Three essays on the American public's war support calculus: evidence from experiments

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THREE ESSAYS ON
THE AMERICAN PUBLIC’S WAR SUPPORT CALCULUS:
EVIDENCE FROM EXPERIMENTS

by

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THREE ESSAYS ON
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ABSTRACT

This dissertation consists of three essays on the American public’s war calculus that hang together in two main ways.

First, these essays all attempt to quantitatively measure how certain things about a mission abroad—including the positions of domestic and international elites on it, its American and enemy casualties, its chance of success and main objective, its dollar cost and duration, its location on the planet—causally impact the public’s support at home. The first essay simultaneously measures the causal impacts of sixty features of a war, obtaining a rough outline of the public’s calculus on war as a mathematical function. The second essay measures the causal impacts of accumulating American and insurgent body counts over time during a war, estimating their dynamic rate of change. And the third essay measures the causal impacts of domestic political elites during parallel real and hypothetical wars, discovering a new mechanism by which the public responds to their position-taking at home.

But these measurements wouldn’t be possible without the second way these three essays hang together: all of them employ the experimental method. These essays
involve survey experiments that are atypical and nonstandard in the use of force literature. The first essay uses a *conjoint experiment*, which can powerfully estimate the causal impact of relatively many treatments with relatively few research subjects because of its efficiency. The second essay uses a *panel experiment*, which can estimate the cumulative causal impact of a dynamic treatments at many different points in time because of its sequential structure. And the third essay uses both *real and hypothetical experiments*, which can uncover how different contexts condition the estimates of treatment impacts.
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1.1 Introduction

Over the last 75 years or so researchers learned a lot about what moves the American public’s support for war. It’s happened in roughly two methodological phases so far that overlap in time: a first observational phase and and a second experimental phase.

What did researchers learn about the public’s war support in the observational phase—and how did they learn it? They observed in the data available to them that some features of a mission abroad are correlates of support for it. The first of these features are events on the ground during a mission. Researchers initially saw that the number of American casualties and support for a mission correlate negatively: if one goes up the other goes down (inter alia, Mueller, 1971, 1973; Lorell et al., 1985; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Burk, 1999; Klarevas, 2000; Gartner, 2008; Gelpi et al., 2009). This is the “casualties hypothesis.” And they eventually generalized this: the occurrence of certain events in general—not just American casualties in particular—and support correlate negatively for “bad” events and positively for “good” events (inter alia, Mueller, 1971, 1973; Lorell et al., 1985; Jentleson, 1992; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; jen; Burk, 1999;
Klarevas, 2000; Eichenberg, 2003; Feaver and Gelpi, 2004; Eichenberg, 2005; Gelpi et al., 2006; Boettcher and Cobb, 2006; Fordham, 2008; Gartner, 2008; Gelpi et al., 2009). This is the “event-response hypothesis”—a generalization of the casualties hypothesis to events other than American casualties. And the second of these features are the positions taken on a mission by partisan elites at home. Researchers saw that the strength of partisan elites’ consensus on a mission and the public’s support for it correlate positively: if one goes up the other also goes up (inter alia, see Belknap and Campbell, 1951; Brody, 1991; Zaller, 1992; Larson, 1996; Berinsky, 2007, 2009; Baum and Groeling, 2009b). This is the “elite-driven hypothesis”—although a simplified description of it. Researchers learn things in the observational phase: that events occurring on the ground—including American casualties—and the strength of partisan elite consensus at home are both correlates—one negative and one positive—of the public’s support for a mission. These claims lead to the event-response and elite-driven hypotheses, respectively.

But the knowledge that researchers gained in the observational phase about the relationships between these features of a mission—events and elites—and the public’s support for it isn’t complete. These relationships are correlations—but they might not be causations. And these features are correlates of support—but they might not be its causes. Are the event-response and elite-driven hypotheses actually right in terms of causation and not just correlation? Researchers found the answers to this questions in the experimental phase. They observed in data from both survey and field experiments they conducted that some features of a mission abroad are actually causes of support for it and not just correlates. In a traditional experiment the random assignment of subjects to be manipulated or not—like changing features such as the number of American casualties or the strength of partisan elite consensus—in expectation means that the difference in support between those manipulated and those
not is actually the causal impact of that manipulation (for a review of this procedure, see Druckman et al., 2010). And many experiments revealed that indeed both the event-response and elite-driven hypotheses are right: accumulating American casualties causes support to go down while strengthening elite consensus causes support to go up—they don’t just correlate with support going down and up, respectively. The observational phase followed by this experimental phase closes the loop on these claims.

But now here’s the next obvious question: do the event-response hypothesis and the elite-driven hypothesis explain the public’s support for a mission abroad equally, or does one explain more than the other? In other words, do events on the ground or do the positions of elites at home affect support more? And here’s the answer, according to the literature: it depends on what information about the war is available to the public.

Members of the public have “accuracy goals, which motivate them to seek out and carefully consider relevant evidence so as to reach a correct or otherwise best conclusion” on a mission abroad (Taber and Lodge, 2006, p. 756). In doing so, people gain the benefit of having the “correct” level of support for the war but also incur the costs of gathering and cognitively processing that information about it—which might include its number of American and enemy casualties, its objective and likelihood of success, its dollar cost and duration, its theater of war, and the positions taken on it by domestic and international political elites, among other things (also see Kunda, 1987, 1990; Fiske and Taylor, 1991; Baumeister and Newman, 1994; Chaiken and Trope, 1999). But people are also rational: when possible, they minimize these information costs while maintaining the accuracy benefit (Downs, 1957).

The causal mechanism underlying the elite-driven hypothesis suggests that if political elites publicly take positions on a mission abroad, people rationally gather and
cognitively process this special information and ignore everything else (Rahn, 1993; Mondak, 1993a; Druckman, 2001; Cohen, 2003; Bullock, 2009; Nicholson, 2011; Hayes and Guardino, 2011; Nicholson, 2012; Druckman et al., 2012). This happens because these elite cues are a heuristic, or shortcut (Chaiken, 1980; Mondak, 1993b; Popkin, 1991; Sniderman et al., 1991; Lupia, 1994; Lupia and McCubbins, 1998; Lau and Redlawsk, 1997; Cutler, 2002; Kam, 2005; Lau and Redlawsk, 2006; Boudreau, 2009). People believe that elites in their party both share common interests with and are more knowledgeable (and that elites in the other party don’t share common interests but are more knowledgeable) than them (Lupia and McCubbins, 1998). People then infer that elites in their party have the “correct” position on the war and take it on themselves (and that elites in the other party have the “incorrect” position and take on the opposite one) and don’t gather nor cognitively process further information like events on the ground—which would only add to their information cost with no additional accuracy benefit (Berinsky, 2007, 2009). So in the presence of the elite cues, the elite-driven hypothesis should explain the public’s war support more than the event-response hypothesis.

But what happens if political elites don’t publicly take positions on a mission abroad? What happens in the absence of elite cues? The causal mechanism underlying the event-driven hypothesis suggests that in this case—without a heuristic or shortcut available—people rationally gather and cognitively process information about war events, incurring information costs in exchange for an accuracy benefit. As people get information, negative events—like American casualties—add to a mission’s costs while positive events—like enemy casualties—add to its benefits (inter alia, Larson, 1996; Klarevas, 2000; Gelpi et al., 2009). And the difference between the costs from these negative events and the benefits from these positive events yields people’s net utility—and this quantity is equivalent to their war support. Every event that
occurs during a mission has a negative or positive impact on people’s net utility and thus their war support. So in the absence of heuristic elite cues, the event-response hypothesis should explain the public’s war support more than the elite-driven hypothesis.

From the above, we expect one hypothesis to explain the public’s war support more than the other depending on the information available. But further, the underlying causal mechanisms of both the elite-driven hypothesis and the event-response hypothesis also imply a relationship between the public’s information and its support for a mission abroad. And more than just any relationship: that support is a function of information about many different elite cues and events. Could research obtain the actual equation for this functional relationship? The equation would take information about everything relevant the public as inputs and yield its war support as the single output—and this means we’d need to measure the causal impacts of many things indeed. I’ve already discussed that researchers in the experimental phase used experiments to measure the causal impacts of many different elites cues and events on support. And perhaps experiments could measure the causal impacts of other things on support, too. This could work but I argue here that it’s a suboptimal method to reach this research goal.

What’s wrong with using experiments to measure the causal impacts of everything about war on the public’s support? An experiment needs a separate group of new subjects for each thing whose causal impact researchers intend to measure. Say researchers want to use an experiment to measure the causal impact on support in moving from zero to ten American casualties. Here they’ll need two groups of subjects: one that sees zero American casualties and one that sees ten. But say they’re also interested in what a hundred American casualties does. Here they’ll need to add another group of subjects. But say they’re also interested in what a thousand
American casualties does. Here they’ll need to add *yet another*. This addition goes so on for every level of casualties that interests them. And this addition also happens for all the many things that interest them besides various levels of American casualties. Say researchers want to use an experiment to measure the causal impact on support for 100 different things about war—so they’ll need over 100 groups of subjects. But this isn’t a feasible number of groups for an experiment.

But there’s another way besides traditional experiments to measure the causal impacts of everything about war on the public’s support. Hainmueller et al. (2014) develops a new technique called a *conjoint experiment* and an accompanying estimation procedure using data produced from it. In a conjoint experiment respondents participate in a sequence of choice tasks. In each choice task they face two “profiles” randomly generated from a collection features and must indicate which profile they prefer. And most importantly: regardless of the number of features, researchers can accurately and precisely estimate the causal impact of each and every one of these features *without needing to increase the number of subjects*. For example Hainmueller and Hopkins (2015) has only 1,407 respondents choose which of two randomly generated potential immigrants they’d prefer to see admitted into the United States and estimates the causal impact of 50 different features of potential immigrants. And Hainmueller et al. (2014) has only 311 respondents choose which of two randomly generated presidential candidates they’d prefer to see elected and estimates the causal impact of 40 different features of presidential candidates. And here I have only 487 respondents choose which of two randomly generated missions abroad they’d approve of more and estimate the causal impact of 60 different features of a mission abroad. Measuring these causal impacts using a traditional experiment would require at least 60 groups which isn’t feasible.
Of course, the *conjoint experiment* that Hainmueller et al. (2014) develops is simply a newer and specific version of an older and general approach called *conjoint analysis* that was developed by both marketing researchers (e.g., Green and Rao, 1971) and sociologists (e.g., Jasso and Rossi, 1977). Conjoint analysis as a general approach—with the conjoint experiment as a specific version of it—estimates the impact—but not necessarily the *causal* impact as in the conjoint experiment—of features on respondents’ choices and ratings of profiles (see Wallander, 2009; Raghavarao et al., 2011). And while the conjoint experiment of Hainmueller et al. (2014) involves *randomly* generated profiles of features and the estimation of average marginal component effects (AMCEs) that are the *causal* impacts of those features, this is not the case in all the versions of conjoint analysis, some versions of which involve *deterministically* generated profiles of features and the estimation of different quantities of interest that are just *correlative* impacts of those features. Hainmueller et al. (2014) claims to study the impacts of profile features from “a causal inference perspective” and uses “the potential outcomes framework of causal inference to formally analyze the causal properties of conjoint analysis” (p. 3; also see Neyman, 1923; Rubin, 1974).

In this paper, I make the first attempt in the literature to obtain a rough outline of the public’s calculus on war as a mathematical function by simultaneously measuring the causal impacts of sixty features of a war. I do so using the technique of the conjoint experiment and its accompanying estimation procedure from Hainmueller et al. (2014). The development of this method makes my attempt possible and the use of this method here represents the first application of it in the use of force literature.
1.2 A Conjoint Experiment

Similar to Hainmueller and Hopkins (2015) I conducted a conjoint experiment on the Qualtrics survey platform from January 19, 2015 to January 22, 2015 and recruited a sample of 487 subjects from the Amazon Mechanical Turk marketplace. The survey first asked respondents the ANES 7-point partisan identification questions and questions on age, education, income, foreign policy knowledge, marital status, race, sex, and state. The survey then presented respondents with a choice task repeated five times which essentially made respondents choose between two randomly generated missions abroad the mission that they preferred more. I summarize this choice task below and Figure 1.1 gives a screenshot of how one instance of this choice task appeared to respondents.

- Each choice task first asked respondents to “please review the two missions detailed below, then please answer the questions.”

- Each choice task then presented respondents with two randomly generated missions. Specifically respondents viewed a table with three columns. The first column lists—in an order randomized for each respondent and maintained for all five repetitions—ten attributes of missions: American casualties, chance of success, cost in dollars, Democratic leaders in Congress, duration, enemy casualties, main objective, region, Republican leaders in Congress, and United Nations Security Council. The second column titled “Mission 1” gives on each row a randomly chosen value for each corresponding attribute in the first column—from a pre-specified set of values that an attribute can take. The third column title “Mission 2” does the same thing. For example the attribute American casualties can take the values “0,” “10,” “100,” “1,000,” “10,000,” “100,000,”
or “1,000,000.” Table 1.1 summarizes the ten attributes of missions considered here and their possible values.

- Each choice task then asked respondents “if you had to choose between the two, which of these two missions abroad would you say you approve of more” with answers “Mission 1” or “Mission 2.”

![Choice task](image)

**Figure 1.1. Choice task.** This is a screenshot of how one instance of this choice task appeared to respondents on Qualtrics.

The coding and analysis of data from this conjoint experiment is straightforward. The unit of observation here is a randomly generated mission and not a respondent.
<table>
<thead>
<tr>
<th>Attributes</th>
<th>Values</th>
</tr>
</thead>
<tbody>
<tr>
<td>American casualties</td>
<td>0, 10, 100, 1,000, 10,000, 100,000, 1,000,000</td>
</tr>
<tr>
<td>Chance of success</td>
<td>10%, 25%, 50%, 75%, 90%, 100%</td>
</tr>
<tr>
<td>Cost in dollars</td>
<td>$1 million (0.001 billion), $10 million (0.01 billion), $1 billion, $10 billion, $100 billion, $1 trillion (1,000 billion), $10 trillion (10,000 billion)</td>
</tr>
<tr>
<td>Democratic leaders in Congress</td>
<td>No position, Oppose, Support</td>
</tr>
<tr>
<td>Duration</td>
<td>1 month, 3 months, 6 months, 1 year, 3 years, 5 years, 10 years, 20 years</td>
</tr>
<tr>
<td>Enemy casualties</td>
<td>0, 10, 100, 1,000, 10,000, 100,000, 1,000,000</td>
</tr>
<tr>
<td>Main objective</td>
<td>Attack on ally, Civil war, Famine, Food and medicine, Military rulers, Relief supplies, Troop withdrawal, Warring factions</td>
</tr>
<tr>
<td>Region</td>
<td>Africa, Central America, Eastern Europe, Middle East, South America, Southeast Asia</td>
</tr>
<tr>
<td>Republican leaders in Congress</td>
<td>No position, Oppose, Support</td>
</tr>
<tr>
<td>United Nations Security Council</td>
<td>No position, Oppose, Support</td>
</tr>
</tbody>
</table>

Table 1.1. **Attributes and values.** This table lists the attributes of a hypothetical mission abroad in the choice task and the possible values that each attribute could randomly take on from a pre-specified set of values.

Recall that there are 487 respondents who each complete 5 choice tasks that each consist of choosing between 2 randomly generated missions. This means that there are
4,870 randomly generated missions. The dependent variable is a dummy for whether a mission is chosen or not. Each independent variable is a dummy for whether a mission has a particular value of an attribute relative to some baseline value for that attribute. For example one independent variable is a dummy for whether a mission has 10 American casualties relative to a baseline 0 American casualties. The 60 values across the 10 attributes for a mission can be represented by 50 dummy variables and 10 excluded baseline categories—so there are 50 independent variables. Ordinary least squares (OLS) regression with clustered standard errors yields the desired results. Each coefficient in the resulting regression equation is the average causal impact on the probability respondents choose a mission resulting from that mission having a particular value for an attribute—the value of the attribute corresponding to that coefficient—relative to the baseline value of the attribute. Figure 1.2 summarizes these coefficients and causal impacts—and the remainder of the paper unpacks these.

The randomization of attribute values in “profiles”—here missions—implies orthogonality of the attributes. And this orthogonality further implies that all profiles with one particular value of an attribute—say 1,000,000 American casualties—and all profiles with another—say zero American casualties—have the same distribution of all the other attribute values in expectation. This means that the average causal impact of moving from one particular value—again say 1,000,000 American casualties—of an attribute to another—again say zero American casualties—is simply the difference in the proportion of times respondents choose profiles with the first value of the attribute compared to the proportion of times they choose profiles with the second one. Hainmueller et al. (2014) calls this difference in proportions—or means—the average marginal component effect (AMCE) and shows that “the AMCE represents of the average difference in the probability of being preferred [...] when comparing two attribute values [...] where the average is taken over all other possible combinations of
Figure 1.2. Causal impacts in general. This figure plots the causal impact (point estimate of AMCE) of each attribute value on the probability that an average respondent prefers a mission. 90% confidence bands and each point without error bars is a baseline value for an attribute.

[...] attributes” (p.537). The same paper shows that that “the AMCE is nonparametrically identified given the conditionally independent randomization of the attributes and can be easily estimated using a regression of the binary outcome variable [...] on sets of indicator variables measuring the levels of each attribute. The advantage of this approach is that the estimator for the AMCEs is fully nonparametric and does
not require functional form assumptions about the choice probabilities.” I estimate
the average causal impacts—AMCEs—of a mission’s attribute values in the conjoint
experiment using this approach.

What about external validity here? There are two main threats to the exter-
nal validity of the results from this conjoint experiment: the representativeness
of its Mechanical Turk sample and the artificiality of its setting and its randomly as-
signed treatments—or more specifically, the choice task and the randomly chosen
values of a mission’s attributes. On the first point, recent research argues that the
results of survey experiments with subject samples collected via Mechanical Turk
are both internally and externally valid for answering research questions emerging
in political science (Berinsky et al., 2012) as well as in economics and psychology
(Buhrmester et al., 2011; Horton et al., 2011). Particularly with respect to generaliz-
ability, Berinsky et al. (2012) shows that samples recruited via Mechanical Turk are
more representative than undergraduate convenience samples but only slightly less
representative than internet and national probability samples (also see Ipeirotis, 2010;
Ross et al., 2010; Paolacci et al., 2010). I paid $0.10 to each subject upon completion
of the panel experiment. Buhrmester et al. (2011) shows that the payment amount
does not affect the quality of subjects’ responses (also see Mason and Watts, 2009;
Chen and Horton, 2010; Horton and Chilton, 2010).

On the second point, Barabas and Jerit (2010) shows that the average treatment
effects (ATEs) in survey experiments are smaller in magnitude but have the same di-
rectional sign as ATEs in otherwise equivalent field experiments. So sufficiently large
and statistically significant ATEs in artificial settings—like survey experiments—
suggest that there would be also be smaller but still statistically significant ATEs
in natural settings—like field experiments. And so experiments’ settings condition
the causal impacts of treatments but don’t threaten their generalizability. The same
goes for conjoint experiments: they are a type of survey experiment and average marginal component effects (AMCEs) of attribute values are analogous to ATEs in survey experiments.

Are the results from conjoint experiments externally valid for the same reasons that survey experiments are? Unfortunately, there’s an additional complication in a conjoint experiment that’s not in a typical survey experiment: the choice task might be psychologically artificial rather than realistic. Recall the choice task in this paper’s conjoint experiment: respondents immediately see two missions abroad consisting of ten attributes each randomly picked from a corresponding pre-specified set, and then respondents choose which of the two missions that they prefer. This choice task might threaten the external validity of the results from the conjoint experiment. Respondents gather and cognitively process ten pieces of information about each mission immediately. But the mechanisms of the two major theories about how information affects public opinion—the receive-accept-sample (RAS) model and the online-processing model—expect people to gather and cognitively process information over time in the real world, not all at once as they do in the choice task. So the literature’s dynamic mechanisms might not be operative in the static choice task because they’re incompatible—perhaps a related set of time-compressed mechanisms are operative instead. At the very least, the conjoint experiment’s choice task primes respondents to make decisions using cost-benefit analysis—rendering their war support more rational than it would be otherwise. But this might be a research tradeoff worth making in order to obtain a rough outline of the equation for the public’s support for war—even one that overstates public’s rationality. Hainmueller and Hopkins (2015) certainly thought so and did the same with immigration.
1.3 American and Enemy Casualties

How does the number of American casualties affect the probability that respondents prefer a mission abroad? How much casualty sensitivity is there: “the overall willingness of the public to continue to support a military operation even as the human toll is rising” (Gelpi et al., 2009, p. 8). Figure 1.3 makes three points about this.

**Figure 1.3. Causal impacts of American casualties.** This figure plots the causal impact (point estimate of AMCE) of the level of American casualties on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is zero casualties.

First, “casualty phobia” occurs when the public only supports a mission abroad with nonzero casualties (for a review, see Gelpi et al., 2009). A public with casualty phobia stops supporting a military operation when the first casualty happens. Figure 1.3 shows that respondents don’t have casualty phobia. Moving from 0 to 10 casualties reduces the probability that respondents prefer a mission by 3 points and isn’t significant (SE = 2.5). This fact means that respondents’ support doesn’t drop—and doesn’t jump—between the first and tenth casualty. But moving from 0 to
100 casualties reduces the probability that respondents prefer a mission by 5 points and is significant (SE = 2.6). Combined these two facts suggest that respondents’ support finally drops sometime between the eleventh and hundredth casualty. Political leaders can adopt policies and the military can behave in ways that incur up to ten casualties—and somewhere between eleven and a hundred—without losing any support for a mission abroad.

