Partisan Bias in Bipartisan Places? A Field Experiment Measuring Attitudes Toward the Presidential Alert In Real Time

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Abstract

An extensive literature examines how partisanship divides public opinion on hot-button political issues, but we know little about its potential to polarize attitudes about bipartisan issues. Recent work shows that while Americans hold strong preferences for bipartisanship, their attitudes toward bipartisan issues quickly become polarized when associated with partisan identities. While prior research has examined the effect of these associations in lab settings, tests outside of the lab are far more rare. In this research note we aim to provide such a test by leveraging a bipartisan issue that became associated with a partisan identities suddenly in 2018: the presidential alert. While the presidential alert—a product of bipartisan efforts to improve the government's capacity to send emergency communications in the wake of Hurricane Katrina—received little notice when it was passed into law, it gained widespread media attention during its inaugural test in 2018. We rapidly recruited a sample of U.S. adults immediately before the alert was sent, such that participants in our study received the alert on their phones while completing the survey. We exploited the timing of the alert to randomize whether respondents answered questions about the alert moments before or after receiving it. Across two experiments we find little evidence that associating the alert with the Trump administration had any polarizing effect on attitudes, even when explicitly associated with a partisan cue, suggesting that at least some bipartisan attitudes are not as easily polarized as prior work implies.

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While a large body of work documents the growing partisan divide on hot-button political issues in the American public (Hetherington 2001; Layman and Carsey 2002; Abramowitz and Saunders 2008), we know far less about how partisanship affects attitudes towards less salient bipartisan issues. Yet it is consensus on these issues that enables even divided governments to fulfill their most basic roles of providing goods and services to the public. Issues that Americans agree on are less visible in the media, but they are no less prevalent. As Kahan et al. (2017, pg. 3) note, "In any modern, liberal pluralistic democracy, the number of cases in which individuals of diverse identities polarize are swamped by the number in which they do not." Moreover, despite being polarized on issues like abortion and immigration, Americans appear to disdain overly partisan conflict on less salient consensus issues (Flynn and Harbridge 2016), punish excessive party loyalty (Carson et al. 2010), and reward bipartisanship that produces legislation (Paris 2017).

However, other recent work suggests that the increasingly partisan nature of politics threatens to polarize even the least partisan issues. Americans are more dismissive of policy proposals offered by the opposing party and their support for traditionally non-partisan democratic values wanes when the out-party gains power (Svolik 2018; Hetherington and Rudolph 2015; Bartels and Johnston, Forthcoming). Growing alignment between partisan and social identities produces animus and distrust across party lines (Huber and Malhotra 2017; Hetherington and Weiler 2018; Mason 2016; Iyengar, Sood, and Lelkes 2012) and can motivate individuals to adopt positions as a means of expressively signaling their identity and worldview to others (Kahan et al. 2017; Johnston, Lavine, and Federico 2017). Kahan et al. (2017) find that even bipartisan policies aimed at a public health concern, such as Zika virus, can become polarized when associated with existing political identities and worldviews. Such influence is concerning, as an erosion of consensus issues threatens to create an environment in which the government lacks public support to provide even the most basic goods and services (Hetherington and Rudolph 2015).

It is possible, however, that recent empirical work has overstated the potential for asso-

ciations with partisan identities to polarize even the most consensus issues. In prior work, such associations are drawn suddenly in a lab setting—respondents are assigned to receive information creating an artificial association between a non-polarized issue with a political identity (e.g., Kahan et al. 2017). It may be that such associations induce demand effects or operate differently outside of the lab, and to our knowledge there has been no work examining how such processes occur outside of the lab. This is largely due to the difficulty of experimentally manipulating associations between issues and partisan identities that, in the real-world, develop gradually (Hillygus and Shields 2008) and cannot easily be assigned to a randomized subset of individuals.

In this study we provide such a test by leveraging a previously non-partisan issue that became associated with a partisan identity suddenly in 2018: the inaugural test of the presidential alert system. At 2:18pm ET on October 3rd, 2018 Americans' phones buzzed, vibrated, and displayed text notifications reading "Presidential Alert: THIS IS A TEST of the National Wireless Emergency Alert System. No action is needed." The presidential alert meets the two criteria laid out by Kahan et al. (2017) to measure measure the potential of bipartisan issues to become polarized: that public opinion toward the issue is both not presently polarized and has the potential to become polarized. The presidential alert was the product of a bipartisan endeavor to improve the government's emergency communication infrastructure in the aftermath of September 11 and Hurricane Katrina. Legislation creating the alert under the Bush administration in 2006 and updating it under the Obama administration in 2016 received nearly unanimous support in the House and Senate.[1] And while the initial legislation received scarce media attention, coverage preceding the alert's inaugural test depicted an intensely partisan reaction. Headlines warned of a barrage of text messages from Trump, calls to protest the alert lit up Twitter with hashtags encouraging Americans to shut off their phones or cancel their wireless plans, and critics even filed lawsuits to terminate the alert altogether. As The Atlantic synopsized in a headline on the day of the alert,

1. congress.gov/bill/109th-congress/house-bill/4954

"What should have been a routine, required national test of the Wireless Emergency Alerts system has become a crucible for public distrust"(Bogost 2018).

To understand how partisanship shaped reactions to the alert, we conducted a national survey experiment during the alert itself. Survey experiments measuring the effect of real-world events on attitudes are scarce due to the difficulty of both rapidly collecting a sufficiently large sample and timing the experiment to coincide with the event. In this study we address both of these challenges. First, we rapidly collected a large sample of U.S. adults in a narrow time interval preceding the alert by simultaneously recruiting online survey respondents from multiple panels. We then exploited the predetermined timing of the alert to randomize whether respondents answered questions related to the alert immediately before or after receiving it, enabling us to measure the extent to which receiving the alert affected attitudes differently across party lines. This design mimics the random assignment of partisan cues to issues that has been featured in lab studies while diminishing potential demand effects and using a naturally-occurring manipulation. Participants who received the alert before expressing attitudes toward it had a greater chance of associating the alert with the current administration than those who received the alert afterwards. In a second experiment included in the same real-time survey, respondents were randomly assigned to receive information explicitly associating the alert with either the Trump or Obama administration.

Design

To capture the public's immediate reaction to the alert in real-time, we required a sample of respondents to begin the survey during a brief time window immediately preceding the alert. We partnered with Lucid, which unlike most online survey firms that recruit respondents from a single panel, simultaneously made the survey available to respondents from multiple panels. This enabled us to rapidly collect a sample of 2,224 U.S. adults in the 25 minutes preceding the alert.[2] Recent research finds that samples drawn from Lucid closely match

2. This sample size reflects the number of respondents in our final analytical sample. Since compliance was dependent on 1) assignment of experimental condition, 2) when respondents completed certain question

the demographic and political characteristics of the U.S. population, replicate experimental findings, and feature a pool of respondents who are less professionalized and politically sophisticated than other non-probability panels (Coppock and McClellan 2019). Upon entering the survey, respondents were randomly assigned to participate in one of two separate experiments.

Experiment 1 was designed to assess whether receiving the presidential alert had any immediate effect on respondents' attitudes. While the alert was a bipartisan issue prior to its inaugural test, it is likely that receiving the alert (which began with "PRESIDENTIAL ALERT") established associations between the alert and the highly polarizing Trump administration. Because the ideal experimental design—manipulating which Americans actually received the alert—is impossible, we make a similar comparison by randomly assigning respondents to answer questions about the alert and privacy either moments before or after receiving the alert. After answering a common set of pre-treatment questions, respondents who were randomly assigned to the *pre-alert condition* answered a series of political attitude questions related to the alert, while respondents in the *post-alert condition* answered unrelated, non-political questions until the moment the alert was sent.[3]

We programmed the survey instrument to interact with the timing of the alert, such that approximately 30 seconds after the alert was sent respondents in the post-alert condition finished answering these unrelated questions and began answering questions related to the alert.[4] Excluding respondents who failed 2 or more of the 3 attention checks, there were

blocks, and 3) the timing of the alert, we recruited more individuals than this to ensure that the number of compliant cases in each condition was sufficiently large (i.e., we analyze respondents who answered the questions they were assigned to answer *when they were assigned to answer them*, rather than analyzing intent to treat (ITT) effects). See Appendix Section 3 for further discussion and Section 6.8 ITT and CACE estimates.

3. The unrelated non-political questions were designed to hold respondents' attention until the alert was sent without influencing their attitudes toward any of the attitude outcomes. The items comprise primarily of consumer behavior questions and are discussed further in the Appendix Sections 6.2 and 8.2.

4. We took several precautions to diminish demand effects. First, the survey invitation and consent form omitted any mention of the alert, instead advertising a survey "about yourself and your opinions and attitudes." Second, questions related to the alert were immediately preceded by political knowledge questions in all conditions, creating a brief buffer between the alert and questions about it. Third, the questions specifically asking about the alert were placed at the very end of a larger battery of questions about privacy and trust in government (we discuss these questions below).

392 respondents in the pre-alert condition and 547 respondents in the post-alert condition in Experiment 1 (See Appendix Section 2 for a detailed discussion of the attention checks). The attitude questions that respondents were randomly assigned to answer either before or after receiving the alert measured opinions about the alert specifically and privacy attitudes, since much of the media coverage preceding the alert focused on privacy violations. Respondents were asked whether "the government should have the ability to send alert messages to all cell phones in the U.S. in the case of a national threat or emergency," whether Americans should be able to opt-out of receiving such alerts, and whether they themselves would opt-out if given the opportunity. With regard to privacy, respondents were asked whether the need to be safe from national emergencies and disasters justifies giving up some privacy, how worried they are about the government monitoring their activities or invading their privacy, and how difficult it would be to increase phone privacy.[5]

It is possible that simply receiving the alert was not sufficient to associate the alert with President Trump. Therefore, we designed a second experiment within the same survey to assess whether directly associating the alert with either Democrats or Republicans influenced the same set of attitudes. All participants in Experiment 2 answered attitude questions after the alert (similar to respondents in the post-alert condition in Experiment 1), but were randomly assigned to receive information either emphasizing Trump's role in testing the alert (*Trump condition*), Obama's role in creating the alert (*Obama condition*), or neither (*Control condition*):

> On Wednesday, October 3, [The Trump Administration will test a system (Trump condition)/ there will be a test of a system created under the Obama Administration (Obama condition) / there will be a test of a system (Control condition)] that will allow the government to send a message to every cell phone in the U.S., using FEMA's mobile alert system. Even though the system was created to alert people about national emergencies, there has been concern that Americans have no way to opt out of the alert.

To conceal our intent of providing an informational treatment and mitigate demand effects,

5. Respondents also answered questions about trust in government and attitudes toward specific political parties and figures. We describe these items and include an analysis of them in Appendix Section 7.

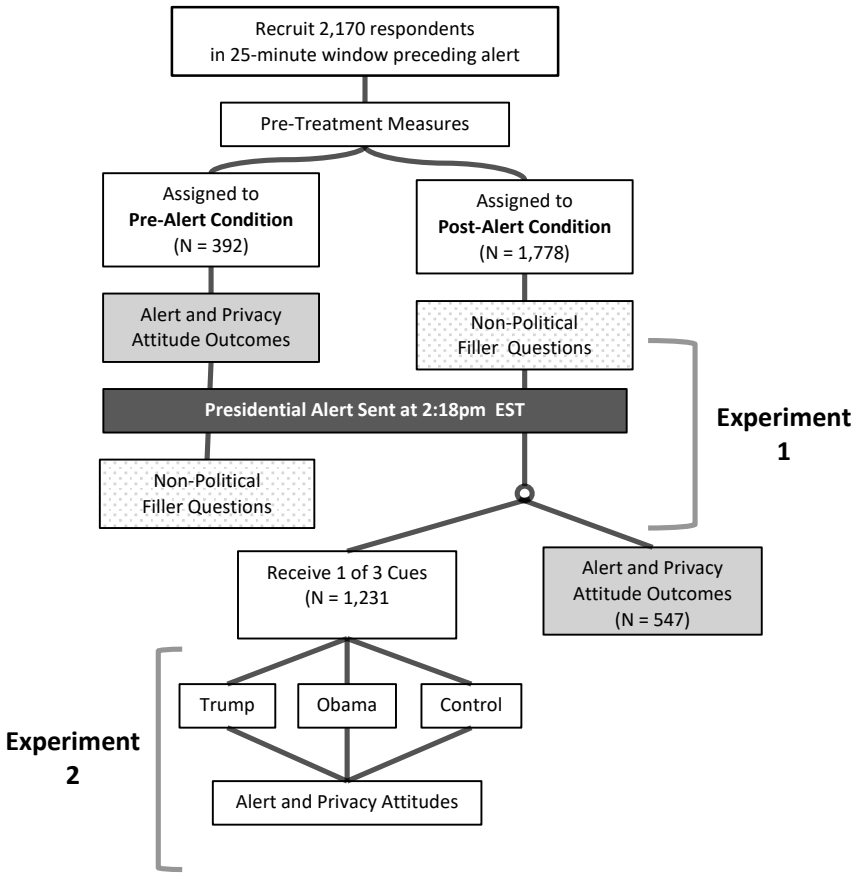

Figure 1: Study Design

Respondents recruited immediately before the alert are randomly assigned to participate in one of two experiments. In Experiment 1 respondents are randomly assigned to answer outcome attitude measures either moments before or after the alert was sent. In Experiment 2 respondents answer the same questions after the alert was sent, but are randomly assigned to receive information associating the alert with either Trump or Obama.

the information was provided in the form of a question asking whether respondents had heard of this information. Information provided in each condition was factually accurate, simply highlighting different aspects of the alert's origin. After excluding respondents who failed two or more of the attention checks, the analytical sample consists of 442 respondents in the control condition, 403 in the Obama condition, and 386 in the Trump condition. Figure 1 provides an overview of the experimental design.

Results

To what extent did partisanship influence the public's immediate reaction to the presidential alert? Figure 1 reports the mean difference in attitudes toward the alert and privacy between conditions in Experiment 1. Responses to the attitude questions were placed on the same scale, on whichm 0 indicates feeling the least concerned about the alert and privacy and 1 indicates feeling the most concerned, and then standardized to have a mean of 0 and standard deviation of 1. We report pre-post alert differences by party, considering Independents who lean toward a party as partisans.[6] While prior research suggests that associating bipartisan issues with partisan identities polarizes attitudes, we find little evidence that receiving the alert elicited a partisan reaction. In Experiment 1, differences between attitudes in the pre- and post-alert conditions are centered around and do not differ significantly from zero. In fact positive attitudes toward the government's ability to send an emergency alert trend upwards after the alert across parties, though these differences are not statistically significant.

