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Response to "On the Empirical Validity of 'Gendered Reactions to Terrorist Attacks Can Cause Slumps not Bumps'"

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This paper responds to:

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Author Response to “On the empirical validity of ‘Gendered reactions to terrorist attacks can cause slumps not bumps’”

Mirya R. Holman, Jennifer L. Merolla, and Elizabeth J. Zechmeister
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Jetter and Stockley (2023) successfully replicate nearly all 140 analyses we report in the original paper and appendix. In the process, they identified two errors. We appreciate this effort and made corrections to the data and code. Revising the analyses to correct these errors results in small changes to the output but does not change the significance, direction, or substantive effects of the central variables in the paper and does not alter our conclusions. The authors of the replication paper then extend their efforts beyond replication and, based on this work, conclude our work "does not provide sufficient support" for a gendered revision to the conventional rally ‘round the flag framework. We respectfully disagree with their conclusion because it ignores theory, disregards key components of the critical test case, ignores evidence provided in the article and supplementary materials, revises the empirical approach, and commits to strict p-value cut-offs that risk Type II errors.

Consider that a major lethal attack is perpetrated by international terrorists in two countries (A, B) that are identical in every respect, except that the leader of country A is a man and the leader of country B is a woman. The rally 'round the flag framework predicts a boost in public opinion in the former case, as per Mueller (1970), and scholarship has accepted this expectation as conventional wisdom. But should we expect that the leader of country B will experience a boost that is similar in magnitude?

Our article provides one answer to this question: "on average, women executives will be less likely to experience a robust rally effect compared with their counterparts who are men" (Holman, Merolla, and Zechmeister [hereafter HMZ], p. 261). In other words, the likelihood of a similar boost is less than 1. Further, in some cases, opinion will tend in the opposite direction ("slump, not bump", HMZ, p. 249).

Jetter and Stockley (2023) [hereafter JS] demonstrate the value of making data and code publicly available via an audit. They successfully replicate nearly all 140 analyses we report in the original paper and appendix. In the process, they identified two errors. Owing to their diligence, we made a set of corrections to the data and code. Importantly, revising the analyses to correct these errors results in small changes to the output but does not change the significance, direction, or substantive effects of the central variables in the paper and thus does not alter our conclusions. We have a corrigendum in process with the *American Political Science Review*; it is reproduced as an appendix with this document.

The authors of the replication paper then extend their efforts beyond replication and, based on this work, conclude there is more uncertainty around the need for a gendered revision to the conventional rally 'round the flag framework. Specifically, JS assert there is insufficient evidence to support the conclusion that, as we stated in our abstract, "conventional theory on rally events requires revision: women leaders cannot count on rallies following major terrorist attacks."

We respectfully disagree with their conclusion because it ignores theory, disregards key components of the critical test case, ignores evidence provided in the article and supplementary materials, revises the empirical approach, and commits to strict p-value cut-offs that risk Type II errors. In what follows, we briefly address each of these points.

Theory

JS question the notion that "conventional theory on rally events requires revision" to account for gender bias. Yet the conventional theory was based on observations absent any consideration of gender (Mueller 1970) and without regard for theory on the gendered nature of evaluations of political leaders. While JS are content to consider empirics alone, our starting point is theory. Without theory, social scientists are severely limited in capacity to understand or predict (Popper 2005 [1935]).

As we note: "The conventional rally 'round the flag framework is silent on the relevance of the executive's gender" (HMZ, p. 249). To fill this void, our article offers a gender-revised rally framework that is grounded in scholarship that documents a tendency for the public to prefer

masculine (over feminine) leadership in general and especially in times of security threat, and especially with respect to evaluations of executives. As in most social science research, the argument is probabilistic rather than deterministic, in that women can present in ways that counter gender effects and factors other than gender can shape outcomes. Yet, so long as public opinion is gendered in its evaluations of leaders – especially in its evaluations of women leaders in the context of a security threat – our deductive thesis holds that women executives will fare less well than executives who are men.

JS conclude their paper with a set of explanations they assert are "more plausible" explanations for a decline in evaluations of then-Prime Minister Theresa May following the 2017 Manchester Arena attack. We are unconvinced. Rather than present a counter-argument to our gender-revised rally framework, they offer a string of ideas that include anecdotes from a mix of cases, one of which does not meet the paper's scope condition. A deep reading of the 'rally scholarship would have pointed JS to the criteria for a rally and a deep reading of our approach provides evidence to how our case meets these criteria (see pages 252-253).

One of their alternative arguments is that May's response to the attack "may have alienated the electorate" (JS, p. 13). They refer to aggressive language she used in early June. This logic presumes that the gender of the leader issuing an aggressive response is irrelevant. If we could document a pattern of slumps in response to aggressive language across types of leaders, this could be compelling. Yet, to the contrary, well-documented "bumps" in opinion followed aggressive policy responses from Blair after the train and bus bombings in 2005 and from Bush post 9/11. Perhaps even more importantly, JS' conjecture ignores the fact that we find a slump appears in analyses that restrict the bandwidth to four and ten days after the Manchester bombing, time periods that *precede* the rhetoric they reference; for example, May's discussion of deportations of foreign suspects (Mason and Dodd 2017) was after the narrow bandwidths we test in the article.

In another thesis that JS describe as "more plausible" they posit that the Manchester Arena bomber selected to attack because May was on the precipice of a public opinion slump. The notion that the attack was somehow endogenous to her weak position is creative brainstorming, but the authors provide no evidence to support this idea. Testimonials from the attacker's family and friends and a five-year long investigation into the attack and response suggest that the motive for the attack was revenge for American caused deaths of Syrian children, the identities of the concert goers (that is, primarily young women), and radicalization by ISIS. We can find no evidence in the three reports produced by the Manchester Arena Inquiry that points to an interest in influencing the election at a point when May was thought to be on the precipice of a public opinion slump. If anything, the perpetrator's timeline suggests that the arena had been selected as the location of the attack as early as February 2017, a full three months prior to the attack and before the beginning of the election period (Manchester Arena Inquiry 2023).

JS also fail to acknowledge that we engage in a wide set of analyses in the article devoted to testing alternative hypotheses, including some that they raise (e.g., Brexit, other features of the election), as well as offering a geographic test of the hypothesis and employing a variety of additional criteria outlined in Muñoz et al (2020). For example, using placebos, limiting the time bandwidth, and examining demographic patterns of those individuals in the "treatment" and

"control" groups (i.e., those surveyed before and after the attack) all point towards the Manchester attack as being a random event for the purpose of our hypothesis tests.

Notably, JS's final point under "more plausible" is that our thesis may actually be correct. Here we agree with them – there are good theoretical and methodological reasons to think the gender-revised framework is valid.

A Critical Test

Our empirical tests of the gender-revised framework center on the Theresa May case. It is given a privileged place in our article because it fits criteria for a critical test and we have access to data that permitted a variety of tests, including of the theorized mechanism and robustness checks.

We consider the May case a critical test because it meets Gerring's (2007) criteria of a "most likely" case. As we describe in the article, the value of a most likely case is that because "on all dimensions except the dimensions of theoretical interest, [it] is predicted to achieve a certain outcome" (Gerring 2007, p. 232), the absence of that outcome is particularly powerful evidence in support of the hypothesis. Our manuscript makes an evidence-based case that May, the bombing, and public attention following the bombing meet the standards for a most likely case.

Data available for the case permit us to maximize on internal validity via an unexpected event during survey design that generates a natural experiment. That this design is unavailable for the global tests increases the comparative weight we place on the May case. Another benefit of the May case is that the available survey data permit us to assess the theorized causal mechanism: gender norms biased against women. We show that the slump in approval is more pronounced among respondents with negative gender attitudes; this effect is robust when we run analyses with each indicator of gender attitudes. The panel data provides a unique opportunity here: the gender attitudes questions are asked in waves prior to the survey experiment, so we have no concerns about these questions priming answers among the respondents in the wave asked before and after the Manchester Attack. JS ignore this evidence in reaching their conclusions. This is surprising to us because it is the most specific test of the gender-revised rally framework.

In brief, analyses of data from the critical test provide convincing evidence that the slump-not-bump in public opinion toward Theresa May following the 2017 Manchester Arena terrorist attack was characterized by three dynamics: 1) a decrease in opinion that can be attributed to the attack itself, and not to other possible confounders (as theorized); 2) a tendency for that slump to have been driven by those with more bias against women (as theorized); and 3) a spill-over effect that turned public opinion comparatively more sour toward May's Conservative Party among those more geographically proximate to the event (consistent with theory, but not predicted). We show the robustness of these effects across many different types of model specifications. Our manuscript then runs a number of additional analyses to account for alternative explanations. As JS note, our results hold up across a wide range of analyses, including new model specifications that they run. In this process, JS identify an error in our code (specifically in the difference-in-difference model we present), but note that the correction actually improves the performance of the model. We have adjusted the code and this set of results in the corrigendum.