Second, the public is “casualty sensitive” if it “views casualties as a negative, preferring less if possible” (Gelpi et al., 2009, p. 8). This implies that as casualties increase the public decreases its support for a mission abroad (inter alia, Mueller, 1971, 1973; Lorell et al., 1985; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Burk, 1999; Klarevas, 2000; Gartner, 2008; Gelpi et al., 2009). The real question is not whether the public is casualty sensitive—and scholars agree it is—but how casualty sensitive it is—and scholars disagree on the answer. The public is less casualty sensitive—“casualty tolerant”—if “casualties do not substantially undermine public support for mission.” The public is more casualty sensitive—“casualty tolerant”—if “casualties do substantially undermine public support for a mission.” Which is it?

I borrow the concept of an elasticity from economics and use it to operationalize—and quantify—casualty sensitivity. An elasticity is the ratio of how much one variable changes in response to how much another variable changes (see Mas-Colell et al., 1995). A canonical one from economics is a good’s price elasticity of demand—the ratio of how much the quantity demanded of the good changes in response to how much the good’s price changes. Now I apply this here. A possible measure of casualty sensitivity is “casualty elasticity of support”—the ratio of much the mission’s support decreases in response to how much the mission’s casualties increase. The public is casualty shy when its casualty elasticity of support is high—for a given increase in casualties, support for a mission decreases a lot. And the public is casualty tolerant
when its casualty elasticity of support is low—for a given increase in casualties, support for a mission decreases a little. Of course the terms “high” and “a lot,” “low” and “a little” in this abstract description are relative so a concrete computation of casualty elasticities from the results may help.

Moving from zero to non-zero casualties reduces the probability that respondents prefer a mission by some number of points in a significant way—with the one exception of moving from 0 to 10 casualties, which causes a 3-point but insignificant reduction. And larger movements in casualties cause greater reductions in preference probability. Figure 1.3 summarize these movements and reductions. This is evidence that respondents are casualty sensitive. But what is the casualty elasticity of support in each of the implicit casualty ranges—0 to 10, 10 to 100, 100 to 1,000, 1,000 to 10,000, 10,000 to 100,000, 100,000 to 1,000,000? How casualty sensitive are respondents?

I show how I compute casualty elasticity of support in each given casualty range by way of example. The first 100 casualties reduce the probability that respondents prefer a mission by 5 points—the point estimate moving from 0 to 100 casualties (SE = 2.6). And the first 1,000 casualties reduce the probability by 16 points—the point estimate moving from 0 to 1,000 casualties (SE = 2.8). This implies that the 900 casualties from the 101st to the 1,000th reduce the probability by 11 points from 5 points to 16 points. With this fact it’s straightforward to compute the casualty elasticity—the ratio of how much the mission’s support decreases in response to how much its casualties increase—in the 100 to 1,000 casualty range. The casualty elasticity comes out to 0.012222—a decrease in 11 points divided by an increase in 900 casualties (and taking the absolute value to make it positive). Another way of interpreting this casualty elasticity is that there’s a 0.012222 decrease in the probability that respondents prefer a mission per casualty incurred in the 101 to 1,000 casualty range—
and that’s how sensitive respondents are to casualties there. Table 1.2 summarizes the casualty elasticity of support for all the possible ranges in this study.

<table>
<thead>
<tr>
<th>Casualty Range</th>
<th>Casualty Elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td>0 - 10</td>
<td>0.300000</td>
</tr>
<tr>
<td>10 - 100</td>
<td>0.022222</td>
</tr>
<tr>
<td>100 - 1,000</td>
<td>0.012222</td>
</tr>
<tr>
<td>1,000 - 10,000</td>
<td>0.000444</td>
</tr>
<tr>
<td>10,000 - 100,000</td>
<td>0.000100</td>
</tr>
<tr>
<td>100,000 - 1,000,000</td>
<td>0.000013</td>
</tr>
</tbody>
</table>

Table 1.2. Casualty elasticity of support. This table lists the casualty elasticity of support in each range of a mission’s American casualties.

Table 1.2 shows that as casualties increase the casualty elasticity of support—casualty sensitivity—decreases. In the 1-10 casualty range respondents have a casualty elasticity of 0.3 and reduce their probability of preferring a mission by 0.3 per casualty. But in the 100,000-1,000,000 casualty range respondents have a casualty elasticity of 0.00001 and reduce their probability by just 0.00001 per casualty. This means that later in a mission when there’s more than 100,000 casualties respondents value each casualty 30,000 times less than they do later when there’s less than 10 casualties. The public’s casualty sensitivity starts high and ends low—here 30,000 times lower. The point of Table 1.2: war collapses how the public values lives lost—here collapsing by a factor of 30,000. The public values the first life lost the most and the last life lost the least—respondents here valuing the first casualty as much as the last 30,000. The public swings from casualty shy to casualty tolerant as casualties mount.

Third, the number of casualties has the potential to change public support for a mission abroad the most. Moving from 0 to 1,000,000 casualties reduces the probability that respondents prefer a mission by 41 points and is significant (SE = 2.6)—the largest movement in this study. And a mission’s cost in dollars and its likelihood of success have the second and third most potential to change public support but just
half the potential of casualties. Moving from $1 million to $10 trillion in cost reduces
support by 23 points (SE = 2.6) and moving from a 10% to a 100% success likelihood
raises support by 20 points (SE = 2.4)—the second and third largest movements in
this study but only half of the 41-point move in support in going from 0 to 1,000,000
casualties. When the public has full information about a mission abroad—about its
number of American casualties, its number of enemy casualties, its cost in dollars, its
anticipated duration, its location, its objective, its likelihood of success, its support
among domestic and international political elites—the number of American casualties
changes the public’s support for a mission abroad more than anything else. American
casualties are the prime mover of war support.

Now what about the number of enemy casualties? Do they affect the probability
that respondents prefer a mission abroad? Does the public view enemy casualties
“as a reasonable indicator of success” that can “contextualize U.S. casualties in the
hopes that high ratios of Iraqi deaths to U.S. deaths will reduce the negative impact of
American losses” and “as a yardstick—measuring both the success of the battle and
the relative significance of U.S. casualties” (Boettcher and Cobb, 2006, p. 833)? Do
enemy casualties measure a mission’s success or contextualize its American casualties
for the public (also see Gartner and Myers, 1995; Burk, 1999)? This implies that
as enemy casualties increase the public increases its support for a mission abroad.
But Figure 1.4 shows that this doesn’t happen among respondents. Moving from
zero enemy casualties to any other number of them fails to change the probability
that respondents prefer a mission in either a substantively or statistically significant
way. This null result may stem from two things. First, respondents already have
explicit information that measures a mission’s success—the likelihood of success as a
percentage—and infer it from implicit information available—the number of enemy
casualties. Second, respondents have eight other pieces of information that they
use to contextualize American casualties besides enemy casualties. When the public has full information about a mission abroad it measures success and contextualizes American casualties with things other than enemy casualties—discounting or even ignoring them so that they don’t change support. The public doesn’t care about enemy casualties when it knows so much more than that about a mission.

Figure 1.4. Causal impacts of enemy casualties. This figure plots the causal impact (point estimate of AMCE) of each level of enemy casualties on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is zero casualties.

1.4 Chance of Success & Main Objective

How does a mission’s likelihood of success affect the probability that respondents prefer that mission? Do the public’s “expectations of success matter the most [...] when it comes to supporting an ongoing military in the face of a mounting human toll” (Gelpi et al., 2009, p. 2)? And does “the public’s expectations of whether the mission will be successful trump other considerations” that “affect the robustness of support” (Gelpi et al., 2009, p. 2; also see Eichenberg, 2005)? A public doing these two things
would increase its support for a mission abroad when it believes the likelihood of
the mission’s success increases—given the number of American casualties. Figure 1.5
shows that respondents act this way. Moving from the 10% baseline chance of success
to a 25%, 50%, 75%, 90% or 100% chance raises the probability respondents prefer a
mission by 4 points (SE = 2.3), 11 points (SE = 2.4), 15 points (SE = 2.4), 16 points
(SE = 2.4), and 20 points (SE = 2.4), respectively and significantly so. Respondents’
support for a mission increases as the chance of success does—almost in a straight
line.

![Impact on Probability of Preferring Mission](image)

**Figure 1.5. Causal impacts of chance of success.** This figure plots the causal impact (point
estimate of AMCE) of the chance of a mission’s success on the probability that an average respondent
prefers it. 90% confidence bands and the baseline is a 10% chance of success.

Just how straight-line is the relationship between support for a mission among
respondents and the chance of success? The “success elasticity of support”—the
ratio of how much the mission’s support increases in response to how much its chance
of success increases—answers this question. Table 1.3 gives the success elasticity of
support across all success ranges. (The previous section gives an explanation of what
an elasticity is and how to compute it.) In the 10-25% success range respondents have a success elasticity of 0.27 and raise their probability of preferring a mission by 0.27 points per point increase in success likelihood. And in the 90-100% success range respondents have a success elasticity of 0.2 and raise their probability by 0.20 points per point increase in success likelihood. For missions with the highest success likelihood the public values each point of success likelihood by a little less than they do for mission with the lowest success likelihood—here just a quarter less. The public’s “success sensitivity”—the value of a point of success likelihood gained—declines just a little over nearly the entire range of success likelihood—here by just a quarter. The public loves a better chance for a mission’s success the same no matter how big the chance of success is already.

<table>
<thead>
<tr>
<th>Success Chance Range</th>
<th>Success Elasticity</th>
</tr>
</thead>
<tbody>
<tr>
<td>10% - 25%</td>
<td>0.27</td>
</tr>
<tr>
<td>25% - 50%</td>
<td>0.28</td>
</tr>
<tr>
<td>50% - 75%</td>
<td>0.16</td>
</tr>
<tr>
<td>75% - 90%</td>
<td>0.20</td>
</tr>
<tr>
<td>90% - 100%</td>
<td>0.20</td>
</tr>
</tbody>
</table>

Table 1.3. Success elasticity of support. This table lists the success elasticity of support in each range of a mission’s chance of success.

Table 1.3 has another interesting result. Moving from a 90% chance of success to a 100% chance raises respondents’ support by only 2 extra points. This isn’t just any 10-point increase in the chance of success—it’s a 10-point increase in the chance of success that eliminates the risk that a mission fails and guarantees that a mission succeeds. But respondents value an extra point of success likelihood the same—at 0.2 extra points of support—when going from a 90% chance of success to a 100% chance as they do going from a 75% chance to a 90% chance. Respondents don’t reward a mission that’s certain to succeed with any premium on support for it. This extends my point in the last paragraph: the public always loves a better chance for a mission’s success.
success the same—whether the mission already got a small chance, a big chance, or a guaranteed chance of success.

Now what about the mission’s objective? Does it affect the probability that respondents prefer a mission abroad? Does “the most powerful and parsimonious explanation for the variations in public support in the post-Cold War era continue to be found in the variable of the principal policy objective (PPO) for which military force is being used” (Jentleson and Britton, 1998, p. 396)? This “PPO theory” splits the possible objectives for a mission abroad into three categories (also see Jentleson, 1992; Eichenberg, 2005; Gelpi et al., 2009, chp. 8).

- **Foreign policy restraint**, or FPR: “force used to coerce FPR by an adversary engaged in aggressive actions against the United States, its citizens, or its interests” (Jentleson and Britton, 1998, p. 397).

- **Internal political change**, or IPC: “force used to try to engineer IPC in another country’s government, whether as support for an existing government considered an ally or an effort to overthrow a government considered an adversary” (Jentleson and Britton, 1998, p. 397).

- **Humanitarian intervention**, or HI: “the provision of emergency relief through military and other means to people suffering from famine or other gross and widespread humanitarian disasters” (Jentleson and Britton, 1998, p. 399-400).

First the PPO theory claims that the public perceives missions with an FPR or HI objective as more legitimate—more obedient to international law—and more efficacious—clearer standards of success—than those with an IPC objective (Jentleson and Britton, 1998; Eichenberg, 2005). And the public rewards FPR and HI missions for this with extra support over IPC missions. But second the PPO theory doesn’t
say anything definitive about the public’s support for FPR missions compared to HI missions—just that “we may not find a fixed preference pattern between HI and FPR but postulate that their levels of support should be more similar than different and that the differences between them should be less than either’s difference with IPC” (Jentleson and Britton, 1998, p. 400).

This study has three possible objectives under each of the three categories of objectives FPR, HI, and IPC. The FPR objectives are “stopping an aggressor’s attack on an ally” (attack on ally), “forcing an aggressor to withdraw troops from a country” (troop withdrawal), and “stopping the regional and local threat to security from an aggressor using nuclear weapons against a country” (nuclear weapons). The HI objectives are “delivering relief supplies to a country” (relief supplies), “distributing food and medicine in a country” (food and medicine), and “ending starvation from famine in a country” (famine). The IPC objectives are “disarming warring factions in a country” (warring factions), “stopping the civil war in a country” (civil war), and “ousting military rulers and reinstalling the democratically elected leaders in a country” (military rulers). Do respondents act like the PPO theory says they should?

Do respondents support missions with the FPR and HI objectives of attack on ally, troop withdrawal, nuclear weapons, relief supplies, food and medicine, and famine more than those with the IPC objectives of warring factions, civil war, and military rulers? Figure 1.6 shows that this happens—mostly. Moving from the attack on ally baseline objective to nuclear weapons, relief supplies, food and medicine, famine, or warring factions changes the probability respondents prefer a mission by -1 points (SE = 3.0), +3 points (SE = 2.8), +0 points (SE = 2.9), +3 points (SE = 2.9), and -3 points (SE = 2.9), respectively but not significantly so. This implies that respondents support missions with these objectives—two with FPR, all three with HI, and just one with IPC—all the same in a statistical sense. And moving from
the attack on ally baseline objective to troop withdrawal, civil war, or military rulers reduces respondents’ support for a mission by 7 (SE = 2.8), 9 points (SE = 2.9), and 6 points (SE = 2.9), respectively and significantly so. And this implies that respondents support missions with these objectives—just one with FPR, two with IPC—less than those in the previous list.

This all means two things. First with one exception—the FPR objective troop withdrawal—respondents support missions with the FPR and HI objectives the same. Second, again with one exception—the IPC objective warring factions—respondents support missions with IPC objectives less than those with FPR and HI objectives. These two findings add evidence to the PPO theory but also suggest that people may respond to the specific mission objectives like troop withdrawal and warring factions in ways that their respective mission categories FPR and IPC don’t indicate under

![Figure 1.6. Causal impacts of a mission’s main objective.](image)

This figure plots the causal impact (point estimate of AMCE) of the mission’s main objective on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is stopping an aggressor’s attack on an ally.
the PPO theory. Future research should update the PPO theory—which appears correct in general here—to account for this. The public likes using force abroad to stop aggressors and to help people—but not to make or break governments.

1.5 Dollar Cost & Duration

How does a mission’s dollar affect the probability that respondents prefer it? Is the public “dollar cost sensitive” just like it is “casualty sensitive”—does the public respond to dollar cost the same way it does to the “human cost of war”—American casualties—“as a negative, preferring less if possible” and decreasing its support for a mission abroad as casualties increase (Gelpi et al., 2009)? And if so how “dollar cost sensitive” is it? So far scholars haven’t answered these questions directly except for showing that the public supports missions that help them economically more than those that don’t (Fordham, 2008). But there’s a germ for an argument here: this suggests that the public supports missions that hurt them economically less than those that don’t and in proportion. And a mission’s dollar cost hurts the public economically—and bigger dollar cost inflicts greater hurt. This syllogism concludes that the public is “cost sensitive”—it prefers lower dollar cost if possible and decreases its support for a mission abroad as dollar cost increases.

I don’t quantify the exact dollar cost sensitivity of respondents by calculating their “dollar cost elasticity of support” because an elasticity doesn’t make sense here—like it does with casualties and the chance of success—because the effect of dollar costs on respondents’ support across its range doesn’t change smoothly. Figure 1.7 shows this. Moving from the $1 million baseline cost to $10 million, $100 million, or $1 billion reduces the probability that respondents prefer a mission by only 2 points (SE = 2.7), 5 points (SE = 2.6), or 2 points (SE = 2.7), respectively, but not significantly except
for the middle case. But moving from the $1 million baseline cost to $10 billion, $100 billion, $1 trillion, or $10 trillion reduces respondents’ support for a mission by 7 points (SE = 2.6), 14 points (SE = 2.7), 13 points (SE = 2.8), and 23 points (SE = 2.6), respectively, and significantly so. This means that dollar costs less than $1 billion for a mission don’t reduce respondents’ support for it but dollars costs above $1 billion do—a threshold.

**Figure 1.7. Causal impacts of a mission’s dollar cost.** This figure plots the causal impact (point estimate of AMCE) of the mission’s dollar cost on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is zero dollars in cost.

Above this threshold the penalty starts with a 7 point drop from a $10 billion cost but jumps to a 13 or 14 point drop from a $100 billion or $1 trillion cost—a ten or one hundred fold cost increase only doubles the damage. And the penalty jumps to a 23 point drop from $10 trillion cost—a ten fold cost increase only about doubles the damage—yet again. For context this 23-point drop in support going from a $1 million cost to a $10 trillion cost is the second-largest movement in the study but still only about half the first largest one—the 41-point drop in support going from
0 to 1,000,000 casualties. In fact the penalty for a $10 trillion cost and only 10,000 casualties is the same relative to their baselines. The public doesn’t like losing money on missions abroad—especially amounts over $1 billion—but it hates losing American soldiers even more.

How does a mission’s duration affect the probability that respondents prefer it? Is the public “time sensitive” just like it is “dollar cost sensitive”? I claim that the same logic that explains the public’s dollar sensitivity explains its time sensitivity—and predicts a similar pattern of impacts. The public only has so many dollars to spend (and what they represent). So in spending dollars on a mission abroad the public consumes a scarce resource. By definition doing so hurts the public economically but might help it by yielding benefits. And there’s even more harm here in the form of opportunity cost. The public foregoes spending these dollars and these scarce resources in ways other than war—trading civilian spending for military spending, guns for butter. Similarly, the military also has only so much time to spend on missions—another scarce resource like dollars. So spending time on a particular mission hurts the public twice in an economic way: it depletes the military’s time stock and forces the public to give up other missions. As argued in a paragraph above the public supports missions that hurt them economically less than those that don’t and in proportion (Fordham, 2008). And as argued in this paragraph a mission’s duration hurts the public economically—and longer durations inflict greater hurt. This syllogism concludes that the public is “time sensitive”—it prefers shorter duration if possible and decreases its support for a mission abroad as its duration increases. Parallel logic explains the impacts of both a mission’s dollar cost and duration on the public’s support.

The effect of duration on respondents’ support across its range changes even less smoothly than that for dollar cost—rendering “time elasticity of support” useless for
quantifying respondents’ time sensitivity. Figure 1.8 shows this. Moving from the 1 month baseline duration to 3 months, 6 months, 1 year, 3 years, or 5 years changes the probability that respondents prefer a mission by only +3 points (SE = 2.7), +1 point (SE = 2.7), -2 points (SE = 2.6), -3 points (SE = 2.7), and -4 points (SE = 2.7), respectively, but not significantly. But moving from the 1 month baseline duration to 10 years or 20 years reduces respondents’ support by 5 points (SE = 2.6) and 8 points (SE = 2.6), respectively. This means that durations 5 years or less don’t reduce respondents’ support for it but durations 10 years or more do—a threshold somewhere between 5 years and 10 years. Both a mission’s duration and its dollar cost must reach minima before they have any impact on respondents’ support.

![Impact on Probability of Preferring Mission](image)

**Figure 1.8. Causal impacts of a mission’s duration.** This figure plots the causal impact (point estimate of AMCE) of the mission’s duration on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is 1 month’s duration.

Above this threshold the penalty starts with a 5 point drop from a 10 year duration but increases to an 8 point drop from a 20 year duration—a doubling of duration from the second-longest duration tested in this study to the first-longest one doesn’t
quite double the damage. For context this 8-point drop in support going from a 1 month duration to a 20 year duration is only about half the second-largest one in this study—the 23-point drop in support going from a $1 million to a $10 trillion cost. The penalty for an 8 year duration, a $10 billion cost, and 100 casualties are all about the same relative to their baselines. The public doesn’t like losing time on missions abroad—those over 10 years—but it hates losing money even more and hates losing American soldiers the most.

1.6 Region

Does the theater of war matter? Does where a mission abroad takes place change the probability that respondents prefer it? Consider two missions exactly identical—in their number of American and enemy casualties, in their objectives and likelihoods of success, in their dollar costs and durations, and in the position-taking of domestic and international political elites—except that one takes place in the Middle East, the other in South America. Which gets more support from the public? How about Africa versus Southeast Asia? Eastern Europe versus Central America? It’s an interesting question that the literature on the use of force hasn’t rigorously addressed but I do here.

Figure 1.9 suggests the answer to this question is no—at least among respondents here. Moving from the baseline theater of war—Africa—to any other—Eastern Europe, Central America, Middle East, Southeast Asia, South America—fails to change the probability that respondents prefer a mission abroad in either a substantively or statistically significant way. The public doesn’t care about where America wages war.

