Quarterly Journal of Political Science, 2017, 12: 37–57

Assessing the Breadth of Framing Effects

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ABSTRACT

Issue frames are a central concept in studying public opinion, and are thought to operate by foregrounding related considerations in citizens' minds. But scholarship has yet to consider the breadth of framing effects by testing whether frames influence attitudes beyond the specific issue they highlight. For example, does a discussion of terrorism affect opinions on proximate issues like crime or even more remote issues like poverty? By measuring the breadth of framing effects, we can assess the extent to which citizens' political considerations are cognitively organized by issues. We undertake a population-based survey experiment with roughly 3,300 respondents which includes frames related to terrorism, crime, health care, and government spending. The results demonstrate that framing effects are narrow, with limited but discernible spillover on proximate, structurally similar issues. Discrete issues not only organize elite politics but also exist in voters' minds, a finding with implications for studying ideology as well as framing.

Keywords: Framing effects; spillover; ideology; persuasion; survey experiment

Research on issue framing has repeatedly demonstrated the capacity of frames to shape public preferences, with no fewer than 46 articles on framing in

*The authors gratefully acknowledge the support of Time-sharing Experiments in the Social Sciences, a project overseen by co-PIs James N. Druckman and Jeremy Freese and funded by the National Science Foundation. This project was approved by the Georgetown

Online Appendix available from:
http://dx.doi.org/10.1561/100.00015139_app
Supplementary Material available from:
http://dx.doi.org/10.1561/100.00015139_supp
MS submitted on 15 October 2015; final version received 7 December 2016
ISSN 1554-0626; DOI 10.1561/100.00015139

leading general-interest political science journals between 2010 and 2015.[1] Yet despite the wealth of research, scholars have yet to identify just how broad framing effects on public opinion are. We know, for example, that exposure to terrorism-related rhetoric can have pronounced impacts on American public opinion. Still, when frames related to terrorism become salient, should we expect sweeping attitudinal changes across a range of issues, such as crime or even poverty?

Assessing the breadth of framing effects is important for multiple reasons. Issue framing is commonly defined as a psychological process through which communications make a subset of the potentially relevant considerations accessible and applicable to a given evaluation (Price and Tewksbury, 1997). Yet framing research to date has paid little attention to the breadth of considerations made accessible and applicable by frames and has only addressed the question indirectly. While some research reports framing effects that are domain-specific (Druckman *et al.*, 2012; Iyengar and Kinder, 1987; Sniderman *et al.*, 2014), research on racial priming indicates that race-related messages can influence attitudes on a host of issues (Valentino *et al.*, 2002). Considered collectively, existing research is indeterminate. Still, assessing framing effects' breadth is critical in understanding how framing operates as an elite tool. If framing effects are narrow, then each issue needs to be framed anew, and successful frames on one issue will have little impact on others. By contrast, if framing effects are broad, effective elite frames on one issue might have important implications for a range of attitudes. A public debate on terrorism might have far-reaching consequences for attitudes on seemingly unrelated issues.

There are conceptual reasons to study whether framing effects are broad or narrow as well. Recent critiques of framing research caution that common research designs cannot differentiate framing from persuasion, information, or other mechanisms (Huber and Paris, 2013; Leeper and Slothuus, 2015; Scheufele and Iyengar, 2012). By examining the breadth of framing effects, we will be better positioned to identify the mechanisms at work. If a political argument is really shaping opinions by framing — by influencing the accessibility and applicability of existing cognitive considerations — we might not expect to

University Institutional Review Board (2011-299). The authors appreciate insightful comments and/or advice from Kevin Arceneaux, Amber Boydstun, David Broockman, John Bullock, Kevin Collins, D.J. Flynn, Seth Goldman, Gabriel Lenz, Alex Coppock, Rob Kurzban, Matt Levendusky, Diana Mutz, Hans Noel, Erik Peterson, David Yokum, and participants at a 2015 panel of the Midwest Political Science Association's Annual Meeting. They also express their thanks to Julia Christensen, Patrick Gavin, Douglas Kovel, Eric Mooring, Owen O'Hare, Gabrielle Rothschild, and Elena Zhou for their tireless research assistance. The authors likewise thank the *QJPS* editors and anonymous reviewers for their very helpful feedback.

[1]The journals searched were the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Politics*.

see attitude changes on issues that are unrelated. But if political arguments act through other channels, such as by reinforcing partisanship, far-reaching effects make more sense.

Understanding the breadth of framing effects also has the potential to illuminate the structure of political cognition. Framing is typically conceived of as a short-term effect, and so is distinguished from more durable influences on political attitudes such as partisanship or ideology. Still, measuring the breadth of framing effects provides us with a new measure of constraint, or the inter-relationship among various political considerations in people's minds. If we observe that frames invoking distrust of government have especially broad effects, for instance, we might suspect that appeals to distrust tap a network of cognitive considerations that are frequently accessed in tandem — and that distrust serves as an important way in which political considerations are organized in voters' minds.

This paper presents a novel, population-based survey experiment fielded by Time-sharing Experiments in the Social Sciences (TESS) and designed to test the breadth of framing effects. In all, the experiment included more than 3,300 American adults. Its unique design and unusually large sample size enable us to consider the effects of four arguments that vary in the frames they convey as well as the issues they address. The four issues are terrorism, crime, health care, and economic stimulus; they are framed either by emphasizing security or distrust of government action. All of these issues were highly salient nationally during the December 2011 experiment, meaning that related attitudes were likely to be chronically accessible in voters' minds. Accordingly, these four issues present a "most likely" case for spillover effects. Put differently, if spillover effects are not observed on issues that were atop the political agenda during the experiment, it is unlikely that we would find them in less propitious contexts. As outcomes, this paper focuses on preferences about federal spending on each of these issues and on fighting poverty, enabling us to measure effects across frames and across outcomes measured on the same scale. This design allows us to estimate the extent to which the frames differentially influence more proximate attitudes.

Throughout this paper, we conceive of pairs of frames and attitudes as being characterized by varying *distance*, by which we mean the similarity between the frame's issue content and structure and those of the attitude in question. Empirically, the paper compares the effects of arguments on the same issue as the attitude in question, arguments on a different issue but which invoke compatible frames, and arguments on different issues with unrelated frames. Across the various arguments and issue frames, the results reinforce the claim that framing effects are narrow. Each frame's largest effect is on either spending attitudes on the same issue or else on the proximate, structurally similar issue. Moreover, only on the criminal justice and health care frames do we detect spillover effects even on proximate issues. At the elite

level, politics is frequently organized by issue, each with its own constellation of interests. But discrete issues also exist in voters' minds, a finding with implications for the study of ideology as well as framing.

Prior Research and Hypotheses

Framing has come to be a foundational concept throughout the social sciences (e.g., Chong and Druckman, 2007; Gamson and Modigliani, 1989; Iyengar, 1991; Iyengar and Kinder, 1987). The term's meaning varies across and even within fields (Price and Tewksbury, 1997; Scheufele and Iyengar, 2012), but there are core similarities in its use: generically, framing encompasses a variety of processes in communication and cognition in which the presentation of information leads people to evaluations which emphasize select elements of a complex object. In other words, what the various conceptions of framing have in common is that they conceive of framing as simplifying a multidimensional reality. In one canonical example, a proposed Ku Klux Klan rally garners more support when framed in free speech terms than when framed in terms of public order (Nelson *et al.*, 1997).

With a concept as popular and contested as framing, it is unsurprising that scholars have sought to distinguish among its types. Here, we follow recent research by focusing on "emphasis framing." In their study of attitudes toward the death penalty, Baumgartner et al. (2008) define an emphasis framing effect as "defining an issue along a particular dimension (e.g., fairness and innocence) at the exclusion of alternate dimensions (e.g., morality, constitutionality, or cost)." In this conception, people have a variety of mental considerations that might be relevant to a given issue. Issue frames are communications which increase the cognitive availability of certain considerations, making the issue more likely to be viewed in a specific way (Chong and Druckman, 2007). They are thought to do this by influencing the accessibility of mental considerations as well as by shaping which considerations are applicable or appropriate to a given evaluation. In fact, Price and Tewksbury (1997) go so far as to define framing as a process through which communications influence evaluations by shaping the applicability of different pieces of knowledge.