Revisions to the Empirical Approach

Our article prioritizes theory and the critical case, and supplements these with a limited global analysis that we dedicate very little space to in the article. JS flip our approach on its head: they place the global analysis at the core of their focus. The result is the shifting of perspective in such a way that it appears that there is more distance between our two sets of conclusions than actually exists. As we noted, JS in fact conclude it is plausible that our thesis is correct. On our read, the core of their skepticism is grounded in concern about the global analysis. We agree that this analysis is limited: the number of women leaders is itself limited and the analytical approach has less internal validity due to our inability to achieve comparable leverage over the presumed causal effect of an attack and the lack of data permitting an assessment of the mechanism. As we note in the article, "As expected, there are a relatively low number of women national executives: of these quarters, only nine are under women leaders" (p. 260).

JS further reduce the leverage provided by the scant number of women in the dataset by removing May. This is not unreasonable, but it is not a strict replication given that we asserted in the main text that our approach was to include all available cases given the small number of women executives who were in power during an international terrorist attack. The result without the May case is in the same expected direction, but the confidence intervals widen as one would expect with fewer degrees of freedom. JS also extend the analyses by assessing their robustness to clustering the errors at the country level. Again, this is not unreasonable. But it is also not a strict replication. Mechanically, the standard errors again increase while the coefficient remains with the expected sign and at a similar level of magnitude.

In brief, the extended analyses by JS demonstrate one of the limitations we noted regarding the global dataset: a low level of power due to a small number of cases in which women leaders were in power during international terrorist attacks. As stated above, another limitation is that the data do not permit an approximation of an experimental design, making the test weaker than the micro-level May tests. We also note in the paper that there is no definitive way to determine what threshold of lethality to use: if we lower the threshold, we would include at least one more case in which there was no rally for a woman leader (Merkel, see footnote 16). And, when we add the corrections to the dataset made by JS, the analysis to determine an appropriate threshold shifts to 14+ deaths.

Coding decisions aside, the global analysis simply cannot offer the strength of test that we achieve with the May case: it is challenged by low numbers of cases of women executives, the standard deficiencies associated with mere correlational analysis, and the lack of micro-level data with which to test the mechanism. We note that the coefficient on the interaction term in the replication report tends to be in the same ballpark as our analyses, while the standard errors are larger. And we note that the corrected data's direction and p-values are *still* consistent with our theoretical expectations: that women leaders would not receive a benefit from 'rally events in the same way as male executives.

The Tyranny of "Conventional" Significance Cut-offs

In another set of revisions to the original analyses, JS consider what would happen if we ran the global analysis without controls or otherwise changed the specification. Their report documents that the size and direction of the coefficient of concern remains effectively unaltered, while the confidence intervals shift such that the p-value at times rises above 0.05. The authors assert that we adhere to a strict 0.05 threshold for assessing statistical significance, yet this is not so. Rather, we take seriously concerns about Type II errors – the possibility of treating a seemingly "insignificant" finding as null when in fact the outcome is a false negative. An example of this approach is found in our assessment of the downstream consequences of the attack for May's Conservative Party, where we note the result is significant at p=0.079, one-tailed (HMZ, p. 258).

In accepting a rigid threshold of 0.05, JS are forced to conclude, for example, that a p-value of 0.058 is insignificant (JS, abstract, p. 2, and p. 7). Armed with copious amounts of data and strong theory predicting a null result, such an application of a strict threshold may be more easily defended. Yet, as noted above, the global analysis has exactly the opposite characteristics: a limited amount of data and strong theory predicting a positive finding. As Kennedy-Shaffer (2019) informs us, debate over the value of a strict 0.05 threshold has existed since its introduction. We will not attempt to resolve that debate here. Rather, we leave readers to determine whether the weight of theory and evidence lands in support of our thesis or against it.

Interestingly, even a quick look at the descriptive data that JS show in Table 2 demonstrates the norm for women executives after a major terrorist attack is a decline in approval, a pattern inconsistent with rally round the flag theory. Rather, these patterns are consistent with our argument that women leaders are more likely to fail to benefit or see a decline in approval following international terrorist attacks.

Conclusion

Our article (MHZ) asserted the need to synthesize theoretical argument and findings from the gender and politics subfield with the conventional rally round the flag thesis. A key contribution of our article is the assertion of a gender-revised rally round the flag thesis. This thesis holds that "women leaders cannot count on rallies following major terrorist attacks." JS challenge this conclusion on the less robust findings for the global analysis. Our position is that this conclusion still holds even if the assessment rests solely on the May case – that is, the totality of the May case as presented in the article, with attention to robustness checks, assessment of the theorized mechanism, and consideration of rival explanations. JS also do not offer argument to counter the notion that evaluations are gendered but, rather, they conclude that the global analysis is insufficient on its own to support the thesis. We point to the key place of theory and the UK case in our manuscript and in grounding its conclusion. That is, the strong findings we document for the May case are consistent with our conclusion.

The contributions of JS's replication work are three-fold. First, they provided important data corrections to our analyses. We are grateful for these corrections; we have adopted these (see the appendix) and affirm our conclusions hold. In the process, the effort demonstrates the value of placing data and do files in publicly accessible depositories. Second, they provide an example of an approach to replication that not only strictly audits the original data and analysis, but also adds additional tests. We caution only that readers discern the difference between replication and

revision and consider for themselves how to assess the sum of evidence that emerges from this exercise. Third, JS implicitly offer a contrasting perspective to our own: that correlational analyses of the global case are more consequential than theory, a critical test case, data that provides leverage for causal inference, and a robust consideration of alternative hypotheses for that case. They further adhere to the view that strict thresholds for statistical significance are useful in the face of under-powered analyses.

We are not so willing to take this path. Rather, we suggest an alternative approach that takes theory and critical case evidence seriously. Under this alternative approach, we see a research program that extends beyond our paper's conclusion in two ways.

First, we encourage scholars to add nuance to our gender-revised theoretical framework to anticipate conditions under which women will be able to secure a rally effect comparable to what a male counterpart would have achieved.

Second, we encourage scholars to deploy empirical tests of the core hypothesis that maximize on internal validity and that foreground the theorized mechanism: gender bias against women in leadership. Meanwhile, we look forward to finding ourselves in a world in which that bias is absent such that women leaders do not have to posture or be immunized by party, experience, or ideology to counter factors that leave them disadvantaged in leading during major security crises. And, though perhaps it goes without saying, we likewise look forward to working in a world where there are more cases of women executives so as to increase the opportunities for scientific investigation into these topics.

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Holman, Mirya R., Jennifer L. Merolla, and Elizabeth J. Zechmeister. 2022. "The Curious Case Of Theresa May And The Public That Did Not Rally: Gendered Reactions To Terrorist Attacks Can Cause Slumps Not Bumps." *American Political Science Review* 116(1): 249-264.

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Appendix: Pre-print of Corrigendum of "The Curious Case of Theresa May and the Public That Did Not Rally: Gendered Reactions to Terrorist Attacks Can Cause Slumps Not Bumps."

Mirya R. Holman, Jennifer Merolla, and Elizabeth J. Zechmeister

This is a pre-print of a submitted corrigendum that corrects errors in the published version of our article "The Curious Case of Theresa May and the Public That Did Not Rally: Gendered Reactions to Terrorist Attacks Can Cause Slumps Not Bumps." The authors acknowledge one error in the code, a set of errors in one of the datasets, and missing information from table and figure notes. The authors apologize for these errors and ask readers to use the corrected output and data. The authors thank Michael Jetter, Kieran Stockley, and the Institute for Replication for their efforts to identify the errors that this corrigendum corrects.

Correction 1: In the difference-in-difference analysis presented in Table 2, the question "Is Theresa May the best Prime Minister" mistakenly included data from wave 8, which included data from before May was the prime minister. We have corrected this analysis in Table 2. None of the central findings associated with the table and difference-in-difference results change. We further updated the table note to state that both models were run using OLS. We have also corrected the supplementary Table A5 that accompanies this table.

Table 2 (Corrected): Difference-in-difference, with Fixed Effects

	May best PM
*Manchester Attack * Time*	-0.0502*
	(0.0038)
Manchester Attack	0.0024
	(0.0046)
Time	0.2286*
	(0.0149)
Constant	0.1999
	(0.0149)
Controls	✓
Wave fixed effects	✓
Observations	62,095

Note: Dependent variables are 11-point favorability scale and perceptions of May as the best PM (OLS regression). Favorability model includes waves 8 to 16, while May best PM models include waves 10 to 12. Results consistent with logistic regression instead of OLS for May best PM model. Standard errors in parentheses. Full controls include whether someone identifies as ethnically British, gender, Labour party membership, other party membership, income, and ideology. See Appendix A (Table A5) for full models. *$p < 0.05$.

Correction 2: We identified errors in the data used to generate the results presented in Table 4. We have produced corrected results, available on Table 4, in the text on page 258, and in the supplementary materials in Figure H1 and Tables H1, H2, and H3. We have also added updated text to the discussion on page 260.

