

Islamist Terrorism Continues to Affect Political Attitudes

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Abstract

This study revisits the question whether contemporary Islamist terrorist attacks affect citizens' political attitudes by leveraging two natural experiments in the United Kingdom. The natural experiments emerged as the fieldwork periods for a popular online voter information tool coincided with both the 2017 and the 2019 London Bridge attacks. Because thousands of voters provided their political opinions via the tool every day around the time of the attacks, we can more precisely estimate the causal effects of the attacks on policy preferences. We find that both the 2017 and the 2019 London Bridge attacks increased support for restrictive security and immigration policies. The effect estimates pass a series of plausibility and robustness checks. This suggests that contrary to recent theorizing and empirical evidence, Islamist terrorism continues to evoke cognitive and emotional reactions which are sufficiently strong to affect political attitudes. Finally, in additional analyses we re-test two prominent hypotheses about individual-level causal heterogeneity: the reactive liberals and the geographic proximity hypotheses. Contrary to these hypotheses, we find no evidence that the effects of the London Bridge attacks were moderated by individuals' ideological predisposition or their geographic proximity to the attacks.

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Significance

Existing work suggests that while Islamist terrorism tended to increase out-group hostility and support for tough security policies in the early 2000s, Islamist terrorism has now become normalized and therefore no longer affects political attitudes. We revisit the normalization hypothesis using two mass online datasets that allow us to more precisely estimate the causal effects of two recent Islamist terrorist attacks, the 2017 and the 2019 London Bridge attacks. Contrary to the normalization hypothesis, we find that support for restrictive security and immigration policies increased in the wake of both attacks. Such changes in public opinion matter because they can increase the electoral success of anti-immigrant parties, motivate policy changes, and bolster the narratives of extremist organizations.

Introduction

The 9/11 attacks and subsequent attacks on transportation systems in Madrid (2004) and London (2005) spawned a large literature examining the consequences of Islamist terrorism for public opinion in Western countries (for a recent review see Godefroidt, 2022). According to this literature, Islamist attacks are likely to cause strong emotional responses, including fear and anger (Skitka et al., 2006), remind people of their own mortality (Das et al., 2009), and heighten perceptions of threat, injustice, and moral violation (Huddy et al., 2005; Lambert et al., 2019). As a result, Islamist terrorism has been shown to affect a wide range of political preferences and attitudes. For example, Islamist terrorism has been found to increase people's willingness to trade off some of their civil liberties in favor of an increased sense of security (Davis & Silver, 2004) and to bolster hawkish military policies (Hetherington & Suhay, 2011). Other research has found evidence that Islamist terrorism hardens people's attitudes towards outgroups in general (Echebarria-Echabe & Fernández-Guede, 2006) and Arabs, Muslims, and immigrants in particular (Panagopoulos, 2006), which often translates into decreased support for liberal immigration policies (van de Vyver et al., 2016) and increased support for retaliatory policies punishing outgroup members (Saleem et al., 2017). Finally, Islamist terrorism has also been found to bolster support for incumbents and trust in the government more generally (Landau et al., 2004). Such kinds of changes in public opinion are important because they can affect election results (Montalvo, 2011), motivate policy changes (Helbling & Meierrieks, 2020), and bolster narratives of extremist organizations (Bail et al., 2018). However, to what extent Islamist terrorism *continues* to affect public opinion remains unclear.

Western Europe and North America have been haunted by a new wave of Islamist terrorism since 2015 (Nesser, 2018). This resurgence of Islamist terrorism has caused renewed interest in its consequences for public opinion in Western countries. As noted by Nussio (2020), a key observation from studies of recent Islamist terrorist attacks in countries such as Belgium, France, Germany, the United Kingdom, and the United States is that results are much more mixed when compared with studies of attacks from the early 2000s. Studies of more recent attacks often report null findings (e.g., Boydston et al., 2018; Brouard et al., 2018; Castanho Silva, 2018; Larsen et al., 2020; Mancosu & Ferrín Pereira, 2021; van Assche & Dierckx, 2021). This has given rise to new theorizing suggesting that, due to repeated exposure, Western citizens may

have become desensitized or, in other words, “numb” to Islamist terrorism. According to this perspective, Islamist terrorism has become normalized and, therefore, no longer elicits emotional and cognitive responses that are strong enough to change political attitudes (Castanho Silva, 2018; Nussio, 2020).¹

In this article, we revisit the question whether contemporary Islamist terrorist attacks continue to affect citizens’ policy preferences using data from *WhoGetsMyVoteUK*, a popular online voter information tool that has been made available to British voters before every general election since 2015. Similar to other so-called Voting Advice Applications (VAAs) in countries such as the Netherlands, Germany, or Switzerland, *WhoGetsMyVoteUK* allowed its users to establish their match with political parties by entering their preferences on a series of policy issues on a webpage (Germann & Gemenis, 2019). The tool then compared users’ policy preferences with the positions of political parties and informed its users about their level of agreement with the different parties (see *SI Appendix*, Section 2 for additional information on the design of *WhoGetsMyVoteUK*).

