Does Islamist Terrorism Still Affect Political Attitudes?

Supporting Information (SI) – Appendix

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Contents

1 Study 1

1.1 Temporal Update

We extended the temporal coverage of Godefroidt's (2023) meta-analytical dataset by adding all relevant studies that were published or otherwise made available (e.g., pre-prints) between January 2020 and March 2023. As noted in the paper, we used the same four-fold search strategy:

1. A search for relevant manuscripts in the three electronic databases (i.e., Web of Science, ProQuest, and EBSCO) using the same search string: (prejudice OR stereotyp* OR out-group OR attitud* OR authoritarian* OR conservat* OR "public opinion" OR "policy support" OR "political consequences" OR "political tolerance" OR ideolog* OR voting OR vote*) AND (terror* OR attack* OR "political violence" OR bomb* OR "September 11" OR "9/11" OR "March 11" OR "Charlie Hebdo" OR "Paris attacks" OR "Utoya" OR "Utøya");

2. A screening of relevant qualitative review articles published after December 2019 (i.e., Ahmed and Lynch 2021; Haghani et al. 2022; Helbling and Meierrieks 2022);

3. A public call on Twitter and via the mailing lists of the European Political Science Association, European Consortium for Political Research, International Society for Political Psychology, and the Society for Political Methodology;

4. For- and backward search, that is, the reference lists of all retrieved studies were screened for additional relevant studies, and so were all recent citations of several influential studies (Davis and Silver 2004; Huddy et al. 2005; Godefroidt 2023) in Web of Science and Google Scholar.

The search led to the identification of 3,031 possibly relevant records. In the next step, we identified and removed duplicates using Zotero ($n = 266$) and subsequently screened the remaining 2,765 records for inclusion in the meta-analysis. As noted in the paper, our criteria for the identification of relevant manuscripts closely follow Godefroidt (2023). Given our research focus, we in addition required that studies provide estimates of the effects of *Islamist* terrorism and on the political attitudes of citizens in *Western* democracies. Table S1.1 includes the full list of inclusion and exclusion criteria.

Again following the approach by Godefroidt (2023), we followed a two-step screening procedure. First, we screened the title and abstract and excluded all records which clearly did not meet our inclusion criteria. Second, we screened the full text of the remaining 88 manuscripts to check in more detail whether our inclusion criteria are met. The PRISMA flowchart in Figure S1.1 provides additional information on the data collection and selection process.

Table S1.1: Inclusion and exclusion criteria

	Inclusion criteria	Exclusion criteria
Original criteria (Godefroidt 2023):		
Constructs	Studies on the relationship between terrorism[*] (independent variable) and political attitudes (dependent variable)[†]	Studies on the impact of civil war, repression, genocide, random or non-political violence, mortality salience, etc.
Units	Studies with individuals as the unit of analysis	Studies with communities, countries, or other aggregated units of analysis. Studies using news articles, vote shares, tweets, etc. as their unit of analysis
Study	English, French, and Dutch studies. No restriction regarding publication type	Studies in other languages or released after late March 2023
Design	Studies reporting a quantitative measure of association and its precision	Systematic reviews and meta-analyses, focus groups, in-depth interviews, case reports, editorials, and commentaries
Statistics	Sufficient information to calculate effect sizes and precision	Insufficient information to calculate effect sizes and precision[‡]
Additional criteria:		
Type of terrorism	Studies on Islamist terrorism	Studies on any kind of terrorism other than Islamist as well as unspecified types of terrorism
Population	Studies conducted in Western democracies[§]	Studies in other countries

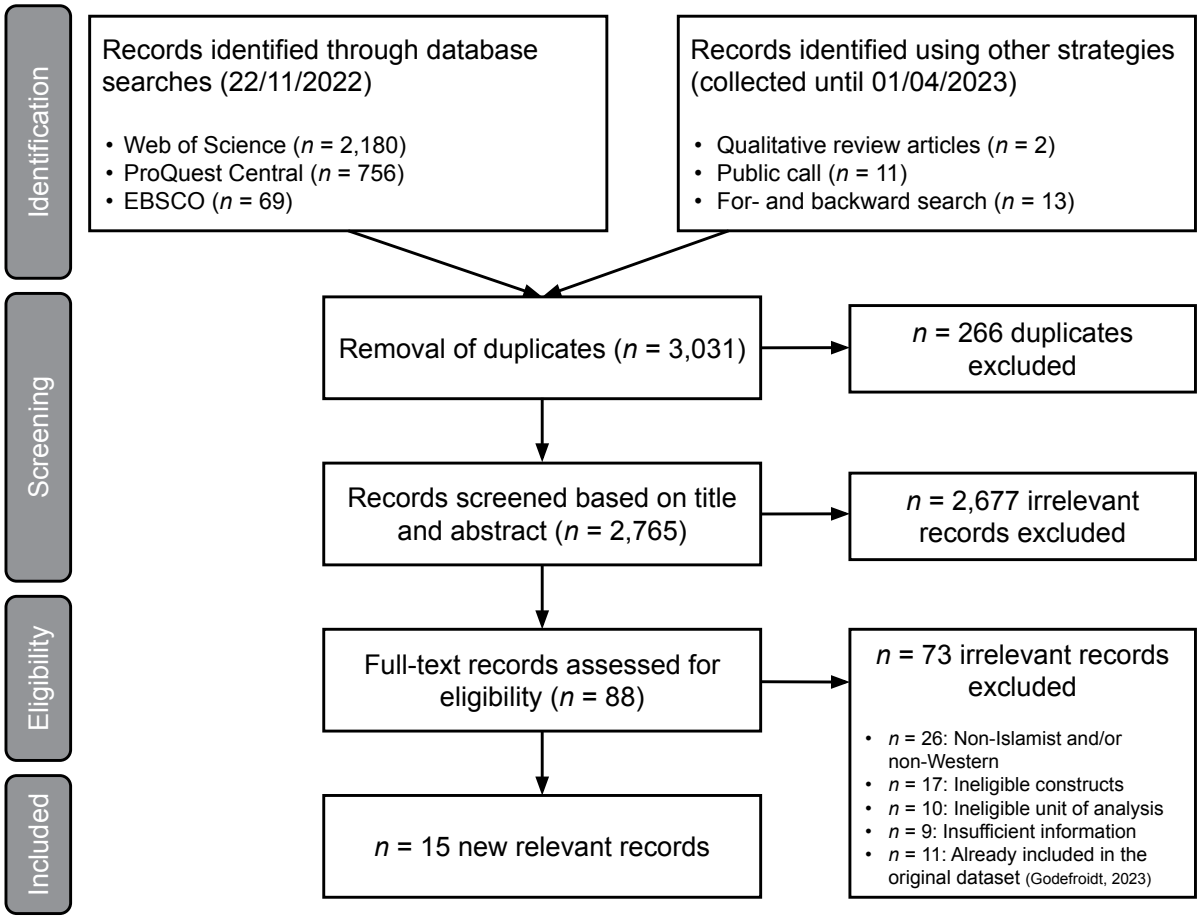
[*] Studies examining reactions to violence falling under the academic consensus definition of terrorism were eligible (Schmid 2011).

[†] Following Godefroidt (2023), we applied a broad definition of political attitudes including (a) measures of generic (e.g., right-wing authoritarianism) or specific (i.e., policy-related) political ideology, (b) outgroup attitudes (including affective, cognitive, and policy-related measures), and (c) "rally-around-the-flag" indicators (including support for incumbents, political trust, and national identification).

[‡] When studies did not report sufficient information, the corresponding author was contacted to ask for the necessary data. A study was dropped from the meta-analysis if the author did not reply or supply this information after two reminders.

[§] Countries in Europe (excluding Belarus and Russia), North America, Australia, and New Zealand are defined as Western democracies.

Figure S1.1: PRISMA flowchart of study collection and selection process



1.2 Model Specification

As noted in the paper, our baseline specification is a three-level random-intercept model:

$$y_{\text{ij}} = \beta_0 + u_{(2)\text{ij}} + u_{(3)\text{j}} + e_{\text{ij}}, \tag{1}$$

whereby the observed effect size y_i in manuscript j is assumed to be a function of the overall population effect of Islamist terrorism (β_0) and a random deviation of the observed effect size from the mean population effect size in manuscript j ($u_{(3)j}$); a random deviation of the observed effect size from the mean effect size in a given manuscript ($u_{(2)ij}$); and, finally, a random deviation of the observed effect size from the population effect due to sampling fluctuation (e_{ij}). All three error terms are assumed to be independent and normally distributed with zero mean. The sampling variance ($\tau^2_{(e)}$) is not estimated as we do not have access to original datasets but is considered as known given that it can be derived from the sample size.[1] This model set-up allows us to account for both the dependency in reported effect sizes *between* and *within* unique studies (for details, see Cheung 2015; Noortgate et al. 2013).

We use this baseline specification to estimate the average effect of Islamist terrorism on political attitudes across all previous studies. We also use it to estimate the average effects of individual attacks (Figure 1 in the paper). To formally assess the desensitization hypothesis and its theoretical alternatives, we add indicators of the study year, the number of previous Islamist terrorist attacks, and several controls to the basic specification, as explained in the paper.

Finally, to estimate the average effect of an Islamist terrorist attack (included in Figure 1 in the paper), we employ a four-level model that additionally accounts for attack-specific variance (i.e., unique characteristics of attacks, such as the number of casualties, geographic location, type and motivation of the attack, extent of media coverage, identities of victims and perpetrators, and the nature of governmental or public response).

[1] For Cohen's d, the sampling variance can be defined as: $\frac{n_t+n_c}{n_t * n_c} + \frac{y_d^2}{2(n_t+n_c)}$, with n equal to the sample size (Cheung 2015, p. 53).

1.3 Robustness Checks and Additional Analyses

1.3.1 Alternative Specifications

Table S1.2: Assessing the robustness of the main (over-time) analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Time trend						
Study year	−0.003	−0.004	−0.006	−0.007	−0.004	−0.005
	(0.004)	(0.005)	(0.005)	(0.004)	(0.005)	(0.004)
Attack severity (*Reference*: Unknown)						
Fatalities: <10	−0.033	−0.035	0.030	−0.059	−0.031	−0.061
	(0.089)	(0.090)	(0.101)	(0.127)	(0.093)	(0.074)
Fatalities: 10–100	−0.003	−0.003	0.070	0.009	0.007	−0.034
	(0.081)	(0.083)	(0.096)	(0.117)	(0.085)	(0.067)
Fatalities: >100	0.031	0.039	0.105	0.053	0.040	0.004
	(0.074)	(0.078)	(0.087)	(0.120)	(0.077)	(0.061)
Study design (*Reference*: Correlational studies)						
Randomized experiment	−0.108	−0.103	−0.051		−0.103	−0.105
	(0.064)	(0.064)	(0.073)		(0.068)	(0.054)
Natural experiment	−0.099	−0.091	−0.097		−0.103	−0.070
	(0.054)	(0.055)	(0.064)		(0.058)	(0.045)
Sample quality (*Reference*: Non-probability sample)						
Probability sample	0.047	0.052	0.080	0.032	0.060	0.025
	(0.048)	(0.049)	(0.055)	(0.047)	(0.052)	(0.041)
Outcome type (*Reference*: Conservative shifts)						
Outgroup hostility	0.035	0.034	−0.000	0.034	0.046	0.007
	(0.030)	(0.030)	(0.046)	(0.041)	(0.033)	(0.028)
Rally tendencies	−0.022	−0.024	−0.052	−0.004	−0.027	−0.031
	(0.035)	(0.035)	(0.050)	(0.048)	(0.038)	(0.032)
Country context (*Reference*: Other countries)						
U.S. study	0.151**	0.149*	0.132*	0.084	0.151**	0.093*
	(0.052)	(0.059)	(0.060)	(0.064)	(0.055)	(0.045)
9/11 (*Reference*: Studies of other and unspecified attacks)						
9/11 study		−0.020				
		(0.090)				
Constant	0.191*	0.198*	0.177	0.129	0.196	0.248**
	(0.096)	(0.100)	(0.113)	(0.141)	(0.101)	(0.080)
Non-Western attacks	✓					
Control for 9/11		✓				
No trait-like predispositions			✓			
Only natural experiments				✓		
No multiple regression estimates					✓	
Drop statistical outliers						✓
N effect sizes.	738	724	586	349	666	716
N manuscripts	112	109	99	50	101	109

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

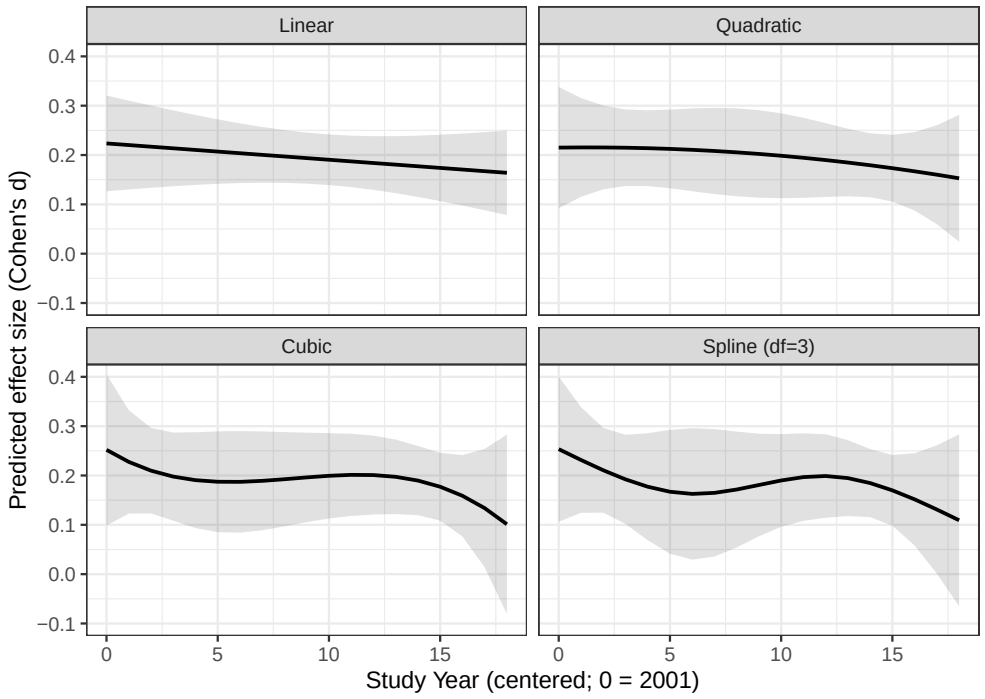
1.3.2 Non-Linear Dynamics

In the paper, we consider linear changes in the size of reported effects over time and as a result of the number of previous Islamist attacks. However, it is principally possible that the effects of Islamist terrorism evolve in more complex, non-linear ways. In this section, we consider a range of different non-linear specifications. These allow us to test whether effects on political attitudes weaken only after a threshold is reached, follow a U- or an inverted U-shape, or exhibit other non-monotonic patterns. The results provide no evidence for a non-linear temporal trend nor a non-linear correlation between the number of previous Islamist attacks and reported effects in previous literature.