But I qualify this null results in the same way as I do for enemy casualties. In this study respondents already as-if have full information about a mission abroad because
of eight other facts they know about it—American casualties, enemy casualties, objective, likelihood of success, dollar cost, duration, and the positions of Democrat and Republican leaders in Congress and the United Nations Security Council—and which they can use to as-if calculate their support for it. It’s possible that respondents discount or even ignore a mission’s theater—just like they seem to do so with enemy casualties—when they know so much else about the war that’s important to them in making a decision about it. Perhaps respondents value these unimportant facts when important ones aren’t available to them—and they can use the former to infer the latter. A future study should adjudicate this. But for now I conclude that the public doesn’t care about theater of war when it knows so much else about a mission.

Figure 1.9. Causal impacts of a mission’s region. This figure plots the causal impact (point estimate of AMCE) of the mission’s region on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is Africa.
1.7 Elite Cues

How do elite cues affect the probability that respondents prefer a mission abroad? Do the positions—opposition or support—of domestic and international political elites on a mission cue the public about which positions it should take? Does the public actually change—and how does it change—its positions on the mission after seeing these elites’ positions?

In brief “elite cue theory” and its extensions say that people change their position on a mission in three possible ways after they see some elites’ position on it (inter alia, Belknap and Campbell, 1951; Brody, 1991; Zaller, 1992; Larson, 1996; Berinsky, 2007, 2009; Baum and Groeling, 2009b). What people do depends on who these elites are.

- People take on the same position as these elites do if they think both that the elites know more about the mission than them and that the elites share their interests. Here these elites are knowledgeable and friendly.

- People take on the opposite position from the one these elites do if they think that the elites know more about the mission than them but that the elites don’t share their interests. Here these elites are knowledgeable but unfriendly.

- People keep their initial position if they think that these elites don’t know more about the mission than them regardless of whether the elites share their interests or not. Here these elites are unknowledgeable and it doesn’t matter whether they’re friendly or unfriendly.

This all means two things occur when people see some elites’ position on a mission (Chaiken, 1980; Mondak, 1993b; Popkin, 1991; Sniderman et al., 1991; Lupia, 1994; Lupia and McCubbins, 1998; Lau and Redlawsk, 1997; Kam, 2005; Lau and
Redlawsk, 2006; Boudreau, 2009). People only change their position if the elites are knowledgeable. And given that they’re knowledgeable, people take the same position as friendly elites and the opposite position from unfriendly elites. Knowledge allows movement. Friendliness directs it.

Who do people think knowledgeable on such missions abroad? Partisan leaders in Congress and the United Nations Security Council? Yes, among others—according to the literature on elite cues. This means that people change their position on the mission when they see the position of either partisan leaders or the Security Council. So there’s movement—but in which direction?

Do people think partisan leaders or the Security Council unfriendly or friendly? It depends on people’s own partisanship—again, according to the literature on elite cues. First, people think congressional leaders with the same partisanship are friendly and those with differing partisanship are unfriendly. This means that when Republicans see the position of Republican leaders in Congress they take it too. And when they see the position of Democrat leaders they take the position opposite from it (Berinsky, 2007, 2009). The logic is the same for Democrats. When Democrats see the position of Democrat leaders they take it too. And when they see the position of Republican leaders they take the position opposite from it. Second, people regardless of their partisanship think that the Security Council is friendly because “the US public is internationalist in general” (Grieco et al., 2011, p. 565). But Democrats think the Security Council is friendlier than Republicans do because they’re “members of the American public who value the institution” and international organizations more (Grieco et al., 2011, p. 564). This means that when people see the position of the Security Council, Democrats move closer to its position than Republicans do (also see Chapman and Reiter, 2004; Voeten, 2005; Thompson, 2006, 2009; Chapman, 2007, 2009; Fang, 2008). Do Figures 1.10, 1.11, and 1.12 bear all of this out? It’s mixed.
First take the domestic elites here in Figures 1.10 and 1.11. How do Republicans respond to position-taking by partisan leaders in Congress? Moving from the baseline of Republican leaders not taking a position on a mission abroad to them opposing or to them supporting it changes the probability that these Republicans prefer it by -5 points (SE = 3.0) and +5 points (SE = 2.9), respectively—these impacts fall just short of statistical significance. And moving from the baseline of Democrat leaders not taking a position to them opposing or supporting it changes support by -2 points (SE = 3.3) and +4 points (SE = 2.9) but not significantly. This means that Republicans take the same position on a mission abroad as the one Republican leaders in Congress do when they see it—as elite cute theory predicts. But Republicans don’t change their position when they see the one Democrat leaders take—but elite cue theory predicts that they take the opposite position.

And how do Democrats respond to position-taking by partisan leaders in Congress? Moving from the baseline of Democrat leaders not taking a position on a mission abroad to them opposing or to them supporting it changes the probability that these Democrats prefer it by -1 point (SE = 2.3) and +1 point (SE = 2.2), respectively, but not significantly. And moving from the baseline of Republican leaders not taking a position to them opposing it or supporting it changes support by +2 points (SE = 2.3) and +6 points (SE = 2.4), respectively—the second of these impacts is statistically significant but he first isn’t. This means that Democrats don’t change their position on a mission abroad when they see the one Democrat leaders take—but elite cute theory predicts that they take the same position. And Democrats take the same position as Republican leaders when they see it—but elite cue theory predicts the opposite of this: that they take the opposite position not the same.

These results suggest that elite cue theory incorrectly predicts the impacts of cues from domestic partisan elites on the public most of the time. In one case—
Figure 1.10. Causal impacts of position-taking by Republican leaders. This figure plots the causal impact (point estimate of AMCE) of position-taking by Republican leaders in Congress on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is no position. The “triangle” point estimates correspond to a Republican subject’s response and the “square” point estimates correspond to a Democrat subject’s response to these cues.

Republican elites cueing Republicans—the theory predicts what happens here. In another other case—Republican elites cueing Democrats—the theory predicts the opposite of what happens here. And in the other two cases—Republican elites cueing Democrats and Democrat elites cueing Democrats—the theory predicts something happens but nothing actually happens here.

Why might elite cues have relatively minor impacts on respondents’ support for a mission abroad—compared to what the elite-driven hypothesis predicts? One possibility is that the conjoint experiment’s choice task might lower or even eliminate the special status of elite cues as a heuristic, or shortcut. Recall that people use elite cues to choose their “correct” level of war support while putting as little effort as possible into gathering and cognitively processing information—maintaining accuracy benefit while minimizing information costs. But the conjoint experiment’s choice task di-
Figure 1.11. Causal impacts of position-taking by Democratic leaders. This figure plots the causal impact (point estimate of AMCE) of position-taking by Democratic leaders in Congress on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is no position. The “triangle” point estimates correspond to a Republican subject’s response and the “square” point estimates correspond to a Democrat subject’s response to these cues.

Directly gives respondents information besides elite cues—reducing the effort required to gather and cognitively process it and the attendant information costs to perhaps near zero. Here in the artificial context of the conjoint experiment’s choice task—unlike in the real world—respondents can gather and cognitively process information besides elite cues to gain additional accuracy benefit without incurring additional information costs—and doing so is rational. Here elite cues have to actually compete with other types of information without their usual information cost advantage—and greatly diminishing their influence on the public’s support for a mission abroad.

Now take the international elites here in Figure 1.12. How do Republicans respond to position-taking by the United Nations Security Council? Moving from the baseline of the Security Council not taking a position on a mission abroad to opposing or supporting it reduces the probability that these Republicans prefer it by 1 point.
(SE = 2.9) and 2 points (SE = 2.8), respectively, but not significantly. And how do Democrats respond? Moving the baseline of the Security Council not taking a position to opposing or supporting it changes support by -1 point (SE = 2.4) and +6 points (SE = 2.4), respectively—the second of these impacts is statistically significant but he first isn’t. This means that Republicans don’t change their position on a mission abroad when they see the one the Security Council takes—as elite cue theory predicts. And Democrats take the same position as the Security Council does when they see it supports a mission—again as elite cue theory predicts. But Democrats don’t change their position when they see the Security Council opposes the mission—contra elite cue theory’s prediction.

Figure 1.12. Causal impacts of position-taking by the UN. This figure plots the causal impact (point estimate of AMCE) of position-taking by the United Nations Security Council on the probability that an average respondent prefers a mission. 90% confidence bands and the baseline is no position. The “triangle” point estimates correspond to a Republican subject’s response and the “square” point estimates correspond to a Democrat subject’s response to these cues.

These results suggests that elite cue theory correctly predicts the impacts of cues from international elites on the public most of the time. In one case—international
elites cueing Republicans—the theory predicts what happens here: nothing. In the other case—international elites cueing Democrats—the theory is half right. The theory predicts what actually happens from an international organization’s support cue—but predicts something happens from the international organization’s oppose cue when nothing actually happens here.

1.8 Discussion & Conclusion

This paper used a new approach developed in Hainmueller et al. (2014)—the conjoint experiment—to simultaneously estimate the causal impacts—the average marginal component effects (AMCEs)—of sixty different features of missions abroad. The estimates of these causal impacts and AMCEs together comprise the use of force literature’s first rough outline of the equation of the public’s calculus on war. Some features of a mission affect support for it more than others when the public considers—like here—many different dimensions at the same time all contextualizing each other—with a relatively larger information set. A first way forward for future research is seeing what happens with fewer dimensions and less contextualizing—with a relatively smaller information set. In closing I point out three takeaways from the research in this paper.

First, there’s more evidence here for the event-response hypothesis than for the elite-driven hypothesis. In fact the causal impacts of events dominate those of elites. The causal impacts of number of American casualties, cost in dollars, and chance of success are the largest in this study and move support by up to 41 points, 23 points, and 20 points, respectively. And the causal impacts of primary policy objective and duration are the second largest and move support by up to 9 points and 8 points, respectively. And the number of enemy casualties and the theater of war don’t do
anything at all to support. But contrast these events with elites. The causal impacts of Republican leaders in Congress and the United Nations Security Council move support by up to 6 points while Democrat leaders don’t do anything at all. In terms of magnitude the causal impact of American casualties almost *septuples*, cost in dollars almost *quadruples*, and chance of success almost *triples* the potential causal impact of elites on support. And not only that: the causal impacts for domestic elites aren’t even in the directions predicted by the elite-driven hypothesis. Does the public care what domestic or international elites think about a mission abroad once events on the ground start? Clearly not in the more artificial setting of the conjoint experiment in this paper—although perhaps the public does in the real world.

Second, there’s more evidence here for the casualties hypothesis—and its reformulation as a component of the costs-benefits hypothesis—than any other hypothesis. In fact the causal impact of American casualties dominates all others and in just the way the casualties hypothesis predicts. The causal impact ranges from a 5-point reduction in support from just 100 American casualties to an unmatched 41-point one from 1,000,000 and everything in between—the reduction in support monotonically increases in the number of casualties. And a realistic 1,000 or 10,000 American casualties reduces support by 16 or 20 points respectively—only an unrealistic $10 trillion in dollar cost or a 90% chance of success changes support by more. This all suggests that the public perceives that American casualties are a cost and that this cost is the most important part of any costs-benefits calculus on war—just like decades of scholarship on the use of force has also found.

Third—and finally—there’s evidence here for a hypothesis that the scholarship on the use of force has ignored for decades: the “dollar cost hypothesis”—another component of the costs-benefits hypothesis. In fact the causal impacts of dollar cost are some of the largest in this study—in the same range as the causal impacts of
American casualties and chance of success. The causal impact of dollar cost ranges from a 7-point reduction in support from $10 billion cost to a 23-point one from $10 trillion cost and everything in between—the reduction in support is monotonically increases in dollar cost. And a realistic $100 billion cost or $1 trillion cost reduces support by around 13 or 14 points—only an unrealistic 100,000 and 1,000,000 American casualties or 75% and 90% chances of success change support by more. The patterns of causal impacts for both dollar cost and American casualties are similar because the public perceives both as costs in their costs-benefits calculus on war—with dollar cost as an important part but not as important as American casualties. Future research should study this dollar cost hypothesis further. The potential causal impacts of dollar cost on support for a mission abroad are nearly as large as those for American casualties and the chance of success—and yet we know so much about the last two and almost nothing about the first at the time of this writing.
2.1 Introduction

The practice of reporting the enemy body count dates back to at least the Peloponnesian War (Phillips, 2009). During the Vietnam War, the military of the United States released daily enemy body counts at press briefings in Saigon called “five o’clock follies” (Gartner and Myers, 1995; Boettcher and Cobb, 2006). During the War in Afghanistan and the War in Iraq, the military initially abandoned this practice. In 2002, Commander of United States Central Command (CENTCOM) General Tommy Franks stated “we don’t do body counts,” echoing the military’s official policy at the time. But eventually, the United States military reversed its official policy, reverting back to this practice from Vietnam and antiquity—if temporarily (Phillips, 2009; Kaplan, 2011). Why? One possible answer is that the military reported the enemy body count during the War in Afghanistan and the War in Iraq as a strategy to make the public evaluate those engagements more highly than it would otherwise.

The United States military must continuously report the number of casualties it sustains during a war for political reasons. The public uses the American body count to evaluate the war: the greater the American body count, the lower the public's
evaluation of the war (Mueller, 1971, 1973; Lorell et al., 1985; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Burk, 1999; Klarevas, 2000; Gartner, 2008; Gelpi et al., 2009). But a problem emerges here: the military decreases the public’s evaluation of the war by continuously reporting the increasing American body count but must report it. A solution to this problem would raise the public’s evaluation of the war, countervailing the negative effect from the military’s mandatory and continuous reporting of the increasing American body count. Boettcher and Cobb (2006) suggests an approach. According to the authors, the public uses the enemy body count to evaluate the war: all else equal, the greater the enemy body count given some American body count, the higher the public’s evaluation of the war. A possible solution emerges here then: the military might increase the public’s evaluation of the war by continuously reporting the increasing enemy body count in addition to continuously reporting the increasing American body count.

I demonstrate two main empirical results in this paper. First, no study to date shows that either this problem exists nor that this solution fixes it using the experimental method, the gold standard in causal inference (for a review, see Druckman et al., 2010), but I do so here. Second, I also show in this paper that both this problem and the effectiveness of this solution not only both exist and but also both fade out of existence—vanishing over time during a war. I use an innovative survey experiment which allows me to infer causality, in contradistinction to most previous works which use observational data and can only detect correlations which suggest causality.¹

¹First, Many studies demonstrate observationally that the problem exists (i.e., the military decreases the public’s evaluation of the war by continuously reporting the increasing American body count)(Mueller, 1971, 1973; Lorell et al., 1985; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Burk, 1999; Klarevas, 2000; Gartner, 2008; Gelpi et al., 2009). However, no study demonstrates experimentally that the problem exists, although just one extant study, Gartner (2008), suggests that it does. In particular, Gartner (2008) demonstrates experimentally two results. First, at any moment when the military reports the increasing American body count, the public’s evaluation of the war
First, consider the existence and vanishing of the problem. Many studies show experimentally that the public’s evaluation of a war decreases at any moment when the military reports the increasing American body count at that moment, but no studies show experimentally that the public’s evaluation of the war decreases over time as the military continuously reports the American body count over time, the problem at hand. In this paper, I present experimental evidence not only that the problem exists, but that it vanishes over the duration of the war. In particular, I show experimentally that the public’s evaluation of the war decreases over time as the military continuously reports the increasing American body count over time but that the rate of decrease decelerates to zero over time. Second, consider the existence and vanishing of the solution. Boettcher and Cobb (2006) suggests (but does not show either observationally or experimentally) that the public’s evaluation of the war increases at any moment when the military reports the increasing enemy body count in addition to the increasing American body count at that moment, but no studies show experimentally that the public’s evaluation of the war increases over time as the military continuously reports the increasing enemy body count in addition to the increasing American body count over time, a possible solution to the problem at hand. In this paper, I present experimental evidence not only that this solution fixes the problem, but that it vanishes over the duration of a war. In particular, I decreases at that moment. Second, this first result is greater when the American body count is accelerating but lesser when the American body count is decelerating. Unfortunately, while these two results from Gartner (2008) suggest that the problem exists, they do not demonstrate experimentally that it in fact does. Second, no studies demonstrate either observationally or experimentally that the solution fixes the problem (i.e., the military increases the public’s evaluation of the war by continuously reporting the increasing enemy body count in addition to continuously reporting the increasing American body count), although Gartner and Myers (1995) and Boettcher and Cobb (2006) suggest that it does. In particular for the latter, Boettcher and Cobb (2006) demonstrates one result: at any moment when the military reports the increasing enemy body count in addition to reporting the American body count for a recent battle during in the war, the public’s evaluation of the war as a whole increases at that moment. Unfortunately, while this result from Boettcher and Cobb (2006) suggests that the solution fixes the problem, it does not demonstrate observationally or experimentally that it indeed does.

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show experimentally that the public’s evaluation of the war increases over time as the military continuously reports the increasing enemy body count along with the increasing American body count over time but that the rate of increase decelerates to zero over time. This paper mostly concerns itself exclusively with public opinion dynamics rather than statics—what happens over time rather than what happens at some moment.

In the remainder of this paper, I present these empirical results and develop a theory that explains them. In particular, I extend the theory of the “elasticity of reality” from Baum and Groeling (2010) and formalize one of its possible underlying mechanisms—and use both to explain my two main empirical results. The elasticity of reality refers to a phenomenon during war where “early in a conflict, typical individuals have limited information [...] Consequently, new information should be relatively influential. Over time, as they acquire new information and their opinions solidify, the influence of additional new information is likely to recede” (Baum and Groeling, 2010, p. 446). The military’s mandatory reporting of the American body count and the enemy body count are such “additional new information” and so their influences on the public’s evaluation of the war recede over time—until perhaps their influence is zero—so that the military’s problem and the candidate solution that I describe here not only exist but also fade out of existence and vanish over the course of a war. So my empirical results in this paper also provide the first support for the theory of elasticity of reality based on experimental data—overcoming many of the problems that Baum and Groeling (2010) suggests occur with the observational data in that study: “testing dynamic patterns in public opinion poses a variety of substantive and methodological challenges. Many factors vary over time, and it is difficult, if not impossible, to account for all potential causal variables” (Baum and Groeling, 2010, p. 446).
2.2 The Event-Response Mechanism

In this section, I develop a theory of how an individual’s evaluation of a war changes at any moment and over time in response to events. I emphasize here that it only considers one mechanism—events—and ignores all others—namely, political elites (for the latter, see Belknap and Campbell, 1951; Zaller, 1992; Larson, 1996; Berinsky, 2007, 2009; Baum and Groeling, 2009b). This is intentional, and for two reasons. First, it allows me clearly explicate the event-response mechanism without distraction from other mechanisms. Second, I test the event-response mechanism in isolation from all other mechanism in my experiment. Future research should consider an integration of all the possible mechanisms present in reality, especially the combination of the event-response mechanism and the elite-driven mechanism.

2.2.1 The Event-Response Mechanism

The event-response literature argues that a citizen’s evaluation of a war changes in response to the occurrence of events over the duration of the war (inter alia, Mueller, 1971, 1973; Lorell et al., 1985; Jentleson, 1992; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Jen; Burk, 1999; Klarevas, 2000; Eichenberg, 2003; Feaver and Gelpi, 2004; Eichenberg, 2005; Gelpi et al., 2006; Boettcher and Cobb, 2006; Fordham, 2008; Gartner, 2008; Gelpi et al., 2009). The claim of the event-response literature consists of three components and assumes that a citizen is rational (see Downs, 1957). First, a citizen gains benefits $b(t)$ from positive events and incurs costs $c(t)$ from the negative events that occur at moment $t$ during the war. Second, the citizen’s utility $u(t)$ at moment $t$ during the war equals the sum $B(t)$ of the benefits that he gains from positive events minus the sum $C(t)$ of the costs he incurs from the negative events that occurred from the beginning at moment 0 up to moment
By definition, \( B(t) = \sum_0^t b(t)dt \) and \( C(t) = \sum_0^t c(t)dt \) in discrete time. Third, the citizen’s evaluation \( v(t) \) of the war at moment \( t \) equals his utility \( u(t) \) at moment \( t \), or \( v(t) = u(t) \), which means that his utility and evaluation of the war are equivalent. These three components of the event-response mechanism together imply that

\[
v(t) = u(t) = B(t) - C(t) = \sum_0^t b(t) - \sum_0^t c(t)
\]

Now consider how the event-response mechanism works. Increasing positive events occur over time during the war, and this implies that the citizen gains an increasing sum of benefits from them over time, or \( B(t) > B(t-1) \) at any moment \( t \). Similarly, increasing negative events occur over time during the war, and this implies that the citizen incurs an increasing sum of costs from them over time, or \( C(t) > C(t-1) \) at any moment \( t \). If during the war the increasing sum of benefits that the citizen gains from positive events exceeds the increasing sum of costs that he incurs from negative events, then his utility and evaluation of the war increase over time. But if during the war the increasing sum of costs that the citizen incurs from negative events exceeds increasing sum of benefits that he gains from positive events, then his utility and evaluation of the war decrease over time. And if during the war the increasing sum of costs that the citizen incurs from negative events equals the increasing sum of benefits that he gains from positive events, then his utility and evaluation of the war do not change over time.