One possible explanation for the lack of partisan reactions to the alert is that partisan media coverage of the alert had *already* polarized attitudes by the time the alert was sent. Indeed, as we show in Appendix Section 6.1., there is some evidence that respondents with prior knowledge of the alert were more polarized in their pre-alert attitudes. To test for the possibility of heterogeneous treatment effects across prior knowledge of the alert, we compared the difference in pre- and post-alert attitudes between respondents who reported having heard about the alert before they received it and those who heard about it for the first time upon receiving it.[7] Models including interactions between the treatment and prior awareness of the alert are included in the Appendix (Section 6.4), however we find no evidence that respondents who learned about the alert for the first time upon receiving it had a more partisan reaction to the alert.[8]

6. Considering leaning Independents as Independents does not change our results, see Appendix Section 6.3.

7. In Experiment 1, 47% of respondents had reported being aware of the alert prior to receiving it.

8. We also considered, but found no evidence of, heterogeneous treatment effects among the 62% of respondents who reported receiving the alert during the survey (Appendix Section 6.5).

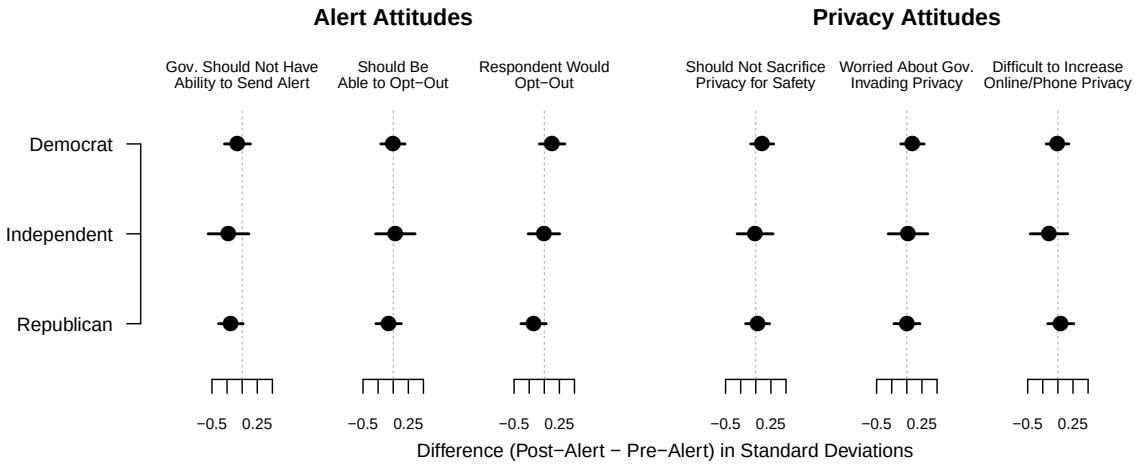

Figure 2: Difference Between Pre- and Post-Alert Attitudes (Experiment 1)

Mean attitude differences between experiment conditions in standard deviations. Each attitude measure was placed on a common scale from 0 (less concerned about alert and privacy) to 1 (more concerned), and then standardized to have a mean of 0 and standard deviation of 1. Independents who report leaning toward a political party are classified as partisans. Horizontal lines represent 95% confidence intervals. Differences with confidence intervals that do not contain zero are statistically significant at the p ¡ .05 level.

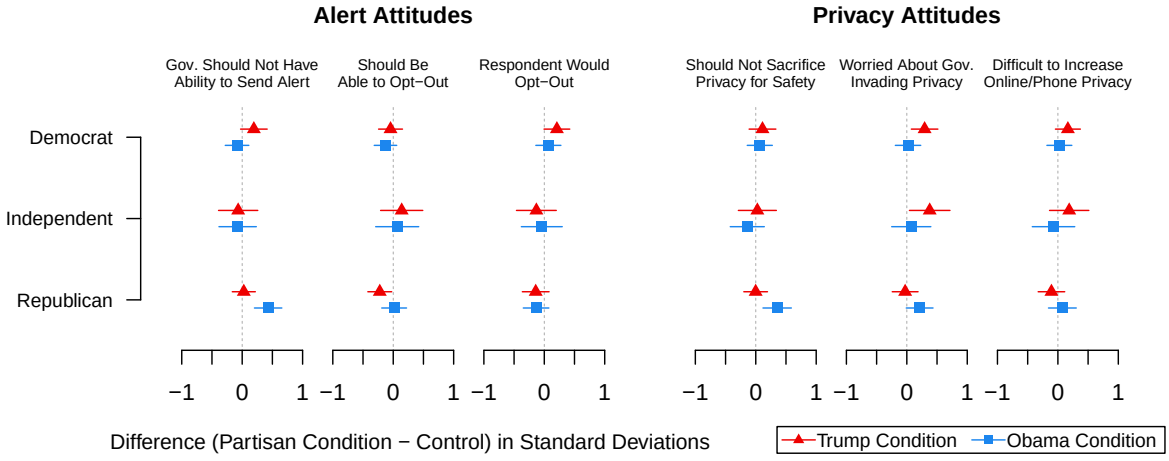

Figure 3: Effect of Party Cues on Attitudes (Experiment 2)

Average treatment effects of associating Trump or Obama with the alert (compared to the control group). Attitude measures were again placed on a common scale from 0 (less concerned about alert and privacy) to 1 (more concerned) and then standardized to have a mean of 0 and standard deviation of 1. Horizontal lines represent 95% confidence intervals. Differences with confidence intervals that do not contain zero are statistically significant at the p ¡ .05 level.

Our second experiment was designed to determine whether receiving partisan cues associating the alert with either Trump or Obama induced a partisan reaction. In other words, does exposure to the kind of partisan information contained in media coverage preceding the alert polarize attitudes? Figure 2 illustrates the differences in attitudes between the Trump and Obama conditions relative to the control ($Attitude_{Trump} - Attitude_{control}$, $Attitude_{Obama} - Attitude_{control}$) separately for respondents in each party. Overall, we find little evidence that these cues polarized attitudes toward the alert and privacy. Of the 36 treatment effects reported in Figure 3, only 5 are statistically significant (p ¡ .05).

For instance, the Obama cue increased Republican's opposition to the government having the ability to send the alert by .43 standard deviations and claim that privacy should not be sacrificed for safety by The largest effects were among Republicans who received the Obama cue, whose opposition to the government having the ability to send the alert was .43 standard deviations more than and more likely to indicate that privacy should not be sacrificed for safety (X vs. Y, p ¡ .05).

Republicans who received the Trump cue were less likely to say that people should be able to opt out (p ¡ .05), while Democrats (and Independents) who received the Trump cue appeared more worried about government invading privacy. However, even these effects are at most .5 standard deviation shift from the mean.[9] Overall, despite the explicit nature of the cues associating the alert with each political party, they had a surprisingly small effect on attitudes toward the alert and privacy.

Discussion

Americans are increasingly divided along partisan lines on everything from policy preferences to lifestyle choices. In this study we sought to understand the extent to which these partisan divisions can polarize even the most bipartisan issues. We designed an experiment that leveraged the timing of a real-world event that associated a previously bipartisan is-

9. For reference, lab studies have found that in-party cues can shift support for something as enduring as political values by up to 10 percentage points (e.g., Goren, Federico, and Kittilson 2009).

sue with the Trump administration. In Experiment 1 we randomly assigned respondents to report relevant political attitudes either moments before or after the test of the inaugural presidential alert. We find that attitudes toward the alert and privacy did not diverge across partisan lines in response to the alert.

It may be that, for some issues, attitudes only polarize after extensive exposure to divisive elite rhetoric (Zaller et al. 1992; Druckman, Peterson, and Slothuus 2013). The key feature of our design that enables us to measure real-time attitudes toward the alert also prevents us from assessing how elites may have influenced attitudes afterwards. While Experiment 2 explicitly associated the alert with partisan cues, it is possible that these cues are insufficient to polarize attitudes on a bipartisan issue like the alert. At the very least, this suggests that the policies aimed at public health and safety are unlikely to trigger immediate partisan reactions merely because they are implemented by the out-party. Yet it is possible that such reactions do polarize in the presence of more pronounced party divisions at the elite level.

The design implemented here–the rapid recruitment of a large sample of survey respondents and a survey instrument programmed to interact with the timing of a political event–enables the measurement of real-time changes in public opinion. Though the presidential alert was unique in it's precise timing and widespread reach, this design could be used by leveraging the approximate time of other political events, such as the the release of election results, debates, or the release of economic data (e.g., unemployment reports).

In all, while the media coverage and response on social media suggested that the alert would prompt partisan backlash, any immediate partisan division over the alert appeared minimal. Future work might evaluate whether these findings extend to other bipartisan policy areas, such as public health risks (e.g., vaping, nuclear energy), pandemics, disaster relief, and publicly-funded scientific research. Of course, the findings presented here by no means suggest that America's hyper-partisan environment is not cause for concern, but they do appear to place bounds on the influence partisanship has over the public's support for the most bipartisan issues and policies.

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Online Appendix for Partisan Bias in Bipartisan Places? A Field Experiment Measuring Attitudes Toward the Presidential Alert

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1 Online Appendix Overview

This document contains additional explanations of the survey experiment, information about the sampling process and sample, and supplementary analyses. The R code needed to replicate all analyses contained here and in the manuscript are included in a version of this markdown file that will be posted to Dataverse (along with the data) upon publication.

2 Data Quality & Attention Checks

Due to our use of an online non-probability sample, we included three attention checks designed to identify respondents who sped through survey without paying attention. The first two attention checks were placed in the beginning of the survey and were designed to mitigate the chance of a respondent recognizing that their attention was being measured, while the third was more explicit and appeared at the end of the survey. The first item asked respondents how many times a week (0 days, 1 day, or more than 1 day) they engaged in the following activities: eaten dinner, gone geocaching, flown a helicopter, run a marathon, used a computer. We make the assumption that respondents will not have engaged in more than one of the three low-incidence activities (flown a helicopter, run a marathon, and gone geocaching) within the same week and flag respondents who reported doing so as potential shirkers. The second attention check was a multiple choice question asking respondents to identify the current president of the United States from the following options: Theresa May, Paul Ryan, Donald Trump, John Stillerman, and Don't Know. Respondents who did not answer Donald Trump were flagged as potential shirkers. The third attention check was the most obvious and was placed at the end of the survey to avoid the negative effects of participants realizing they are being watched. The question read: "Some people fail to read surveys carefully, and simply click through. To show you do not do this, simply ignore the response options below and mark 'definitely true." ' 80.14% of respondents passed all of the attention checks, 13.68% failed only 1, 5.08% failed 2, and 1.10% failed 3. We exclude the 5.18% of respondents who failed 2 or more attention checks from the primary analysis.

3 Study Design & Experimental Conditions

Because both Experiments 1 and 2 were designed to interact with the timing of the presidential alert, the assignment of experimental conditions depends both on the condition respondents were assigned to (intent-to-treat conditions) and the timing of when they began and completed specific sets of questions.

Upon entering the survey, respondents were immediately assigned a random value `condition` between 1 and 25 (inclusive), which indicated both the experiment and experimental condition to which they were assigned.

If `condition` = [1:8], respondents answered the attitudinal question block *before* answering holding questions. If `condition` = [9:25], respondents answered the attitudinal question block *after* answering holding questions.

3.1 Experiment 1

Experiment 1 was designed to measure the difference between respondents' attitudes toward the alert immediately before and after the alert. Some respondents answered political attitude questions early in the survey, while some answered them later in the survey. Respondents were randomly assigned to one of two conditions:

- **Pre-alert Condition**: Respondents answered a block of political attitude questions earlier in the survey (`condition` = 1:8)
- **Post-alert Condition**: Respondents answered a block of political attitude questions later in the survey (`condition` = 9:16)

The intent was for respondents in the Control condition to answer the political attitude questions before the alert was sent, and for respondents in the Treatment condition to answer the attitude questions after the alert was sent. This assignment is represented by the intent-to-treat variable `exp1_itt`.

```
# create intent-to-treat variable for Experiment 1 condition (treatment = 1, control = 0)
data$exp1_itt <- ifelse(data$condition <= 8, "pre_alert",
                    ifelse(data$condition >= 9 & data$condition <= 16, "post_alert", NA))

# Assign NA values if respondent began survey before distributed (e.g., test surveys)
# and/or if began survey after alert was sent
data$exp1_itt[data$timer_start_dem_all < start_recruit_exp1] <- NA
data$exp1_itt[data$timer_start_dem_all > cut_time] <- NA
```

Because respondents entered the survey at different times, some respondents in each condition did not answer the attitude questions when we intended for them to. Respondents who were assigned to the pre-alert condition were supposed to complete the attitude questions before the alert (`cut_time`), but some did not (e.g., respondents who didn't finish the attitude questions before the alert was sent). Likewise, respondents assigned to the post-alert condition were supposed to complete the attitude questions after the alert (e.g., respondents who finished all holding questions before the alert was sent).

Therefore, we created the final experimental condition variable `exp1_condition` that excludes these non-compliers (non-compliers are assigned an NA value for the `exp1_condition` variable).