Over-time analysis. We consider three non-linear specification: quadratic, cubic, and natural splines (df= 3). The quadratic specification allows for a single bend (e.g., initially stable effects that weaken later), the cubic specification allows for two bends, and the natural splines flexibly captures smooth, non-parametric changes in effect sizes over time.

Table S1.3 reports the numerical results and Figure S1.2 shows predicted values with 95% confidence intervals. We find little evidence for a non-linear time trend. Likelihood-ratio tests suggest that adding quadratic or cubic terms does not improve model fit relative to the linear model ($p = 0.28$ and $p = 0.17$, respectively). The information criteria (AIC and BIC) lead to the same conclusion, and, notably, none of the higher-order terms is statistically significant. While the plot of the spline model hints at somewhat larger effects immediately after 9/11, a decline during the following decade, and a modest uptick around the mid-2010s (ISIS period), these bends are small and well within the confidence intervals (as also evidenced by higher AIC and BIC values). In sum, while we observe some fluctuations, these are consistent with random variation and we find no evidence of systematic non-linear temporal dynamics.

Figure S1.2: Non-linear predicted values for over-time variation in effect sizes



Note: Controls are held at their sample means.

Table S1.3: Non-linear specifications (over-time analysis)

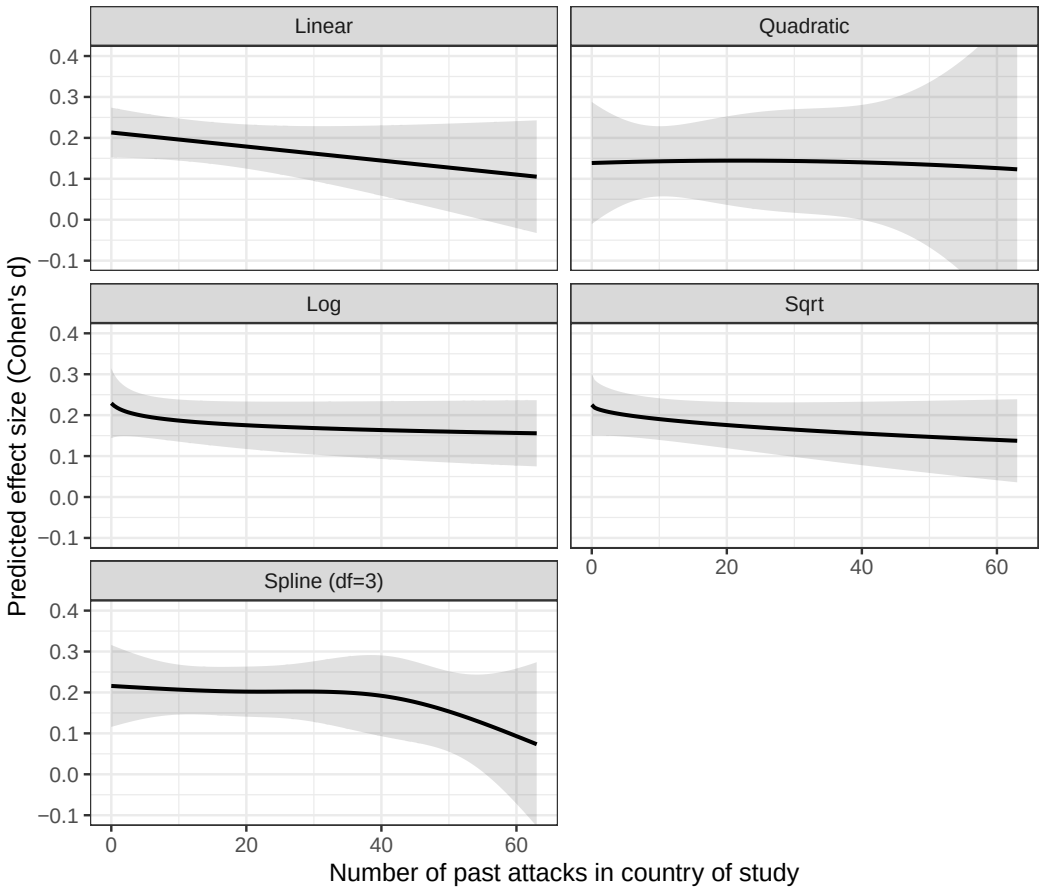
	Linear	Quadratic	Cubic	Spline (df=3)
Study year	-0.00	0.00	-0.03	
	(0.00)	(0.02)	(0.04)	
Study year^2		-0.00	0.00	
		(0.00)	(0.01)	
Study year^3			-0.00	
			(0.00)	
Spline 1				0.03
				(0.10)
Spline 2				-0.21
				(0.22)
Spline 3				-0.07
				(0.09)
Constant	0.19*	0.18	0.24	0.24*
	(0.10)	(0.10)	(0.12)	(0.12)
Controls	✓	✓	✓	✓
DF resid.	713	712	711	711
Log likelihood	-131.79	-131.38	-130.77	-130.72
Deviance	263.57	262.75	261.53	261.43
AIC	289.57	290.75	291.53	291.43
BIC	348.98	354.71	360.03	359.93
AICc	290.09	291.36	292.22	292.12
N effect sizes	724	724	724	724

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Number of previous attacks. Next, we consider whether there is a non-linear correlation between the number of previous Islamist attacks and previously reported effects on political attitudes. We focus on attacks in the same country in all previous years. As before, we report a quadratic specification, which allows for a single bend. In addition, we report log- and square-root transformations, which capture diminishing marginal effects. Finally, we again report a natural spline with three degrees of freedom, which provides the most flexibility.

Table S1.4 reports the numerical results while Figure S1.3 shows predicted values including 95% confidence intervals. In all models, we find a negative trajectory, with higher numbers of prior attacks associated with somewhat smaller effect sizes. However, the negative trend is never statistically significant and, critically, neither likelihood-ratio tests nor information criteria (AIC/BIC) suggest that non-linear models have better fit. Overall, the conclusion from the paper remains: the number of previous attacks does not systematically condition the reported effects of Islamist terrorism on political attitudes.

Figure S1.3: Non-linear predicted values for the number of past attacks in the country of study



Note: Controls are held at their sample means.

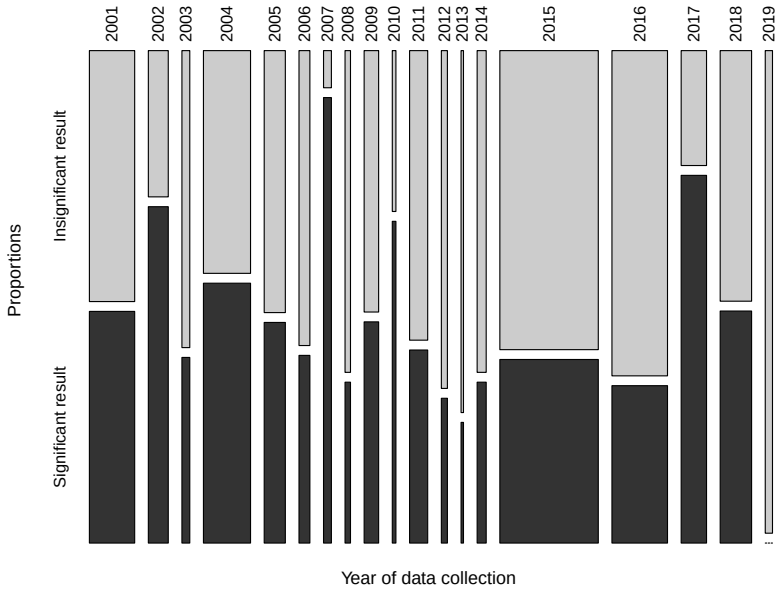
Table S1.4: Non-linear specifications (previous attacks analysis)

	Linear	Quadratic	Log	Sqrt	Spline (df=3)
Number of previous attacks	-0.00	-0.00			
	(0.00)	(0.00)			
Number of previous attacks^2		-0.00			
		(0.00)			
Log(number of previous attacks + 1)			-0.02		
			(0.02)		
Sqrt(number of previous attacks)				-0.01	
				(0.01)	
Spline 1					0.00
					(0.09)
Spline 2					-0.11
					(0.11)
Spline 3					-0.13
					(0.11)
Constant	0.16*	0.16	0.18*	0.17*	0.17
	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)
Controls	✓	✓	✓	✓	✓
DF resid.	712	711	712	712	710
Log likelihood	-131.29	-131.35	-131.56	-131.46	-131.14
Deviance	262.58	262.71	263.12	262.91	262.29
AIC	290.58	292.71	291.12	290.91	294.29
BIC	354.53	361.21	355.08	354.87	367.33
AICc	291.18	293.40	291.73	291.52	295.07
N effect sizes	724	724	724	724	724

Note: The number of previous attacks refers to the number of prior Islamist attacks in the same country in all previous years. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

1.3.3 Null Results over Time

Figure S1.4: Proportion of statistically significant (black) and statistically insignificant (gray) effects on political attitudes by year of data collection



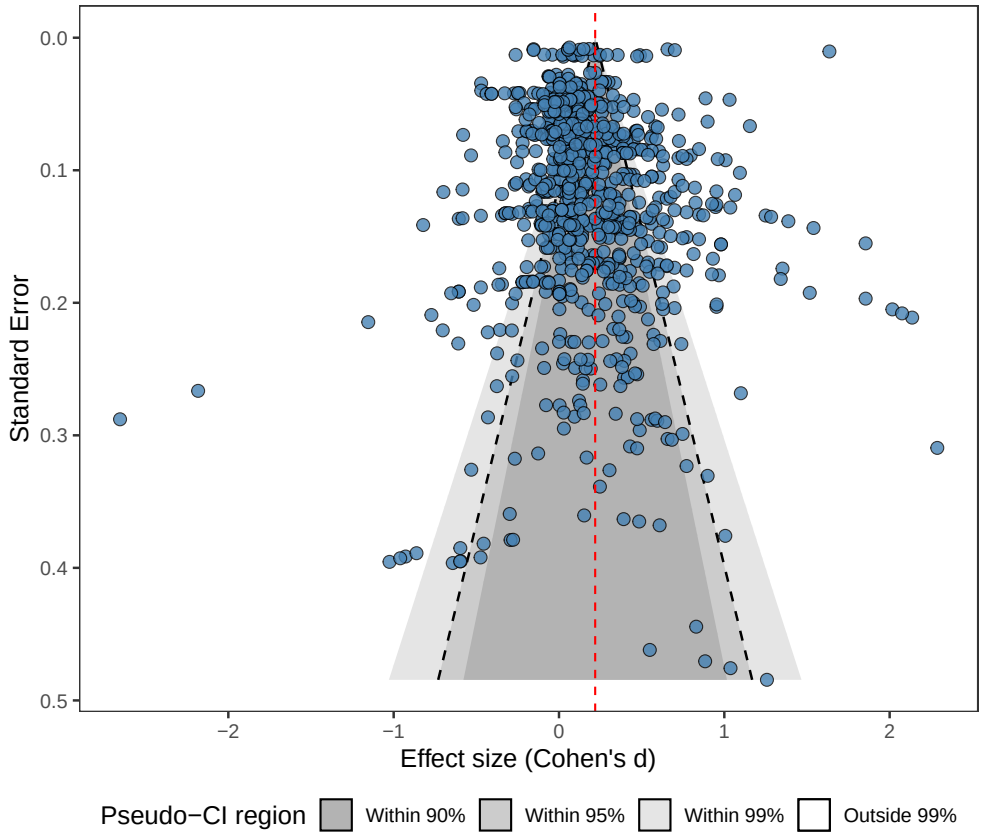
Note: The column widths is proportional to the number of effect estimates in a given year. We use the 5% threshold to determine statistical significance.