### 2.2.2 An Application

Now consider a particular instance of the event-response mechanism at work. The event-response literature identifies several possible negative and positive events that occur during a war—consider in turn an example of each. First, this literature argues
that a citizen incurs costs from the killings of American soldiers during the war, and they are thus negative events (Mueller, 1971, 1973; Lorell et al., 1985; Larson, 1996; Gartner et al., 1997; Gartner and Segura, 1998; Burk, 1999; Klarevas, 2000; Gartner, 2008). Since the killings of American soldiers increase during the war, the citizen incurs an increasing sum of costs from them, and all else equal (i.e., no other events occur) his utility and evaluation of the war decrease over time. This is the problem at hand, and it is my first hypothesis, stated formally below.

**Hypothesis 1** (Problem Existence). As the American body count increases from moment 0 to moment \( t \) during a war (i.e., as \( C(t) \) increases), all else equal (i.e., no other events occur) a citizen’s evaluation of the war decreases from moment 0 to moment \( t \) (i.e., \( v(t) \) decreases).

Second, this literature argues that a citizen gains benefits from the killings of enemy soldiers during the war, and they are thus positive events (Gartner and Myers, 1995; Boettcher and Cobb, 2006). Since the killings of enemy soldiers increase during the war, the citizen gains an increasing sum of benefits from them, and all else equal (i.e., no other events occur) his utility and evaluation of the war increase over time. This is a possible solution to the problem, and it is my second hypothesis, stated formally below.

**Hypothesis 2** (Solution Existence). As the enemy body count increases from moment 0 to moment \( t \) during a war (i.e., as \( B(t) \) increases), all else equal (i.e., no other events occur) a citizen’s evaluation of the war increases from moment 0 to moment \( t \) (i.e., \( v(t) \) increases).

Third, since the killings of Americans soldiers increase at the same time the killings of enemy soldiers do, the citizen incurs an increasing sum of costs from the former
and an increasing sum of benefits from the latter over time. During the war, if the increasing sum of costs that the citizen incurs from the killings of American soldiers exceeds the increasing sum of benefits that he gains from the killings of enemy soldiers, then all else equal (i.e., no other events occur) his utility and evaluation of the war decrease over time. Here this solution fixes the problem only partially. But during the war, if the increasing sum of benefits that the citizen gains from the killings of enemy soldiers exceeds the increasing sum of costs that he incurs from the killings of American soldiers, then all else equal (i.e., no other events occur) his utility and evaluation of the war increase over time. Here the solution completely fixes the problem. And during the war, if the increasing sum of costs that the citizen incurs from the killings of American soldiers equals the increasing sum of benefits that he gains from the killings of enemy soldiers, then all else equal (i.e., no other events occur) his evaluation of the war does not change over time. Here the solution fixes the problem completely, too. These latter two statements form my third hypothesis, stated formally below.

Hypothesis 3 (Solution Effective). As the American body count increases and the enemy body count increases from moment 0 to moment t during a war (i.e., as \( C(t) \) and \( B(t) \) increase) and the increasing sum of benefits from the latter exceeds the increasing sum of costs from the former (i.e., \( B(t) \geq C(t) \)), all else equal (i.e., no other events occur) a citizen’s evaluation of the war is non-decreasing from moment 0 to moment t (i.e., \( v(t) \) increases or does not change).

2.2.3 Completing the Event-Response Mechanism

I contend that the event-response mechanism as described in the previous section is incomplete. I argue that independent of the relative magnitudes of the increasing
sum of benefits a citizen gains from increasing positive events and the increasing sum of costs he incurs from negative events over time, after some moment during a war his utility and evaluation of the war no longer changes. This implies that the citizen’s utility and evaluation of the war converge to some value at some moment, after which he no longer responds to the occurrence of new events. Is this argument and its implication reasonable?

The argument I make here is nearly parallel to the one Baum and Groeling (2010) make with the concept of “the elasticity of reality” (p. 445; also see Baum and Potter, 2008). According to Baum and Groeling (2010), “as the public gathers more information over time, the potential gap between reality and its representation [...] will likely recede, as will the public’s responsiveness to additional information [...] we refer to this relative change in responsiveness as the ‘elasticity of reality’ ” (p. 445). During a war, as the public gathers more information over time about positive and negative events that occur, the public’s response to additional information recedes. That is, the public gains diminishing benefit from positive events and incurs diminishing costs from negative events until its utility and evaluation of the war no longer change and in fact converge to fixed values. But what is the underlying mechanism that causes this elasticity of reality during a war?

Recall from the previous section that the event-response literature claims that a citizen gains benefits $b(t)$ from positive events and incurs costs $c(t)$ from negative events that occur at some moment $t$ during a war. But the event-response literature does not specify the mechanism that determines the magnitudes $|b(t)|$ and $|c(t)|$ of these benefits and costs at moment $t$, respectively, in a rigorous way. By definition, the mechanism that determines these magnitudes would be the same as the one that causes the elasticity of reality. One basis for this mechanism might be found in Lodge
et al. (1995)—“the online model of information processing” or online (OL) model (also see Lodge et al., 1989).

According to the OL model, as people receive a piece of information about something, first they immediately extract the “affective value” either positive or negative of that received information and add it to their running evaluation of an object—also called their “affective integrator” or online (OL) tally (Lodge et al., 1995, p. 310). And second, they then forget that received information stored in short-term memory but remember their online tally—with their evaluations of that object now updated—stored in long-term memory. The OL model implies that although people forget the received information, they still know their online tally of the object—so that received information continues to impact their evaluations, albeit indirectly. For example, a negative message about a political candidate running for election might decrease people’s running evaluations of him and then eventually be forgotten—but they’ll still know their online tally that has been lowered by this information about him (for an alternative explanation of information processing, see Zaller, 1992; Zaller and Feldman, 1992).

I describe here a simplified and stylized mechanism based on this online (OL) model of information processing—but with some departure from it. First, a citizen knows information set $I(t)$ at moment $t$ that contains indicators for negative and positive events that have occurred up to moment $t$ during a war. In particular, the elements of $I(t)$ are 1 or $-1$ where a 1 indicates a positive event and a $-1$ indicates a negative that has occurred up to moment $t$ during a war. I emphasize here that while the citizen knows indicators for negative and positive events up to a particular time, this doesn’t mean that he remembers anything about these events themselves—he
only tallies the benefits gained and costs incurred from them. For example, \( I(3) = \{1, -1, 1, 1\} \) means that three positive events and one negative event have occurred up to moment \( t = 3 \) during a war. Second, he calculates his benefits gained \( B(I(t)) \) and costs incurred \( C(I(t)) \) from the positive events and negative events, respectively, in this information set \( I(t) \)—and the difference between these two quantities equals his utility \( u(I(t)) \) at moment \( t \). In particular, he calculates his benefits at time \( t \) as the average of the indicators for positive events—and since all of them take the value 1 their average is also 1—weighted by the proportion in the information set \( I(t) \), and he also calculates his costs at time \( t \) as the average of the absolute value of the indicators for negative events—and since all of them take the value \(-1\) this average is also 1—weighted by the proportion of them in the information set \( I(t) \).

Continuing the example, \( B(I(3)) = 0.75 \) and \( C(I(3)) = 0.25 \) so \( u(I(3)) = 0.5 \). Third, his evaluation \( v(t) \) of the war at moment \( t \) equals his utility \( u(I(t)) \) at moment \( t \), or \( v(t) = u(I(t)) \). In particular, his evaluation is a function of his utility, and his utility is a function of his information set, so his evaluation is also a function of his information set, or \( v(I(t)) = v(t) \). Continuing the example, \( v(I(3)) = u(I(3)) = 0.5 \).

These components of the mechanism together imply that

\[
v(I(t)) = u(I(t)) = B(I(t)) - C(I(t)) = \sum_{0}^{t} b(t) - \sum_{0}^{t} c(t)
\]

This equation differs from the one that the event-response mechanism implies because the former specifies a citizen’s evaluation of a war as a function of his information set \( I(t) \) at moment \( t \) while the latter specifies his evaluation as a function of time \( t \) itself.

Now consider how this mechanism actually works in practice. Recall that in extended example, the citizen knows information set \( I(3) = \{1, -1, 1, 1\} \) and calculates

---

\(^2\)I use the term information set here because it parallels a similar concept from game theory (see Mas-Colell et al., 1995).
his benefits $B(I(3)) = 0.75$ and costs $C(I(3)) = 0.5$ and thus his utility and evaluation of the war as $v(I(3)) = u(I(3)) = 0.5$. What happens when a single new positive event occurs here? Say this single new positive event occurs by moment $t = 4$ during the war. First, now the citizen knows information set $I(4) = \{1, -1, 1, 1, 1\}$ so that four positive events and one negative event have occurred up to moment $t = 4$. Second, now the citizen calculates his benefits $B(I(4)) = 0.8$ equal to the average of the indicators for positive events weighted by the proportion of them in information set $I(4)$. And now he calculates his costs $C(I(4)) = 0.2$ equal to the average of the indicators for negative events weighted by the proportion of them in information set $I(4)$. Third, now he calculates his utility and evaluation of the war $v(I(4)) = u(I(4)) = 0.6$ equal to his benefits $B(I(4))$ less his costs $C(I(4))$. So this new positive event from moment $t = 3$ to moment $t = 4$ during the war causes his evaluation to increase by 0.1 from $v(I(3)) = 0.5$ to $v(I(4)) = 0.6$.

But what happens when *yet another* single new positive event occurs? Say this single new positive event occurs by moment $t = 5$ during the war. Following the same steps as the previous paragraph, now he knows information set $I(5) = \{1, -1, 1, 1, 1\}$, he calculates his benefits $B(I(5)) = 0.67$ and costs $C(I(5)) = 0.17$, and his utility and evaluation of the war are $v(I(5)) = u(I(5)) = 0.67$. So this new positive event from moment $t = 4$ to moment $t = 5$ during the war causes his evaluation to increase by 0.07 from $v(I(4)) = 0.6$ to $v(I(5)) = 0.67$. But recall that before this a new positive event from moment $t = 3$ to moment $t = 4$ causes his evaluation to increase by 0.1 from $v(I(3)) = 0.5$ to $v(I(4)) = 0.6$. Here, a positive event occurring at an earlier moment during a war—when fewer event have already occurred—has a greater impact on a citizen’s evaluation than a positive event occurring at a later moment—when more events have already occurred. An analogous argument applies to negative events.
What is going on here? As a citizen’s information set contains more indicators for events that occur during a war, he averages over more of them in calculating his costs and benefits and ultimately his utility and evaluation. Mathematically, this means that the impacts of new events on his costs and benefits must eventually diminish to zero—and this means that his utility and evaluation of the war eventually converge to some fixed value and never change again. Below I prove this claim for the simplified and stylized version of the mechanism presented here—although the claim and its intuition are certainly robust to other versions.

**Lemma 1.** For \( x, y > 0 \),

\[
\frac{x + 1}{y + 1} - \frac{x}{y} > \frac{x + 2}{y + 2} - \frac{x + 1}{y + 1}
\]

*Proof.* Note that \( \frac{x + 1}{y + 1} - \frac{x}{y} = \frac{y - x}{y^2 + y} \) and \( \frac{x + 2}{y + 2} - \frac{x + 1}{y + 1} = \frac{y - x}{y^2 + 3y + 2} \). But since \( y^2 + 3y + 2 > y^2 + y \), then \( \frac{y - x}{y^2 + y} > \frac{y - x}{y^2 + 3y + 2} \) and the conclusion follows. \( \square \)

**Proposition 1** (Convergence from the Elasticity of Reality). *If the mechanism as described above holds, then a citizen’s evaluation \( v(I(t)) \) of a war converges to some fixed value over time, or \( v(I(t)) \to v^* \).*

*Proof.* Let \( I_B(t) \subset I(t) \) be the set of all indicators for positive events in information set \( I(t) \). Thus by definition \( B(I(t)) = \frac{|I_B(t)|}{|I(t)|} \). Now choose \( t, t - 1, t - 2 \) such that \( |I_B(t)| = |I_B(t - 1)| + 1 \) and \( |I_B(t - 1)| = |I_B(t - 2)| + 1 \). Note that \( b(t - 1) = B(I(t - 1)) - B(I(t - 2)) = \frac{|I_B(t - 1)|}{|I(t - 1)|} - \frac{|I_B(t - 2)|}{|I(t - 2)|} = \frac{|I_B(t - 2)| + 1}{|I(t - 2)| + 1} - \frac{|I_B(t - 2)|}{|I(t - 2)|} \) and that \( b(t) = B(I(t)) - B(I(t - 1)) = \frac{|I_B(t)|}{|I(t)|} - \frac{|I_B(t - 1)|}{|I(t - 1)|} = \frac{|I_B(t - 1)| + 2}{|I(t - 1)| + 2} - \frac{|I_B(t - 2)| + 1}{|I(t - 2)| + 1} \). Thus by Lemma 1 \( b(t) < b(t - 1) \) for all \( t \) and a similar argument shows that \( c(t) < c(t - 1) \) for all \( t \). Thus \( b(t) \to 0 \) and \( c(t) \to 0 \). Thus \( B(t) \to B^* \) and \( C(t) \to C^* \). Thus \( v(t) \to B^* - C^* = v^* \). \( \square \)
The logic of the preceding explanation implies two things. First, while the citizen gains an increasing sum of benefits and incurs an increasing sum of costs from increasing positive and negative events during the war, or $B(t) > B(t - 1)$ and $C(t) > C(t - 1)$, respectively, he does so at a decreasing rate, or $b(t) < b(t - 1)$ and $c(t) < c(t - 1)$ at any moment $t$, respectively, because of the elasticity of reality. And second, after some moment during the war, this process does not happen at all, or $b(t) \to 0$ and $c(t) \to 0$, because of the elasticity of reality. These two things together imply that the sum of the benefits that the citizen gains from positive events and the sum of the costs that he incurs from negative events no longer changes after some moment, both converging to some values, or $B(t) \to B^*$ and $C(t) \to C^*$. And this further implies that the citizen’s evaluation of the war no longer changes after this moment, converging to some value, or $v(t) \to B^* - C^* = v^*$. Under the logic of this section then, my argument makes sense, completing the event-response mechanism.

This completed event-response mechanism suggests that the effect of events on the individual’s evaluation of the war is greatest at its onset and declines over its duration until they have none at all because of the elasticity of reality. And this suggests that the citizen’s evaluation of the war converges to some value at some moment during the war and no longer changes afterward. While the first suggestion is valid in any context, I emphasize again here that this second suggestion is valid when the event-response mechanism is the only one in motion—and in reality this is not the case, although it certainly is in the context of my experiment. In particular, the elite-driven mechanism would predict that political elites like Congress and the President affect the individual’s evaluation of the war simultaneously with events—and the former would continue to do so after the effectiveness of the latter fades.
2.2.4 A Reapplication

Now consider the previous particular instance of the complete event-response mechanism at work. First, recall that since the killings of American soldiers are negative events, as they increase, a citizen incurs an increasing sum of costs from them, and \textit{all else equal} (i.e., no other events occur) a citizen’s utility and evaluation of the war decrease over time. But by the logic of the previous section and because of the elasticity of reality, this process happens at a decreasing rate. This means the problem at hand vanishes over time, and it is my fourth hypothesis, stated formally below.

\textbf{Hypothesis 4 (Vanishing Problem).} As the American body count increases from moment 0 to moment \( t \) during a war (i.e., as \( C(t) \)), \textit{all else equal} (i.e., no other events occur) a citizen’s evaluation of the war decreases at a decreasing rate from moment 0 to moment \( t \) (i.e., \( v(t) \) decreases at a decreasing rate).

Second, recall that since the killings of enemy soldiers are positive events, as they increase, the citizen incurs an increasing sum of benefits from them, and \textit{all else equal} (i.e., no other events occur) a citizen’s utility and evaluation of the war increase over time. But by the logic of the previous section and because of the elasticity of reality, this process happens at a decreasing rate. This means the solution vanishes over time, and it is my fifth hypothesis, stated formally below.

\textbf{Hypothesis 5 (Vanishing Solution).} As the enemy body count increases from moment 0 to moment \( t \) during a war (i.e., as \( B(t) \) increases), \textit{all else equal} (i.e., no other events occur) a citizen’s evaluation of the war increases at a decreasing rate from moment 0 to moment \( t \) (i.e., \( v(t) \) increases at a decreasing rate).

\footnote{This is similar to the prediction of Mueller (1971) that the public’s support is inversely related to the logarithm of cumulative casualties over time.}
Third, since by the logic of the previous section and because of the elasticity of reality after some moment during the war, these two processes do not happen at all after this moment and a citizen’s utility and evaluation do not change for the war’s remainder. This means not only that the problem and its solution fully vanish, but that the citizen’s evaluation of the war converges to some value at some moment and does not change afterward. This is my sixth hypothesis, stated formally below.

**Hypothesis 6 (Convergence).** A citizen’s evaluation of a war converges to some value at some moment $t^*$ (i.e., $v(t)$ converges to $v^* = B^* - C^*$) and remains fixed for the remainder of the war.

### 2.3 A Panel Experiment

I obtain data useful for testing these six hypotheses through a so-called panel experiment, a research design first coined and developed in Gartner (2008). I used Amazon’s Mechanical Turk to recruit a nonprobability sample of 420 subjects to complete “a 23-question opinion survey on U.S. military action abroad” and used Qualtrics to collect their responses from June 26, 2012 through July 3, 2012. I obtained 348 responses in total because I eliminated the responses of subjects that failed to pass an instructional manipulation check. This restriction does not significantly change the results in this study.footnote{In the panel experiment the instructional manipulation check consisted of a statement a question that all subjects received. The statement was: “Recent research on decision making shows that choices are affected by context. Differences in how people feel, their previous knowledge and experience, and their environment can affect choices. To help us understand how people make decisions, we are interested in information about you. Specifically, we are interested in whether you actually take the time to read the directions; if not, some results may not tell us very much about decision making in the real world. To show that you have read the instructions, please ignore the question on the next slide and instead choose only the “successful” option as your answer.” The question following was: “Some people believe that the U.S. has been successful in its operations in Afghanistan, while others believe we have not been so successful. How do you feel about the progress the U.S. has made in Afghanistan?” The question had five possible responses: “very unsuccessful,” “unsuccessful,” “evenly mixed,” “successful,” and “very successful.”} Recent research argues that the results of survey experiments...
with subject samples collected via Mechanical Turk are both internally and externally valid for answering research questions emerging in political science (Berinsky et al., 2012) as well as in economics and psychology (Buhrmester et al., 2011; Horton et al., 2011). Particularly with respect to generalizability, Berinsky et al. (2012) shows that samples recruited via Mechanical Turk are more representative than undergraduate convenience samples but only slightly less representative than internet and national probability samples (also see Ipeirotis, 2010; Ross et al., 2010; Paolacci et al., 2010). I paid $0.10 to each subject upon completion of the panel experiment. Buhrmester et al. (2011) shows that the payment amount does not affect the quality of subjects’ responses (also see Mason and Watts, 2009; Chen and Horton, 2010; Horton and Chilton, 2010).

A panel experiment randomly assigns subjects to two or more control and treatment groups which receive different treatments and then complete the same post-treatment measures just like a typical experiment. However, a panel experiment differs from a typical experiment in the way subjects receive treatment and complete the measures. In a panel experiment, subjects receive a treatment consisting of a sequence of survey panels which contain content and complete the same measures on or after each survey panel in the sequence. But in a typical experiment—subjects receive a treatment on a single survey panel which contains content and then complete the same measures on or after this one survey panel. Both a panel experiment and a typical experiment takes place in a single setting and subjects are not recontacted again. Again, Gartner (2008) coins the term “panel experiment” in contradistinction to a typical experiment, so I continue to use it here and note that a panel experiment does indeed produce panel data: it observes the same respondents with the same measures at different points in time—although in a single setting in which time is marked by sequential panels instead of sequential recontacts.
Consider the panel experiments in Gartner (2008) as an example. First, subjects in each treatment group receive a sequence of ten survey panels that each report the American body count for a particular month and the cumulative American body count up to that month inclusive in a ten-month hypothetical war. As Gartner (2008) describes: “Subjects are presented with a hypothetical U.S. military scenario. […] Subjects then view newspaper-like reports (text and graph) of the resulting monthly and cumulative U.S. casualties. […] There are ten potential panels” (p. 100–101). Second, at each survey panel (corresponding to a particular month), subjects answer a question in which they “choose whether the United States should continue to employ force” with the two possible responses of “yes” and “no” where “[a] ‘no’ answer leads to exit questions and termination. A ‘yes’ answer leads to another casualty report and opportunity to choose whether to support or oppose the operation” (p. 101). Third, the monthly pattern of American body count accumulation varies across treatment groups. Again, as Gartner (2008) describes: “the figures presented in the report depend on the subject’s randomly assigned casualty group. Each group has a unique […] casualty pattern—that is, each group provides subjects with a different casualty experience.” Altogether, this allows the author to infer the causal effect of a particular monthly pattern of American body count accumulation on when a subject chooses to end the hypothetical war in question.