Unlike `exp1_itt`, `exp1_condition` represents respondents in the pre-alert condition who actually completed attitude questions before the alert, and respondents in the post-alert condition who began attitude questions after the alert. Of the 757 respondents assigned to the pre-alert condition, 417 finished the attitude questions before the alert. Of the 741 respondents assigned to the post-alert condition, 577 began the attitude questions after the alert. This results in a final sample of 994 respondents in Experiment 1. Of these respondents, 94% passed at least two of the three attention checks, resulting in a total N of 931 for Experiment 1 (385 in pre-alert condition, 546 in post-alert condition).

```
data$exp1_condition <- ifelse(data$exp1_itt == "pre_alert" &
                                    data$timer_end_DV_pre < cut_time, "pre_alert",
                          ifelse(data$exp1_itt == "post_alert" &
                                   data$timer_start_DV_post >= cut_time, "post_alert",
                                 NA))
```

3.2 Experiment 2

Experiment 2 was designed to measure how perceived responsibility for the alert affects the same set of political attitude items used in Experiment 1. All respondents in Experiment 2 received the attitude questions later in the survey (just like respondents in the Post-Alert Condition in Experiment 1). However, just before answering these attitude questions, respondents saw one of 3 messages about the alert:

- **No-Blame Condition**: Respondents were given the information about the date and purpose of the alert. (`condition` = [17-19])
- **Obama Condition**: In addition to receiving information about the date and purpose of the alert, Obama's role in the alert was emphasized. (`condition` = [20-22])
- **Trump Condition**: In addition to receiving information about the date and purpose of the alert, Trump's role in the alert was emphasized. (`condition` = [23-25])

```
# Create intent to treat variable for experiment 2
data$exp2_itt <- ifelse(data$condition >= 17 & data$condition <= 19, "noblame",
                          ifelse(data$condition >= 20 & data$condition <= 22, "obama",
                            ifelse(data$condition >= 23 & data$condition <= 25, "trump",
                                   NA)))
```

However, just as in Experiment 1, some respondents did not receive the treatment as intended. We define compliers as respondents who received the information about the alert after the alert was sent out. In order to capture the immediate response to the alert, we made the decision to remove exclude respondents who began the survey more than 30 minutes before the alert was sent out.

```
data$exp2_condition <- as.factor(ifelse(data$timer_start_DV_post >= cut_time,
                                        data$exp2_itt,
                                        NA))

# remove respondents who started survey more than 30 minutes after alert was sent out
data$exp2_condition[data$timer_start_dem_all >
                      paste(substr(current_time, 1, 10),
```

```
                              " 14:48:00 EDT",
                              sep = "")] <- NA
```

4 New Numbers for Editor

```
# numbers for Experiment 1 footnotes (editor revisions)----------------------

addmargins(table(data$exp1_itt)) # 741, 757, 1498
```

```
##
## post_alert  pre_alert        Sum
##        741        757       1498
```

```
addmargins(table(data$exp1_condition)) # 577, 417, 994
```

```
##
## post_alert  pre_alert        Sum
##        577        417        994
```

```
addmargins(table(data$exp1_condition[data$fail_count < 2])) # 547, 392, 939
```

```
##
## post_alert  pre_alert        Sum
##        547        392        939
```

```
# percent of exp1_condition respondents who passed attention checks
939/994 # 94.47
```

```
## [1] 0.944668
```

```
addmargins(prop.table(table(data$exp1_condition, data$fail_count < 2),1))
```

```
##
##                   FALSE       TRUE        Sum
##   post_alert 0.05199307 0.94800693 1.00000000
##   pre_alert  0.05995204 0.94004796 1.00000000
##   Sum        0.11194511 1.88805489 2.00000000
```

```
# 94.8%
# 94.0%


# numbers for Experiment 2 footnotes (editor revisions)----------------------

addmargins(table(data$exp2_itt)) # 531, 502, 488, 1521
```

```
##
```

```
## noblame   obama   trump     Sum
##     531     502     488    1521
```

```
addmargins(table(data$exp2_condition)) #471, 429, 415, 1315
```

```
##
## noblame   obama   trump     Sum
##     471     429     415    1315
```

```
addmargins(table(data$exp2_condition[data$fail_count < 2])) # 442, 403, 386, 1231
```

```
##
## noblame   obama   trump     Sum
##     442     403     386    1231
```

```
# percent of exp2_condition respondents who passed attention checks
1231/1315 # 93.61
```

```
## [1] 0.9361217
```

```
addmargins(prop.table(table(data$exp2_condition, data$fail_count < 2),1))
```

```
##
##                FALSE       TRUE        Sum
##   noblame 0.06157113 0.93842887 1.00000000
##   obama   0.06060606 0.93939394 1.00000000
##   trump   0.06987952 0.93012048 1.00000000
##   Sum     0.19205670 2.80794330 3.00000000
```

```
# 93.84
# 93.94
# 93.01
# 2.81


# total number of respondents recruited:
1498 +1521  # 3019
```

```
## [1] 3019
```

```
# total number of respondents who met timing requirements:
994 + 1315 # 2309
```

```
## [1] 2309
```

```
# total number of respondents who passed timing and attention checks:
939  + 1231 # 2,170
```

```
## [1] 2170
```

```
# completion rate-------------------------------------

# among recruited respondents
prop.table(table(data$finished_closing[!is.na(data$exp1_itt) | !is.na(data$exp2_itt)], useNA = "always"
```

```
##
##         1      <NA>
## 0.8638622 0.1361378
```

```
# among respondents in final analytical sample
prop.table(table(data$finished_closing[!is.na(data$exp1_condition) | !is.na(data$exp2_condition)], useN
```

```
##
##          1       <NA>
## 0.92334344 0.07665656
```

4.1 Balance Tables for Experiments 1 & 2

We provide a breakdown of the demographic and political characteristics of our final analytical sample below. Each demographic and political characteristic is a binary indicator with the exception of age and income group. Income was measured on a 13-point scale with the following response categories: 1 = Less than $10,000, 2 = $10,000 to $19,999, 3 = $20,000 to $29,999, 4 = $30,000 to $39,999, 5 = $40,000 to $49,999, 6 = $50,000 to $59,999, 7 = $60,000 to $69,999, 8 = $70,000 to $79,999, 9 = $80,000 to $89,999, 10 = $90,000 to $99,999, 11 = $100,000 to $119,999, 12 = $120,000 to $149,999, 13 = Over $150,000. All demographic questions were included in the pre-treatment question block, with the exception of race and income, which were included at the very end of the survey.

	Exp. 1 Mean	SE	Exp. 2 Mean	SE
Age	44.54	0.53	44.61	0.46
Female	0.68	0.02	0.69	0.01
Income	6.19	0.11	6.08	0.10
White	0.77	0.01	0.74	0.01
Black	0.10	0.01	0.11	0.01
Latino	0.06	0.01	0.07	0.01
Other	0.07	0.01	0.08	0.01
Democrat	0.42	0.02	0.44	0.01
Independent	0.18	0.01	0.16	0.01
Republican	0.40	0.02	0.39	0.01
Liberal	0.32	0.02	0.31	0.01
Moderate	0.36	0.02	0.36	0.01
Conservative	0.32	0.02	0.33	0.01

Below we report demographic and political differences between experimental conditions for Experiments 1 and 2. Since Experiment 2 contained three separate conditions, we make separate comparisons between 1) the Trump and control conditions and 2) the Obama and control conditions. For each demographic and political variable in the comparisons we include a within-condition mean and standard deviation, as well as the p-value from a two-sample t test across conditions. In Experiment 1, differences for all demographic and

political characteristics are small and not significantly different from 0. In Experiment 2, there are far more respondents who identify as "Other Race" in the control (No Blame) condition than the Obama condition (13% vs. 7%), though relatively equal proportions of the other racial groups. The Obama condition also contained less Democrats than the control condition, though because we present our results by party this difference is not problematic for the purposes of this study.

Table 1: Experiment 1 Balance Table

	Pre-Alert		**Post-Alert**		
	Mean	SE	Mean	SE	p
Age	43.57	0.80	45.23	0.70	0.12
Female	0.69	0.02	0.67	0.02	0.71
Black	0.10	0.02	0.10	0.01	0.84
Latino	0.06	0.01	0.06	0.01	0.93
White	0.76	0.02	0.77	0.02	0.74
Other Race	0.08	0.01	0.06	0.01	0.47
Income	6.36	0.18	6.07	0.15	0.23
Democrat	0.43	0.03	0.42	0.02	0.85
Republican	0.39	0.02	0.40	0.02	0.73
Independent	0.18	0.02	0.17	0.02	0.85
Liberal	0.33	0.02	0.32	0.02	0.71
Conservative	0.32	0.02	0.32	0.02	0.94
Moderate	0.35	0.02	0.36	0.02	0.66

Table 2: Experiment 2 Balance Table (Trump vs. No Blame Condition)

	Trump		**No Blame**		
	Mean	SE	Mean	SE	p
Age	44.80	0.85	44.98	0.75	0.87
Female	0.69	0.02	0.69	0.02	0.91
Black	0.12	0.02	0.12	0.02	0.98
Latino	0.07	0.01	0.07	0.01	0.86
White	0.75	0.02	0.75	0.02	0.94
Other Race	0.06	0.01	0.07	0.01	0.79
Income	5.88	0.18	6.14	0.18	0.33
Democrat	0.43	0.03	0.41	0.02	0.55
Republican	0.41	0.03	0.40	0.02	0.90
Independent	0.16	0.02	0.19	0.02	0.34
Liberal	0.32	0.02	0.31	0.02	0.85
Conservative	0.33	0.02	0.33	0.02	0.80
Moderate	0.36	0.02	0.36	0.02	0.94

Table 3: Experiment 2 Balance Table (Obama vs. No Blame Condition)

	Obama		**No Blame**		
	Mean	SE	Mean	SE	p
Age	44.03	0.79	44.98	0.75	0.38
Female	0.69	0.02	0.69	0.02	0.95
Black	0.09	0.01	0.12	0.02	0.21
Latino	0.07	0.01	0.07	0.01	0.70
White	0.71	0.02	0.75	0.02	0.27
Other Race	0.12	0.02	0.07	0.01	0.01
Income	6.22	0.18	6.14	0.18	0.74
Democrat	0.49	0.02	0.41	0.02	0.01
Republican	0.37	0.02	0.40	0.02	0.26
Independent	0.14	0.02	0.19	0.02	0.07
Liberal	0.30	0.02	0.31	0.02	0.82
Conservative	0.33	0.02	0.33	0.02	0.82
Moderate	0.37	0.02	0.36	0.02	0.66

5 Description of Main Experimental Outcomes

5.1 Alert Attitudes

Respondents were asked how much they agree or disagree with the following 2 statements (original coding: 1 = strongly disagree, 6 = strongly agree):

1. The government should have the ability to send alert messages to all cell phones in the U.S. in the case of a national threat or emergency. [`anti_alert`, reversed coded]
2. People should be able to out-out of receiving alert messages like this. [`opt_out_gen`]

Respondents were also asked:

3. Would you personally choose to opt out of receiving these alert messages? [`opt_out_you`]

- Yes, No, Unsure

All 3 attitude items were recoded such that positive values represent negative attitudes toward the alert (i.e., disagreeing that the government should have the ability to sent the alert, agreeing that people should be able to opt-out of receiving the alert, and indicating that they themselves would opt-out).

5.2 Privacy Attitudes

1. Some believe that Americans need to be willing to give up privacy in order to be safe from national emergencies and disasters. Others believe that Americans shouldn't have to give up privacy in order to be safe from national emergencies. How much do you agree or disagree with the following statement?: Americans need to be willing to give up some privacy in order to be safe from national emergencies and disasters.' ' [`privacy_safety`]

- Strongly disagree, moderately disagree, slightly disagree, neither agree nor disagree, slightly agree, moderately agree, strongly agree

2. How worried are you about the US government monitoring your activities or invading your privacy? [`gov_monitor`]

- Very worried, Somewhat worried, Not too worried, Not at all worried

3. If you wanted to be more private while you were using the Internet or your cell phone, how easy do you think it would be for you to find tools and strategies that would help you?

- Extremely easy, very easy, somewhat easy, somewhat difficult, very difficult, extremely difficult

All items were recoded such that positive values reflect concern over privacy (e.g., disagreeing that Americans should have to give up some privacy).

6 Effect Sizes for Main Experimental Results

In the tables below we report the statistics that are plotted in Figures 2 and 3 of the manuscript.

6.1 Experiment 1

	Party	Outcome	Pre Mean	Post Mean	Mean Diff.	95% CI
1	Democrat	Gov. Should Not Send Alert	0.14	0.06	-0.08	(-0.29,0.14)
2	Independent	Gov. Should Not Send Alert	0.17	-0.05	-0.23	(-0.56,0.11)
3	Republican	Gov. Should Not Send Alert	-0.04	-0.23	-0.19	(-0.39,0.01)
4	Democrat	Should Be Able to Opt-Out	-0.01	-0.02	-0.00	(-0.21,0.2)
5	Independent	Should Be Able to Opt-Out	0.07	0.11	0.03	(-0.29,0.36)
6	Republican	Should Be Able to Opt-Out	0.05	-0.03	-0.08	(-0.29,0.13)
7	Democrat	R Would Opt-Out	-0.00	0.13	0.13	(-0.08,0.34)
8	Independent	R Would Opt-Out	-0.21	-0.21	-0.01	(-0.27,0.25)
9	Republican	R Would Opt-Out	0.08	-0.10	-0.18	(-0.39,0.03)
10	Democrat	Should Not Sacrifice Privacy	0.08	0.18	0.11	(-0.08,0.3)
11	Independent	Should Not Sacrifice Privacy	0.46	0.45	-0.01	(-0.31,0.29)
12	Republican	Should Not Sacrifice Privacy	-0.04	-0.01	0.03	(-0.17,0.23)
13	Democrat	Worried about Privacy	0.02	0.11	0.09	(-0.1,0.29)
14	Independent	Worried about Privacy	0.05	0.07	0.02	(-0.31,0.35)
15	Republican	Worried about Privacy	0.01	0.01	0.00	(-0.21,0.22)
16	Democrat	Difficult to Increase Privacy	-0.03	-0.04	-0.01	(-0.19,0.18)
17	Independent	Difficult to Increase Privacy	0.25	0.10	-0.15	(-0.46,0.16)
18	Republican	Difficult to Increase Privacy	-0.02	0.03	0.05	(-0.17,0.26)

Table 4: Experiment 1

6.2 Experiment 2

	Party	Outcome	No Blame Mean	Obama Mean	Mean Diff.	95% CI
1	Democrat	Gov. Should Not Send Alert	0.05	-0.03	-0.08	(-0.28,0.11)
2	Independent	Gov. Should Not Send Alert	0.07	-0.00	-0.08	(-0.38,0.23)
3	Republican	Gov. Should Not Send Alert	-0.24	0.19	0.43	(0.2,0.66)
4	Democrat	Should Be Able to Opt-Out	0.03	0.05	0.02	(-0.18,0.23)
5	Independent	Should Be Able to Opt-Out	0.03	-0.04	-0.07	(-0.42,0.28)
6	Republican	Should Be Able to Opt-Out	-0.02	0.06	0.07	(-0.16,0.3)
7	Democrat	R Would Opt-Out	-0.02	0.04	0.07	(-0.14,0.27)
8	Independent	R Would Opt-Out	-0.14	-0.29	-0.14	(-0.42,0.14)
9	Republican	R Would Opt-Out	-0.14	0.22	0.35	(0.12,0.59)
10	Democrat	Should Not Sacrifice Privacy	-0.20	-0.18	0.02	(-0.19,0.23)
11	Independent	Should Not Sacrifice Privacy	-0.19	-0.12	0.07	(-0.25,0.4)
12	Republican	Should Not Sacrifice Privacy	-0.37	-0.16	0.21	(-0.01,0.43)
13	Democrat	Worried about Privacy	0.16	0.03	-0.13	(-0.31,0.06)
14	Independent	Worried about Privacy	-0.16	-0.09	0.07	(-0.29,0.42)
15	Republican	Worried about Privacy	-0.06	-0.04	0.02	(-0.19,0.22)
16	Democrat	Difficult to Increase Privacy	-0.08	-0.01	0.07	(-0.14,0.28)
17	Independent	Difficult to Increase Privacy	0.09	0.05	-0.04	(-0.38,0.3)
18	Republican	Difficult to Increase Privacy	0.17	0.04	-0.14	(-0.35,0.08)