1.3.4 Publication Bias

Publication bias constitutes a common challenge in meta-analyses (Begg 1994). It occurs when studies with statistically significant results are more likely to be published than those with null findings. When present, publication bias inflates estimated effect sizes and may distort conclusions from meta-analyses. To mitigate this risk, Godefroidt (2023) included as many unpublished working papers and datasets as could be located, and we continued that practice with the update. Still, such efforts cannot exclude publication bias. Hence, we conducted several additional tests to assess potential publication bias.

Detecting and correcting publication bias is difficult, and no single gold-standard method exists (Carter et al. 2019). We therefore apply several approaches. As a first step, we generate a contour-enhanced funnel plot from an intercept-only random-effects model (Figure S1.5). Funnel plots display effect sizes plotted against against some measure of study precision (such as the standard error). Ideally, the plot should resemble a funnel shape, with more precise studies clustering around the average effect estimate and less precise studies dispersed more widely. We add the overall average (red dashed line) and shaded regions indicating conventional significance thresholds, which facilitate visual inspection of whether asymmetry is driven by statistical significance. In our case, the funnel plot shows that more precise estimates are distributed symmetrically around the mean and nearly all lie within the expected confidence regions. This suggests minimal publication bias: small-study effects are unlikely to drive our overall results.

Figure S1.5: Funnel plot



Note: Red dashed line represents overall effect.

Next, we formally test for asymmetry in the funnel plot using several versions of Egger's regression test. The logic is straightforward: if observed effect sizes are systematically related to a measure of study precision (e.g., the standard error or its inverse), the funnel plot will appear asymmetric, which can be an indication of publication bias.

Table S1.5 presents results from both classical unilevel Egger tests and multilevel extensions aligned with our main model specification. When using the standard error as predictor in unilevel and multilevel ML models, we find positive and statistically significant slopes, suggesting potential small-study effects. However, these associations attenuate and become nonsignificant once we adjust for clustering with CR2 inference, and precision-based specifications are consistently null. Taken together, the evidence for funnel asymmetry is therefore weak, and the results are broadly consistent with the visual impression from Figure S1.5.

Table S1.5: Egger's regression tests

Model	Coefficient (SE)	p-value
Unilevel (ML), slope on SE	0.44 (0.18)	0.01
Unilevel (ML), SND $\sim$ precision	-0.38 (0.32)	0.24
Multilevel (ML), slope on SE	0.76 (0.29)	0.01
Multilevel (ML), slope on precision	-0.00 (0.00)	0.39
Multilevel (ML + CR2), slope on SE	0.76 (0.43)	0.08
Multilevel (ML + CR2), slope on precision	-0.00 (0.00)	0.59

Note: Unilevel Egger tests are based on two-level random-effects models assuming independent effect sizes. The multilevel versions mirror our main model structure with random intercepts at both manuscript and effect-size level. Precision is operationalized as the inverse of the standard error ($\frac{1}{SE}$). CR2 denotes cluster-robust variance estimation by manuscript.

As a further sensitivity check, we estimate PET–PEESE models, which regress observed effects on their standard error (PET) or squared standard error (PEESE). The intercept from these models, often called the "limit estimate", reflects the expected effect size of a hypothetical infinitely precise study. The PET model tests whether a true, nonzero average effect exists after accounting for small-study effects (which are often linked to publication bias). If such significant nonzero effect is found, the PEESE model is used to provide a better-adjusted estimate by using the squared standard error as the predictor. The PET intercepts in Table S1.6 indicate a significant positive average effect, while the PEESE models yield bias-adjusted estimates that closely resemble our overall meta-analytic mean of about $d = 0.22$. Taken together, the funnel plot, Egger tests, and PET–PEESE analyses suggest that publication bias is unlikely to meaningfully distort findings from this meta-analytical dataset, though the significant slopes in the standard error specifications caution that small-study effects cannot be ruled out entirely.

Table S1.6: PET-PEESE analysis

	Three-level model		Four-level model	
	β(se)	p-value	β(se)	p-value
Panel A: PET				
Intercept	0.126 (0.046)	0.007	0.135 (0.047)	0.004
Standard Error	0.758 (0.293)	0.010	0.681 (0.304)	0.025
Panel B: PEESE				
Intercept	0.184 (0.034)	< 0.001	0.183 (0.034)	< 0.001
Variance	1.666 (0.840)	0.047	1.712 (0.873)	0.050

Note: The three-level model accounts for dependencies between effect sizes from the same manuscript as well as dependencies between manuscripts by treating effect sizes as nested within the manuscripts. The four-level model additionally treats unique studies as nested within manuscripts given that some manuscripts report multiple studies.

Finally, the tests reported above examine small-study and publication bias with respect to the overall meta-analysis. In a final set of analyses, we assess whether publication bias could specifically affect our main (i.e., the time trend) result by comparing published and unpublished studies in our sample. To do so, we add a binary indicator of publication status to the meta-analytic regressions. Table S1.7 indicates that published studies report effect sizes about 0.12 Cohen's d larger than unpublished studies, but this difference does not reach conventional significance levels ($p = 0.22$). More importantly, we also directly evaluate whether publication bias alters the estimated time trends reported in Table 2 in the main paper by including study year and its interaction with publication status. As shown in both Model 1 (raw time trend) and Model 2 (full statistical controls), the interaction term is substantively close to zero and statistically insignificant. These results increase confidence that our central findings are not driven by publication bias.

Table S1.7: Impact of publication status

	Baseline	Model 1	Model 2
Study year		−0.021	−0.003
		(0.020)	(0.020)
Publication dummy	0.123	−0.049	0.176
	(0.099)	(0.273)	(0.268)
Study year * Published		0.013	0.001
		(0.020)	(0.020)
Constant	0.093	0.324	0.017
	(0.096)	(0.270)	(0.289)
Controls			✓
N effect sizes	837	724	724
N manuscripts	170	109	109

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Overall, our publication-bias diagnostics provide little reason to doubt the robustness of our findings. Although some Egger regressions using the standard error as predictor yield positive slopes, these effects are not consistent across specifications. The contour-enhanced funnel plot shows no clear asymmetry, and PET–PEESE models produce bias-adjusted estimates that closely resemble the observed meta-analytic mean. Finally, direct comparisons of time trends in published versus unpublished studies reveal no systematic differences. Taken together, these analyses suggest that while small-study effects cannot be ruled out entirely, there is no consistent evidence that publication bias meaningfully distorts either the pooled effect size or our central conclusion: that the impact of Islamist terrorism on public opinion has remained stable over time.

1.3.5 Disaggregation by Outcome Type

Table S1.8: Annual change in reported effects by outcome category

	Conservatism	Outgroup	Rally
Time trend			
Study year	0.009	−0.001	−0.009
	(0.008)	(0.007)	(0.005)
Attack severity (*Reference*: Unknown)			
Fatalities: <10	−0.188	0.139	−0.003
	(0.261)	(0.136)	(0.153)
Fatalities: 10–100	−0.175	0.020	0.081
	(0.122)	(0.124)	(0.148)
Fatalities: >100	−0.048	0.042	0.219
	(0.106)	(0.118)	(0.137)
Study design (*Reference*: Correlational studies)			
Randomized experiment	−0.194*	−0.151	0.127
	(0.092)	(0.095)	(0.133)
Natural experiment	0.014	−0.226*	0.070
	(0.080)	(0.089)	(0.091)
Sample quality (*Reference*: Non-probability sample)			
Probability sample	−0.054	0.213**	−0.003
	(0.072)	(0.073)	(0.099)
Country context (*Reference*: Other countries)			
U.S. study	0.129	0.132	0.160
	(0.079)	(0.090)	(0.086)
Constant	0.190	0.174	0.035
	(0.145)	(0.152)	(0.170)
N effect sizes	243	305	176
N manuscripts	55	66	39

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

1.3.6 Between-Study Moderation of Temporal Patterns

Our results indicate that repeated exposure has not changed the average attitudinal response to Islamist terrorism. This stability could arise for two reasons. First, individual-level responses may remain stable across attacks. Second, repeated exposure may produce divergent reactions across segments of the population, with smaller effects in some groups and larger effects in others, yielding no net change at the aggregate level. If such offsetting dynamics were present, we would expect to observe systematically different time trends in studies conducted among different types of samples. We assess this possibility by estimating interaction meta-regressions that combine our indicators of repeated exposure with several study-level measures of sample composition (Table S1.9). Although many studies report only a limited number of sample descriptives, we are able to examine four characteristics: mean age, share female, student versus non-student samples, and U.S. versus non-U.S. samples. Across all specifications, we find no evidence that the temporal pattern of estimated effects differs by sample type. This suggests that the aggregate stability we observe is unlikely to mask subgroup-specific shifts and is instead consistent with broadly stable attitudinal responses at the individual level.

Table S1.9: Investigating heterogeneous responses to repeated exposure to Islamist terrorism

	(1)	(2)	(3)	(4)	(5)
Study year	−0.051				
	(0.033)				
Number of previous Islamist terrorist attacks in:					
Same country in previous 5 years		−0.095*			
		(0.048)			
All Western countries in previous 5 years			−0.007		
			(0.006)		
Same country in all previous years				−0.018	
				(0.024)	
All Western countries in all previous years					−0.005
					(0.004)
Percentage of female respondents	−0.002	−0.002	−0.001	0.001	−0.003
	(0.003)	(0.002)	(0.002)	(0.002)	(0.005)
Mean age of respondents	−0.012	−0.008	−0.008	−0.002	−0.015
	(0.009)	(0.006)	(0.007)	(0.006)	(0.011)
Student sample	−0.201	−0.223	−0.214	−0.272	−0.172
	(0.186)	(0.132)	(0.146)	(0.153)	(0.267)
U.S. study	0.006	0.154	0.090	0.163	−0.008
	(0.125)	(0.091)	(0.089)	(0.128)	(0.175)
Study year * Female share	0.000				
	(0.000)				
Year of study * Mean age	0.001				
	(0.001)				
Study year * Student population	−0.006				
	(0.016)				
Study year * US study	0.012				
	(0.011)				
Past attacks * Female share		0.001	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
Past attacks * Mean age		0.001	0.000	0.000	0.000
		(0.001)	(0.000)	(0.000)	(0.000)
Past attacks * Student population		−0.004	−0.001	0.002	−0.001
		(0.025)	(0.003)	(0.012)	(0.002)
Past attacks * U.S. study		0.011	0.001	0.003	0.001
		(0.013)	(0.003)	(0.006)	(0.002)
Constant	0.978	0.792	0.752	0.434	1.162
	(0.436)	(0.307)	(0.333)	(0.342)	(0.571)
Controls	✓	✓	✓	✓	✓
N effect sizes	512	512	512	512	512
N manuscripts	79	79	79	79	79

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

2 Study 2

2.1 Application Design

WhoGetsMyVoteUK constitutes what is frequently referred to as a Voting Advice Application (VAA) in the academic literature (Garzia and Marschall 2019). VAAs are online tools that provide their users with information on the extent to which their policy views match those of parties or candidates. VAAs have proliferated across advanced democracies in recent years, especially in Europe, where they play an increasingly integral part in election campaigns (Germann and Gemenis 2019). Both the 2017 and 2019 versions of *WhoGetsMyVoteUK* were developed by international teams of researchers including political and computer scientists. The team responsible for the 2017 version was co-led led by Prof. Tim Bale (Queen Mary University of London) and Dr. Jonathan Wheatley (Oxford Brookes University). The team responsible for the 2019 version was led by Dr. Jonathan Wheatley (Oxford Brookes University). Two of the authors of this paper (Germann (only in 2019) and Mendez (both years)) participated in the development of *WhoGetsMyVoteUK*.

The design of both the 2017 and 2019 versions of *WhoGetsMyVoteUK* was similar to other VAAs previously deployed in the UK and in other countries. In both years, voters could access the tool by navigating to http://www.whogetsmyvoteuk.com on the web. Upon accessing the tools, users were presented with a landing page, which featured a welcome message explaining the basic purpose of the application (see Figure S2.6a). In addition, the landing page included information on data protection and data privacy. Voters could only proceed from the landing page if they provided their informed consent. Next, users were asked to choose their region (see Figure S2.6b). In both years, slightly different versions were prepared for England, Scotland, and Wales. The main difference was that the Scottish and Welsh versions included additional regional parties (i.e., the Scottish National Party and Plaid Cymru, respectively). In 2017, a fourth version was created for Northern Ireland. Relatively few voters made use of the Northern Irish version and the questionnaire is largely incompatible with the ones used in the other regions. Therefore, we do not include data from Northern Ireland and focus on Britain (England, Scotland, and Wales).