Due to the richness of the data they generate, VAAs are increasingly used not just by voters as a source of information, but also by researchers as a source of data (e.g., Otjes & Krouwel, 2019; Wheatley & Mendez, 2021; Wurthmann et al., 2021). For the present purposes, reliance on VAA data has at least two important advantages. First, VAA users have a clear incentive to reveal their true policy preferences because they want to learn about their ideological congruence with the various parties contesting the election, thus reducing the risk of social desirability bias. Second, and even more importantly, VAAs like *WhoGetsMyVoteUK* tend to have very large numbers of users every day, which makes it possible to accurately estimate the causal effects of salient events, such as terrorist attacks, if they coincide with the fieldwork period. In this article, we exploit the fact that precisely this scenario played out in both 2017 and 2019, when, in both years, *WhoGetsMyVoteUK*’s fieldwork period coincided with an Islamist terrorist attack in the heart of London. The first attack occurred just five days before the 2017 general election and involved a group of terrorists deliberately ramming a van into pedestrians on London Bridge and subsequently stabbing people in a nearby food market. A total of 11 people were killed,

¹ See *SI Appendix*, Section 1, for more systematic evidence that studies of recent Islamist terrorist attacks increasingly report null effects based on a re-analysis of a meta-analytic dataset collected by Godefroidt (2022).

including the three perpetrators, and 48 more were wounded. The second attack occurred at almost exactly the same location around two weeks before the 2019 general election, when a single perpetrator stabbed several people in the area. A total of three people were killed, including the perpetrator, and three more were wounded. Both attacks were claimed by the Islamic State (IS) and fueled debates over immigration and security policy.

We argue that whether an individual accessed *WhoGetsMyVoteUK* before or after the London Bridge attacks was as-if random, at least after covariate adjustment and within a short time window. Accordingly, we estimate the causal effects of the London Bridge attacks on policy preferences by comparing users who accessed the tool just before and just after the terrorist attacks. This identification strategy approximates an experimental design in which we would randomly assign exposure to a real-world terrorist attack (Muñoz et al., 2020) and, given the large number of daily users, allows us to significantly improve over prior attempts to estimate the causal effects of Islamist terrorist attacks. Recent years have seen significant methodological advances across the Social Sciences including in the literature on Islamist terrorism. Nonetheless, significant issues with research design persist. For example, many studies continue to rely on cross-sectional designs that cannot identify causal effects (e.g., Canetti et al., 2017; Cheung-Blunden, 2020). Other studies have relied on priming experiments, which improve causal identification but at the same time raise questions about generalizability to real-world behavior in the aftermath of attacks (e.g., Gadarian, 2010; Lambert et al., 2010; Landau et al., 2004). Finally, researchers have increasingly drawn on natural-experimental approaches similar to ours, which in principle combine high internal validity with high external validity. However, prior natural-experimental studies tended to rely on public opinion surveys that rarely interview large numbers of individuals per fieldwork day—often not more than a few dozen. Therefore, researchers had to either rely on small samples, which increases the risk of false negatives and inflated effect sizes. Or, they had to rely on long temporal bandwidths, often spanning weeks before and after an attack, at the risk of introducing bias due to other events and time trends. By contrast, *WhoGetsMyVoteUK* was accessed by thousands of British voters every day around the time of the attacks, which allows us to more accurately estimate causal effects within short time frames.

Evidently, in the absence of researcher control over treatment assignment, causal inferences

always hinge on strong assumptions. Therefore, we systematically assess the plausibility of our identifying assumptions using placebo tests and an analysis of newspaper headlines around the time of the attacks. Despite the clear methodological advice to test observable implications of identifying assumptions in natural experiments (Muñoz et al., 2020), such tests are still rarely conducted in the literature on Islamist terrorism (but see Giani, 2021). The robustness and generalizability of our findings is further strengthened by the fact that we analyze two attacks, albeit two attacks in the same country.

Finally, after establishing the average effects of the London Bridge attacks we proceed to investigate individual-level treatment effect heterogeneity. Interest in the characteristics of citizens who are most affected by Islamist terrorism has increased in recent years, with much of the attention going to the reactive liberals and the geographic proximity hypotheses. First, the reactive liberals hypothesis suggests that Islamist terrorism primarily influences the views of left-wing voters (liberals in U.S. parlance) because of a ceiling effect: right-wing voters already tend to prefer tough security measures, strict immigration laws, and other authoritarian policies, leaving them less room to move further to the right on these issues (Nail et al., 2009). Second, the geographic proximity hypothesis suggests that voters who live in close proximity to the site of a terrorist attack are more likely to feel threatened and, therefore, also more likely to change their policy views (Nussio et al., 2019). While several existing studies have examined these hypotheses, the evidence remains mixed (e.g., Agerberg & Sohlberg, 2021; Böhmelt et al., 2020; Castanho Silva, 2018; Giani, 2021; Hetherington & Suhay, 2011; Nail et al., 2009; Nussio et al., 2019; van de Vyver et al., 2016). We contribute to the ongoing debate on individual-level moderators by reassessing the reactive liberals and the geographic proximity hypotheses using our large-scale online data.