One way to formalize emphasis framing is with reference to the expectancy-value model of attitude formation, in which attitude A toward an object is represented as the sum of a series of considerations c_i and corresponding weights w_i (Leeper and Slothuus, 2015; Nelson *et al.*, 1997). An overall attitude A is thus given by $\sum_{\forall i} c_i w_i$, meaning the sum of each weighted consideration. In a simple example, suppose that people view free speech positively ($c_1 > 0$) but also view social disruption negatively ($c_2 < 0$). If those are the only relevant considerations, then the overall attitude depends on the strength of each consideration and its corresponding weight. Framing changes attitudes

by shifting the weights. But the expectancy-value model of attitude formation also puts an outstanding question into sharp relief: just how many weights change in response to a frame?

Notice that emphasis framing is distinctive from persuasion, as it does not change the set of considerations c_i people have. Instead, it leads them to draw on some considerations more heavily when constructing their evaluation. That said, a single political argument — an act of communication intended to influence others' opinions — can and often does invoke multiple frames. A single political argument can also provide new information at the same time that it shapes attitudes through framing (Leeper and Slothuus, 2015; Lenz, 2013).

Are Framing Effects Narrow or Broad?

By these definitions, framing effects seem likely to be narrow, as they only increase the availability of *relevant* considerations in people's minds. As Chong and Druckman (2011) explain, "even when they incorporate more than one dimension, cognitive limitations and economies of thought may cause most individuals to rely on no more than a few considerations." In the expectancy-value model, this means that the number of considerations with non-zero weights is likely to be small (Chong and Druckman, 2007). Still, the handful of studies which have examined spillover empirically have found limited evidence of it (Broockman and Kalla, 2016; Druckman *et al.*, 2012; Iyengar, 1991; Sniderman *et al.*, 2014).

Beyond those four studies, we should ask: how broad or narrow is the set of considerations activated by a given frame? As an example, consider again the free speech frame and attitudes toward the proposed KKK rally. A narrow framing effect might mean that the frame only influences a limited set of considerations, perhaps those related to the free speech rights of the KKK. By contrast, a broad framing effect might make many considerations accessible, including those related to other rights, to other types of groups, or to the partisan groups that typically make free speech arguments. In that case, we might expect framing effects on a variety of questions, some only distantly related to free speech or the KKK.

Prior framing studies almost always consider framing effects only on attitudes that are closely related to the subject of the frame. Table 1 in the Appendix summarizes the dependent variables for 12 prominent framing experiments.[2] The studies typically concern themselves exclusively with dependent

[2]These studies were identified by searching for studies which appeared in political science journals, reported a novel framing experiment, cited Iyengar and Kinder (1987), and have garnered over 200 citations (when published before 2010) or over 50 citations (when published after 2009).

variables on the same issue as the frame — and they also tend to emphasize issues that are not salient. Eleven of the twelve studies do not estimate spillover effects. Even the one study which explicitly discusses spillover effects, Druckman and Bolsen (2011), does so to eliminate a potential source of bias rather than to answer a question of interest. In this respect, these studies are representative of framing studies generally. In the very design of their survey instruments, the vast majority of framing studies ignore the possibility that framing effects might be broad.

But research in psychology on spreading activation suggests that spillover may indeed be occurring. For example, Nisbett and Wilson (1977, p. 243) describe an experiment where students memorized words and then named a laundry detergent. Those who were asked to memorize "ocean-moon" were more likely to name "Tide," likely because the words subconsciously activated associations that included both ocean tides and a detergent with the same name. In this view, the subset of considerations activated by a cue hinges on networks of mental associations that extend far beyond the domain of the original cue (Collins and Loftus, 1975). Recent research finds evidence that human cognition is to some extent metaphorical (Thibodeau and Boroditsky, 2011), meaning that external stimuli can activate cognitive schema that can then be applied to other questions.

Empirical evidence indicates that such metaphorical processes can influence political attitudes. For example, Winter (2008) concludes that race and gender provide metaphors that help Americans organize their attitudes on a variety of specific policy issues. In fact, the extensive research on racial priming finds that by activating racial schema, racially charged cues can shape attitudes far removed from the content of the cue (Valentino *et al.*, 2002). Notice that in this conception, framing effects are likely to extend well beyond the proximate domains mentioned in the frames themselves. Issues are no longer the central boundary delineating where framing effects are likely. Thus, prior evidence points in very different directions about the likely breadth of framing effects.

In investigating the breadth of framing effects, it is logical to ask about the attributes — either of frames or of their recipients — that might influence that breadth. Developing hypotheses on that question is beyond the scope of this manuscript.[3] Nonetheless, research on the role of metaphors in cognition suggests that the breadth of framing effects depends on the structural similarity between the frame and the target situation (Thibodeau and Boroditsky, 2011;

[3]For instance, we might inquire about the relationship between frames' strength and their breadth: do strong frames make a wider range of considerations accessible, and so have more breadth? It is also possible that other aspects of frames make them more susceptible to broad effects, including their metaphorical content (Thibodeau and Boroditsky, 2011), emotional impact (Gadarian, 2010), narrative coherence, cognitive simplicity, emphasis on losses or threats (Boydstun and Glazier, 2013), connection to social groups (Winter, 2008), or resonance with predispositions such as authoritarianism or ethnocentrism.

Winter, 2008). Here, we define the structural similarity between two political issues as the extent to which the arguments and frames typically invoked in the two domains overlap. By this definition, issues that are discussed using common language and metaphors (say, terrorism and criminal justice) are more closely related than those that are not. Accordingly, one possibility is that frames' breadth will depend on whether their structure is applicable to other policy questions as well as the issues on which they are invoked. A frame that focuses on specific factors about the KKK might be less broadly applicable than a frame that makes general, rights-based claims that apply to various situations.

Research Design and Methods

The overwhelming majority of framing experiments are designed in similar ways: they present respondents with randomly varied frames and assess how those frames influence attitudes on the same issue as the frame. But in this case, we want to compare the effects of multiple frames *on multiple outcomes* — and doing so places very specific requirements on our research design. First, we need respondents to be randomly assigned to arguments that vary in their issue content and frames. Beyond that, we need to ask about various political attitudes in a way that allows for the detection of spillovers. To meet these criteria, we conducted an online experiment in December 2011 through GfK's Knowledge Panel as part of TESS.[4] Our goal is to compare the treatment effects of various frames, so we included 3,318 respondents, a sample size substantially larger than in most framing experiments.[5]

Many recent framing experiments have been conducted on issues that are low in salience. But high-salience issues are more likely to be chronically accessible, and perhaps to facilitate framing effects which span multiple issues. Arguably, they represent especially credible tests of framing effects, as citizens are more likely to have encountered these arguments previously. We thus developed four political arguments with discernible frames on salient political issues: crime, terrorism, economic policy, and health care policy. Although the four arguments are distinctive, two convey similar frames about security while the remaining two convey frames about distrust in government. For simplicity's sake, all four arguments were written to be typical of conservative political figures and were attributed to an unnamed U.S. Senator. The experimental

[4]GfK's Knowledge Panel is recruited from the population of English-speaking U.S. adults through random-digit dialing and address-based sampling, allowing our sample to include a wider range of respondents than are available through other recruitment techniques (Mutz, 2011).

[5]Of the panelists, 5,281 were invited to complete the survey, yielding a completion rate of 63%.

design required most respondents to read two arguments, so the arguments were written so as to be broadly comparable without being alarmingly similar.

Our crime and terrorism arguments are both compatible with a single, generic frame about preventative action in the face of violence. For instance, the terrorism argument reads: "The September 11th attacks and the news that al-Qaeda was planning new attacks on U.S. soil show how vulnerable America still is to terrorists. Innocent people can be killed while traveling to visit family or going to work. Across the country, we have to do everything we can to reduce the threat of terrorism. We have to stop terrorists before they act. This means conducting more frequent searches of suspicious people boarding planes, trains, subways, and buses." The argument was constructed to be familiar, meaning that its primary effect should be to change how people use pre-existing considerations rather than to provide novel considerations. To be sure, identifying the precise boundaries of a given frame is an uncertain enterprise, as what may be a single frame to one observer (e.g., security in the face of threat) might be two or more to another (e.g., the value of pre-emptive action, the randomness of threat, the danger of transportation, etc.). Still, one core frame embedded in this argument highlights the importance of sacrificing some measure of individual liberty in the name of collective security. Our argument about criminal justice invoked a structurally similar frame even while it pointed to different facts and policy solutions, and is provided in the Appendix. These two issues have other important similarities that may increase spillover, including that both address physical threats and can tap ethno-racial distinctions between groups.