Table 5: Experiment 2: Obama Cue

	Party	Outcome	No Blame Mean	Trump Mean	Mean Diff.	95% CI
1	Democrat	Gov. Should Not Send Alert	0.05	0.25	0.19	(-0.03,0.41)
2	Independent	Gov. Should Not Send Alert	0.07	0.01	-0.07	(-0.39,0.26)
3	Republican	Gov. Should Not Send Alert	-0.24	-0.22	0.03	(-0.16,0.22)
4	Democrat	Should Be Able to Opt-Out	0.03	0.19	0.17	(-0.04,0.37)
5	Independent	Should Be Able to Opt-Out	0.03	0.22	0.19	(-0.14,0.51)
6	Republican	Should Be Able to Opt-Out	-0.02	-0.12	-0.11	(-0.33,0.11)
7	Democrat	R Would Opt-Out	-0.02	0.09	0.11	(-0.11,0.33)
8	Independent	R Would Opt-Out	-0.14	-0.12	0.03	(-0.29,0.34)
9	Republican	R Would Opt-Out	-0.14	-0.14	-0.00	(-0.2,0.19)
10	Democrat	Should Not Sacrifice Privacy	-0.20	0.10	0.29	(0.07,0.51)
11	Independent	Should Not Sacrifice Privacy	-0.19	0.19	0.38	(0.04,0.71)
12	Republican	Should Not Sacrifice Privacy	-0.37	-0.40	-0.03	(-0.24,0.19)
13	Democrat	Worried about Privacy	0.16	0.11	-0.04	(-0.24,0.15)
14	Independent	Worried about Privacy	-0.16	-0.02	0.14	(-0.21,0.49)
15	Republican	Worried about Privacy	-0.06	-0.28	-0.22	(-0.42,-0.03)
16	Democrat	Difficult to Increase Privacy	-0.08	0.13	0.21	(0,0.42)
17	Independent	Difficult to Increase Privacy	0.09	-0.04	-0.13	(-0.46,0.2)
18	Republican	Difficult to Increase Privacy	0.17	0.03	-0.14	(-0.36,0.08)

Table 6: Experiment 2: Trump Cue

7 Robustness Checks

7.1 Attitude Polarization Prior to Alert

In Section 6.5 of this appendix we examine the possibility that prior awareness of the alert moderates the effect of receiving the alert on attitudes. We would not expect receiving the alert (and associating it with the Trump administration) to have as strong of an effect on attitudes among those whose attitudes were already polarized by the media and elites. Here we consider whether attitudes were in fact polarized among those with prior knowledge of the alert.

To begin, we report mean attitude responses among respondents in the pre-alert condition by prior awareness of the alert and party. Although we expect prior awareness of the alert to primarily influence attitudes toward the alert, we include privacy attitudes as well. The means and 95% confidence intervals are reported in Figure 1 below. As expected, we find that attitudes toward the alert are more polarized among those who reported prior awareness of the alert. We observe this relationship for two of the three alert attitudes (general opposition to the alert and belief that people should be allowed to opt out of the alert), but not for whether the respondent themselves would opt out of the alert or any of the privacy attitudes.

Additionally, to measure differential polarization across prior levels of awareness, we modeled attitudes toward the alert with an interaction between prior awareness of the alert. One potential issue is the possibility that our measure of whether respondents had prior awareness of the alert captures both prior awareness of the alert and respondents' interest in politics and attention to the news. Given that we care about whether respondents had prior awareness of the alert, we also ran models that included controls for interest in politics and news consumption.

The results from these models are included in Table 7 below. The interaction coefficients are positive and statistically significant (alpha = .05) for one outcome (opposition to the alert) in the models that do not control for political interest and media consumption, and positive and statistically significant for two outcomes (opposition to the alert and supporting the ability to opt-out of the alert) when controlling for these variables. Together, we interpret these results as suggestive that attitudes toward the alert were polarized by prior awareness of the alert.

These results are helpful in interpreting the absence of a heterogeneous treatment effect across levels of prior awareness of the alert (Section 6.5 of this appendix). This absence could reflect that either media coverage of the alert did not polarize attitudes prior to the survey to begin with or that those whose attitudes were polarized by prior media coverage did not respond differently to the alert than those who were not exposed to prior media coverage. The results presented here suggest that, at least for attitudes toward the alert, the latter explanation is more likely.

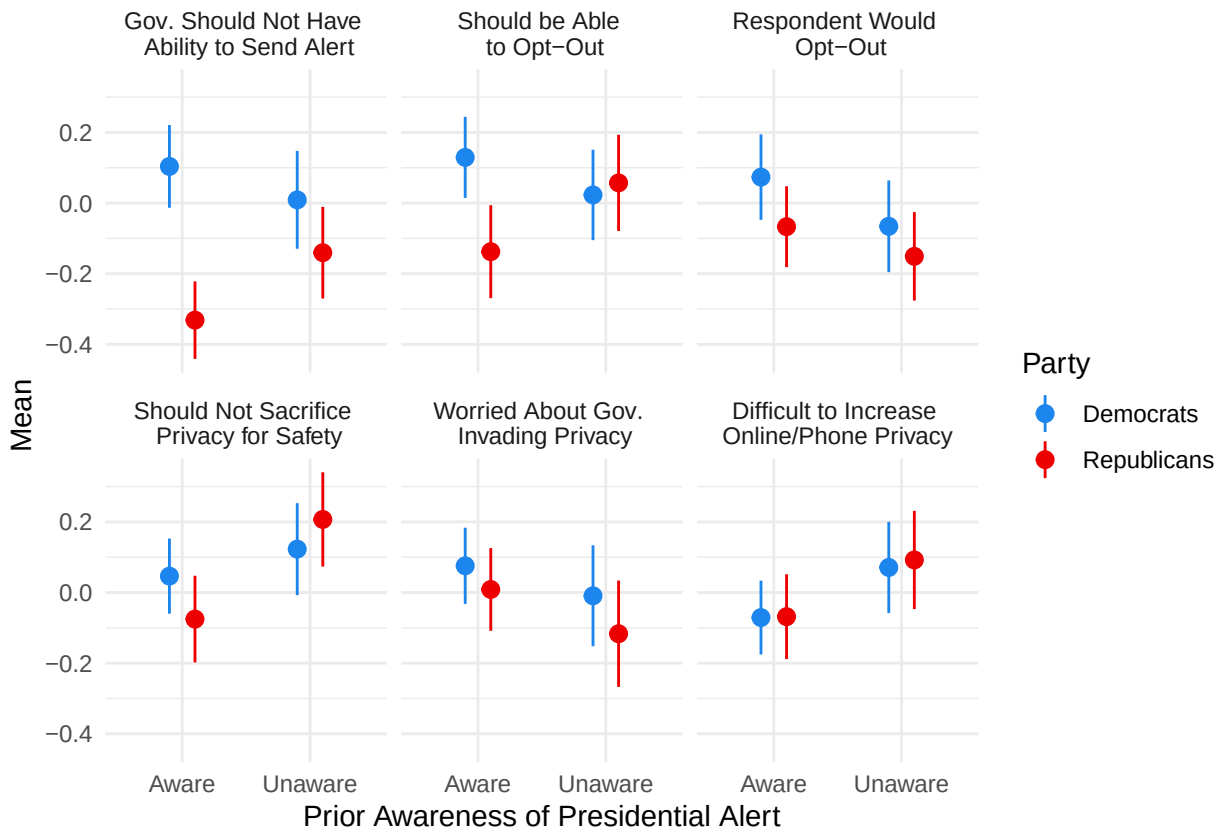


Figure 1: Average Attitudes Prior to Alert

Table 7: Prior Alert Awareness X Party

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Prior Awareness of Alert	−0.18*	−0.19*	−0.19*	−0.21*	0.46	0.33	0.17	0.13	−0.17	−0.13	−0.24**	−0.22*
	(0.09)	(0.09)	(0.09)	(0.09)	(0.24)	(0.24)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Dem. (vs. Rep.)	0.19*	0.16	−0.07	−0.08	0.22	0.13	0.15	0.13	−0.02	0.01	−0.02	−0.03
	(0.10)	(0.10)	(0.10)	(0.10)	(0.26)	(0.26)	(0.10)	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)
Attention to Politics		0.08*		0.06		0.54***		0.15***		−0.14***		−0.02
		(0.04)		(0.04)		(0.10)		(0.04)		(0.04)		(0.04)
News Consumption		−0.06**		−0.02		−0.17***		−0.04*		0.05**		−0.01
		(0.02)		(0.02)		(0.05)		(0.02)		(0.02)		(0.02)
Prior Awareness X Dem.	0.24	0.27*	0.31*	0.31*	0.03	0.07	−0.09	−0.08	−0.01	−0.02	0.14	0.15
	(0.13)	(0.13)	(0.13)	(0.13)	(0.32)	(0.33)	(0.13)	(0.13)	(0.12)	(0.12)	(0.12)	(0.13)
Constant	−0.13	−0.17	0.06	−0.07	−1.67***	−2.79***	−0.15*	−0.46***	0.06	0.31**	0.15*	0.25*
	(0.07)	(0.11)	(0.07)	(0.11)	(0.19)	(0.31)	(0.07)	(0.11)	(0.07)	(0.11)	(0.07)	(0.11)
Observations	1,012	1,011	1,011	1,010	1,011	1,010	1,012	1,011	1,012	1,011	1,012	1,011
R^2	0.03	0.04	0.01	0.01			0.01	0.02	0.01	0.02	0.01	0.01
Adjusted R^2	0.03	0.04	0.01	0.01			0.004	0.02	0.005	0.02	0.01	0.01
Log Likelihood					−527.71	−510.33						
Akaike Inf. Crit.					1,063.41	1,032.65						

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

7.2 Difference in Attitudes Based on Time of Completion (Pre-Alert Condition Only)

Because we randomize whether respondents answer attitude questions before or after the alert, we avoid making the assumption that people who enter the survey before and after the alert are identical, which comes close to the ideal experimental design of randomly assigning only some individuals to receive the alert. This requires making the assumption that the non-political filler questions did not influence attitudes toward the alert. Because we continued our data collection after the alert was sent, we are able to test this assumption with a subset of our respondents. We examined only respondents who were assigned to answer the attitude questions before the alert (and, therefore, before the filler questions). Among these respondents, 394 answered the attitude questions before the alert was sent and 814 after the alert was sent.

Following the same approach used in the main analysis, we compare the responses of those who answered the questions before and after the alert below. Overall, differences between attitude questions answered before and after the alert largely reflect those reported in the main experimental analysis. We observe no statistically significant differences among Democrats. Among Independents, two privacy attitudes come close to statistical significance, but point in opposite directions. For Republicans, there are only statistically significant differences for two of the six outcomes: believing that the government should not have the ability to send the alert and stating that they would opt out of the alert if given the chance.

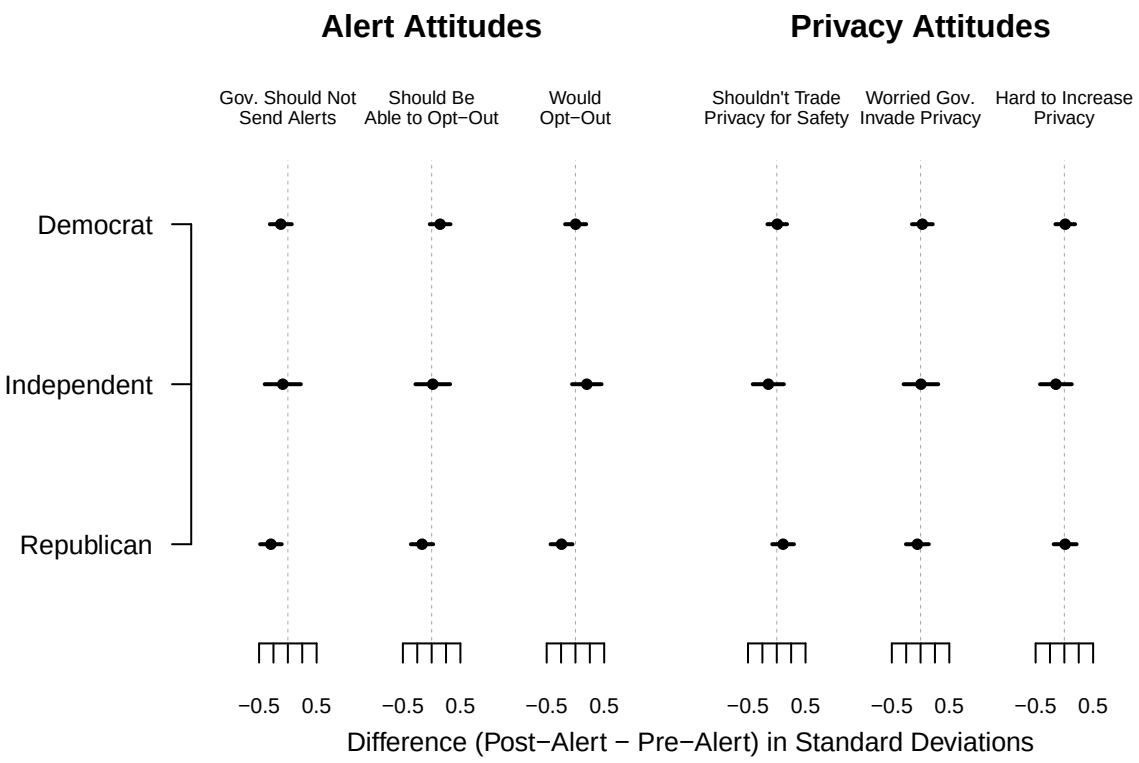


Figure 2: Attitudes Across Time

7.3 Main Analysis with Alternative Operationalization of Party Identification

We re-ran the main analysis with an alternative operationalization of Independents. While in the main analysis presented in the manuscript Independents who report leaning toward the Democratic or Republican party are classified as partisans, here we classify them as Independents. We report the results below in Figures 3 and 4 below. These results are nearly identical to those reported in the main analysis.