In this study, I run a panel experiment that differs in format from the one in Gartner (2008) in three ways. First, subjects in each treatment group receive a sequence of ten survey panels corresponding to a ten-year hypothetical war that each report either just the American body count for a particular year and the cumulative American body count up to that year inclusive, just the enemy body count for a particular year and the cumulative enemy body count up to that year inclusive, or both the American and enemy body counts up to that year inclusive. This differs
from Gartner (2008) in that subjects might receive the per period and cumulative enemy body either alone or together with the per period and cumulative American body count instead of just the per period and cumulative American body count alone (and further panels correspond to a particular year of war instead of just a particular month). Second, at each survey panel (corresponding to a particular year), subjects answer a question in which they “grade the progress the U.S. has made in the country so far” with eleven possible responses on an ordered scale from “F” to “A+.” Again, this differs from Gartner (2008) in that subjects rate the war’s progress on a continuous scale instead of indicating whether they want to continue the war or not in a binary way (and further subjects see all ten survey panels instead of exiting the survey based on their prior answers). Third, the yearly patterns of American body count accumulation and enemy body count accumulations does not vary across treatment groups. Instead, whether either or both of these yearly patterns of American body count accumulation and enemy body count accumulation is received or not varies across treatment groups. And again, this differs from Gartner (2008) in that subjects see different types of body counts accumulations—American, enemy, or both—in the same patterns instead of different patterns of the same type of body count accumulations—just American. Altogether, this allows me to infer the causal effect of the fixed yearly pattern of American body count accumulation, the fixed yearly pattern of enemy body count accumulation, or both on how a subject grades the progress the United States has made over time at any of the ten particular years in the hypothetical war in question. Unlike Gartner (2008), my study does not investigate the causal impact of treatments on when a subject chooses to end a war, but something else: the subjects’ grading of the United States’ progress in the war at any of its ten particular years. In sum, the panel experiments in this study and in Gartner (2008) differ in terms of their treatments, measures, and causal impacts under investigation.
It is also worth discussing how the “cross-sectional” experiment in Boettcher and Cobb (2006) differs from the panel experiments in this study and in Gartner (2008). First, in Boettcher and Cobb (2006), subjects in each treatment group receive a single survey panel that reports just the American body count, just the enemy body count, or both the American and enemy body counts for a particular hypothetical battle during the War in Iraq. As the authors describe: “we created five mock *New York Times* articles about a military operation in Iraq. Each article was identical, except for a sentence describing American, insurgent, American and insurgent, or American and terrorist dead” (p. 842). By contrast, panel experiments in Gartner (2008) and in this study have multiple survey panels that report various per period and cumulative body counts corresponding to various periods of time during an entire war. Second, in Boettcher and Cobb (2006), at a single survey panel subjects answer a question in which they indicate how “they feel about the progress the U.S. has made in Iraq after the war ended” in its postwar operations with the five possible responses of “very unsuccessful,” “somewhat unsuccessful,” “evenly mixed,” “somewhat successful”, and “very successful” (p. 851). Again by contrast, the panel experiment in this study has subjects answer a similar question but instead by giving a rating on an 11-point continuous and ordered scale on multiple survey panels, while the panel experiment in Gartner (2008) has subjects answer an entirely different question in a binary way on multiple survey panels. Third, in Boettcher and Cobb (2006), whether either or both American casualties and enemy casualties during a particular hypothetical battle during the War in Iraq are reported varies across treatment groups. By contrast, panel experiments in Gartner (2008) and in this study vary the patterns of the per period and cumulative body counts over time during a war. Altogether, this allows the authors to infer the causal effect of the American body count, the enemy body count, or both body counts on how a subject grades the progress the United States
has made since the end of the War in Iraq in its postwar operations in that country at a particular point in time. So the experiments in this study, in Gartner (2008), and in Boettcher and Cobb (2006) differ in terms of treatments, measures, and the causal impacts under investigation—which I summarize in the table below.

<table>
<thead>
<tr>
<th></th>
<th>Type</th>
<th>Treatments</th>
<th>Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td>This Study</td>
<td>10-panel</td>
<td>American/enemy/both body counts</td>
<td>Progress in war</td>
</tr>
<tr>
<td>Gartner (2008)</td>
<td>10-panel</td>
<td>Different American body counts</td>
<td>Continuing war</td>
</tr>
<tr>
<td>Boettcher &amp; Cobb (2006)</td>
<td>1-panel</td>
<td>American/enemy/both body counts</td>
<td>Progress in war</td>
</tr>
</tbody>
</table>

Table 2.1. Differences across related studies. Two studies related to this one—Gartner (2008) and Boettcher & Cobb (2006)—have experiments that differ from the one in this study in terms of type of experiment, treatments, and measures, and altogether the causal effects under investigation.

Now consider how I implemented the panel experiment in the present study. Subjects advanced through ten survey panels. On the introductory panel, all subjects received the following statement about a hypothetical use of force based on the War in Iraq and instructions: “It is now the year 2013, and U.S. armed forces invade a country in the central Middle East. The following panels describe some events that take place during this conflict. At each panel, please grade the progress that the U.S. has made in this country up to that point using the slider, where A+ is the best grade and F is the worst grade.” Each of the following nine survey panels corresponds to exactly one of the nine years of the scenario from 2013 through 2021 in chronological order. On each of these survey panels, all subjects received a statement about the scenario’s events during the corresponding year along with the question: “How would you grade the progress the U.S. has made in this country so far?” which all subjects then answered with one of thirteen possible responses between “A+” and “F.”

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5On these panels, all subjects received the statements “in 2013, U.S. forces overwhelm and disband the country’s army and remove its dictatorial regime,” “in 2014, insurgents begin an extended campaign of killing, kidnapping, and beheading civilians and military personnel,” “in 2015, U.S. forces install a democratic regime in the country,” “in 2016, U.S. forces kill the leader of the insurgents,” “in 2017, the U.S. deploys additional troops to the country in order to stabilize violence around its capital city,” “in 2018, the U.S. hands over security responsibilities for one of the coun-
example, on the fifth panel corresponding to the year 2017, all subjects received the one additional statement: “In 2017, the U.S. sends additional troops to the country in order to stabilize violence around its capital city.”

The panel experiment randomly assigned subjects to either a control group or one of three treatment groups. Subjects in the control group received on each survey panel only a statement about the scenario’s non-casualty events during the corresponding year during the use of force. For example, on the fifth panel corresponding to the year 2017, subjects in the control group received the statement: “In 2017, the U.S. deploys additional troops to the country in order to stabilize violence around its capital city.” Subjects in the first treatment group additionally received on each survey panel the cumulative American body count $X$ at the corresponding year given by the statement: “At this point, a total of $X$ U.S. troops have been killed.” For example, on the fifth panel corresponding to the year 2017, subjects in the first treatment group receive the statement: “In 2017, the U.S. deploys additional troops to the country in order to stabilize violence around its capital city. At this point, a total of 3,908 U.S. troops have been killed.” Subjects in the second treatment group additionally received on each panel the cumulative enemy body count $Y$ at the corresponding year given by the statement: “At this point, a total of $Y$ enemy insurgents have been killed.” For example, on the fifth panel corresponding to the year 2017, subjects in the second treatment group received the statement: “In 2017, the U.S. deploys additional troops to the country in order to stabilize violence around its capital city. At this point, a total of 21,300 enemy insurgents have been killed.” Subjects in the third treatment try’s most peaceful provinces to the country’s own military,” “in 2019, U.S. forces withdraw from the country’s capital and other major cities,” “in 2020, the U.S. officially ends combat operations in the country,” “in 2021, the last U.S. forces leave the country,” respectively.

6On these panels in chronological order, $X$ takes on the values 486; 1,335; 2,181; 3,004; 3,908; 4,222; 4,371; 4,431; and 4,485; respectively.

7On these panels in chronological order, $Y$ takes on the values 603; 7,404; 10,651; 14,553; 21,300; 23,328; 23,816; 24,492; and 24,943; respectively.
group additionally received on each panel both the cumulative American body count $X$ and the cumulative enemy body count $Y$ at the corresponding year given by the statement: “At this point, a total of $X$ U.S. troops have been killed, and a total of $Y$ enemy insurgents have been killed.” For example, on the fifth panel corresponding to the year 2017, subjects in the third treatment group received the statement: “In 2017, the U.S. deploys additional troops to the country in order to stabilize violence around its capital city. At this point, a total of 3,908 U.S. troops have been killed, and a total of 21,300 enemy insurgents have been killed.”

Summarily, the panel experiment has a $2 \times 2$ fully crossed design. The first factor is no exposure or exposure on each survey panel to the cumulative American body count at the corresponding year, and the second factor is no exposure or exposure on each survey panel to the cumulative enemy body count at the corresponding year. These two factors result in a control condition and three treatment conditions: exposure to neither cumulative body count, called “Neither Cumulative Body Count” or “C”; exposure to only the cumulative American body count, called “American Cumulative Body Count” or “T1”; exposure to only the cumulative enemy body count, called “Enemy Cumulative Body Count” or “T2”; and exposure to both cumulative body counts, called “Both Cumulative Body Counts” or “T3.”

<table>
<thead>
<tr>
<th>No Exposure</th>
<th>American C.B.C.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Enemy C.B.C.</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Neither C.B.C. (C)</td>
</tr>
<tr>
<td></td>
<td>Only Enemy C.B.C. (T2)</td>
</tr>
</tbody>
</table>

**Table 2.2. The design of the panel experiment.** The panel experiment has a $2 \times 2$ fully crossed design. The first factor is no exposure or exposure on each survey panel to the cumulative American body count at the corresponding year, and the second factor is no exposure or exposure on each panel to the cumulative enemy body count at the corresponding year. These two factors result in a control condition and three treatment conditions: no exposure to either cumulative body count, called “Neither” or “C”; exposure to only the cumulative American body count, called “Only American Cumulative Body Count” or “T1”; exposure to only the cumulative enemy body count, called “Only Enemy Cumulative Body Count” or “T2”; and exposure to both cumulative body counts, called “Both Cumulative Body Counts” or “T3.”
2.4 Analysis

I test the hypotheses using data obtained from the panel experiment described in the previous section.

2.4.1 Variable Coding

Control Variables

I code in a standard way four control variables that indicate a subject’s partisan identification, sex, highest level of education attained, and political knowledge, given by \textit{Sex}, \textit{Education}, and \textit{Knowledge}, respectively.

Treatment Variables

I code three variables that indicate a subject’s treatment group assignment in the panel experiment. First, if the survey instrument assigned the subject to the first treatment group (T1), where he received on each panel the cumulative American body count at the corresponding year, the variable \textit{American} takes on the value 1 but 0 otherwise. Second, if the survey instrument assigned the subject to the second treatment group (T2), where he received on each panel the cumulative enemy body count at the corresponding year, the variable \textit{Enemy} takes on the value 1 but 0 otherwise. Third, if the survey instrument assigned the subject to the third treatment group (T3), where he received on each panel both the cumulative American body count and the cumulative enemy body count at the corresponding year, the variable \textit{Both} takes on the value 1 but 0 otherwise. Under this coding, if the survey instrument assigned the subject to the control group (C), \textit{American}, \textit{Enemy}, and \textit{Both} all take on the value 0.
**Dependent Variables**

I code nine dependent variables. Each dependent variable $\text{Grade } T$ corresponds to exactly one panel’s question, where $T$ is that panel’s corresponding year, of which there are nine. For example, the dependent variable $\text{Grade } 2017$ corresponds to the question on the fifth panel with corresponding year 2017. If a subject’s answer is “A+” then $\text{Grade } T$ takes on the value 13. If the subject’s answer is “A” then $\text{Grade } T$ takes on the value 12. This pattern continues, so that if the subject’s answer is “F,” then $\text{Grade } T$ takes on the value 1.

**2.4.2 Estimation**

I perform two methods of analysis on the data obtained from the panel experiment. In the first method, I perform five sets of nine Welch unequal variances t-tests. The first set tests the difference in the mean of $\text{Grade } T$ between those subjects assigned to the control group (C) and those assigned to the first treatment group (T1) for all $T$. The second set tests the difference in the mean of $\text{Grade } T$ between those subjects assigned to the control group (C) and those assigned to the second treatment group (T2) for all $T$. The third set tests the difference in the mean of $\text{Grade } T$ for those subjects assigned to the first treatment group (T1) and those assigned to the third treatment group (T3) for all $T$. The fourth set tests the difference in the mean of $\text{Grade } T$ for those subjects assigned to the second treatment group (T2) and those assigned to the third treatment group (T3). The fifth set tests the difference in the mean of $\text{Grade } T$ between those subjects assigned to the control group (C) and those assigned to the third treatment group (T3). In the second method, I estimate an ordinary least squares regression model for $\text{Grade } T$ for all $T$. Both methods
yield approximately identical results. Therefore, I only present the results of the first method, which are the easier to understand for the general reader.

2.5 Results

In this section, I briefly present the results of the analysis described in the previous section. First consider Figure 2.1b, which plots for each survey panel and corresponding year the cumulative American body count (given by A) and the cumulative enemy body count (given by E). Both the cumulative American body count and the cumulative enemy body count continuously increase simultaneously over all of the nine panels and corresponding years, analogous to the casualty pattern during the War in Iraq. This result indicates that the necessary conditions for all six hypotheses are met.

Now consider Figure 2.1a, which plots for each survey panel and corresponding year the mean grade of subjects assigned to the control group (C) (given by C), those assigned to the first treatment group (T1) who additionally received on each survey panel the cumulative American body count at the corresponding year (given by A), those assigned to the the second treatment group (T2) who additionally received on each survey panel the cumulative enemy body count at the corresponding year (given by E), and those assigned to the third treatment group (T3) who additional received on each survey panel both body counts at the corresponding year (given by B).

Unfortunately, the result of the experiment for the second survey panel corresponding to the year 2014 is particularly anomalous with subjects in the control group (C) giving a lower mean grade than those in any of the treatment groups (T1, T2, and T3). I present one possible explanation. The statement about the scenario’s non-casualty events during the second year on this second survey panel is relatively
negative: “In 2014, insurgents begin an extended campaign of killing, kidnapping, and beheading civilians and military personnel.” In receiving only this statement about strongly negative non-casualty events, subjects in the control group lack context and a frame of reference that any information about negative or positive casualty events provide and so their response is volatile and has no ballast. On the other hand, in receiving this statement about strongly negative non-casualty events, subjects in any of the treatment groups have context and a frame of reference that information about negative and positive casualty events provide and so their response is stable and has a ballast. This explanation is speculative but reasonable, and regardless of its merit, the following results still emerge from inspection when disregarding first and second survey panels corresponding to the years 2013 and 2014, and I confirm all of them formally in the next section.

Problem Existence & Vanishing Problem

Consider evidence for the problem existence (1) and vanishing problem (4) hypotheses. First, the problem existence (1) hypothesis predicts that receiving the cumulative American body count during a war will decrease a citizen’s evaluation of the war. Second, the vanishing problem (4) hypothesis predicts that receiving the cumulative American body count during a war will decrease a citizen’s evaluation of the war at a decreasing rate. Consistent with both of these hypotheses, receiving the cumulative American body count at the third survey panel corresponding to the year 2015 decreases subjects’ mean grade from B to C and at all subsequent survey panels corresponding to later years similarly decreases subjects’ mean grade by one full letter. In this case, the decreasing rate at which a citizen’s evaluation of the war decreases over time drops to zero instantaneously at the third survey panel corresponding to the year 2015.
**Solution Existence & Vanishing Solution**

Consider evidence for the solution existence (2) and vanishing solution (5) hypotheses. First, the solution existence (2) hypothesis predicts that receiving the cumulative enemy body count during a war will increase a citizen’s evaluation of the war. Second, the vanishing solution (5) hypothesis predicts that receiving the cumulative enemy body count during a war will increase a citizen’s evaluation of the war at a decreasing rate. Inconsistent with both of these hypotheses, receiving the cumulative enemy body count at the third survey panel corresponding to the year 2015 nor at any subsequent survey panels corresponding to later years changes subjects’ mean grade. Prima facie, this result suggests that Boettcher and Cobb (2006) proposes a solution that does not work: the military cannot increase the public’s evaluation of a war by reporting the increasing enemy body count. But Boettcher and Cobb (2006) would disagree with this suggestion: it is possible that the military can increase the public’s evaluation of a war by reporting the increasing enemy body count but only in addition to reporting the increasing American body count—the efficacy of the former requiring the context of the latter. The next result evaluates this claim and provides a better test of the claim of Boettcher and Cobb (2006).

**Effective Solution & Convergence**

Consider evidence for the solution effective (3) and convergence (6) hypotheses. First, the solution existence (3) hypothesis predicts that receiving the cumulative enemy body count along with the cumulative American body count will increase a citizen’s evaluation of the war compared to receiving the cumulative American body count without the cumulative enemy body count. Second, the convergence hypothesis (6) predicts that citizens’ evaluation of the war will converge to some value over time.
Consistent with both of these hypotheses, receiving the cumulative enemy body count along with the cumulative American body count at the third survey panel corresponding to the year 2015 increases subjects’ mean grade from C to B- and at all subsequent survey panels corresponding to later years similarly increases subjects’ mean grade by two-thirds of one full letter with convergence to an approximate mean grade of B-. This result suggests that Boettcher and Cobb (2006) proposes a solution that does indeed work and requires two pieces of information that contextualize each other: the military can increase the public’s evaluation of a war by reporting the increasing enemy body count but only in addition to the increasing American body count.

2.5.1 Formal Tests

In this section, I present the results of my analysis rigorously. Each of the figures plots the difference in mean grades between subjects assigned to two different experimental groups at each of the thirteen panels and corresponding years, accompanied by ninety percent confidence bands from Welch unequal variance t-tests. At a given panel and corresponding year, confidence bands for the difference in mean grades that do not cross zero indicate a statistically significant difference. For any two panels and corresponding years, confidence bands for the difference in mean grades that overlap indicate no statistically significant change in the difference in mean grades from the first panel and corresponding year to the second one. The variable Grade \( T \) measures a subject’s mean grade for a war on a 13-point scale of integers from “1” (equivalent to “F”) to “13” (equivalent to “A+”), inclusive, with a greater value corresponding to a higher grade.
Figure 2.1. Mean Grade and Cumulative Body Counts. Subfigure (a) plots for each survey panel and corresponding year the mean grade of subjects assigned to the control group (C) (given by C), those assigned to the first treatment group (T1) who additionally received on each survey panel the cumulative American body count at the corresponding year (given by A), those assigned to the second treatment group (T2) who additionally received on each survey panel the cumulative enemy body count at the corresponding year (given by E), and those assigned to the third treatment group (T3) who additional received on each survey panel both body counts at the corresponding year (given by B). Subfigure (b) plots for each panel and corresponding year the cumulative American body count (given by A) and the cumulative enemy body count (given by E).

Problem Existence & Vanishing Problem

Consider Figure 2.2a, which plots for each of the thirteen panels and corresponding years the difference in mean grades between subjects assigned to the control group (C) and subjects assigned to the first treatment group (T1) who additionally received
on each survey panel the cumulative American body count at the corresponding year. At the third survey panel corresponding to the year 2015, receiving the cumulative American body count decreases subjects’ mean grade relative to the control condition. However, at subsequent survey panels and corresponding years over which the American body count increases, receiving the cumulative American body count does not decrease subjects’ mean grade any further. These results are consistent with the problem existence (1) and vanishing problem (4) hypotheses.

Solution Existence & Vanishing Solution

Consider Figure 2.3a, which plots for each of the thirteen panels and corresponding years the difference in mean grades between subjects assigned to the control group (C) and subjects assigned to the second treatment group (T2) who additionally received on each panel the cumulative enemy body count at the corresponding year. With the exception of the second survey panel corresponding to the year 2014, at all survey panels and corresponding years, receiving the cumulative enemy body count does not change subjects’ mean grade relative to the control condition. This result is inconsistent with both the solution existence (2) and vanishing solution (5) hypotheses.

Effective Solution & Convergence

Consider Figure 2.2b, which plots for each of the thirteen panels and corresponding years the difference in mean grades between subjects assigned to the first treatment group (T1) who additionally received on each survey panel the cumulative American body count at the corresponding year and subjects assigned to the third treatment group (T3) who additionally received on each survey panel both the cumulative American body count and the cumulative enemy body count at the corresponding year.
Figure 2.2. Neither (C) to Only American (T1) to Both (T3). Subfigure (a) plots for each survey panel and corresponding year the difference in the mean grade (where each difference of 1-point equates to a difference in one step on the ordered scale between “F” and “A+”) between subjects assigned to the control group (C) and subjects assigned to the first treatment group (T1) who additionally received on each survey panel the cumulative American body count at the corresponding year. Subfigure (b) plots for each survey panel and corresponding year the difference in the mean grade between subjects assigned to the first treatment group (T1) and subjects assigned to the third treatment group (T3) who additionally received on each survey panel both the cumulative American body count and the cumulative enemy body count at the corresponding year. Ninety percent confidence bands from a Welch unequal variance t-test.