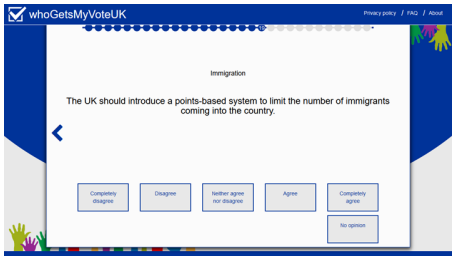
After selecting their region, users were directed to the main part of the application. Here, users were asked to indicate their preferences on a total of 30 policy statements (see Figure S2.6c). Policy statements were carefully selected to reflect salient political issues across a number of policy areas (e.g., economy, immigration, security, and climate change). Different policy statements were used in 2017 and 2019. Response options always included 'Completely agree', 'Agree', 'Neither agree nor disagree', 'Disagree', and 'Completely disagree'. Respondents could also select 'No opinion'. The purpose of the 30 policy statements was to establish users' levels of agreement with the various political parties on political issues. In addition to the policy statements, the main part included several supplementary questions on demographics, general political attitudes, identity, and past voting behavior (see Figure S2.6d for an example).

Finally, after completion of the questionnaire, the application calculated the match between users' and parties' policy views. Parties' policy positions were measured using iterative expert surveys (Gemenis 2015). By default, users were then presented with a bar chart, which ranks the parties from worst to best-matching (see Figure S2.6e). Users could also access alternative forms of results display including a two-dimensional political map (see Figure S2.6f).

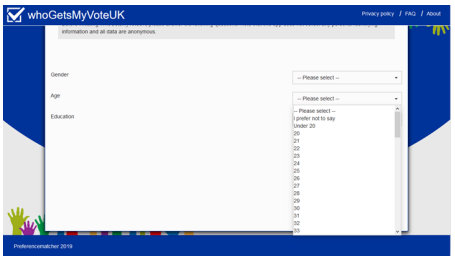
Figure S2.6: Example screenshots

(a) Landing page

(b) Region selection

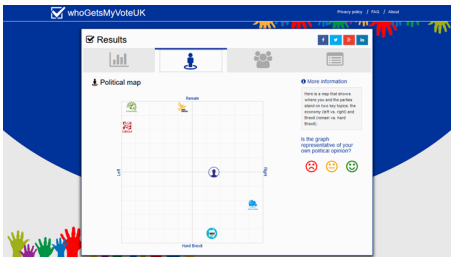


(c) Example policy statement



(d) Supplementary questions

(e) Default results screen



(f) Alternative results screen

2.2 Data Cleaning

VAAs are freely available online. Hence, there is a risk that the same individuals use VAAs more than once. It is also possible that people use VAAs who are not eligible to vote. Similar to other kinds of surveys, some users may furthermore exhibit satisficing behavior. As noted in the paper, we therefore exclude certain observations from analyses reported in the paper, including: (1) repeated attempts from the same computer (which likely represent repeated attempts by the same individuals); (2) users who indicated that they are not eligible to vote in the UK; and (3) speeders who rushed through the tool in less than one-third of average time. Table S2.10 provides an overview.

Table S2.10: Data cleaning

	All obs.	Repeated	Ineligible	Speeders	Dropped	Remaining
2017	82280	3387	2311	446	5814	76466
	(100%)	(4%)	(3%)	(0.5%)	(7%)	(93%)
2019	49442	1481	469	455	2272	47170
	(100%)	(3%)	(1%)	(1%)	(5%)	(95%)

2.3 Sample Descriptives

Figure S2.7: Daily sample sizes

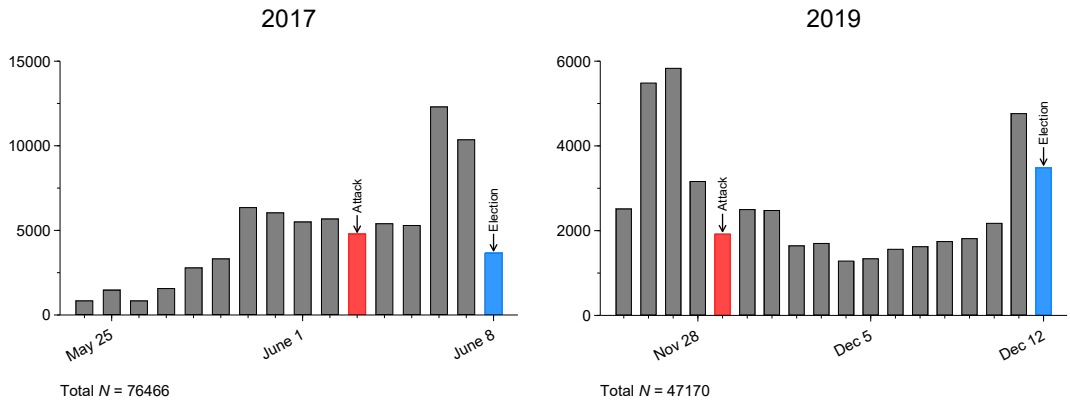


Table S2.11: Demographics including population reference figures

		2017					2019				
		Full sample	**± 1 day**	**± 2 days**	**± 3 days**	**Population**	**Full sample**	**± 1 day**	**± 2 days**	**± 3 days**	**Population**
Region	England	67%	67%	68%	68%	86%	84%	86%	88%	88%	86%
	Scotland	23%	24%	23%	23%	9%	12%	9%	8%	8%	9%
	Wales	10%	9%	9%	9%	5%	4%	4%	4%	4%	5%
Gender	Female	51%	52%	51%	53%	51%	56%	54%	55%	56%	51%
	Male	48%	48%	49%	47%	49%	44%	45%	44%	44%	49%
Age	<25	15%	11%	12%	12%	11%	16%	19%	19%	18%	11%
	25-34	30%	34%	32%	30%	16%	28%	32%	32%	31%	16%
	35-44	24%	24%	24%	25%	15%	24%	24%	24%	24%	15%
	45-54	18%	18%	17%	19%	18%	17%	14%	14%	14%	18%
	55-64	10%	9%	10%	10%	16%	11%	7%	7%	8%	16%
	65+	5%	4%	4%	5%	24%	5%	4%	4%	4%	24%
Education	Degree	55%	57%	57%	54%	35%	63%	65%	65%	65%	35%
	No degree	45%	43%	43%	46%	65%	37%	35%	35%	35%	65%
Previous general election	Conservatives	27%	29%	28%	29%	24%	26%	23%	25%	25%	29%
	Labour	25%	25%	25%	25%	20%	33%	36%	35%	34%	27%
	Liberal Democrats	10%	10%	10%	10%	5%	13%	13%	13%	14%	5%
	Did not vote	7%	6%	6%	6%	34%	8%	8%	8%	8%	31%
Brexit referendum	Leave	32%	32%	32%	33%	37%	24%	23%	23%	23%	37%
	Remain	61%	62%	62%	61%	35%	61%	60%	61%	62%	35%
	Did not vote	7%	6%	6%	6%	28%	7%	8%	8%	8%	28%
	Was not eligible						7%	8%	8%	8%	

Note: The Brexit question included an additional response option in 2019 ("I was not eligible to vote"). The demographic reference figures (region, gender, age, and education) represent the proportions of Britain's voting-eligible population. Election and referendum outcomes represent the proportions of Britain's registered voters' population. Data for the demographic reference figures were calculated based on the 2017 Annual Population Survey combined with additional data from Understanding Society about the education levels of the oldest age groups (Moon, Green, and Bogdan 2017). Data for election and referendum outcomes are based on the official results. Some of the percentages do not add up to 100% due to rounding.

2.4 Covariates

Table S2.12 provides details on the measurement of control variables. Note that control variables are measured after the attacks in the treatment group. Most of our covariates are nevertheless highly unlikely to induce post-treatment bias (basic demographics and past events). The biggest risk emerges in case of individuals' left-right position. However, most extant theories do not expect that Islamist terrorism affects ideological predisposition defined as broadly, and most studies do not find effects of terrorism on people's left-right position (e.g., Lambert et al. 2010; Lambert, Eadeh, and Hanson 2019; Nail and McGregor 2009; Eadeh and Chang 2020). At the same time, left-right position is likely to be strongly related to political attitudes and in particular positions on security and immigration policy. Adjusting for left-right position therefore constitutes a conservative approach that minimizes the risk of bias due to imbalances between the pre- and post-attack samples. In controlling for left-right position, we follow several recent studies of Islamist terrorism (e.g., Brouard, Vasilopoulos, and Foucault 2018; Castanho Silva 2018; Hetherington and Suhay 2011).

Table S2.12: Measurement of covariates

Covariate	**Categories**
Gender	Female, male, other
Age	<25, 25-34, 35-44, 45-54, 55-64, 65+
Education	Below secondary, secondary, further, technical/vocational, university, postgraduate
Political interest	2017: not at all, not very, fairly, very; 2019: not much, some, a good deal
Left-right	0 (left) – 10 (right)
Previous general election	Conservatives, Labour, Liberal Democrats, Greens, UKIP, SNP, Plaid Cymru, Other, did not vote, ineligible, (only in 2019:) spoiled
Brexit referendum	Leave, remain, did not vote, (only in 2019:) ineligible
Region	England, Scotland, Wales

2.5 Outcomes

Table S2.13 contains the exact wordings of our outcome questions. Tables S2.14–S2.16 provide information on the unidimensionality (Gerbing and Anderson 1988) and reliability (Lord and Novick 1968) of all multi-item scales. As psychometric analyses require multi-item scales, we cannot establish the psychometric properties of outcome variables that are measured with a single item.

We assess unidimensionality using Mokken scale analysis (MSA) (Mokken 1971; van der Ark 2007; Schuur 2003). A set of items can be considered a unidimensional Mokken scale if 1) the overall Loevinger's H-coefficient is ≥ 0.30; 2) all item-specific H_i-coefficients are ≥ 0.30; and 3) all *crit*-values are < 80 (Schuur 2003). Mokken scales with overall H-scores ≥ 0.30 are considered weak, ≥ 0.40 moderate, and ≥ 0.50 strong. For the reliability assessment, we draw on Cronbach's α. $\alpha \geq 0.60$ is generally seen as indicating a minimally acceptable level of reliability, though $\alpha \geq 0.70$ is preferable. As becomes evident, all multi-item outcome variables constitute strong Mokken scales and have acceptable scale reliability.

Table S2.13: Wordings of outcome questions

2017	2019
Tough security	
The security services should be allowed to monitor people's Internet use.	1. UK citizens who travelled to Syria to support ISIS should lose their British citizenship. 2. The police should be given more powers to stop and search suspected criminals.
Anti-immigration	
1. The UK should continue to allow free movement of people to and from the European Union.*	1. Free movement of people between the UK and the rest of the European Union should end.
2. The UK should introduce quotas to limit the number of immigrants coming into the country.	2. The UK should introduce a points-based system to limit the number of immigrants coming into the country.
3. Britain should welcome a larger number of migrants from war-torn countries.*	3. The UK should accept more refugees from conflict zones.*
	4. Immigration undermines the cultural values of the UK.
British identity	
To what extent do you consider yourself as any of the following, where a 0 means "this does not describe me at all" and a 10 means "this describes me perfectly"? – British	To what extent do you consider yourself as any of the following, where a 0 means "this does not describe me at all" and a 10 means "this describes me perfectly"? – British
English identity	
To what extent do you consider yourself as any of the following, where a 0 means "this does not describe me at all" and a 10 means "this describes me perfectly"? – English	To what extent do you consider yourself as any of the following, where a 0 means "this does not describe me at all" and a 10 means "this describes me perfectly"? – English

Note: Response options for the security and immigration questions included 'Completely agree', 'Agree', 'Neither agree nor disagree', 'Disagree', 'Completely disagree', and 'No opinion'. Items indicated with a star (*) are reversed.

Table S2.14: Security scale [2019]

Item text	***Hi***	***crit***
1. UK citizens who travelled to Syria to support ISIS should lose their British citizenship.	0.53	0
2. The police should be given more powers to stop and search suspected criminals.	0.53	0
H		0.53
α		0.65
N		46000

Table S2.15: Immigration scale [2017]

Item text	***Hi***	***crit***
1. The UK should continue to allow free movement of people to and from the European Union.*	0.69	0
2. The UK should introduce quotas to limit the number of immigrants coming into the country.	0.71	0
3. Britain should welcome a larger number of migrants from war-torn countries.*	0.68	0
H		0.69
α		0.83
N		75308

Note: Items indicated with a star (*) are reversed.

Table S2.16: Immigration scale [2019]

Item text	***Hi***	***crit***
1. Free movement of people between the UK and the rest of the European Union should end.	0.60	0
2. The UK should introduce a points-based system to limit the number of immigrants coming into the country.	0.67	0
3. Immigration undermines the cultural values of the UK.	0.62	0
4. The UK should accept more refugees from conflict zones.*	0.60	0
H		0.62
α		0.84
N		45048

Note: Items indicated with a star (*) are reversed.

2.6 Identifying Assumptions

This section reports the results of several tests we conducted to assess the plausibility of our causal identification assumption.

2.6.1 Media Analysis

As noted in the paper, we investigate the content of top headline news stories around the time of the London Bridge attacks attacks. We focus on the 10 British newspapers with the highest daily reach in 2019, including both print and digital content (cf. PAMCo 2020): The Sun/The Sun on Sunday; Daily Mail/The Mail on Sunday; Daily Mirror/Sunday Mirror; The Guardian/Observer; Metro; Daily Express/Sunday Express; Daily Telegraph/Sunday Telegraph; Independent; Evening Standard; The Times/The Sunday Times. The key results are shown in Figure S2.8 while more detailed information on the content of headline news stories can be found in Table S2.17. As it turns out, almost all newspapers led with the attacks on the first, second, and third day after both attacks. This makes it likely that most of the subjects in the treatment groups were exposed to news about the terrorist attacks.