Table 4 (Corrected): All countries, attacks with more than 14 deaths

	Same approach, updated data & 14+ deaths
Int'l terrorist attack	2.122**
	(0.724)
Int'l terrorist attack x Woman executive	-3.918**
	(1.796)
Controls	✓
Observations	4,637
R^2	0.29968

The updated Table 4 leads us to update the text on p. 260 to indicate that the threshold is 14 deaths (not 16 as previously noted). We specifically update these pieces of the text:
The pooled dataset with these measures includes 44 countries from 1975 to 2017 (5,469 country-quarters); 19 countries had a woman leading the country during this period.
We define this as any attack that involved an international component (using the definition from the Global Terrorism Database) and had more than 14 deaths. The dataset contains 63 country-quarters with such an attack. As expected, there are a relatively low number of women national executives: of these quarters, only ten are under women leaders.
The effects on men's increased approval (2.1 points) and women's decreased approval (3.9 points) are substantively meaningful.
We add to Footnote 19: "If we drop the case of the Manchester attack (by omitting May's approval ratings for the 1 period after the attack), the interaction between a woman head of state and a large terrorist attack remains significant and negative. If we omit May entirely from the dataset, the interaction retains the same sign (negative) but is no longer significant. Post-hoc comparisons of the coefficients of these models indicate that men receive a statistically larger bump than do women with a threshold of 14 deaths (*p-value*=0.026) and a marginally significant bump with a threshold of 16 deaths (*p-value*=0.056)."
Correction 3: We have also updated the table and figure notes for all figures and tables in the manuscript to indicate which wave we use of the British Election Study.
Table 1: Table note should also say "Wave 12, British Elections Study."
Table 3: Table note should also say "Wave 12, British Elections Study."
Figure 1: Figure note should say "Waves 12 (dependent variable) and Wave 10 (gender attitudes), British Elections Study; see Appendix C, Table C1."
Figure 2: Figure note should also say "Wave 12, British Elections Study."
Figure 3: Figure note should also say "Wave 12, British Elections Study."

All data and materials to verify the reproducibility of the original and amended versions of the code have been posted to the American Political Science Review Dataverse.
Updated Supplementary materials: Please see updated supplementary materials available on the American Political Science Review Dataverse.
Specifically, we have updated the following tables in the supplementary materials:

Table A5: Appendix to Table 2: Difference-in-Difference, with fixed effects and average treatment effects
Figure H1: Effect of terrorist events on executive approval
Table H1: Gender-Revised Rally Effects
Table H2: Changing Thresholds and the Gender-revised Rally Effects
Table H3: Interactive effects with Executive Ideological Placement
We have updated the table notes for Tables A1, A2, A3, A4, A5, A6; B1, B2; C1, C2, C3, C4; D3, D4, D5; E1, E2, E3, E4, E5 and Figures C1, C2, E2 to reflect which waves of the British Elections Survey that we draw data from for the results.

Appendix for The Curious Case of Theresa May and the Public that Did Not Rally: Gendered Reactions to Terrorist Attacks can Cause Slumps Not Bumps

CORRECTED, March 2023

Table of Contents:

Appendix A: Full results of all models presented in paper, plus auxiliary tests

Table A1: Appendix to Table 1: Manchester attack and evaluations of May; OLS without Controls

	Like May	Like May	May Best PM	May Best PM	Like May	May Best PM
Surveyed after Manchester attack	-0.332*	-0.351*	-0.204*	-0.333*	-0.411*	-0.056*
	(0.054)	(0.043)	(0.038)	(0.042)		
Labour	-2.815*	-3.002*	-1.782*	-2.730*		
	(0.053)	(0.068)	(0.046)	(0.066)		
Ethnically British		0.436*		0.779*		
		(0.065)		(0.078)		
Gender		0.440*		-0.160*		
		(0.041)		(0.035)		
Other Party ID		-2.413*		-2.211*		
		(0.046)		(0.066)		
Income		-0.015*		0.022*		
		(0.007)		(0.007)		
Ideology		0.608*		0.486*		
		(0.011)		(0.011)		
Constant	5.642*	2.788*	0.306*	-1.191*	4.905*	0.480*
	(0.043)	(0.142)	(0.027)	(0.127)	(0.031)	(0.005)
Observations	32642	26506	34394	27844	32642	34394
R^2/ *Pseudo* R^2	0.143	0.442	0.0899	0.3293	0.004	0.003

Clustered errors on day of survey. Survey weights applied. * p<.05. Final two columns are OLS without controls. Wave 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

Table A2: Adding Control Variables – Like May

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Surveyed after Manchester attack	-0.332*	-0.335*	-0.364*	-0.366*	-0.349*	-0.351*
	(0.054)	(0.054)	(0.049)	(0.047)	(0.044)	(0.043)
Labour	-2.815*	-2.822*	-4.973*	-4.992*	-3.035*	-3.002*
	(0.053)	(0.053)	(0.042)	(0.051)	(0.069)	(0.068)
Gender		0.355*	0.363*	0.316*	0.443*	0.440*
		(0.045)	(0.039)	(0.041)	(0.041)	(0.041)
Other Party ID			-3.542*	-3.576*	-2.442*	-2.413*
			(0.041)	(0.039)	(0.046)	(0.046)
Income				-0.027*	-0.017*	-0.015*
				(0.007)	(0.007)	(0.007)
Ideology					0.608*	0.608*
					(0.011)	(0.011)
Ethnically British						0.436*
						(0.065)
Constant	5.642*	5.467*	7.629*	7.911*	3.639*	2.788*
	(0.043)	(0.044)	(0.039)	(0.056)	(0.103)	(0.142)
Observations	32642	32642	32642	26506	26506	26506
R^2	0.143	0.146	0.337	0.342	0.440	0.442

Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table A3: Adding Control Variables – May Best PM

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Surveyed after Manchester attack	-0.204*	-0.204*	-0.313*	-0.304*	-0.325*	-0.333*
	(0.038)	(0.038)	(0.042)	(0.038)	(0.042)	(0.042)
Labour	-1.782*	-1.783*	-3.845*	-3.836*	-2.776*	-2.730*
	(0.046)	(0.046)	(0.053)	(0.060)	(0.067)	(0.066)
Gender		-0.167*	-0.161*	-0.229*	-0.149*	-0.160*
		(0.028)	(0.030)	(0.028)	(0.034)	(0.035)
Other Party ID			-2.859*	-2.838*	-2.246*	-2.211*
			(0.065)	(0.062)	(0.067)	(0.066)
Income				0.009	0.021*	0.022*
				(0.006)	(0.007)	(0.007)
Ideology					0.478*	0.486*
					(0.012)	(0.011)
Ethnically British						0.779*
						(0.078)
Constant	0.306*	0.392*	2.502*	2.522*	-0.595*	-1.191*
	(0.027)	(0.032)	(0.063)	(0.072)	(0.115)	(0.127)
Observations	34394	34394	34394	27844	27844	27844
Pseudo R^2	0.090	0.091	0.259	0.259	0.322	0.329

Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Logistical regression.

Table A4: Alternative specifications: Adding May's Likeability in previous wave

	Like May	May Best PM
Surveyed after Manchester attack	-0.334*	-0.424*
	(0.028)	(0.047)
Like May previous wave	0.790*	
	(0.006)	
May best PM previous wave		2.725*
		(0.049)
Ethnically British	0.000	0.586*
	(0.053)	(0.085)
Gender	0.089*	-0.159*
	(0.028)	(0.046)
Labour	-0.899*	-2.493*
	(0.054)	(0.082)
Other Party ID	-0.647*	-2.060*
	(0.041)	(0.070)
Income	-0.009*	0.022*
	(0.004)	(0.008)
Ideology	0.108*	0.386*
	(0.010)	(0.015)
Constant	0.894*	-1.822*
	(0.089)	(0.155)
Observations	20100	27844
R^2 /*Pseudo* R^2	0.794	0.488

Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 11 and 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

CORRECTED Table A5: Appendix to Table 2: Difference-in-Difference, with fixed effects and Average treatment effects

	Fixed effects		Average treatment effect model	
	Like May	May best PM	Like May	May best PM
Manchester Attack	0.018	0.0024	0.021	0.002
	(0.028)	(0.0046)	(0.028)	(0.005)
Time	-0.501*	0.2286*	-0.592*	0.006*
	(0.023)	(0.0149)	(0.013)	(0.003)
Manchester Attack * Time	-0.127*	-0.0502*	-0.126*	-0.050*
	(0.019)	(0.0038)	(0.019)	(0.004)
Ethnically British	0.498*	0.1204*	0.485*	0.121*
	(0.044)	(0.0075)	(0.044)	(0.008)
Gender	0.452*	0.0116*	0.456*	0.012*
	(0.025)	(0.0043)	(0.025)	(0.004)
Labour	-2.473*	-0.4035*	-2.475*	-0.403*
	(0.042)	(0.0072)	(0.042)	(0.007)
Other Party ID	-2.079*	-0.2835*	-2.082*	-0.283*
	(0.034)	(0.0058)	(0.034)	(0.006)
Income	-0.006	0.0025*	-0.003	0.003*
	(0.004)	(0.0007)	(0.004)	(0.001)
Ideology	0.530*	0.0817*	0.525*	0.082*
	(0.007)	(0.0012)	(0.007)	(0.001)
Constant	1.944*	0.1999*	2.610*	0.222*
	(0.088)	(0.019)	(0.087)	(0.015)
Wave fixed effects	✓	✓		
Observations	143499	62095	143499	62095

Note: * p<.05. Wave 8-12 of the British Elections Study (Like May) and Wave 9-12 of the British Elections Study (May best PM).