At the third panel corresponding to the year 2015, receiving the cumulative enemy cumulative body count along with the American cumulative body count increases subjects’ mean grade relative to receiving just the American body count. However, at subsequent survey panels and corresponding years over which both the American
Figure 2.3. Neither (C) to Only Enemy (T2) to Both (T3). Subfigure (a) plots for each survey panel and corresponding year the difference in the mean grade where each difference of 1-point equates to a difference in one step on the ordered scale between “F” and “A+”) between subjects assigned to the control group (C) who did not receive anything else and subjects assigned to the second treatment group (T2) who additionally received on each survey panel the cumulative enemy body count at the corresponding year. Subfigure (b) plots for each panel and corresponding year the difference in the mean grade between subjects assigned to the second treatment group (T2) and subjects assigned to the third treatment group (T3) who additionally received on each survey panel both the cumulative American body count and the cumulative enemy body count at the corresponding year. Ninety percent confidence bands from a Welch unequal variance t-test.

body count and the enemy body count increase, receiving the enemy body count along with the American body count does not increase subjects’ mean grade any further. These results are consistent with the effective solution (3) and convergence (6) hypotheses.
Now consider figure 2.4, which plots for each of the thirteen survey panels and corresponding years the difference in mean grades between subjects assigned to the control group (C) and subjects assigned to the third treatment group (T3) who additionally received on each survey panel both the cumulative American body count and the cumulative enemy body count at the corresponding year. With the exception of the second and fifth panels corresponding to the years 2014 and 2017, respectively, at all other panels and corresponding years, receiving the cumulative enemy body count along with the cumulative American body count decreases subjects’ mean grade relative to the control condition. The previous result indicates that receiving the cumulative enemy body count along with the cumulative American body count increases subject’ mean grade relative to receiving just the American body count. The present result indicates that receiving the enemy body count along with the cumulative body count decreases subjects’ mean grade relative to the control condition. This last result does not support a particular hypothesis. Rather, it shows that the solution is effective but does not completely solve the problem—subjects’ give the highest mean grades when American casualties are not reported, the second-highest when American casualties are reported along with enemy casualties, and the lowest when American casualties are reported alone.

### 2.6 Discussion & Conclusion

In this section, I present the three major findings from my analysis. Recall that my goal in this paper is similar to that of Gartner (2008): to elucidate the micro foundations of public opinion during war. Gartner (2008) states that “reexamining the same historical wartime surveys is unlikely to generate a better understanding of the micro-foundations of wartime behavior” and instead uses survey experiments in
the context of hypothetical wars to do so. I have done the same. But in the process, both Gartner (2008) and I have traded off external validity for internal validity in the pursuit of our research objectives. This bargain struck limits our insight into how our results translate into the real world—a question for future research—but I take a stab at it in this section.

First, the problem in question exists but is limited. The military must report the ever-increasing American body count continuously, but doing so does not continuously decrease the public’s evaluation of a war (relative to the public’s evaluation when the military reports nothing else). Rather, the public’s evaluation decreases in response to one of the military’s first reports on the American body count, but subsequent reports do not decrease it any further (relative to the public’s evaluation when the military reports nothing else). In the panel experiment, receiving the cumulative American body count at the third panel corresponding to the year 2015 decreases subjects’ mean grade by approximately one full letter grade (e.g., from B to C) relative to not
receiving anything else. However, receiving the cumulative American body count at subsequent panels and corresponding years does not decrease subjects’ mean grade any further than this—the decrease is always one full letter grade. This suggests that the public’s evaluation of a war decreases in response to one of the military’s first reports on the American body count, but subsequent reports do not decrease it any further. This finding is not only consistent with the theory presented in this paper but also nearly consistent with one of the earliest theories on the impact of increasing American casualties from the 1970s: the “log of casualties” theory. Mueller (1971, 1973) claims that the public’s war support is decreasing in the logarithm of the cumulative American casualties. But this claim implies by the nature of the logarithm function that the marginal negative impact of additional American casualties on war support necessarily declines over time—possibly to zero—which is what I find here.

Second, the solution under consideration does not completely solve the limited problem in question. The military may report the ever-increasing enemy body count along with the ever-increasing American body count continuously, but doing so does not continuously increases the public’s evaluation of a war (relative to the public’s evaluation when the military reports just the American body count continuously). Rather, the public’s evaluation increases in response to one of the military’s first reports on both body counts (relative to the public’s evaluation when the military reports just the American body count), but subsequent reports do not increase it any further. Further, the increase in the public’s evaluation from the military reporting the enemy body count along with the American body count (relative to when the military reports just the American body count) does not fully compensate for the decrease from the military reporting the American body count at all. In the panel experiment, receiving the cumulative enemy body count along with the cumulative American body count at the third panel corresponding to the year 2015 increases
subjects’ mean grade by approximately two-thirds of a full letter grade (e.g., from C to B-) relative to receiving just the cumulative American body count. However, receiving the cumulative enemy body count along with the cumulative American body count at subsequent panels and corresponding years does not increase subjects’ mean grade any further than this—the increase is always approximately two-thirds of a full letter grade. And note that receiving the cumulative enemy body count along with the cumulative American body count at the third panel corresponding to the year 2015 decreases subjects’ mean grade by approximately one-third of a full letter grade (e.g., from B to B-) relative to receiving nothing else. The public’s evaluation of a war increases in response to one of the military’s first reports on both body counts (relative to the public’s evaluation when the military reports just the American body count), but subsequent reports do not decrease it any further—the decrease is always approximately one-third of a full letter grade. Altogether, this suggests that the increase in the public’s evaluation of the war from the military reporting the enemy body count along with the American body count (relative to when the military reports just the American body count) does not fully compensate for the decrease from the military reporting the American body count at all, falling short by one third of a full letter grade.

Third, the limited solution under consideration does nothing without the limited problem in question. Given that the military could report just the ever-increasing enemy body count continuously, doing so would not change the public’s evaluation of a war (relative to the public’s evaluation when the military reports nothing else). The public’s evaluation of the war only changes in response to the military continuously reporting both the enemy body count along with the American body count. In the panel experiment, receiving just the cumulative enemy body count at any panel and corresponding year does not change subjects’ mean grade relative to receiving
nothing else—the public’s evaluation of a war does not change in response to the military reporting just the enemy body count, without also reporting the American body count. As discussed before, this result suggests that Boettcher and Cobb (2006) is correct in its claim that the mechanism at play is the information on enemy body count contextualizing information on the American body count, not the former having an independent effect, although the latter certainly does.

There is a reason that “enemy death tolls have been a feature of war ever since armies stuck heads on pikes” (Phillips, 2009). In this paper, I have shown two main empirical results. First, as the military must continuously report the number of casualties it sustains, the public’s evaluation of a war continuously decreases over time at a rate that rapidly decelerates to zero and decreases no further—by about a full letter grade on a scale from F to A+. Second, as the military continuously reports the enemy body count along with the American body count, the public’s evaluation of a war continuously increases over time relative to the public’s evaluation when the military reports just the American body count at a rate that rapidly decelerates to zero and increases no further—by about two-thirds of a full letter grade on the same scale. As a result, the military can make the public evaluate a war more highly than it would otherwise by reporting the enemy body count along with the American body count. Reporting enemy death tolls, sticking heads on pikes—these strategies work. The United States military wisely reverted back to these practices during its recent engagements abroad.

But the two main results I have shown in this paper represent more than just a basis for recommendations about the military’s practices during war—particularly whether they should stick heads on pikes or not. First, as aforementioned, many studies show both observationally and experimentally that the public’s evaluation of a war decreases at any moment when the military reports the increasing American
body count at that moment, but no studies show experimentally, although many do observationally, that the public’s evaluation of the war decreases over time as the military continuously reports the American body count over time—which I have done here, for the first time. While Gartner (2008) comes closest, suggesting this phenomenon, that study’s experimental design precludes demonstrating it. In this paper, I demonstrate the phenomenon suggested by Gartner (2008) using a new experimental design but in the same spirit. Second, as aforementioned, Boettcher and Cobb (2006) suggests, but does not show either observationally or experimentally, that the public’s evaluation of the war increases at any moment when the military reports the increasing enemy body count in addition to the increasing American body count at that moment, but no studies show experimentally that the public’s evaluation of the war increases over time as the military continuously reports the increasing enemy body count in addition to the increasing American body count over time—which I have also done here for the first time.

But in this paper, my most important contribution may not be these two main empirical results, but the paradigm that they suggest. Future research research should add to our understanding of how events during a war cause public opinion across time—dynamically—rather than just at one moment in time—statically—which past research has already investigated. Lab, survey, and field experiments are the obvious way to accomplish this, just as with other causal rather than correlative questions. In this paper, taking this approach revealed the first causal rather than correlative support for the theory of the elasticity of reality of Baum and Groeling (2010) from dynamic experimental data rather than just dynamic observational data. Most critical for this research agenda—as well as the present study—is finding ways of extending the operationalization of these experiments outside the lab and the survey and into
the field—from where their results are internally valid to where they are externally valid.
3.1 Introduction

Do polarized elites result in a polarized public? Scholars arguing in favor of what Berinsky (2007, 2009) calls the elite-driven hypothesis answer this question with yes, especially on the issue of the use of force abroad by the United States (inter alia, Belknap and Campbell, 1951; Zaller, 1992; Larson, 1996; Berinsky, 2007, 2009; Baum and Groeling, 2009b). The elite-driven hypothesis holds that a citizen uses the positions of elites to choose his own position on a war.\footnote{Scholars arguing in favor of the event-response hypothesis critique this claim, arguing that a citizen uses information about a war such as estimates of its casualties and monetary costs, its probability of success, or its objective to choose his position (inter alia, Mueller, 1971, 1973; Lorell \textit{et al.}, 1985; Jentleson, 1992; Larson, 1996; Gartner \textit{et al.}, 1997; Gartner and Segura, 1998; Jen; Burk, 1999; Klarevas, 2000; Eichenberg, 2003; Feaver and Gelpi, 2004; Eichenberg, 2005; Gelpi \textit{et al.}, 2006; Boettcher and Cobb, 2006; Fordham, 2008; Gartner, 2008; Gelpi \textit{et al.}, 2009).}

At the start of a war, the president supports the war and a citizen chooses the same position as him and also supports it. At the same time, elites other than the president might take a position either opposing or supporting the war but a citizen ignores this information. This results in a “rally ’round the flag”: at the start of a war, the public’s support for it increases regardless of elite dissensus or consensus (Brody and Shapiro, 1991; Brody, 1991). But eventually this rally fades and a citizen
uses the positions of elites other than the president to choose his own position. How a citizen does this depends on which elites take positions. In the first case, a two-sided information flow, elites of one party support a war but elites in the other party oppose it. Here elites are polarized. In this case, Zaller (1992) and Berinsky (2007, 2009) argue that a citizen chooses the same position as elites in his party, but this equates to a citizen choosing the opposite position from elites in the other party. So here the public is polarized. In the second case, a one-sided information flow, elites of one party take a position on a war but elites of the other party do not. Here elites are unpolarized. In this case, Berinsky (2007, 2009) argues one of two things happens. Given elites in his party take a position but elites in the other party do not, a citizen chooses the same position as elites in his party. On the other hand, given elites in the other party take a position but elites in his party do not, a citizen instead chooses the opposite position from elites in the other party. So here too the public is polarized. The elite-driven hypothesis predicts only one outcome after the war’s initial rally fades and as long as there is at least some position-taking by elites: a polarized public. Polarized elites, polarized public. Unpolarized elites, polarized public, too. This is the argument and the conclusion of the elite-driven hypothesis.

This conclusion is incomplete because the basic mechanism of the elite-driven hypothesis—elite cues in the form of position-taking—does not allow the degree of the public’s polarization on a war to differ during two-sided information flows—when elites are polarized on the war—versus one-sided information flows—when elites are unpolarized on it. But intuitively, two-sided information flows and polarized elites should result in a greater degree of polarization in the public than one-sided information flows and unpolarized elites do. Research in the “backlash” literature like Nyhan and Reifler (2010) and Kriner and Howell (2012) not only show that this is the case but also argue that a specific mechanism causes this: counterarguing. When
elites in the two parties take opposite positions from each other on a war—during two-sided information flows and elite polarization—a citizen not only initially takes on the same position as elites in his party but also counterargues that opposite position of elites in the other party so he ends up with an even more extreme position than he would otherwise. But when elites in only one party take a position on a war—during one-sided information flows and elite unpolarization—the citizen takes on the same position as elites in his party but there is not an opposite position for him to counterargue, so he ends up with the same position that he initially takes (Lodge and Taber, 2000; Taber et al., 2001; Redlawsk, 2002; Taber and Lodge, 2006; Taber et al., 2009; Kim et al., 2010; Strickland et al., 2011). Grafting this counterarguing mechanism into the elite-driven hypothesis allows degrees of the public’s polarization during different information flows and degrees of elite polarization in a way that is intuitively expected. Unpolarized elites, polarized public—but polarized elites, even more polarized public.

I disagree that this is always true. Perhaps counterintuitively, two-sided information flows and polarized elites should result in a lesser degree of polarization in the public than one-sided information flows and unpolarized elites do—at least sometimes. In this paper I show that this is the case and develop one possible mechanism that might cause this: hedging. When elites in the two parties take opposite positions from each other—during two-sided information flows and elite polarization—a citizen not only initially takes the same position as elites in his party, but also hedges it with that opposite position of elites in the other party, so he ends up with a more moderate position than he would otherwise. But when elites in only one party take a position on a war—during one-sided information flows and elite unpolarization—the citizen takes on the same position as elites in his party but there is not an opposite position for him to use as a hedge, so he ends up with the same position he initially
takes. Grafting this hedging mechanism into the elite-driven hypothesis allows for different degrees of the public’s polarization during different information flows and degrees of elite polarization in a way that is not intuitively expected. Unpolarized elites, polarized public—but polarized elites, even less polarized public.

To summarize, the original version of the elite-driven hypothesis predicts that during two-sided information flows, the public is just as polarized as elites. But with the counterarguing mechanism added, the elite-driven hypothesis instead predicts that during two-sided information flows the public is even more polarized than elites. And with the hedging mechanism added, the elite-driven hypothesis instead predicts that during two-sided information flows the public is even less polarized than elites. Regardless of version, the elite-driven hypothesis predicts that during one-sided information flows the public is just as polarized as elites.

The hedging effect that I propose in this paper—which predicts a more moderate and less extreme public in response to elite polarization than the elite-driven hypotheses does—is broadly consistent with recent literature showing that the American public not significantly more polarized now than it was in the early 1970s—despite the fact that American political elites have become significantly more polarized over this same time period (Fiorina and Abrams, 2008). First, in terms of ideological self-classification, Fiorina (2016) shows that on American National Election Studies (ANES), General Social Survey (GSS), and New York Times (NYT) / CBS News time series asking respondents to classify themselves ideologically, there is no significant change in the percent answering “moderate” or “don’t know” between 1972 and 2012. And second, in terms of ideological belief, the same study shows that on ANES time series asking respondents to give their position on five policy issues, there is no significant change in the distribution of answers across the ideological continuum between 1984 and 2012. Many other studies show similar results suggesting that
there is no significant polarization of the American public over past four decades or so in terms of ideological self-classification nor in terms of issue positions (see Fiorina et al., 2005; Fiorina and Abrams, 2008; Abramowitz and Saunders, 2008; Fischer and Mattson, 2009). This evidence contradicts one of the dominant frames of the American media and one of the dominant beliefs of the American public in modern times, with both misperceiving—and in particular, overestimating—the level of polarization and its rate of increase over time (Ahler, 2014; Levendusky and Malhotra, 2016a,b). Perhaps the hedging effect attenuated the level of the American public’s polarization as elites became more polarized over the last four decades—but it is certainly beyond the scope of this paper to show—and this is a task I leave for future research.

In the remainder of this paper, I accomplish two main tasks. First, I develop a theoretical extension of the elite-driven hypothesis, the hedging effect. Again, I emphasize here that there are other possible theoretical extensions of the elite-driven hypothesis besides the hedging effect that might also explain the degree of the public’s polarization during two-sided information flows, but I leave the task of empirically adjudicating among these possibilities for future research. Second, I test the original version of the elite-driven hypothesis, the version with the backlash effect, and the version with the hedging effect using data I obtained from two original survey experiments in the context of the Libyan Civil War of 2011. The version of the elite-driven hypothesis with the hedging effect performs the best of the three based on the general pattern of empirical results.
3.2 Elite Cues

3.2.1 How Citizens Choose Positions

How does a citizen choose his position on a war? Before he chooses his position, a citizen first chooses to gather and cognitively process some quantity of information. As the quantity he chooses increases, the “correctness” of the position he later chooses also increases, yielding more benefit. However, as the quantity he chooses increases, his effort in information gathering and cognitive processing also increases, yielding more cost. A citizen chooses the quantity that maximizes his net benefit, his benefit less his cost (Downs, 1957). A citizen trades off the correctness of the position he later chooses and his effort in information gathering and cognitive processing.

The effort in information gathering and cognitive processing is obviously a cost to a citizen—but why is the correctness of his position a benefit to him? According to Taber and Lodge (2006), people are motivated by “accuracy goals, which motivate them to seek out and carefully consider relevant evidence so as to reach a correct or otherwise best conclusion” (p. 756; also see Fiske and Taylor, 1991; Baumeister and Newman, 1994). Here a citizen values the correctness of the position that he chooses as a benefit because this correctness satisfies his accuracy goals (see Kunda, 1987, 1990; Chaiken and Trope, 1999). Of course this also begs the question: what is a “correct or otherwise best conclusion” here and why do they have accuracy goals? My answer would be—at least in the context here—that a correct position is one in a citizen’s interest and he chooses it over other positions because of this (also see Lupia and McCubbins, 1998).

A citizen achieves the greatest correctness of the position he later chooses and incurs the least effort in information gathering and cognitive processing—maximizing his net benefit—using just a single, special piece of information: a heuristic, or
shortcut (Chaiken, 1980; Mondak, 1993b; Popkin, 1991; Sniderman et al., 1991; Lupia, 1994; Lupia and McCubbins, 1998; Lau and Redlawsk, 1997; Cutler, 2002; Kam, 2005; Lau and Redlawsk, 2006; Boudreau, 2009). Thus when available a citizen chooses his position using a heuristic and ignores all other pieces of information—and this behavior is rational. Of course there is one possible exception where a citizen does not use a heuristic—or any other piece of information—to choose his position: when the net benefit from doing so is negative. This means that the benefit he gains in position correctness from using the heuristic is less than the cost he incurs in effort to gather and cognitively process it. In this case, a citizen remains ignorant—and this behavior is also rational.

Elites’ positions on a war are such a heuristic, and thus a citizen almost always chooses to gather and cognitively process it when it is available (Rahn, 1993; Mondak, 1993a; Druckman, 2001; Cohen, 2003; Bullock, 2009; Nicholson, 2011; Hayes and Guardino, 2011; Nicholson, 2012; Druckman et al., 2012). Considered as a heuristic, elites’ positions are called elite cues. A citizen believes that elites in his party are more knowledgeable than him and share common interests with him (Lupia and McCubbins, 1998). Thus, a citizen also believes that elites in his party take a correct position on a war. As a result, a citizen chooses the same position on a war as elites in his party. Similarly, a citizen believes that elites in the other party are more knowledgeable than him but share no common interests with him. Thus, a citizen also believes that elites in the other party take an incorrect position on a war. As a result, a citizen chooses the opposite position on a war from elites in the other party (Berinsky, 2007, 2009).

In the following two sections, I specify which position on a war a citizen chooses under the two interesting configurations of elite position-taking on it. In the first configuration, a one-sided information flow, elites of one party take a position on
a war but elites in the other party do not. The expected outcome from this first configuration follows from the argument of the heuristics literature in the preceding paragraph. In the second configuration, a two-sided information flow, elites of one party support a war but elites of the other party oppose it. The expected outcome from this second configuration follows from the argument of the heuristics literature as well, but the backlash literature offers a competing argument and expectation, and so do I.

The explanation in the following two sections concerns how a citizen chooses his position on a war only after the “rally ’round the flag” effect fades (again see Brody and Shapiro, 1991; Brody, 1991). At the start of a war a citizen chooses the same position on a war as the president does—support—regardless of which positions non-presidential elites take, causing an initial rally in the public’s support. But eventually a citizen chooses his position on the war based on the positions that non-presidential elites take—opposition or support—regardless of the president’s support position, causing the initial rally to fade. Again, the theory in the next two sections only applies to the second, post-rally phase of a war.

3.2.2 One-Sided Information Flows

In the case of one-sided information flows, when elites of one party take a position on a war but elites of the other party do not, which position does a citizen choose? Using the argument of the previous section, Berinsky (2007, 2009) predicts an outcome in this case.² First, given elites in his party take a position and elites in the other party

²Zaller (1992) predicts the same outcome as Berinsky (2007, 2009) does in the first of these cases: if elites in a citizen’s party take a position on a war but elites in the other party do not, then he chooses the same position as elites in his party. But Zaller’s receive-accept-sample (RAS) theory predicts that nothing happens in the second of these cases: if elites in the other party take a position on a war but elites in a citizen’s party do not, then his position does not change. This is in fact the main motivation for Berinsky’s development of elite cue theory (ECT) as a refinement of Zaller’s RAS theory.
do not, a citizen chooses the same position as elites in his party, following their correct position. Second, given elites in the other party take a position and elites in his party do not, a citizen chooses the opposite position from elites in the other party, avoiding their incorrect position. Thus, when Democratic elites support a war and Republican elites do not take a position on it, a Democratic citizen supports it, following the correct position, but a Republican citizen opposes it, avoiding the incorrect position. Similarly, when Democratic elites do not take a position on a war and Republican elites oppose it, a Democratic citizen support it, avoiding the incorrect position, but a Republican citizen supports it, following the correct position.