Figure S2.8 furthermore shows that British newspapers led with a potpourri of different stories in the run-up to the attacks (e.g., stories related to the UK's national health service, proposed tax hikes, and sex scandals), most of which are highly unlikely to affect the kind of political attitudes we are interested in. Among the small number of possible exceptions range reporting on Brexit, the Manchester Arena bombing, and perhaps allegations of anti-semitism against the Labour Party.

However, reporting on these topics was of a more continuous nature and is therefore unlikely to be responsible for the short-term effects we observe. Overall, the content of front page headlines in the pre-treatment phase increases confidence that the effects reported in the paper can be attributed to the London Bridge attacks and not another, more or less simultaneously occurring event.

Figure S2.8: Top headline news stories in 10 leading British newspapers around the time of the London Bridge attacks

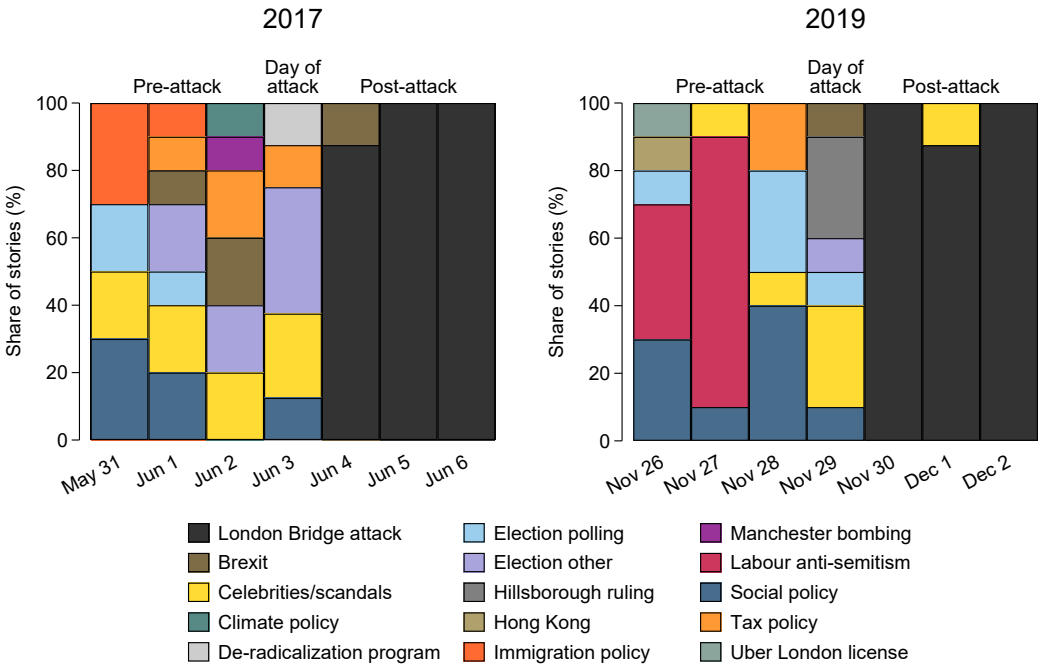


Table S2.17: Summaries of the content of top headline news stories around the time of the London Bridge attacks

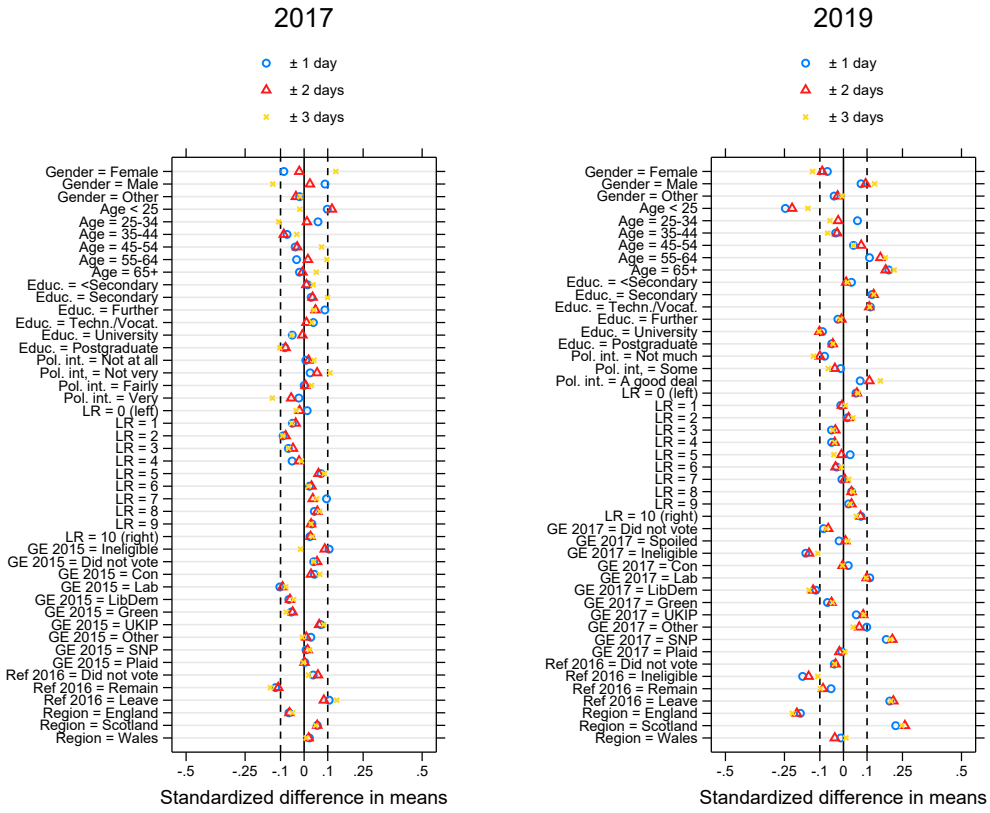
	2017		2019	
Theme	**#**	**Content description**	**#**	**Content description**
London Bridge attack	27	Coverage of the 2017 London Bridge attack, its perpetrators and victims, weaknesses in the UK's counter-terrorism strategy which could have facilitated the attack, and the risk of further attacks	25	Coverage of the 2019 London Bridge attack, its perpetrator and victims, weaknesses in the UK's counter-terrorism stategy which could have facilitated the attack, potential policy responses, and the risk of further attacks
Brexit	4	Main parties' plans for implementing Brexit and their ability to get a good deal for UK in (then) ongoing negotiations with EU about Brexit	1	Coverage suggesting Labour is misrepresenting its plans regarding renegotiating the UK-EU Brexit deal
Celebrities/scandals	8	Various scandals including an expense scandal involving a Conservative MP, a sex scandal involving a TV star, and surgeon who was sentenced for performing unnecessary breast cancer operations	6	Coverage of a lottery winner, allegations that Prince Andrew is engaging in illit financial activities, an alleged sex scandal, and several other scandals
Climate policy	1	US withdrawal from the Paris Accords	0	–
De-radicalization program	1	Allegations that Islamic groups are undermining Prevent, the UK's anti-radicalisation program	0	–
Election polling	3	Election polls which indicate increased support for the opposition and a narrowing race	5	Various reports on election forecasts
Election other	7	Coverage of various election-related themes, including the possibility of a coalition between Labour and the Scottish National Party and allegations that Labour uses fake web accounts to help boost its vote	1	Conservative allegations that TV station is biased against them
Hillsborough ruling	0	–	3	Coverage of acquittal of David Druckenfield, who was the police officer in command at the time of the Hillsborough disaster, a fatal human crush during a football match in Sheffield in 1986, and had been accused of gross negligence manslaughter
Hong Kong	0	–	1	Coverage of pro-democracy protests in Hong Kong
Immigration policy	4	Right-wing media suggesting that migration is increasing and alleging that Labour has plans to increase migration even further	0	–
Manchester bombing	1	Continued coverage of victims of the Manchester Arena bombing	0	–
Labour anti-semitism	0	–	12	Coverage of allegations of anti-semitism within the Labour party and the unwillingness of its leader, Jeremy Corbyn, to apologize for it during a TV interview
Social policy	6	Coverage related to the national health service (NHS) and the main parties' plans for health care, child care, free school lunches, and housing policy	9	Coverage of the main parties' plans for health care, elderly care, and other social policies
Tax policy	4	Main parties' plans regarding taxes, including income and inheritance tax	2	Main parties' plans for tax rises
Uber London license	0	–	1	Uber loses taxi license in London over safety concerns

Note: # indicates the number of headline news stories related to the different themes over the 7-day periods studied in the paper.

2.6.2 Covariate Balance

Figure S2.9 shows the level of balance between treatment (i.e., post-attack) and control (i.e., pre-attack) groups across several possible confounders. Balance is evaluated using the standardized difference in means, which measures the difference in means between the treatment and control group divided by the pooled standard deviation (Stuart 2010). Unlike other balance measures, such as the t-test, the standardized difference is not influenced by sample size. A zero standardized difference indicates identical means; negative/positive scores indicate a lower/higher mean in the treatment group. While there is no universally agreed upon threshold value after which standardized differences are seen as important, a standardized difference less than $|\ 0.10\ |$ is often seen as negligible (Austin 2011).

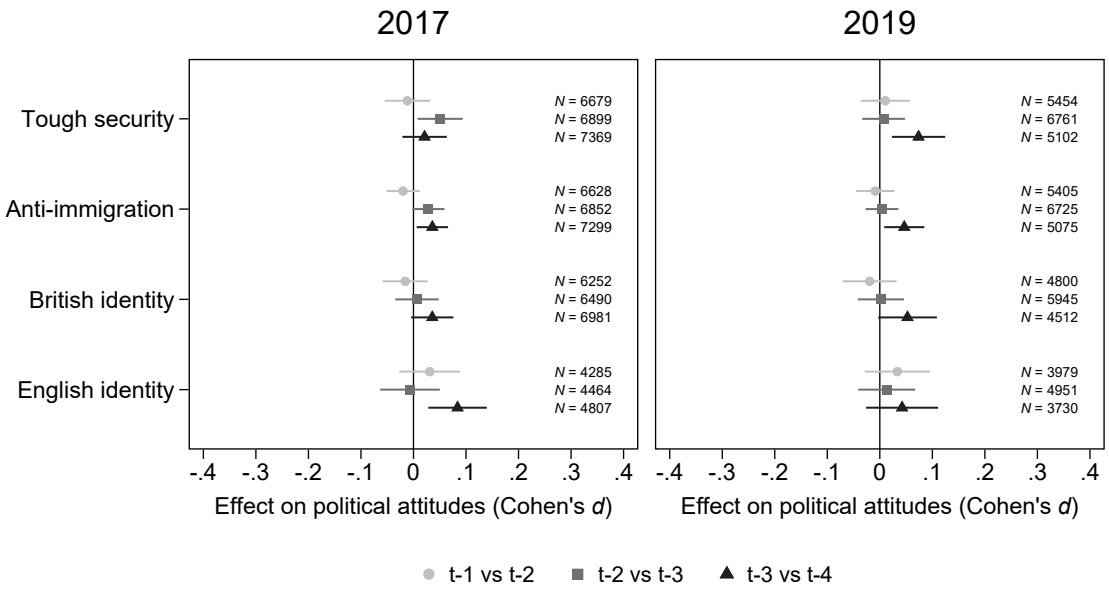
Figure S2.9: Pre/post covariate balance



2.6.3 Placebo Checks

First, we conduct placebo treatment tests in which we compare people who accessed *WhoGetsMyVoteUK* on a specific day during the pre-treatment period with people who accessed *WhoGetsMyVoteUK* the day before. We control for the same covariates as in the paper and use linear regression. Reassuringly, we find that the placebo treatment effects are, in almost all cases, substantively close to zero and lack statistical significance despite the large sample sizes ranging from 3,700 to more than 7,000 (see Figure S2.10). This increases confidence that the effects we report are not due to an unrelated time trend or another event that occurred during the pre-treatment period. It also improves confidence that there are no unobserved imbalances between the control and treatment groups.

Figure S2.10: Placebo treatment tests



Note: The spikes represent 95% confidence intervals.