The other issue pairing is economics and health care, with the full text of those arguments provided in the Appendix as well. Although on distinctive issues, these two arguments convey similar distrustful frames about the federal government's inability to effectively intervene in complex domains. Like the terrorism–criminal justice pairing, they were written to have a common structure, as both lay out the complexity of the problem before arguing that the federal government is incapable of effectively providing a solution. Each closes with a rhetorical question designed to tap Americans' deep distrust of the government's capacity to act effectively. All four arguments were written to be approximately the same length and reading level, and to be read in any combination.

Outcomes and Experimental Design

Here, we are interested not only in the effects of each argument on directly related dependent variables, but also on dependent variables on separate but proximate issues (e.g., anti-crime attitudes after the terrorism argument) and on dependent variables that are quite distant from the frame's domain (e.g.,

health spending attitudes after the terrorism argument). This assessment requires a research design with several unique attributes. First, to compare the impact of arguments and frames across issues, it is critical to ask questions across policy domains that have comparable structure and response categories. We thus asked variants of the government spending questions in the 2008 National Election Study.[6] Four of the included issues are obvious: preferred spending levels on terrorism, crime, economic stimulus, and health care. We also asked about anti-poverty spending, an issue that is thought to be central to Americans' partisanship and political beliefs. We rescale the spending items to vary from 0 to 100 and summarize them in Appendix Table 2. The directional predictions depend on the argument in question, so each variable is coded in the direction of the frame.[7] For instance, 100 indicates people who want anti-crime spending to increase a great deal and health care spending to decrease a great deal.[8]

Figure 1 illustrates the experimental design. All respondents were asked for their attitudes on the five spending questions in a randomized order. Separately, we randomly assigned 25% of respondents to a control group which saw no arguments and simply reported their attitudes on the five spending items. The remaining 75% of respondents were randomly assigned to read one of the four arguments and then were asked an open-ended question, "do you think the Senator is making a convincing argument?" This question enables us to assess the strength of each argument while also encouraging respondents to evaluate the argument and develop stronger attitudes in response (see also Druckman and Leeper, 2012). Respondents then evaluated two of the five spending items before being exposed to a second randomly assigned argument, answering a second question about how convincing the latest argument was, and then evaluating the remaining three spending items.

For all estimated treatment effects, the control group is the same: the roughly 25% of respondents who were exposed to no arguments and answered

[6]Specifically, the questions asked: "Should federal spending . . . be increased, decreased, or kept the same?", with the missing section filled in by "dealing with crime," "on the war on terrorism," "on aid to the poor," "on health care," and "to stimulate the economy." The seven response options include "increased a great deal," "increased a moderate amount," "increased a little," "kept about the same," "decreased a little," "decreased a moderate amount," and "decreased a lot."

[7]We did not present an argument that dealt directly with anti-poverty spending, but because all arguments espoused a conservative point of view, anti-poverty responses were coded so that a score of 7 indicated a preference to decrease spending "a lot."

[8]On balance, our control group shows the highest levels of support for increased anti-crime spending (mean = 56), with lower levels of support for increased anti-terror spending (44), decreased anti-poverty spending (44), decreased health care spending (42), or decreased stimulus spending (48). The standard deviations are lower for anti-crime and anti-terror spending (at 21 and 24, respectively), and higher for the more politicized issues of health care (29) and stimulus spending (33).

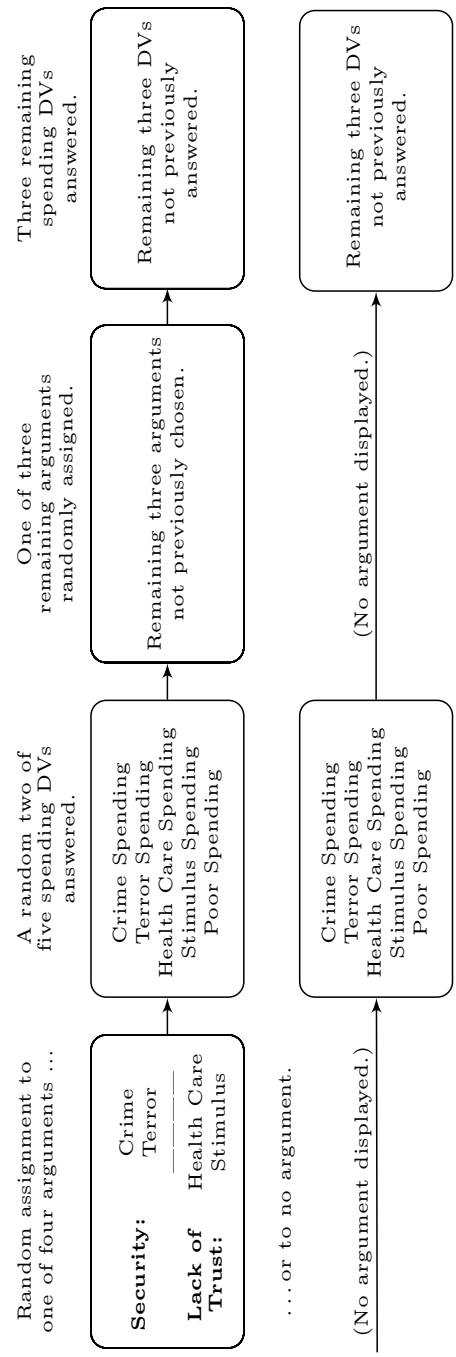

Figure 1: Experimental design.

the five spending items in a randomized order.[9] The treatment groups vary by the specific argument–attitude pairing. For any given attitude and argument, we consider the treatment group to include all respondents who reported the relevant attitude after exposure to the argument in question.[10] On average, 614 respondents were asked the relevant spending question after exposure to an argument in the first position, and another 369 respondents were asked the relevant spending question after exposure to an argument in the second position. That yields an average treated population for a given argument–spending item pairing of 983 respondents.

Results

Are framing effects limited to the specific issue mentioned in the argument, or can they spill over to structurally similar issues given an applicable frame? To answer that question, this section presents the effects of the frames on each spending attitude. The arguments vary slightly in their strength, so Appendix Table 3 presents the percentage of respondents who find each frame convincing as well as each frame's impact on same-issue spending attitudes.[11]

For any given frame–attitude pairing, the treatment group consists of any respondent who saw the frame in question before providing the attitude in question, while the control group consists of the roughly 25% of respondents who read no frames. Accordingly, for each pairing of a frame and an attitude, the treatment group will consist of a different subset of the 75% of respondents who were exposed to frames. One way to estimate the 20 treatment effects of interest — for each of the four frames and five spending attitudes — would be to identify the treated respondents in each case and then use a straightforward estimation technique such as separate linear regressions. Instead, we estimate highly similar quantities of interest simultaneously from a single OLS model. To do so, we first stack our data set such that the unit of observation is the respondent–attitude–position triplet, which accounts for the fact that 75% of respondents were exposed to frames in one of the two positions in the questionnaire. To help readers understand our data set, the Appendix provides an image of the first 20 rows of data used in our models. Although we stack

[9]This definition of the control group is the cleanest, as it avoids including respondents exposed to other arguments.

[10]Suppose, for example, that we are interested in the effects of the health care argument. If a respondent was exposed to the health care argument first, we consider her responses to any of the five dependent variables as treated. However, if a respondent was exposed to the health care argument second, we only include her as treated if she answered the relevant dependent variable *after exposure to the argument.*

[11]While the distinction between the overall political arguments and the frames they convey is an important one, our empirical discussion refers to the treatments at times as arguments and at times as frames.

the data to facilitate estimation in a single model, every estimated treatment effect involves exclusively cross-individual comparisons.