Table A6: Bridge Attack and Manchester Attack and Views of May

	Like May	Like May	May Best PM	May Best PM
Surveyed after Bridge attack	-0.317*	-0.134*	-0.273*	-0.085
	(0.045)	(0.043)	(0.058)	(0.061)
Surveyed after Manchester attack		-0.300*		-0.299*
		(0.045)		(0.047)
Ethnically British	0.407*	0.414*	0.758*	0.768*
	(0.063)	(0.063)	(0.079)	(0.079)
Gender	0.442*	0.443*	-0.160*	-0.159*
	(0.042)	(0.042)	(0.036)	(0.036)
Labour	-3.014*	-3.006*	-2.735*	-2.735*
	(0.069)	(0.070)	(0.068)	(0.068)
Other Party ID	-2.410*	-2.413*	-2.209*	-2.221*
	(0.048)	(0.048)	(0.069)	(0.068)
Income	-0.015*	-0.015*	0.023*	0.024*
	(0.007)	(0.007)	(0.007)	(0.007)
Ideology	0.609*	0.609*	0.485*	0.486*
	(0.012)	(0.011)	(0.012)	(0.012)
Constant	3.131*	3.241*	-1.448*	-1.347*
	(0.133)	(0.128)	(0.136)	(0.120)
Observations	25780	25780	27089	27089
R^2 /*Pseudo* R^2	0.439	0.441	0.3267	0.3287

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

Appendix B: Random assignment tests

Table B1: Random Assignment evaluation

	Surveyed after Manchester attack
Ethnically British	0.001
	(0.044)
Gender	0.005
	(0.025)
Conservative	0.058
	(0.034)
Labour	0.115*
	(0.032)
Income	-0.001
	(0.004)
Ideology	-0.007
	(0.007)
How would you vote in another EU ref	-0.017
	(0.029)
Constant	-0.104
	(0.074)
Observations	25848
R^2	0.0006

Note: Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table B2: Manchester bombing does not shape views of non-related item or evaluations of May in time n-1 and n+1

	Support UK keeping Nuclear Submarines	Like May previous wave	Like May next wave
Surveyed after Manchester attack	0.030	-0.043	-0.024
	(0.022)	(0.046)	(0.044)
Ethnically British	0.325*	0.525*	0.397*
	(0.034)	(0.093)	(0.090)
Gender	-0.139*	0.503*	0.392*
	(0.014)	(0.046)	(0.045)
Labour	-0.152*	-2.647*	-2.959*
	(0.028)	(0.078)	(0.078)
Other Party ID	-0.299*	-2.230*	-2.250*
	(0.017)	(0.058)	(0.061)
Income	0.003	-0.009	-0.010
	(0.002)	(0.007)	(0.007)
Ideology	0.231*	0.628*	0.501*
	(0.004)	(0.013)	(0.013)
Constant	2.408*	3.001*	2.736*
	(0.045)	(0.144)	(0.148)
Observations	25746	20437	20973
R^2	0.215	0.417	0.387

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 11 and 12 of the British Elections Study. Ordinary least squares regression.

Table B3: Demographic Stability across Weeks of Wave 12

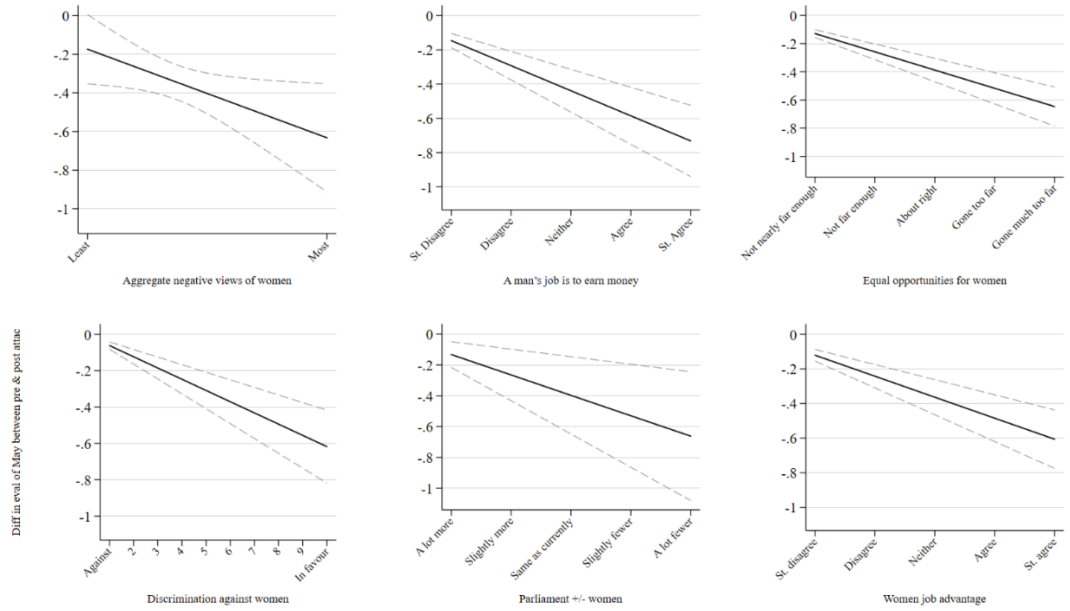
	% Ethnically British	% Women	% Conservative	% Labour	Income category	Ideology
Week 1	90%	52%	28%	27%	7.1	5.0
Week 2	90%	51%	28%	27%	7.0	5.0
Week 3	91%	52%	28%	28%	7.0	5.0
Week 4	91%	52%	28%	29%	7.1	5.0
Week 5	90%	52%	28%	29%	7.1	5.0

Note: Wave 12 of the British Elections Study.

Appendix C: Within group differences

Negative views of women.
We construct a scale (0-1) of the following questions
Gender Roles: How much do you agree or disagree with the following statement?
A man's job is to earn money, a woman's job is to look after the home and family
1 "St. Disagree" 2 "Disagree" 3 "Neither" 4 "Agree" 5 "St. Agree"
Asked in Wave 10
Equal Opportunities
Please say whether you think these things have gone too far or have not gone far enough in Britain
Attempts to give equal opportunities to women
1 "Not gone nearly far enough" 2 "Not gone far enough" 3 "About right" 4 "Gone too far" 5 "Gone much too far"
Asked every wave
Discrimination against women
How much discrimination is there for or against the following groups? Women
1 "A lot of discrim against" 10 "A lot of discrim in favour"
Asked in Wave 10
Women in Office
To what extent do you believe that more or fewer MPs in Parliament should come from the following backgrounds? To what extent do you believe that Parliament should have more or fewer MPs with the following background? Women
1 "A lot more" 2 "Slightly more" 3 "Same as currently" 4 "Slightly fewer" 5 "A lot fewer"
Asked in wave 6
Women's Jobs
How much do you agree or disagree with the following statement? Nowadays, women are given unfair advantages over men when applying for jobs
1 "St. disagree" 5 "St. agree"
Asked in wave 10

