

Identifying the Effect of Political Rumor Diffusion

Using Variations in Survey Timing

Online Appendix

Jin Woo Kim

Eunji Kim

Appendix A: Measurement

Appendix B: Balance Check

Appendix C: Difference-in-Differences Setup

Appendix D: Parallel Trends Assumption

Appendix E: Homogeneous Treatment Effects Assumption

Appendix F: Regression Model Specifications

Appendix G: Operational Definition of Groups A and B

Appendix H: Robustness Checks

Appendix I: Heterogeneous Effects of Rumor Circulation by Partisan Leaning

Appendix J: Specifying the Role of *Online* Media

Appendix K: Exploring the Long-run Effect and Electoral Consequence of the Event

Appendix L: An Example of “My Muslim Faith” Message

Appendix A: Measurement

This appendix provides information on the measurement of variables that are used in the analyses presented in the main text and/or balance checks shown below. Variables are constructed using Stata's alpha command, which utilizes all observations with at least one available item, by dividing the sum of available scores by the number of valid responses. We provide reliability statistics when applicable. All continuous variables are rescaled to 0-1. We also list the variable names in the original ANES dataset (e.g., der02) and question wordings. See DeBelle et al. (2010) for additional information.

Groups. Indicator variable. Those who took the survey by September 10 (N = 1,874) are defined as Group B, and those who took the interview between September 11 and October 2 are defined as Group A (N = 786). Constructed from *w9date*.

Rumor belief (Waves 9 and 11). Measured by the survey item "What is Barack Obama's religion?" The survey questionnaire wordings are as follows:

What is Barack Obama's religion? Is he Christian, Jewish, Muslim, Buddhist, or not religious? __ Christian __ Jewish __ Muslim __ Buddhist __ not religious? (w9v3, w11wv3)

In Wave 9, majority of respondents (73.2%) correctly stated that Obama is a Christian (coded as 0), and a smaller but substantial number of respondents (20.0%) said he is a Muslim (coded as 1). This leaves about 5 percent of respondents (N = 132) who noted either that Obama is a Jewish, or a Buddhist, or that he is not religious, and 2 percent of respondents (N = 47) who did not answer the question at all.

Table A1: Distribution of Beliefs about Obama's Religion by Group and Wave

	Group B		Group A	
	Frequency	Percent	Frequency	Percent
Panel A: Wave 9				
Christian	1,416	74.9	546	69.2
Jewish	10	0.5	5	0.6
Muslim	352	18.6	183	23.2
Buddhist	11	0.6	5	0.6
Not Religious	70	3.7	34	4.3
No Answer	31	1.6	16	2.0
Total	1,890	100	789	100
Panel B: Wave 11				
Christian	1,325	74.6	518	73.9
Jewish	7	0.40	0	0
Muslim	349	19.6	140	20.0
Buddhist	7	0.4	8	1.1
Not Religious	77	4.4	28	4.0
No Answer	12	0.7	7	1.0
Total	1,777	100	701	100

Although they were ignorant or misinformed about Obama's religion, we exclude them from the main analyses as our study deals with the particular rumor that Obama is a Muslim. This approach allows us to define our dependent variable as *change* in rumor belief, which makes the analyses and interpretation of results more straightforward. To ensure that our result is not driven by this decision, we cross tabulate (unweighted) distribution of belief about Obama's religion—including options omitted from the main analyses—by group and wave in Table A1. As the table shows, in each group, very small number of individuals said Obama was Jewish or Buddhist. While more substantial portion of respondents (4 percent) thought Obama was not religious, the cross-tab shows little evidence that this (incorrect) belief was noticeably different across groups in either Wave 9 or Wave 11. We also want to note that the frequency of the belief that John Mc Cain was not religious was quite comparable to Obama—5.3 percent in Wave 9 and 4.0 percent in Wave 11.

In addition, while ANES respondents were not given the option to choose “don’t know” in the online survey, they were able to provide no answer to the question (labeled “no answer” in the original dataset). In Waves 9 and 11 respectively, 47 and 22 respondents chose to do so. Again, these numbers are very similar to those who didn’t answer what John McCain’s religion was—44 and 25 respondents in each wave.

Table A2: Robustness Check across Coding Approaches to the Dependent Variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Difference-in-Differences	0.062* (0.031)	0.085* (0.032)	0.096* (0.032)	0.074* (0.030)	0.087* (0.030)	0.063 (0.032)	0.077* (0.032)
Observations	2066	2266	2312	2266	2312	2266	2312
Other Responses Coded	Drop	0	0	.5	.5	1	1
No Response Coded	Drop	Drop	0	Drop	0	Drop	0
Covariates	No	No	No	No	No	No	No
Weights	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: * $p < 0.05$ (two-tailed). OLS estimates with standard errors in parentheses. Column 1 is taken from Column 1 of Table 1 in the main text. Columns 2 to 7 use the same model specifications except for the coding of the dependent variable.

In order to directly address the concern that the coding decision affected our estimates, we provide a range of robustness checks using alternative specifications in Table A2. Column 1 reports the model using the main specification, dropping other answers and non-responses. We define the dependent variable as the belief that Obama is a Muslim (as opposed to any other perceptions) and code other *answers* as 0 (excluding no answer) in Column 2, and code all other responses (including no answer) as 0 in Column 3. In Columns 4 and 5, we follow the approach taken by Nyhan et al. (2017), who coded other misperceptions as the midpoint between the belief that Obama is a Muslim and the (correct) belief that he is a Christian. In Columns 6 and 7, we define our dependent variable as any misperception about Obama’s religion and thus code any answer other than the correct one as 1. As shown in Table A2, the alternative approaches

generated similar or larger estimates—ranging from 6.3 to 8.7 percentage points (see Columns 2 to 7)—compared to the estimate reported in the manuscript (6.2 points, $p = 0.047$; see Column 1). In short, to the extent that our coding decision altered the estimates reported in the main text, it seems that it led us to understate the effect of exposure to online rumor.

Table A3: Treatment Effect on Other Misperceptions about Obama’s religion

	(1)	(2)	(3)	(4)
Difference-in-Differences	-0.022 (0.025)	-0.019 (0.025)	0.004 (0.020)	0.004 (0.019)
Observations	2266	2312	2266	2312
<i>Dependent Variable (Misperception) Coding</i>				
Christian and Muslim coded as	0	0	0	0
Not Religious coded as	1	1	1	1
Buddhist and Jewish coded as	1	1	0	0
No response coded as	Drop	0	Drop	0
Covariates	No	No	No	No
Weights	Yes	Yes	Yes	Yes

Note: * $p < 0.05$ (two-tailed). OLS estimates with standard errors in parentheses.

We also examined the possibility that the treatment altered other misperceptions about Obama’s religion (e.g., that Obama is not religious, Buddhist or Jewish). As shown in Table A3, we found no evidence that it did.

Age. Continuous variable. Constructed from *der02*.

Female. Indicator variable. Coded 1 for females, 0 for males. Constructed from *der01*.

White. Indicator variable. Coded 1 for white, 0 for other ethnicity. Constructed from *der03a*.

Christian. Indicator variable, coded 1 for Catholic and Protestant, 0 for other religion or no religion. Constructed from *der22*.

Education. Indicator variable, coded 1 for bachelor's degree or higher, 0 for some college or lower. Constructed from *der05*.

Income. Indicator variable, coded 1 for \$60,000 or higher, 0 for lower. Constructed from *der06*.

Political Knowledge (Wave 2). Constructed by counting the number of correct answers to factual knowledge items, which was rescaled to 0-1, with 1 indicating correct on every item (Cronbach's alpha = 0.57). The question wordings are as follows:

Do you happen to know how many times an individual can be elected President of the United States under current laws? Type the number. For how many years is a United States Senator elected – that is, how many years are there in one full term of office for a U.S. Senator? Type the number.

How many U.S. Senators are there from each state? Type the number. For how many years is a member of the United States House of Representatives elected – that is, how many years are there in one full term of office for a U.S. House member? Type the number.

According to federal law, if the President of the United States dies, is no longer willing or able to serve, or is removed from office by Congress, the Vice President would become the President. If the Vice President were unable or unwilling to serve, who would be eligible to become president next? (The Chief Justice of the Supreme Court, the Secretary of State, or the Speaker of the House of Representatives / The Speaker of the House of Representatives, the Secretary of States, or the Chief Justice of the Supreme Court)?

What percentage vote of the House and the Senate is needed to override a Presidential veto? (A bare majority, two-thirds, three-fourths, or ninety percent / Ninety percent, three-fourths, two-thirds, or a bare majority)?

Political Knowledge (Waves 9). Constructed by counting the number of correct answers to the following items (Cronbach's alpha = 0.80). For the analyses of heterogeneous effects, we quartile-split the variable.

Does Barack Obama favor, oppose, or neither favor nor oppose an amendment to the U.S. Constitution banning marriage between to people who are the same sex? (w9pb1, w11pb1)

Does Barack Obama favor, oppose, or neither favor nor oppose raising federal income taxes for people who make more than \$200,000 per year? (w9pb4, w11pb4)

Does Barack Obama favor, oppose, or neither favor nor oppose raising federal income taxes for people who make less than \$200,000 per year? (w9pb7, w11pb7)

Does Barack Obama favor, oppose, or neither favor nor oppose the U.S. government paying for all necessary medical care for all Americans? (w9pb13, w11pb13)

Imagine that the U.S. government suspects a person in the United States of being a terrorist. Does Barack Obama favor, oppose, or neither favor nor oppose the government being able to put this person in prison for months without ever bringing the person to court and charging him or her with a crime? (w9pb16, w11pb16)

Does Barack Obama favor, oppose, or neither favor nor oppose the U.S. government being required to get a court order before it can listen in on phone calls made by American citizens who are suspected of being terrorists? Citizens of other countries who have come to live in the United States without the permission of the U.S. government are called "illegal immigrants." (w9pb19, w11pb19)

Does Barack Obama favor, oppose, or neither favor nor oppose the U.S. government making it possible for illegal immigrants to become U.S. citizens? (w9pb25, w11pb25)

Does Barack Obama favor, oppose, or neither favor nor oppose the federal government lowering the amount of these gases that power plants are allowed to put into the air? (w9pb27, w11pb27)

Does Barack Obama favor, oppose, or neither favor nor oppose the federal government requiring automakers to build cars that use less gasoline? (w9pb29, w11pb29)

Does Barack Obama favor, oppose, or neither favor nor oppose increasing taxes on gasoline so people either drive less or buy cars that use less gas? (w9pb31, w11pb31)

Does John McCain favor, oppose, or neither favor nor oppose an amendment to the U.S. Constitution banning marriage between two people who are the same sex? (w9pj1, w11pj1)

Does John McCain favor, oppose, or neither favor nor oppose raising federal income taxes for people who make more than \$200,000 per year? (w9pj4, w11pj4)

Does John McCain favor, oppose, or neither favor nor oppose raising federal income taxes for people who make less than \$200,000 per year? (w9pj7, w11pj7)

Does John McCain favor, oppose, or neither favor nor oppose the U.S. government paying for all necessary medical care for all Americans? (w9pj13, w11pj13)

Imagine that the U.S. government suspects a person in the United States of being a terrorist. Does John McCain favor, oppose, or neither favor nor oppose the government being able to put this person in prison for months without ever bringing the person to court and charging him or her with a crime? (w9pj16, w11pj16)

Does John McCain favor, oppose, or neither favor nor oppose the U.S. government being required to get a court order before it can listen in on phone calls made by American citizens who are suspected of being terrorists? Citizens of other countries who have come to live in the United States without the permission of the U.S. government are called "illegal immigrants." (w9pj19, w11pj19)

Does John McCain favor, oppose, or neither favor nor oppose the U.S. government making it possible for illegal immigrants to become U.S. citizens? (w9pj25, w11pj25)

Does John McCain favor, oppose, or neither favor nor oppose the federal government lowering the amount of these gases that power plants are allowed to put into the air? (w9pj27, w11pj27)

Does John McCain favor, oppose, or neither favor nor oppose the federal government requiring automakers to build cars that use less gasoline? (w9pj29, w11pj29)

Does John McCain favor, oppose, or neither favor nor oppose increasing taxes on gasoline so people either drive less or buy cars that use less gas? (w9pj31, w11pj31)

What state does U.S. Senator John McCain represent in Congress? ☐ Arizona ☐ Colorado ☐ New Hampshire ☐ New Mexico What state does U.S. Senator Barack Obama represent in Congress? ☐ Illinois ☐ Michigan ☐ Indiana ☐ New Jersey (w9v1, w11wv1)

What state does U.S. Senator Barack Obama represent in Congress? ☐ Illinois ☐ Michigan ☐ Indiana ☐ New Jersey (w9v2, w11wv2)

What is John McCain's religion? Is he Christian, Jewish, Muslim, Buddhist, or not religious? ☐ Christian ☐ Jewish ☐ Muslim ☐ Buddhist ☐ not religious (w9v4, w11wv4)

Before he was elected to the U.S. Congress, where did Barack Obama work? ☐ A state legislature ☐ The U.S. military ☐ An oil company ☐ A television station (w9v5, w11wv5)

Before he was elected to the U.S. Congress, where did John McCain work? ☐ A state legislature ☐ The U.S. military ☐ An oil company ☐ A television station (w9v6, w11wv6)

Does John McCain favor, oppose, or neither favor nor oppose setting a deadline for withdrawing all U.S. troops from Iraq? (w9q8, w11q8)

Does Barack Obama favor, oppose, or neither favor nor oppose setting a deadline for withdrawing all U.S. troops from Iraq? (w9q10, w11q10)

Political Knowledge (Waves 11). 27-point scale, constructed from the number of correct answers to the same questions as Wave 9 (Cronbach's alpha = 0.79)

2004 Turnout. Coded 1 for "definitely voted," 0 for "definitely did not vote" and "not completely sure." Constructed from der19.

Political Participation (Wave 6). Nine-point scale, constructed from the following questions (Cronbach's alpha = 0.77):

Have you done this, or have you never done it? Joined in a protest march, rally, or demonstration (w6y21)

Have you done this, or have you never done it? Attended a meeting of a town or city government or school board (w6y22)

Have you done this, or have you never done it? Signed a petition on the Internet about a political or social issue (w6y23)

Have you done this, or have you never done it? Signed a petition on paper about a political or social issue (w6y24)

Have you done this, or have you never done it? Gave money to a religious organization (w6y25)

Have you done this, or have you never done it? Not counting a religious organization, gave money to any other organization concerned with a political or social issue (w6y26)

Have you done this, or have you never done it? Attended a meeting to talk about political or social concerns (w6y27)

Have you done this, or have you never done it? Invited someone to attend a meeting about political or social concerns (w6y28)

Have you done this, or have you never done it? Distributed information or advertisements supporting a political or social interest group (w6y29)

Internet News Exposure (Waves 1, 9, 10, 19) was measured based self-reported frequency with which one uses the Internet for news, which was rescaled to 0-1. The question wordings are as follows:

During a typical week, how many days do you watch or read news on the Internet, not including sports? ___ days (w1h3, w9f3, w10f3, w19f3)

Mass Media News Exposure (Waves 1, 9, 10, 19) was measured by combining the frequencies with which one uses three channels (TV, radio and news paper) for news, which was

rescaled to 0-1. (Cronbach's alphas = 0.37, 0.40, 0.42, 0.43). The question wordings are as follows:

During a typical week, how many days do you watch news on TV, not including sports? ___ days (w1h1, w9f1, w10f1, w19f1)

During a typical week, how many days do you listen to news on the radio, not including sports? ___ days (w1h2, w9f2, w10f2, w19f2)

During a typical week, how many days do you read news in a printed newspaper, not including sports? ___ days (w1h4, w9f4, w10f4, w19f4)

Political Interest (Waves 1, 2, 9, 10, 11, 19). Five-point scale, constructed from the following question:

How interested are you in information about what's going on in government and politics? (Extremely interested, very interested, moderately interested, slightly interested, or not interested at all / Not interested at all, slightly interested, moderately interested, very interested, or extremely interested)? (w1k1, w2g1, w9h1, w10h1, w11h1, w19h1)

Political Discussion (Waves 1, 2, 9, 10, 11, 19). Constructed from the following question:

During a typical week, how many days do you talk about politics with family or friends? ___ days (w1k2, w2g2, w9h2, w10h2, w11h2, w19h2)

Party Identification (Waves 1, 9, 11, 17, 19). Seven-point scale, constructed from der08w1, der08w9, der08w11, der08w17, der08w19. Recoded to vary between 0 and 1 so that 0 indicates "Strong Democrat" and 1 indicates "Strong Republican."

Voted for Kerry in 2004. Coded 1 if voted for Kerry, 0 if voted for Bush or "Other."

Constructed from der20.

Attitude toward Muslims (Waves 6, 17). Five-point scale, constructed from the following questions. Recoded to vary between 0 and 1 so that 0 indicates "Extremely Cold" and 1 indicates "Extremely Warm."

Do you feel warm, cold, or neither warm nor cold toward Muslims? (w6ya51, w17ya51) [IF WARM]: Do you feel (extremely warm, moderately warm, or a little warm / a little warm, moderately warm, or extremely warm) toward [Muslims]? (w6ya52, w17ya52) [IF COLD]: Do you feel (extremely cold, moderately cold, or a little cold / a little cold, moderately cold, or extremely cold) toward [Muslims]? (w6ya53, w17ya53)

Attitude toward Iraq (Waves 6, 17). Five-point scale, constructed from the following questions. Recoded to vary between 0 and 1 so that 0 indicates “Extremely Cold” and 1 indicates “Extremely Warm.”

Do you feel warm, cold, or neither warm nor cold toward Iraq? (w6ya63, w17ya63) [IF WARM]: Do you feel (extremely warm, moderately warm, or a little warm / a little warm, moderately warm, or extremely warm) toward [Iraq]? (w6ya64, w17ya64) [IF COLD]: Do you feel (extremely cold, moderately cold, or a little cold / a little cold, moderately cold, or extremely cold) toward [Iraq]? (w6ya65, w17ya65)

Attitude toward Iran (Waves 6, 17). Five-point scale, constructed from the following questions. Recoded to vary between 0 and 1 so that 0 indicates “Extremely Cold” and 1 indicates “Extremely Warm.”

Do you feel warm, cold, or neither warm nor cold toward Iran? (w6ya66, w17ya66) [IF WARM]: Do you feel (extremely warm, moderately warm, or a little warm / a little warm, moderately warm, or extremely warm) toward [Iran]? (w6ya67, w17ya67) [IF COLD]: Do you feel (extremely cold, moderately cold, or a little cold / a little cold, moderately cold, or extremely cold) toward [Iran]? (w6ya68, w17ya68)

Attitude toward Blacks (Waves 2, 10, 20). Five-point scale, constructed from the following questions. Rescaled between 0 and 1 so that 0 indicates “Extremely Cold” and 1 indicates “Extremely Warm.”

Do you feel warm, cold, or neither warm nor cold toward blacks? (w2d11, w10d11, w20d11) [IF WARM]: Do you feel (extremely warm, moderately warm, or a little warm / a little warm, moderately warm, or extremely warm) toward [blacks]? (w2d12,

w10d12, w20d12) [IF COLD]: Do you feel (extremely cold, moderately cold, or a little cold / a little cold, moderately cold, or extremely cold) toward [blacks]? (w2d13, w10d13, w20d13)

Intent to Vote for Obama vs. Giuliani (Waves 1, 2). Coded 1 if Obama, 0 if Giuliani.

Constructed from derw1fog, derw2fog.

Intent to Vote for Obama vs. Romney (Waves 1, 2). Coded 1 if Obama, 0 if Romney.

Constructed from derw1for, derw2for.

