

# A Field Experimental Test of Political Primes: Does the Constitution Affect Values, Vote Choice, or Turnout?

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## Abstract

Existing theory and evidence on the effects of subtle primes suggests that even small manipulations can have large and long-lasting political consequences. We conduct a massive field experiment in which 4 million Americans living in 6,057 voting precincts in six nonbattleground states were randomly assigned to be mailed either nothing or a pocket edition of the Declaration of Independence and US Constitution in advance of the 2016 presidential election. Because we doubt that subjects actually read the constitution when it is unexpectedly delivered to them, we imagine that any effects would be due to priming patriotism and shared national values. We find that, despite the enormous historical and political importance of these documents and the larger-than-usual role that pocket Constitutions played in the 2016 election, receiving the Constitution in the mail does not appear to affect subjects' values, vote choice, or turnout.

In psychology, the term priming is used to refer to instances in which a stimulus activates mental representations or motivations that in turn produce changes in behavior (Molden 2014). Although some of the most arresting examples of priming involve instances in which individuals are unaware of a stimulus or its relevance to subsequent behavior, priming also applies to instances in which people are well aware of or make active use of stimuli ranging from physical sensations to detailed information (Higgins and Eitam 2014). Thus, although propositions such as “priming of the goal to cooperate cause[s] participants to replenish a commonly held resource more readily” (Bargh 1984, p. 1014) have been tested using subtle priming tasks such as unscrambling words related to cooperation, in principle the proposition could also be tested by having participants read an essay extolling the merits of cooperation.

The influence of priming on public opinion and political behavior is an active research topic and one where there seems to be little consensus about whether and to what extent

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priming is politically consequential. Subtle primes are often said to have strong effects. To cite just a few well-known examples, voters who cast their ballots in school buildings are more likely than those who vote elsewhere to support ballot measures that fund schools (Berger, Meredith and Wheeler 2008); voter turnout is higher among citizens who are encouraged to vote with postcards that include a graphic of watchful eyes (Panagopoulos 2014); when primed to think about their national identity, Dutch citizens become more opposed to immigration (Sniderman, Hagendoorn and Prior 2004). On the other hand, claims that subtle primes have substantial political effects quite often attract critics who argue that the findings fail to replicate in other settings (Matland and Murray 2015, but see Panagopoulos 2015), are attributable to methodological artifacts such as lack of blinding (Doyen et al. 2012), or enjoy exaggerated credibility due to publication bias (Schimmack, Heene and Kesavan 2017). Less subtle interventions, such as explicit reminders and requests seem to enjoy clearer empirical support. For example, text messages reminding people to vote seem to increase turnout (Dale and Strauss 2009; Malhotra et al. 2011) even when they are delivered days before the election (Bhatti et al. 2017). Such studies, however, lack the caché of research on subtle primes. Few would be surprised to learn from a large randomized trial that a text message encouragement to vote elevates turnout by 0.8 percentage points.

Somewhere between subtle primes that are overt requests and scarcely perceived lies a theoretically meaningful class of primes that are symbolic in nature. Flags, anthems, religious artifacts, and cultural symbols are all potentially evocative primes (Butz 2009; Carter, Ferguson and Hassin 2011; Bhatti et al. 2015). In this study, we consider one such prime, the U.S. Constitution. During the 2016 election campaign, we distributed copies of the Constitution by mail to a very large pool of registered voters. Importantly, the Constitution was packaged in a transparent envelope so that the recipient can readily see the contents. Attentiveness to the prime was unusually high for a treatment conveyed by mail: a post-election survey indicated that a majority of the treatment group recalled receiving the Constitution. We did not expect recipients to read the document that we sent them; rather, we sought to assess whether the Constitution, as a symbol, primed thoughts or feelings that induced people to vote, endorse key constitutional tenets, or express patriotic sentiments.<sup>1</sup>

Although our study is quite well-powered by the standards of the priming literature, we fail to find statistically significant effects for any of the outcome variables that we listed in our pre-analysis plan. The electoral data show no effects on turnout or vote choice. The survey data show no change in support for constitutional principles or patriotism more broadly. At most, the results hint that subjects sent the Constitution express more pride in being American, but this estimated effect is very small, at only 0.06 of a scale point on a 4-point scale. It appears that priming voters with this political symbol largely failed to produce enduring effects. We conclude by discussing these results in light of other experiments that assess the effects of exposure to political and cultural symbols.