Specifically, we estimate the effects of each frame on each outcome simultaneously using a linear model with multiplicative interactions and respondent-clustered standard errors:

$$score = \alpha + \sum_{j=1}^{4} \Psi_j Frame_j + \sum_{k=1}^{4} \Omega_k Attitude_k + \sum_{j=1}^{4}\sum_{k=1}^{4} \Phi_{j,k} Frame_j Attitude_k + \mathbf{Z}\gamma + \epsilon.$$

Here, *score* is a column vector containing the responses to whichever spending outcome was asked for a given observation, α is an intercept, $Frame_j$ is an indicator denoting which frame is serving as the treatment for a given observation (with those who saw no frame as the reference category), $Attitude_k$ is an indicator denoting which of the k spending outcomes was asked (with one outcome omitted as a reference category), $\mathbf{Z}$ is a matrix of indicators denoting levels of party ID for each respondent, ϵ is a respondent-clustered error term, and Ψ, Ω, Φ, and γ are vectors of coefficients to be estimated. The *Frame* and *Attitude* vectors serve to isolate the observations with the frame and attitude of interest. Given this model, the effect of frame j on the omitted spending outcome relative to the control condition is Ψ_j. The effect of the same frame on some other outcome, $Attitude_k$, is $\Psi_j + \Phi_{j,k}$, while the difference between the two effects is $\Phi_{j,k}$. In other words, $\Phi_{j,k}$ is the differential effect of frame j on outcome k relative to its effect on the baseline outcome.[12]

Our initial estimand is the effect of each frame on each of the issue-specific spending attitudes. We present the coefficients from the OLS model in Table 1 and illustrate the treatment effects of interest in Figure 2, with the mean magnitude of each effect presented using a gray bar. The thicker vertical black lines illustrate one standard error above and below the point estimate, while the thin black lines present 95% confidence intervals. Under each gray bar, we provide the two-sided p-value associated with the null hypothesis that reading the argument had no effect on attitudes as well as the respondent-clustered standard error. Due to the recoding of the dependent variables to vary from

[12]The research design and estimation strategy outlined before the experiment's implementation aimed to maximize statistical power, and so included as treated all respondents who were exposed to an argument and then answered the relevant spending question, even if there was a second, intervening frame read between treatment and response. As a robustness check, we re-analyzed the data including as treated only those respondents who answered the relevant spending question before exposure to a second frame. The results, presented in Figure 2 in the Appendix, are quite similar. So, too, are the results when we restrict all respondents — treatment and control — to have been asked the relevant dependent variable prior to the second frame, as Appendix Figure 3 shows.

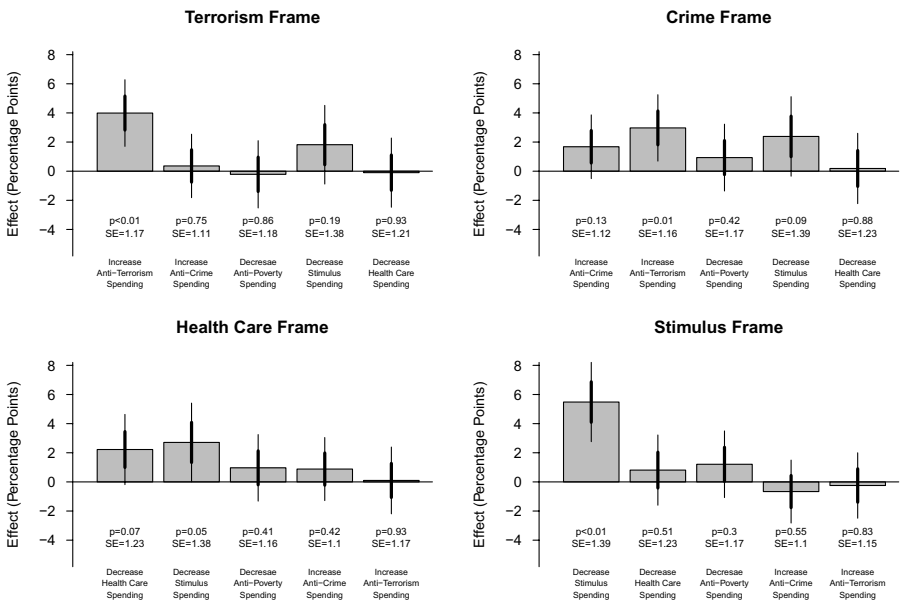

Figure 2: Effects of four arguments on spending preferences.

This figure illustrates the effect of each frame/argument on each of the five spending areas as compared to an 861-person control group that was not exposed to any arguments. The treatment groups vary slightly in size, but averages 983 respondents. The gray bars indicate mean effects on spending scales ranging from 0 to 100, while the vertical lines indicate 95% confidence intervals. Under each bar, we report the corresponding two-sided p-values and standard errors. The experiment was conducted using the GfK Knowledge Panel in December 2011.

0 to 100, the treatment effects can be interpreted as percentage-point shifts. To allow for an initial assessment of how the effects vary as the fit between the argument and the attitude changes, we arrange the results such that the pairings with the closest fit are found on the left side of each panel.

Figure 2 depicts the effects of the four frames on each of the five spending attitudes. In general, a staircase pattern descending from left to right is indicative of framing effects which decline as the target attitudes become more distant. The results generally conform to that pattern, and so provide clear evidence that frames are typically more influential on closely related spending attitudes. For instance, the top-left panel shows that the anti-terrorism argument has a sizable effect of 3.99 percentage points on anti-terror spending ($p < 0.01$), an effect which is 16% of the dependent variable's standard deviation. However, the anti-terrorism argument has no especially strong effects on other spending attitudes.

Table 1: This table presents the results of an OLS model with respondent-clustered standard errors fit to the full data set of 23,601 respondent–outcome pairings. The dependent variable is spending attitudes, and the baseline category is anti-crime spending attitudes.

	Coefficient (SE)
Intercept	50.46 (1.95)
Ψ	
Crime Frame	1.68 (1.12)
Health Frame	0.88 (1.10)
Stimulus Frame	−0.66 (1.10)
Terrorism Frame	0.36 (1.11)
Ω	
Outcome = Anti-terror Spending	−12.48 (0.88)
Outcome = Anti-Poverty Spending	−11.82 (1.30)
Outcome = Health Care Spending	−14.23 (1.40)
Outcome = Stimulus Spending Spending	−7.91 (1.45)
Φ	
Crime Frame, Terror Spending	1.29 (1.27)
Health Frame, Terror Spending	−0.78 (1.26)
Stimulus Frame, Terror Spending	0.42 (1.25)
Terrorism Frame, Terror Spending	3.63 (1.25)
Crime Frame, Anti-poverty Spending	−0.74 (1.81)
Health Frame, Anti-poverty Spending	0.08 (1.80)
Stimulus Frame, Anti-poverty Spending	1.87 (1.81)
Terrorism Frame, Anti-poverty Spending	−0.57 (1.84)
Crime Frame, Health Care Spending	−1.49 (1.91)
Health Frame, Health Care Spending	1.34 (1.89)
Stimulus Frame, Health Care Spending	1.47 (1.91)
Terrorism Frame, Health Care Spending	−0.46 (1.89)
Crime Frame, Stimulus Spending	0.71 (2.01)
Health Frame, Stimulus Spending	1.83 (1.97)
Stimulus Frame, Stimulus Spending	6.15 (1.99)
Terrorism Frame, Stimulus Spending	1.46 (1.99)
γ	
Strong Republican	18.30 (1.93)
Weak Republican	11.23 (1.98)
Lean Republican	12.77 (1.92)
Lean Democratic	−0.48 (1.91)
Weak Democrat	0.70 (1.92)
Strong Democrat	−5.33 (1.91)

When assessing the results, it is important to remember that some spending areas are generally more responsive to arguments than others. For example, anti-crime attitudes are relatively stable, and show the smallest effect of the same-issue frame of any outcome (see Appendix Table 3). Given that,

it is not surprising that the crime frame's largest effect is actually on anti-terrorism attitudes (2.97, $p = 0.01$), which are more malleable and address a structurally similar issue. Even so, the effect on anti-crime attitudes is roughly similar in magnitude (1.68, $p = 0.13$) and statistically indistinguishable from zero. The crime frame does appear to have spillover effects on anti-terrorism attitudes, and possibly on attitudes toward stimulus spending as well (2.39, $p = 0.09$).[13] Notice, however, that the crime frame has little impact on anti-poverty attitudes or health care attitudes.

Similarly, health care attitudes are heavily politicized, and attitudes on health care spending are accordingly less treatment responsive. Still, we find that the health care frame has similar effects on health care spending attitudes (2.22, $p = 0.07$) and stimulus spending attitudes (2.71, $p = 0.05$), with markedly smaller effects on the other three spending attitudes in question. This pattern makes sense, as health care was framed using language that could easily be applied to the issue of stimulating the economy. And for the anti-stimulus frame, the results are unambiguous: the stimulus-related frame has a far stronger impact on stimulus-related attitudes (5.5, $p < 0.01$) than on any other spending attitudes. In fact, this frame–attitude pairing produces the largest treatment effect of any we observe.[14]

Overall, these results consistently indicate that spillover is not broad. In two cases, the frame's strongest effect was on the attitude in the same domain; in the other two cases, it was on the structurally similar issue. What's more, Appendix Figure 1 presents the same results grouped by attitude and demonstrates that every attitude is most strongly influenced by the same-issue frame. These results also suggest that these findings are indeed framing effects, and not partisan priming. If hearing an argument associated with a party primes partisanship, we should expect it to influence a wide range of salient, partisan issues such as health care or stimulus spending. As in Boudreau and MacKenzie (2014), citizens respond to the specific content of the arguments and not simply to any party-related cues.

