Table C.1 for more detail).

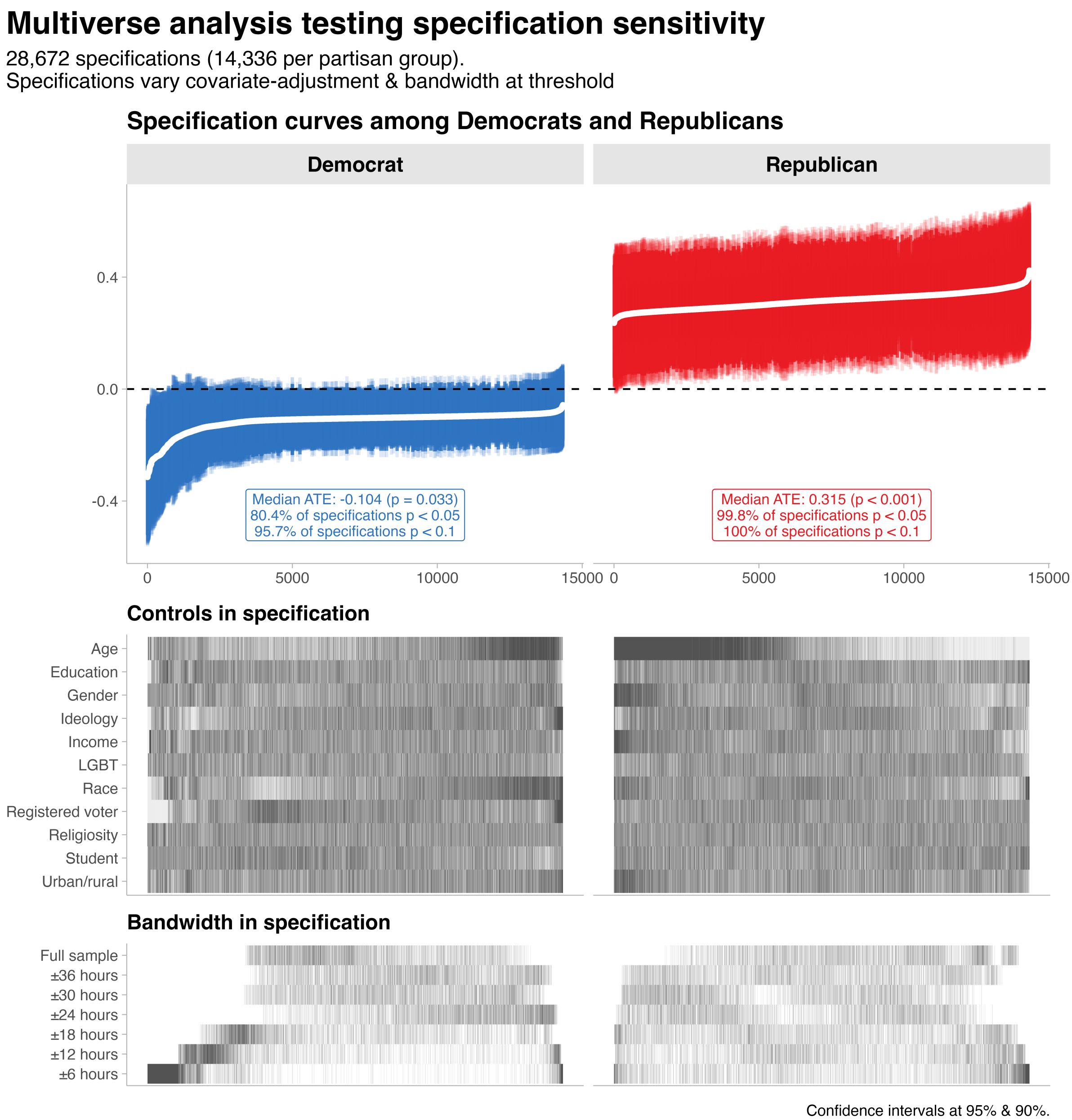

Figure 4: Robustness of evidence across diverse specification choices

In addition to the multiverse analysis reported here, which demonstrates the robustness of our causal estimates in terms of both significance and effect magnitude is not sensitive 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.[6] Second, given the potential increased risk of Type I errors in UESD applications (Frese & Riaz, 2025), we assess whether the observed effects are distinct from those which might be identified by chance. We rely on randomization inference (Gerber & 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.[7] Third, we test for the conditional effect of treatment on distinct placebo items (evaluations of inflation and immigration) which, theoretically, should 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 in Iran is not a product of broader general partisan division that emerge 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.