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<sup>1</sup>Carter, Ferguson and Hassin (2011), for example, estimate that a single exposure to the American flag in an online survey context raised support for John McCain by 10.7 percentage points.

# 1 Experimental Context

Our experiment was conducted in the context of the 2016 US presidential election, which featured a series of events directly related to pocket Constitutions specifically. Khizr Khan, the father of a Muslim soldier who died in Iraq, spoke at the Democratic National Convention in Philadelphia, pointedly asking Donald Trump “have you even read the United States Constitution” before offering to lend his own copy. Khan exhorted Trump to look for the words “liberty” and “equal protection of law.” The speech was widely shared; Trump extended the shelf-life of the story by discussing it on ABC News, in comments that were interpreted by many as critical of the Khan family. Khan’s gesture sparked a nationwide run on pocket Constitutions. Within days of the speech, the American Civil Liberties Union reported they had officially sold out with 100,000 orders from all 50 states.<sup>2</sup> An edition published by the National Center for Constitutional Studies, a conservative group, rose to No. 2 on Amazon’s list of best-selling books, after “Harry Potter and the Cursed Child.” The U.S. Government Printing Office promoted its own edition on Twitter (McPhate 2016).

By August 4, 2016, at a Trump campaign rally in Portland, Maine, protesters stood silently, with pocket Constitutions held high, reportedly as a gesture of solidarity with the Khans (Kurtzleben 2016). This phenomenon continued, as protesters waved pocket Constitutions at Trump speeches and rallies for the duration of the campaign. Of course, Khan and the protesters he inspired are not the first to evoke the Constitution as a rhetorical gesture. Over the years, pocket Constitutions have made star appearances at tea party rallies, during presidential debates, in newsrooms, and at press conferences.<sup>3</sup>

The political consequences of the Constitution, whether they operate through priming or other means, are not of pedestrian interest to the Cato Institute, a Washington think tank where one of the present study’s co-authors works. During the 2016 campaign, Cato sold 231,191 of its own pocket edition of the U.S. Constitution and Declaration of Independence, an all-time record. Over the years, Cato has distributed more than 6 million copies, through all manner of requests: Civics teachers contact Cato to request boxes of pocket Constitutions for students. Chambers of Commerce request copies for gift bags at events. Tea party groups requested thousands for giveaways at meetings and rallies. U.S. soldiers in Iraq requested boxes of Cato’s Arabic translation of the pocket Constitution to distribute. At the start of new legislative sessions, Cato sends copies of its pocket Constitution to all U.S. Senators and Congressmen. Cato distributes hundreds of thousands more as part of fundraising solicitations.

Our experiment therefore has practical implications for Cato, the ACLU, and similar institutions that distribute the constitution as part of their standard activities. Does mailing out copies of a pocket Constitution change citizens’ attitudes, feelings, or behaviors?

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<sup>2</sup><https://www.aclu.org/blog/speak-freely/constitution-still-bestseller-after-all-these-years>

<sup>3</sup>For example, in a 2007 presidential primary debate, Dennis Kucinich brandished his pocket Constitution to criticize Dick Cheney. World News Tonight anchor Peter Jennings reportedly kept a pocket Constitution with him on set, every night. In 1998, at a Capitol Hill press conference after the Supreme Court rejected the line-item veto, Senators Carl Levin and Robert Byrd held up their pocket Constitutions, as Senator Pat Moynihan looked on (Woodruff 2015).

## 2 Design

The subjects of our experiment are voters who live in medium-sized precincts in 6 states: Idaho, Kansas, New Mexico, South Dakota, Utah, and Washington. We chose these states because pre-election polling shows that in these states, the outcome of the Presidential contest was not in doubt at the time treatment was deployed. Indeed, Donald Trump won Idaho, Kansas, South Dakota, and Utah by healthy margins, and Hillary Clinton won Washington and New Mexico easily. We wanted to conduct our experiment in nonbattleground states not only because we wanted to minimize the (already very small) probability that our treatments could alter electoral outcomes, but also because we surmised that in such places, subjects’ political attitudes and vote choice would be more susceptible to our treatment.