Intent to Vote for Obama vs. McCain (Wave 6). Coded 1 if Obama, 0 if McCain.

Constructed from derw6fbom.

Voted for Obama in Primary. Coded 1 if Obama, 0 if other candidates. Constructed from der12.

Attitude toward Obama (Waves 1, 2, 6, 9, 10, 17, 19). Five-point scale, constructed from the following questions. Recoded to vary between 0 and 1 so that 0 indicates “dislike a great deal” and 1 indicates “like a great deal.”

Do you like Barack Obama, dislike him, or neither like nor dislike him? (w1e38, w2e38, w6e38, w9e38, w10e38, w17e38, w19e38) IF LIKE: Do you like [him/her] (a great deal, a moderate amount, or a little / a little, a moderate amount, or a great deal)? (w1e39, w2e39, w6e39, w9e39, w10e39, w17e39, w19e39) IF DISLIKE: Do you dislike [him/her] (a great deal, a moderate amount, or a little / a little, a moderate amount, or a great deal)? (w1e40, w2e40, w6e40, w9e40, w10e40, w17e40, w19e40)

Affects toward Obama (Waves 2, 9, 11, 17, 19). Multi-item index, consisting of the four affects measured as below (Cronbach’s alpha = 0.85, 0.86, 0.89, 0.88, 0.89). Negative emotions are reverse coded for the index. Rescaled to 0-1 so that 0 indicates most negative emotions and 1 indicates most positive emotions.

When you think about Barack Obama, how angry does he make you feel? (Extremely angry, very angry, moderately angry, slightly angry, or not angry at all / Not angry at

all, slightly angry, moderately angry, very angry, or extremely angry)? (w2t7, w9t7, w11wt7, w17wt7, w19wt7)

When you think about Barack Obama, how hopeful does he make you feel? (Extremely hopeful, very hopeful, moderately hopeful, slightly hopeful, or not hopeful at all? / Not hopeful at all, slightly hopeful, moderately hopeful, very hopeful, or extremely hopeful?) (w2t8, w9t8, w11wt8, w17wt8, w19wt8)

When you think about Barack Obama, how afraid does he make you feel? (Extremely afraid, very afraid, moderately afraid, slightly afraid, or not afraid at all / Not afraid at all, slightly afraid, moderately afraid, very afraid, or extremely afraid)? (w2t9, w9t9, w11wt9, w17wt9, w19wt9)

When you think about Barack Obama, how proud does he make you feel? (Extremely proud, very proud, moderately proud, slightly proud, or not proud at all / Not proud at all, slightly proud, moderately proud, very proud, or extremely proud)? (w2t10, w9t10, w11wt10, w17wt10, w19wt10)

Anti-Black Prejudice (Wave 9). Multi-item index, consisting of 9 items (Cronbach alpha=0.76). Each item was rescaled to 0-1 so that 0 indicates least, and 1 indicates most prejudiced/negative responses against African Americans.

Do you favor, oppose, or neither favor nor oppose the federal government in Washington seeing to it that -- blacks get fair treatment in jobs? (w9zb1) Do you [favor/oppose] that (a great deal, moderately, or a little? / a little, moderately, or a great deal?) (w9zb2)

Thinking not about Barack Obama but instead thinking / Thinking) about all of the (/ other) black people who could be president in the future, does the idea of a black person being president make you feel (extremely uncomfortable, very uncomfortable, moderately uncomfortable, slightly uncomfortable, or not uncomfortable at all / not uncomfortable at all, slightly uncomfortable, moderately uncomfortable, very uncomfortable, or extremely uncomfortable)? (w9zb3)

(Thinking not about Barack Obama but instead thinking / Thinking) about all of the (/ other) black people who could be president in the future, does the idea of a black person being president make you feel (extremely pleased, very pleased, moderately pleased, slightly pleased, or not pleased at all / not pleased at all, slightly pleased, moderately pleased, very pleased, or extremely pleased)? (w9zb4)

Do you personally hope that the United States has a black president in your lifetime, do you hope the United States does not have a black president in your lifetime, or do you not hope either way? (w9zb5)

Do you think that most white candidates who run for political office are better suited to be an elected official than are most black candidates, that most black candidates are better

suited to be an elected official than are most white candidates, or do you think white and black candidates are equally suited to be an elected official? (w9zb7) A great deal better suited, moderately better suited, or slightly better suited? (w9zb8)

Do you think that most white candidates who run for political office are better suited in terms of their intelligence to serve as an elected official than are most black candidates, that most black candidates are better suited in terms of their intelligence to serve as an elected official than are most white candidates, or do you think white and black candidates are equally suited in terms of their intelligence to serve as an elected official? (w9zb9) A great deal better suited, moderately better suited, or slightly better suited? (w9zb10)

Would you say that blacks have too much influence in American politics, just about the right amount of influence in American politics, or too little influence in American politics? (w9zb23)

How often have you felt sympathy for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w9zb24)

How often have you felt admiration for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w9zb25)

Anti-Black Prejudice (Wave 11). Index, consisting of 8 items (Cronbach alpha=0.73).

Each item was rescaled to 0-1 so that 0 indicates least, and 1 indicates most prejudiced/negative responses against African Americans.

Do you favor, oppose, or neither favor nor oppose the federal government in Washington seeing to it that -- blacks get fair treatment in jobs? (w11zb1) Do you [favor/oppose] that (a great deal, moderately, or a little? / a little, moderately, or a great deal?) (w11zb2)

Does a black person having been elected President make you feel (extremely uncomfortable, very uncomfortable, moderately uncomfortable, slightly uncomfortable, or not uncomfortable at all / not -- uncomfortable at all, slightly uncomfortable, moderately uncomfortable, very uncomfortable, or extremely uncomfortable)? (w11zb3b)

Does a black person having been elected President make you feel (extremely pleased, very pleased, moderately pleased, slightly pleased, or not pleased at all / not pleased at all, slightly pleased, moderately pleased, very pleased, or extremely pleased)? (w11zb4b)

Do you think that most white candidates who run for political office are better suited to be an elected official than are most black candidates, that most black candidates are better suited to be an elected official than are most white candidates, or do you think white and

black candidates are equally suited to be an elected official? (w11zb7) A great deal better suited, moderately better suited, or slightly better suited? (w11zb8)

Do you think that most white candidates who run for political office are better suited in terms of their intelligence to serve as an elected official than are most black candidates, that most black candidates are better suited in terms of their intelligence to serve as an elected official than are most white candidates, or do you think white and black candidates are equally suited in terms of their intelligence to serve as an elected official? (w11zb9) A great deal better suited, moderately better suited, or slightly better suited? (w11zb10). ”

Would you say that blacks have too much influence in American politics, just about the right amount of influence in American politics, or too little influence in American politics? (w11zb23)

How often have you felt sympathy for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w11zb24)

How often have you felt admiration for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w11zb25)

Anti-Black Prejudice (Wave 17). Index, consisting of 6 items (Cronbach alpha=0.64).

Coded so that 0 indicates least prejudiced, and 1 indicates most prejudiced against African Americans.

Do you favor, oppose, or neither favor nor oppose the federal government in Washington seeing to it that -- blacks get fair treatment in jobs? (w17x1) Do you [favor/oppose] that (a great deal, moderately, or a little? / a little, moderately, or a great deal?) (w17x2)

Do you think that most white candidates who run for political office are better suited to be an elected official than are most black candidates, that most black candidates are better suited to be an elected official than are most white candidates, or do you think white and black candidates are equally suited to be an elected official? (w17x7) A great deal better suited, moderately better suited, or slightly better suited? (w17x8)

Do you think that most white candidates who run for political office are better suited in terms of their intelligence to serve as an elected official than are most black candidates, that most black candidates are better suited in terms of their intelligence to serve as an elected official than are most white candidates, or do you think white and black candidates are equally suited in terms of their intelligence to serve as an elected official? (w17x9) A great deal better suited, moderately better suited, or slightly better suited? (w17x10)

Would you say that blacks have too much influence in American politics, just about the right amount of influence in American politics, or too little influence in American politics? (w17x23) Rescaled to 0-1 so that 0 indicates “too little influence” and 1 indicates “too much influence.”

How often have you felt sympathy for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w17x24) Rescaled to 0-1 so that 0 indicates “never” and 1 indicates “always.”

How often have you felt admiration for blacks? (Always, most of the time, about half the time, once in a while, or never / Never, once in a while, about half the time, most of the time, or always)? (w17x25) Rescaled to 0-1 so that 0 indicates “never” and 1 indicates “always.”

Democratic Social Network. The number of Democratic discussants in one’s social network was counted the following questions. Rescaled to 0-1 so that 0 indicates none, and 1 indicates 3 Democrats.

During the last six months, did you talk with anyone face-to-face, on the phone, by email, or in any other way about government or elections, or did you not do this with anyone during the last six months? (w9zd1) What are the first names of the people who you talked with about government or elections during the past six months? (w9zd2)

Generally speaking, does (NAME) probably think of (himself/herself) as a [Democrat/Republican], [Republican/Democrat], Independent, or something else? (w9zd12_1, w9zd12_2, w9zd12_3, w9zd13_1, w9zd13_2, w9zd13_3)

Economic Perception (Wave 9, 11) Five-point scale, constructed from the following question. This variable was rescaled 0–1 such that 0 is “Much Worse”, 0.25 is “Somewhat Worse”, 0.5 is “About the Same”, 0.75 is “Somewhat Better”, and 1 is “Much Better.”

Compared to 2001, would you say the following is now (much better, somewhat better, about the same, somewhat worse, or much worse / much worse, somewhat worse, about the same, somewhat better, or much better)? The nation’s economy (w9r1, w11u1).

Republican Social Network. The number of Republican discussants in one’s social network was counted using the questions noted above.

Implicit Attitudes toward Obama. (Waves 9, 10). The Affective Misattribution Procedure (AMP) was used. The AMP measures implicit attitudes by flashing a picture of an object (e.g., candidate) on the screen for a fraction of a second, flashing a Chinese character on the screen for a longer fraction of a second, and then asking participants to say whether the Chinese character was pleasant or unpleasant. The respondents were randomly assigned to see faces of Obama and McCain or faces of non-famous black and white young men in Wave 9 (indicated by *w9amp_ver* in the ANES datafile). The assignment to the task was flipped in Wave 10 such that those saw the candidates' faces in Wave 9 saw non-famous men in Wave 10 (indicated by *w10amp_ver* in the ANES datafile). The AMP task was repeated for 48 symbol-face pairings in each Wave (see DeBell et al. 2010, 117 for more information).

For implicit attitudes toward Obama, people's reactions to the pairings of Obama's pictures and Chinese characters were used. The choice between "pleasant" and "unpleasant" was counted and rescaled to 0-1 so that 0 indicates all 48 pairings of the Chinese symbols and Obama's pictures were noted "unpleasant" and 1 indicates all 48 pairings were noted "pleasant."

Push the "P" key if you think the drawing is more pleasant than average and the "Q" key if it is less pleasant than average. But this time, you'll see a quick flash of a person's face before you see each drawing. After the flash of the face, the drawing will appear for only a second, and then the screen will turn gray. So, watch the screen carefully so you can see each drawing and decide whether it's more pleasant than the average drawing or less pleasant than the average drawing...

(Wave 9: *w9amp_q2_face1_choice*, *w9amp_q2_face2_choice*, *w9amp_q2_face3_choice* ... *w9amp_q2_face48_choice* if *w9amp_ver=1*)

(Wave 10: *w10amp_q2_face1_choice*, *w10amp_q2_face2_choice*, *w10amp_q2_face3_choice*, ... , *w10amp_q2_face48_choice* if *w10amp_ver=2*)

Implicit Anti-Black Bias. (Waves 9, 10). The AMP was used. For implicit anti-black bias, people's reactions to the pairings of non-famous black men's faces and Chinese characters were used. The choice between "pleasant" and "unpleasant" was counted and rescaled to 0-1 so that 0

indicates all 48 pairings of the Chinese symbols and black men's pictures were noted "pleasant"
and 1 indicates all 48 pairings were noted "unpleasant."

*(Wave 9: w9amp_q2_face1_choice, w9amp_q2_face2_choice, w9amp_q2_face3_choice ...
w9amp_q2_face48_choice if w9amp_ver=2)*

*(Wave 10: w10amp_q2_face1_choice, w10amp_q2_face2_choice,
w10amp_q2_face3_choice, ... , w10amp_q2_face48_choice if w10amp_ver=1)*

Appendix B: Balance Check

To identify the effect of online rumor circulation, we compared (overtime changes in) rumor beliefs between Group A and B. In this Appendix, we provide balance statistics between Group A and B. The existing research on rumor beliefs and misinformation (e.g., Garrett 2011; Hollander 2010; Nyhan 2010) indicates two broad classes of individual characteristics that are likely correlated with the outcome of interest, which may cause selection bias.¹ First, those who are less attentive to politics are more likely to believe the rumor, because they do not have best available information regarding the issue (e.g., Obama's active involvement in church). Second, those who are predisposed to dislike Obama are also more likely to think he is a Muslim, through selective exposure or motivated cognition.² An important question here is whether Group A and B differ on these characteristics.

With regard to political leaning, on the one hand, it is difficult to explain how people of different partisan stripes would differ systematically in the timing they choose to respond to a survey invitation. On the other hand, it is plausible that those more interested in or informed about politics would be quicker to respond—at a minimum, they would find answering the questionnaires more engaging. In this appendix, we empirically examine these

¹ An omitted variable does not cause a bias unless it is correlated with both the independent and dependent variables.

² Unsurprisingly, we find that both political preference and attentiveness are correlated with rumor belief; for example, bivariate regression models indicate a “knowledge gap” in rumor belief between those most versus least knowledgeable individuals is 49 percentage points ($p < 0.01$) and a “partisan gap” between strong Democrats and Republicans is 21 percentage points ($p < 0.01$). Interestingly, there is no significant interaction between political knowledge and party ID, when these variables are included in a single regression model ($p = 0.99$), indicating that knowledge gap parallels across partisan groups.

possibilities/concerns by checking covariate imbalances in various indicators of political preferences, political attentiveness and demographics.

Table B1 compare the groups on demographics, which include age, gender, race (white), education, religion (Christian), and income, and various indicators of political attentiveness, including political knowledge, turnout in 2004 election, turnout in the primaries, political interest, political participation, Internet news exposure, mass media exposure, political discussion, measured various waves, including wave 9. Table B2 compare the groups on political preferences, including party ID, vote for Kerry in 2004, attitude toward Muslims, attitude toward Iraq, attitude toward Iran, attitude toward blacks, intent to vote for Obama, vote choices for Obama in the primaries and general election, attitudes toward Obama, affects toward Obama, anti-Black prejudices, Democratic social network, and Republican social network and economic perceptions. In each table, we report the estimates of group mean differences, standard error, and p-values for the differences.

In terms of demographic characteristics (Table B1), we find that Group A—those responded on September 10 or later—was younger, less white, less educated, less affluent beyond chance. But two groups were balanced on gender, and Christian religion up to chance. We also find that Group B was more politically attentive on a variety of indicators. Depending in model specifications, we find 18 to 20 significant imbalances, among 28 covariates measuring political attentiveness, all in the direction of Group A being less politically attentive or aware.

On the other hand, we show that there is very little evidence that the groups had different political opinions or attitudes as reported in Table B2. Especially, partisan identity and attitudes toward Obama were nearly identical in Wave 9. More generally, on 46 indicators of political

leaning measured various points in time, we find only one imbalance statistically significant (on economic perception in Wave 9).³

Table B1: Balance Checks on Demographics and Political Attentiveness

	Diff (Weighted)	SE	N	Diff (Unweighted)	SE	N
Panel A: Demographic						
Age	0.035*	(0.015)	2586	0.023*	(0.009)	2757
Female	-0.042	(0.030)	2586	-0.035	(0.020)	2757
White	0.059*	(0.026)	2586	0.035*	(0.014)	2757
Education (\geq Bachelor's degree)	0.079*	(0.021)	2585	0.063*	(0.020)	2732
Christian	0.032	(0.030)	2452	-0.012	(0.020)	2512
Income (\geq 60k)	0.071*	(0.029)	2563	0.083*	(0.021)	2705
Panel B: Pre-treatment Attentiveness						
General Political Knowledge (W2)	0.085*	(0.024)	1138	0.061*	(0.015)	1321
2004 Turnout	0.050	(0.028)	2502	0.025	(0.016)	2569
Primary Turnout	0.032	(0.030)	2554	0.025	(0.021)	2606
Political Interest (W1)	0.059*	(0.026)	1142	0.030*	(0.015)	1423
Political Interest (W2)	0.052	(0.027)	1137	0.022	(0.015)	1319
Political Participation (W6)	0.066*	(0.026)	1144	0.030	(0.016)	1325
Internet News Exposure (W1)	0.143*	(0.033)	1140	0.136*	(0.021)	1421
Mass Media News Exposure (W1)	0.037	(0.024)	1141	0.039*	(0.014)	1422
Political Discussion (W1)	0.076*	(0.027)	1141	0.040*	(0.017)	1422
Political Discussion (W2)	0.063*	(0.027)	1135	0.058*	(0.018)	1317
Panel C: Wave 9 Attentiveness						
Political Knowledge (Candidate Backgrounds/Positions)	0.045*	(0.013)	2584	0.025*	(0.008)	2723
Political Interest	0.011	(0.017)	2586	-0.003	(0.010)	2739
Internet News Exposure	0.083*	(0.022)	2586	0.098*	(0.015)	2740
Mass Media News Exposure	0.011	(0.016)	2586	0.004	(0.010)	2741
Political Discussion	0.036*	(0.017)	2583	0.013	(0.013)	2736
Panel D: Post-Wave 9 Attentiveness						
Political Knowledge (Candidate Backgrounds/Positions, W11)	0.038*	(0.014)	2309	0.022*	(0.008)	2475
Political Interest (W10)	0.026	(0.019)	2312	0.011	(0.011)	2511
Political Interest (W11)	0.043*	(0.018)	2312	0.021	(0.011)	2478
Political Interest (W19)	0.019	(0.020)	1846	0.022	(0.012)	2205
Internet News Exposure (W10)	0.098*	(0.023)	2310	0.096*	(0.016)	2510
Internet News Exposure (W19)	0.064*	(0.027)	1846	0.075*	(0.018)	2205
Mass Media News Exposure (W 9 & 10 Combined)	0.006	(0.016)	2312	0.007	(0.010)	2748
Mass Media News Exposure (W10)	0.010	(0.017)	2311	0.009	(0.011)	2511
Mass Media News Exposure (W19)	-0.007	(0.020)	1847	0.009	(0.012)	2207
Political Discussion (W10)	0.037	(0.021)	2310	0.024	(0.013)	2509
Political Discussion (W11)	0.061*	(0.019)	2308	0.040*	(0.014)	2473
Political Discussion (W19)	0.018	(0.022)	1846	0.031*	(0.014)	2204
Weights		Yes			No	

Note. Group A is the baseline. * $p < 0.05$

³ The observed difference in Wave 9 economic perception probably reflects the collapse of the U.S. economy in September 2008; people in Group A was more likely to say the economy is worse than 2001. This cannot be a plausible alternative explanation for our main finding, because, if anything, it would make people feel more favorable towards Obama (from non-incumbent party), thereby lowering, not increasing, Group A's acceptance of the Muslim rumor in September.