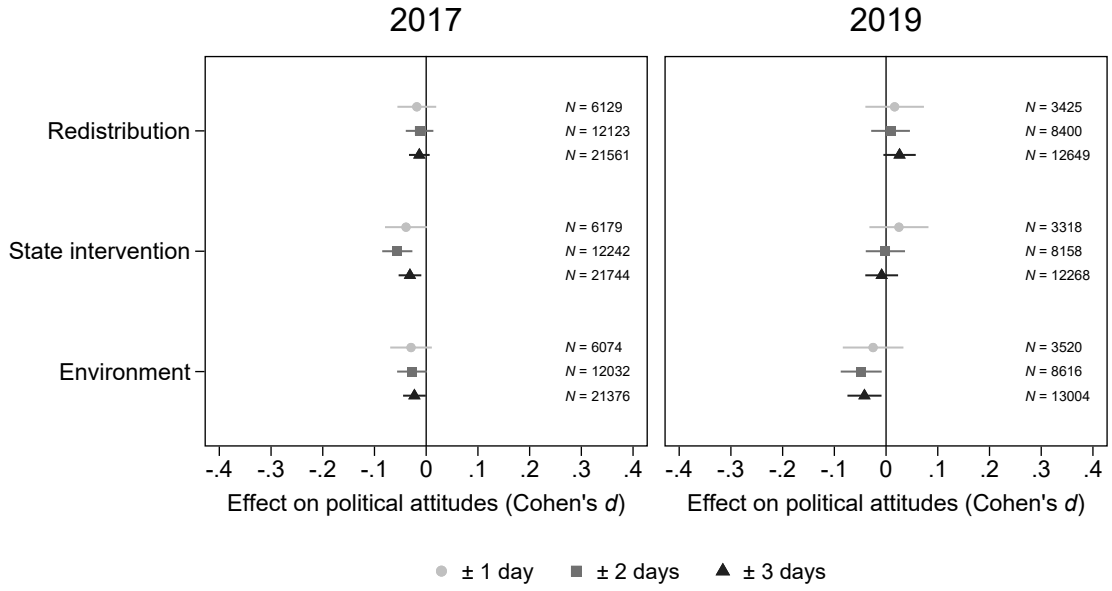
Second, we conduct placebo outcome tests in which we estimate the effects of the London Bridge attacks on support for economic redistribution, support for state interventions in the economy, and support for environmental protection. Since most extant theories would not expect Islamist terrorism to affect economic and environmental policy preferences, any significant placebo effects would undermine the plausibility of our causal estimates.

We measure placebo outcomes using summated rating scales consisting of two to four of *WhoGetsMyVoteUK*'s policy statements. Question wordings and information on psychometric performance can be found in Tables S2.18–S2.23. Response options always included 'Completely agree', 'Agree', 'Neither agree nor disagree', 'Disagree', and 'Completely disagree'. Respondents could also select 'No opinion', which we treat as missing data. We assess scales' unidimensionality using Mokken scale analysis (MSA) and scale reliability using Cronbach's α. A discussion of those methods and thresholds which need to be satisfied can be found in SI Appendix §2.5.

Analogously to the main analysis, we estimate placebo effects using linear regressions including the same set of controls and exclude the day of the attacks themselves. Figure S2.11 shows that all placebo effects are substantively close to zero. Most also lack statistical significance, despite the large sample sizes of up to 22,000 observations.

Finally, it is worth noting that while most extant theories do not expect Islamist terrorist attacks to affect economic and environmental preferences (Lambert, Eadeh, and Hanson 2019), *Conservatism as Motivated Social Cognition Theory* constitutes a notable exception since it assumes that terrorism, including Islamist terrorism, affects a very broad set of attitudes even including economic and environmental preferences (Jost et al. 2003). However, empirical evidence for effects of Islamist terrorism on economic and environmental policy is weak (e.g., Lambert et al. 2010; Lambert, Eadeh, and Hanson 2019; Nail and McGregor 2009; Eadeh and Chang 2020). The results of our placebo outcome tests can be read as further evidence against such a general conservative shift.

Figure S2.11: Placebo outcome tests



Note: The spikes represent 95% confidence intervals.

Table S2.18: Economic redistribution scale [2017]

Item text	***Hi***	***crit***
1. Income tax should be increased for higher earners.	0.48	0
2. Unemployed young people should not be automatically entitled to housing benefits.*	0.34	0
3. Corporation tax on businesses should be reduced.*	0.41	0
4. The government should take active measures to reduce the pay gap between the highest and lowest earners in the public sector.	0.44	0
H		0.42
α		0.70
N		73186

Note: Items indicated with a star (*) are reversed.

Table S2.19: Economic redistribution scale [2019]

Item text	***Hi***	***crit***
1. Income tax should be reduced.*	0.38	0
2. Inheritance tax should be reduced.*	0.44	0
3. The state should actively redistribute from the richest people to the poorest.	0.25	128
H		0.36
α		0.60
N		44553

Note: Item 3 violates the assumptions of the Mokken measurement model. However, the extent of the violations is moderate and the item's substantive content clearly fits the target construct. Therefore, we nevertheless include item 3. Items indicated with a star (*) are reversed.

Table S2.20: State intervention in economy scale [2017]

Item text	***Hi***	***crit***
1. The railways should be under public ownership.	0.53	0
2. Private sector involvement in the NHS should be reduced.	0.53	0
H		0.53
α		0.65
N		73931

Table S2.21: State intervention in economy scale [2019]

Item text	***Hi***	***crit***
1. Large companies should be forced to give company shares to their workers.	0.41	0
2. Rail services should be renationalised and run by the state, not private companies.	0.41	0
3. Zero hours contracts should be banned.	0.37	0
4. Upper limits should be placed on rents charged by private landlords on houses and flats.	0.38	0
H		0.39
α		0.69
N		42999

Table S2.22: Environmental protection scale [2017]

Item text	***Hi***	***crit***
1. The government should allow the extraction of underground shale gas (fracking).	0.43	0
2. The UK should continue to meet the EU's green energy targets.	0.43	0
H		0.43
α		0.53
N		72521

Note: Unfortunately, there are no other items related to environmental protection in the 2017 questionnaire which we could add to the scale to increase scale reliability.

Table S2.23: Environmental protection scale [2019]

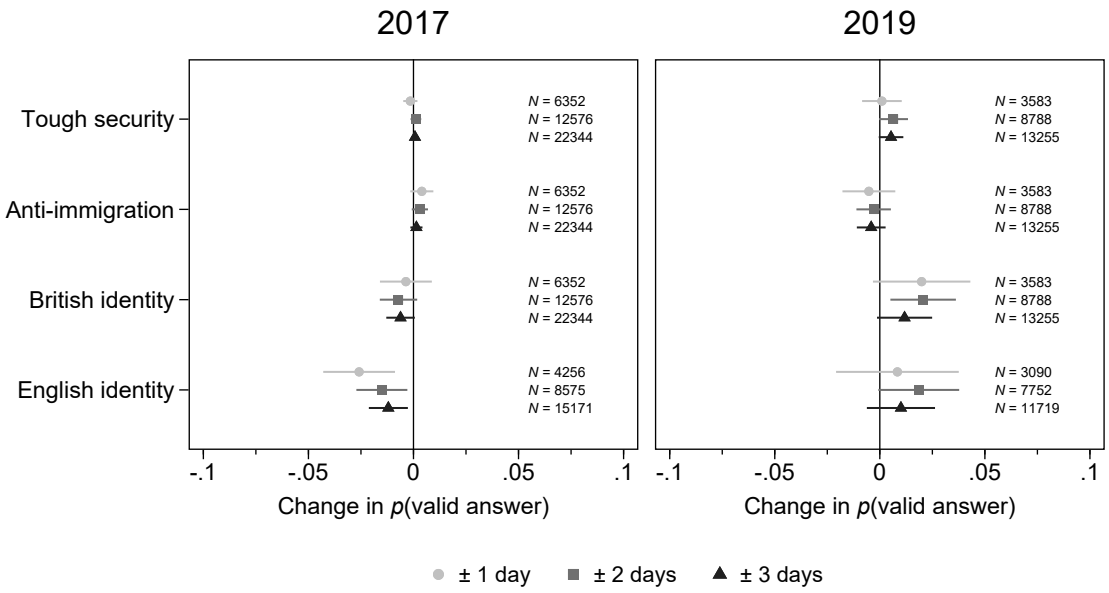
Item text	***Hi***	***crit***
1. The government should introduce a carbon tax to cut greenhouse emissions.	0.49	0
2. All new homes should be built to zero-carbon standards during the next parliament.	0.49	0
H		0.49
α		0.62
N		45688

2.6.4 Attrition

Figure S2.12 assesses whether VAA users became more or less likely to answer our outcome questions after the London Bridge attacks. Results are based on linear regressions including the same set of control variables. As in the paper, we exclude the day of the attacks themselves. We find little evidence for changes in valid answers. We do observe a small but consistently statistically

significant change in one case: VAA users became somewhat less likely to validly answer the question on English identity in 2017. However, a small difference of 1–2 percentage points is unlikely to account for the results reported in the paper, and the effect does not replicate in 2019.

Figure S2.12: Assessing attrition



Note: The spikes represent 95% confidence intervals.

2.7 Generalizability and Comparability

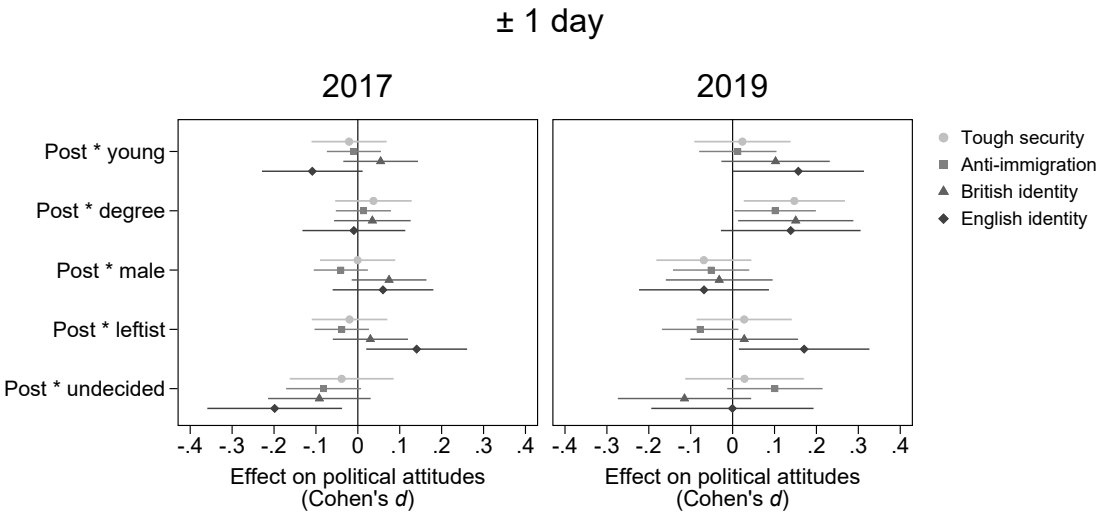
This section reports the results of several tests we conducted to assess threats to the generalizability and comparability of our estimates.

2.7.1 Sub-Group Analysis

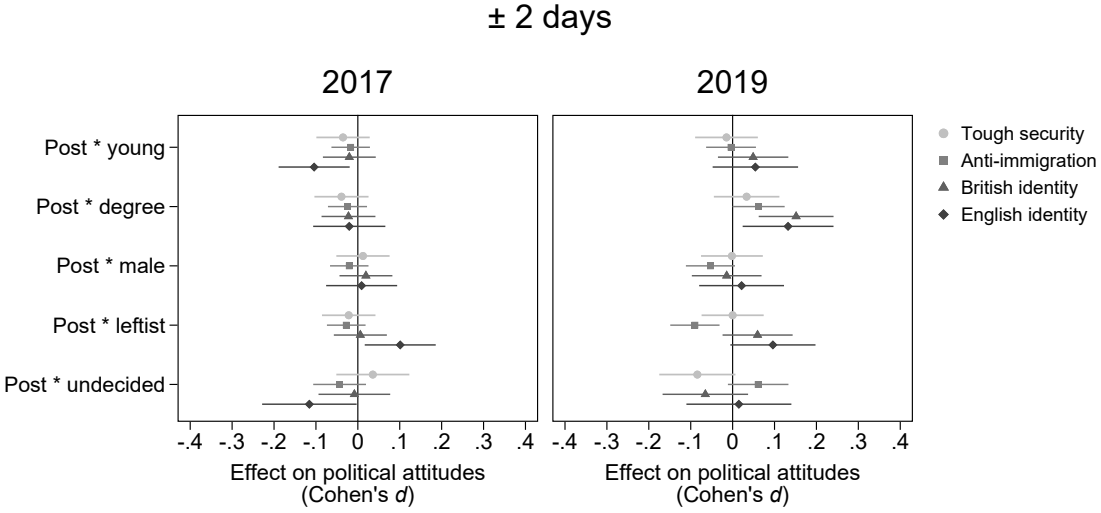
To assess effects heterogeneity, we estimate a series of multiplicative interaction models in which we interact the treatment group indicator with several possible sources of causal heterogeneity: young age (<35), high educational attainment (university degree), male gender, left-wing political orientation (0–4 on a 0–10 a left-right scale), and being undecided whom to vote for in the upcoming election.

Figure S2.13 reports the key result from those models—the multiplicative interaction terms. We find that despite the large sample sizes, most interaction terms are substantively close to zero and cannot statistically be distinguished from zero. While there are some interaction terms that do reach statistical significance, the number of statistically significant effects is small and significant effects tend not to replicable between variables and years. A partial exception emerges for general political orientation: in some of the models, left-wing voters are estimated to have experienced somewhat stronger increases in English identification after the attacks. While this supports the reactive liberals' hypothesis (Nail et al. 2009), it is worth noting that the result does not replicate for any of the other outcome variables. Overall, we find remarkably little evidence for effect heterogeneity.