Research Design

We identify the causal effects of the 2017 and 2019 London Bridge attacks on policy preferences by comparing *WhoGetsMyVoteUK* users who accessed the tool before and after the attacks. This identification strategy rests on three main assumptions. First, *WhoGetsMyVoteUK* users who accessed the tool after the attacks must have been exposed to news about the terrorist attacks (compliance assumption). Second, the changes in policy preferences must be attributable

to the terrorist attacks and not another event which occurred around the same time or an unrelated time trend (excludability assumption). Third, whether somebody accessed *WhoGetsMyVoteUK* before or after the attacks must be as-if random (ignorability assumption). Critically, the latter two assumptions are harder to meet the longer the time frame considered. On the one hand, longer time frames increase the risk of bias due to other events and unrelated time trends (Hausman & Rapson, 2018). On the other hand, longer time frames also increase the risk of imbalances between the pre- and post-attack samples. In traditional surveys, the timing of interviews is often partly dependent on demographic characteristics, for example, because some quotas are easier to fill compared to others (Eggers & Harding, 2021; Muñoz et al., 2020). Similarly, the demographic profile of VAA users is likely to vary over time, especially over longer time frames. For example, while people with high political interest may turn to VAAs weeks before an election, people who are less interested in politics may only use VAAs when elections are imminent (Germann et al., 2015).

Fortunately, *WhoGetsMyVoteUK*'s large numbers of daily users allow us to draw on exceptionally short temporal bandwidths and compare individuals who accessed the tool within one, two, and three days of the attacks without sacrificing statistical power. As it can be unclear whether individuals were immediately exposed to news about the attacks, we drop users who accessed *WhoGetsMyVoteUK* on the day of the attacks themselves. Bias due to unrelated events or time trends is unlikely over such short time frames. And, consistent with as-if randomization, we find that the demographic profile of individuals who accessed *WhoGetsMyVoteUK* within one, two, and three days of the attacks is very similar (see *SI Appendix*, Section 3). However, even small imbalances can bias causal estimates (Stuart, 2010). Therefore, we match treated (i.e., post-attack) and control (i.e., pre-attack) subjects exactly on a series of demographic variables including age, gender, education, region of residence, political interest, general political orientation, and past voting behavior (see *SI Appendix*, Section 3 for details). Exact matching constitutes the gold standard method of covariate adjustment because it removes imbalances between treated and control observations fully and not just approximately (Ho et al., 2007). As *WhoGetsMyVoteUK* was accessed by large numbers of voters every day, we retain between 1,100 and 12,500 observations even after exact matching, depending on the year and temporal bandwidth. In additional analyses, we also estimate the effects of the London Bridge attacks

beyond the maximum three-day bandwidth, though we note that more long-term effects are less well identified.

Data from *WhoGetsMyVoteUK* allows us to draw inferences regarding two widely studied outcomes in the literature on Islamist terrorism: support for tough security measures and support for restrictive immigration policies (Godefroidt, 2022). With the exception of security preferences in 2017, we measure all outcome variables using summated rating scales consisting of two to four policy statements. For security preferences in 2017 we use a single item because the 2017 version of *WhoGetsMyVoteUK* included only one statement related to security policy. Examples of items we use to measure security preferences include “The security services should be allowed to monitor people’s Internet use” and “The police should be given more powers to stop and search suspected criminals”. Examples of items we use to measure immigration preferences include “The UK should accept more refugees from conflict zones” and “The UK should introduce quotas to limit the number of immigrants coming into the country”. *WhoGetsMyVoteUK* users provided their answers on five-point Likert scales ranging from ‘Completely agree’ to ‘Completely disagree’. Users could also select ‘No opinion’, which we treat as missing data. All summated rating scales are unidimensional (Loevinger’s H : 0.53 – 0.69) and have acceptable scale reliability (Cronbach’s α ’s: 0.65 – 0.84). To facilitate interpretation, we normalize all outcome variables so that they range from 0 to 100, whereby higher values indicate higher support for tough security and restrictive immigration policies. *SI Appendix*, Section 4 provides the wordings of all outcome questions and the complete results of the scaling analysis.

All analyses reported in the paper exclude likely repeated attempts by the same individuals, individuals who indicated that they are not eligible to vote in the UK, and speeders who rushed through the tool in less than one-third of average time. This type of data cleaning is standard procedure when working with VAA data (Andreadis, 2014; Wheatley & Mendez, 2021) and in our case leads to the dropping of approximately 5% and 7% of the total number of observations in 2019 and 2017, respectively. As shown in *SI Appendix*, Section 5, the results remain similar when all available observations are included.

Finally, it is important to note that online tools such as *WhoGetsMyVoteUK* tend to appeal more to certain demographics, including younger and more highly educated voters (Munzert & Ramirez-Ruiz, 2021). Therefore, the effects we report can only be generalized to the British