Table B2: Balance Check on Political Leaning

		Diff (Weighted)	SE	N	Diff (Unweighted)	SE	N
Panel A: Pre-treatment							
General	Party ID (W1)	0.000	(0.031)	1140	-0.009	(0.021)	1420
	Voted for Kerry in 2004	0.012	(0.031)	2124	0.008	(0.024)	2179
	Attitude toward Muslims (W6)	0.021	(0.020)	1143	-0.015	(0.014)	1325
	Attitude toward Iraq (W6)	0.029	(0.017)	1143	-0.003	(0.012)	1325
	Attitude toward Iran (W6)	0.020	(0.016)	1143	-0.000	(0.013)	1325
Obama	Attitude toward Blacks (W2)	0.012	(0.018)	1137	0.001	(0.012)	1318
	Vote for Obama vs. Giuliani (W1)	0.038	(0.048)	1114	0.010	(0.029)	1390
	Vote for Obama vs. Romney (W1)	0.035	(0.047)	1114	0.030	(0.029)	1391
	Vote for Obama vs. Giuliani (W2)	0.028	(0.047)	1121	0.017	(0.030)	1302
	Vote for Obama vs. Romney (W2)	0.014	(0.047)	1123	0.024	(0.030)	1305
	Vote for Obama vs. McCain (W6)	-0.023	(0.047)	1123	-0.007	(0.030)	1304
	Voted for Obama in Primary	0.038	(0.035)	1447	0.022	(0.026)	1474
	Attitude toward Obama (W2)	0.023	(0.028)	1136	-0.002	(0.019)	1317
	Attitude toward Obama (W6)	-0.014	(0.030)	1145	0.003	(0.021)	1326
	Affects toward Obama (W2)	0.024	(0.025)	1134	0.003	(0.017)	1316
Panel B: Wave 9							
General	Party ID	0.009	(0.021)	2585	-0.001	(0.015)	2733
	Anti-Black Prejudice#	-0.001	(0.010)	2584	-0.002	(0.006)	2660
	Implicit Anti-Black Bias	0.015	(0.024)	1317	0.014	(0.018)	1317
	Democratic Social Network	-0.018	(0.033)	2071	0.008	(0.024)	2098
	Republican Social Network	-0.004	(0.032)	2071	-0.031	(0.023)	2098
Obama	Economy is Better since 2001	0.034*	(0.012)	2585	0.033*	(0.009)	2692
	Intent to vote For Obama	-0.034	(0.030)	2556	-0.010	(0.021)	2715
	Attitude toward Obama	0.001	(0.020)	2384	-0.014	(0.015)	2516
	Affects toward Obama	0.006	(0.018)	2581	0.002	(0.012)	2682
	Implicit Attitude toward Obama	0.006	(0.028)	1267	0.000	(0.020)	1267
Panel C: Wave 10 or Later							
General	Party ID (W11)	-0.006	(0.025)	2311	-0.014	(0.017)	2477
	Party ID (W17)	-0.021	(0.027)	1980	-0.032	(0.018)	2213
	Party ID (W19)	0.006	(0.028)	1846	-0.014	(0.018)	2204
	Attitude toward Muslims (W17)	0.010	(0.019)	1842	-0.003	(0.011)	2203
	Attitude toward Iraq (W17)	0.023	(0.016)	1843	0.005	(0.010)	2205
	Attitude toward Iran (W17)	0.011	(0.018)	1843	-0.003	(0.010)	2205
	Attitude toward Blacks (W10)	0.001	(0.014)	2308	0.001	(0.009)	2510
	Attitude toward Blacks (W20)	-0.017	(0.017)	1772	-0.011	(0.009)	2120
	Anti-Black Prejudice# (W11)	0.014	(0.012)	2311	0.010	(0.007)	2477
	Anti-Black Prejudice# (W17)	0.005	(0.012)	1980	0.009	(0.007)	2212
	Implicit Anti-Black Bias (W10)	-0.012	(0.024)	1116	0.014	(0.017)	1194
	Economy is Better since 2001 (W11)	0.002	(0.011)	2311	-0.001	(0.007)	2475
	Voted for Obama in General Election	0.000	(0.035)	2065	0.000	(0.023)	2259
	Attitude toward Obama (W17)	0.008	(0.026)	1977	-0.005	(0.016)	2218
	Attitude toward Obama (W19)	-0.024	(0.028)	1845	-0.002	(0.017)	2204
Obama	Affects toward Obama (W11)	0.004	(0.020)	2309	-0.001	(0.013)	2475
	Affects toward Obama (W17)	0.007	(0.021)	1977	0.007	(0.013)	2208
	Affects toward Obama (W19)	0.013	(0.023)	1844	0.008	(0.014)	2198
	Implicit Attitude toward Obama (W10)	0.001	(0.029)	1195	-0.006	(0.020)	1270
Weights		Yes			No		

Note. Group A is the baseline. Items used for anti-black prejudice are not identical across waves because of some of Wave 9 items were unavailable or worded differently in later surveys. * $p < 0.05$.

Appendix C: Difference-in-Differences Setup

In this appendix, we provide a formal description of the difference-in-differences (DD) setup for estimating the effect of an online circulation of “my Muslim faith” messages.

Let $Y_i(1)$ and $Y_i(0)$ be, respectively, the treated and untreated potential outcomes of respondent i . We will call someone “treated” if a particular rumor has spread by the time one completes a survey. As such, what we mean by “treatment effect” is the effect of rumor circulation (the ITT effect), not the effect of exposure. The outcome Y is the belief about Obama’s religion, where 1 indicates one believes that Obama is a Muslim and 0 indicates one believes Obama is a Christian. The average treatment effect of rumor circulation is defined as $E[Y_i(1)] - E[Y_i(0)]$. Given that communication effects typically decay overtime, we distinguish immediate versus lasting treatment effects. Accordingly, the immediate average treatment effects on Group A and Group B in Wave 9 for are defined as:

$$\tau_{A9} = E[Y_i(1)|G_i = A, W = 9] - E[Y_i(0)|G_i = A, W = 9]$$

$$\tau_{B9} = E[Y_i(1)|G_i = B, W = 9] - E[Y_i(0)|G_i = B, W = 9],$$

where G_i denotes Respondent i ’s Group (A or B) and W denotes Wave (9 or 11) and the long-run average treatment effect on Group A and Group B in Wave 11 are defined as:

$$\tau_{A11} = E[Y_i(1)|G_i = A, W = 11] - E[Y_i(0)|G_i = A, W = 11]$$

$$\tau_{B11} = E[Y_i(1)|G_i = B, W = 11] - E[Y_i(0)|G_i = B, W = 11].$$

We cannot directly estimate any of these quantities because each equation contains an unobserved term. For example, we do not see the untreated Group A in Wave 9, $E[Y_i(0)|G = A, W = 9]$. As an alternative, we compare group means since respondents’ treatment status varies by group in Wave 9 such that only those in Group A receive the

treatment. However, since people self-selected into the groups—by virtue of choosing when to take the survey—the group difference in Wave 9, $E[Y_i(1)|G_i = A, W = 9] - E[Y_i(0)|G_i = B, W = 9]$ does not represent an average treatment effect of interest. This term, instead, is the sum of the average treatment effect on Group A, $\tau_{A9} = E[Y_i(1)|G_i = A, W = 9] - E[Y_i(0)|G_i = A, W = 9]$, and selection bias, denoted α . Selection bias is defined as the difference that the groups would have exhibited anyway without the treatment: $\alpha = E[Y_i(0)|G_i = A, W = 9] - E[Y_i(0)|G_i = B, W = 9]$. In short:

Equation 1: Group Difference in Wave 9 = Immediate Effect on Group A + Selection Bias

$$\begin{aligned}
 & \underbrace{E[Y_i|G_i = A, W = 9] - E[Y_i|G_i = B, W = 9]}_{\text{Observed Group Difference in W9}} \\
 &= \underbrace{E[Y_i(1)|G_i = A, W = 9] - E[Y_i(0)|G_i = A, W = 9]}_{\text{Immediate Treatment Effect on Group A}} \\
 &+ \underbrace{E[Y_i(0)|G_i = A, W = 9] - E[Y_i(0)|G_i = B, W = 9]}_{\text{Selection Bias}} = \tau_{A9} + \alpha.
 \end{aligned}$$

To identify the effect of rumor circulation based on Equation 1 alone, one would have to assume that α is zero. This is a strong assumption because we know, for instance, that Group B is more politically interested (see Appendix B). But the fact respondents were asked about Obama’s religion once again in November (Wave 11), a time when both Groups had then been treated, can mitigate the concern about the group differences.

To do so, we replace the selection-bias-is-zero assumption with the “parallel trends assumption,” which states that, in the absence the treatment, the group *difference* (i.e., selection

bias) will remain the same across waves—or, equivalently, that the overtime *changes* will be the same between groups (e.g., Angrist and Pischke 2009, 228). That is, we assume:

Identifying Assumption 1: Parallel Trends Assumption

$$\begin{aligned}
\alpha &= \underbrace{E[Y_i(0)|G_i = A, W_i = 9] - E[Y_i(0)|G_i = B, W_i = 9]}_{\text{Selection Bias in W9}} \\
&= \underbrace{E[Y_i(0)|G_i = A, W_i = 11] - E[Y_i(0)|G_i = B, W_i = 11]}_{\text{Selection Bias in W11}} \\
\omega &= \underbrace{E[Y_i(0)|G_i = A, W = 11] - E[Y_i(0)|G_i = A, W = 9]}_{\text{Wave Effect on Group A}} \\
&= \underbrace{E[Y_i(0)|G_i = B, W = 11] - E[Y_i(0)|G_i = B, W = 9]}_{\text{Wave Effect on Group B}}
\end{aligned}$$

where α is selection bias and ω is a “wave effect” which we define as the overtime change that would have taken place without the treatment. Under this assumption, the difference in group differences between waves—or, equivalently, the difference in overtime changes between groups—cannot be attributed to selection bias.

To illustrate, note that the difference between the two groups in Wave 11 can no longer reflect τ_{A9} as *both* groups had been treated by then. Instead, the remaining difference will consist of the selection bias term *that we assume to be a constant* (α) and possibly the difference in the lasting treatment effects ($\tau_{A11} - \tau_{B11}$). In short:

Equation 2: Group Difference in Wave 11 = Selection Bias + Difference in Lasting Effects

$$\begin{aligned}
& \underbrace{E[Y_i|G_i = A, W = 11] - E[Y_i|G_i = B, W = 11]}_{\text{Observed Group Difference in W11}} \\
&= \underbrace{E[Y_i(1)|G_i = A, W = 11] - E[Y_i(0)|G_i = A, W = 11]}_{\text{Lasting Treatment Effect on Group A}} \\
&+ \underbrace{E[Y_i(0)|G_i = A, W = 11] - E[Y_i(0)|G_i = B, W = 11]}_{\text{Selection Bias}} \\
&- \underbrace{(E[Y_i(1)|G_i = B, W = 11] - E[Y_i(0)|G_i = B, W = 11])}_{\text{Lasting Treatment Effect on Group B}} \\
&= \tau_{A11} + \alpha - \tau_{B11}.
\end{aligned}$$

Accordingly, we calculate the DD by subtracting Equation 2 from Equation 1: DD = Equation 1 – Equation 2 = $\tau_{A9} + \alpha - (\tau_{A11} + \alpha - \tau_{B11}) = \tau_{A9} + (\tau_{B11} - \tau_{A11})$, which states that the DD captures the immediate treatment effect on Group A and the difference in lasting treatment effects between the groups. In a nutshell, the DD approach allows us to cancel out the selection bias term by subtracting group difference in Wave 11 from its counterpart in Wave 9 under the key identifying assumption.

We can calculate the DD also by comparing the overtime change among Group B to that of Group A. Since Group B experienced the treatment period between the two waves, their change score reflects the average treatment effect among those in Group B lasting in Wave 11 (τ_{11}) and the “wave effect” (ω).

Equation 3: Overtime Change among Group B = Lasting Effect on Group B + Wave Effect

$$\begin{aligned}
& \underbrace{E[Y_i|G_i = B, W = 11] - E[Y_i|G_i = B, W = 9]}_{\text{Over-time Change among Group B}} \\
&= \underbrace{E[Y_i(1)|G_i = B, W = 11] - E[Y_i(0)|G_i = B, W = 11]}_{\text{Lasting Treatment Effect on Group B}} \\
&+ \underbrace{E[Y_i(0)|G_i = B, W = 11] - E[Y_i(1)|G_i = B, W = 9]}_{\text{Wave Effect}} \\
&= \tau_{B11} + \omega.
\end{aligned}$$

Since people in Group A's Wave 9 responses were collected after the treatment period, their overtime change captures the decay of the average treatment effects across the two waves ($\tau_{A11} - \tau_{A9}$) and the wave effect (ω) that is assumed to be parallel across the groups under the DD framework:

Equation 4: Overtime Change among Group A = Decay for Group A + Wave Effect

$$\begin{aligned}
& \underbrace{E[Y_i|G_i = A, W = 11] - E[Y_i|G_i = A, W = 9]}_{\text{Over-time Change among Group A}} \\
&= \underbrace{E[Y_i(1)|G_i = A, W = 11] - E[Y_i(0)|G_i = A, W = 11]}_{\text{Lasting Treatment Effect on Group A}} \\
&+ \underbrace{E[Y_i(0)|G_i = A, W = 11] - E[Y_i(0)|G_i = A, W = 9]}_{\text{Wave Effect}} \\
&- \underbrace{(E[Y_i(1)|G_i = A, W = 9] - E[Y_i(0)|G_i = A, W = 9])}_{\text{Immediate Treatment Effect on Group A}} \\
&= \tau_{A11} + \omega + -\tau_{A9} = \underbrace{\tau_{A11} - \tau_{A9}}_{\text{Decay}} + \omega.
\end{aligned}$$

Subtracting Equation 4 from Equation 3, again, we get: DD = Equation 3 – Equation 4 =

$$\tau_{B11} + \omega - (\tau_{A11} - \tau_{A9} + \omega) = \tau_{A9} + (\tau_{B11} - \tau_{A11}).$$

Note that while the DD calculator purges the selection bias term out of the equation, it does not identify one of the average treatment effects of interest (e.g., τ_{A9}). Instead, it gives us the sum of the immediate treatment effect on Group A and the difference in lasting treatment effects between the groups. This is because our research design is a reverse of a typical DD setup. In a typical DD design, a baseline selection bias is measured in T1 and a differential treatment takes place in T2. In our case, outcomes in T2 (Wave 11) serve essentially as a baseline—what T1 outcomes do in a typical setup—but Wave 11 does not give a “clean” baseline in the sense that Wave 11 outcomes can reflect a difference in long-lasting effects across groups, in addition to selection bias.

For identification, we further assume that the treatment had the same average effects on Group A and Group B. That is, we assume:

Identifying Assumption 2: Homogeneous Treatment Effects Assumption

$$\tau_9 = \tau_{A9} = \tau_{B9} \text{ and } \tau_{11} = \tau_{A11} = \tau_{B11},$$

where τ_9 and τ_{11} are, respectively the immediate and long run effects that are assumed to be homogeneous across groups. Now, the DD equation can be rewritten as: $DD = \tau_9 + (\tau_{11} - \tau_{11}) = \tau_9$, which is the average effect of the rumor transmission in Wave 9.

As with any DD approach, Assumption 1 (the parallel trends assumption) is central to establishing the causal effect of the treatment. In the main text and Appendix D, we carefully examine several scenarios under which Assumption 1 may not hold and rule them out with a range of placebo tests. On the other hand, we argue Assumption 2 (the homogeneous effects assumption) is plausible and—even if implausible—relatively inconsequential. In Appendix E, we elaborate on this claim.

The following list recaps the definitions of the key parameters discussed in this appendix.

Key Parameters:

τ_{A9} : Treatment Effect in Wave 9 for Group A

τ_{A11} : Treatment Effect Lasting in Wave 11 for Group A

$\tau_{A11} - \tau_{A9}$: Treatment Effect Decay for Group A

τ_{B9} : Treatment Effect in Wave 9 for Group B

τ_{B11} : Treatment Effect Lasting in Wave 11 for Group B

$\tau_{B11} - \tau_{B9}$: Treatment Effect Decay for Group B

α : Selection Bias

ω : Wave Effect

Appendix D: Parallel Trends Assumption

As we argued in the main text and Appendix C, despite apparent imbalances in political attentiveness between Groups A and B that, the treatment effect can be identified under the assumption that selection bias is stable over time—that is, the overtime change in the dependent variable would have been the same in the absence of rumor circulation. In this appendix, we provide a set of evidence that corroborates the parallel trends assumption.

One of the possibilities that may potentially undermine our assumption is that differences in political knowledge (and other indicators of political engagement) narrowed down by November. Then, the differential trends that our DD estimator captures may be our DD estimator is picking up the effects of “non-parallel” political learning (i.e., Group A’s political knowledge caught up to that of Group B between September and November), instead of the effects of the circulation of political rumor. In Table D1, we empirically check if A and B show (non)parallel trend in learning between Waves 9 and 11, based on 27 repeatedly measured knowledge items and an additive political knowledge index. Weighted OLS regressions estimating the analogs of the main regression model (Equation 1) show that overtime change (i.e., increase) in general political knowledge was just about the same for both groups (Diff = 0.007, SE = 0.012, $p = 0.61$).⁴ Looking at the 27 items individually, we find only one statistically discernable difference at the 5% level (and it was B who learned more). Indeed, without weights, the groups’ overall learning trends appear even more balanced (Diff = 0.0003, SE = 0.006), which is estimated precisely enough to rule out the possibility that A learned more by even a small margin (95% CI = [-0.012, 0.013]).

⁴ A and B are coded as 0 and 1, respectively; a positive coefficient indicates that B learned more.

Table D1: Parallel Trend Check on “Learning” between Waves 9 and 11

	DD	SE	Intercept	SE	N	DD	SE	Intercept	SE	N
Political Knowledge (Index)	0.007	(0.011)	0.032*	(0.009)	2309	0.000	(0.006)	0.037*	(0.006)	2462
McCain’s state	-0.012	(0.034)	0.172*	(0.030)	2233	-0.034	(0.020)	0.168*	(0.018)	2364
Obama’s state	-0.044	(0.027)	0.127*	(0.024)	2254	-0.033	(0.017)	0.108*	(0.015)	2385
McCain’s religion	0.047*	(0.023)	-0.038	(0.021)	2262	0.005	(0.011)	0.009	(0.010)	2393
Obama’s job before senator	-0.037	(0.028)	0.038	(0.026)	2220	-0.019	(0.015)	0.017	(0.013)	2348
McCain’s job before senator	0.053	(0.036)	-0.047	(0.030)	2234	0.006	(0.025)	0.002	(0.021)	2361
Obama on same sex marriage	0.015	(0.040)	0.032	(0.035)	2302	0.001	(0.028)	0.032	(0.024)	2454
Obama on income tax over 200k	-0.020	(0.036)	0.096*	(0.032)	2303	-0.017	(0.020)	0.070*	(0.018)	2453
Obama on income tax under 200k	0.034	(0.039)	0.119*	(0.034)	2304	0.040	(0.026)	0.102*	(0.022)	2455
Obama on healthcare	0.017	(0.030)	0.025	(0.027)	2302	0.001	(0.021)	0.050*	(0.018)	2452
Obama on terror suspect habeas	-0.042	(0.036)	0.073*	(0.031)	2300	-0.032	(0.023)	0.057*	(0.020)	2451
Obama on terror suspect wiretap	-0.020	(0.037)	0.016	(0.031)	2299	-0.041	(0.026)	0.038*	(0.022)	2450
Obama on illegal immigrants	0.044	(0.039)	-0.016	(0.035)	2299	0.017	(0.025)	0.003	(0.021)	2449
Obama on power plant emissions	0.005	(0.036)	0.059	(0.031)	2293	0.003	(0.025)	0.052*	(0.021)	2441
Obama on automaker requirements	0.001	(0.032)	0.059*	(0.028)	2300	0.000	(0.019)	0.047*	(0.016)	2446
Obama on gasoline taxes	0.044	(0.038)	-0.012	(0.034)	2290	0.031	(0.025)	-0.004	(0.022)	2436
Obama on Iraq deadline	0.028	(0.027)	-0.005	(0.023)	2296	0.014	(0.018)	0.000	(0.016)	2436
McCain on same sex marriage	-0.004	(0.037)	0.017	(0.032)	2297	0.005	(0.027)	0.019	(0.022)	2442
McCain on income tax over 200k	0.001	(0.034)	0.055	(0.028)	2296	0.006	(0.023)	0.075*	(0.020)	2442
McCain on income tax under 200k	-0.024	(0.036)	0.037	(0.031)	2299	-0.032	(0.025)	0.050*	(0.021)	2444
McCain on healthcare	-0.024	(0.038)	0.100*	(0.033)	2296	0.003	(0.023)	0.075*	(0.019)	2441
McCain on terror suspect habeas	-0.072	(0.040)	0.048	(0.035)	2294	-0.014	(0.026)	0.000	(0.022)	2439
McCain on terror suspect wiretap	0.054	(0.034)	-0.032	(0.027)	2291	0.020	(0.026)	-0.021	(0.022)	2435
McCain on illegal immigrants	0.023	(0.034)	-0.025	(0.029)	2293	0.005	(0.023)	0.012	(0.020)	2436
McCain on power plant emissions	-0.012	(0.037)	0.016	(0.032)	2291	-0.017	(0.027)	0.036	(0.023)	2435
McCain on automaker requirements	-0.014	(0.031)	-0.005	(0.026)	2296	-0.011	(0.024)	-0.009	(0.020)	2439
McCain on gasoline taxes	0.000	(0.034)	-0.017	(0.029)	2292	0.027	(0.025)	-0.033	(0.021)	2434
McCain on Iraq deadline	-0.002	(0.034)	0.093*	(0.030)	2296	0.013	(0.020)	0.080*	(0.018)	2437
Weights	Yes					No				

Note. Group A is the baseline. “DD” captures Group B’s deviation from Group A’s change—the extent to which B learned more, relative to A. * $p < 0.05$.