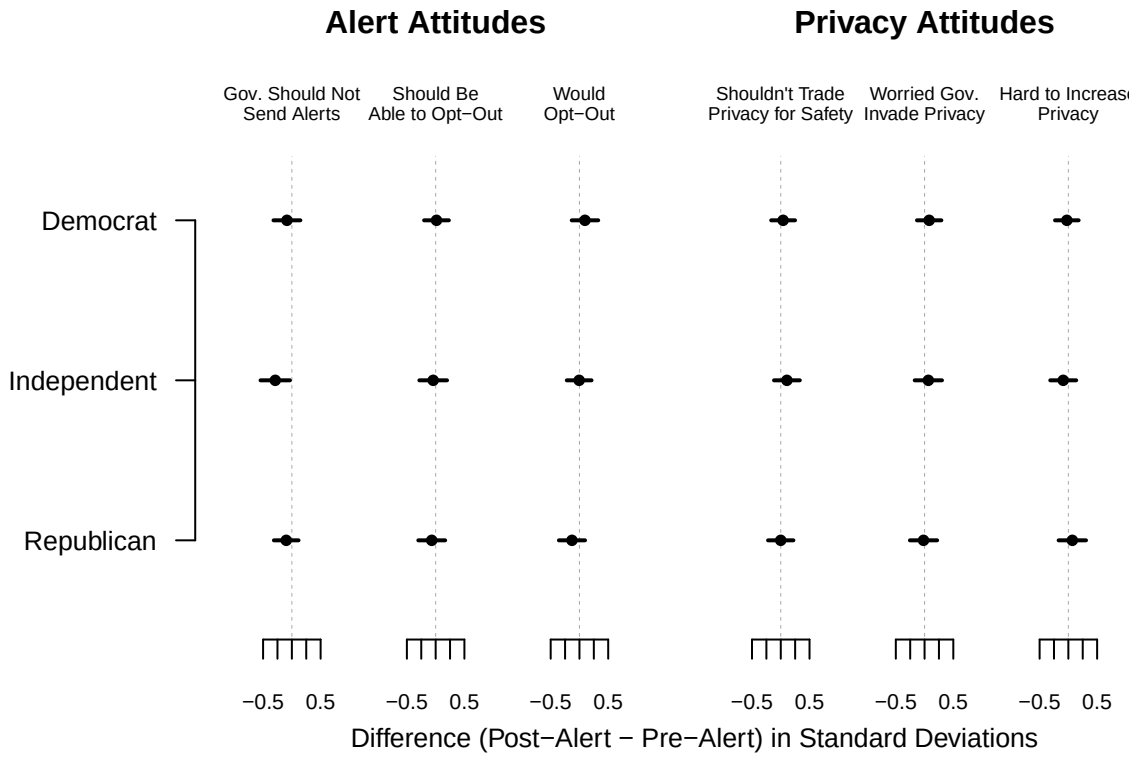


Figure 3: Experiment 1 Results-No Leaners

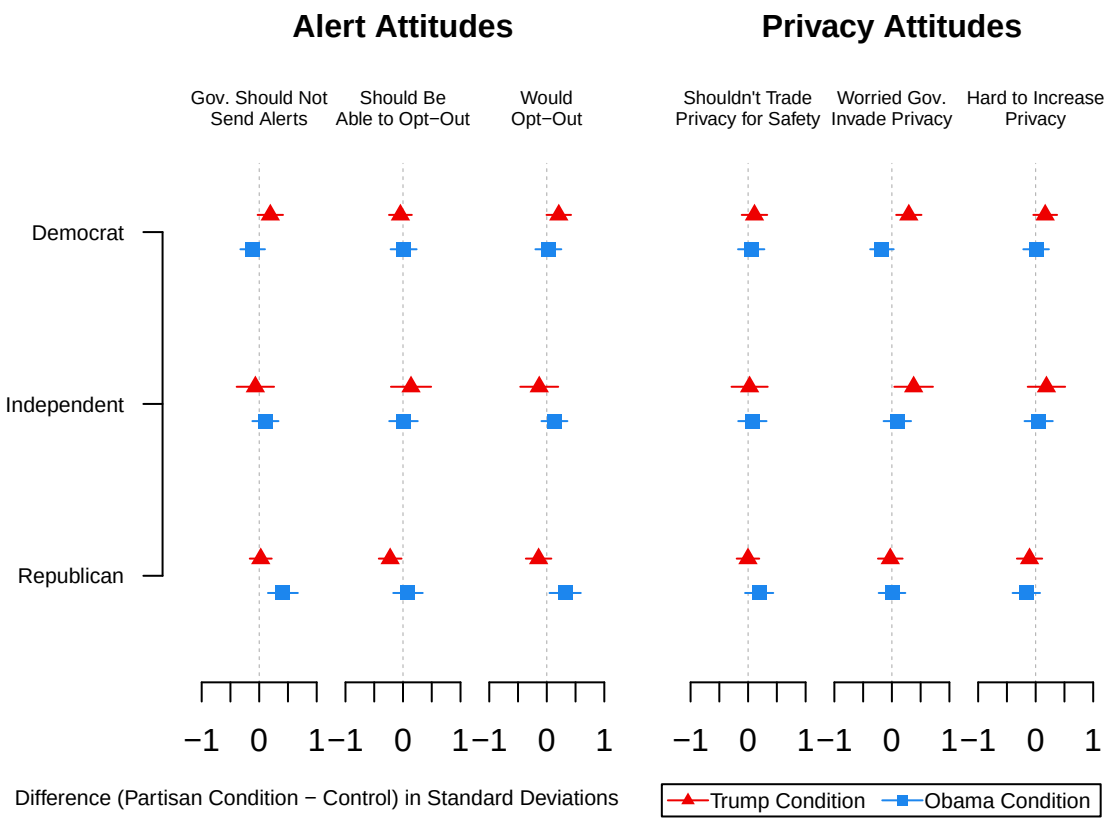


Figure 4: Experiment 2-No Leaners

7.4 Heterogeneous Treatment Effects: Prior Awareness of Alert

Was the effect of the alert muted by participants who had already heard about the alert prior to receiving it? At the end of our survey respondents were asked, "Before today, had you heard about the test of FEMA's alert system scheduled for today?'' We run regression models predicting each of the 6 main experimental outcomes (3 alert attitudes and 3 privacy attitudes) and include interactions between the treatment and an indicator for whether or not a respondent reported being aware of the alert prior to receiving it. For ease of interpretation, we run models separately for Democrats and Republicans. This results in 12 models for Experiment 1 (Table 8). We also present models with three-way interactions in Table 10. If the alert's effect was moderated by participants' prior awareness of the alert, we would observe positive and statistically significant coefficient terms for these interactions. However, we find no such evidence of heterogeneous treatment effects here.

7.5 Heterogeneous Treatment Effects: Receiving the Alert

Was the effect of the alert on attitudes muted by respondents who had not received the alert? At the end of the survey respondents were asked "Did you receive an alert message on your cell phone that said *Presidential Alert*?'' We run regression models predicting each of the 6 main experimental outcomes (3 alert attitudes and 3 privacy attitudes) and include interactions between the treatment and an indicator for whether or not a respondent reported receiving the alert. For ease of interpretation, we run models separately for Democrats and Republicans. This results in 12 models for Experiment 1 (Table 9). We also present models with three-way interactions in Table 11. Across these models we find no evidence that respondents who did not receive the alert dampened the treatment effects reported in the main analysis.

Table 8: Prior Alert Awareness: Experiment 1

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Alert	−0.01	−0.23	−0.02	0.03	0.91*	−0.06	0.10	−0.24	0.01	0.02	−0.02	0.03
	(0.17)	(0.15)	(0.15)	(0.17)	(0.43)	(0.48)	(0.15)	(0.16)	(0.15)	(0.17)	(0.14)	(0.16)
Aware-Alert	−0.02	−0.24	0.02	−0.13	1.19**	0.82	−0.02	−0.36*	0.0003	0.11	−0.02	−0.01
	(0.17)	(0.16)	(0.16)	(0.17)	(0.44)	(0.44)	(0.15)	(0.16)	(0.15)	(0.17)	(0.15)	(0.17)
Female	−0.06	0.12	0.08	0.02	−0.90***	−0.72*	−0.15	0.26*	−0.13	0.004	0.23*	0.30**
	(0.12)	(0.11)	(0.11)	(0.12)	(0.26)	(0.29)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.11)
Age	0.003	−0.01	0.003	0.001	−0.003	−0.04***	0.01	0.002	0.0001	−0.01*	0.01***	0.01***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.01)	(0.01)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Income	−0.004	0.01	0.02	0.02	0.02	0.03	−0.03*	−0.03	−0.01	−0.01	0.01	−0.004
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
White	−0.16	−0.21	0.01	0.05	−0.62*	−0.79*	−0.11	−0.03	−0.27*	−0.58***	0.002	0.43**
	(0.13)	(0.15)	(0.12)	(0.17)	(0.28)	(0.37)	(0.11)	(0.15)	(0.11)	(0.16)	(0.11)	(0.16)
Post-Alert:Aware-Alert	−0.17	0.26	0.06	−0.26	−0.99	−0.26	−0.07	0.36	0.11	0.10	−0.03	−0.15
	(0.23)	(0.21)	(0.21)	(0.23)	(0.55)	(0.60)	(0.20)	(0.21)	(0.20)	(0.22)	(0.19)	(0.22)
Constant	0.22	0.38	−0.35	−0.09	−1.02	0.88	0.23	0.15	0.35	0.88**	−0.79***	−1.08***
	(0.24)	(0.24)	(0.22)	(0.27)	(0.55)	(0.66)	(0.21)	(0.25)	(0.21)	(0.26)	(0.20)	(0.26)
Observations	367	342	368	342	367	342	368	342	368	342	368	342
R^2	0.01	0.04	0.01	0.03			0.03	0.05	0.03	0.06	0.06	0.10
Adjusted R^2	−0.01	0.02	−0.01	0.01			0.01	0.03	0.01	0.04	0.05	0.08
Log Likelihood					−191.63	−150.80						
Akaike Inf. Crit.					399.25	317.61						

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

Table 9: Receipt of Alert: Experiment 1

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Alert	−0.21	0.01	0.04	0.12	−0.05	0.48	0.21	−0.19	0.15	0.10	0.02	−0.09
	(0.18)	(0.17)	(0.17)	(0.19)	(0.41)	(0.49)	(0.16)	(0.18)	(0.16)	(0.19)	(0.15)	(0.18)
Got Alert	−0.29	−0.18	−0.05	0.01	−0.19	0.58	0.15	−0.21	0.14	0.09	0.23	−0.11
	(0.18)	(0.16)	(0.16)	(0.18)	(0.41)	(0.46)	(0.16)	(0.17)	(0.16)	(0.18)	(0.15)	(0.17)
Female	−0.02	0.13	0.08	0.02	−0.88***	−0.73*	−0.15	0.27*	−0.16	0.01	0.21*	0.29**
	(0.12)	(0.11)	(0.11)	(0.12)	(0.26)	(0.29)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.11)
Age	0.002	−0.01	0.003	0.0004	0.0005	−0.04***	0.005	0.002	0.0003	−0.01*	0.01***	0.01***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.01)	(0.01)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Income	−0.005	0.01	0.02	0.02	0.03	0.04	−0.03*	−0.03	−0.01	−0.01	0.01	−0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
White	−0.17	−0.19	0.01	0.06	−0.54*	−0.71	−0.11	−0.04	−0.26*	−0.56***	0.01	0.42*
	(0.13)	(0.15)	(0.12)	(0.17)	(0.28)	(0.37)	(0.11)	(0.16)	(0.11)	(0.17)	(0.11)	(0.16)
Post-Alert:Got Alert	0.20	−0.15	−0.06	−0.36	0.48	−1.12	−0.20	0.24	−0.15	−0.06	−0.12	0.07
	(0.24)	(0.21)	(0.22)	(0.24)	(0.53)	(0.61)	(0.21)	(0.22)	(0.21)	(0.23)	(0.20)	(0.23)
Constant	0.40	0.33	−0.34	−0.14	−0.43	0.85	0.15	0.10	0.24	0.87**	−0.90***	−1.02***
	(0.25)	(0.25)	(0.23)	(0.28)	(0.53)	(0.66)	(0.22)	(0.26)	(0.22)	(0.27)	(0.20)	(0.26)
Observations	366	341	367	341	366	341	367	341	367	341	367	341
R^2	0.02	0.05	0.01	0.02			0.03	0.04	0.03	0.06	0.07	0.10
Adjusted R^2	−0.004	0.03	−0.01	0.001			0.01	0.02	0.01	0.04	0.05	0.08
Log Likelihood					−193.88	−151.45						
Akaike Inf. Crit.					403.77	318.90						

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

Table 10: Prior Alert Awareness: Experiment 1

	Dependent variable:					
	Anti-Alert	Pro Opt Out	Opt Out Personally	Privacy-Safety	Gov-Monitoring	Privacy Difficult
	OLS	*OLS*	*logistic*	*OLS*	*OLS*	*OLS*
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Alert	−0.26	0.03	−0.18	−0.25	−0.01	0.04
	(0.17)	(0.17)	(0.47)	(0.16)	(0.16)	(0.15)
Democrat	0.07	−0.15	−0.59	−0.14	−0.04	0.02
	(0.18)	(0.17)	(0.50)	(0.16)	(0.17)	(0.16)
Aware-Alert	−0.26	−0.13	0.72	−0.38*	0.09	−0.02
	(0.17)	(0.17)	(0.43)	(0.16)	(0.17)	(0.16)
Female	0.03	0.05	−0.81***	0.05	−0.07	0.26***
	(0.08)	(0.08)	(0.19)	(0.08)	(0.08)	(0.08)
Age	−0.001	0.002	−0.02**	0.004	−0.003	0.01***
	(0.002)	(0.002)	(0.01)	(0.002)	(0.002)	(0.002)
Income	0.001	0.02	0.02	−0.03**	−0.01	0.004
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
White	−0.17	0.03	−0.65**	−0.09	−0.36***	0.15
	(0.10)	(0.10)	(0.22)	(0.09)	(0.09)	(0.09)
Post-Alert:Democrat	0.27	−0.04	1.16	0.36	0.03	−0.07
	(0.23)	(0.23)	(0.64)	(0.21)	(0.22)	(0.21)
Post-Alert:Aware-Alert	0.27	−0.26	−0.21	0.37	0.09	−0.14
	(0.22)	(0.22)	(0.59)	(0.21)	(0.22)	(0.21)
Democrat:Aware-Alert	0.26	0.16	0.56	0.35	−0.06	−0.02
	(0.24)	(0.23)	(0.62)	(0.22)	(0.23)	(0.22)
Post Alert:Democrat:Aware-Alert	−0.45	0.31	−0.83	−0.42	0.01	0.12
	(0.31)	(0.31)	(0.80)	(0.29)	(0.30)	(0.29)
Constant	0.22	−0.15	0.05	0.27	0.55**	−0.87***
	(0.20)	(0.20)	(0.49)	(0.19)	(0.19)	(0.19)
Observations	709	710	709	710	710	710
R^2	0.03	0.02		0.03	0.04	0.07
Adjusted R^2	0.02	0.003		0.02	0.02	0.06
Log Likelihood			−346.77			
Akaike Inf. Crit.			717.54			
Note:						*p<0.05; **p<0.01; ***p<0.001