[13] Attitudes about stimulus spending appear responsive to a broad set of arguments, perhaps because stimulus spending attitudes are relatively closely integrated with partisanship: of the five spending categories, its correlation with a seven-category measure of partisanship is the highest (polychoric correlation = −0.46).

[14] As the figure illustrates, none of the arguments have a meaningful effect on anti-poverty attitudes, with even the largest effect (stimulus spending) inducing only a 1.2 percentage-point change ($p = 0.3$). Like issues of criminal justice and terrorism, poverty has been a racially charged issue in the U.S. since at least the 1960s, so the small effects identified here suggest that the terrorism and criminal justice arguments are not priming race-related considerations to such a substantial degree as to generate spillover effects.

Measuring Spillover via Differences-in-Differences

Estimating the effect of each argument on each attitude is a stopover in pursuit of our central goal: estimating whether these treatment effects are stronger when there is a close fit between the argument and the attitude. We are interested, for example, in whether the terrorism-related argument has a larger effect on anti-terrorism spending attitudes than on more distant issues such as crime and health care. Figure 3 formally tests these possibilities by presenting difference-in-difference estimates of the effects of each argument on the spending attitude of interest when compared to a baseline spending attitude.

Specifically, we begin with the estimated treatment effects of each argument on each spending area from Table 1 and Figure 2. We use each frame's effect on same-issue attitudes as our baseline. We then estimate the effect of the frame on the other issues, and subtract the within-issue effect to calculate each frame's differential effect. Put differently, we use a difference-in-difference estimator to identify whether frames have consistently larger effects on proximate issues.[15] Formally, in each panel of Figure 3, each bar denotes an estimate of $(E[Y_d|frame_j] - E[Y_d|control]) - (E[Y_s|frame_j] - E[Y_s|control])$, where Y denotes a vector of responses to a spending question. The subscripts s and d — for "same" and "different" — denote spending items which do or do not match the frame.[16]

Repeatedly in Figure 3, we see negative differential treatment effects indicating that framing effects are strongest when the frame's issue content matches the attitude in question. In each panel, the gray bar illustrates the average difference-in-difference estimate, while thick vertical lines depict standard errors and thin lines depict 95% confidence intervals. Beneath each bar, we provide the corresponding two-sided p-value and standard error. Consider the top-left panel. At left, we see an estimate of -3.63, indicating that the anti-terrorism frame has a markedly stronger impact on anti-terrorism attitudes than on anti-crime attitudes ($p = 0.004$). In the panel's other three bars, we see that roughly the same pattern holds for attitudes on stimulus spending, health spending, or anti-poverty spending. These results demonstrate that the anti-terrorism frame's impacts are concentrated on anti-terror spending — and thus that framing effects are narrow.

[15] This estimator requires us to make directional predictions about the effects of arguments on distant attitudes, and to do so, we assume that conservative arguments in one domain are more likely to produce conservative effects in others. However, as Figure 2 above illustrated, the effects of the frames on distant dependent variables are consistently small, making our results invariant to this assumption.

[16] Y_s is the vector of spending responses on the issue listed in the title of each panel, while Y_d is the vector of responses for any of the other spending attitudes. Note that our models again control for partisan identification, specified via six indicator variables.

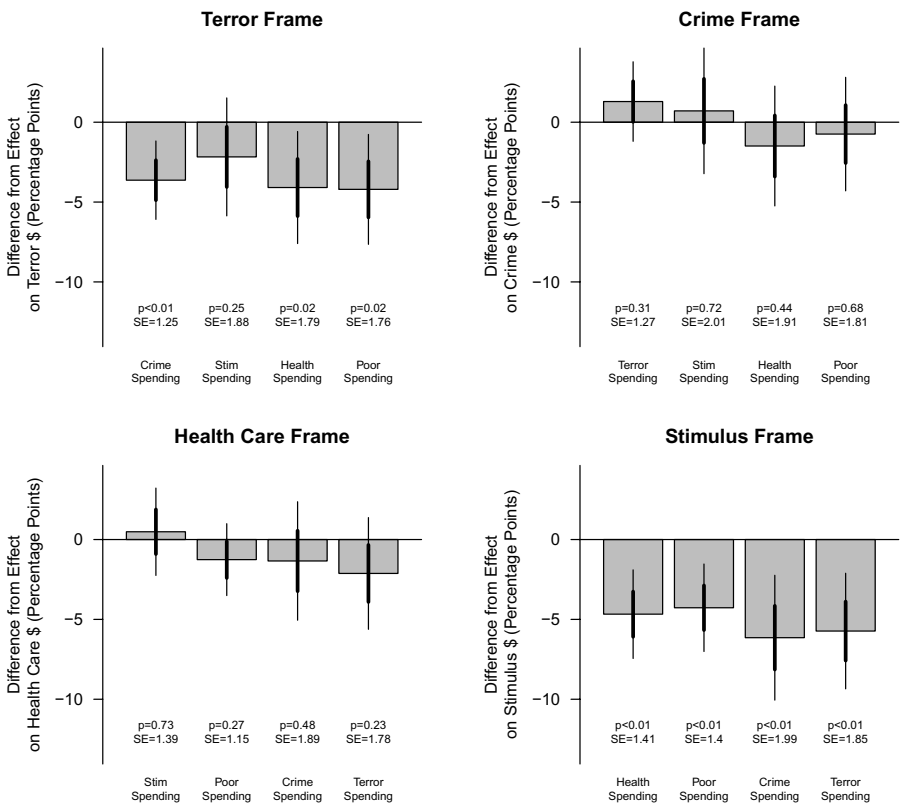

Figure 3: Relative effects of arguments on spending preferences.

This figure illustrates difference-in-difference estimates comparing the effects of frames on the spending attitude listed at bottom with the baseline spending attitude listed on the y-axis. The bars indicate the mean estimated difference between the two framing effects, while the thick lines present standard errors and the thin lines present 95% confidence intervals. Under each bar, we report the corresponding two-sided p-values and standard errors.

The evidence for the anti-crime frame is more ambiguous, as it has roughly similar effects on anti-terror spending and stimulus spending. In fact, as we see in the top-right panel of Figure 3, the anti-crime frame's effects are statistically indistinguishable across outcomes. But the strongest differential effect of the anti-crime frame is on the related issue of terrorism spending, again suggesting that framing effects are narrow. With respect to the health care frame, most of the effects are negative, indicating that the health care frame has a stronger impact on health care spending attitudes than on attitudes on anti-poverty

spending (−1.26), anti-crime spending (−1.34), or anti-terrorism spending (−2.12).

As the bottom-right panel of Figure 3 illustrates, the stimulus frame shows very clear evidence of limited spillover. All of the differential effects are negative, meaning that the stimulus frame has a stronger impact on stimulus spending attitudes than on any others. In fact, the effect of the anti-stimulus argument on attitudes toward stimulus spending is 5.7 percentage points larger than the same argument's effect on anti-terror spending ($p < 0.01$). Again, this is indicative of limited spillover effects.

To this point, we have presented the results separately by argument, although each difference-in-difference estimate tests the same hypotheses about framing effects' spillover. In Appendix Figure 4, we present the results when we pool the various frame–attitude pairings based on whether they are direct, close, or distant. Those results reinforce our core conclusion by showing stronger effects when frames are more closely related to the attitudes in question.

Conclusion

Framing is so central to contemporary politicians' toolkits that in 2000, a state legislator named Barack Obama argued for his Congressional candidacy by saying that he was positioned to "best articulate and frame the issues that are most important to voters in the district" (Frontline, 2009). Still, despite the sustained attention to framing from politicians and scholars alike, there are many outstanding questions about the limits of and mechanisms behind framing effects. One has occupied us here: does a frame embedded within an argument on a particular issue have the capacity to shape attitudes on other issues, especially those that share common elements or structure with the frame? If so, we should expect that salient frames on a particular issue will have far-reaching consequences — and that public opinion on specific issues will hinge on how other, contemporaneous issues are being discussed.