**Hypothesis 1** (Support). *If Democratic elites support a war and Republican elites do not take a position on it, then Democratic citizens support it and Republican citizens oppose it.*

**Hypothesis 2** (Opposition). *If Democratic elites do not take a position on a war and Republican elites oppose it, then Democratic citizens support it and Republican citizen oppose it.*

### 3.2.3 Two-Sided Information Flows

In the case of two-sided information flows, when the elites of one party support a war but the elites of the other party oppose it, which position does a citizen choose? The heuristics literature, the backlash literature, and I offer three competing arguments which predict three different outcomes in this case.

*Heuristics*

The heuristics literature predicts the same outcome in the case of two-sided information flows as it does in the case of one-sided information flows. A citizen chooses the
same position as elites in his party, following their correct position. But at the same
time, a citizen chooses the opposite position from elites in the other party, avoiding
their incorrect position. These two mechanisms operating during two-sided informa-
tion flows result in a citizen taking the exact same position, though—so there is no
need to adjudicate which one dominates here, although it is certainly an interesting
question for future research to answer. This logic implies that when Democratic elites
support a war and Republican elites oppose it, a Democratic citizen supports it—from
his perspective, following the correct position of Democratic elites in his party and
avoiding the incorrect position of Republican elites in the other party—but a Repub-
lican citizen opposes the war—from his perspective, following the correct position of
Republican elites in his party and avoiding the incorrect position of Democratic elites
in the other party.

**Hypothesis 3 (Heuristics).** If Democratic elites support a war and Republican elites
oppose it, then Democratic citizens support it and Republican citizens oppose it the
same as they do in the case of one-sided information flows.

**Backlash**

One interpretation of the backlash literature predicts a different outcome in the case
of two-sided information flows than the heuristics literature does. Nyhan and Rei-
fler (2010) summarize the *typical* version of what they call the “backfire” effect as
follows: “individuals who receive unwelcome information may not simply resist chal-
lenges to their views. Instead, they may come to support their original opinion even
more strongly” (p. 307). This process consists of two steps. As a first step, like the
prediction of the heuristics literature, a citizen chooses the same position as elites in
his party, following their correct position. But as a second step, unlike the prediction
of the heuristics literature, a citizen then counterargues the incorrect position of the elites in the other party (Taber and Lodge, 2006; Taber et al., 2009; Kim et al., 2010; Nyhan and Reifler, 2010; Strickland et al., 2011; Kriner and Howell, 2012). As a result of this second step, a citizen maintains and reinforces the position he chose in the first step, and he ultimately chooses a position more extreme than he does in the case of a one-sided information flow (Miller and Ross, 1975; Lord et al., 1979; Ditto and Lopez, 1992; Edwards and Smith, 1996; Kruglanski and Webster, 1996; Ditto et al., 1998; Rudolph, 2006; Meffert et al., 2006; Lebo and Cassino, 2007; Gollust et al., 2009; Redlawsk et al., 2010). Here I apply to elite position-taking a phenomenon Nyhan and Reifler (2010) and Kriner and Howell (2012) call a backlash effect (also see Taber and Lodge, 2006; Baum and Groeling, 2009a; Slothuus and de Vreese, 2010; Grieco et al., 2011). According to Taber and Lodge (2006), people are motivated by “partisan goals, which motivate them to apply their reasoning powers in defense of a prior, specific conclusion” (p. 756; also see Fiske and Taylor, 1991; Baumeister and Newman, 1994). In the first step of this process, people acquire a prior. And in the second step of this process, people defend their prior and “vigorously counterargue incongruent evidence” in the form of the position-taking of elites in the other party and “become attitudinally more extreme” than their prior (Taber and Lodge, 2006, p. 756). As a result of this process, when Democratic elites support a war and Republican elites oppose it, a Democratic citizen supports it—from his perspective, following the correct position of Democratic elites in his party and avoiding the incorrect position of Republican elites in the other party—and a Republican citizen opposes the war—from his perspective, following the correct position of Republican elites in his party and avoiding the incorrect position of Democratic elites in the other party—even more strongly than they do when elites of only one party take a position—in which a citizen can acquire a prior but there is no second elite position to counter-
argue. This interpretation of the backlash literature predicts that the outcome from two-sided information flows is more extreme than than the one that the heuristics literature predicts.

**Hypothesis 4 (Backlash).** If Democratic elites support a war and Republican elites oppose it, then Democratic citizens support it and Republican citizens oppose it more strongly than in the case of a one-sided information flow.

To clarify, what I describe above is a modified version of the backlash effect which differs from the typical version of it. In the typical version, people already have a prior position on a war and then receive a single piece of information incongruent with it that they counterargue rather than resist—the position of elites in the other party—moving to a posterior position that is more extreme than their prior position. But in my modified version here, two things happen. This typical version consists of just one impact on people’s position. First, those people receive a piece of information that they accept—the position of elites in their party—acquiring or making salient a prior position, and moving to a more extreme position than before. Second, these people then receive a piece of information incongruent with their prior position that they then counterargue rather than resist—the position of elites in the other party—moving to a posterior position that is even more extreme than their prior position. This modified version consists two impacts on people’s position that accumulate and together should be larger than the single impact of the typical version—but these versions of the backlash effect do not contradict each other although they are certainly different. My modified version simply accounts for the fact that the acquisition of or the making salient of a prior position is a necessary condition for counterarguing—and thus for any manifestation of the backlash mechanism. This modified version also brings the backlash mechanism up to conceptual parity with both the heuristics
and hedging mechanisms. But I do not develop this modified version of the backlash effect any further here because it is neither the central theoretical contribution of this paper nor is there evidence of it in this paper’s empirical results.

3.2.4 Hedging

I predict a different outcome in the case of two-sided information flows from either the heuristics literature or the backlash literature. This process consists of two steps. As a first step, like the prediction of either of these literatures, a citizen chooses the same position as elites in his party, following their correct position. But as a second step, unlike the predictions of either of these literatures, a citizen then hedges his chosen position using the opposite position, the incorrect position of the elites in the other party. As a result of this second step, a citizen weakens the position he chose in the first step, and he ultimately chooses a position less extreme than he does in the case of a one-sided information flow. Here I apply to elite position-taking a phenomenon I call the hedging effect. I will elaborate on the microfoundations of this hedging effect below—which is a complex mechanism—but essentially one can think of it as the citizen choosing a position that is some weighted average of the the correct position of elites in his party and the incorrect opposite position of elites in the other party. And as a result of this process, when Democratic elites support a war and Republican elites oppose it, a Democratic citizen supports it—from his perspective, following the correct position of Democratic elites in his party and hedging it with the incorrect position of Republican elites in the other party—and a Republican citizen opposes the war—from his perspective, following the correct position of Republican elites in his party and hedging it with the incorrect position of Democratic elites in the other party—even more weakly than they do when elites of only one party take a position—in which a citizen can acquire a prior but there is no second elite position.
with which to hedge. I predict that the outcome from two-sided information flows is less extreme than than the one that either the heuristics literature or the backlash literature predicts.

**Hypothesis 5** (Hedging). *If Democratic elites support a war and Republican elites oppose it, then Democratic citizens support it and Republican citizens oppose it more weakly than in the case of a one-sided information flow.*

One reasonable argument against the hedging mechanism might be that it seems—in general—to contradict the resistance axiom of the receive-accept-sample (RAS) model of Zaller (1992). The resistance axiom—verbatim but with emphasis mine—is the following: “People tend to resist arguments that are inconsistent with their political predispositions, but they do so only to the extent that they possess the contextual information necessary to perceive a relationship between messages and their predispositions” (Zaller, 1992, p. 45). In the context of this paper, one could apply the resistance axiom straightway: *messages* are elite positions as elite cues, the contextual information necessary is information about elite partisanship as well as comprehending its meaning, and a citizen’s predispositions are his interests.

So this application of the resistance axiom predicts two different outcomes in the case of two-sided information flows depending on whether the citizen has contextual information in the form of elite partisanship and comprehension of it—or not. If the citizen has this contextual information, he is resistant and accepts the cues from elites in his own party and their position becomes one of his considerations but resists the cues from elites in the other party and their position does not become one of his considerations—so his own position moves toward the elites of his own party and away from those in the other party. On the other hand, if the citizen is unresistant and does not have this contextual information, he accepts the cues from elites in
his own party and their position becomes one of his considerations and he accepts the cues from elites in the other party and their position also become one of his considerations—so his own position moves somewhere between the elites of his own party and those in the other party. And recall the single outcome that a similar application of the hedging mechanism predicts in the case of two-sided information flows: the citizen first takes on the same position as elites in his own party but then hedges his position with the position of elites in the other party, ultimately taking on a position between the elites of his own party and those in the other party.

What’s the contradiction between the hedging mechanism and the resistance axiom as applied in the context of this paper? In the case of two-sided information flows, the hedging mechanism predicts that a citizen takes a moderate position somewhere between his own party and the other party. In the same case, while the resistance axiom predicts that the citizen without the necessary contextual information takes a moderate position somewhere between his own party and the other party, too, the resistance axiom also predicts that the citizen with the necessary contextual information takes an extreme position that is the same as his own party and the opposite from the other party. In sum, under the resistance axiom, the necessary contextual information conditions the impact of two-sided information flows on the position-taking of citizens, but no such conditioning occurs under the hedging mechanism. So empirically, it should be possible to adjudicate between the resistance axiom and the hedging mechanism and resolve the contradiction between them as long as there exists a measure for a research subject having this necessary contextual information or not, and Zaller (1992) suggests that “political knowledge—that is, a person’s summary score across a series of neutral, factual tests of public affairs knowledge” (p. 43) is one such proxy. I do so in an appendix and find relatively little evidence for the resistance axiom—although future research should pursue a more intentional test of
these competing explanations with a larger sample that allows better investigation of subgroup impacts.

3.2.5 How Polarized Is the Public?

To summarize, the heuristics hypothesis (3), the backlash hypothesis (4), and the hedging hypothesis (5) predict different degrees of the public’s polarization when elites are polarized during a two-sided information flow—when Democratic elites support a war and Republican elites oppose it—compared to when elites are unpolarized during a one-sided information flow—when Democratic elites support the war or Republican elites oppose it but not both. First, under the heuristics hypothesis, the public is just as polarized as elites during a two-sided information flow, which I call moderate polarization—Democratic citizens have moderate support and Republican citizens have moderate opposition to the war, so the public’s polarization is also moderate. Second, under the backlash hypothesis, the public is even more polarized than elites during the two-sided information flow, which I call strong polarization—Democratic citizens have strong support and Republican citizens have strong opposition to the war, so the public’s polarization is also strong. Third, under the hedging hypothesis, the public is less polarized than elites during the two-sided information flow, which I call weak polarization—Democratic citizens have weak support and Republican citizens have weak opposition to the war, so the public’s polarization is also weak. I summarize these expectations of the three hypotheses that predict the public’s polarization relative to that of political elites in Table 3.1 below. I now turn to the central theoretical task for this paper: developing the microfoundations of the hedging mechanism that I propose here.
Table 3.1. Public’s polarization during a elite polarization and two-sided information flow. The three hypotheses discussed in this paper (columns) predict different levels of support for a war among Democratic citizens and opposition to the war among Republican citizens (first two rows) during elite polarization and a two-sided information flow when Democratic elites support the war and Republican elites oppose it. The predictions of these three hypotheses (columns) also imply different degrees of the public’s polarization during this elite polarization and two-sided information flow (last row).

3.3 The Hedging Effect

Recall from the previous section that a citizen chooses his position on a war using the heuristic of elite cues when available—taking on the “correct” position of elites in his party and the opposite position from the “incorrect” position of elites in the other party—and in doing so achieving the twin goals of maximizing the accuracy of his taken position while minimizing the cost of gathering and cognitively processing information (inter alia, Downs, 1957; Lupia and McCubbins, 1998; Taber and Lodge, 2006). Certainly this position-taking procedure minimizes the information cost for the citizen—his first goal—but does it really always maximize the accuracy of his taken position—his second goal? Consider two possibilities in which the answer to this question is no.

First, it is possible that the position on a war of elites in a citizen’s party is sometimes actually “incorrect” for him. That is, they might not know more about the war than him or not share his interests with respect to it, and so taking on their position as his usual position-taking procedure prescribes actually fails to maximize accuracy and fails to achieve his goal. Second, it is possible that the position on a war of elites in the other party from a citizen is sometimes actually “correct” for him. That is, they might know more about a war than him and share his interests with
respect to it, so taking on the opposite position from them as his position-taking procedure prescribes actually fails to maximize accuracy and fails to achieve his goal.

The rational citizen recognizing either of these two possibilities—however small or large their probabilities—would incorporate them into how he chooses his position on a war (Downs, 1957). I speculate here about one way he might do this. Essentially, his recognition means that he knows a risk exists that the position of elites in his party is “incorrect” for him, and so taking on the opposite position from them actually maximizes accuracy and achieves his goal, contrary to his usual position-taking procedure. And his recognition here also means that he knows a risk exists that the position of elites in the other party is “correct” for him, and so taking on their position similarly maximizes accuracy and achieves his goal, again contrary to his position-taking procedure. The rational citizen could respond to his recognition of these risks by altering his position-taking procedure to mitigate them—and one risk-mitigation strategy pursued by rational agents in other domains is hedging (Mas-Colell et al., 1995).

3.3.1 Hedging an Investment

What is hedging in general? The term comes from finance and, in particular, investing (inter alia, Mas-Colell et al., 1995; Sharpe et al., 1999), and I illustrate hedging in this context by way of example to fix ideas, although there is no loss of generality here. An investor hedges a first asset he owns when he buys a different second asset in order to offset an adverse change in the price of the first asset. An investor faces the following problem.

- He must completely invest $2.
There are only two available assets A and B. Each of these two assets has a price of $1. He can buy multiples of the same asset but not fractions of them.

Immediately after he buys, the price of one asset will decrease to $0 and the price of the other asset will increase to $2. The prices of these two assets will never change again.

What should an investor do if he wants to maximize the value of his portfolio of assets as well as minimize the volatility of this value? He can only do three things here: buy two units of asset A, buy two units of asset B, or buy one unit of asset A and one unit of asset B. In the first two strategies—buying two units of asset A or two units of asset B—his portfolio has a value of either $0 or $4—either losing the initial $2 investment or doubling it. I call these two strategies “unhedged A” and “unhedged B.” And in the third and last strategy, his portfolio has a guaranteed value of $2—preserving the initial $2 investment. I call this strategy “hedged AB.” An investor has a portfolio worth $0 or $4 from following the unhedged A or unhedged B strategies—these are volatile lotteries in which his portfolio either loses all its value or doubles it. And the investor has a portfolio worth $2 from following the hedged AB strategy—this is a certainty in which his portfolio maintains its value.

Consider first the unhedged strategies. If asset A has a relatively high probability of doubling in price (and equivalently asset B dropping to zero price), then the unhedged A strategy has a relatively high probability of yielding a portfolio worth $4 instead of $0. But asset A still has some relatively low probability of dropping to zero price (and equivalently asset B doubling in price), so the unhedged strategy A still has some relatively low probability of yielding a portfolio worth $0 instead of $4. A similar reasoning applies to asset B having a relatively high probability of doubling in price (and equivalently asset B dropping to zero price). Thus these
unhedged strategies yield portfolios with values that are volatile—both the best and worst outcomes are possible.

Now consider the hedged strategy. A citizen can reduce the volatility of his portfolio’s value by following the hedged strategy. Regardless of whether asset A or asset B has a relatively high probability of doubling in price (and the other asset dropping to zero price), the hedged AB strategy yields a portfolio worth $2 with certainty. The maintained value of this portfolio has no volatility whatsoever—but neither the best nor worst outcomes are possible like they are in the case of the two unhedged strategies.

So which of the three strategies does an investor choose? Between the two unhedged strategies, he rationally chooses the one that has a relatively high probability of yielding a portfolio worth $4 instead of $0. And between the hedged strategies and the better of the unhedged strategies, which one he rationally chooses depends on his risk-reward preference. If he is sufficiently risk averse, he rationally chooses the hedged strategy over the better of the unhedged strategies because the former yields a portfolio worth less but also less volatile in value. And if he is sufficiently risk seeking, he rationally chooses the better of the unhedged strategies because the former yields a portfolio worth more value but also more volatility. Hedging reduces both reward and risk while failure to hedge raises both risk and reward.

3.3.2 Hedging a Position

Now what is hedging in the sense I mean the term in this paper? A citizen hedges his prior position—perhaps the same position as elites in his party—on a war when he considers a different position—perhaps the same position as elites in the other party—on it and takes on a posterior position somewhere between his prior and this different position in order to reduce adverse changes in the accuracy of his prior position. This
is completely analogous to hedging in the sense from finance and investing—except the rational agent is a citizen instead of an investor and thinks about positions instead of assets and accuracy instead of price. I illustrate the microfoundations of position hedging by way of example to fix ideas, although there is no loss of generality here. A citizen faces the following problem.

- He must allocate exactly two positions on a war to his “position portfolio.” The average of the two positions in his portfolio is his expressed position.

- There are only two available positions A and B defined by elites. Elites in his party take position A and elites in the other party take position B. He can allocate multiples of the same position but not fractions of them.

- Immediately after he allocates, the accuracy of positions A and B are revealed: one will have an accuracy of 0—zero accuracy—and the other an accuracy of 1—perfect accuracy. The average accuracy of the two positions in his portfolio is its accuracy and that of his expressed position.

What should a citizen do if he wants to maximize the expected accuracy of his position portfolio—and his expressed position—as well as minimize the volatility of its accuracy? He can only do three things here: allocate position A twice, allocate position B twice, or allocate position A once and allocate position B once to his portfolio. In the first two strategies, allocating position A twice or allocating position B twice, his portfolio has an accuracy of either 0 or 1—zero accuracy or total accuracy. I call these two strategies “unhedged A” and “unhedged B.” And in the third and last strategy, his portfolio has a guaranteed accuracy of 0.5—middling accuracy. I call this strategy “hedged AB.” A citizen has a portfolio, and his expressed position, with an accuracy of 0 or 1 from following the unhedged A or unhedged B strategies—these are
volatile lotteries in which his portfolio has either zero accuracy or perfect accuracy. And the citizen has a portfolio with an accuracy of 0.5 from following the hedged AB strategy—this is a certainty in which his portfolio has middling accuracy.

Consider first the unhedged strategies. If position A has a relatively high probability of total accuracy (and equivalently position B having zero accuracy), then the unhedged A strategy has a relatively high probability of yielding a portfolio with an accuracy of 1 rather than 0. But position A still has some relatively low probability of zero accuracy (and equivalently position B having total accuracy), so the unhedged strategy A still has some relatively low probability of yielding a portfolio with an accuracy of 0 rather than 1. A similar reasoning applies to position B having a relatively high probability of total accuracy (and equivalently position A having zero accuracy). Thus these unhedged strategies yield portfolios with accuracies that are volatile—both the best and worst outcomes are possible.

Now consider the hedged strategy. A citizen can reduce the volatility of his portfolio’s accuracy, and his expressed position, by following the hedged strategy. Regardless of whether position A or position B has a relatively high probability of total accuracy (and the other position zero accuracy), the hedged AB strategy yields a portfolio with an accuracy of 0.5 with certainty. The middling accuracy of this portfolio has no volatility whatsoever—but neither the best nor worst outcomes are possible like they are in the case of the two unhedged strategies.

So which of the three strategies does a citizen choose? Between the two unhedged strategies, he rationally chooses the one that has a relatively higher probability of yielding a portfolio, an expressed position, with an accuracy of 1 rather than 0. And between the hedged strategies and the better of the unhedged strategies, which one he rationally chooses depends on his risk-reward preference. If he is sufficiently risk averse, he rationally chooses the hedged strategy over the better of the unhedged
strategies because the former yields a portfolio that has lower accuracy but also lower volatility. And if he sufficiently risk seeking, he rationally chooses the better of the unhedged strategies because the former yields a portfolio that has higher accuracy but also higher volatility. The same thing is true here for position-taking as it is for financial investing: hedging reduces both reward and risk while failure to hedge raises both risk and reward.

3.4 Two Survey Experiments

I obtain data useful for testing these hypotheses—including the hedging effect—through two survey experiments. Both survey experiments have a $2 \times 2$ fully crossed design. The first factor is no exposure or exposure to a Democrat support cue, and the second factor is no exposure or exposure to a Republican opposition cue. These two factors result in a control condition and three treatment conditions: no exposure to either cue, called “Neither Cue” or “C”; exposure to only a Democrat support cue, called “Only Democrat Support Cue” or “T1”; exposure to only a Republican opposition cue, called “Only Republican opposition cue” or “T2”; and exposure to both cues, called “Both Cues” or “T3.” The two survey experiments are exactly the same except for the wars they use. One survey experiment uses the Libyan Civil War of 2011, while the other uses a hypothetical war based on it. A priori, I do not have firm expectations about whether the results from these two survey experiments will differ or not, although some literature suggests they might (see Gelpi, 2010; Nicholson, 2012). I include these two survey experiments in this study mainly as a robustness check.