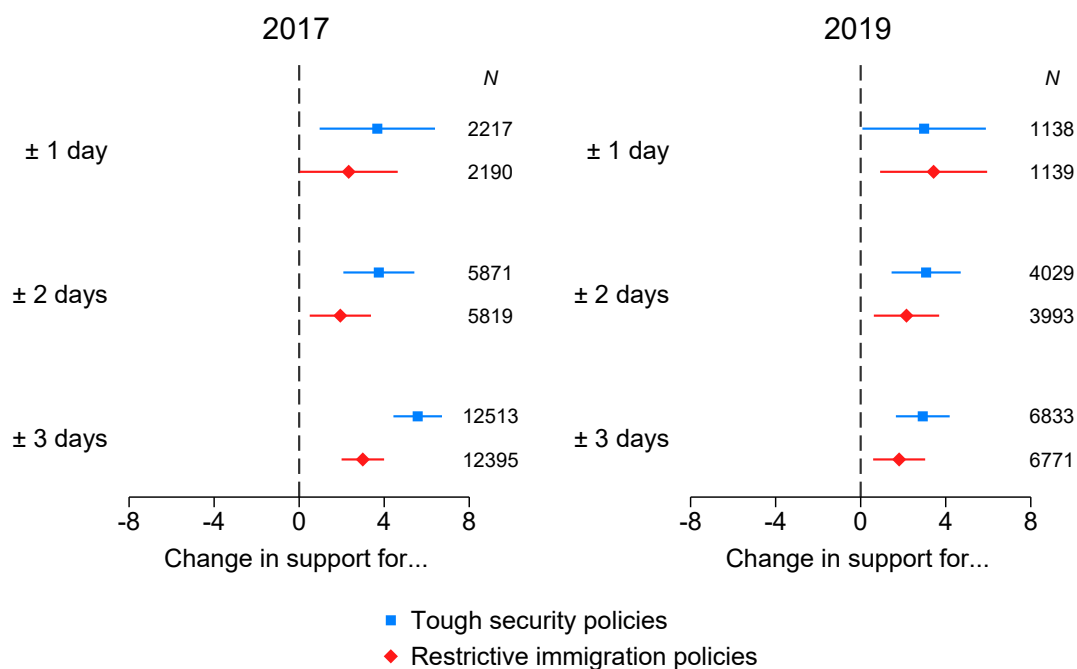
population under certain assumptions. That said, *WhoGetsMyVoteUK* was promoted using a variety of channels including press releases, articles in local and national newspapers, interviews in print and broadcast media, and paid advertisements on social media. As a result, our samples are demographically diverse and similar in composition to other online surveys (see *SI Appendix*, Section 6). Existing evidence suggests that causal effects are likely to generalize from diverse online samples to the population except under severe treatment effect heterogeneity (Coppock, 2019; Mullinix et al., 2015).

Results

Average Effects: We find that both the 2017 and the 2019 London Bridge attacks affected British voters' security and immigration preferences. As the left panel in Figure 1 shows, support for tough security measures increased by 3.5 to 5.5 points as a result of the 2017 attack, depending on the temporal bandwidth, whereas support for restrictive immigration policies increased by 2 to 3 points (always on scales ranging from 0 to 100). We find similarly sized effects for the 2019 attack. As the right panel in Figure 1 shows, support for tough security measures is estimated to have increased by around 3 points in the aftermath of the 2019 attack. Again, we find similarly sized increases of 2 to 3.5 points for support of restrictive immigration policies.

It is important to note that these are relatively small effects, with Cohen's d ranging from 0.11 to 0.17 for security preferences and from 0.07 to 0.16 for immigration preferences. Still, as we show in *SI Appendix*, Section 1, the effects of the London Bridge attacks are comparable in size to estimates of other prominent Islamist terrorist attacks from the early 2000s including the assassination of Theo van Gogh in the Netherlands in 2004 and the 2005 London bombings, and around half as big as the effects of 9/11 and the 2004 train bombings in Madrid. That 9/11 and Madrid 2004 caused somewhat stronger public reactions is hardly surprising given their much larger scope, casualty count, and historical significance. More important, therefore, is the broad similarity of the effect sizes we report compared to those reported for attacks of the early 2000s. This finding is particularly noteworthy in the case of the 2017 London Bridge attack as that attack occurred less than two weeks after another Islamist terrorist attack in the

Figure 1: Effects of the London Bridge attacks on security and immigration preferences



Note: The figure shows point estimates including 95% confidence intervals.

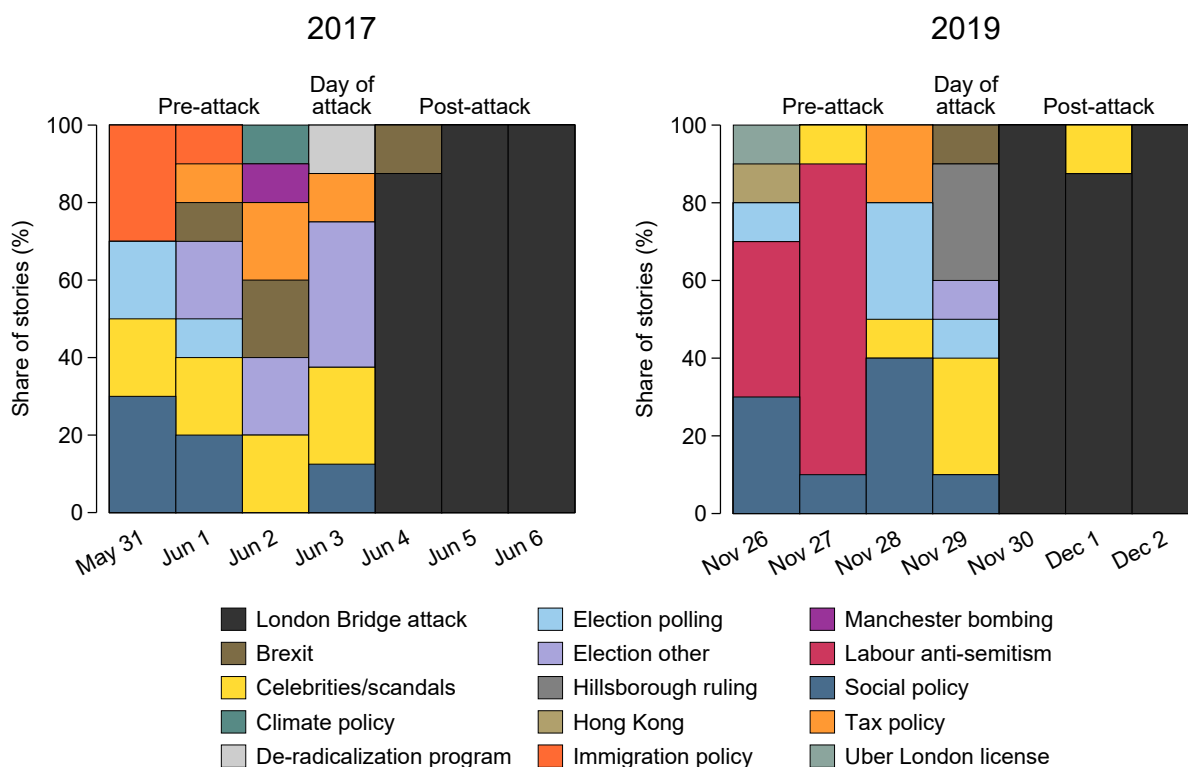
UK, the Manchester Arena bombing.² Overall, our results lead us to reject the normalization hypothesis: Western (or at least UK) citizens do not seem to have become numb to Islamist terrorism.