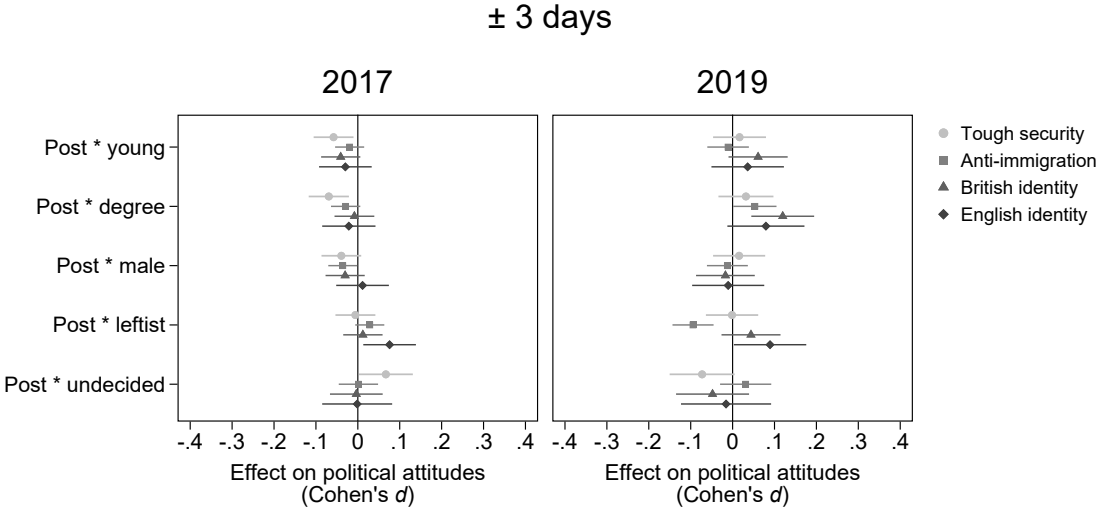
Figure S2.13: Sub-group analysis



Note: The spikes represent 95% confidence intervals.



Note: The spikes represent 95% confidence intervals.

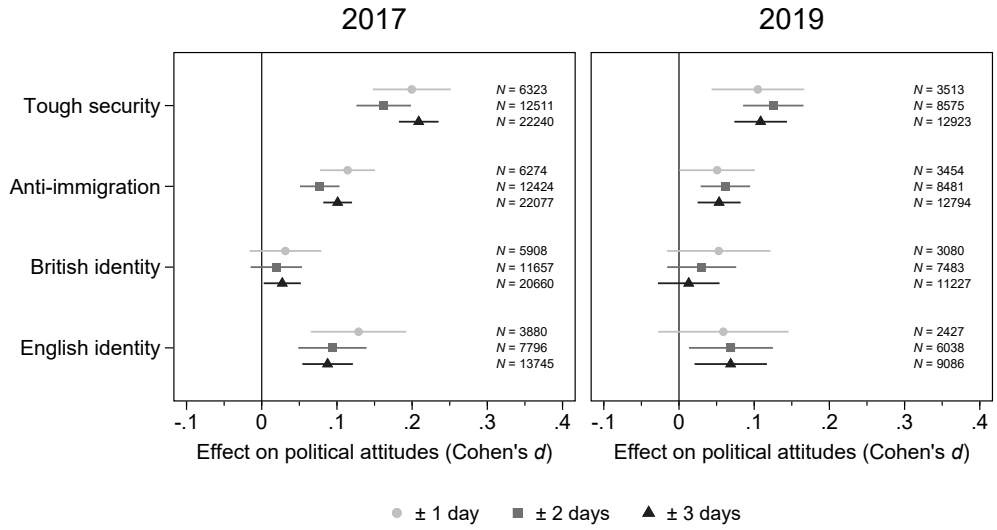


Note: The spikes represent 95% confidence intervals.

2.7.2 Weighted Regressions

Figure S2.14 reports the results when survey weights are included that adjust the sample to the following census targets: region of residence, age, and gender. The specification is otherwise analogous to the paper.

Figure S2.14: Results of weighted regressions

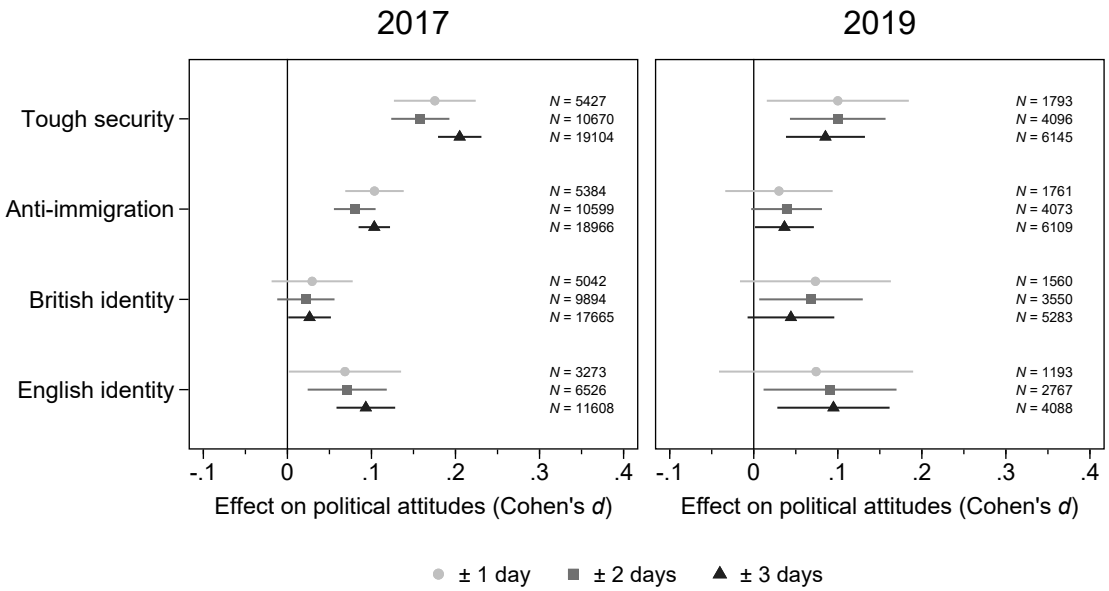


Note: The spikes represent 95% confidence intervals.

2.7.3 Facebook Referrals

Figure S2.15 reports the results when only VAA users are included that have been referred by Facebook. A majority of traffic came from Facebook in both 2017 and 2019. Focusing on Facebook referrals only increases sample comparability. The specification is otherwise analogous to the paper.

Figure S2.15: Results when only Facebook referrals are included

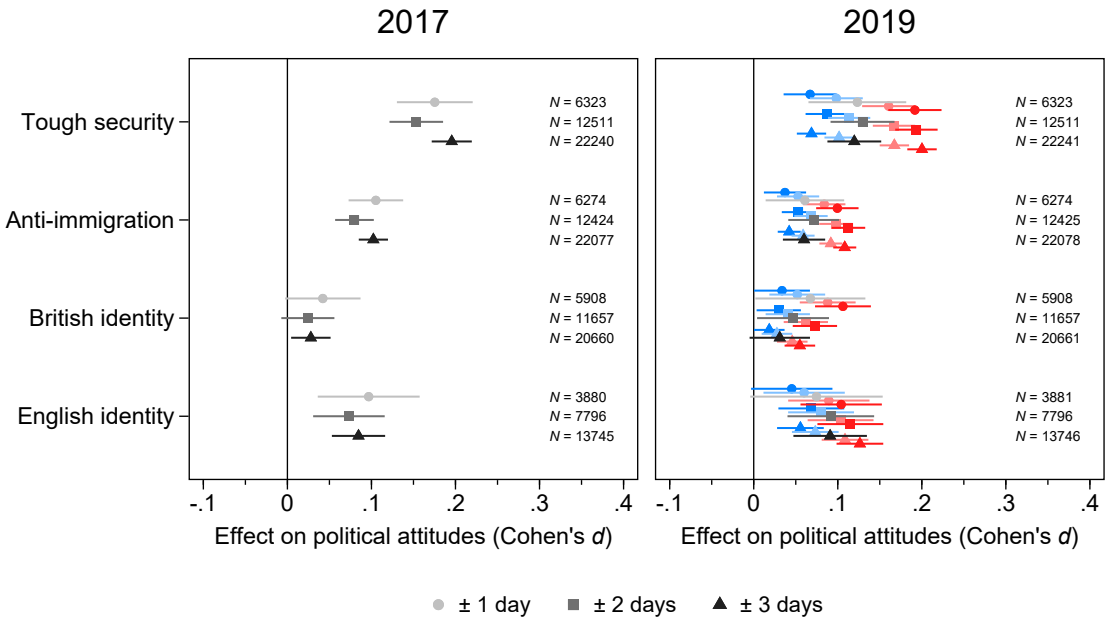


Note: The spikes represent 95% confidence intervals.

2.7.4 Sensitivity Analysis

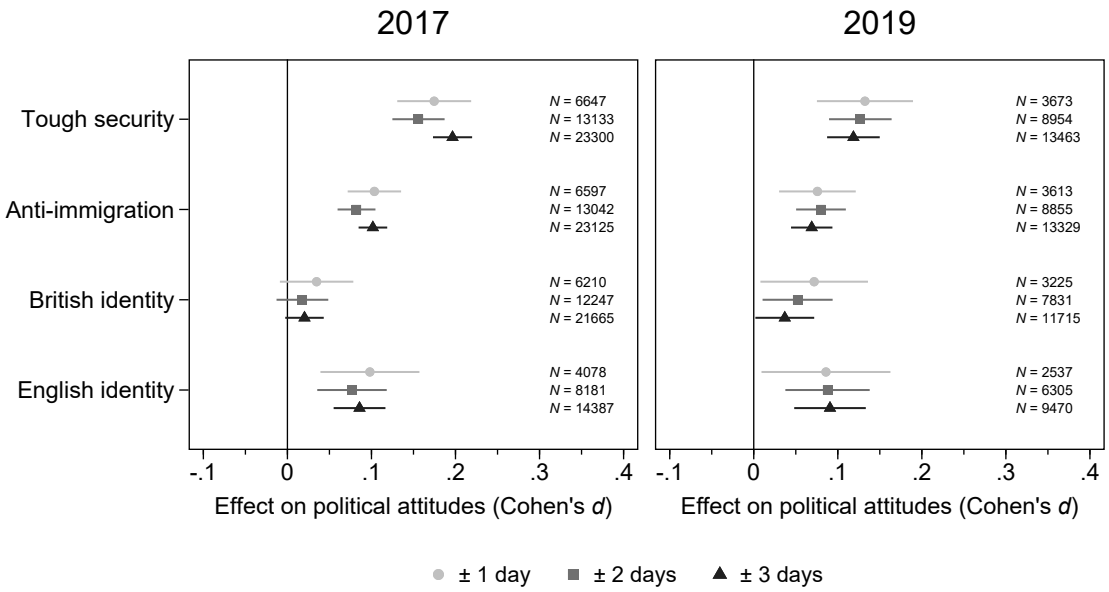
Figure S2.16 reports the results of a sensitivity analysis which adds artificial observations to the 2019 sample such that the sample sizes in 2019 match 2017. We variably assume that the effect of exposure to the 2019 attack among artificial observations was zero (blue estimates); 50% of the measured effect (light blue estimates); 150% of the measured effect (light red estimates); and 200% of the measured effect (red estimates). The actual measured effects are in (different shades of) gray.

Figure S2.16: Sensitivity analysis



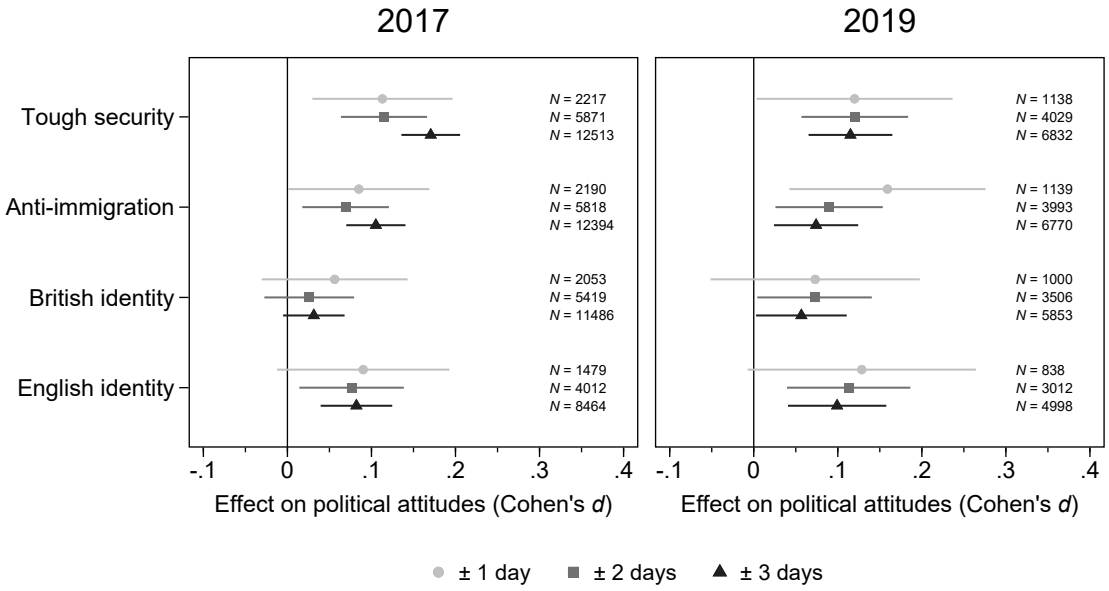
2.8 Additional Robustness Checks

Figure S2.17: Full samples including repeated attempts, ineligible citizens, and repeated attempts