Another reasonable concern is that Group B has more stable political beliefs. Politically attentive citizens are more likely to reject attitudinally incongruent information (Zaller 1992; Taber and Lodge 2006). According to this logic, over-time changes in rumor belief may appear to

be different simply because (*misinformed*) Group B respondents were more likely to keep their views intact, despite flows of corrective information from the mainstream media. In Appendix Table D2, we address this concern by demonstrating that Group B was no more likely (in fact, slightly less likely) than Group A to maintain a “placebo” misbelief about Obama—that he *favours* raising income taxes for people making less than \$200,000, a policy that was widely unpopular between Waves 9 and 11.

Table D2: Obama’s Position on Raising Taxes on Incomes under \$200,000 a year by Group and Wave

		Group B	Group A	Group B	Group A
Panel A: Wave 9					
	Oppose	31.46	30.72	29.8	28.9
	Favor	52.1	51.32	55.6	56.3
	Neither Favor Nor Oppose	16.44	17.96	14.6	14.8
	F (2)	0.19 (p = 0.82)		0.11 (p = 0.90)	
Panel B: Wave 11					
	Oppose	22.27	24.49	22.2	24.18
	Favor	67.29	62.09	69.75	66.52
	Neither Favor Nor Oppose	10.43	13.42	8.06	9.3
	F (2)	1.69 (p = 0.19)		1.27 (p = 0.28)	
Weights		Yes		No	

Note. F-statistics are from multinomial regression models where people’s responses on Obama’s position on the tax policy was regressed on group assignment.

Table D3 demonstrates that this piece of misinformation is comparable to the Muslim rumor, in terms of its association with a set of *attitudinal* covariates—thus allowing for a better placebo test than other political knowledge items tapping into more or less “neutral” facts; e.g., Democrats would not refuse to accept that John McCain represents Arizona, not Colorado.

Table D3: Correlations between Individual Characteristics and False Beliefs about Obama

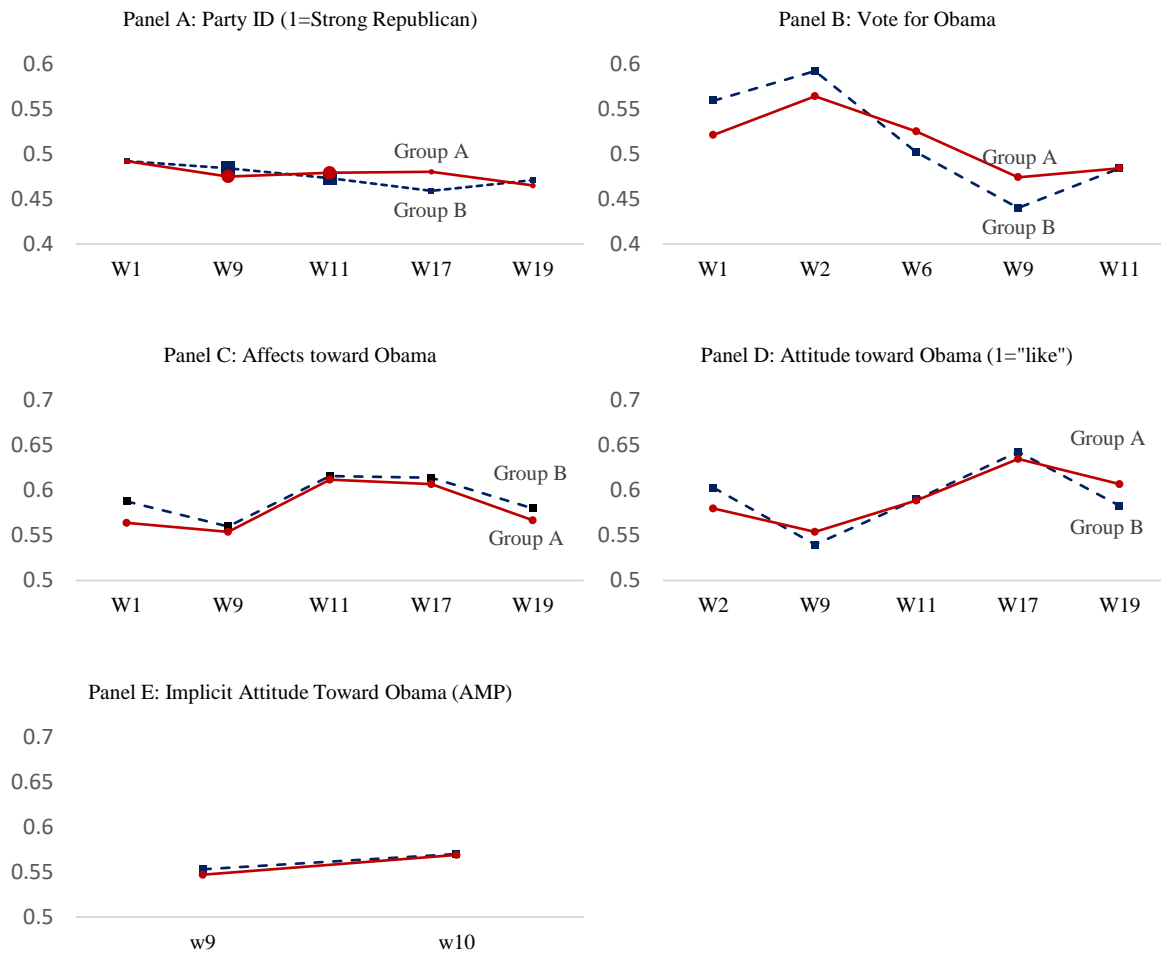
	(1)	(2)	(3)	(4)
	Christian, Not Muslim		Favors Tax Increase for Poor	
Party ID (Wave 9; 1 = Strong Rep)	0.161* (0.035)	0.153* (0.022)	0.304* (0.034)	0.355* (0.023)
Racial Prejudice	0.664* (0.085)	0.657* (0.056)	0.469* (0.086)	0.609* (0.058)
Attitude toward Obama (Wave 9)	-0.333* (0.036)	-0.311* (0.026)	-0.438* (0.036)	-0.465* (0.025)
Intent to Vote for Obama (Wave 9)	-0.207* (0.025)	-0.175* (0.016)	-0.275* (0.024)	-0.296* (0.016)
Weight	Yes	No	Yes	No

Note. * $p < 0.5$ Estimates are bi-variate regression coefficients from OLS models where each false belief about Obama was regressed on the respective independent variable on the list. In Columns 3 and 4, “favor” was coded 1 and other options (“opposes” and “neither favor nor oppose” were coded) were coded 0.

Additionally, in Appendix F, we find that our results hold up when we use a specification that drops all respondents who did not change their rumor belief, which suggests the groups differed in the *direction* of belief change, but not necessarily in *whether* they changed their belief.

More generally, we find that Groups A and B are generally comparable in terms of both their attitudes towards Obama throughout multiple ANES waves (including W9 in September), moving mostly in parallel. Figure D1, which plots several key attitudinal measures reported in Table B2, underscores this point.

Figure D1: Trends in Placebo Variables



Note: The graphs plot group means derived from the same regression models reported in the first column of Table B2. Mean differences in each wave are statistically insignificant.

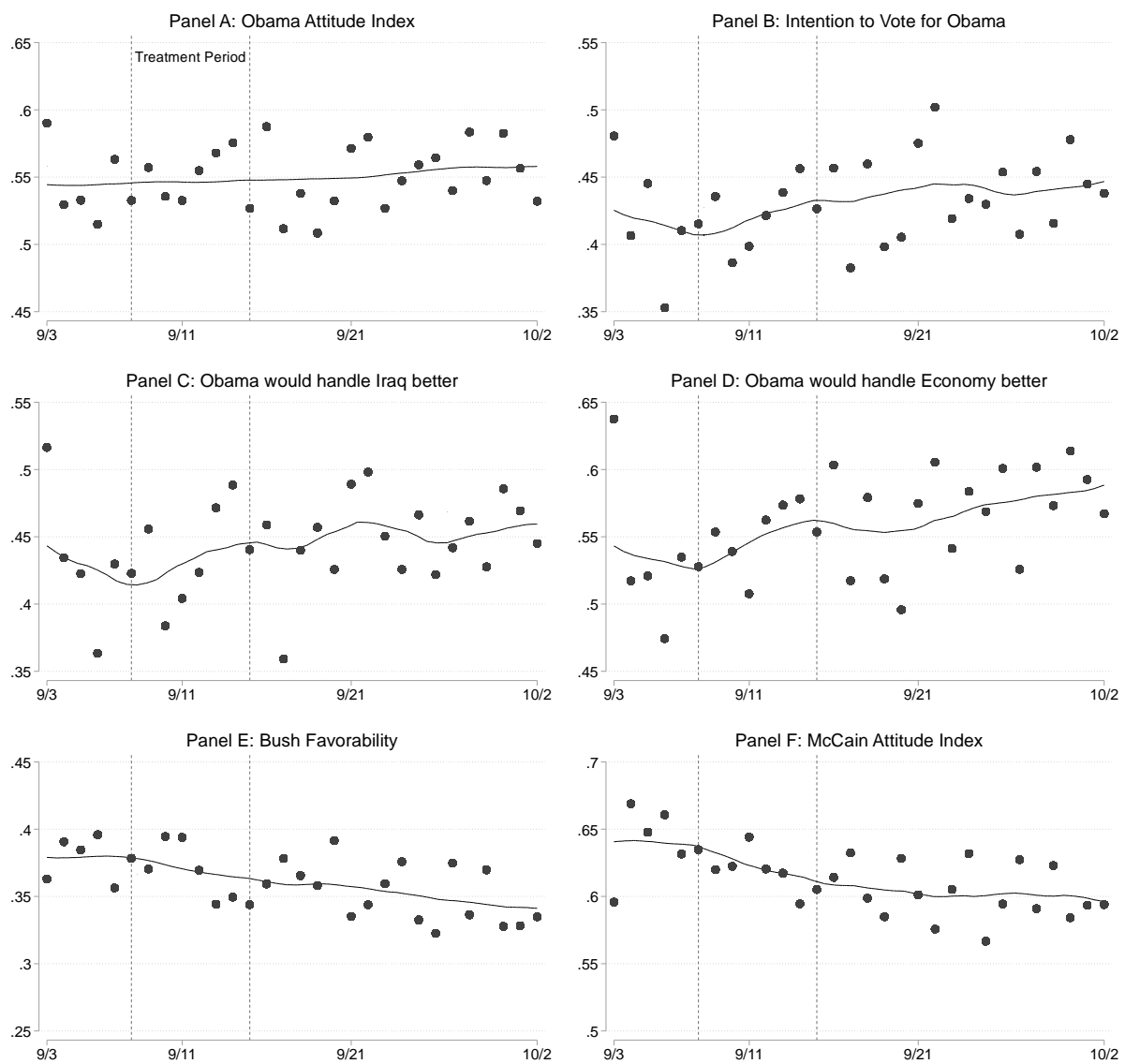
The other threat to causal inference is the presence of other political events that happen to temporally coincide with rumor circulation in September (i.e., confounding history). For

example, a negative campaign against Obama and/or the seventh anniversary of the September 11 attacks may have produced extraneous shifts in public opinion (e.g., attitude toward Obama). If that is the case, then the increase in the “Obama-is-a-Muslim” responses may have simply been a byproduct of increased anti-Obama sentiment, whereby people try to rationalize their increased discontent with Obama by questioning his religious background. This alternative explanation is inconsistent with Obama’s strong performance in the polls at that time (e.g., see Toner and Nagourney 2008), and with our own finding that Groups A and B did not differ in their attitudes toward Obama in W9. Also recall that, in Table D2, we have shown that the two groups did not differ in W9 in the placebo false belief that Obama supports a policy that increases taxes on low- to mid-income earners. Taken together, the cheerleading hypothesis is roundly rejected by the ANES data.

To rule out this possibility even further based on “placebo” trends in public opinion, we draw on the 2008 National Annenberg Election Survey (NAES)—a rolling cross-section representative survey that interviewed a few hundred individuals each day.⁵ In Figure D2, we plot trends in (1) attitudes toward Obama, (2) the intention to vote for Obama, (3) the belief that Obama will better handle the Iraq War, (4) the belief that he will handle the economy better, as well as people’s attitudes toward (5) Bush and (6) McCain.

⁵ While the NAES did not measure belief in the Muslim rumor, it is more useful for ruling out the history concern because respondents did not self-select their interview date, unlike ANES. Thus the number of observations is reasonably large in both early and late September, which allows for much more precise observation of public opinion trends. For comparability with our primary data, we use NAES interviews that took place between September 3 and October 2.

Figure D2: Placebo Variables in September, 2008



Note. Source: 2008 NAES

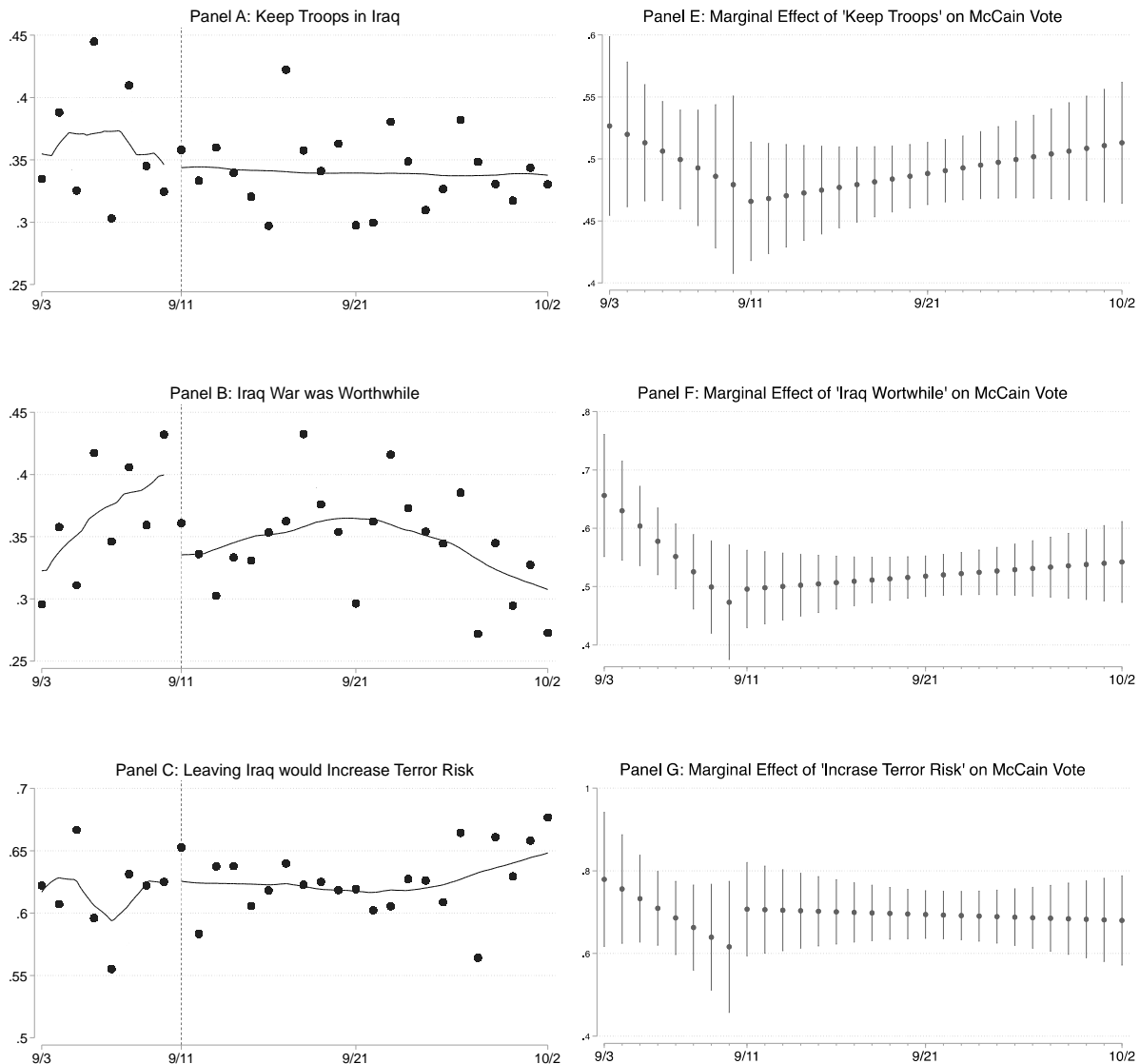
As the figure demonstrates, we find no evidence that people’s opinions on Obama grew negative in September 2008.⁶ To the extent that that people changed their mind, they became slightly more favorable toward Obama, and less favorable to his opponents—probably driven by the collapsing economy (see Panel D)—, casting doubt on the possibility that our result reflect increased public discontent with the Democratic candidate. In a similar vein, in the balance checks on various political attitudes and opinions reported in Appendix Table B2, the only significant result was that we find that Group A had evaluated the status of the economy less charitably than Group B in September (but not in in November), which also can be attributed to the fact that the U.S. economy collapsed during the month of September 2008. It is important to note that the effect of the financial crisis does not provide a plausible alternative explanation of our findings because (1) the causal link from the economic downfall to support for the Muslim rumor is unclear and (2) if anything, it would make people feel more favorable towards Obama (from non-incumbent party), thereby lowering, not increasing, Group A’s acceptance of the Muslim rumor in September.⁷

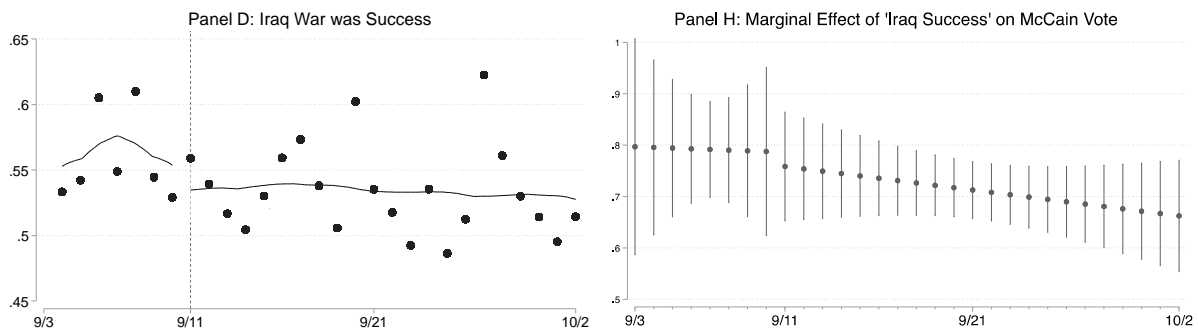
⁶ A significant change in attitudes or opinions—which we do *not* find—could have been caused by rumor belief itself. On the other hand, for other attitude to be the cause of the change in rumor belief and not the other way around, we should find its increase was greater than rumor belief (e.g., 2 times more pronounced, if its causal effect was 0.5), because not everyone unhappy with Obama would rationalize or “cheerlead” by saying he is a Muslim.