Identifying Assumptions: We begin to evaluate the plausibility of our causal identification assumptions by investigating the content of top headline news stories around the time of the London Bridge attacks. While individuals can learn about terrorist attacks in different ways, newspapers constitute a key source of information about major events such as terrorism. Therefore, extensive news coverage and especially coverage on front pages makes it likely that people are exposed to news about an event. Reassuringly, that is precisely what we find as almost all of the 10 leading British newspapers led with the attacks on the first, second, and third day after the attacks in both 2017 and 2019 (see Figure 2).³ This makes it likely that subjects in our post-attack samples knew about the London Bridge attacks and, therefore, strengthens confidence in the compliance assumption.

² A total of 23 people were killed in the Manchester Arena bombing including the attacker. Unfortunately, our data does not allow us to estimate the effects of the Manchester bombing as *WhoGetsMyVoteUK* was launched only after the attack.

³ A list of the newspapers included in this analysis and additional information on the content of headline news stories can be found in *SI Appendix*, Section 7.

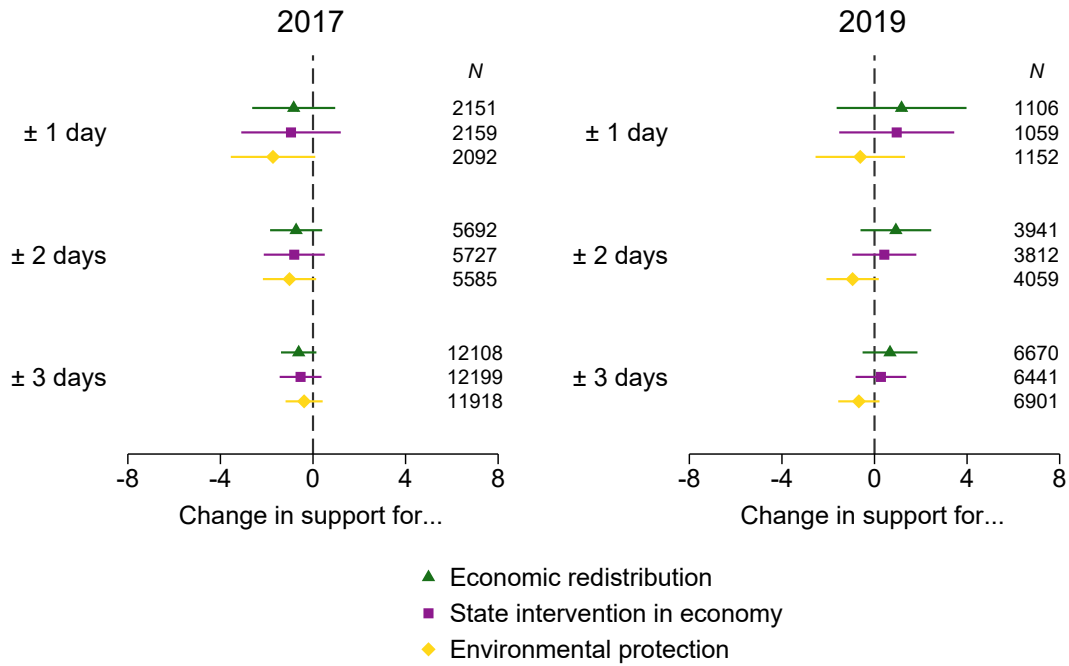
Figure 2: Top headline news stories in 10 leading British newspapers around the time of the London Bridge attacks



In addition to this, newspaper headlines enable an initial test of the plausibility of the excludability assumption. As Figure 2 shows, leading British newspapers not only almost universally led with the attacks after they had occurred, they also led with a potpourri of different stories in the run-up to the attacks, most of which are highly unlikely to affect security and/or immigration preferences (e.g., stories related to the UK’s national health service, proposed tax hikes, and sex scandals). Among the small number of possible exceptions range reporting on Brexit, the Manchester bombing, and perhaps allegations of anti-semitism against the Labour party. However, reporting on these topics was of a continuous nature and is therefore unlikely to be responsible for the short-term effects we observe. Overall, the content of newspaper headlines published around the time of the attacks increases our confidence that the effects we measured can be attributed to the attacks and not some other, simultaneously occurring event.