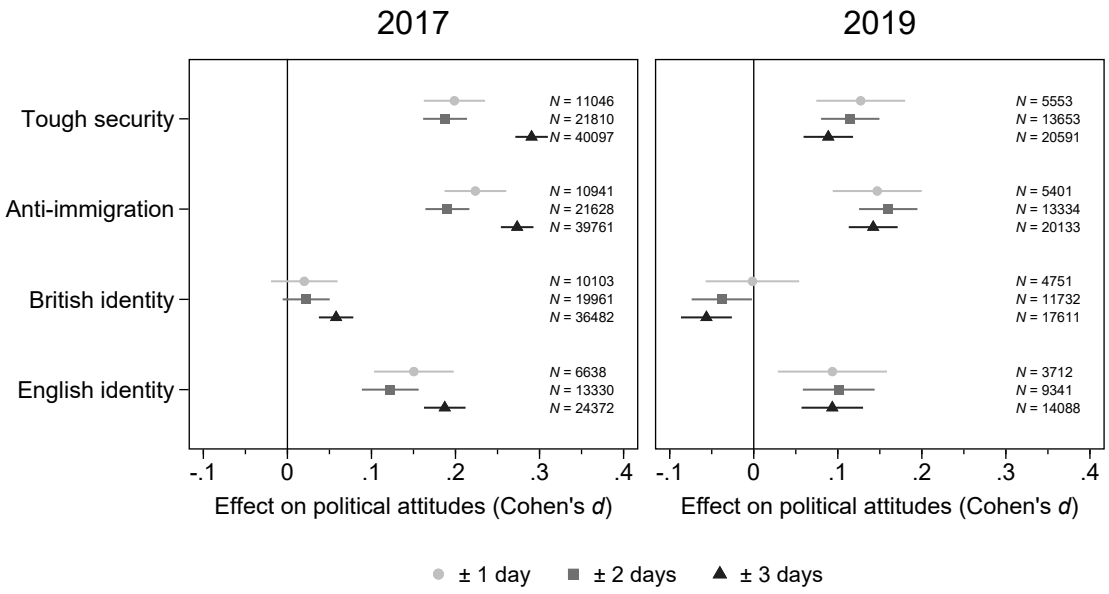
Note: The spikes represent 95% confidence intervals.

Figure S2.18: Exact matching



Note: The spikes represent 95% confidence intervals.

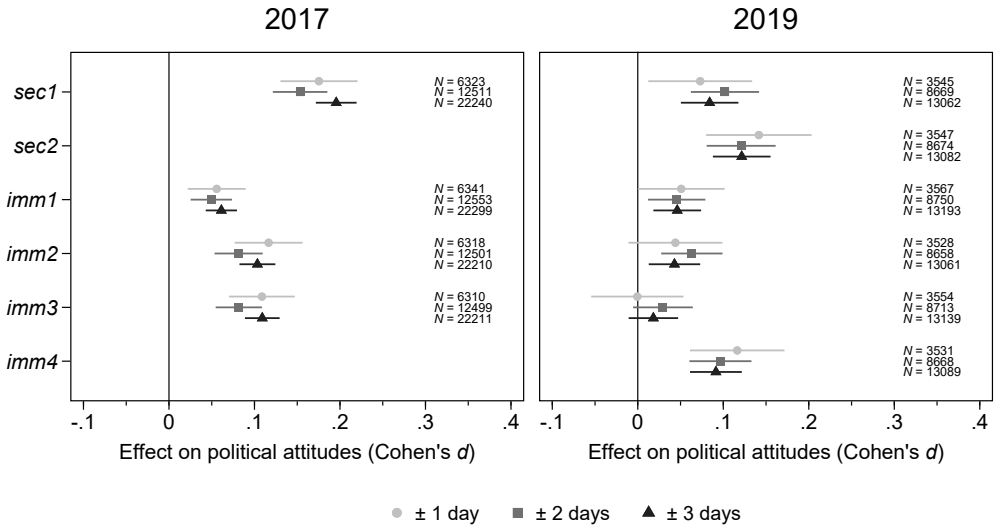
Figure S2.19: Naive pre/post comparisons not including any covariates



Note: The spikes represent 95% confidence intervals.

2.9 Item-by-Item Analysis

Figure S2.20: Item-by-item analysis



Note: The spikes represent 95% confidence intervals.

2017:
sec1: The security services should be allowed to monitor people's Internet use.
imm1: The UK should continue to allow free movement of people to and from the European Union. (reversed)
imm2: The UK should introduce quotas to limit the number of immigrants coming into the country.
imm3: Britain should welcome a larger number of migrants from war-torn countries. (reversed)

2019:
sec1: UK citizens who travelled to Syria to support ISIS should lose their British citizenship.
sec2: The police should be given more powers to stop and search suspected criminals.
imm1: Free movement of people between the UK and the rest of the European Union should end.
imm2: The UK should introduce a points-based system to limit the number of immigrants coming into the country.
imm3: Immigration undermines the cultural values of the UK.
imm4: The UK should accept more refugees from conflict zones. (reversed)

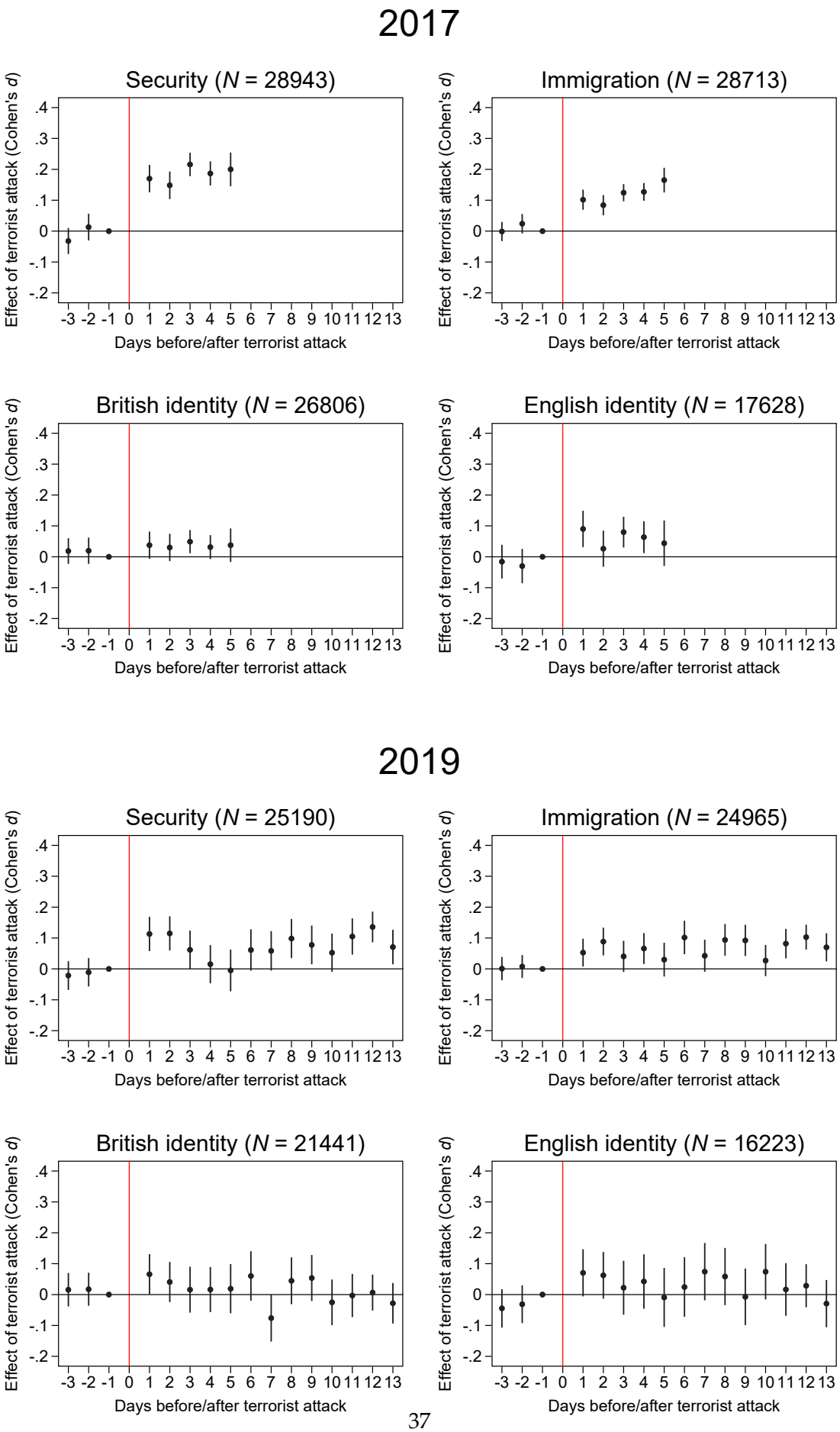
Note: Specifications are analogous to the main results.

2.10 Durability

Our ability to establish the longer-term effects of the London Bridge attacks is hindered by the fact that *WhoGetsMyVoteUK* stopped data collection after election day in both 2017 and 2019. This means that we have only five days' worth of data to play with after the 2017 attack, but a slightly more generous 13 days after the 2019 attack. We assess effects durability in two ways.

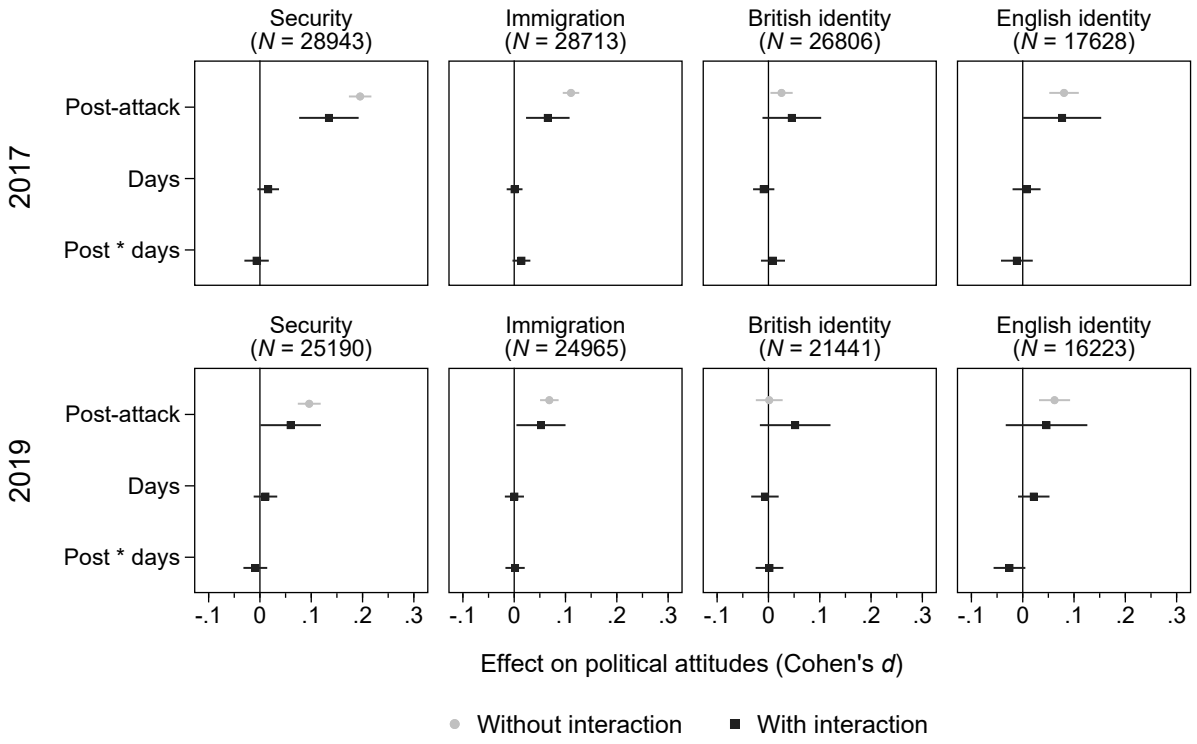
First, Figure S2.21 shows effect estimates for each individual day after the attacks. Analogous to the paper, we use the three days before the attacks as our control group and estimate effects using linear regression including the same set of controls. The 2017 results suggest very little change over the five days data is available. There is some more fluctuation in 2019, especially when it comes to security preferences. Still, no consistent downward trend is visible.

Figure S2.21: (Slightly) longer-term effects of the London Bridge attacks



Second, we provide a more formal test of effects durability by testing the interaction between our treatment (post-attack) indicator and a count of the number of days since the attack (Muñoz, Falcó-Gimeno, and Hernández 2020). We include all available observations after the attacks. Again, the three days before the attacks again serve as our control group, and we estimate effects using linear regression including the same set of controls. As Figure S2.22 shows, the interactions with the number of days since the attacks are consistently close to zero and never reach statistical significance. This suggests that the effects of the London Bridge attacks remained stable for a minimum of five days in 2017 and 13 days in 2019.

Figure S2.22: Formal tests of effects durability



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