⁷ The effects of economic news in September 2008 can be examined using the analytic framework outlined in our paper. First, since the US economy collapsed throughout the month of September 2008, we know that people in “Group A” were more likely to have heard bad economic news by the time they responded to the W9 survey. Second, since both groups had gone through the “treatment period” (i.e., the financial crisis) by W11, one could build a DD strategy similar to ours. The DD estimate of the effects of economic news in September (W9 difference in economic perception minus W11 difference) is a 3.3-percentage-point decrease ($p < 0.01$).

With regard to the media coverage of the September 11 attacks, even if it could have primed the threat of Islamic terrorism, it is difficult to see how that would lead people to change their belief about Obama's religion *without* changing overall favorability toward him. Nonetheless, we assess the plausibility of this concern, drawing on a set of Iraq War-related variables from the NAES data.

Figure D3: Trends in Iraq War Related Placebo Variables around September 11, 2008





Note. Source: 2008 NAES

In Figure D3, we display daily averages (dots), and kernel-weighted moving averages, which were fit respectively both before and after the cutoff point (left-hand side panels) as well as the marginal effect of a set of Iraq War-related opinions on the voting intention for McCain to examine the potential priming effects of the media coverage of the September 11 attacks (right-hand side panels). As the figure demonstrates, it is doubtful that the coverage led people's opinions on the Iraq War become more hawkish or prime the salience of the issue.

Appendix E: Homogeneous Treatment Effect Assumption

The parallel trends assumption (Assumption 1) allows us to purge the selection bias term out of the equation. However, it does not identify one of the average treatment effects of interest (e.g., τ_9). Instead, it identifies a combination of the average treatment effect on Group A in W9 and the group difference in the long-term treatment effects in W11: $DD = \tau_{A9} + (\tau_{B11} - \tau_{A11})$.

For identification, we assumed that the treatment had the same effects on Groups A and B ($\tau_9 = \tau_{A9} = \tau_{B9}$ and $\tau_{11} = \tau_{A11} = \tau_{B11}$). We argued that the homogeneous effects assumption is plausible and—even if implausible—relatively inconsequential. In this appendix, we elaborate on this claim.

First, let us consider why the treatment effects may be different between Groups A and B. One can imagine that the rumor transmission had a greater impact on Group B because they were more politically attentive, and thus more likely to be exposed to the rumor discussions online. However, since attentive people are known to be more resistant to communication effects (e.g., see Zaller 1992, 47), one can also imagine that there is a lower probability that exposure will lead to belief change in Group B. Taken together, the theoretical implications of the known characteristics of Groups A versus B could cut both ways.

But more importantly, from an empirical standpoint, we find little evidence of heterogeneous communication effects in terms of either learning or attitude changes throughout the bevy of placebo tests reported in Appendix D. With regard to learning, recall that both groups appear to have received roughly the same amount of new information between W9 and W11 (see Table D1 above); their political knowledge scores increased by 3 to 4 percentage points. It is also

worth noting that the group’s learning appears to be well balanced in the additive political knowledge index score as well as for each of the 27 individual items. To us, this indicates that the two groups learned the same pieces of information, and did so more or less in the same proportions. Whatever Group B learned (or did not learn), Group A did the same (up to chance). Therefore, it seems plausible that roughly the same proportions of people in Groups A and B would have been exposed to online discussions of the rumor during the same time window. Recall also that, throughout the multiple waves of the 2008–2009 ANES, flows of political information appear to have had similar persuasive effects on Groups A and B, leading them to change other political attitudes in parallel (see Table B2 and Figure D1).

In short, these considerations give us *some* confidence that the assumption is plausible. That is not to say, however, that one can be certain the rumor circulation had the same effects on both groups. Thus, what if this assumption does not hold? We argue that the ramifications of such violations are relatively inconsequential, at least compared to those of violating the parallel trend assumption. They are inconsequential because one can reject the null hypothesis without making Assumption 2. In fact, violating Assumption 2—the presence of heterogeneous treatment effects—automatically rules out the null hypothesis of no causal impacts. In this scenario, the average treatment effects on groups are *homogeneously* zero.

To illustrate, consider the null hypothesis that the average treatment effects on Groups A and B are both zero in W9: $\tau_{A9} = \tau_{B9} = 0$. Then, the “lasting” effects in W11 should also be zero: $\tau_{A11} = \tau_{B11} = 0$. In this scenario, the DD is also expected to be zero: $DD = \tau_{A9} + (\tau_{B11} - \tau_{A11}) = 0 + (0 - 0) = 0$. Accordingly, if the DD estimate is significantly greater than zero, the null hypothesis is not supported by the data, irrespective of Assumption 2. In the case of the “Obama-is-a-Muslim” rumor circulation, the DD estimate was 6 percentage points,

statistically significant at $p < 0.05$. This suggests that either τ_{A9} or τ_{B11} (and τ_{B9} by extension) should be non-zero. In thinking about how to interpret this estimate—beyond the fact that the null hypothesis is rejected—we consider a few possibilities.

First, consider the scenario where the immediate treatment effects for each group were different ($\tau_{A9} \neq \tau_{B9}$), but they converged to the same values two months after the treatment ($\tau_{B11} = \tau_{A11}$). One may find this possibility fairly plausible given that communication effects tend to decay quickly (Gerber et al. 2011; Hill et al. 2013); one can imagine that the treatment effects were tending towards zero by November (but see Appendix K). Under this scenario, the second term ($\tau_{B11} - \tau_{A11}$) in the DD equation cancels out. The equation can be rewritten as: $DD = \tau_{A9} + (\tau_{B11} - \tau_{A11}) = \tau_{A9}$, which identifies the average treatment effect on Group A, instead of the average treatment effect for everyone. Note that, under this scenario, our DD design becomes equivalent to the typical DD design (where the baseline is measured at T1 and a treatment takes place at T2), which also identifies the average treatment effect on the treated.

Second, consider the scenario where the amount of decay in the average treatment effects was the same across Group A and Group B ($\tau_{A9} - \tau_{A11} = \tau_{B9} - \tau_{B11}$), even if $\tau_{A9} \neq \tau_{B9}$ and $\tau_{A11} \neq \tau_{B11}$. Then the DD equation can be rewritten as: $DD = \tau_{A9} + (\tau_{B11} - \tau_{A11}) = \tau_{B11} + (\tau_{A9} - \tau_{A11}) = \tau_{B11} + (\tau_{B9} - \tau_{B11}) = \tau_{B9}$, which identifies the average treatment effect on Group B.

Third, even when neither the treatment effects—both immediate and long-run—nor decays of treatment effects are the same across the groups, the DD calculator identifies the *minimum* of the sum of the treatment effects on Group A and B in Wave 9, provided that

treatment effects are non-negative for both groups ($\tau_{B9} \geq 0$; $\tau_{A9} \geq 0$).⁸ To see why, first let λ and κ respectively be the treatment effect decay rates for Group A and Group B. That is, $\tau_{A11} = \lambda \cdot \tau_{A9}$ and $\tau_{B11} = \kappa \cdot \tau_{B9}$, where $0 \leq \kappa \leq 1$ and $0 \leq \lambda \leq 1$. We rewrite the DD equation as:

$$DD = \tau_{B11} + (\tau_{A9} - \tau_{A11}) = \kappa \cdot \tau_{B9} + (\tau_{A9} - \lambda \cdot \tau_{A9}) = \kappa \cdot \tau_{B9} + (1 - \lambda) \cdot \tau_{A9}.$$

Given that the maximum of both κ and $1 - \lambda$ is 1, and that $\tau_{B9} \geq 0$; $\tau_{A9} \geq 0$, the sum of the average treatment effects cannot be smaller the DD:

$$\tau_{B9} + \tau_{A9} \geq \kappa \cdot \tau_{B9} + (1 - \lambda) \cdot \tau_{A9} = DD \quad (\text{if } \tau_{B9} \geq 0; \tau_{A9} \geq 0).^9$$

If $\kappa = 1$ and $\lambda = 0$, the DD would be the sum of the average treatment effects on each group in Wave 9: $DD = \tau_{B9} + \tau_{A9}$. Since our DD estimate was 6 percentage points, either the estimate of τ_{A9} or τ_{B9} has to be 3 points or greater. Crucially, this condition ($\kappa = 1$ and $\lambda = 0$) is very implausible because it indicates the average treatment effect on Group B remained intact for two months whereas the average treatment effect on Group A disappeared completely by November. In other cases, we get greater estimates of treatment effects. For example, if the treatment effects decayed (say) by half between the waves for both groups ($\kappa = 0.5$ and $\lambda = 0.5$), the estimated sum of the group average treatment effects in Wave 9 would be 12 percentage points, $\widehat{\tau_{B9} + \tau_{A9}} = 0.012$.

⁸ That is, the circulation of the “my Muslim faith” message did not “backfire” by making misinformed people correct their false beliefs. Note that a “negative” value in this context indicates decrease in rumor belief (i.e., increase in correct belief).

⁹ To illustrate, first, we define $k' = 1 - k$ and $\lambda' = 1 - \lambda$, and $DD' = \kappa' \cdot \tau_{B9} + (1 - \lambda') \cdot \tau_{A9}$, such that the sum of DD and DD' is equal to $\tau_{B9} + \tau_{A9}$:

$$DD + DD' = \kappa \cdot \tau_{B9} + (1 - \lambda) \cdot \tau_{A9} + \kappa' \cdot \tau_{B9} + (1 - \lambda') \cdot \tau_{A9} = \tau_{B9} + \tau_{A9}.$$

$$DD' \geq 0 \text{ because } 0 \leq k' \leq 1; 0 \leq 1 - \lambda' \leq 1; \tau_{B9} \geq 0; \tau_{A9} \geq 0.$$

$$\text{Therefore, } \tau_{B9} + \tau_{A9} = DD + DD' \geq DD.$$

Finally, if either τ_{B9} or τ_{A9} was negative, the DD may not identify the minimum of $\tau_{B9} + \tau_{A9}$.¹⁰ But consider if $\tau_{A9} < 0$, we get: $\kappa \cdot \tau_{B9} = DD - (1 - \lambda) \cdot \tau_{A9} \geq DD$, because $(1 - \lambda) \cdot \tau_{A9} \leq 0$. Given the maximum of κ is one, τ_{B9} should be equal to or greater than DD . By the same token, if $\tau_{B9} < 0$, we have to conclude that $\tau_{A9} \geq DD$. In short, our DD estimate (6 percentage points; $p < 0.05$) would suggest that, if treatment effects *were* heterogeneous such that rumor messages *decreased* the false belief that Obama was a Muslim for one group (if either $\tau_{B9} < 0$ or $\tau_{A9} < 0$), the average effect in Wave 9 should be higher than 6 points for the other group.¹¹

¹⁰ This is because DD' in Footnote 8 may be negative.

¹¹ We find the final scenario very unlikely. It is hard to imagine that “my Muslim faith” messages would reduce the rumor belief on average. It is even harder to imagine that they did so for one group, while substantially increasing the false belief for another, especially considering that the groups were nearly identical in terms of partisan leaning.

Appendix F: Regression Model Specifications

This appendix describes the specifications of the regression models reported Table 1 in the main text in greater details.

Table 1 provided the regression estimates of Equation 1. The column (1) shows the baseline model: weighted OLS without covariates.¹² In column (2), we controlled for (time-variant effects of) pre-treat covariates and we add (time-variant) covariates measured in Waves 9 and 11 in Columns (3) and (4). Unweighted results were reported in Column (5). Table F1 supplements Table 1 in the main manuscript, by presenting the control variables that were included in each specification (but omitted from the table) and the coefficients on these variables. Most continuously coded covariates were included without recoding (which assumes a linear relationship between these variables and rumor belief). But for the indicators of reception of “communication flows,” we allowed for non-linear relationships, in keeping with the RAS model (Zaller 1992, 1996). To do so, we quartile split the following variables and controlled for their

¹² We use linear probability models (estimated with OLS models), instead of non-linear models such as logistic or probit regressions. While the disadvantage of linear probability models is that they may generate predicted probabilities outside the 0-1 range, that cannot be the case for our main specification model (Column 1 of Table F1), which includes just one dummy variable. The main reason behind our choice, however, is that non-linear models with individual-specific fixed effects require dropping all respondents, whose rumor belief (the dependent variable) did not change between Waves 9 and 11—88 percent of all available observations. This is a *potentially* serious issue. For example, consider a scenario where 97% of group A did not change, and 1% changed in the direction of correcting one’s belief 2% changed in the opposite direction, and 98 % of Group B did not change, and 1 % changed in one direction and 1 % change the other direction. In terms of percentage changes, this groups appear virtually the same—i.e., 1 percent point change (A) versus 0 percentage point (B). But in terms of the (log) odds, group A may appear substantially different from Group B, which would be a misleading conclusion. Recent studies with fixed effects models have used linear probability models even when the dependent variable is binary, as is the case in our study (see Treisman 2015 for a recent example). That said, as we show in Appendix H, results remain quite consistent when replicated with logistic regression models.

fixed effects: political knowledge, Internet news use, mass media news use, political discussion. For example, Internet use frequency was divided into the following categories—low (not at all), low-medium (1 to 3 days), medium-high (4 to 6 days), and high (7 days). In a similar vein, we included the fixed effects of the numbers of Democratic and Republican political discussants, which respectively vary between 0 and 3. For control variables that had more than 10 missing observations, we coded missing as 0, and created dummy variables indicating missing values, and included both the control variable and the dummy.

Table F1: Full Description of Table 2 in the Main Text (Effects of Online Rumor Circulation)

	(1)	(2)	(3)	(4)	(5)
Difference-in-Differences	0.062*	0.077*	0.076*	0.063*	0.038*
	(0.031)	(0.030)	(0.030)	(0.028)	(0.017)
Age		-0.040	-0.044	-0.027	
		(0.062)	(0.069)	(0.064)	
Female		-0.016	-0.016	-0.012	
		(0.026)	(0.026)	(0.026)	
White		-0.003	-0.005	-0.021	
		(0.042)	(0.043)	(0.043)	
Christian		0.010	0.010	-0.000	
		(0.029)	(0.029)	(0.029)	
Christian Missing Dummy		0.029	0.034	0.041	
		(0.052)	(0.052)	(0.051)	
Education (\geq Bachelor's degree)		-0.001	0.002	-0.008	
		(0.023)	(0.023)	(0.022)	
Income (\geq 60k)		-0.033	-0.036	-0.038	
		(0.026)	(0.026)	(0.025)	
Income (\geq 60k) Missing Dummy		0.048	0.048	0.231	
		(0.119)	(0.124)	(0.149)	
Vote for Kerry in 2004		-0.002	-0.004	0.085	
		(0.028)	(0.028)	(0.044)	
Vote for Kerry Missing Dummy		-0.093	-0.083	-0.051	
		(0.052)	(0.055)	(0.061)	
2004 Turnout		-0.031	-0.028	-0.057	
		(0.049)	(0.050)	(0.053)	
2004 Turnout Missing Dummy		0.178*	0.170	0.149	
		(0.089)	(0.090)	(0.090)	
Primary Turnout		0.001	0.006	0.001	
		(0.060)	(0.057)	(0.060)	
Primary Turnout Missing Dummy		0.109	0.095	0.091	
		(0.096)	(0.094)	(0.097)	
Vote for Obama in Primary		0.020	0.023	0.045	
		(0.038)	(0.039)	(0.041)	
Vote for Obama in Primary Dummy		0.089	0.100	0.107	
		(0.062)	(0.061)	(0.062)	
Political Interest (Wave 9)			-0.019	-0.025	
			(0.081)	(0.079)	
Political Knowledge Mid-Low (Wave 9)			-0.004	-0.018	
			(0.041)	(0.041)	
Political Knowledge Mid-High (Wave 9)			0.033	0.031	
			(0.041)	(0.041)	
Political Knowledge High (Wave 9)			0.029	0.031	
			(0.041)	(0.042)	
Internet News Mid-Low (Wave 9)			0.019	0.020	
			(0.038)	(0.035)	
Internet News Mid-High (Wave 9)			0.010	0.014	
			(0.039)	(0.037)	
Internet News High (Wave 9)			-0.005	-0.005	
			(0.035)	(0.033)	
Mass Media News Mid-Low (Wave 9)			0.039	0.024	
			(0.038)	(0.036)	
Mass Media News Mid-High (Wave 9)			0.001	-0.005	
			(0.037)	(0.034)	
Mass Media News High (Wave 9)			0.022	0.012	
			(0.039)	(0.037)	
N	2066	2066	2063	2061	2191
Pre-treatment Covariates	No	Yes	Yes	Yes	No
Waves 9 & 11 Covariates (Attentiveness)	No	No	Yes	Yes	No
Waves 9 & 11 Covariates (Preferences)	No	No	No	Yes	No
Weights	Yes	Yes	Yes	Yes	No

Table F1 (Continued)

	(1)	(2)	(3)	(4)	(5)
Political Discussion Mid-Low (Wave 9)			-0.006 (0.037)	-0.004 (0.036)	
Political Discussion Mid-High (Wave 9)			0.029 (0.046)	0.029 (0.043)	
Political Discussion High (Wave 9)			0.020 (0.047)	0.017 (0.045)	
Political Knowledge Mid-Low (Wave 11)			0.067 (0.049)	0.060 (0.046)	
Political Knowledge Mid-High (Wave 11)			0.021 (0.053)	0.008 (0.047)	
Political Knowledge High (Wave 11)			-0.003 (0.050)	-0.024 (0.046)	
Political Interest (Wave 11)			-0.022 (0.082)	-0.040 (0.081)	
Political Discussion Mid-Low (Wave 11)			0.033 (0.036)	0.024 (0.036)	
Political Discussion Mid-High (Wave 11)			0.039 (0.046)	0.031 (0.044)	
Political Discussion High (Wave 11)			0.000 (0.052)	-0.002 (0.052)	
Vote Intention for Obama (Wave 9)				0.037 (0.056)	
Vote Intention for Obama Missing Dummy (Wave 9)				-0.272 (0.196)	
Party ID (Wave 9)				0.043 (0.089)	
Attitude toward Obama (Wave 9)				0.030 (0.077)	
Attitude toward Obama Missing Dummy (Wave 9)				0.057 (0.072)	
Affects toward Obama (Wave 9)				0.228* (0.098)	
Racial Prejudice (Wave 9)				-0.253 (0.140)	
1 Democratic Discussant (Wave 9)				0.005 (0.035)	
2 Democratic Discussants (Wave 9)				0.025 (0.034)	
3 Democratic Discussants (Wave 9)				-0.037 (0.049)	
1 Republican Discussant (Wave 9)				0.016 (0.031)	
2 Republican Discussants (Wave 9)				-0.007 (0.040)	
3 Republican Discussants (Wave 9)				0.017 (0.045)	
Party ID (Wave 11)				0.023 (0.088)	
Racial Prejudice (Wave 11)				0.206 (0.133)	
Affects toward Obama (Wave 11)				-0.321** (0.095)	
Vote for Obama in Election				-0.073 (0.053)	
Vote for Obama in Election Missing Dummy				-0.118* (0.057)	
Intercept	-0.048 (0.028)	-0.034 (0.092)	-0.099 (0.117)	0.020 (0.141)	-0.032* (0.015)
N	2066	2066	2063	2061	2191
Pre-treatment Covariates	No	Yes	Yes	Yes	No
Waves 9 & 11 Covariates (Attentiveness)	No	No	Yes	Yes	No
Waves 9 & 11 Covariates (Preferences)	No	No	No	Yes	No
Weights	Yes	Yes	Yes	Yes	No

Note: OLS estimates with standard errors in parentheses. * $p < 0.05$.