Next, we report a series of placebo outcome tests in which we estimate the effects of the London Bridge attacks on three kinds of placebo policy preferences: support for economic redistribution, support for state interventions in the economy, and support for environmental

Figure 3: Placebo outcome tests

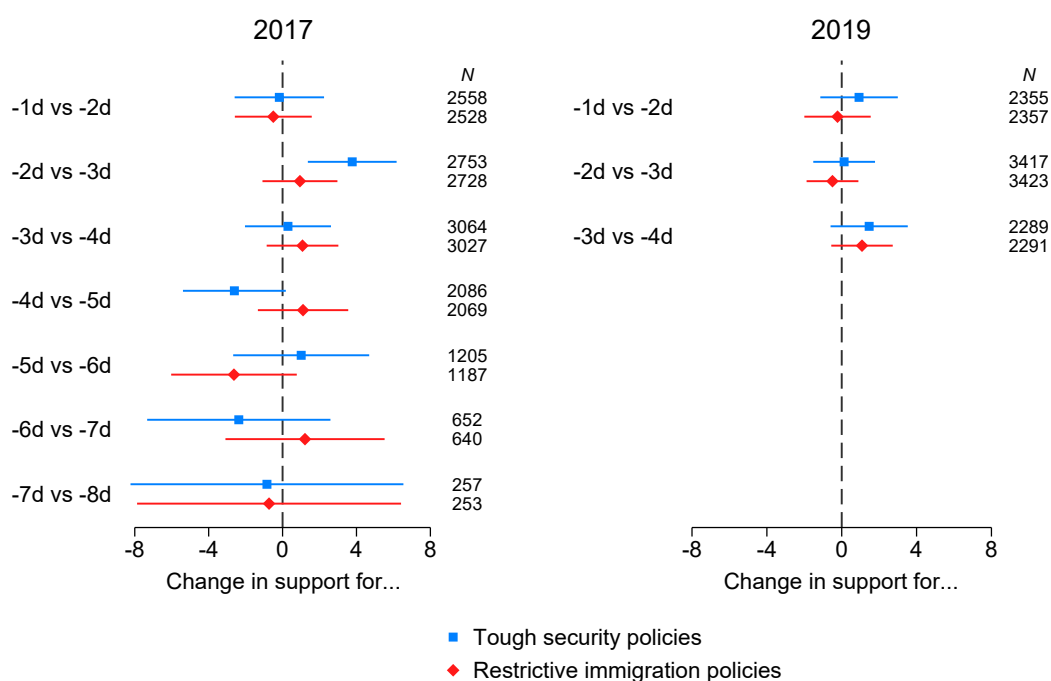


Note: The figure shows point estimates including 95% confidence intervals.

protection. According to extant theories, Islamist terrorism may affect citizens' preferences on security and immigration but should not affect their economic or environmental preferences (Lambert et al., 2019).⁴ Therefore, statistically significant effects of the London Bridge attacks on the placebo policy preferences would undermine the plausibility of our causal estimates and point to bias due to an unrelated event or time trend which affected a broader range of policy preferences, or to bias due to imbalances between the control and treatment groups. We measure placebo outcomes using summated rating scales consisting of two to four policy statements. Question wordings and information on psychometric performance can be found in *SI Appendix*, Section 8. Analogously to above, all placebo outcomes are normalized so that they range from 0 (low support) to 100 (high support) and we match exactly on the same covariates as in the main analysis. Bolstering confidence in our causal estimates, Figure 3 shows that all placebo effects are substantively close to zero and lack statistical significance despite the large sample sizes of up to 12,000 observations.

⁴ A notable exception is the *conservatism as motivated social cognition* theory, which leads to the expectation that Islamist terrorism affects a very broad set of attitudes including preferences on economic policy and even the environment (Jost et al., 2003). However, the empirical evidence that Islamist terrorism affects policy preferences beyond security and outgroup-related policies more generally is weak (e.g., Eadeh & Chang, 2020; Lambert et al., 2019; Lambert et al., 2010; Nail & McGregor, 2009). The results of our placebo outcome tests can be read as further evidence against such a general conservative shift.

Figure 4: Placebo treatment tests



Note: The figure shows point estimates including 95% confidence intervals.

Finally, we report placebo treatment tests comparing exactly matched subjects who accessed *WhoGetsMyVoteUK* on a given day during the pre-treatment period with subjects who accessed *WhoGetsMyVoteUK* the day before. We use the same covariates for exact matching as above. Non-zero placebo treatment effects could point to an unrelated time trend, to bias due to an unrelated event which occurred during the pre-treatment period, or to imbalances between the control and treatment groups. Reassuringly, Figure 4 shows that placebo treatment effects are generally close to zero and, with just one exception (out of 20 estimated models), lack statistical significance.