We obtained lists of registered voters from the voter file vendor i360. Because we employed a precinct-level random assignment, we restricted our sample to subjects who lived in precincts that comprise between 250 and 500 households, where household is defined as any group of people living at a single address according to the i360 voter file. Restricting our universe of subjects to those living in these medium-sized precincts meant that we could treat a relatively large number of homogeneously-sized precincts.

We employed a blocked-and-clustered experimental design. Individuals are nested within households which are in turn nested within voting precincts. Randomization was carried out at the precinct (cluster) level: 49 precincts were chosen within each state among all medium-sized precincts. All households within a treated precinct were mailed a copy of the U.S. Constitution and Declaration of Independence. Households received the Constitutions starting the week of October 16th, approximately two to three weeks before the election. We consider all individuals within a treated household to be treated. Precincts were grouped into blocks according to their size, as shown in Table 1. This scheme allows us to treat a greater number of precincts given resource constraints because smaller precincts are relatively less expensive to treat. Table 2 shows the resulting number of treatment and control units, at all three levels of aggregation: individual, household, and precinct. Our randomization scheme generates different probabilities of assignment to treatment for different units. We incorporate these differences in our analysis procedure by including inverse probability weights.

Table 1: Block Randomization Scheme

Block (Precinct Size)	Number of Treated Precincts in Each State
[250, 300)	16
[300, 350)	12
[350, 400)	10
[400, 450)	7
[450, 500]	4

We observe outcomes at all three levels of aggregation. Turnout decisions are observed on post-election voter files at the *individual* level. Turnout is coded 1 if the person voted

Table 2: Experimental Design

Treatment	State	Individuals	Households	Precincts
Control	ID	211573	122530	319
Control	KS	515874	292289	802
Control	NM	255497	143492	389
Control	SD	90348	51396	141
Control	UT	786954	407951	1089
Control	WA	1954397	1093974	3023
Treatment	ID	28709	16964	49
Treatment	KS	29363	16870	49
Treatment	NM	30238	16877	49
Treatment	SD	30775	16922	49
Treatment	UT	31468	16950	49
Treatment	WA	30048	16735	49

(by any method) and 0 others. Political attitudes are measured at the *household* level with a telephone survey (described in further detail below). Because we cannot control who answers the phone when a household is called, we consider the responses to the phone survey to be household-level measurements. We observe aggregate vote choice (and turnout) at the *precinct* level. Trump vote share will be calculated as the the proportion of votes cast in a precinct that were for Trump. Precinct-level turnout will be defined as the number of votes cast in a precinct divided by the number of voters registered in a precinct according to the i360 voter file we obtained prior to the election. Theoretically, this number could exceed 100% if some voters in a precinct did not, for some reason (including registering close to the election) appear on our voter file. The advantage of this method is that we do not condition on a possibly post-treatment variable (registering) when calculating turnout (Coppock and Green 2016; Nyhan, Skovron and Titiunik 2017).

## 2.1 Individual-Level Turnout

[As of this writing, individual-level 2016 turnout data are not yet available.]

## 2.2 Household-Level Survey

The post-election household survey consisted of 11 questions that were administered via robocall to the subset of households for which i360 had a non-mobile telephone number. We surveyed all treated households (66,200 households) and a random subsample of the control households of equivalent size (68,078). We fielded the survey over four days, between November 17 and November 20, ten days after the election. We obtained some response from 10,818 households, for an overall response rate of 8%. This response rate did not appear to differ across treatment and control households. The estimated difference in response rates

was -0.2 percentage points, with a cluster-robust standard error of 0.2 percentage points ( $p = 0.26$ ). In addition to survey nonresponse, we also experienced item nonresponse: the last question on the survey received only 7,866 responses, and again, this nonresponse did not appear to vary with treatment status.