Figure C1: Effect of all gender views on Liking May



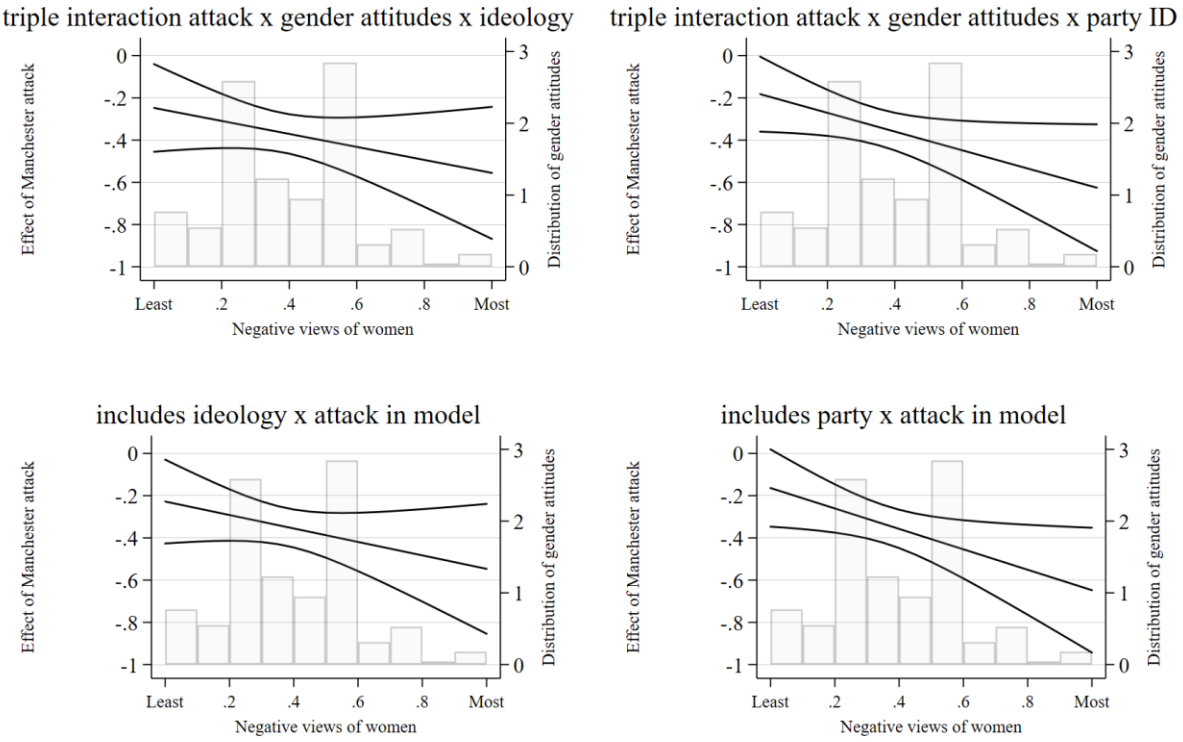
Note: Wave 10 and 12 of the British Elections Study.

Table C1: Manchester attack, Gender attitudes, and view of leaders (Figure 1)

	Like May	May Best PM
Surveyed after Manchester attack	-0.174	-0.450*
	(0.088)	(0.096)
Gender Attitudes	1.397*	0.439*
	(0.142)	(0.165)
Surveyed after Manchester attack * Gender Attitudes	-0.459*	-0.261
	(0.207)	(0.228)
Ethnically British	0.463*	0.738*
	(0.070)	(0.079)
Gender	0.523*	-0.123*
	(0.040)	(0.041)
Labour	-2.982*	-2.733*
	(0.068)	(0.071)
Other Party ID	-2.394*	-2.168*
	(0.045)	(0.061)
Income	-0.012	0.023*
	(0.007)	(0.007)
Ideology	0.579*	0.478*
	(0.011)	(0.015)
Constant	2.735*	-1.440*
	(0.131)	(0.129)
Observations	25823	26918
R^2 /*Pseudo* R^2	0.449	0.3341

Note: Standard errors in parentheses. Wave survey weights applied. * p<.05. Wave 10 and 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

Figure C2: Manchester attack, Gender attitudes, and view of leaders, with interactions for ideology and party



Note: Wave 10 and 12 of the British Elections Study.

Table C2: Party ID (full results from Figure 2)

	Like May Labour	Like May Conservatives	May best PM Labour	May best PM Conservatives
Surveyed after Manchester attack	-0.362*	-0.342*	-0.474*	-0.478*
	(0.074)	(0.053)	(0.076)	(0.103)
Ethnically British	0.154	0.613*	0.853*	0.629*
	(0.133)	(0.150)	(0.151)	(0.180)
Gender	0.200*	0.376*	-0.341*	0.136
	(0.074)	(0.054)	(0.076)	(0.102)
Income	0.003	-0.057*	0.026*	-0.006
	(0.012)	(0.009)	(0.012)	(0.018)
Ideology	0.625*	0.299*	0.440*	0.361*
	(0.022)	(0.018)	(0.023)	(0.032)
Constant	0.416*	5.495*	-3.814*	-0.334
	(0.172)	(0.217)	(0.213)	(0.300)
Observations	7589	7779	7775	7865
R^2 /*Pseudo* R^2	0.1738	0.0827	0.1074	0.0532

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 10 and 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

Table C3: Effect of Manchester Attack on Evaluations of May by Respondent Gender

	Men	Women
Surveyed after Manchester attack	-0.428*	-0.279*
	(0.056)	(0.053)
Ethnically British	0.338*	0.530*
	(0.106)	(0.110)
Labour	-2.773*	-3.216*
	(0.097)	(0.090)
Other Party ID	-2.410*	-2.421*
	(0.075)	(0.068)
Income	-0.020*	-0.010
	(0.009)	(0.009)
Ideology	0.631*	0.584*
	(0.017)	(0.015)
Constant	3.199*	3.699*
	(0.176)	(0.161)
Observations	13139	13367
R^2	0.4494	0.4339

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table C4: Effect of Manchester Attack on May's Likeability by Respondent Ideology (predicted effects by ideological placement)

	Coefficient	Standard Error
Left	-0.077	0.072
	-0.121*	0.061
	-0.165*	0.05
	-0.209*	0.041
	-0.252*	0.035
	-0.296*	0.032
	-0.340*	0.035
	-0.383*	0.041
	-0.427*	0.051
	-0.471*	0.061
Right	-0.515*	0.072

Note: * p<.05. Wave 12 of the British Elections Study.

Appendix D: Party reputations and assessment of other party leaders

Table D1: Comparative Manifesto Project Data on Issue Ownership (2017)

Party name	Law & Order	Militarism
Conservative Party	6.15	3.075
Labour Party	4.142	2.636
Liberal Democrats	2.653	2.476
Scottish National Party	2.414	1.525

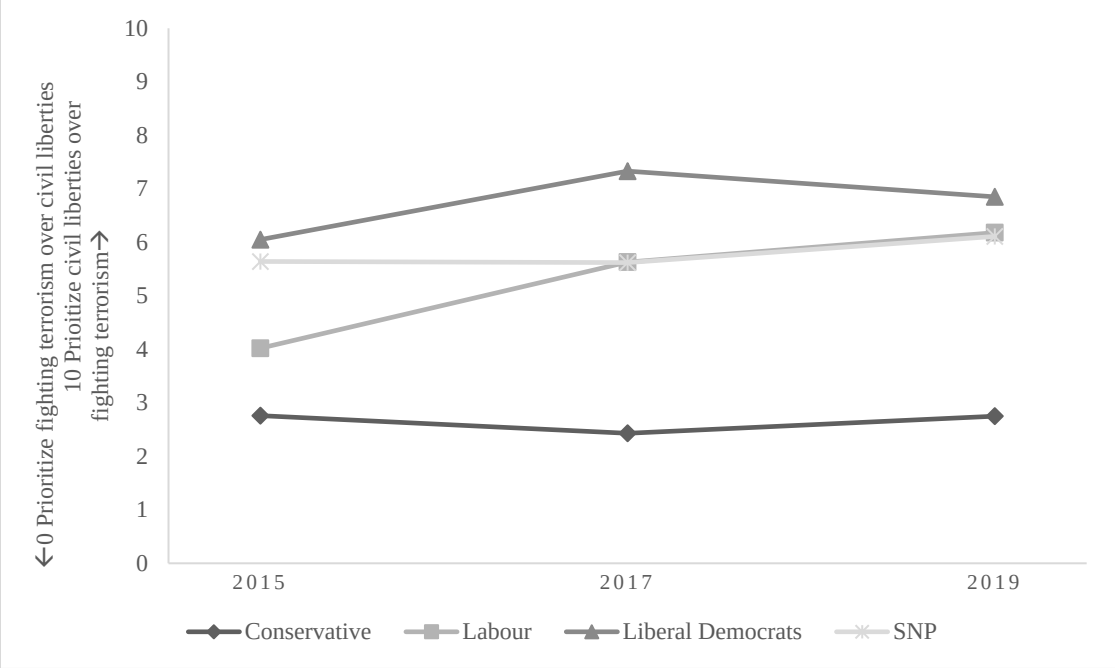
Note: Each variable provides "the share (percentage) of quasi sentences related to the focal category" in the party's manifesto documents. Data from Volkens, Andrea, Burst, Tobias, Krause, Werner, Lehmann, Pola, Matthieß Theres, Merz, Nicolas, Regel, Sven, Weßels, Bernhard, Zehnter, Lisa (2020): The Manifesto Data Collection. Manifesto Project (MRG/CMP/MARPOR). Version 2020b. Berlin: Wissenschaftszentrum Berlin für Sozialforschung (WZB).

Table D2: Mean ratings of Likeability of UK Parties, 1997, 2015, and 2015.

	1997	2005	2015
Conservatives	3.64	4.13	4.79
Labour	6.12	5.16	4.13

Note: CSES Integrated Module Dataset [IMD], (version December 8, 2020).