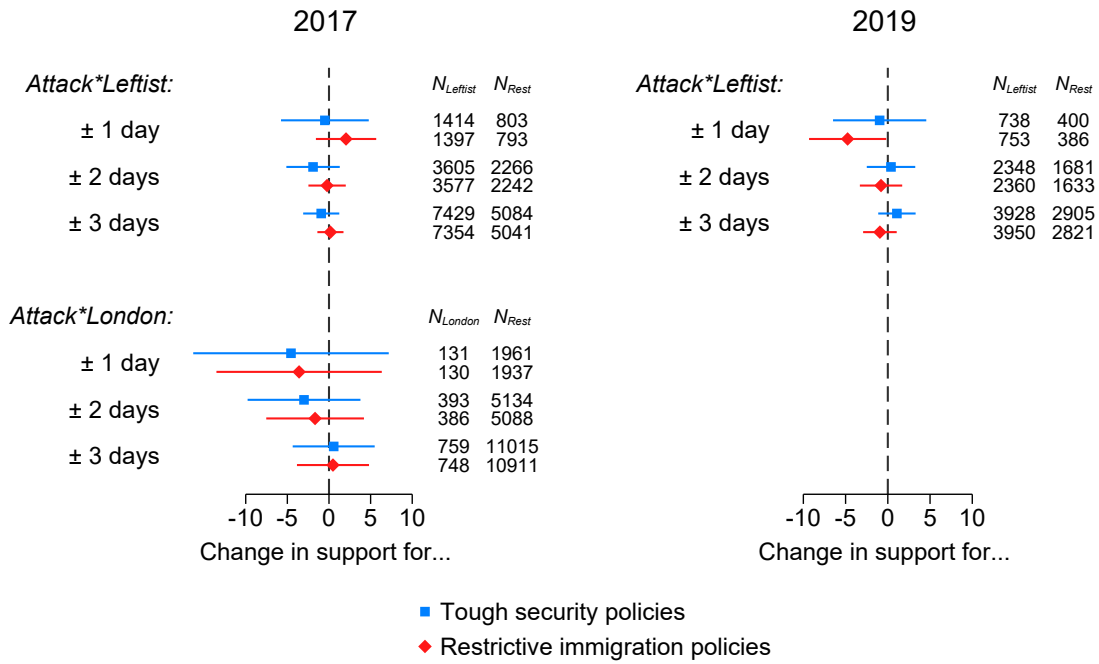
Additional Plausibility and Robustness Checks: We report additional plausibility and robustness checks in *SI Appendix*, Section 9. First, we investigate an additional way in which the ignorability assumption could be violated: subjects becoming more or less likely to answer our outcome questions after the terrorist attacks (attrition). Reassuringly, we find that subjects were equally likely to answer questions on security and immigration policy before and after the attacks. Second, we repeat all analyses while not adjusting for any covariates. The results are similar, which supports our assumption that usage of *WhoGetsMyVoteUK* is (close to) random during the three-day bandwidth. Third, we repeat all analyses while matching exactly on the

referring site (e.g., Facebook, Twitter, or a specific news site) in addition to the covariates used above. The results are again similar. This alleviates potential concerns that results could be biased because over-time variation in promotional activities made people with certain demographic profiles more likely to use *WhoGetsMyVoteUK* during specific times. Finally, we consider the effects of the London Bridge attacks on each of the individual items measuring security and immigration preferences instead of combining them to multi-item scales. The effects are similar for most items, with the most notable difference being that we find somewhat larger decreases in people’s willingness to accept refugees from war-torn countries.

Treatment Effect Heterogeneity: Having established the average effect estimates and their robustness, we next investigate individual-level causal heterogeneity. To test the reactive liberals hypothesis, we interact our treatment indicator with a binary indicator of whether an individual identifies as left of the political center. To test the geographical distance hypothesis, we focus on the 2017 attack and interact our treatment indicator with a binary indicator of whether an individual lived in London at the time of the attack. Unfortunately, the 2019 version of *WhoGetsMyVoteUK* does not include sufficiently fine-grained location data for an analogous test. The reactive liberals and geographic distance hypotheses are supported if the multiplicative interaction terms are significant-positive. As shown in Figure 5, this is not what we find. While some of the interaction terms do have a positive sign, others have a negative sign and with just one exception, the interaction terms all lack statistical significance. Additional information on measurement and the complete regression output can be found in *SI Appendix*, Section 10.

In *SI Appendix*, Section 10 we also report the results of multiplicative interaction models using continuous measures of subjects’ left-right position and their distance from the attack site. This allows us to investigate the effects of the London Bridge attacks across the full left-right spectrum and at various distances from the attack site. Analogously to above, we find no evidence to suggest that the London Bridge attacks had stronger effects on the policy preferences of more left-wing subjects or on individuals who live closer to London. As a final robustness check, we re-evaluate the reactive liberals hypothesis using two alternative proxies for individuals’ position along the ideological spectrum: (1) whether subjects voted for Labour or the Conservatives in the previous general election and (2) whether subjects voted for Remain