Research on the role of metaphors in cognition suggests that framing might well influence a broad range of attitudes, as metaphorical thinking facilitates connections across disparate issues (e.g., Thibodeau and Boroditsky, 2011). Research on the pervasiveness of partisanship in contemporary public opinion leads to the same expectation, albeit for different reasons. The population-based survey experiment reported here was designed to test these hypotheses. It coupled an unusually large sample size ($\approx$ 3,300) with an assessment of five issue attitudes using the same scale. And to be sure, the experiment does find some evidence in keeping with these hypotheses of spillover. The crime-related argument and corresponding security-oriented frame influenced attitudes on anti-terror spending. At the same time, the health care argument

and its frame of distrust in government shaped attitudes on stimulus spending. But the central finding is that framing effects are narrow, meaning that they are largely confined to direct or proximate issues. Two of the frames had their strongest effects on the within-issue attitudes; the other two had their strongest effects on proximate issues. Moreover, all of the spending attitudes were most influenced by frames on the same political issue. Such results suggest that our respondents do have issue-specific considerations that are made accessible by the frames, and that the frames did more than simply cue partisan considerations.

As framing experiments have proliferated, so too have concerns about whether the experiments are actually isolating framing (Huber and Paris, 2013; Leeper and Slothuus, 2015; Scheufele and Iyengar, 2012). In many framing experiments, the issue frames that are experimentally manipulated differ in multiple ways from one another, making it difficult to distinguish framing from persuasion or information effects. This experiment identifies a potential way forward: by asking about a variety of attitudes after exposure to the frame, researchers can assess the breadth of framing effects — and in doing so, they can better identify whether issue framing is the likely mechanism at work. It also indicates the importance of choosing appropriate dependent variables when designing an issue framing experiment, including dependent variables that help rule out alternative explanations. In some instances, these results might justify embedding multiple survey experiments in the same survey, as a manipulation on one topic is unlikely to influence attitudes on distant issues.

To an important extent, elite-level American politics today is organized around political issues. Journalists, interest groups, and politicians alike specialize in particular political issues, and many campaign advertisements and websites detail candidates' positions on the issues. One strategic choice facing political campaigns is their choice of a focal issue. Still, the extent to which citizens' minds are organized around discrete political issues has been a source of ongoing debate (Ahler and Broockman, 2015; Ellis and Stimson, 2012). This framing experiment adds to those debates by showing that American adults do respond to frames in issue-specific ways. Consistently, frames prove to be most potent when they are embedded in arguments that address an issue proximate to the target attitude. That holds true even in the case of criminal justice and terrorism, where the particular arguments employed in our experiment make no explicit mention of government spending, our outcome of interest.

This framing experiment also suggests a novel approach to measuring the structure of mass political attitudes. Rather than exclusively examining political attitudes for strong pairwise correlations, we might also examine them for common influences. Those features of frames which are able to influence a wider range of attitudes are more likely to be central in how people's attitudes are structured.

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Online Appendix: Assessing the Breadth of Framing Effects

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December 14th, 2016

Appendix: Prior Framing Experiments

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Table 1: This table presents a summary of dependent variables for select, prominent framing studies. It also indicates whether each study explicitly estimates spillover effects on more distant issues.

	Issues Covered by Dependent Variables	Estimates Spillover?
1. Nelson, Clawson, and Oxley (1997)	KKK rally	No
2. Nelson, Oxley, and Clawson (1997)	Welfare policy	No
3. Nelson and Oxley (1999)	Land management Welfare reform	No
4. Haider-Markel and Joslyn (2001)	Concealed handguns Columbine	No
5. Druckman (2001)	Spending on poor KKK rally	No
6. Druckman and Nelson (2003)	Campaign finance	No
7. Druckman (2004)	Community grants Disease prevention Employment policy Youth crime	No
8. Chong and Druckman (2007)	Urban growth Hate group rally	No
9. Slothuus and De Vreese (2010)	Welfare reform Trade policy	No
10. Chong and Druckman (2010)	Patriot Act Urban growth	No
11. Druckman and Bolsen (2011)	Carbon nanotubes Genetically modified food	Yes
12. Arceneaux (2012)	Disease prevention HS gay-straight club	No

Arguments

Security Frame

- The September 11th attacks and the news that al-Qaeda was planning new attacks on U.S. soil show how vulnerable America still is to terrorists. Innocent people can be killed while traveling to visit family or going to work. Across the country, we have to do everything we can to reduce the threat of terrorism. We have to stop terrorists before they act. This means conducting more frequent searches of suspicious people boarding planes, trains, subways, and buses.

- As the recent killings in Arizona remind us, America is very vulnerable to violent crime. Innocent people can be killed in their front yards. Across the country, we have to do everything we can to reduce the threat of violent crime. We have to stop violent criminals before they act. This means cracking down on the smaller offenses that all too often lead to violent crime, and making sure that convicted criminals always serve out their full sentences.

Lack of Trust Frame

- With a recession as deep as this one, there are more than 10 million unemployed Americans, and it's going to take years for our economy to recover. In February 2009, the government in Washington made things worse by passing an $800 billion stimulus package, which is more than $2,500 for every person living in this country. Now, it looks like a lot of that money didn't help the economy. Unemployment is still very high. The money went to pork-barrel projects and federal bureaucrats rather than creating jobs for unemployed Americans. The government in Washington can't even balance its own budget. How can we trust it to spend so much taxpayer money?

- Health care is one of the most complicated issues we face. It involves 1 of every 6 dollars spent here in the United States. The health care system includes millions of doctors and nurses and thousands of hospitals and clinics. Together, they regularly make decisions that can mean life or death. The government in Washington can't even balance its own budget. How can we trust it to run something as complicated as the health care system?

Data Set Structure

Here are the first 20 rows of the data. Each respondent-DV-round gets a row. The "treat" column displays the frame that is serving as the treatment in a given row. "Round" refers to whether we are considering the first frame or the second frame as the treatment. The outcome score is displayed in the "score" column. Also, if the DV was asked prior to the frame in that row being shown, (as conveyed by the "q.ord" variables), "score" gets an NA.

	CaseID	frame	q1.ord	q2.ord	q3.ord	q4.ord	q5.ord	DV	score	treat	round
1	3	crime	terror	health	stim	crime	poor	CRMSPND	50.00000	crime	1
3319	3	crime	terror	health	stim	crime	poor	TRRSPND	16.66667	crime	1
6637	3	crime	terror	health	stim	crime	poor	POORSPND	50.00000	crime	1
9955	3	crime	terror	health	stim	crime	poor	HLTHSPND	33.33333	crime	1
13273	3	crime	terror	health	stim	crime	poor	STIMSPND	50.00000	crime	1
16591	3	stim	terror	health	stim	crime	poor	CRMSPND	50.00000	stim	2
33191	3	stim	terror	health	stim	crime	poor	TRRSPND	16.66667	<NA>	2

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66371       3         stim terror health   stim  crime   poor POORSPND 50.00000         stim     2
99551       3         stim terror health   stim  crime   poor HLTHSPND 33.33333         <NA>     2
132731      3         stim terror health   stim  crime   poor STIMSPND 50.00000         stim     2
2           4 NOT ASSIGNED  crime terror health   stim   poor  CRMSPND 83.33333 NOT ASSIGNED     1
3320        4 NOT ASSIGNED  crime terror health   stim   poor  TRRSPND 33.33333 NOT ASSIGNED     1
6638        4 NOT ASSIGNED  crime terror health   stim   poor POORSPND 50.00000 NOT ASSIGNED     1
9956        4 NOT ASSIGNED  crime terror health   stim   poor HLTHSPND 33.33333 NOT ASSIGNED     1
13274       4 NOT ASSIGNED  crime terror health   stim   poor STIMSPND 33.33333 NOT ASSIGNED     1
21000       4 NOT ASSIGNED  crime terror health   stim   poor  CRMSPND 83.33333 NOT ASSIGNED     2
33201       4 NOT ASSIGNED  crime terror health   stim   poor  TRRSPND 33.33333 NOT ASSIGNED     2
66381       4 NOT ASSIGNED  crime terror health   stim   poor POORSPND 50.00000 NOT ASSIGNED     2
99561       4 NOT ASSIGNED  crime terror health   stim   poor HLTHSPND 33.33333 NOT ASSIGNED     2
132741      4 NOT ASSIGNED  crime terror health   stim   poor STIMSPND 33.33333 NOT ASSIGNED     2
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Additional Results

Table 2: This table summarizes the five spending variables for the 861 respondents assigned to the control group. Each variable ranges from 0 to 100 and is coded such that 100 is in the direction of the corresponding argument.