Appendix G: Operational Definition of Groups A and B

For brevity, we have relied on the operationalization of group assignment that defines those who took Wave 9 interview on September 10 or earlier as Group A, and on September 11 or later as Group B in the Main Text. This appendix explains this decision in greater details, and describes alternatives that are used in the robustness check below.

To define these groups operationally, we use a cutoff point at which the probability of exposure to one of the rumor messages is likely to pass the half of the maximum.¹³ So for example, if the average probability of exposure to a “my Muslim faith” message changed from 0 to 50 percent between the beginning and end of September, we wanted to identify the time point at which it passes 25 percent. Since message *reception* is not directly observable however, as its proxy, we simulated the cumulative percent of message reception up to each ANES interview date, using information on the timing and number of “my Muslim faith” message posting captured in the MemeTracker dataset. To do so, we need to account for the fact that the overtime increase in message *reception* should lag somewhat behind the overtime increase of the rumor message postings, because of time gap between posting and viewing. For example, that 50 percent of total messages were sent out by a certain point in time doesn’t mean that 50 percent of potential viewers had viewed one by then; the actual time point would depend on the pace with which messages are diffused to potential receivers—e.g., 60% of total views within the first 24 hours after posting, and then 80%, 90%, and so on by the following days.

¹³ Defining these groups is not as straightforward as a regression discontinuity design because the increase in the likelihood of exposure was not categorical, but continuous. Figure 1 depict show that it took several days for online rumor diffusion to reach a saturation point (around September 13), and smaller portions kept on appearing even afterwards.

To simulate this “diffusion curve” we use the cumulative distribution function (CDF) of the exponential distribution. The CDF is:

$$F(t; \lambda) = 1 - e^{-\lambda t} \text{ (Equation G1)}$$

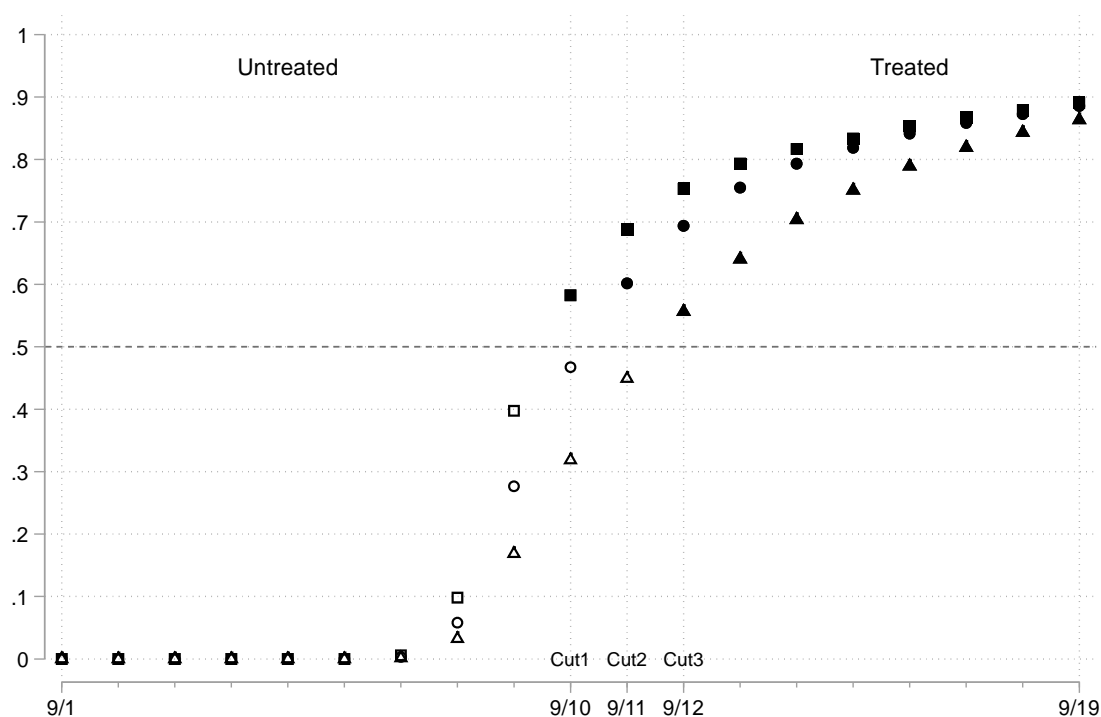
where the cumulative percent of views (i.e., reception) of an online message is a function of t , which is the number of days passed since a message is posted. This function assumes that each message reaches its largest share of viewers on the first day, and the number of views *exponentially* decrease as time passes, while the specific shape of the diffusion curve depends on the rate parameter $\lambda > 0$. For example, setting the rate parameter to be 0.8 indicates that 55 % of a message’s total diffusion is achieved in 24 hours, followed by 80% and 91% penetrations in 48, and 72 hours. Alternatively, one might employ rate parameters assuming faster ($\lambda=1.6$) and slower ($\lambda=0.8$) diffusion rates. The former presumes that 90% of a message’s potential views would be achieved in 36 hours, while the latter presumes 6 days.

As an example, consider the following scenario; for 10 days, a dose of 100 messages are sent out via the Internet on a daily basis, and each message is read by 100 individuals eventually. So there will be 10 doses*100 messages*100 viewers=100,000 people who will receive one of the messages. Assuming that messages are sent out anytime throughout the day, by the middle of Day 1, 50 of the first does of messages will be posted online (in expectation), and the time passed since appearance of each message will vary between 0 and 12 hours, averaging on 6 hours. With the rate parameter set to 0.8, this would indicate that about $50*100*0.18 = 900$ people (0.9%) will be exposed at that point.¹⁴ And by (the middle of) Day 2, about $50*100*0.55$

¹⁴ Entering $t = 0.25$ (i.e., 6 hours) and $\lambda=0.8$ into Equation E1 gives 0.18.

= 5,500 people (5.5%) will be exposed to the first dose,¹⁵ and another $50 \cdot 100 \cdot 0.18 = 900$ (0.9%) will be exposed to the second dose, so 6,400 (6.4%) in total. Likewise, the amount of total exposure will be $100 \cdot 100 \cdot 0.80 + 100 \cdot 100 \cdot 0.55 + 50 \cdot 100 \cdot 0.18 = 14,400$ by (the middle of) day 3.

Figure G1: Cumulative Percent of Expected Message Reception



Note: Dotted lines depict cumulative percent of message reception generated based on our simulations.

We apply this framework to the MemeTracker data (see Figure 1 in the Main Text) to generate the expected cumulative percent of message reception on each day. The three dotted lines in Figure G1 depict the cumulative percent of message reception; each assuming different speeds at which the “my Muslim faith” messages would reach their receivers. For example, had

¹⁵ The time passed since message postings would be 24 hours on average, ranging between 12 hours and 36 hours. Plugging $t = 1$ (i.e., 24 hours) and $\lambda=0.8$ to Equation E1 gives 0.55.

it taken 3 days for an online rumor message to be exposed to 90 percent of its potential viewers/readers ($\lambda=0.8$), our simulation indicates, the relative “dosage” of treatment reception would cumulate from about 0 to 28 percent between September 7 and 9, and passed the 50 percent cutoff on September 11 (circles in Figure 2). We used this specification in the main text. Assuming faster or slower diffusion rates (i.e., 90% exposures in 1.5 or 4 days), the 50 percent threshold for qualifying as Group A would be September 10 or 12 (squares and triangles in Figure 2).

We check the robustness of the findings, using alternative operationalizations of group assignment in the following Appendix H. First, we replicate the analyses using other plausible cutoffs (September 10 and 12). Second, we drop “partially treated” participants who took the survey during the treatment period (between 9/8 and 9/12), and compare “fully treated” participants interviewed after a “saturation” point (September 13 or later) to “untreated” respondents, surveyed on September 7 or earlier. Third, we use the three dotted (message reception) lines plotted in Figure G1 as the independent variable, instead of cutoffs, which enables us to exploit gradual increases in treatment intensity; in this case participants are divided into 31 “groups” assigned with varying probabilities of message reception.¹⁶

¹⁶ For example, based on the circle markers in Figure G1, respondents interviewed on September 7, 8, and 9 are coded respectively as 0.001, 0.116, and 0.308, and so on. The same coding scheme applies to squares and triangles (see Croke et al. 2015 for a similar approach).

Appendix H: Robustness Check

We check the robustness of the findings presented in the main text, which relied on a certain set of specification decisions. As the following tables shows, the results are robust when alternative choices are used.

Table H1: Robustness Check for Table 2 (Effect of Online Rumor Circulation)

Treatment Operationalization	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Cutoff 9/10							
Treatment	0.050+	0.062*	0.059*	0.049+	0.028+	0.055+	0.431+
	(0.029)	(0.028)	(0.028)	(0.026)	(0.017)	(0.029)	(0.261)
Observations	2066	2066	2063	2061	2191	2066	269
Panel B: Cutoff 9/11							
Treatment	0.062*	0.077*	0.076*	0.063*	0.038*	0.067*	0.608*
	(0.031)	(0.030)	(0.030)	(0.028)	(0.017)	(0.031)	(0.276)
Observations	2066	2066	2063	2061	2191	2066	269
Panel C: Cutoff 9/12							
Treatment	0.081*	0.098*	0.099*	0.082*	0.049*	0.088*	0.801*
	(0.032)	(0.032)	(0.031)	(0.029)	(0.017)	(0.032)	(0.293)
Observations	2066	2066	2063	2061	2191	2066	269
Panel D: Cutoff 9/13 (Dropping 8 to 12)							
Treatment	0.061+	0.076*	0.076*	0.062*	0.043*	0.070*	0.714*
	(0.035)	(0.034)	(0.033)	(0.031)	(0.019)	(0.035)	(0.318)
Observations	1642	1642	1639	1638	1746	1642	204
Panel E: % Received (Fast)							
Treatment	0.065+	0.084*	0.082*	0.067*	0.040*	0.073*	0.638*
	(0.036)	(0.035)	(0.035)	(0.033)	(0.020)	(0.037)	(0.323)
Observations	2066	2066	2063	2061	2191	2066	269
Panel F: % Received (Moderate)							
Treatment	0.072+	0.091*	0.089*	0.074*	0.044*	0.079*	0.712*
	(0.038)	(0.037)	(0.036)	(0.034)	(0.021)	(0.038)	(0.334)
Observations	2066	2066	2063	2061	2191	2066	269
Panel G: % Received (Slow)							
Treatment	0.081*	0.101*	0.099*	0.083*	0.050*	0.089*	0.817*
	(0.041)	(0.040)	(0.039)	(0.037)	(0.022)	(0.042)	(0.358)
Observations	2066	2066	2063	2061	2191	2066	269
Pre-treat Covariates	No	Yes	Yes	Yes	No	No	No
Waves 9 & 11 Covariates (Attentiveness)	No	No	Yes	Yes	No	No	No
Waves 9 & 11 Covariates (Attitudes)	No	No	No	Yes	No	No	No
Weights	Yes	Yes	Yes	Yes	No	Yes	No
State Fixed Effects	No	No	No	No	No	Yes	No
OLS or Logistic	O	O	O	O	O	O	L

Note. + $p < 0.1$ * $p < 0.05$. OLS coefficients in Columns 1 to 6. Logistic coefficient in Column 7.

Specifically, Table H1 replicates the results of the (average) effects of rumor circulation presented in Table 1 in Main Text. Panels A to C report the estimates from models using September 10, 11, and 12 respectively as the cutoff point. Panel D reports the estimates from a

model that omit the respondents interviewed during the treatment period. Panels E to G report the estimates from models using gradual increases in treatment intensity depicted in Figure 2 as the independent variable, each assuming different speed of online message diffusion. In each panel, Columns 1 to 5 use specifications akin to Table 2. In addition, Column 6 reports the result that control for state fixed effects, ensuring that the results are driven by state-level heterogeneity. Also, Column 7 replicates the simplest OLS specification (i.e., Column 5) using logistic regression models.¹⁷ The results are largely consistent.

¹⁷ As noted earlier (footnote 9), those who did not change rumor belief between Waves 9 and 11 are eliminated from logistic models. The coefficients in Column 7 therefore indicates the difference in the log odds of changing in the direction of accepting rumor, relative to changing in the opposite direction—i.e., correcting.

Appendix I: Heterogeneous Effects of Rumor Circulation by Political Predispositions

The effect of rumor circulation can be heterogeneous across individual factors that determine exposure and acceptance. In this appendix we examine the moderating effects of individual predispositions to dislike Obama.

People tend to seek out and accept information that confirms their pre-existing viewpoints (Taber and Lodge 2006; Zaller 1992) – a tendency that is exercised more easily on the internet thanks to the enlarged set of choices and the low cost of gathering information (Sunstein 2002). This implies that partisan predisposition and existing attitudes toward ingroups/outgroups will moderate the online media treatment effects as well.

To test how political predispositions condition the effects of exposure to the rumor, we draw on several predictors of support for Obama (e.g., Tesler 2013).¹⁸ For example, we allow the effect of online rumor diffusion to differ by party identification using an extended version of Equation 3 of the form:

$$\Delta Y_{iw} = \beta_0 + \beta_1 G_i + \beta_2 PID_i + \beta_3 G_i \times PID_i + \varepsilon_i, \quad (\text{Equation I1})$$

¹⁸ We expect that the impacts of online rumor circulation were larger for those predisposed to dislike Obama, because they would have higher probabilities of exposure to rumor messages on the Internet (selective exposure), and given exposure, higher probabilities of acceptance (motivated reasoning). However, our data do not allow us to examine which mechanism is more prominent between selective exposure and motivated reasoning.

where G_i is a dummy variable for group (0 = Group A; 1 = Group B), PID_i is a dummy variable for party identity (0 = strong Democrat; 1 = strong Republican), β_1 is treatment effect for Democrats, β_3 is the partisan difference in treatment effect, and thus $\beta_1 + \beta_3$ indicates the treatment effect for Republicans. We fit an analog of Equation I1 for other indicators of predisposition to dislike Obama; vote choice in the 2004 presidential election; explicit and implicit biases against African Americans; and race (white versus non-white).

Table I1 shows the findings. Column 1 and 2 present the estimates of Equation I1, using Party ID (measured in Wave 9) as the moderating variable, the former from a weighted, and the latter from an unweighted OLS model. Pure independents were dropped. Column 3 and 4 present the estimates of Equation I1, using 2004 vote choice as the moderating variable, the former from a weighted, and the latter from an unweighted OLS model. Those who did not turnout or voted for a candidate other than Bush or Kerry in 2004 were excluded. Column 5 and 6 presents the estimates of Equation I1, explicit anti-black bias (measured in Wave 9) as the moderating variable, the former from a weighted, and the latter from an unweighted OLS model. Column 7 and 8 present the estimates of Equation I1, using implicit anti-black bias as the moderating variable, the former from a weighted, and the latter from an unweighted OLS model. Implicit racial bias was measured either in Wave 9 or Wave 10 by random assignment (see Appendix B). We pooled the responses to create one variable. Finally, Column 9 and 10 present the estimates of Equation I1, race as the moderating variable, the former from a weighted, and the latter from an unweighted OLS model.

Consistent with our expectations, we find that rumor circulation had much a greater impact on Republicans ($\beta_1 + \beta_3 = 13$ percentage points) than Democrats ($\beta_1 = 2$ percentage points; $p = 0.66$; see Column 1 of Table I1). The partisan difference in the treatment effect is

estimated to be $\beta_3 = 11$ percentage points, which is correctly signed and substantively large, although somewhat noisy statistically ($SE = 0.062$, $p < 0.1$). The unweighted estimates are smaller but more precise (Column2). We find similar patterns for other moderating variables. Among the 10 models reported in G1, seven models show a significant treatment heterogeneity at $p < 0.1$, and three show a significant treatment heterogeneity at $p < 0.05$.

II: Heterogeneous Effects of Online Rumor Circulation by Political Predispositions

Moderating Variable:	(1) Rep	(2) Rep	(3) Voted for Bush in 04	(4) Voted for Bush in 04	(5) Racial Bias	(6) Racial Bias	(7) Implicit Bias	(8) Implicit Bias	(9) White	(10) White
Treatment Effect for anti-Obama Side ($\beta_1 + \beta_3$)	0.131* (0.039)	0.075* (0.028)	0.127* (0.044)	0.069* (0.028)	0.095* (0.048)	0.070* (0.027)	0.100* (0.041)	0.061* (0.025)	0.100* (0.030)	0.053* (0.019)
Treatment Effect for pro-Obama Side (β_1)	0.023 (0.048)	0.003 (0.024)	0.019 (0.037)	0.021 (0.025)	0.029 (0.040)	0.008 (0.021)	0.011 (0.049)	-0.012 (0.024)	-0.089 (0.088)	-0.057 (0.043)
Heterogeneity in Treatment Effect (β_3)	0.108+ (0.062)	0.071+ (0.037)	0.109+ (0.058)	0.049 (0.037)	0.066 (0.062)	0.061+ (0.034)	0.088 (0.064)	0.072* (0.035)	0.189* (0.093)	0.110* (0.047)
Observations	1848	1953	1696	1779	2066	2188	2060	2116	2066	2191
Other Covariates	No	No	No	No	No	No	No	No	No	No
Weights	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No

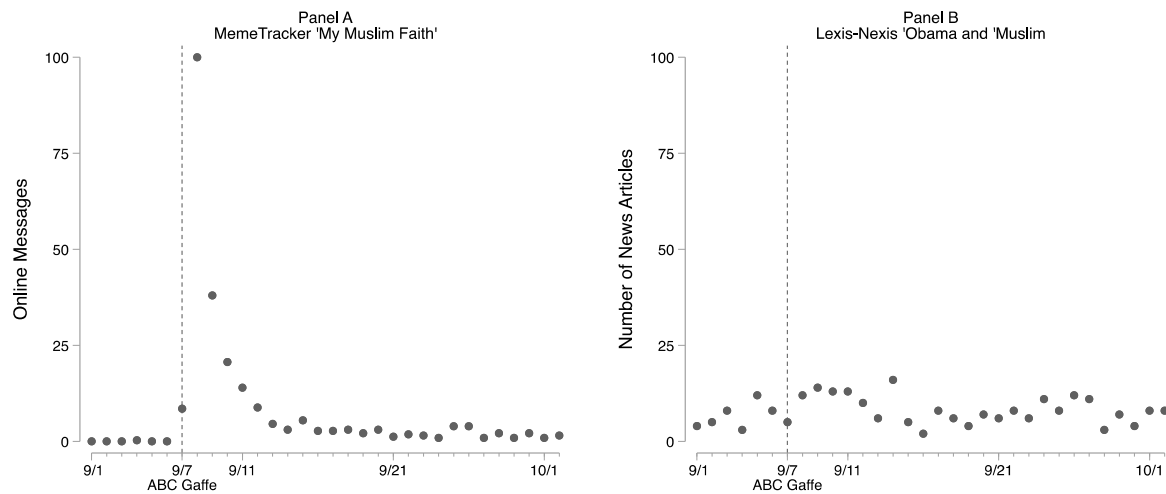
Note. + $p < 0.1$ * $p < 0.05$. OLS estimates of Equation II. Standard errors in parentheses. All moderating variables were dichotomized and coded so that 0 is the pro-Obama side and 1 is the anti-Obama side. Non-whites were coded 0 and whites were coded 1. In Columns 1 and 2, “pure” independents were excluded; Democratic- and Republican-leaning independents were included in the respective partisan groups. In Columns 3 and 4, those who did not turnout or voted for a candidate other than Bush or Kerry in 2004 were excluded.