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. It relies on *naturally occurring* treatment

[6]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 the Republicans, the linear trends are negative (the reverse of treatment) but not significantly distinct from zero.

[7]The results of the randomization inference approach are reported in Appendix Figure C.3 and Table C.1). 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.

exposure. Partisan reasoning is often tested via experimental manipulations within (online) surveys (Barber & Pope, 2019; Bolsen et al., 2014; Broockman & Butler, 2017), 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, discussion, and challenged by countervailing narratives. 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 of 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 (Newton, 2006; Paluck & Green, 2009; Zaller, 1992) - it is remarkable how quickly people polarise, even given the context of the United States.

This has substantive and methodological implications. Substantively, it 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; 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. Methodologically, similar studies with longer bandwidths (that is, measured in days and weeks rather than minutes and hours) are likely valid: our results (e.g., Figure 2) suggest that there is immediate partisan polarization in the hours following the announcement that is stable until 40+ hours after. Thus, whilst we can't be sure, our results are at least consistent with existing conclusions. Our contribution, however, is to show not only that this happens, but elite cues exert an immediate, substantive, and relatively long-lasting.

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Appendix:

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Appendix

A Summary statistics & balance

The online survey was administered by YouGov US to a sample of online panel respondents (N=1590) that reflect the demographic composition of the US adult population when weighted. All of the analysis reported in the main paper and the accompanying analysis relies on the post-stratification weights provided by YouGov.

Table A.1: Descriptive statistics (numeric variables)

	Unique (#)	Missing (%)	Mean	SD	Min	Median	Max
Supports intervention in Iran	3	0	0.3	0.4	0.0	0.0	1.0
Positive eval. of Trump handling Israel/Iran conflict	3	0	0.3	0.5	0.0	0.0	1.0
Positive eval. of Trump handling inflation	3	0	0.3	0.5	0.0	0.0	1.0
Positive eval. of Trump handling immigration	3	0	0.4	0.5	0.0	0.0	1.0
Weight	1295	0	1.0	1.0	0.1	0.8	6.5

Table A.2: Descriptive statistics (categorical variables)

		N	%
Treatment condition	Control	595	37.4
	Treatment	995	62.6
Party ID	Democrat	555	34.9
	Independent	596	37.5
	Republican	439	27.6
LGBT status	Not LGBT	1330	83.6
	LGBT	260	16.4
Gender	Man	692	43.5
	Woman	898	56.5
Race	White	1077	67.7
	Black	193	12.1
	Hispanic	203	12.8
	Other	117	7.4
Age	Under 30	304	19.1
	30-44	416	26.2
	45-64	523	32.9
	65+	347	21.8
Education level	High school	423	26.6
	Some college	554	34.8
	College grad.	390	24.5
	Postgrad.	223	14.0
Urban/rural	Urban	470	29.6
	Suburban	622	39.1
	Rural/Other	498	31.3
Family income	Under $50K	624	39.2
	$50K-100k	480	30.2
	$100K+	330	20.8
	Prefer not to say	156	9.8
Religiosity	Not Religious	693	43.6
	Religious	897	56.4
Registered voter	Registered	1455	91.5
	Not registered	135	8.5
Student status	Student	136	8.6
	Not student	1454	91.4

Table A.3: Survey question wording of core outcome & placebos

Outcome question	Response items	Recoded
Do you think the U.S. military should or should not bomb Iranian nuclear facilities?	Should Should not Unsure	1-0, Unsure NA
Do you approve or disapprove of the way Donald Trump is handling these specific issues? 1. Inflation/prices 2. Immigration 3. Israel/Iran conflict	Strongly approve Somewhat approve Somewhat disapprove Strongly disapprove No opinion	1-0, No opinion NA