	Mean	SD
Increase Anti-Crime Spending	56.00	20.72
Increase Anti-Terror Spending	43.52	24.44
Decrease Anti-Poverty Spending	44.13	27.30
Decrease Health Care Spending	41.75	29.34
Decrease Stimulus Spending	48.10	32.60

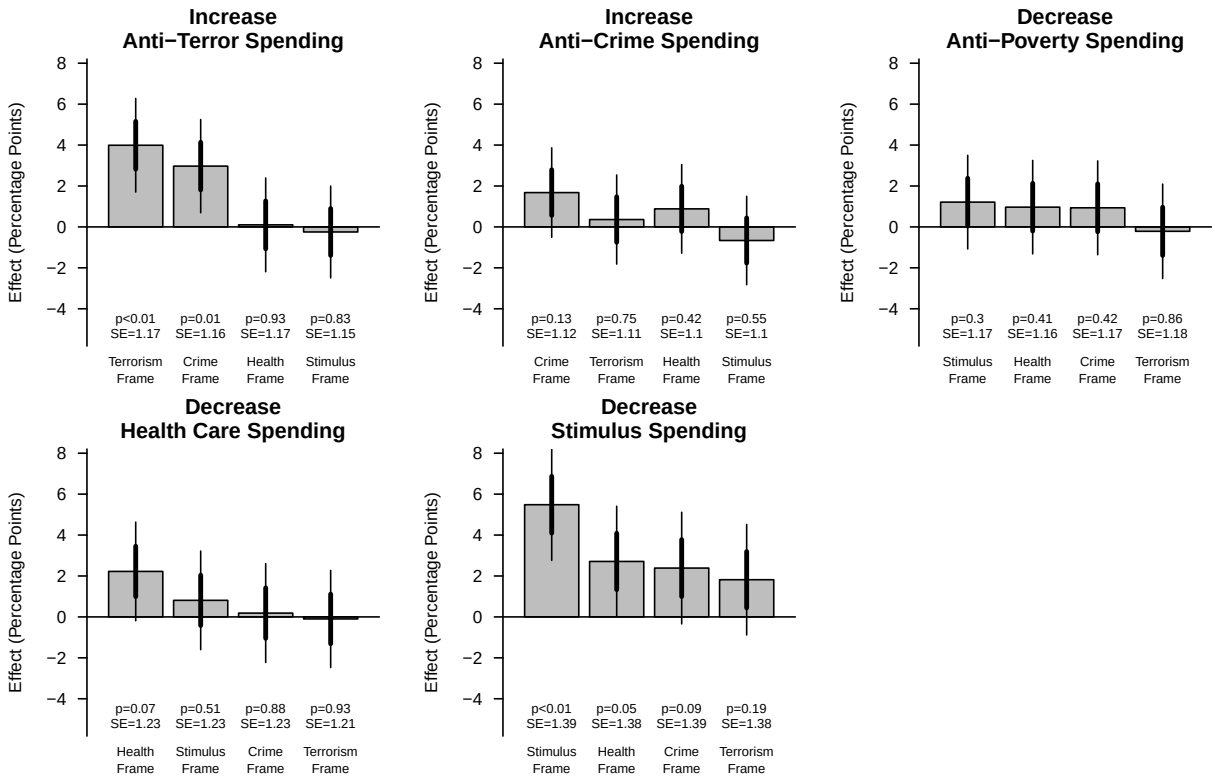
Assessing the Arguments' Strength

One important question is about the arguments' relative strength, as it is possible that stronger arguments will have broader effects. Accordingly, Table 3 presents the share of all respondents who deemed the first argument "convincing" in response to the open-ended question. As the table makes clear, the four arguments are all perceived as convincing by a majority of respondents, although the crime argument appears to be the weakest, with just a slim majority saying that it is convincing. As a point of comparison, in the table's third column, we present the estimated effects of each argument on spending preferences on that issue, effects whose estimation is described below. As the table makes clear, there is not a strong relationship between the arguments respondents found convincing and those that moved attitudes. For one thing, the health care argument is seen as very convincing, yet the attitudinal shift it induces is comparatively small, perhaps because of the entrenched partisan divisions on health care. Still, each of the four arguments worked in the sense that it moved issue preferences in the expected direction.

Table 3: This table reports the share of respondents who were exposed to each argument first who found that frame to be convincing. It also indicates the effect of each argument on spending preferences on that issue controlling for party ID, with spending preferences coded to vary from 0 to 100.

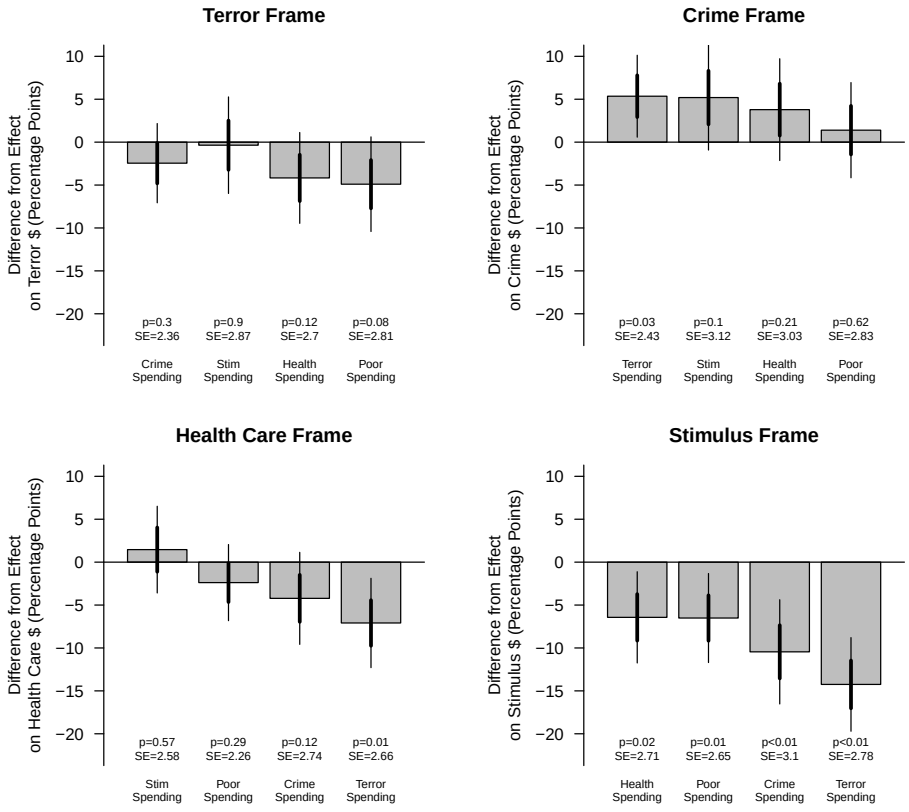
	% Convincing	Within-Issue Effect
Crime Argument	51.3	1.68
Terrorism Argument	59.7	3.99
Stimulus Argument	66.7	5.48
Health Argument	66.9	2.22

Figure 1: *Effects of four arguments on spending preferences.*



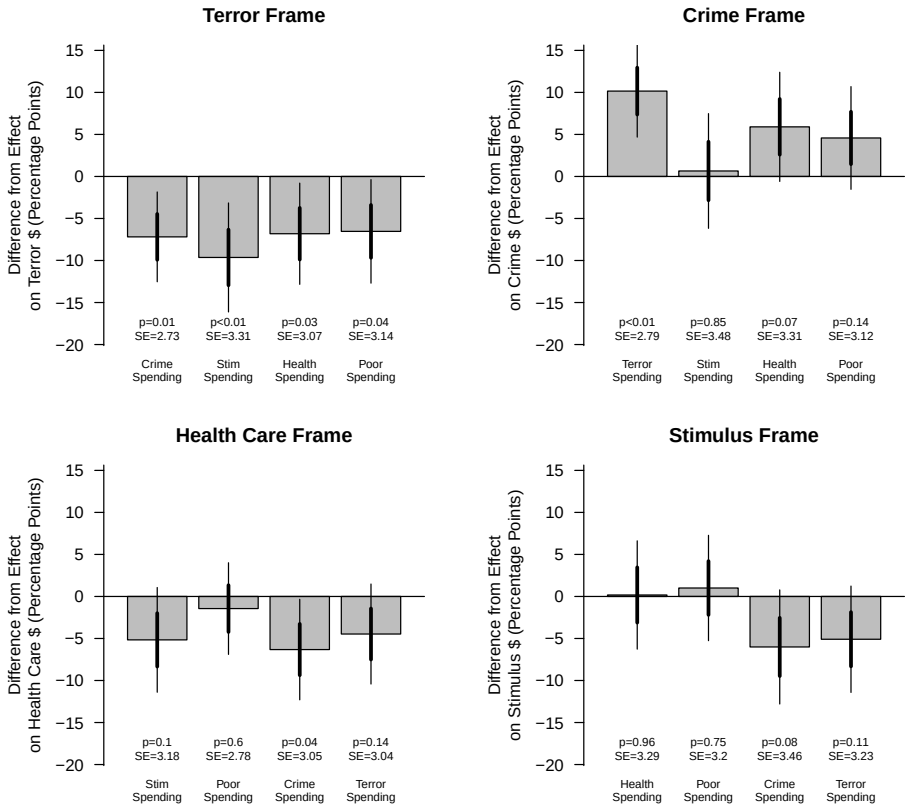
This figure illustrates the effect of each frame/argument on each of the five spending areas as compared to an 861-person control group that was not exposed to any arguments. The treatment groups vary slightly in size, but average 983 respondents. The gray bars indicate mean effects on spending scales ranging from 0 to 100, while the vertical lines indicate 95% confidence intervals. Under each bar, we report the corresponding two-sided p-values and standard errors.