We asked subjects the following 13 questions. After each question, we also indicate our pre-registered guess for the direction of the average treatment effect. These predictions were made without a large prior literature from which to reason about signs and magnitudes of treatment effects, so all tests of statistical significance for these questions will be two-sided.

*Follow Election* How closely did you follow the 2016 presidential race: very closely, somewhat closely, or not so closely? [Very Closely; Somewhat Closely; Not So Closely. Prediction: more closely]

*Government Size* Some people say the less government, the better. Others think there are more things that government should be doing. Which comes closer to your view? [The less government, the better; There are more things the government should be doing. Prediction: ambiguous]

*Government Values* Some people think the government should promote traditional values in our society. Others think the government should not favor any particular set of values. Which comes closer to your view? [Government should promote traditional values; Government should not favor any particular set of values. Prediction: not favor any particular values]

*Individual Rights* Which comes closer to your view? If a majority of people want something to happen, the rights of a few shouldn't stand in the way; OR individual rights should be protected, even when that means saying no to something the majority of people want to happen. [The rights of the few shouldn't stand in the way of a majority; Individual rights should be protected, even if a majority want something to happen. Prediction: Individual rights should be protected]

*Gun Rights* What do you think is more important...? [Protecting the Constitutional right of Americans to own guns; Controlling gun ownership to prevent gun violence. Prediction: Protecting the Constitutional right of Americans to own guns]

*Free Speech Religion* Should the government be able to prevent people from publicly saying things that are offensive to your religion or beliefs [People should be able to say these types of things publicly; The government should be able to prevent people from saying these things publicly. Prediction: People should be able to say these types of things publicly.]

*Free Speech Minorities* Should the government be able to prevent people from publicly saying things that are offensive to minority groups [People should be able to say these types of things publicly; The government should be able to prevent people from saying these things publicly. Prediction: People should be able to say these types of things publicly.]

*Vote Choice* If you got a chance to vote in the recent presidential election, who did you vote for? [Donald Trump; Hillary Clinton; Gary Johnson; Evan McMullin; Another candidate; did not get a chance to vote. Prediction: ambiguous]

*Tried Persuasion* During the campaign, did you talk to any one and try to persuade them to vote for or against one the parties or candidates? [Yes; No. Prediction: Yes]

*Proud American* How proud are you to be an American? [Extremely proud; very proud; somewhat proud; or not at all proud. Prediction: more proud]

*Recall* Over the past month or so, do you recall receiving in the mail a pocket-sized US Constitution and Declaration of Independence? [Yes; No. Prediction: Yes]

*Read Constitution* Have you ever read the US Constitution, either all or part of the way through? [Yes; No. Prediction: Yes]

*Read Declaration* Have you ever read the Declaration of Independence, either all or part of the way through? [Yes; No. Prediction: Yes]

Subjects were randomly assigned to respond to one of *Free Speech Religion* and *Free Speech Minorities* and one of *Read Constitution* and *Read Declaration*. We will assess treatment effects on all 13 of these questions separately and pooling the split-ballot questions together.

## 2.3 Precinct-Level Vote Choice

All six states in our experiment have released precinct level vote totals for all candidates; while these data are in principle available, preparing it for analysis is a painstaking process. As of this writing, we have prepared the Washington and New Mexico files, with the remainder to be completed in the coming weeks. We have two main dependent variables: the share of ballots cast for Donald Trump and the total votes cast.

## 2.4 Analysis

In our preregistration plan, we stated that we would report two sets of inverse probability weighted estimates: unadjusted differences in means and covariate adjusted estimates, with different covariates used at the individual, household, and precinct levels. Gathering and merging the covariate data was more difficult than anticipated, so we will instead present a single set of estimates that includes inverse probability weights and experimental block, which accounts for both state and precinct size. Standard errors for the individual-level and household-level regressions will be clustered at the precinct level. We will employ HC2 robust standard errors for the precinct-level regressions, as they are already at the correct level of aggregation.

### 3 Results

In this section, we will present evidence that the constitutions did not impact political attitudes or behavior. For that reason, we will begin our analysis with a manipulation check to verify that the experiment proceeded as designed.