Table 11: Receipt of Alert: Experiment 1

	Dependent variable:					
	Anti-Alert	Pro Opt Out	Opt Out Personally	Privacy-Safety	Gov-Monitoring	Privacy Difficult
	OLS	*OLS*	*logistic*	*OLS*	*OLS*	*OLS*
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Alert	−0.03	0.11	0.29	−0.20	0.07	−0.08
	(0.19)	(0.18)	(0.47)	(0.17)	(0.18)	(0.17)
Democrat	0.27	−0.04	0.18	−0.16	−0.12	−0.19
	(0.19)	(0.19)	(0.48)	(0.18)	(0.19)	(0.18)
Got Alert	−0.19	0.01	0.46	−0.21	0.08	−0.12
	(0.18)	(0.18)	(0.44)	(0.17)	(0.17)	(0.17)
Female	0.06	0.05	−0.80***	0.06	−0.08	0.25**
	(0.08)	(0.08)	(0.19)	(0.08)	(0.08)	(0.08)
Age	−0.002	0.002	−0.02*	0.003	−0.004	0.01***
	(0.002)	(0.002)	(0.01)	(0.002)	(0.002)	(0.002)
Income	0.002	0.02	0.04	−0.03**	−0.01	0.002
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
White	−0.17	0.03	−0.57**	−0.09	−0.35***	0.15
	(0.10)	(0.10)	(0.22)	(0.09)	(0.09)	(0.09)
Post-Alert:Democrat	−0.15	−0.06	−0.21	0.42	0.10	0.10
	(0.25)	(0.25)	(0.62)	(0.24)	(0.24)	(0.23)
Post-Alert:Got Alert	−0.12	−0.35	−0.97	0.25	−0.06	0.09
	(0.23)	(0.23)	(0.59)	(0.22)	(0.23)	(0.22)
Democrat:Got Alert	−0.10	−0.04	−0.62	0.34	0.05	0.35
	(0.24)	(0.24)	(0.60)	(0.23)	(0.24)	(0.23)
Post Alert:Democrat:Got Alert	0.28	0.28	1.31	−0.44	−0.11	−0.20
	(0.32)	(0.32)	(0.79)	(0.30)	(0.31)	(0.30)
Constant	0.20	−0.23	−0.06	0.22	0.54**	−0.80***
	(0.21)	(0.21)	(0.50)	(0.20)	(0.20)	(0.19)
Observations	707	708	707	708	708	708
R^2	0.04	0.02		0.03	0.03	0.08
Adjusted R^2	0.03	0.001		0.01	0.02	0.06
Log Likelihood			−350.68			
Akaike Inf. Crit.			725.36			
Note:						*p<0.05; **p<0.01; ***p<0.001

7.6 Heterogeneous Treatment Effects: Partisan Strength

Heterogeneous treatment effects across strength of one's party attachments are also possible. To assess this possibility we run regression models predicting each of the 6 main experimental outcomes (3 alert attitudes and 3 privacy attitudes) and include interactions between the treatment and a three point measure of partisan strength created from the 7-point party ID scale. The partisan strength measure has a value of 0 for Independents who report leaning toward the party, 1 for 'weak' party identifiers, and 2 for 'strong' party identifiers. We run separate modes for each outcome and political party, resulting in 12 models for Experiment 1 and 12 models for Experiment 2.

Overall we find very little evidence that treatment effects vary by strength of partisanship in either experiment (Tables 12 and 13). Only in the first experiment were there any significant interaction coefficients: For Democrats, the positive effect of receiving the alert on willingness to trade privacy for safety was weaker among more strongly identified Democrats. For Republicans, the negative effect of of receiving the alert on their opposition to the alert was weaker among strongly identified Republicans.

Table 12: Partisan Strength: Experiment 1

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Alert	0.09	−0.56*	0.09	−0.34	1.19	−0.99	0.46*	0.15	0.17	0.04	0.10	−0.20
	(0.24)	(0.22)	(0.22)	(0.24)	(0.66)	(0.68)	(0.21)	(0.22)	(0.22)	(0.24)	(0.20)	(0.23)
Party Str.	0.17	−0.49*	0.11	−0.57*	1.28	−0.05	0.25	0.06	0.07	0.07	−0.09	−0.13
	(0.24)	(0.22)	(0.22)	(0.24)	(0.68)	(0.61)	(0.21)	(0.22)	(0.22)	(0.24)	(0.20)	(0.23)
Female	−0.03	0.10	0.09	−0.04	−0.79**	−0.64*	−0.11	0.25*	−0.13	0.02	0.24*	0.29*
	(0.12)	(0.11)	(0.11)	(0.12)	(0.26)	(0.30)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.12)
Age	0.002	−0.01*	0.004	0.001	−0.001	−0.04***	0.005	0.001	0.0005	−0.01*	0.01***	0.01***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.01)	(0.01)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Income	−0.01	0.005	0.02	0.02	0.02	0.03	−0.03*	−0.03	−0.01	−0.01	0.02	−0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
White	−0.16	−0.22	0.01	0.05	−0.56*	−0.83*	−0.12	−0.01	−0.27*	−0.58***	−0.02	0.42**
	(0.13)	(0.15)	(0.12)	(0.17)	(0.27)	(0.37)	(0.11)	(0.15)	(0.11)	(0.17)	(0.11)	(0.16)
Post-Alert:Party Str.	−0.26	0.69*	−0.12	0.29	−1.21	1.13	−0.56*	−0.29	−0.17	0.07	−0.22	0.21
	(0.31)	(0.28)	(0.29)	(0.31)	(0.80)	(0.84)	(0.28)	(0.28)	(0.28)	(0.30)	(0.26)	(0.29)
Constant	0.10	0.64*	−0.43	0.32	−1.47*	1.32	0.04	−0.07	0.28	0.85**	−0.76**	−0.96**
	(0.29)	(0.28)	(0.26)	(0.31)	(0.73)	(0.77)	(0.25)	(0.29)	(0.26)	(0.31)	(0.24)	(0.30)
Observations	368	342	369	342	368	342	369	342	369	342	369	342
R^2	0.01	0.05	0.01	0.03			0.04	0.04	0.03	0.06	0.07	0.09
Adjusted R^2	−0.01	0.03	−0.01	0.01			0.02	0.02	0.01	0.04	0.06	0.08
Log Likelihood					−193.88	−151.72						
Akaike Inf. Crit.					403.75	319.45						

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

Table 13: Partisan Strength: Experiment 2

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Obama-Cue	0.01	0.60**	0.17	−0.08	0.89	1.80*	−0.19	0.21	0.04	0.002	0.04	−0.04
	(0.23)	(0.21)	(0.22)	(0.22)	(0.63)	(0.71)	(0.24)	(0.22)	(0.22)	(0.20)	(0.23)	(0.22)
Trump Cue	0.18	0.30	0.41	0.20	1.25*	0.47	0.37	0.17	0.07	−0.20	0.13	−0.16
	(0.23)	(0.20)	(0.23)	(0.21)	(0.63)	(0.76)	(0.24)	(0.21)	(0.22)	(0.20)	(0.23)	(0.21)
Party Str.	0.11	0.15	0.08	0.17	0.95	1.45*	−0.31	−0.26	0.37	−0.22	−0.25	−0.15
	(0.21)	(0.18)	(0.21)	(0.20)	(0.61)	(0.67)	(0.22)	(0.19)	(0.21)	(0.18)	(0.21)	(0.19)
Female	−0.10	−0.10	0.05	−0.04	−0.60**	−0.74**	0.07	0.03	0.06	−0.08	0.27**	0.26**
	(0.10)	(0.09)	(0.10)	(0.10)	(0.23)	(0.27)	(0.10)	(0.10)	(0.09)	(0.09)	(0.10)	(0.10)
Age	−0.003	−0.01**	0.0004	−0.01***	−0.01	−0.05***	−0.001	−0.01*	−0.001	−0.005	0.01*	0.01*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.01)	(0.01)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Income	−0.02	−0.02	−0.001	0.002	0.04	0.15***	−0.04**	−0.05***	0.003	−0.02	−0.01	−0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
White	−0.08	−0.19	0.05	0.05	−0.43	−0.59	−0.13	0.11	−0.04	−0.10	−0.03	0.002
	(0.09)	(0.14)	(0.09)	(0.15)	(0.22)	(0.35)	(0.10)	(0.14)	(0.09)	(0.13)	(0.09)	(0.14)
Obama Cue:Party Str.	−0.15	−0.34	−0.20	0.18	−0.97	−1.35	0.28	0.10	−0.25	0.01	−0.07	−0.23
	(0.29)	(0.28)	(0.28)	(0.30)	(0.77)	(0.88)	(0.30)	(0.29)	(0.28)	(0.28)	(0.29)	(0.29)
Trump Cue:Party Str.	0.01	−0.47	−0.37	−0.53	−1.26	−0.85	−0.16	−0.24	−0.21	−0.02	0.01	0.01
	(0.30)	(0.27)	(0.29)	(0.29)	(0.78)	(0.94)	(0.31)	(0.28)	(0.29)	(0.27)	(0.30)	(0.28)
Constant	0.34	0.47	−0.10	0.35	−1.38*	−0.57	0.33	0.30	−0.06	0.59*	−0.22	0.12
	(0.24)	(0.24)	(0.24)	(0.26)	(0.65)	(0.79)	(0.25)	(0.25)	(0.23)	(0.24)	(0.24)	(0.25)
Observations	512	448	510	447	512	448	512	448	512	448	512	448
R^2	0.02	0.08	0.01	0.05			0.05	0.09	0.01	0.04	0.04	0.09
Adjusted R^2	0.01	0.07	−0.01	0.03			0.03	0.07	−0.003	0.02	0.02	0.07
Log Likelihood					−266.45	−181.28						
Akaike Inf. Crit.					552.89	382.57						

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

7.7 Heterogeneous Treatment Effects: Political Knowledge

It is also possible that treatment effects may have varied by political knowledge, with the most knowledgeable citizens being most responsive to the alert and/or party cues. To assess this possibility we run regression models predicting each of the 6 main experimental outcomes (3 alert attitudes and 3 privacy attitudes) and include interactions between the treatment and a measure of political knowledge. Each of these models is separated by political party resulting in 12 models for Experiment 1 and 12 models for Experiment 2. The political knowledge items were included post-treatment, however we do not suspect that responses to these items are likely to have been influenced by either the alert itself or our party cues.

The political knowledge measure was constructed using the following six items, and scaled to range between 0 and 1:

- Who is the current Speaker of the U.S. House of Representatives?
- How long is the term of office for a U.S. Senator?
- What job or political office is now held by Angela Merkel?
- What is the length of a term for a member of the House of Representatives?
- What job or political office is now held by John Roberts?
- Who is the current Vice-President of the United States?

We find no evidence of heterogeneous treatment effects for Republicans in Experiment 1 (Table 14). For Democrats only one of the six interaction coefficients is statistically significant: the positive effect of receiving the alert on a desire to opt out of the alert was weaker among more knowledgeable Democrats. In Experiment 2 there is one significant interaction for Republicans: the negative effect of the Trump cue on concerns about government monitoring was weaker among more knowledgable Republicans (Table 15). For Democrats, we observe statistically signficant coefficients for two of the twelve outcomes: the positive effect of the Obama cue on beliefs that privacy is difficult to obtain and the positive effect of the Trump cue on preferences toward opting out of the alert were both weaker among more knowledgeable Democrats.

Table 14: Political Knowledge: Experiment 1

	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Alert	0.04	−0.36	−0.13	−0.10	1.33*	−0.66	0.36	−0.06	−0.13	−0.22	0.06	0.02
	(0.22)	(0.19)	(0.20)	(0.21)	(0.56)	(0.53)	(0.19)	(0.19)	(0.19)	(0.21)	(0.18)	(0.20)
P. Knowl.	0.16	−0.12	−0.08	0.04	1.92**	−0.72	0.38	0.11	0.02	−0.44	−0.003	−0.09
	(0.29)	(0.25)	(0.27)	(0.27)	(0.71)	(0.66)	(0.25)	(0.25)	(0.26)	(0.27)	(0.24)	(0.26)
Female	−0.06	0.17	0.09	0.03	−0.79**	−0.74*	−0.11	0.29*	−0.13	−0.002	0.21*	0.27*
	(0.12)	(0.11)	(0.11)	(0.12)	(0.26)	(0.30)	(0.11)	(0.11)	(0.11)	(0.12)	(0.10)	(0.12)
Age	0.002	−0.01*	0.003	0.0004	−0.003	−0.04***	0.005	0.0003	−0.001	−0.01	0.01***	0.01***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.01)	(0.01)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)
Income	−0.01	0.001	0.02	0.02	0.01	0.04	−0.03*	−0.03*	−0.01	−0.01	0.02	−0.002
	(0.02)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
White	−0.18	−0.20	0.01	0.03	−0.64*	−0.75*	−0.10	−0.03	−0.30**	−0.55***	0.002	0.43**
	(0.13)	(0.15)	(0.12)	(0.17)	(0.28)	(0.37)	(0.11)	(0.16)	(0.11)	(0.16)	(0.11)	(0.16)
Post-Alert:P. Knowl.	−0.25	0.56	0.25	−0.02	−1.92*	0.91	−0.54	0.03	0.38	0.59	−0.21	−0.16
	(0.37)	(0.32)	(0.34)	(0.35)	(0.87)	(0.90)	(0.33)	(0.32)	(0.33)	(0.34)	(0.31)	(0.33)
Constant	0.20	0.39	−0.31	−0.11	−1.34*	1.47*	0.02	−0.03	0.40	1.09***	−0.81***	−1.07***
	(0.26)	(0.25)	(0.24)	(0.28)	(0.63)	(0.66)	(0.23)	(0.26)	(0.23)	(0.27)	(0.22)	(0.27)
Observations	365	342	366	342	365	342	366	342	366	342	366	342
R^2	0.01	0.04	0.01	0.01			0.03	0.03	0.03	0.07	0.06	0.10
Adjusted R^2	−0.01	0.02	−0.01	−0.01			0.02	0.01	0.01	0.05	0.04	0.08
Log Likelihood					−188.89	−152.81						
Akaike Inf. Crit.					393.78	321.62						