Appendix J: Specifying the Role of *Online* Media

In the main text, we did not devote much discussion on whether the observed treatment effects occurred through online media or other channels of political communication. But there are several reasons to think the role of online media was essential. In this appendix, we elaborate on this claim.

First, mainstream media reporters typically adhere to the journalistic norms of gatekeeping to determine if certain issues are “unworthy of being heard”, which are then relegated to the “sphere of deviance” (Hallin 1989, 117). Given the “Obama-is-a-Muslim” myth would belong to a “sphere of deviance,” we expected that the traditional press’s reaction to the “my Muslim faith” gaffe should be quantitatively and qualitatively different from that of online media. To see if this was the case, we conducted a Lexis-Nexis search for newspaper articles and television news reports that contain the words “Obama” and “Muslim” in the same sentence (within 15 words) and compared its pattern against the Memetracker data which capture online discussions of the rumor. The following figure (taken from Figure 1 in the main text) shows that the volume of mainstream media coverage of the rumor is fairly stagnant, providing little evidence that the news coverage of Obama’s religion was particularly salient at a certain point in time. This is consistent with prior findings that the mainstream media’s coverage of the rumor remained quite low except for the period after Obama secured the Democratic Party nomination (Weeks and Southwell 2010; Pew Research Center 2008).

Figure J1: ‘Obama-is-a-Muslim’ rumor circulation and media coverage in September 2008



Note: the number of MemeTracker messages in Panel A is rescaled to range from 0 to 100. The actual minimum and maximum values are 0 and 329. Panel B graphs the number of newspaper articles and TV news reports that contain the words ‘Obama’ and ‘Muslim’ within 15 words (that is, within a sentence) from a Lexis-Nexis search.

Qualitative differences in how the rumor was covered are also worth emphasizing. To the extent that the mainstream media covered the rumor, much of the coverage was intended to *dispel* the rumor, according to previous studies (Weeks and Southwell 2010). This includes partisan cable television news programs,¹⁹ but not conservative radio talk shows (Johnson et al. 2008), which are the only exception, to the best of our knowledge. In addition, Obama’s ‘Muslim faith’ was not a part of John McCain’s campaign against Obama (e.g., Bumiller 2008). We checked FactCheck.org and PolitiFact.com for incorrect claims made during September 2008, and found no indication that the Republican campaign attacked Obama using the Muslim rumor. In sum, there are theoretical reasons and empirical evidence to doubt that voters’ acceptance of

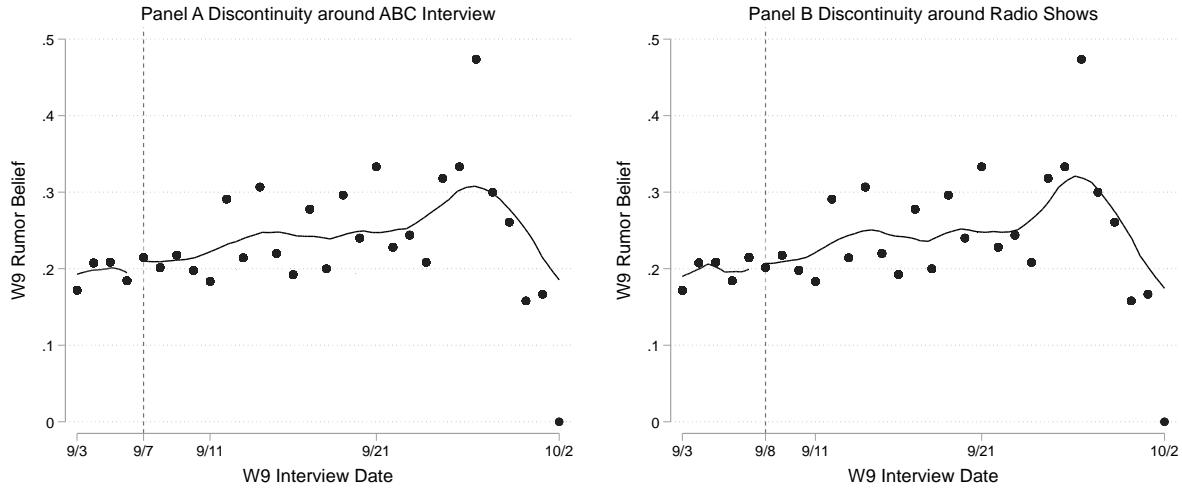
¹⁹ For example, Fox News did not question Obama’s religion (at least in September 2008); one host said ‘we were very sensitive to all the rumors about Obama’s religion and sought to set the record straight’ (Hemmer 2008). Glenn Beck – another conservative (then) CNN Headline News host – even encouraged voters to denounce the rumor (Beck 2008).

the rumor could have been directly driven by elite discourse in the “traditional” media in September 2008, except for the ABC interview itself and perhaps some radio shows seized on the ABC interview to promote the rumor the very next day (See Johnson et al. 2008).

How plausible is the assumption that the gaffe affected rumor belief *only through the online rumor circulation* that it inspired, but neither directly nor indirectly through other channels of political (mis)information especially political radio shows? We used two empirical strategy to examine this question.

1. Placebo Regression Discontinuity Estimates. First, we examine the causal impacts of the ABC interview itself and radio shows using regression discontinuities. Since broadcasting entails immediate and (usually) one-time shocks – unlike the multitude of online messages that diffuse gradually over time – the effects of exposure to the ABC interview can in fact be identified by a regression discontinuity design, with 7 September as the cut-off point. Therefore, if the gaffe itself was responsible for our results, we should find a discontinuous jump after the cut-off. By the same logic, we should find a discontinuity before and after 8 September if radio shows’ coverage was influential. Contrary to these possibilities, Figure J2 shows that the trend was continuous around both 7 and 8 September, ruling out the impact of the ABC interview and conservative radio shows as plausible alternative explanations.

Figure J2: ‘Obama-is-a-Muslim’ rumor circulation and media coverage in September 2008



Note: The X-axis is ANES Wave 9 interview date (3 September to 2 October). The Y-axis is rumor belief rates. Local polynomial lines are plotted (using Stata’s *lpoly* command default option), with 95 per cent confidence intervals in dotted lines. Scattered dots represent daily averages. Horizontal dashed lines show the baselines (average of Group B). All estimates are unweighted.

Table J1 supplements the graphical evidence with regression estimates. Specifically, to capture the effect of the ABC show we fit the following regression model:

$$Y_i = \beta_0 + \beta_1 Cut_i + \beta_2 T_i + \beta_3 Cut_i T_i + \varepsilon_i \quad (\text{Equation J1})$$

where Y_i is wave 9 rumor belief, Cut_i is the dummy variable indicating whether one’s interview date was September 7 or later, T_i is interview date—i.e., the “running” variable—, and β_1 is the causal effect of interest.

The regression discontinuity approach identifies the causal effect under the (relatively mild) assumption that the potential outcome is continuous over interview dates in the absence of the “treatment”—i.e., ABC interview. Similarly, we fit a model similar to Equation J1, but this time, using September 8 as the cutoff to assess the impact of political radio shows aired on that day. Table J1 presents the estimates (with and without weights), which suggest that the discontinuous jumps at these cutoffs were estimated to be either incorrectly signed, or effectively zero, consistent with the graphical evidence.

Table J1: No Regression Discontinuities around ABC Interview and Radio Shows

	(1)	(2)	(3)	(4)
Cutoff	9/7	9/7	9/8	9/8
Discontinuity	-0.082	0.010	-0.087	0.001
	(0.057)	(0.035)	(0.056)	(0.035)
Observations	2416	2497	2416	2497
Weights	Yes	No	Yes	No

Note. OLS estimated with standard errors in parentheses. All $p > 0.1$.

2. *Heterogeneous Treatment Effects across Communication Channels.* If the internet, rather than the broadcast media, is really the main channel through which citizens are exposed to misinformation, the treatment effect should depend on individuals’ internet use. We examined this possibility building on Zaller’s RAS model. First, those who do not read internet news at all are unlikely to be exposed to *online* rumors to begin with, and therefore should not be affected. Secondly, given exposure, the most frequent internet news users are the least likely to update their beliefs on the basis of what they see in these messages (for example, “my Muslim faith”), because of strong priors formed from past exposure to the rumor (and/or rebuttal evidence) circulating on the Web.²⁰ It is therefore those in the middle who are most likely to be influenced

²⁰ The rumor had been circulating on the internet long before September 2008.

by online rumor diffusion. To test this hypothesis, we classify internet use frequency into four categories using quartiles – low (not at all), low-medium (1 to 3 days per week), medium-high (4 to 6 days per week) and high (7 days per week) – and allow the treatment effect to vary across these categories²¹ by estimating an extended version of the main regression model (Equation 1 in the main text) that includes three dummies for the categories, and their interactions with group assignment.

Table J2: Marginal Effects of Online Rumor Circulation by Internet Use

	(1)	(2)	(3)
Marginal Effects of Treatment			
at Internet Use=None	0.000 (0.065)	-0.007 (0.061)	-0.006 (0.061)
at Internet Use =1-3 Days	0.146* (0.052)	0.163* (0.052)	0.164* (0.052)
at Internet Use =4-6 Days	0.080 (0.071)	0.079 (0.065)	0.084 (0.066)
at Internet Use =Everyday	-0.013 (0.032)	-0.043 (0.041)	-0.044 (0.039)
F-static for Interactions (<i>df</i> =3)	2.49+	3.29*	3.36*
Observations	2,066	2,065	2,065
Treatment interacted with other channels	No	Yes	Yes
Mass Media Items from	NA	W9	W9 & W10

Note: Weighted OLS with standard errors in parentheses. * $p < 0.05$; + $p < 0.1$.

The results, displayed in Column 1 of Table J2, suggest that the effects of rumor circulation indeed depend on internet use levels. Consistent with our argument derived from the RAS model, the marginal effect is estimated to be approximately zero for those who do not use the internet for news and those who use it everyday. Indeed, the average treatment effect reported above comes entirely from moderate internet users. Among those with a “low-medium” level of internet use, rumor diffusion produced a 15-percentage-point increase in rumor belief. A somewhat smaller and statistically insignificant marginal effect – 8 percentage points – is found for the ‘medium-high’ group ($p = 0.26$). This heterogeneity is unlikely to occur by chance,

²¹ See Huber and Arceneaux 2007 for a similar approach to testing the RAS model.

according to the F-statistic testing the null hypothesis that the interaction terms are jointly zero ($F(3) = 2.49, p = 0.06$).

Since those using the internet a few days a week may do so alongside other channels, this pattern may be picking up the moderating role of any channel through which the ‘Obama-is-a-Muslim’ rumor was circulating. To address this possibility, we incorporate three variables that measure interpersonal political talk (frequency of political conversation, number of Republican discussants in social network, and number of Democratic discussants in social network) and two proxies for habitual news reception (exposure to offline media news, and political knowledge) (Zaller 1996).²² We allow the treatment effect to differ by these variables – all simultaneously including the internet in Column 2. The moderating effects of the internet are nearly identical (in fact, they are slightly more pronounced) after controlling for other channels ($F(3) = 3.29, p < 0.05$). Given that the three items used to construct mass media exposure had low reliability score (Cronbach’s $\alpha = 0.40$), we created a new index by combining the mass media consumption items measured in both Waves 9 and 10. Cronbach’s α for the six-item index was 0.60 (as opposed to 0.40). The moderating effect of the internet barely changes when the six-item index was entered into the model along with other channels ($F(3) = 3.36, p < 0.05$).

In Table J3, we examine the moderating effect of each channel separately in each column. As can be seen, we find no evidence that the treatment effect is particularly more or less pronounced depending on any alternative source of political communication (all $p > 0.1$). News exposure via mass media did not interact significantly with treatment whether it drew only on the Wave 9 items (Column 5), or on Wave 9 and Wave 10 items (Column 6). As can be seen in non-significant F-statistic values for interactions, we fail to reject the null that the coefficients are

²² We created dummies for the actual numbers of discussants, and quartile split other variables.

different across the levels of communication. Importantly, these results are not readily reconcilable with the possibility that our findings reflect the effects of *offline* channels of political rumors.

Table J3: Marginal Effects of Online Rumor Circulation by Alternative Channels

	(1) Talk 1: Discussion Frequency	(2) Talk 2: Republican Social Network	(3) Talk 3: Democratic Social Network	(4) General 1: Political Knowledge	(5) General 2: Mass Media (W9 only)	(6) General 2: Mass Media (W9 & W10)
Marginal Effects of Treatment						
<i>at</i> Channel=Low	0.097 (0.085)	0.053 (0.046)	0.033 (0.047)	0.075 (0.066)	0.038 (0.072)	0.045 (0.065)
<i>at</i> Channel=Low-Medium	0.042 (0.052)	0.079 (0.062)	0.196* (0.074)	0.046 (0.063)	0.057 (0.055)	0.081 (0.054)
<i>at</i> Channel=Medium-High	0.086 (0.053)	0.081 (0.051)	0.014 (0.049)	0.071 (0.048)	0.072 (0.057)	0.032 (0.067)
<i>at</i> Channel=High	0.026 (0.035)	0.053 (0.065)	0.015 (0.065)	0.040 (0.029)	0.098 (0.061)	0.107+ (0.058)
<i>F</i> -statistic for Interactions (<i>df</i> =3)	0.41	0.09	1.60	0.15	0.15	0.30
Observations	2,065	2,066	2,066	2,066	2,066	2,066

Note: Weighted OLS with standard errors in parentheses. * $p < 0.05$.

One key limitation of the heterogeneous effects analyses is the possible measurement error in self-report of internet use. People are known to systematically over-report news exposure. And this error cannot be random; by definition, those who report no internet news consumption cannot under-report, and those who consume it daily cannot over-report. This leads to a non-monotonic misreporting in which those who are in the middle range of news consumption are most likely to overstate their consumption. It also makes intuitive sense that most misreporting occurs in the middle, because people are known to overstate their media consumption mainly due to imperfect memory (Prior 2009b). Thus, it seems reasonable to expect that the self-report measure would do a better job of distinguishing non-users from heavy-users than more frequent users from less frequent ones.

Yet since we coded internet use frequency into four categories using quartiles—not at all, low-medium (1 to 3 days per week), medium-high (4 to 6 days per week) and daily—the most likely instances of over-reporting (e.g., saying 3 days instead of 1 day) would not affect our results. As an additional robustness check, we estimated a model that collapsed those who are reporting 1–6 days of internet news consumption into one category, eliminating the possibility that over-reporting in the middle affects our result (see Table J4). Consistent with the original estimate, we found that the treatment effects were entirely driven by “moderate” (1–6 days) internet news users (coefficient = 0.12; standard error = 0.04; $p < 0.01$); the estimated treatment effects were not significant among non- or heavy users (coefficient = -0.013; standard error = 0.032; $p = 0.69$). The null hypothesis that the two interaction terms are jointly zero was rejected at $p < 0.1$ ($F(2) = 2.62$). Controlling for other the moderating effects of the other channels did not alter the finding (see Columns 2 and 3 of Table J4).

Table J4: Marginal Effects of Online Rumor Circulation by Internet Use (Robustness Check)

	(1)	(2)	(3)
Marginal Effects of Treatment			
at Internet Use=None	0.000 (0.065)	-0.008 (0.061)	-0.007 (0.061)
at Internet Use=1-6 Days	0.117* (0.043)	0.126* (0.041)	0.128* (0.042)
at Internet Use=Everyday	-0.013 (0.032)	-0.042 (0.041)	-0.043 (0.040)
<i>F</i> -static for Interactions ($df=2$)	3.10*	3.84*	3.93*
Observations	2,066	2,065	2,065
Treatment interacted with other channels	No	Yes	Yes
Mass Media Items from	NA	W9	W9 & W10

Note: Weighted OLS with standard errors in parentheses. * $p < 0.05$; + $p < 0.1$ (two-tailed)

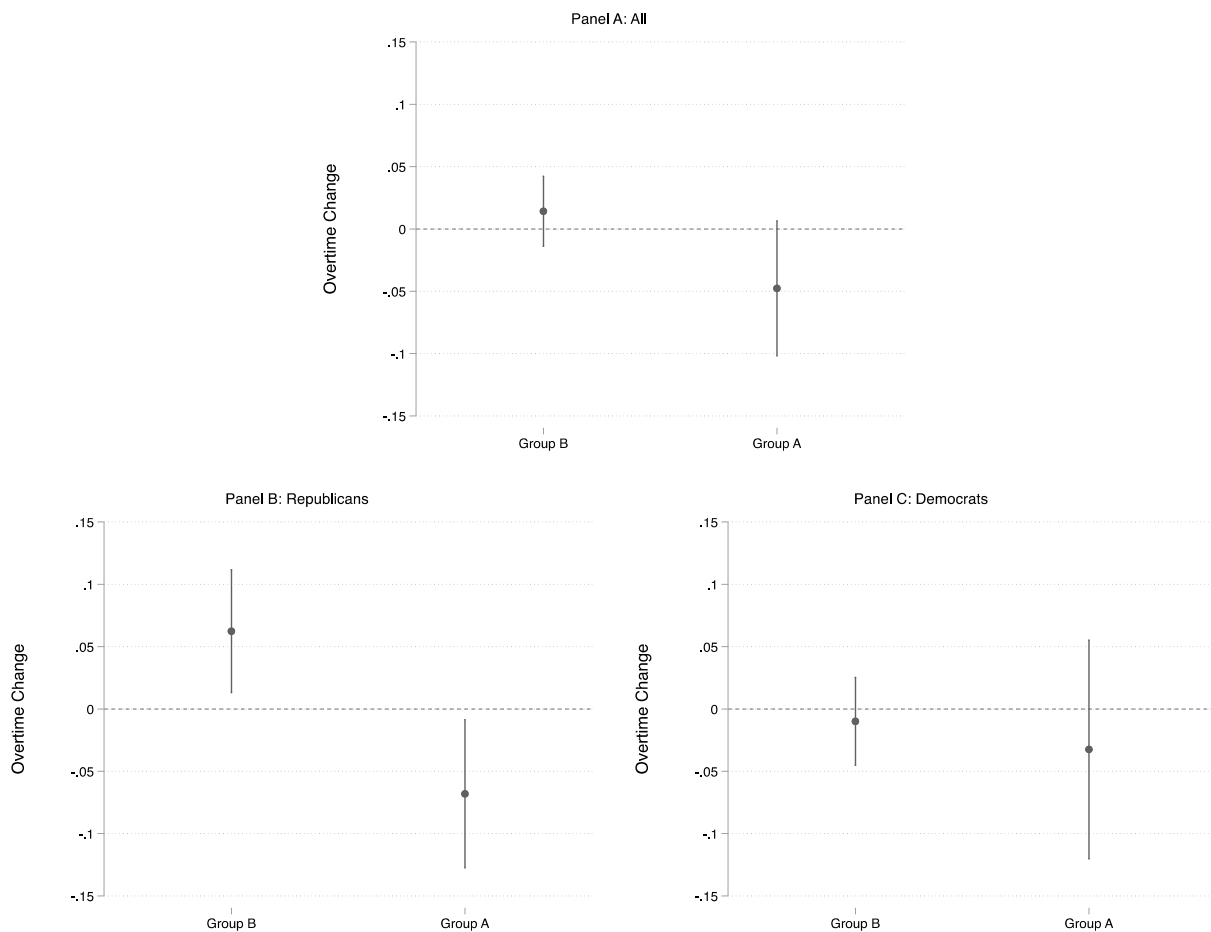
Appendix K: Exploring the Long-Run Effect and Electoral Consequence of the Event

In this appendix, we draw on a range of *suggestive* evidence to explore the far-reaching political consequences of the rumor circulation, beyond the immediate effect on rumor acceptance reported in Table 1. In particular, we examine whether it is plausible that the incident had some long-lasting effects on the misbelief itself, and that the spread of the “my Muslim faith” memes had measurable impacts on people’s vote choices or general attitudes toward Obama.