#### 3.1 Manipulation Checks

Table 3 displays estimates of the effect of our mailer on whether subjects recalled receiving a pocket constitution in the mail and whether they read the Constitution or the Declaration of Independence. As indicated by the “Control Mean” row, 10% of untreated households report receiving a Constitution in the mail. This figure is probably due to a mix of measurement error, faulty memories, and other organizations sending constitutions through the mail. Our treatment households, however, were 54 percentage points more likely to report receiving the mailer.

Large fractions of our control group claim to have read the Constitution (86%) or the Declaration of Independence (91%). The treatment had a small but statistically significant impact (2.7 percentage points,  $p = 0.03$ ) on reporting having read the Constitution. Curiously, we observe a negative effect of similar magnitude on having read the Declaration of Independence, though that estimate is not statistically significant. We conclude from these manipulation checks that subjects who were assigned to receive pocket constitutions in the mail did indeed receive them; some treated subjects may have read it as a result.

Table 3: Manipulation Check

	Recall (1)	Read Constitution (2)	Read Declaration (3)	Read Either (4)
Mailed Constitution	0.544*** (0.012)	0.027** (0.013)	-0.021 (0.013)	0.003 (0.010)
Block Fixed Effects	Yes	Yes	Yes	Yes
Control Mean	0.102	0.859	0.906	0.882
N	7,923	3,960	3,906	7,866

\* $p < .1$ ; \*\* $p < .05$ ; \*\*\* $p < .01$

All models include inverse probability weights.

Cluster-robust standard errors are in parentheses.

#### 3.2 Household-level Political Attitudes

By far the most consequential political outcome that we measured in the household-level survey was 2016 vote choice. As mentioned earlier, both the Clinton and Trump campaigns

used explicitly constitutional language to criticize the other. Pocket-sized constitutions were especially potent symbols after the Democratic convention. Table 4 shows the estimated effect of being sent a pocket constitution on the probability of voting for Donald Trump. In the control group, 46% of our households report voting for Trump; the treatment group was 1 percentage point more likely to vote for Trump, though as indicated by the standard error of 1.8 points, this estimate cannot be distinguished from zero.

Table 4: Household Survey: Vote Choice

	Voted for Trump
Mailed Constitution	0.010 (0.018)
Block Fixed Effects	Yes
Control Mean	0.459
N	8,063

\*p < .1; \*\*p < .05; \*\*\*p < .01

All models include inverse probability weights.

Cluster-robust standard errors are in parentheses.

Turning next to subjects' support for individual rights, the right to bear arms, and freedom of speech even when it is offensive, we see in Table 5 that the majority of our control households are in favor of these constitutional values. Receiving the Constitution did not appear to change these views.

Table 5: Household Survey: Constitutional Values

	Individual Rights (1)	Gun Rights (2)	Free Speech Religion (3)	Free Speech Minorities (4)	Free Speech (5)
Mailed Constitution	-0.004 (0.012)	0.015 (0.018)	0.0002 (0.015)	-0.004 (0.015)	-0.001 (0.010)
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes
Control Mean	0.603	0.598	0.830	0.765	0.798
N	8,666	8,511	4,177	4,103	8,280

\*p < .1; \*\*p < .05; \*\*\*p < .01

All models include inverse probability weights.

Cluster-robust standard errors are in parentheses.

Table 6 shows the effect of treatment on whether subjects report following the election, their views on the size of government, the values the government should promote, whether subjects attempted to influence the votes of others, and whether subjects feel proud to be an American. Using a one-tailed test (as pre-registered in our analysis plan), our estimate of the average treatment effect on pride is statistically significant at the 5% level, though

the magnitude of the effect (0.06 scale points on a 4-point scale) is relatively small. The estimated effects on all other dependent variables in Table 6 are close to zero and not statistically significant.