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

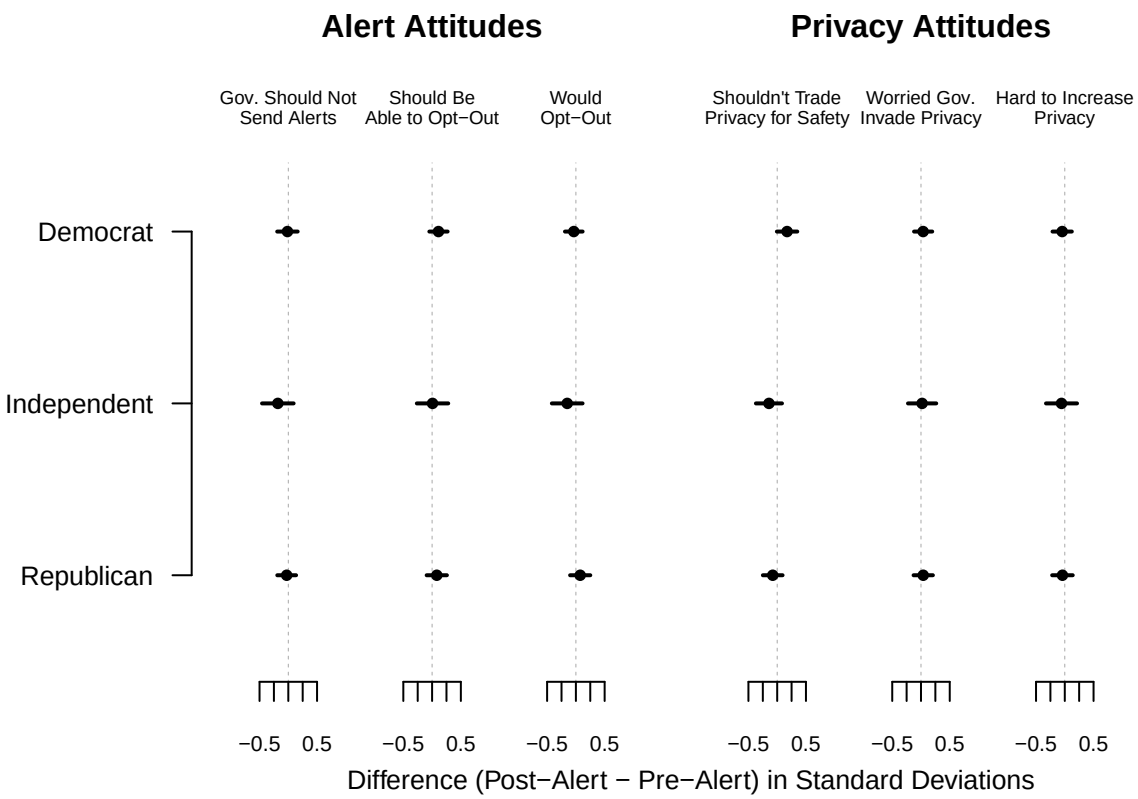
Table 15: Political Knowledge: Experiment 2

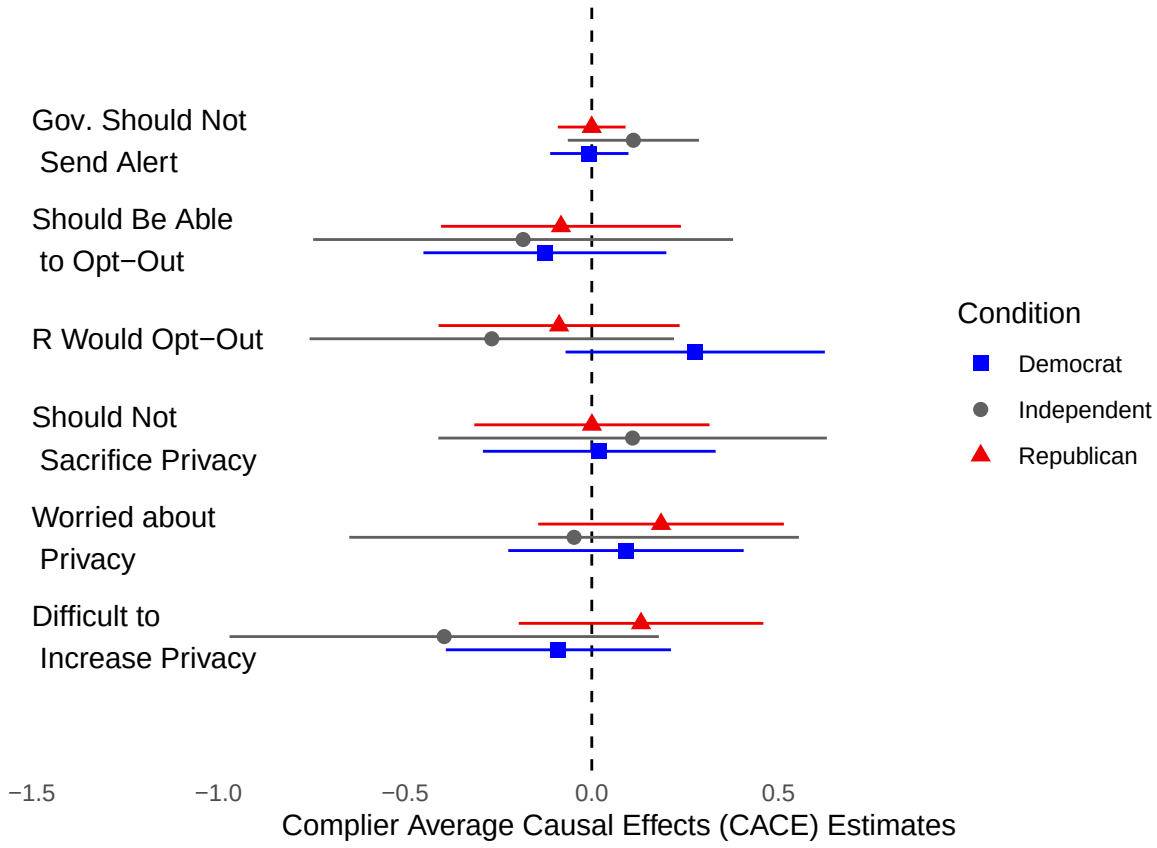
	Dependent variable:											
	Anti-Alert		Pro Opt Out		Opt Out Personally		Privacy-Safety		Gov-Monitoring		Privacy Difficult	
	OLS		*OLS*		*logistic*		*OLS*		*OLS*		*OLS*	
	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep	Dem	Rep
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Obama Cue	0.16	0.43	0.26	−0.22	0.48	1.67**	0.15	−0.02	−0.04	−0.13	0.34	−0.51*
	(0.21)	(0.22)	(0.20)	(0.23)	(0.51)	(0.64)	(0.22)	(0.23)	(0.20)	(0.22)	(0.21)	(0.23)
Trump Cue	0.52*	−0.01	0.69**	−0.49*	0.48	−0.06	0.35	−0.15	0.19	−0.58**	0.31	−0.34
	(0.21)	(0.21)	(0.21)	(0.22)	(0.53)	(0.70)	(0.22)	(0.22)	(0.21)	(0.21)	(0.21)	(0.22)
P. Knowl.	0.43	−0.04	0.35	−0.22	0.01	0.70	−0.02	−0.47	0.19	−0.13	0.63*	−0.39
	(0.25)	(0.25)	(0.24)	(0.27)	(0.64)	(0.79)	(0.26)	(0.26)	(0.24)	(0.25)	(0.25)	(0.26)
Female	−0.10	−0.11	0.05	−0.03	−0.63**	−0.75**	0.06	0.01	0.07	−0.06	0.29**	0.25*
	(0.10)	(0.10)	(0.09)	(0.10)	(0.23)	(0.28)	(0.10)	(0.10)	(0.09)	(0.10)	(0.10)	(0.10)
Age	−0.003	−0.01**	0.001	−0.01***	−0.01	−0.05***	−0.001	−0.01*	−0.001	−0.01*	0.004	0.01*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.01)	(0.01)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Income	−0.02	−0.02	0.002	0.001	0.05	0.15***	−0.03*	−0.05***	0.01	−0.03*	−0.01	−0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
White	−0.09	−0.21	0.08	0.06	−0.39	−0.67	−0.10	0.12	−0.05	−0.09	−0.02	0.05
	(0.09)	(0.14)	(0.09)	(0.15)	(0.22)	(0.35)	(0.10)	(0.14)	(0.09)	(0.14)	(0.09)	(0.14)
Obama Cue:P. Knowl.	−0.51	−0.07	−0.46	0.45	−0.60	−1.51	−0.27	0.52	−0.19	0.23	−0.69*	0.63
	(0.34)	(0.36)	(0.34)	(0.39)	(0.86)	(1.06)	(0.36)	(0.38)	(0.33)	(0.36)	(0.35)	(0.38)
Trump Cue:P. Knowl.	−0.72	−0.02	−1.03**	0.68	−0.26	−0.09	−0.17	0.31	−0.52	0.70*	−0.33	0.37
	(0.37)	(0.35)	(0.37)	(0.38)	(0.92)	(1.12)	(0.39)	(0.37)	(0.36)	(0.35)	(0.37)	(0.37)
Constant	0.23	0.57*	−0.26	0.63*	−0.74	0.03	0.09	0.40	0.07	0.60*	−0.62**	0.24
	(0.21)	(0.24)	(0.21)	(0.26)	(0.52)	(0.71)	(0.22)	(0.25)	(0.20)	(0.24)	(0.21)	(0.25)
Observations	511	444	509	443	511	444	511	444	511	444	511	444
R^2	0.03	0.08	0.02	0.04			0.04	0.08	0.01	0.04	0.04	0.09
Adjusted R^2	0.01	0.06	0.01	0.02			0.02	0.06	−0.01	0.02	0.03	0.07
Log Likelihood					−267.15	−182.13						
Akaike Inf. Crit.					554.30	384.26						

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

7.8 Additional Causal Estimators

In our main analysis we report the difference in attitudes between A) those who were assigned to answer attitude outcomes post-treatment (and who actually did answer them post-treatment) and B) those who were assigned to answer attitude outcomes pre-treatment (and who actually did answer them pre-treatment). While non-random selection into the treatment group is unlikely to be problematic given that it depends on the timing of when respondents began the survey (and how long they took to complete it), we present ITT and CACE estimates below. The former was calculated as the difference in outcomes between those assigned to the post-alert condition and pre-alert condition, regardless of whether they actually answered the outcome questions before or after the alert. The latter was calculated with an instrumental variable model, where the instrument was the random assignment to the pre- or post-alert condition. In both cases the treatment effects are not statistically significant.


Alert Attitudes
Privacy Attitudes
Gov. Should Not Send Alerts
Should Be Able to Opt−Out
Would Opt−Out
Shouldn't Trade Privacy for Safety
Worried Gov. Invade Privacy
Hard to Increase Privacy
Democrat
Independent
Republican
−0.5
0.5
Difference (Post−Alert − Pre−Alert) in Standard Deviations


Gov. Should Not Send Alert
Should Be Able to Opt−Out
R Would Opt−Out
Should Not Sacrifice Privacy
Worried about Privacy
Difficult to Increase Privacy
−1.5
−1.0
−0.5
0.0
0.5
Complier Average Causal Effects (CACE) Estimates
Condition
Democrat
Independent
Republican

8 Additional Outcomes

In addition to measuring attitudes about the alert and privacy, we measured a secondary set of attitudes that could plausibly have been affected by the alert. These include attitudes toward government interference, small government, and trust in government. We include the full wording of these items below, as well as experimental results. The main finding presented in the main text is consistent with the results presented here: across both experiments we find little evidence that the alert, nor information about who was responsible for the alert, polarized attitudes.

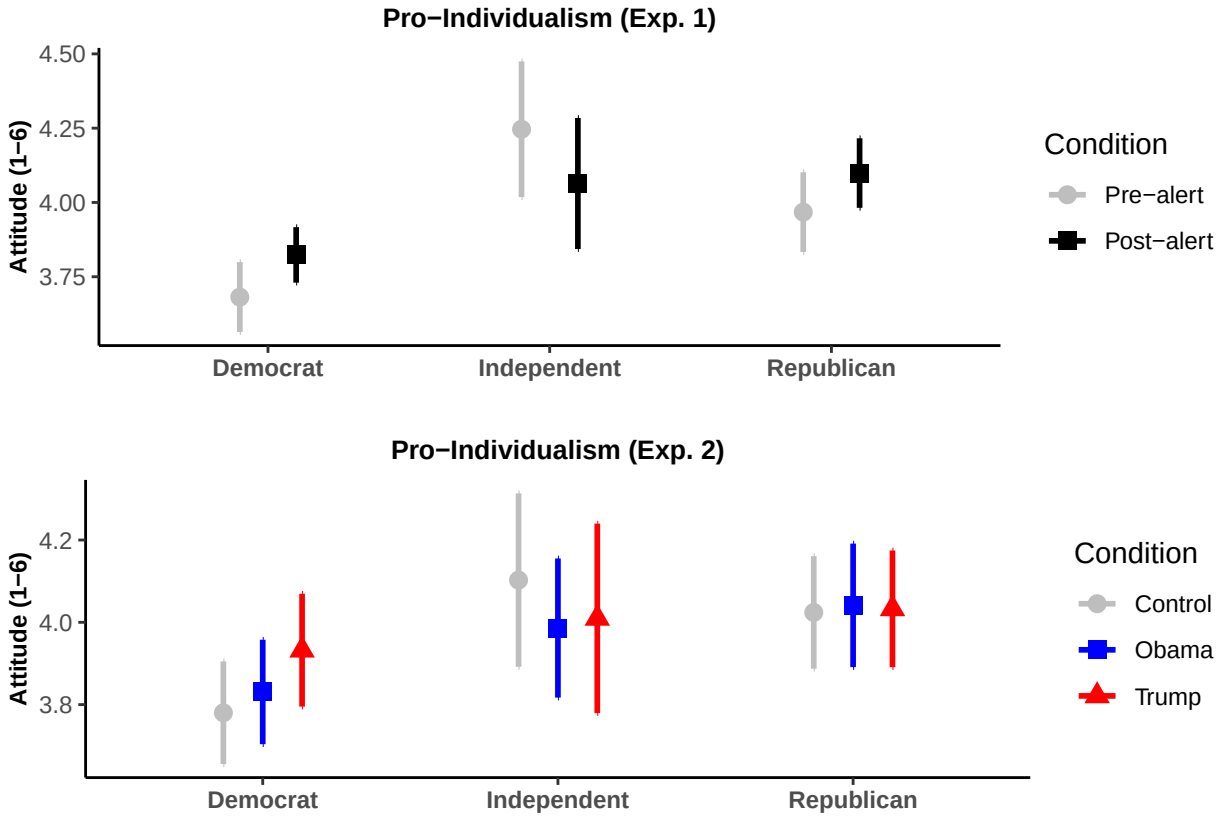
8.1 Cultural Worldviews-Individualism

Scale developed by Kahan 2012 in order to measure latent cultural predisposition associated with the "cultural theory of risk." This scale is comprised of two dimensions: Individual-Communitarianism and Hierarchy-Egalitarianism. We use the short form (6 items) Individual-Communitarianism scale. Respondents were asked: "How strongly you agree or disagree with each of these statements?" (strongly disagree [1], moderately disagree, slightly disagree, slightly agree, moderately agree, strongly agree [6])

1. (CWV_1) The government interferes far too much in our everyday lives.
2. (CWV_2) Sometimes government needs to make laws that keep people from hurting themselves.
3. (CWV_3) It's not the government's business to try to protect people from themselves.
4. (CWV_4) The government should stop telling people how to live their lives.
5. (CWV_5) The government should do more to advance society's goals, even if that means limiting the freedom and choices of individuals.