Figure 2: *Relative effects of frames on attitudes reported immediately following exposure.*



This figure reports the difference-in-difference results when we narrow the definition of "treated" to include only respondents who saw the frame in question first and then assessed the relevant spending question without reading any intervening frames. All observations from those assigned to see no argument are retained. The gray bars indicate the mean difference-in-difference estimate. The corresponding two-sided p-values and standard errors are listed below each estimate. The thick vertical lines indicate standard errors, while the thin lines indicate 95% confidence intervals.

Figure 3: *Relative effects of frames on attitudes reported immediately following exposure.*



This figure reports the results when we narrow the definition of "treated" to include only respondents who saw the frame in question first and answered a given spending question in the first or second out of five positions. Unlike the models used to estimate Figure 2, responses from the third, fourth and fifth positions were also excluded for those assigned to control. The gray bars indicate the mean difference-in-difference estimate. The corresponding two-sided p-values and standard errors are listed below each estimate. The thick vertical lines indicate standard errors, while the thin lines indicate 95% confidence intervals.

Pooled Results

The results in the main manuscript provide an issue-by-issue look at frames' capacity to influence attitudes at varying degrees of distance from the frame's content. Here, we conduct tests which pool across the different issues to provide an overall assessment of the breadth of framing effects. To do so, we must first categorize each argument-attitude pairing based on the fit or distance between them. We then pool across the relevant pairings, exploiting the fact that all of the spending preferences were measured on the same scale.

Specifically, we estimate three quantities of interest, all of which are differences in treatment effects across the experimental conditions. The first compares the effect of arguments on directly related outcomes (e.g. the terrorism argument and anti-terror spending) with the effect of arguments on distant or seemingly unrelated outcomes (e.g. the terrorism argument and health care spending). The second estimates the effect of arguments on directly related outcomes versus the effect of arguments on proximate outcomes for which the frame still applies even though the issue differs (e.g. the terrorism argument and anti-crime spending). The third comparison is between the effect of arguments on a proximate outcome and the effect of arguments on the two most distant outcomes jointly (e.g. the terror argument and health care/ stimulus spending).[1] Table 4 displays the various definitions of distance used for these tests. Given the content and structure of the frames, terrorism and crime are considered proximate issues, as are health care and economic stimulus.

Table 4: This table presents the classification of argument-spending attitude pairings for the pooled tests. The rows denote the argument in question while the columns indicate the distance between the frame and the outcome of interest.

Argument	Direct	Proximate	Distant (a)	Distant (b)
Terror	Anti-terror	Crime	Stimulus	Health Care
Crime	Crime	Anti-terror	Stimulus	Health Care
Health Care	Health care	Stimulus	Anti-terror	Crime
Stimulus	Stimulus	Health Care	Anti-terror	Crime

Our three quantities of interest are differences in treatment effects (i.e. differences-in-differences). To estimate each of these quantities, we generated indicator variables in our long-form data set (in which each row represents a respondent-argument-outcome triad), for whether the conditions above were met. For example, to compare the effect of direct frames to distant ones, we generated a "direct" indicator that took a 1 if the argument being offered directly related to the spending outcome being measured (e.g. a crime argument and crime spending) and a zero if no argument was offered and the same spending outcomes were measured. We employed the analogous coding for the "distant" indicator. Spending outcomes which did not fit the definition of either pooled conditions (e.g. anti-poverty spending) were omitted. We then estimated the following least squares model with respondent-clustered standard errors:

[1] In most cases, assessing the distance between frame topics and outcome topics was straightforward, but some subjectivity was unavoidable in identifying the "most distant" frames. For this reason, we estimated two versions of the "most distant frame" tests, and expect roughly similar results in each.

$$Score \quad = \quad \alpha + \beta_1 direct + \beta_2 distant + \mathbf{Z}\gamma + \epsilon$$

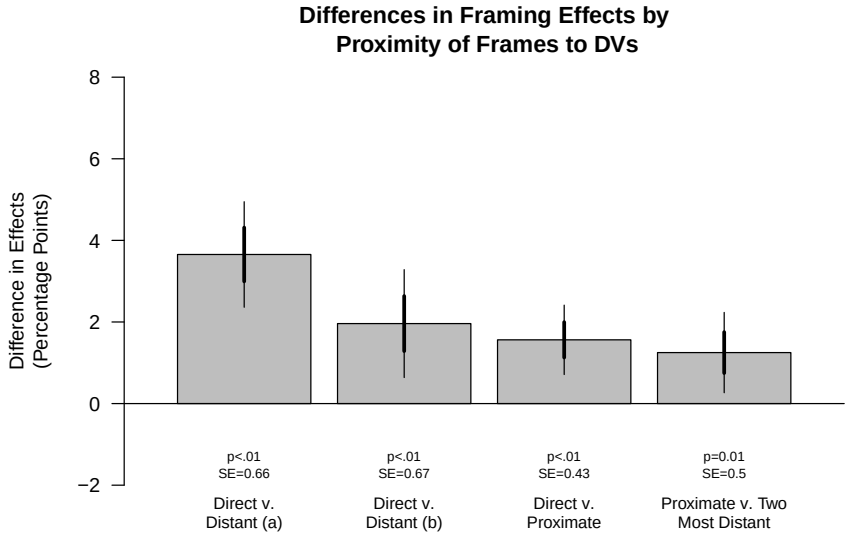
where Y is a vector of responses to various spending outcomes, α is an intercept, *direct* is an indicator for being in the "direct" condition, *distant* is an indicator for being in the "distant" condition, $\mathbf{Z}$ is a vector of party ID indicators to improve efficiency, and ϵ is an error term. The control condition was omitted as a reference category. The average difference in responses after seeing a directly related frame vs. seeing no frame is therefore represented by β_1; the average difference in responses after seeing an unrelated (i.e. distant) frame vs. seeing no frame is represented by β_2; and the difference-in-differences, (the quantity of interest), is simply $\beta_1 - \beta_2$. The standard error for the difference-in-difference, SE_{DID}, was computed analytically as:

$$\widehat{SE}_{\widehat{DID}} \quad = \quad \sqrt{\widehat{Var}_{\widehat{\beta_1}} + \widehat{Var}_{\widehat{\beta_2}} - 2 * \widehat{Cov}_{\widehat{\beta_1},\widehat{\beta_2}}}$$

We performed an analogous procedure to estimate the other aforementioned differences-in-differences (i.e. direct vs. proximate and proximate vs. two most distant).

As the two bars on the left side of Figure 4 show, the strongest differences in framing effects appear when comparing the effects of same-issue frames with the effects of distant-issue frames. The differences in treatment effects are 3.65 and 1.96 percentage points on the spending scales, and are fairly precisely estimated even after adjusting for within-respondent clustering ($p < 0.01$ for both estimates). The third vertical bar from the left shows that the same-issue frames are also stronger in their average effects than are frames which are structurally related to the spending attitude being asked about, though the difference shrinks to 1.56 percentage points ($p < 0.01$). The arguments on related issues also appear to have slightly stronger effects than the arguments on more distant issues, with a difference of 1.25 percentage points ($p = 0.01$). Overall, there is some evidence of spillover, but also very clear evidence that frames are more influential for attitudes on the issues to which they directly pertain.

Figure 4: *Relative effects of arguments on spending attitudes by argument-attitude distance.*



This figure illustrates the pooled effects of the four arguments on attitudes that vary in their distance from the issue and frame. The effects were estimated by pooling argument-attitude pairings at similar levels of distance and then comparing them to the control group that was exposed to no arguments. The gray bars indicate the mean differential effect, while the thick vertical lines show standard errors and the thin vertical lines show 95% confidence intervals. The corresponding two-sided p-values and standard errors are listed under each bar.