Figure D1: BES Expert Surveys of Party Reputations



Note: Data from BES Expert Surveys, 2015-2019; Question: Some people feel that, in order to fight terrorism, we have to accept infringements on privacy and civil liberties, others feel that privacy and civil liberties are to be protected at all costs. Please place the following parties on a scale where: 0 Fighting terrorism should always have priority over civil liberties & 10 Civil liberties should always have priority over fighting terrorism.

Table D3: Effect of Manchester Attack on Evaluations of All Party Leaders

	Like May	Like Corbyn	Like Farron	Like Sturgeon
Surveyed after Manchester attack	-0.351*	0.398*	0.059	0.115*
	(0.043)	(0.057)	(0.053)	(0.041)

Ethnically British	0.436*	-0.708*	-0.278*	-1.262*
	(0.065)	(0.083)	(0.083)	(0.078)
Gender	0.440*	0.191*	0.300*	0.330*
	(0.041)	(0.040)	(0.047)	(0.043)
Labour	-3.002*	2.769*	0.378*	0.886*
	(0.068)	(0.094)	(0.059)	(0.082)
Other Party ID	-2.413*	1.001*	0.486*	0.987*
	(0.046)	(0.063)	(0.057)	(0.056)
Income	-0.015*	-0.015*	0.071*	0.032*
	(0.007)	(0.006)	(0.007)	(0.007)
Ideology	0.608*	-0.528*	-0.212*	-0.520*
	(0.011)	(0.015)	(0.013)	(0.012)
Constant	3.227*	5.851*	3.894*	5.708*
	(0.125)	(0.165)	(0.125)	(0.135)
Observations	26506	26330	23285	25636
R^2	0.4415	0.3608	0.0639	0.2231

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table D4: Differences-in-Differences evaluation of all leaders, fixed effects

	May	Corbyn	Farron	Sturgeon
Manchester Attack *	-0.127*	0.136*	0.001	0.027
Time	(0.019)	(0.015)	(0.020)	(0.019)
	(0.019)	(0.015)	(0.020)	(0.019)
Manchester Attack	0.018	-0.043	0.034	-0.005
	(0.028)	(0.027)	(0.028)	(0.033)
Time	-0.501*	-1.364*	-0.292*	-0.696*
	(0.023)	(0.019)	(0.021)	(0.048)
Ethnically British	0.498*	-0.575*	0.047	-1.090*
	(0.044)	(0.046)	(0.046)	(0.054)
Gender	0.452*	0.221*	0.311*	0.229*
	(0.025)	(0.027)	(0.026)	(0.031)
Labour	-2.473*	2.384*	0.250*	0.651*
	(0.042)	(0.044)	(0.044)	(0.051)
Other Party ID	-2.079*	0.869*	0.389*	1.148*
	(0.034)	(0.036)	(0.036)	(0.042)
Income	-0.006	-0.024*	0.060*	0.025*
	(0.004)	(0.004)	(0.004)	(0.005)
Ideology	0.530*	-0.558*	-0.188*	-0.566*
	(0.007)	(0.007)	(0.007)	(0.009)
Constant	1.944*	6.019*	3.468*	6.353*
	(0.088)	(0.092)	(0.091)	(0.110)
Wave Fixed Effects	✓	✓	✓	✓
Observations	143499	180204	109869	103237

Note: * p<.05. Wave 8-12 of the British Elections Study. Ordinary least squares regression.

Table D5: Evaluations of leaders, including evaluations of likability from previous wave

	Like May	Like Corbyn	Like Farron	Like Sturgeon
Surveyed after	-0.334*	0.460*	0.056	0.087*
Manchester attack	(0.041)	(0.059)	(0.044)	(0.026)
Like May previous	0.790*			

wave				
	(0.008)			
Like Corbyn previous wave		0.788*		
		(0.009)		
Like Farron previous wave			0.725*	
			(0.008)	
Like Sturgeon previous wave				0.839*
				(0.006)
	(0.041)	(0.059)	(0.044)	(0.026)
Ethnically British	0.000	-0.110	-0.119*	-0.173*
	(0.050)	(0.064)	(0.053)	(0.048)
Gender	0.089*	0.056*	0.079	0.046
	(0.028)	(0.024)	(0.049)	(0.028)
Labour	-0.899*	0.863*	0.154*	0.129*
	(0.057)	(0.078)	(0.066)	(0.053)
Other Party ID	-0.647*	0.321*	0.145*	0.130*
	(0.045)	(0.036)	(0.050)	(0.041)
Income	-0.009	0.003	0.014*	-0.003
	(0.005)	(0.006)	(0.006)	(0.004)
Ideology	0.108*	-0.141*	-0.073*	-0.089*
	(0.010)	(0.012)	(0.012)	(0.011)
Constant	0.894*	1.338*	1.226*	1.051*
	(0.097)	(0.128)	(0.126)	(0.105)
Observations	20100	19915	16274	19288
R^2	0.7944	0.7439	0.5656	0.7718

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 11 and 12 of the British Elections Study. Ordinary least squares regression.

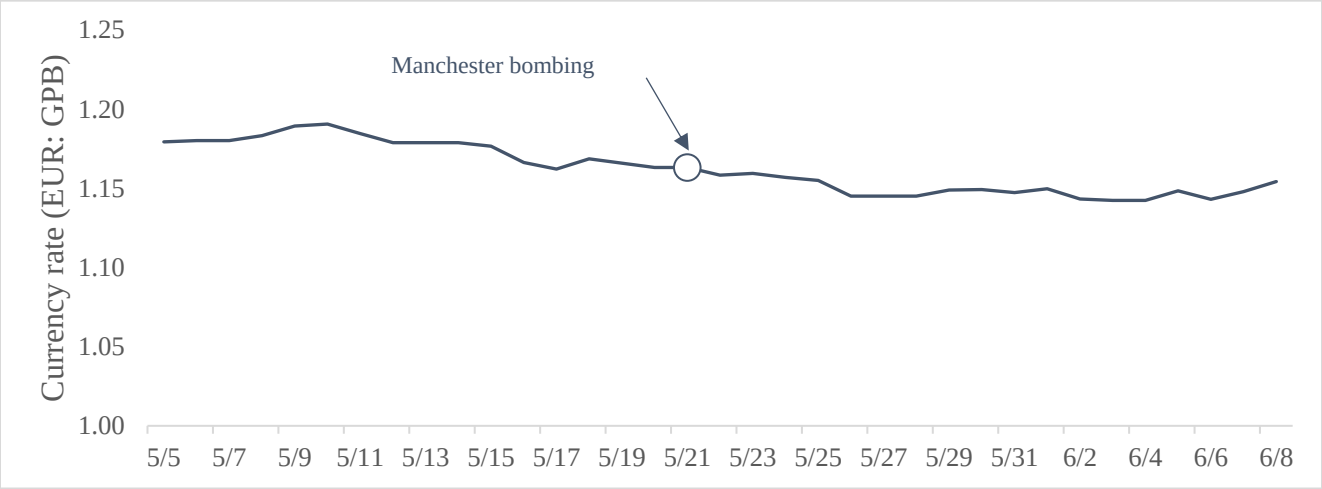
Appendix E: Temporal Stability

Table E1: Split control group at median and estimate effect on DVs

	Like May	May Best PM
Median of control group	-0.091	-0.104
	(0.058)	(0.065)
Ethnically British	0.359*	0.570*
	(0.117)	(0.118)
Gender	0.367*	-0.046
	(0.058)	(0.065)
Labour	-2.454*	-2.497*
	(0.106)	(0.121)
Other Party ID	-2.057*	-1.946*
	(0.082)	(0.105)
Income	-0.024*	0.016
	(0.009)	(0.011)
Ideology	0.687*	0.487*
	(0.018)	(0.022)
Constant	2.356*	-1.108*
	(0.215)	(0.242)
Observations	11040	11214
R^2/*Pseudo* R^2	0.491	0.3549

Note: Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression (Like May) and Logistical regression (May Best PM).