6. (CWV_6) Government should put limits on the choices individuals can make so they don't get in the way of what's good for society.

After recoding items such that higher values reflect concern about government interference, we created a mean index (Chronbach's alpha = .60)

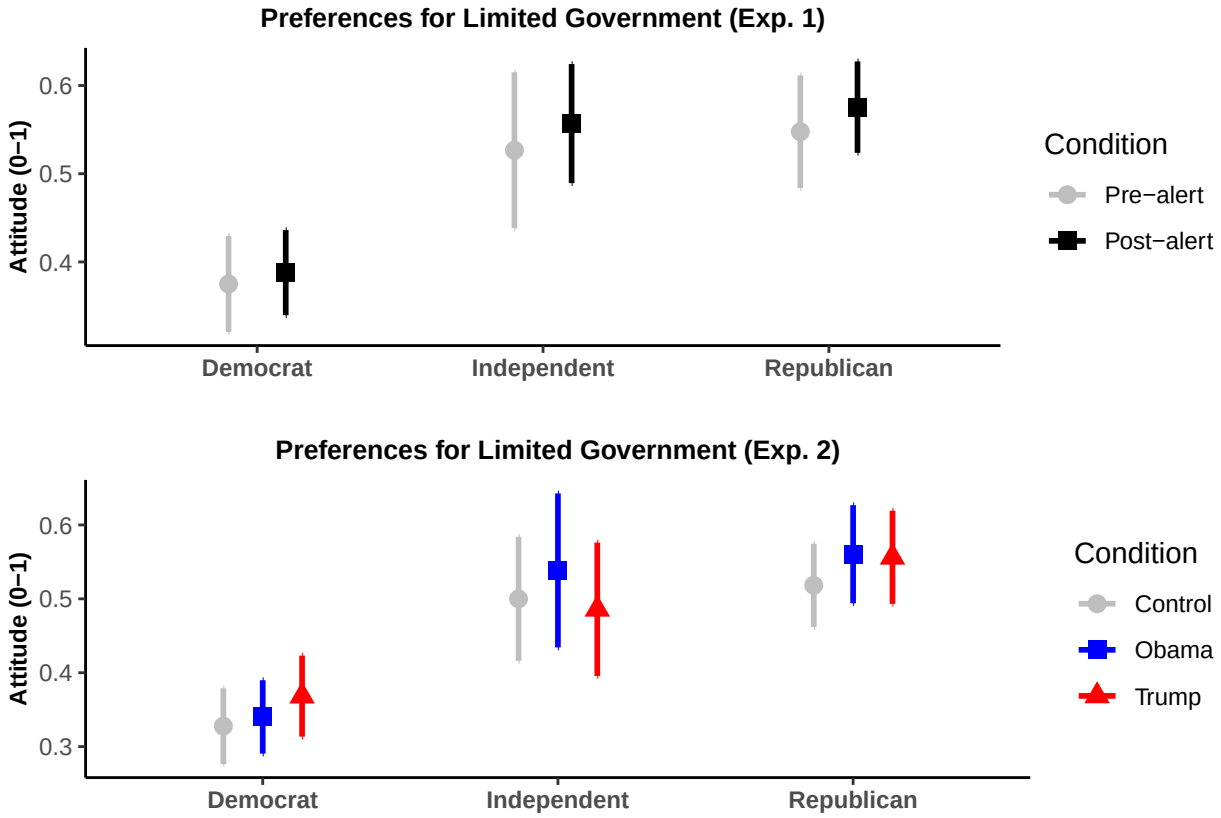


8.2 Goren (2005) Privacy Attitudes

Respondent's were asked: "Of the following statements, which comes closest to your view?"

1. The less government the better **OR** There are more things the government should be doing
2. We need strong government to handle today's complex economic problems **OR** The free market can handle these problems without government being involved.
3. The main reason government has become bigger over the years is because it has gotten involved in things that people should do for themselves **OR** Government has become bigger because the problems we face today have become bigger.

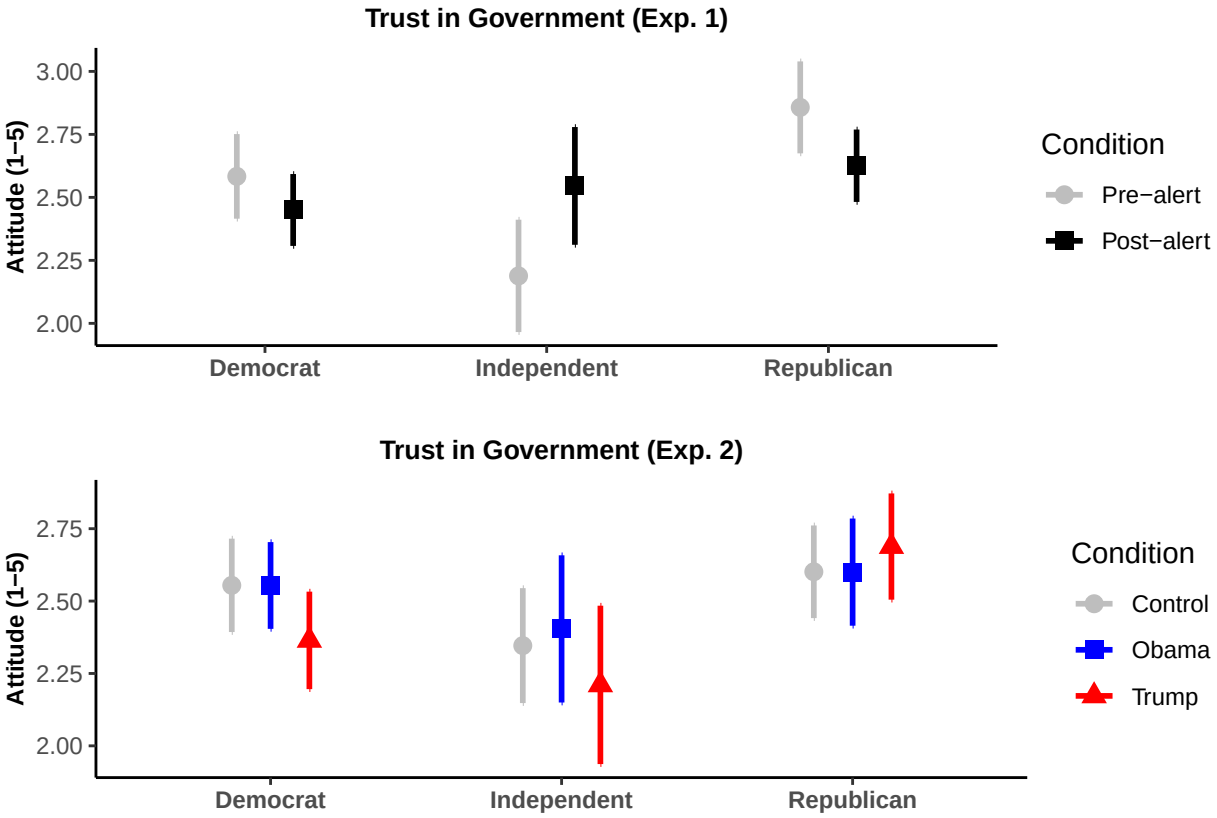
After recoding items such that higher values reflect preferences for the limited government, we created a mean index (Chronbach's alpha = .60)



8.3 Trust in Goverment

Respondents were asked: *How much of the time do you think you can trust the federal government in Washington to make decisions in a fair way?* (Always, Most of the time, About half of the time, Some of the time, Never)

Responses were recoded such that higher values reflect greater trust in government.


Trust in Government (Exp. 1)
Attitude (1–5)
3.00
2.75
2.50
2.25
2.00
Democrat
Independent
Republican
Condition
Pre−alert
Post−alert
Trust in Government (Exp. 2)
Attitude (1–5)
2.75
2.50
2.25
2.00
Democrat
Independent
Republican
Condition
Control
Obama
Trump

9 Additional Study Details

9.1 Lucid Recruitment

We partnered with Lucid to recruit participants through their Fulcrum Exchange platform. Lucid is an online market research firm that is used by academics and commercial clients to collect opinion data. Through the Fulcrum Exchange, survey opportunities are made available to a network of companies that each maintain a panel of respondents, and have partnered with Lucid. As a result, we were able to partner with Lucid to ensure our survey opportunity was made available to hundreds of survey panelists, and collect a large amount of responses around the time of the alert.

Recruitment of these participants began 25 minutes preceding the alert, and continued until a few minutes after the alert had occurred. We did not employ quotas or restrict respondents to those on taking the survey on a specific platform in order to maximize our sample size. As such, we emphasize that this sample is a convenience sample and does not reflect the demographics of the general population.

9.2 Non-Political Filler Questions

We used a set of filler questions to control whether respondents answered attitude questions prior to or after receiving the alert. We programmed the survey such that respondents who were randomly assigned to answer attitude questions after receiving the alert, but completed the pre-treatment questions prior to the time at which the alert was sent, answered a series of unrelated non-political questions until the alert was sent. These questions were designed to avoid any potential of influencing their attitudes toward any of the attitude outcomes. To make these questions less conspicuous we consulted with Lucid to design items that would be familiar to respondents in their panels. The items comprise primarily of consumer behavior questions about music, cars, and phone preferences, and we include a selection of them in the appendix. We also included transitions between the political questions that constitute the majority of the survey, and these non-political filler questions. We include a sampling of these questions below.

- How many hours a day do you listen to music? (Less than an hour, 1-3 hours, 4-6 hours, More than 6 hours)
- How often do you make playlists? (Extremely often, Very often, Moderately often, Somewhat often, Not very often)
- How often do you purchase music digitally? (Extremely often, Very often, Moderately often, Somewhat often, Not very often)
- Who are your three favorite musical artists? (text entry)
- Think about your three favorite musical artists, which one would you most like to meet in person? (text entry)
- Who are three musical artists that you feel are overrated? (text entry)
- What is your least favorite musical genre? (text entry)

- How likely are you to rank songs or give songs a "thumbs up" rating when using a music streaming? (Extremely likely, Moderately likely, Slightly likely, Neither likely nor unlikely, Slightly unlikely, Moderately unlikely, Extremely unlikely)
- What year did The Beatles release "The White Album" (1968, 1964, 1960, 1970)
- Which movie genres do you like? (check all that apply)
- Who are your 3 all-time favorite actors? (text-entry)
- Generally, how do you feel about cars? (They get me from point A to point B, Somewhere in-between, I am an automobile enthusiast)
- What kind of car do you prefer? (SUV, Truck, Minivan, Sedan, Eco-Friendly Car, Convertible)
- Please rate the following car feature in terms of how important it is for you personally when buying a car: keyless entry, cruise control, automatic start, touch-screen interface, built-in navigation, all-wheel drive)
- Out of every 100 people living in the United States, how many do you think own a dishwasher? (text-entry)
- Out of every 100 people living in the United States, how many do you think own a home?
- Out of every 100 people living in the United States, how many do you think own a washing machine for washing clothes? (text-entry)

9.3 Informed Consent

(Note that identifying information has been removed during peer review.)

Thank you for your interest in participating in this survey research project by Duke University researchers! This study is a brief survey about yourself and your opinions and attitudes. Your participation in this survey is voluntary and your responses are confidential. At no point will researchers disclose your individual responses or any identifying information about you.

On average this survey is expected to take about 15-20 minutes to complete, although the duration may vary based on your own pace and attentiveness. As specified by the online research company that invited you to participate in this survey, you will receive an incentive for your participation. You may withdraw at any time and you may refuse to answer any question, but you must continue to the end of the survey to receive the incentive.

If you have any questions about this survey, please contact Brian Guay (brian.guay[at]duke.edu). For answers to any questions you may have about your rights as a research subject, contact the Chair of the Duke University Human Subjects Committee (campusirb[at]duke.edu; (919) 684-3030).

To indicate that you read the above information and consent to participate in this research, please click the “Next” button.

9.4 Debriefing

(Note that identifying information has been removed during peer review.)

This study received approval from the Duke University Campus Institutional Review Board, protocol #2019-0138. Although the study did not involve deception, some respondents in Experiment 2 were given different information emphasizing the role that the Obama or Trump administration played in the presidential alert. Therefore, respondents received the following debriefing afterward completing the survey.

This study was designed to coincide with FEMA's test an alert system that was scheduled to take place today. While the our study focuses on your attitudes towards issues related to this alert, we played no role in you receiving the alert today. The alert was sent to nearly all cell phones in the U.S. The alert itself is the result of a bipartisan effort during the George W. Bush and Obama administrations to create an updated national alert system in case of a national emergency. An Executive Order creating the Presidential Alert system was signed by George W. Bush in 2006, legislation signed into law by Obama in 2016 enabled this system to be modernized, and under the Trump Administration, FEMA conducted the system's first test. The alert system can only be used in the event of a test or a national emergency.

9.5 Institutional Review Board

(Note that identifying information has been removed during peer review.)

This study was approved by the Duke University Campus Institutional Review Board (Protocol # 2019-0138).