Figure 5: Assessing treatment effect heterogeneity

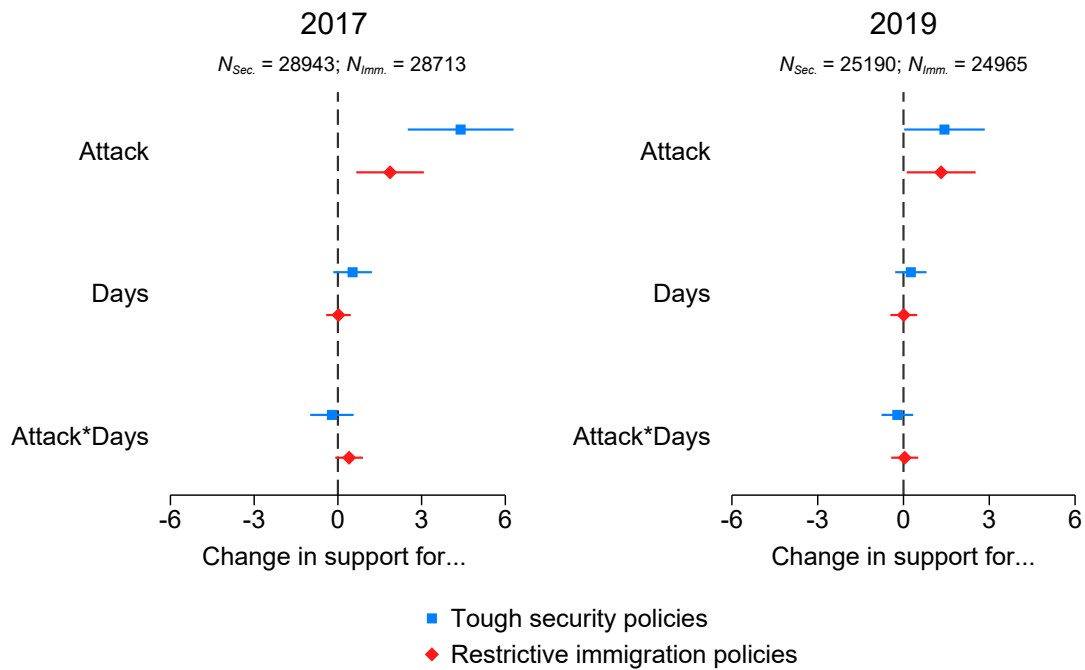


Note: The figure shows point estimates including 95% confidence intervals.

or for Leave in the 2016 Brexit referendum. Contrary to the reactive liberals hypothesis, we do not find that the attacks had stronger effects on Labour or Remain voters.

Impacts Beyond the Three-Day Bandwidth: An important remaining question is to what extent the London Bridge attacks continued to affect policy preferences beyond the maximum three-day bandwidth we have focused on thus far. To shed light on this question, we estimate linear regression models in which we interact our treatment indicators with the number of days since the London Bridge attacks. Contrary to all analyses reported above, we now include all available observations after the attacks. Since data collection stopped after election day, this amounts to a total of five days of data after the 2017 attack and 13 days after the 2019 attack. Individuals who accessed *WhoGetsMyVoteUK* during the three days before the attacks act as the control group. All models adjust for the same covariates we used in the main analysis. As Figure 6 shows, the interaction between the treatment indicator and the number of days since the attack is close to zero and not statistically significant in both 2017 and 2019. This suggests that the effects of the London Bridge attacks remained similarly sized for up to two weeks after the attacks, though it is worth repeating that effects beyond the three-day bandwidth are less well identified. Lack of data prevents us from investigating effects beyond

Figure 6: Interactions with the number of days since the attacks



Note: The figure shows point estimates including 95% confidence intervals.

two weeks after the attacks.

Conclusion

Our results suggest that Islamist terrorism continues to affect Western citizens' security and immigration preferences. This is an important result. Effects on public opinion play a central role in our understanding of how terrorism works and, confronted with increasing null results, social scientists have started to speculate that due to repeated exposure, Islamist terrorism no longer affects attitudes and policy preferences (Castanho Silva, 2018; Nussio, 2020). Drawing on exceptionally large datasets that enabled us to estimate the real-world effects of two Islamist terrorist attacks with a high level of internal validity and precision, we presented evidence against the normalization hypothesis and, notably, found similar effects in both the natural experiments we studied. Overall, these results suggest that Islamist terrorism continues to evoke reactions that are sufficiently strong to shape political attitudes. In turn, such effects could help to legitimize more restrictive immigration rules or tougher security laws that could compromise civil liberties. Especially when attacks occur in close temporal proximity to elections, as was the case with both of the attacks we studied, Islamist terrorism could also have knock-on effects

on electoral outcomes by, for example, fueling support for anti-immigrant parties (Helbling & Meierrieks, 2020).

At the same time, it is important to keep in mind that our study identifies short-term effects and that the effects we found are relatively small. That said, the effects of the London Bridge attacks are broadly comparable in size to several frequently studied attacks from the early 2000s and we report suggestive evidence that the effects of the London Bridge attacks remained stable over a span of up to two weeks after the attacks. Another important limitation is that our evidence is limited to a single country, the United Kingdom. While our study presents strong evidence that UK citizens have not become numb to Islamist terrorism, it remains an open empirical question whether the same also applies to other countries. For example, it is possible that the way Islamist terrorist attacks tend to be portrayed in UK media evokes comparatively strong public reactions given the country's high density of tabloid newspapers, which are known to report on Islamist terrorism in undifferentiated ways that do not clearly distinguish Islamist terrorists from Muslims more generally (cf. von Sikorski, Matthes, et al., 2021; von Sikorski, Schmuck, et al., 2021). All in all, more evidence on the effects of Islamist terrorist attacks in other countries as well as the durability of effects on political attitudes is needed.

Finally, in additional analyses we found no support for two prominent hypotheses about individual-level causal heterogeneity: (1) the hypothesis that the effects of Islamist terrorism are shaped by ideological predisposition; and (2) the hypothesis that individuals who live more closely to an attack site are more strongly affected. These null findings suggest that the individual-level sources of treatment effect heterogeneity remain poorly understood and/or that the effects of Islamist terrorism are more homogeneous than previously thought. To arrive at a better understanding of causal heterogeneity, future studies should consider other possible sources of individual-level heterogeneity which we could not test due to data availability, including citizens' ethnic and religious identity (Shoshani & Slone, 2016) and motivation to control prejudice (Steen-Johnsen & Winsvold, 2019).

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