Figure E1: Average Currency Value Across the 2017 Election



Note: Values from OFX.com

Table E2: Table to Accompany Figure 3a – May likability w controls for economy, Brexit, and reducing bandwidth to 10 and 4 days

	Control for economy	Control for Brexit	10 day bandwidth	4 day bandwidth
Surveyed after Manchester attack	-0.166*	-0.360*	-0.270*	-0.178*
	(0.081)	(0.038)	(0.047)	(0.087)
Exchange rate	-9.336*			
	(3.458)			
How would you vote in another EU ref		1.582*		
		(0.046)		
Ethnically British	0.438*	0.179*	0.448*	0.437*
	(0.077)	(0.078)	(0.095)	(0.171)
Gender	0.440*	0.455*	0.412*	0.345*
	(0.039)	(0.038)	(0.048)	(0.087)
Labour	-3.003*	-2.765*	-2.977*	-3.020*
	(0.067)	(0.065)	(0.081)	(0.144)
Other Party ID	-2.426*	-2.221*	-2.434*	-2.485*
	(0.052)	(0.050)	(0.063)	(0.113)
Income	-0.015*	0.011	-0.013	-0.027
	(0.006)	(0.006)	(0.008)	(0.014)
Ideology	0.606*	0.483*	0.602*	0.573*
	(0.012)	(0.012)	(0.014)	(0.026)
Constant	11.186*	2.997*	3.215*	3.460*
	(2.946)	(0.121)	(0.153)	(0.281)
Observations	25760	25037	18012	5406
R^2	0.442	0.494	0.431	0.422

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table E3: Table to Accompany Figure 3b – May best PM w controls for economy, Brexit, and reducing bandwidth to 10 and 4 days

	Control for economy	Control for Brexit	10 day bandwidth	4 day bandwidth
Surveyed after Manchester attack	-0.193*	-0.411*	-0.267*	-0.139
	(0.082)	(0.043)	(0.048)	(0.087)
Exchange rate	-7.258*			
	(3.539)			
How would you vote in another EU ref		1.333*		
		(0.046)		
Ethnically British	0.796*	0.512*	0.876*	0.912*
	(0.077)	(0.084)	(0.096)	(0.165)
Gender	-0.167*	-0.136*	-0.173*	-0.203*
	(0.040)	(0.043)	(0.048)	(0.087)
Labour	-2.737*	-2.708*	-2.723*	-2.641*
	(0.070)	(0.073)	(0.084)	(0.151)
Other Party ID	-2.222*	-2.095*	-2.220*	-2.137*
	(0.060)	(0.064)	(0.073)	(0.130)
Income	0.023*	0.048*	0.025*	0.009
	(0.007)	(0.007)	(0.008)	(0.015)
Ideology	0.484*	0.400*	0.484*	0.494*
	(0.014)	(0.015)	(0.017)	(0.032)
Constant	4.833	-1.479*	-1.470*	-1.516*
	(3.016)	(0.144)	(0.166)	(0.295)
Observations	27059	25848	18904	5649
Pseudo R^2	0.3307	0.3831	0.3279	0.3205

Note: Clustered errors on day of survey. Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Logistical regression.

Table E4: Effect of Manchester attack on May Likability with Time-related controls

	Cluster on day	Cluster on day, with date controls	Interaction btw date fixed effect and Manchester attack	Multilevel model with clustered errors on day
Surveyed after Manchester attack	-0.351*	-0.095	-0.566*	-0.305*
	(0.043)	(0.078)	(0.004)	(0.043)
Ethnically British	0.436*	0.436*	0.433*	0.566*
	(0.065)	(0.064)	(0.063)	(0.054)
Gender	0.440*	0.441*	0.443*	0.366*
	(0.041)	(0.041)	(0.041)	(0.031)
Labour	-3.002*	-3.003*	-3.002*	-2.812*
	(0.068)	(0.068)	(0.068)	(0.051)
Other Party ID	-2.413*	-2.413*	-2.413*	-2.328*
	(0.046)	(0.047)	(0.047)	(0.042)
Income	-0.015*	-0.015*	-0.015*	-0.021*
	(0.007)	(0.007)	(0.007)	(0.005)
Ideology	0.608*	0.608*	0.608*	0.667*
	(0.011)	(0.011)	(0.011)	(0.008)
Constant	3.227*	320.233*	3.316*	2.764*
	(0.125)	(75.902)	(0.130)	(0.096)
Observations	26506	26506	26506	26506
R^2	0.4415	0.4420	0.4429	--

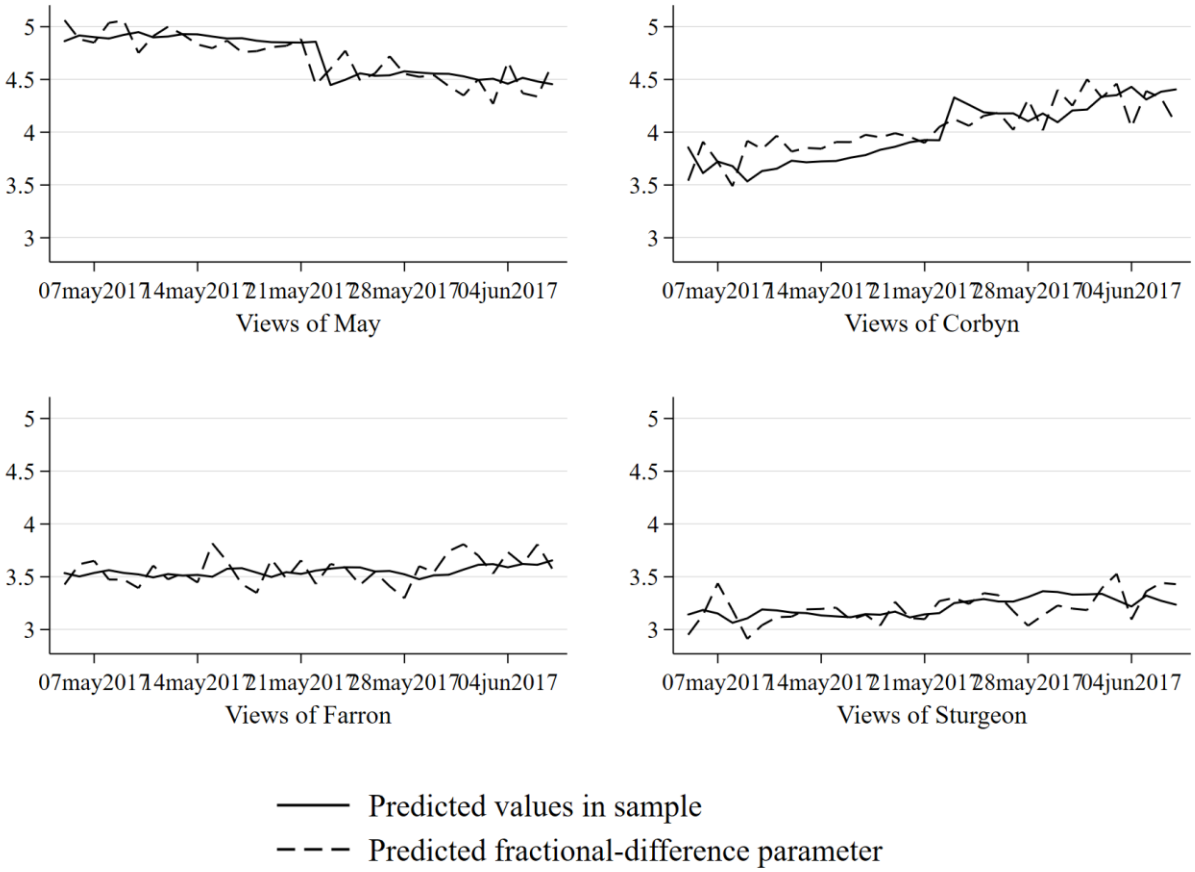
Note: Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Ordinary least squares regression.

Table E5: Effect of Manchester attack on May Best PM with Time-related controls

	Cluster on day	Cluster on day, with date controls	Interaction between date fixed effect and Manchester attack	Multilevel model with clustered errors on day
Surveyed after Manchester attack	-0.333*	-0.107	-0.562*	-0.050*
	(0.042)	(0.064)	(0.008)	(0.006)
Ethnically British	0.779*	0.781*	0.781*	0.114*
	(0.078)	(0.077)	(0.077)	(0.008)
Gender	-0.160*	-0.161*	-0.158*	-0.024*
	(0.035)	(0.035)	(0.035)	(0.005)
Labour	-2.730*	-2.733*	-2.737*	-0.445*
	(0.066)	(0.066)	(0.066)	(0.008)
Other Party ID	-2.211*	-2.214*	-2.216*	-0.352*
	(0.066)	(0.066)	(0.066)	(0.006)
Income	0.022*	0.022*	0.023*	0.002*
	(0.007)	(0.007)	(0.007)	(0.001)
Ideology	0.486*	0.486*	0.487*	0.079*
	(0.011)	(0.011)	(0.011)	(0.001)
Constant	-1.351*	278.334*	-1.250*	0.285*
	(0.115)	(69.896)	(0.112)	(0.014)
Observations	27844	27844	27839	27844
Pseudo R2	0.3293	0.3298	0.3308	

Note: Survey weights applied. * p<.05. Wave 12 of the British Elections Study. Logistical regression.

Figure E2: AFRIMA models of leader evaluations



Note: Wave 12 of the British Elections Study.

Appendix F: Experimental results

This study received approval from the Institutional Review Boards at Claremont Graduate University and Vanderbilt University and the protocols were consistent with APSA's Principle and Guidelines for Human Subjects Research (https://connect.apsanet.org/hsr/principles-and-guidance/).