Table 6: Household Survey: Political Attitudes

	Follow Election	Government Size	Government Values	Tried Persuasion	Proud American
	(1)	(2)	(3)	(4)	(5)
Mailed Constitution	-0.019 (0.016)	0.007 (0.015)	0.009 (0.014)	0.024 (0.020)	0.067* (0.035)
Block Fixed Effects	Yes	Yes	Yes	Yes	Yes
Control Mean	0.603	0.598	0.830	0.765	0.798
N	10,818	9,621	9,188	4,020	3,973

\*p < .1; \*\*p < .05; \*\*\*p < .01

All models include inverse probability weights.

Cluster-robust standard errors are in parentheses.

### 3.3 Precinct-level Vote Choice and Turnout

Our precinct-randomized design allows us to measure the effects of our treatment on actual vote choice directly, rather than on survey expressions of vote intention or recall. Table 7 shows the estimated effects among the 3,509 Washington and New Mexico precincts in our study. In control precincts, Trump vote share averaged 35.6% and was 0.3 points lower in treatment precincts. This estimate has the opposite sign from our survey estimate of the effect on Trump vote preference and is similarly statistically insignificant. On average, 461.7 votes were cast for president in control precincts; our best guess is that the Constitution decreased turnout in treatment precincts by 7.6 votes, though this estimate too is insignificant.

### 3.4 Individual-level Turnout

[This section forthcoming]

Table 7: Precinct Level Outcomes (WA and NM only)

	Trump Vote Share	Total Votes
	(1)	(2)
Mailed Constitution	-0.003 (0.024)	-7.649 (13.202)
Block Fixed Effects	Yes	Yes
Control Mean	0.365	461.674
N	3,509	3,509

\*p < .1; \*\*p < .05; \*\*\*p < .01

All models include inverse probability weights.

HC2 robust standard errors are in parentheses.

## 4 Discussion

The literature on priming has drawn fire for the surprising findings that frequently emerge from underpowered (Schimmack, Heene and Kesavan 2017) or methodologically deficient experiments (Doyen et al. 2012). In the domain of politics, it is difficult to know how much stock to put in findings that, for example, suggest that viewing a series of four photographs (ostensibly to assess whether they were taken during the morning, afternoon, or evening), two of which contain images of the American flag, causes people to express significantly more support for Republicans, and that exposure to flags while people are reporting their vote intentions seems to have effects that last for months (Carter, Ferguson and Hassin 2011). If priming effects could be generated in a dependable way, the potential for persuasion and mobilization would seem to be enormous.

Inspired by the prospect of measuring the political effects of evocative primes, we distributed Constitutions randomly on a grand scale. While the Constitution may be evocative in most political contexts, we note that pocket Constitutions made for an unusually evocative prime in the 2016 campaign cycle. Unlike other priming experiments, ours is well-powered and draws its outcome measures from a combination of survey interviews and administrative data. Surveys are used to gauge effects on individual policy preferences and vote choice; precinct-level vote outcomes allow us to assess aggregate effects on voter turnout and vote choice.

The bottom line – with the caveat that we still have yet to code and analyze all of the administrative data – can be summarized quite simply. The arrival of the Constitution in the mail was a memorable event: our manipulation check offers one of the strongest recall effects ever reported in a large-scale field experiment. Despite the fact that the treatment group was 54 percentage points more likely to recall receiving the Constitution than their control group counterparts, the estimated effects on attitudes and behavior hover near zero. We see no appreciable effect on voter turnout, vote choice, values, or policy preferences. At most, there is a glimmer of support for the hypothesis that Constitutions stir feelings of pride in being American, but this relationship is relatively weak and our estimate is just significant at the 5% level using a one-tailed test.

To be sure, the Constitution is just one stimulus among many, and our failure to find that it generates noteworthy priming effects may simply indicate that everyone on both left and right feels that the Constitution stands for the same things that they do. (Of course, it remains unclear why this feel-good prime did not set in motion an increase in political participation.) We should note that our research team recently experimented with other evocative primes (e.g., the Bible) and found similarly weak effects. Although these experiments cannot rule out small effects, they contribute some important stubborn facts to the literature on priming effects. The upper bound of our confidence interval is smaller than many reported effects. Any theoretical attempt to synthesize the literature must now explain not only why a single exposure to the American flag produces large effects but also why Constitutions and Bibles leave no footprints.

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