Long-run Effects on Rumor Belief. It is worth noting, first, that the long-run effect of the rumor diffusion on the Muslim misbelief (i.e., τ_{11}) is *not* identified under our DD setup. Let us begin by elaborating on what the over-time change in rumor belief indicates. First, Group A’s change in rumor belief can be decomposed into two parts: the decay of the treatment effects ($\tau_{11} - \tau_9$) and wave effect (ω)—the over-time change that would have taken place without the treatment. Intuitively, since Group A’s Wave 9 responses were gathered right after the treatment, their over-time trends by Wave 11 are likely to have been driven by two forces: 1) people may have forgotten the message and 2) they may have been affected by other events between September and November (e.g., John McCain’s famous denouncement of the Muslim rumor at a town hall meeting in October 2008; see Bumiller 2008). Since Group B’s survey responses were collected before the treatment, their change score can be decomposed into the average treatment effects that persisted until Wave 11 (τ_{11}) and the wave effect (ω). Note that neither group’s over-

time change in rumor belief can be explained in terms of the longevity of treatment effects without accounting for the wave effect. Specifically, one should subtract the wave effect (ω) from Group B's change in rumor belief between W9 and W11 (i.e., $\tau_{11} + \omega - \omega = \tau_{11}$) to identify the long-run effect. Crucially, we cannot estimate the common wave effect ($\omega = E[Y_i(0)|, W = 11] - E[Y_i(0)|, W = 9]$) because we never observe $E[Y_i(0)|, W = 11]$.

Figure K1: Over-time Change Scores by Group and Party ID



Note: Panel A estimates were drawn from the regression model in the Column 1 of Table 1 in the main text. Estimates in Panel B and C were drawn from the regression models using only Republicans and only Democrats, respectively. Leaners were included in the corresponding partisan groups. More results on partisan heterogeneity are reported in Appendix I. Observations were weighted. Other covariates were not included.

Going back to the actual estimates, we found that the newly treated Group B’s acceptance of the Obama Muslim rumor increased by 1.4 percentage point (see Panel A of Figure K1; see also Table 1 in the main text), which was not statistically significant. This would be our estimate of the long-run effect ($\tau_{11} = 0.014$; $p > 0.1$), but only under the assumption that the wave effect was zero ($\omega = 0$). If people’s rumor belief would have *increased* by 1.4 percentage point anyway without the treatment (i.e., $\omega = 0.014$), the estimated long-run effect would be zero. If people’s rumor belief would have *decreased* by 1.4 percentage point without the treatment (i.e., $\omega = -0.014$), the estimated long-run effect would be 2.8 percentage points—or roughly the half of the immediate treatment effect (6.2 points). Which of these scenarios are most plausible?

While we do not observe ω , there are several reasons to believe that it has a negative sign, meaning that acceptance of the false rumor would have decreased in the absence of the treatment. First, as shown in Table K1, people corrected their beliefs about the presidential candidates by 4 percentage points on average. Looking at the individual items, people’s beliefs about the presidential candidates became significantly more accurate on 12 among the 27 factual knowledge items (e.g., Table D1). The median change among the 27 items was a 2-point *increase*. And there here was not a single item on which people’s belief became less accurate.²³

²³ We showed that there was no significant difference in belief correction between the groups in Table D1 in Appendix D.

Table K1: Belief Corrections between Wave 9 and Wave11

	Change	SE	N
Political Knowledge (Index)	0.037*	(0.005)	2309
McCain's state	0.163*	(0.014)	2233
Obama on income tax under 200k	0.143*	(0.017)	<i>2304</i>
Obama's state	0.095*	(0.012)	2254
McCain on Iraq deadline	0.091*	(0.015)	2296
Obama on income tax over 200k	0.082*	(0.015)	2303
McCain on healthcare	0.082*	(0.016)	2296
Obama on power plant emissions	0.063*	(0.016)	2293
Obama on automaker requirements	0.059*	(0.013)	2300
McCain on income tax over 200k	0.056*	(0.016)	2296
Obama on same sex marriage	0.043*	(0.017)	2302
Obama on terror suspect habeas	0.043*	(0.016)	2300
Obama on healthcare	0.037*	(0.012)	2302
Obama on gasoline taxes	0.020	(0.016)	2290
McCain on income tax under 200k	0.019	(0.016)	2299
Obama on illegal immigrants	0.016	(0.016)	2299
Obama on Iraq deadline	0.015	(0.013)	2296
McCain on same sex marriage	0.015	(0.016)	2297
Obama's job before senator	0.012	(0.011)	2220
McCain on terror suspect wiretap	0.007	(0.016)	2291
McCain on power plant emissions	0.007	(0.016)	2291
Obama on terror suspect wiretap	0.001	(0.016)	2299
McCain on terror suspect habeas	-0.004	(0.017)	2294
McCain's religion	-0.005	(0.009)	2262
McCain on illegal immigrants	-0.008	(0.014)	2293
McCain's job before senator	-0.009	(0.016)	2234
McCain on automaker requirements	-0.015	(0.015)	2296
McCain on gasoline taxes	-0.017	(0.015)	2292
Weights			Yes

Note: OLS estimates with standard errors in parentheses. * $p < 0.05$.

Granted, some of the knowledge items are about neutral facts—thus measuring ignorance, not misinformation. Nonetheless, respondents substantially corrected the misbelief that Obama supported a highly unpopular tax policy (14 points on average; italicized in Table

K1). And Republicans showed more substantial belief correction (17 points) than Democrats (11 points). It is plausible that the flows of corrective information about the Muslim rumor also moved people in the same direction. For instance, two prominent Republicans (John McCain and Colin Powell) publicly denounced the rumor in October 2008, and this “unexpected” defense of their political opponent received heightened media attention. These in-partisan elite sources are known to have the best chance of increasing the willingness to reject political rumors (Berinsky 2017). To the extent that these extraneous events undercut people’s rumor acceptance between September and November (and thus $\omega < 0$), it is possible that the long-run effect is in fact larger than the observed change of Group B’s rumor belief ($\tau_{11} > 0.014$).

Finally, we address one important aspect of the finding that Group B’s rumor belief did not change significantly between Waves 9 and 11. This is true, on average. It is not true when we look at partisan groups separately (see Panels B and C of Figure K1).²⁴ Among Republicans, the combination of long-term treatment effects and wave effects was positive and significant (see the Group B’s estimate in Panel B). We therefore find it implausible, albeit possible, that τ_{11} is zero for Republicans, because then ω would have to be 0.06—that is, Republicans would have increased their rumor belief by 6 percentage points even without the treatment. Considering the general improvement in other types of political knowledge and the highly publicized denouncement of the rumor by Republican elites, a positive wave effect ($\omega = 0.06$) seems unlikely. Thus, we interpret Figure K1 as suggestive evidence that the rumor circulation may have had a long-term effect, at least among Republicans.

²⁴ See Appendix I for evidence and discussion on the moderating effect of partisanship.

Electoral Consequence. Another potential implication of the rumor diffusion may be that it impacted not only the rumor belief itself but also people's vote choices or at least their attitudes toward Obama. However, we find this possibility extremely implausible for several reasons.

First, throughout various placebo tests drawing both on the ANES and on the rolling cross-section surveys from the NAES, we found that neither people's voting intentions nor their attitudes toward Obama significantly changed around the treatment period (see Figures D1 and D2 in Appendix D). And this was the case whether we look at explicit measures or an implicit measure of attitudes toward Obama.

Second, such null findings are not surprising because the effect of rumor circulation on vote choice should be smaller than its impact on rumor belief itself (which was 6.2 percentage points in our main estimator), unless the effect of rumor belief fully translates into vote choice such that everyone who believes the rumor does not vote for Obama and everyone who does not believe the rumor votes for Obama. In most likelihood, the causal impact on rumor belief on other political outcomes would be far less than 1. To examine this possibility empirically, we report regression models where the W9 to W11 *change* in vote choice (and attitudes) were regressed on W9 to W11 *change* in rumor belief (i.e., individual fixed effects models) in Table K2. These models would identify the causal impact of the rumor belief under the assumption that there were no time-varying confounders (i.e., the parallel trends assumption) and there was no reverse causation (i.e., voting for Obama wouldn't affect people's belief in rumor).

As can be seen, the effect of the rumor on voting for Obama was (insignificant) 3 to 4 percentage point decrease. Its impact on affects toward Obama was 5 to 6 percentage point decrease. That means the rumor circulation may have decreased vote for Obama by 0.06×0.03

= 0.0018 to $0.06 \times 0.04 = 0.0024$ (or 0.18 to 0.24 percentage point), and negatively affected people's affects toward Obama by $0.06 \times 0.05 = 0.003$ to $0.06 \times 0.06 = 0.0036$ (or 0.30 to 0.36 percentage point). Small as these estimates may seem, they are likely to be the upper bound of the actual effects. Most importantly, there can very well be time-varying confounders omitted in Table K2—e.g., change in use of partisan media—that are correlated with both rumor belief and vote choice in the same direction. In such a case, the electoral consequence of the rumor circulation may be even smaller than 0.2 percentage point.

Table K2: Fixed-Effects Estimates of Rumor Belief on Vote Choice and Affects toward Obama

	(1)	(2)	(3)	(4)
Dependent Variable	Vote for Obama		Affects toward Obama	
Muslim Rumor Belief	-0.049 (0.032)	-0.033 (0.028)	-0.060* (0.019)	-0.051* (0.016)
Observations	1863	1859	2064	2061
Covariates	No	Yes	No	Yes
Individual Fixed Effects	Yes	Yes	Yes	Yes
Weights	Yes	Yes	Yes	Yes

Note. The set of covariates was the same as Table F1, except those included as the DV in the model (e.g., vote choice was not included as a covariate in Column 2). * $p < 0.05$.

Finally, previous theories in communication effects suggest the treatment effect would be more pronounced among those predisposed to dislike Obama, and we showed empirically that the observed treatment effects were driven almost entirely by such individuals (see Appendix I). Given that they would have been unlikely to vote for Obama or liked him in the first place, it is highly doubtful that many people changed their voting decisions because of the rumor diffusion.

Taken as a whole, it seems very unlikely that the event affected the election outcome to any measurable extent.

Appendix L: An Example of “My Muslim Faith” Messages

He is Muslim.²⁵

B. Hussein Obama in an ABC interview slipped up by saying “my Muslim faith.”

I am a Christian. If I were in an interview or otherwise, I would never even think of saying “my Buddhist faith” or “my New Age faith” or “my Episcopalian faith.”

I am none of these. I have never intended to be one of these. I never plan on being one of these. I have never thought of myself under these labels.

Therefore, I would never let slip out of lips any such confession.

Yet B. Hussein did just that. He said “my Muslim faith.”

That is because Muslims consider Jesus a prophet, hence B. Hussein stating to media that he prays to Jesus every day. Maybe he does, praying to the Muslim prophet Jesus.

Biblical Christians believe Jesus to be the biblical God incarnate.. Christ said that no one comes to the Father but by Him. Christ called Himself the Light of the World, the Way, Truth and the Life, the Door, and so forth.

Christ’s followers regarded Him as deity in flesh and bones. They were persecuted for believing that. They were adamant however in holding to that tenet for they had faith that the Messiah appeared in the Carpenter of Nazareth.

Biblical Christians never regard Jesus as merely a prophet or social example or humanitarian.

Muslims consider Jesus as a prophet who will return with their Islamic messiah. The Islamic messiah will return to Earth in the midst of a holocaust, hence the Iranian thug president planning on blowing up the world in order to usher in the Islamic messiah.

On 9-11, there were those Muslims who exclaimed aloud, “I am Christian.” Reports are that they did so for safety’s sake. They were in fact Muslims. But they claimed to be “Christian.”

That was permissible for Jesus is regarded in the line of prophets; therefore, a Muslim can claim to be “Christian” while all the while giving full allegiance to Allah.

Interestingly enough, members of the United Church of Christ (Congregational) to which B. Hussein belongs can claim to be anything religious, even Muslim and Christian. The UCC is that theologically liberal members can write their own creeds, their own ‘holy writ.’

Consequently, for B. Hussein to testify that he is “Christian” is in keeping with Islam and the UCC.

Further, Islamics are taught via the Koran to lie in order to advance Allah’s cause. Therefore, for B. Hussein even to lie about his religious label is permissible.

I repeat: I would never refer to myself as a Buddhist, New Age devotee, Anglican or Hindu because those tags never come to my mind when speaking about my relationship to God.

I am Christian. I say I am Christian. I would never let slip another religious allegiance for that would never cross my mind.

That “my Muslim faith” from B. Hussein is fact. He is Muslim.

²⁵ <http://obambi.wordpress.com/2008/09/08/he-is-muslim/>

References

- DeBell, Matthew, John A. Krosnick, and Arthur Lupia. 2010. *Methodology Report and Users Guide for the 2008-2009 ANES Panel Study*. Palo Alto, CA, and Ann Arbor, MI: Stanford University and the University of Michigan.
- Berinsky, A. J. 2017. "Rumors and Health Care Reform: Experiments in Political Misinformation." *British Journal of Political Science* 47 (2): 241–62.
- Berinsky, Adam J. 2018. "Telling the Truth about Believing the Lies? Evidence for the Limited Prevalence of Expressive Survey Responding." *The Journal of Politics* 80 (1): 211-224.
- Bumiller, Elisabeth. 2008. McCain Draws Line on Attacks as Crowds Cry 'Fight Back'. *New York Times*, 10 October.
- Bullock, J.G., Gerber, A.S., Hill, S.J. and Huber, G.A. 2015. "Partisan Bias in Factual Beliefs about Politics." *Quarterly Journal of Political Science* 10: 519–78.
- Dilliplane, S., Goldman, S. K., and Mutz, D. C. 2013. "Televised Exposure to Politics: New Measures for a Fragmented Media Environment." *American Journal of Political Science* 57 (1): 236–48.
- Garrett, R. Kelly. 2011. "Troubling consequences of online political rumoring." *Human Communication Research* 37 (2): 255-274.
- Gerber, Alan S., James G. Gimpel, Donald P. Green, and Daron R. Shaw. 2011. "How large and long-lasting are the persuasive effects of televised campaign ads? Results from a randomized field experiment." *American Political Science Review* 105 (1): 135-150.
- Hallin, Daniel C. 1989. *The Uncensored War: The Media and Vietnam*. Berkeley: University of California Press.

- Hill, Seth J., James Lo, Lynn Vavreck, and John Zaller. 2013. "How quickly we forget: The duration of persuasion effects from mass communication." *Political Communication* 30 (4): 521-547.
- Hollander, Barry A. 2011. "Persistence in the Perception of Barack Obama as a Muslim in the 2008 Presidential Campaign." *Journal of Media and Religion* 9 (2): 55-66.
- Johnson, Greg, Greg Lewis, Julie Millican, Varun Piplani, and Nathan Tabak. 2008. "Conservative radio hosts seize on Obama comment to revive false rumors about his faith." *Media Matters for America*, September 10. Retrieved from <http://mediamatters.org/research/2008/09/10/conservative-radio-hosts-seize-on-obama-comment/144935>.
- Khanna, K., and Sood, G. 2018. "Motivated Responding in Studies of Factual Learning." *Political Behavior* 40 (1): 79–101.
- Lopez, J., and Hillygus, D. S. 2018. "Why So Serious?: Survey Trolls and Misinformation." Working Paper.
- Nyhan, B. 2010. Why the "Death Panel" Myth Wouldn't Die: Misinformation in the Health Care Reform Debate. *The Forum*, 8(1).
- Nyhan, B. Jason Reifler, Christopher Edelman, William Passo, Ashley Banks Emma Boston, Andrew Brown, Robert Carlson, KayAnne Gummersall, Elizabeth Hawkins, Lucy McKinstry, Jonathan Mikkelsen, Emily Roesing, Vikram Srinivasan, Sarah Wakeman, Lindsey Wallace, and Rose Yan. 2017. "Answering on cue? How corrective information can produce social desirability bias when racial differences are salient." *Unpublished Manuscript*. Accessed at: <http://www.dartmouth.edu/~nyhan/obama-muslim.pdf>

- Pew Research Center. 2008. How the News Media Covered Religion in the 2008 General Election: False Rumors that Obama is a Muslim, 20 November. Available at <http://www.journalism.org/2008/11/20/false-rumors-that-obama-is-a-muslim/>, accessed 30 March 2017.
- Price, V., and Zaller, J. 1993. "Who Gets the News? Alternative Measures of News Reception and their Implications for Research." *Public Opinion Quarterly* 57 (2): 133–64.
- Prior, M., Sood, G., and Khanna, K. 2015. "You Cannot be Serious: The Impact of Accuracy Incentives on Partisan Bias in Reports of Economic Perceptions." *Quarterly Journal of Political Science* 10 (4): 489–518.
- Prior, M. 2009a. "The Immensely Inflated News Audience: Assessing Bias in Self-Reported News Exposure." *Public Opinion Quarterly* 73 (1): 130–43.
- Prior, M. 2009b. "Improving Media Effects Research through Better Measurement of News Exposure." *Journal of Politics* 71 (3): 893–908.
- Prior, M. 2013. "The Challenge of Measuring Media Exposure: Reply to Dilliplane, Goldman, and Mutz." *Political Communication* 30 (4): 620–34.
- Taber, C. S., and Lodge, M. 2006. "Motivated skepticism in the evaluation of political beliefs." *American Journal of Political Science* 50 (3): 755–769.
- Tesler, Michael. 2013. The Return of Old-Fashioned Racism to White Americans' Partisan Preferences in the Early Obama Era. *The Journal of Politics* 75 (1):110–23.
- Toner, R., and Nagourney, A. 2008. "McCain Seen as Less Likely to Bring Change, Poll Finds." *The New York Times*, September 18. Retrieved from <https://www.nytimes.com/2008/09/19/us/politics/19pollcnd.html>

Treisman, D. (2015), Income, Democracy, and Leader Turnover. *American Journal of Political Science*, 59.

Weeks, Brian, and Brian Southwell. 2010. The Symbiosis of News Coverage and Aggregate On-Line Search Behavior: Obama, Rumors, and Presidential Politics. *Mass Communication & Society* 13 (4):341–60.

Zaller, J. 1992. *The Nature and Origins of Mass Opinion*. New York: Cambridge University Press.