Table F1. : OLS on Feeling Thermometers by Pooled Condition, IPSOS Study, 2012

	May	Cameron	Warsi	Clegg	Harman
Terror threat conditions	6.728*	5.839*	5.197*	5.113*	5.966*
	(2.225)	(2.427)	(2.451)	(2.288)	(2.233)
Constant	36.000*	36.931*	35.711*	35.817*	38.526*
	(1.799)	(1.978)	(1.974)	(1.868)	(1.806)
Observations	462	563	384	558	451
R^2	0.01949	0.01021	0.01163	0.00890	0.01565

Note: Beta coefficients listed with standard errors in parentheses. *p<.05. Treatments available from the authors.

Appendix G: Geographic effects

Table G1: Distance to Manchester & Change in Conservative Party Vote

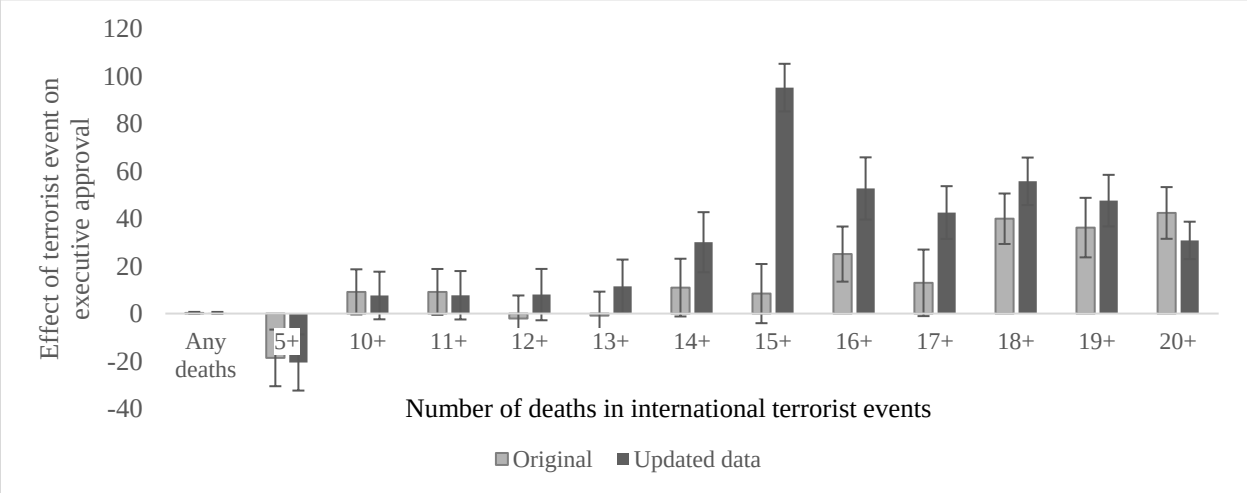
	Conservative vote change, 2015-2017	Conservative vote change, 2010-2015
Distance to Manchester	0.00405*	-0.00028
	(0.00170)	(0.00191)
population	-0.00001	0.00000
	(0.00002)	(0.00002)
Share of population over 65	-0.18522*	0.13591*
	(0.06053)	(0.06823)
Ethnicity - White	-0.03880	-0.05646^
	(0.02949)	(0.03324)
Country of birth - UK	-0.06263	0.01853
	(0.03999)	(0.04507)
Religion - Christian	0.03155	0.03971
	(0.02636)	(0.02971)
Unemployed	-0.81318*	1.22733*
	(0.14804)	(0.16686)
Vote for Brexit	-30.67845***	1.55994
	(1.89188)	(2.13227)
Constant	24.91640*	-8.76532*
	(3.18002)	(3.58409)
Observations	532	532
R^2	0.695	0.222

Note: Standard errors in parentheses. * p<.05. Ordinary least squares regression.

Appendix H: Global Analysis

As the rally literature largely evaluates the effects of terrorist attacks on immediate or near-immediate attitudes towards the chief executive or using single case studies, we first engage in an inductive approach for identifying the threshold of deaths that might produce a rally. We do so by estimating a series of models that regress presidential approval on international[1] terrorist attacks, varying the threshold of the number of deaths in the event. Because rally events are conceptualized as large, shocking events, we are interested in the effect of an attack overall, not the number of attacks nor the effect of the number of deaths in the attacks. Figure H1 presents the coefficients of the effect of the terrorist attack on presidential approval (measured in the next quarter) from these models. As Figure H1 shows, the presence of *any* international terrorist attack is not associated with an increase in presidential approval, nor are attacks with lower casualty counts. Indeed, it is not until the casualty count exceeds 13 deaths that we see a reliable positive relationship with executive approval. After that point, however, a terrorist event with a high casualty count is associated with an increase in executive approval in the next quarter. We thus use this threshold in our global analysis. Our framework applies to lead executives. In most countries, there is one head of government (e.g., in presidential systems and in constitutional monarchies like the UK where the prime minister is the head of government). In some countries, such as semi-presidential systems like France, both the prime minister and the president hold roles that can be considered chief executive positions; in our analyses we include approval of both these individuals when both can be considered governing executives.

CORRECTED Figure H1: Effect of terrorist events on executive approval



Note: Coefficients from linear regression with panel-corrected standard errors using time-series cross-sectional data. Dependent variable is lagged approval rating from the Executive Approval Database. Terrorism event data from the Global Terrorism Database. Controls for the presence of a female head of state, GDP, Inflation (logged), Executive ideology, and election in that quarter, with country fixed effects. Error bars are panel-corrected standard errors.

[1] We use the Global Terrorism Database classification of any attack that involved an international component.

CORRECTED Table H1: Gender-Revised Rally Effects

	Lagged approval
Int'l terrorist attack 14+ death	2.122*
	(0.724)
Woman head of state	1.323
	(0.770)
Int'l terrorist attack 14+ death * Woman head of state	-3.918*
	(1.796)
Growth in real GDP	$6.49e^{-08}$
	($1.149e^{-06}$)
Growth in real GDP $_{t-1}$	$-1.13e^{-07}$
	($1.49e^{-06}$)
ln(Inflation)	-0.227
	(0.213)
ln(Inflation$_{t-1}$)	-0.093
	(0.211)
Presidential Election	2.942*
	(0.259)
Presidential Election $_{t+1}$	2.345*
	(0.315)
Presidential Election $_{t+2}$	1.760*
	(0.329)
Presidential Election $_{t+3}$	1.260*
	(0.311)
Presidential Election $_{t+4}$	0.733*
	(0.253)
Right Ideology	-0.623
	(0.971)
Center Ideology	-0.691
	(1.110)
Left Ideology	0.072
	(1.009)
Constant	47.044*
	(2.884)
Observations	4637
R^2	0.29968

Note: Panel-corrected standard errors in parentheses. Country fixed effects. * p<.05

CORRECTED Table H2: Changing Thresholds and the Gender-revised Rally Effects

	8+ deaths	10+ deaths	12+ deaths	14+ deaths	16+ deaths	18+ deaths
Terrorist event	0.987	1.088	1.415*	2.122	2.113*	2.543*
	(0.602)	(0.642)	(0.701)	(0.724)	(0.743)	(0.802)
Woman head of state	1.330	1.291	1.300	1.323	1.323	1.287
	(0.772)	(0.770)	(0.769)	(0.770)	(0.770)	(0.769)
Terrorist event * woman	-1.847	-1.279	-1.957	-3.918*	0.407	1.953

head of state						
	(1.343)	(1.945)	(2.488)	(1.796)	(4.214)	(2.619)
Constant	47.049*	47.058*	47.060*	47.044*	47.045*	47.102*
	(2.886)	(2.885)	(2.885)	(2.884)	(2.884)	(2.881)
Observations	4637	4637	4637	4637	4637	4637
R^2	0.2987	0.2984	0.2990	0.2997	0.2994	0.2999

Linear regression using time-series cross-sectional data of country-quarters. Controls for the presence of a female head of state, GDP, Inflation (logged), the left-center-right placement of the leader, and election in that year, with country fixed effects. Panel-corrected standard errors in parentheses. Dataset includes all countries in the executive approval database that also appear in the Global Terrorism Database (N countries = 66). Dependent variable is executive approval. Standard errors in parentheses. * p<.05

CORRECTED Table H3: Interactive effects with Executive Ideological Placement

	Lagged approval
Int'l terrorist attack 14+ death	-0.795
	(0.958)
Woman head of state	-0.954
	(0.904)
Int'l terrorist attack 14+ death * Woman head of state	-6.012*
	(1.971)
Right	0.404
	(0.999)
Center	-0.184
	(1.269)
Left	0.764
	(1.073)
Right Ideology * Int'l terrorist attack 15+ death	5.889*
	(1.888)
Center Ideology * Int'l terrorist attack 15+ death	5.969*
	(2.557)
Left Ideology * Int'l terrorist attack 15+ death	2.167
	(1.876)
Constant	46.445*
	(2.947)
Observations	4194
R^2	0.51531

Linear regression using time-series cross-sectional data of country-quarters. Controls for GDP, Inflation (logged), and election in that year, with country fixed effects. Panel-corrected standard errors in parentheses. * p<.05