Table A.4: Balance test predicting treatment assignment

(Intercept)	0.530***
	(0.097)
Party ID: Independent	0.037
	(0.044)
Party ID:Republican	0.003
	(0.050)
LGBT	0.031
	(0.050)
Gender: Woman	0.021
	(0.035)
Race: Black	0.013
	(0.059)
Race: Hispanic	0.010
	(0.053)
Race: Other	0.005
	(0.077)
Age: 30-44	−0.032
	(0.059)
Age: 45-64	−0.042
	(0.056)
Age: 65+	−0.057
	(0.061)
Education: Some college	−0.034
	(0.048)
Education: College grad.	−0.048
	(0.051)
Education: Postgrad.	−0.008
	(0.059)
Urban/rural: Suburban	0.053
	(0.041)
Urban/rural: Rural/Other	−0.007
	(0.047)
Income: $50K-100k	0.032
	(0.041)
Income: $100K+	0.077
	(0.049)
Income: Prefer not to say	0.047
	(0.062)
Religious	0.077*
	(0.039)
Not registered to vote	−0.006
	(0.058)
Not student	0.037
	(0.075)
N	1590
R2	0.017

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

B Regression output

Table B.1: Modelling (OLS) treatment effect on support for US intervention

	Treatment only	Treatment*Party interaction
(Intercept)	0.255***	0.162***
	(0.032)	(0.044)
Treatment	0.136**	−0.109*
	(0.043)	(0.046)
Republican		0.182**
		(0.065)
Treatment*Republican		0.469***
		(0.075)
N	992	992
R2	0.020	0.327
RMSE	0.46	0.39

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.2: Modelling (OLS) treatment effect on alternative outcomes

	US intervention	Israel/Iran conflict	Placebo: Inflation	Placebo: Immigration
(Intercept)	0.162***	0.078**	0.061*	0.114**
	(0.044)	(0.028)	(0.026)	(0.035)
Treatment	−0.109*	−0.044	0.019	−0.025
	(0.046)	(0.030)	(0.036)	(0.043)
Republican	0.182**	0.492***	0.538***	0.718***
	(0.065)	(0.061)	(0.060)	(0.054)
Treatment*Republican	0.469***	0.291***	0.120	0.081
	(0.075)	(0.070)	(0.075)	(0.066)
N	992	991	992	991
R2	0.327	0.504	0.406	0.592
RMSE	0.39	0.34	0.35	0.30

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: Models including treatment*time-trend interaction by partisanship

	Democrats	Republicans	Pooled interaction
(Intercept)	0.316**	0.327***	0.316**
	(0.106)	(0.087)	(0.106)
Treatment	−0.254*	0.346**	−0.254*
	(0.109)	(0.109)	(0.109)
Running variable	0.015*	−0.002	0.015*
	(0.007)	(0.007)	(0.007)
Treatment*running variable	−0.015*	0.003	−0.015*
	(0.007)	(0.008)	(0.007)
Republican			0.011
			(0.137)
Treatment*Republican			0.600***
			(0.154)
Running variable*Republican			−0.016+
			(0.010)
Treatment*Running variable*Republican			0.018+
			(0.010)
N	554	438	992
R2	0.075	0.125	0.336
RMSE	0.30	0.48	0.39

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C UESD diagnostics

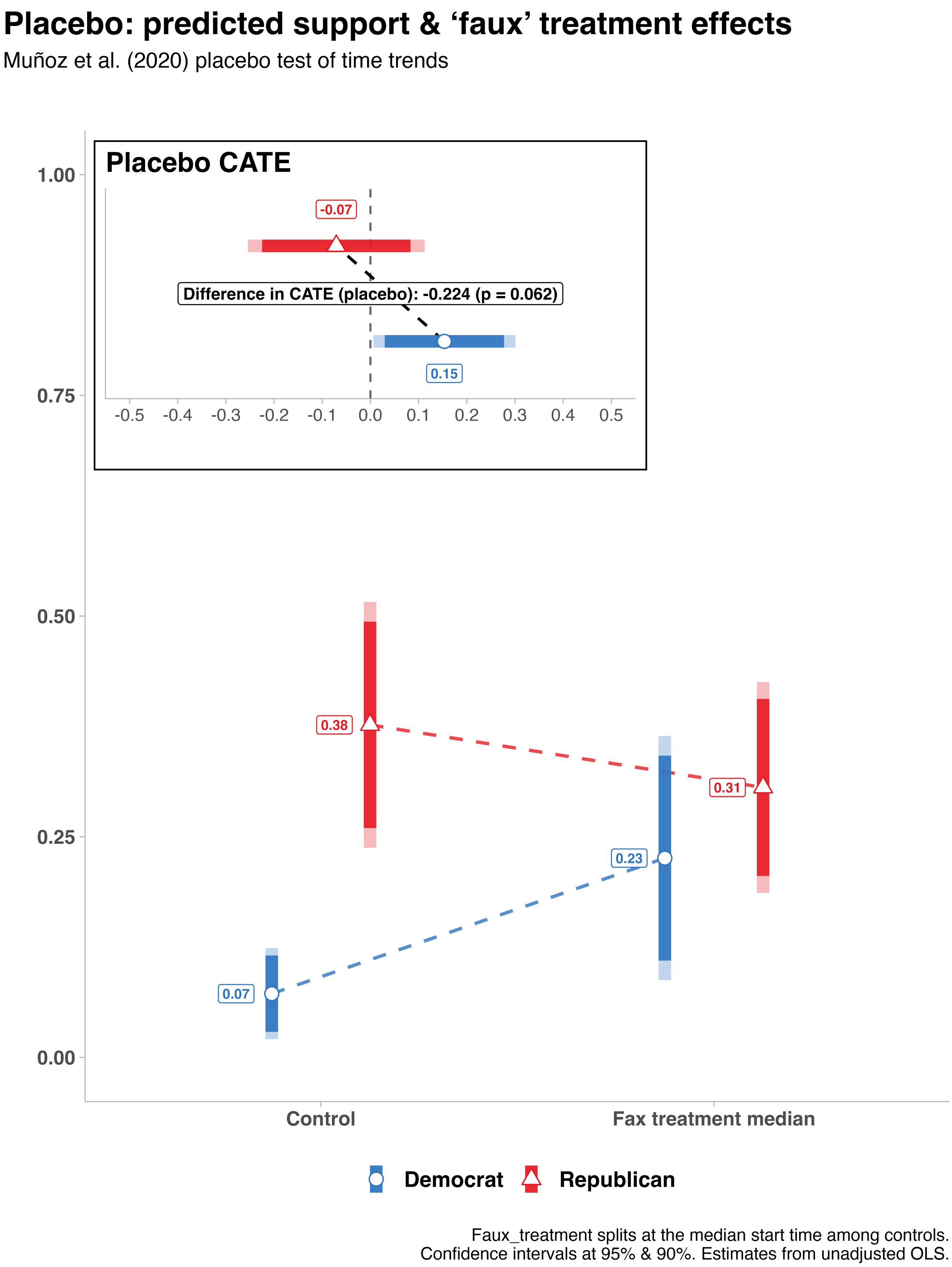

Figure C.1: Placebo test for pre-treatment time trends

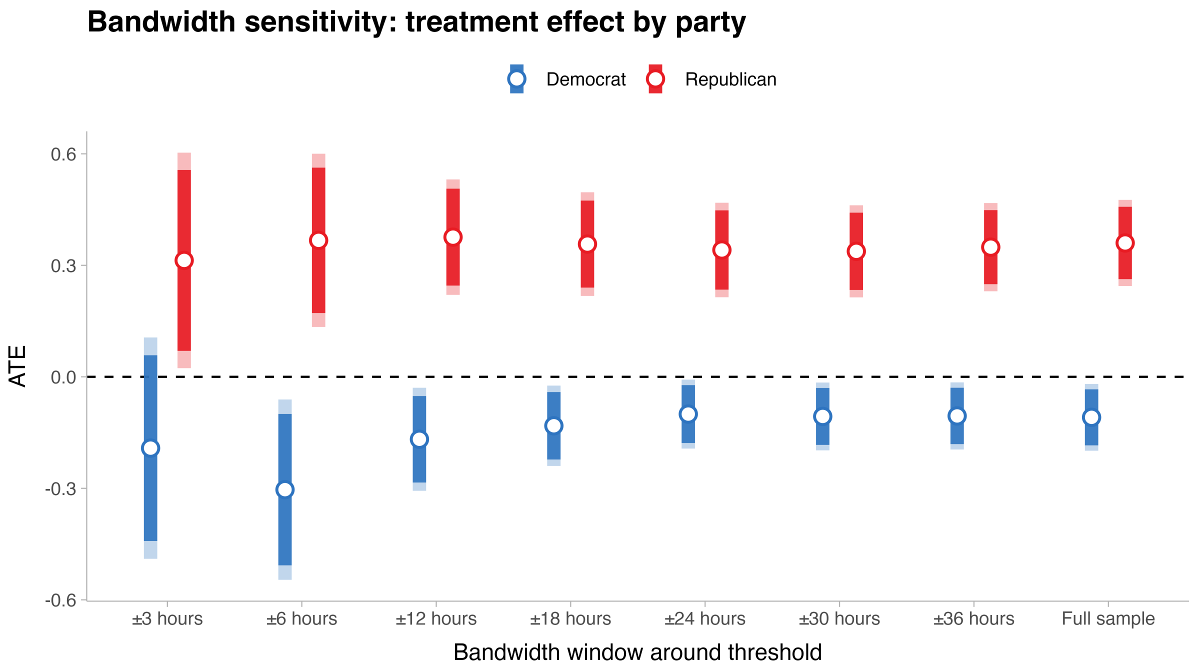

Figure C.2: Bandwidth sensitivity test

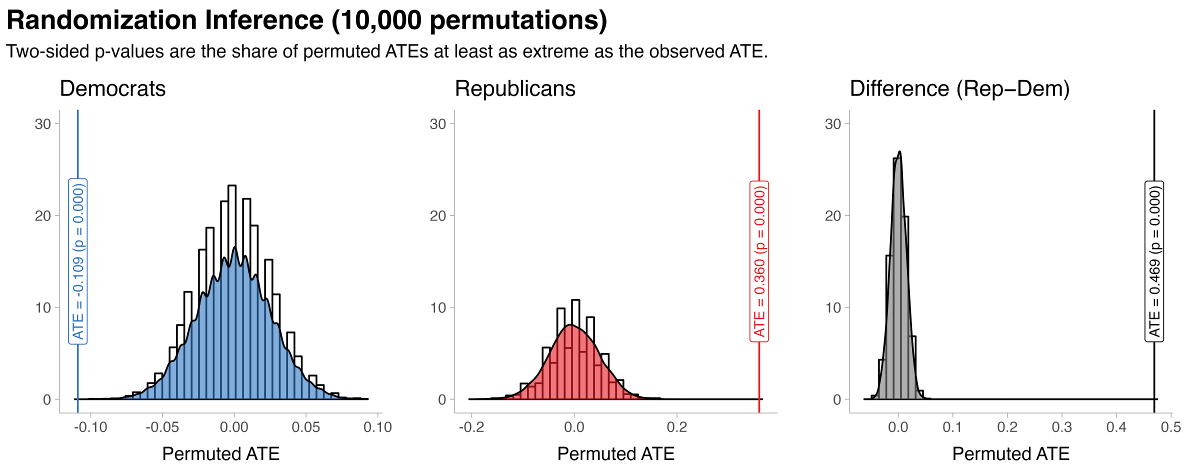

Figure C.3: Permutation test (randomisation inference)

Table C.1: Randomization inference — treatment by party

Group	T(obs)	c	n	p	CI 2.5%	CI 97.5%	H0
Democrats	-0.1090	0.00	10000	$p < 0.001$	0.00000	0.00037	Treatment = 0
Republicans	0.3602	0.00	10000	$p < 0.001$	0.00000	0.00037	Treatment = 0
Difference (Rep vs. Dem)	0.4692	0.00	10000	$p < 0.001$	0.00000	0.00037	Diff. in treatment = 0