The two survey experiments also measure subjects’ position on a war in an equivalent way: a set of four questions that are exactly the same except for the war
Table 3.2. The design of the two survey experiments. The two survey experiment have a $2 \times 2$ fully crossed design. The first factor is no exposure or exposure to a Democrat support cue, and the second factor is no exposure or exposure to a Republican opposition cue. These two factors result in a control condition and three treatment conditions: no exposure to either cue, called “Neither Cue” or “C”; exposure to only a Democrat support cue, called “Only Democrat Support Cue” or “T1”; exposure to only a Republican opposition cue, called “Only Republican opposition cue” or “T2”; and exposure to both cues, called “Both Cues” or “T3.”

mentioned in each question. The four questions ask subjects about their approval of a war, their approval of the president’s handling of the war, the “rightness” of the war, and the level of U.S. involvement in the war. The literature on the use of force disputes whether these four questions—among others—measure four separate dimensions of war support or one common, underlying dimension of it (see Berinsky and Druckman, 2007; Gelpi and Reifler, 2008). Since the arguable start of the modern literature on the use of force with Mueller (1973) scholars have used multiple questions to measure war support in attempts to tap “a sort of generalized support for war” (also see Burk, 1999, p. 43). A recent exception has been work by Christopher Gelpi, Peter D. Feaver, and Jason Reifler who concentrate on only one measure of war support: individual casualty tolerance (among other works Gelpi et al., 2006, 2009). Berinsky and Druckman (2007) critiques this approach—to which Gelpi and Reifler (2008) responds. Resolving this dispute is not my purpose in this study so I not only use four different questions to measure war support but also present the results for all of them. The interested read can judge for himself here. In doing so, I follow the conventional wisdom of the literature on the use of force articulated in Eichenberg (2005): “a single question on any issue will be a misleading gauge of the public mood because an infinite variety of question wordings on any issue is conceivable, and each is likely to yield a different set of responses [...] a reliable analysis requires the study
of many survey questions that employ a variety of wordings” (p. 153). Berinsky and Druckman (2007) echoes this: “when examining generalized levels of support for war—a concept measured only imperfectly by any single item—it is best to look at multiple indicators of such support” (p. 5).

3.4.1 An Experiment Using a Hypothetical War

For the survey experiment using a hypothetical war, I used Amazon’s Mechanical Turk to recruit a nonprobability sample of 420 subjects to complete “a 23-question opinion survey on U.S. military action abroad” and used Qualtrics to collect their responses from June 26, 2012 through July 3, 2012. I obtained 348 responses in total because I eliminated the responses of subjects that failed to pass an instructional manipulation check. This restriction does not significantly change the results in this study.\(^3\)

All subjects received the statement: “It is now the year 2023, and the United States is deciding whether or not to support a NATO mission against a country in Northern Africa.” The survey instrument then randomly assigned subjects to either a control group or one of three treatment groups. Subjects in the control group received neither a Democrat support cue nor a Republican opposition cue. Subjects in the first treatment group received only a Democrat support cue given by

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\(^3\)In the survey experiment using a hypothetical war, the instructional manipulation check consisted of a statement a question that all subjects received. The statement was: “Recent research on decision making shows that choices are affected by context. Differences in how people feel, their previous knowledge and experience, and their environment can affect choices. To help us understand how people make decisions, we are interested in information about you. Specifically, we are interested in whether you actually take the time to read the directions; if not, some results may not tell us very much about decision making in the real world. To show that you have read the instructions, please ignore the question on the next slide and instead choose only the “successful” option as your answer.” The question following was: “Some people believe that the U.S. has been successful in its operations in Afghanistan, while others believe we have not been so successful. How do you feel about the progress the U.S. has made in Afghanistan?” The question had five possible responses: “very unsuccessful,” “unsuccessful,” “evenly mixed,” “successful,” and “very successful.”
the statement: “The Democrat President encouraged members of the U.S. House of Representatives to vote in favor of a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in this country. The vote took place on November 14, 2023.” Subjects in the second treatment group received only a Republican opposition cue given by the statement: “On November 14, 2023, a majority of Republicans in the U.S. House of Representatives voted against a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in this country.” Subjects in the third treatment group received both a Democrat support cue and a Republican opposition cue given by the statement: “The Democrat President encouraged members of the U.S. House of Representatives to vote in favor of a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in this country. When the vote took place on November 14, 2023, a majority of Republicans in the U.S. House of Representatives voted against the authorization bill.”

All subjects then received four questions in a randomized sequence that asked them about their positions on this hypothetical war. The first question was: “Which of the following best describes your feelings about the current military action by the U.S. and other nations against this country?” This first question had five possible responses: “strongly disapprove,” “disapprove,” “neutral,” “approve,” and “strongly approve.” The second question was: “Overall, do you approve or disapprove of the way the president is handling the situation in this country?” This second question had five possible responses: “strongly disapprove,” “disapprove,” “neutral,” “approve,” and “strongly approve.” The third question was: “Do you think the U.S. is doing the right thing by taking part in the current military conflict in this country now, or should the U.S. not be involved in this country now?” This third question had five possible responses: “very wrong decision,” “wrong decision,” “neutral,” “right
decision,” and “very right decision.” The fourth question was: “Do you think the level of U.S. military involvement in the situation with this country should be increased, decreased, or kept about the same as it is now?” This fourth question had three possible responses: “decreased”, “kept about the same,” and “increased.”

3.4.2 An Experiment Using the Libyan Civil War

For the survey experiment using the Libyan Civil War of 2011, I used Amazon’s Mechanical Turk to recruit a nonprobability sample of 450 subjects to complete “a 24-question opinion survey on Afghanistan and Libya” and used Qualtrics to collect their responses from June 30, 2011 through July 3, 2011. I obtained 418 responses in total because I eliminated the responses of subjects that failed to pass an instructional manipulation check. This restriction does not significantly change the results in this study.⁴

All subjects received the statement: “Now here are some questions on the U.S. involvement in Libya.” The survey instrument then randomly assigned subjects to either a control group or one of three treatment groups. Subjects in the control group received neither a Democrat support cue nor a Republican opposition cue. Subjects in the first treatment group received only a Democrat support cue given by the statement: “Democrat President Barack Obama encouraged members of the

⁴In the survey experiment using the Libyan Civil War of 2011, the instructional manipulation check consisted of a statement a question that all subjects received. The statement was: “Recent research on decision making shows that choices are affected by context. Differences in how people feel, their previous knowledge and experience, and their environment can affect choices. To help us understand how people make decisions, we are interested in information about you. Specifically, we are interested in whether you actually take the time to read the directions; if not, some results may not tell us very much about decision making in the real world. To show that you have read the instructions, please ignore the question on the next slide and instead choose only the ‘evenly mixed’ option as your answer.” The question following was: “Some people believe that the U.S. has been successful in its operations in Iraq while others believe we have not been so successful. How do you feel about the progress the U.S. has made in Iraq?” The question had five possible responses: “very unsuccessful,” “unsuccessful,” “evenly mixed,” “successful,” and “very successful.”
U.S. House of Representatives to vote in favor of a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in Libya. The vote took place on June 24, 2011.” Subjects in the second treatment group received only a Republican opposition cue given by the statement: “On June 24, 2011, a majority of Republicans in the U.S. House of Representatives voted against a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in Libya.” Subjects in the third treatment group received both the a Democrat support cue and a Republican opposition cue given by the statement: “Democrat President Barack Obama encouraged members of the U.S. House of Representatives to vote in favor of a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in Libya. When the vote took place on June 24, 2011, a majority of Republicans in the U.S. House of Representatives voted against the authorization bill.”

All subjects then received four questions in a randomized sequence that asked them about their positions on the Libyan Civil War. The first question was: “Which of the following best describes your feelings about the current military action by the U.S. and other nations against Libya?” This second question had five possible responses: “strongly disapprove,” “disapprove,” “neutral,” “approve,” and “strongly approve.” The second question was: “Overall, do you approve or disapprove of the way President Barack Obama is handling the situation in Libya?” This second question had five possible responses: “strongly disapprove,” “disapprove,” “neutral,” “approve,” and “strongly approve.” The third question was: “Do you think the U.S. is doing the right thing by taking part in the current military conflict in Libya now, or should the U.S. not be involved in Libya now?” This third question had five possible responses: “very wrong decision,” “wrong decision,” “neutral,” “right decision,” and “very right decision.” The fourth question was: “Do you think the level of U.S.
military involvement in the situation with Libya should be increased, decreased, or kept about the same as it is now?” This fourth question had three possible responses: “decreased”, “kept about the same,” and “increased.”

3.4.3 Elite Cues in the Experiments

The support cue and the opposition cue in both the hypothetical and Libya experiments might seem arbitrary to the reader. First, the support cue is only given by a Democrat—identified as “Democrat” in both experiments—and the opposition cue is only given by a Republican—identified as “Republican.” Second, the support cue is only given by the President—identified as “President” or “President Barack Obama” in the hypothetical and Libya experiments, respectively—and the opposition cue is only given by a majority of one party’s members in the U.S. House of Representatives—identified as “a majority of Republicans in the U.S. House of Representatives” in both experiments. Third, the support cue is only given in the form of one elite encouraging another to do something—identified as “encouraged members of the U.S. House of Representatives to vote in favor of a bill” in both experiments—and the opposition cue is only given in the form of one elite actually doing something else—identified as “voted against” the bill in both experiments. Why do support cue and opposition cue in both experiments take these particular forms?

I chose the treatments for the Libya experiment that I conducted between June 30, 2011 and July 3, 2011 in order to use support and opposition elite cues from an event that occurred during the Libyan Civil War of 2011 only six days beforehand on June 24, 2011. In fact, just as the Democrat support cue reads, “Democrat President Barack Obama encouraged members of the U.S. House of Representatives to vote in favor of a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in Libya. The vote took place on June 24, 2011.” And
in fact, just as the Republican opposition cue reads, “on June 24, 2011, a majority of Republicans in the U.S. House of Representatives voted against a bill authorizing the limited use of the United States Armed Forces in support of the NATO mission in Libya.” So the treatments that I chose for the Libya experiment are real support and opposition cues from political elites during a war: the opposition cue is only given by a Democrat, by a President, and in the form of encouragement to do something, while the support cue is only given by a Republican, by a majority in the House, and in the form of actually doing something. I then chose the treatments for the hypothetical experiment that I conducted approximately a year later between June 26, 2012 and July 3, 2012 to parallel as closely as possible these treatments from the Libya experiment—with a support cue as a Democrat President encouraging the Republican House to vote in favor of a bill and the opposition cue as Republican House voting against that bill—in order to both replicate and check the robustness of my results in a different context. Future research should consider an obvious extension missing from this paper: conducting another hypothetical experiment with treatments in opposite configuration from both the Libya experiment and my hypothetical experiment—with a support cue as a Republican President encouraging a Democrat House to vote in favor of a bill and the opposition cue as a Democrat House voting against that bill.

And as a brief aside, the treatments for both the hypothetical and Libya experiment might also seem relatively—as Kinder (2007) puts it—“emaciated” to the reader. In particular, neither the support cue nor the opposition cue contains an argument in favor of the use of force or one against it, respectively, but only suggest that elites take a position in favor of the use of force or one against it. I chose these more emaciated treatments for the two experiments intentionally. I claim that they minimally satisfy the actual definition of an elite cue and thus should provide a more crucial test of my hypotheses about elites cues than less emaciated treatments would.
3.5 Analysis

I test the hypotheses using data obtained from the two survey experiments.

3.5.1 Variable Coding

I begin by coding variables that indicate a subject’s partisan identification and treatment group assignment in each of the survey experiments.

Partisan Identification Variables

Before each survey experiment, the survey instrument asked the two-part branching question used by the ANES Time Series Studies to classify respondents into one of seven ordered and mutually exclusive partisan identification categories: strong Democrat, weak Democrat, independent Democrat, independent Independent, independent Republican, weak Republican, and strong Republican. I code two variables that indicate a subject’s partisan identification. First, if the subject is a strong Democrat, a weak Democrat, or an independent Democrat, the variable Democrat takes the value 1 but the value 0 otherwise. Second, if the subject is a strong Republican, a weak Republican, or an independent Republican, the variable Republican takes the value 1 but the value 0 otherwise. Under this coding, if the subject is an independent Independent, the Democrat and Republican both take on the value 0.

Treatment Variables

I code three variables that indicate a subject’s treatment group assignment in each survey experiment. First, if the survey instrument assigned the subject to the first treatment group (T1), where he received only a Democrat support cue, the variable President takes on the value 1 but 0 otherwise. Second, if the survey instrument
assigned the subject to the second treatment group (T2), where he received only a Republican opposition cue, the variable *House* takes on the value 1 but 0 otherwise. Third, if the survey instrument assigned the subject to the third treatment group (T3), where he received both a Democrat support cue and a Republican opposition cue, the variable *Both* takes on the value 1 but 0 otherwise. Under this coding, if the survey instrument assigned the subject to the control group (C), where he did not receive a Democrat support cue nor a Republican opposition cue, *President*, *House*, and *Both* all take on the value 0.

**Dependent Variables**

I code four dependent variables corresponding to the four post-treatment questions in each survey experiment: *Approval*, *Handling*, *Right*, and *Level*. These variables take on ordered values which correspond to their ordered answers. For example, *Approval* corresponds to the post-treatment question that asks subjects to give their approval of the war. First, if the subject’s answer is “strongly disapprove,” then *Approval* takes on the value 1. Second, if the subject’s answer is “disapprove,” then *Approval* takes on the value 2. Third, if the subject’s answer is “neutral,” then *Approval* takes on the value 3. Fourth, if the subject’s answer is “approve,” then *Approval* takes on the value 4. Fifth, if the subject’s answer is “strongly approve,” then *Approval* takes on the value 5. An analogous coding applies to the other three dependent variables.

### 3.5.2 Estimation

For each survey experiment, I perform three methods of analysis on the data obtained. Let \( y \in \{Approval, Handling, Right, Level\} \). In the first method, separately for Democratic subjects (\( Democrat = 1 \)) and Republican subjects (\( Republican = 1 \)), I perform three Welch unequal variances t-tests. The first tests the difference in the
mean of each dependent variable $y$ between those subjects assigned to the control group (C) and those assigned to the first treatment group (T1). The second tests the difference in the mean of each dependent variable $y$ between those subjects assigned to the control group (C) and those assigned to the second treatment group (T2). The third tests the difference in the mean of each dependent variable $y$ for those subjects assigned to the control group (C) and those assigned to the third treatment group (T3). In the second and third method, I estimate an ordinary least squares regression model and an ordered logistic regression model for each dependent variable $y$. All three methods yield nearly identical results. Therefore, I only present the results of the first and second methods, which are both equivalent and the easier to understand than the third method.

3.6 Results

For each of the four dependent variables Approval, Handling, Right, and Level in each of the two wars, I present the difference in the means between subjects assigned to the control group and subjects assigned to a treatment group, for each of the three treatment groups, separately for Democratic and Republican subjects. Each figure plots these three differences in the mean for a particular dependent variable in a particular war accompanied by ninety percent confidence bands from a Welch unequal variance t-test, with Democratic subjects as “squares” and Republican subjects as “triangles.” Confidence bands that do not cross zero indicate a statistically significant difference in means. I also present in parentheses equivalent results from an ordinary least squares regression of each of the four dependent variables on the three indicator treatment variables for both Democratic and Republican subjects, and full results from this method are available in the appendix to this paper.
What is evidence for each of the five hypotheses here? I explain using the dependent variable \textit{Approval} but the following explanation is analogous for the other three dependent variables \textit{Handling}, \textit{Right}, and \textit{Level} which also measure war support. I explain using Democratic subjects but the following explanation is analogous for Republican subjects. First, evidence for the support hypothesis (1) consists of those Democratic subjects receiving only a Democrat support cue having higher mean \textit{Approval} than those in the control group receiving no cues. Second, evidence for the opposition hypothesis (2) consists of those Democratic subjects receiving only a Republican opposition cue having higher mean \textit{Approval} than those in the control group receiving no cues. Evidence for the next three hypotheses depends on whether or not there is evidence for either of these first two hypotheses. Third, evidence for the heuristics hypothesis (3) consists of those Democratic subjects receiving both cues having higher mean \textit{Approval} than those in the control group receiving no cues \textit{given} that there is also evidence for either the support hypothesis (1) or the opposition hypothesis (2). Fourth, evidence for the backlash hypothesis (4) consists of those Democratic subjects receiving both cues having higher \textit{Approval} than those in the control group receiving no cues \textit{given} that there is \textit{not} evidence for either the support hypothesis (1) or the opposition hypothesis (2). Fifth, evidence for the hedging hypothesis (5) consists of those Democratic subjects receiving both cues having the same mean \textit{Approval} as those in the control group receiving no cues \textit{given} that there is also evidence for either the support hypothesis (1) or the opposition hypothesis (2). Again, this explanation is analogous for the other three dependent variables and for Republican subjects. Unfortunately the relatively small sample sizes in the two experiments—especially the number of Republican subjects—means that this study is underpowered and cannot statistically distinguish more nuanced treatment impacts.
As such, the standards of evidence that I present here are less nuanced than in a study with more power but still suggestive of general patterns of results.

### 3.6.1 Approval

*Approval* measures a subject’s approval of each war on a 5-point scale \{1, 2, 3, 4, 5\}, with greater values corresponding to higher approval. Consider first the *Approval* results for the hypothetical war in Figure 3.1a. For Democratic subjects, receiving separately either a Democrat support cue or a Republican opposition cue increases their approval (+0.33* with SE = 0.18 and +0.35* with SE = 0.18, respectively) consistent with the support hypothesis (1) and opposition hypothesis (2), respectively. Further, for Democratic subjects, receiving both cues together increases their approval (+0.32* with SE = 0.19) by about the same as receiving either cue separately, a pattern generally consistent with the heuristics hypothesis (3) but inconsistent with the backlash hypothesis (4) and the hedging hypothesis (5). On the other hand, nothing changes the approval of Republican subjects, inconsistent with all hypotheses.

Now consider the *Approval* results for Libya in Figure 3.1b. Nothing changes the approval of Democratic nor Republican subjects, inconsistent with all hypotheses.

### 3.6.2 Handling

*Handling* measures a subject’s approval of each president’s handling of a war on a 5-point scale \{1, 2, 3, 4, 5\}, with greater values corresponding to higher approval. Consider first the *Handling* results for the hypothetical war in Figure 3.2a. For Democratic subjects, receiving separately either a Democrat support cue or a Republican opposition cue does not change their approval of the president’s handling (−0.07 with SE = 0.18 and +0.21 with SE = 0.19, respectively), inconsistent with the support hypothesis (1) and the opposition hypothesis (2). But, for Democratic subjects,
receiving both cues together increases their approval (+0.40* with SE = 0.20), a pattern generally consistent with the backlash hypothesis (4) but inconsistent with the heuristics hypothesis (3) and the hedging hypothesis (5). On the other hand, nothing changes the approval of Republican subjects, inconsistent with all hypotheses.

Now consider the Handling results for Libya in Figure 3.2b. Nothing changes the approval of Democratic subjects, inconsistent with all hypotheses. On the other hand, for Republican subjects, receiving separately either a Democrat support cue or a Republican opposition cue decreases their approval (−0.53* with SE = 0.28 and −0.80* with SE = 0.28, respectively), consistent with the support hypothesis (1) and the opposition hypothesis (2). But, for Republican subjects, receiving both cues together does not change their approval (−0.36 with SE = 0.28), a pattern generally consistent
with the hedging hypothesis (5) but inconsistent with the heuristics hypothesis (3) and the backlash hypothesis (4).

![Graph](image)

**Figure 3.2. Handling.** For the given war, each figure plots the differences in the means for Handling between subjects assigned to the control group and subjects assigned to a treatment group, for each of the treatment groups. Ninety percent confidence bands from a Welch unequal variance t-test, with Democrat subjects as “squares” and Republican subjects as “triangles.”

### 3.6.3 Right

*Right* measures a subject’s perceived rightness of each war on a 5-point scale \{1, 2, 3, 4, 5\}, with greater values corresponding to higher rightness. Consider first the *Right* results for the hypothetical war in Figure 3.3a. For Democratic subjects, receiving separately either a Democrat support cue or a Republican opposition cue increases their perception (+0.39\(^*\) with SE = 0.17 and +0.40\(^*\) with SE = 0.18, respectively), consistent with the support hypothesis (1) and the opposition hypothesis (2). Further, for Democratic subjects, receiving both cues together increases their perception (+0.37\(^*\) with SE = 0.19) by about the same as receiving either cue separately, a pat-
tern generally consistent with the heuristics hypothesis (3) but inconsistent with the backlash hypothesis (4) and the hedging hypothesis (5). On the other hand, for Republican subjects, receiving only the Democrat support cue decreases their perception ($-0.56^* \text{ with } SE = 0.30$), consistent with the support hypothesis (1), but receiving only the Republican opposition cue does not change their perception ($-0.37 \text{ with } SE = 0.28$), inconsistent with the opposition hypothesis (2). But for Republican subjects, receiving both cues together does not change their approval ($-0.33 \text{ with } SE = 0.32$), a pattern generally consistent with the hedging hypothesis (5) but inconsistent with the heuristics hypothesis (3) and the backlash hypothesis (4).

Now consider the Right results for Libya in Figure 3.3b. Nothing changes the perception of Democratic subjects, inconsistent with all hypotheses. On the other hand, for Republican subjects, receiving separately either a Democrat support cue or a Republican opposition cue decreases their perception ($-0.57^* \text{ with } SE = 0.30$ and $-0.52^* \text{ with } SE = 0.31$, respectively), consistent with the support hypothesis (1) and the opposition hypothesis (2). But, for Republican subjects, receiving both cues together does not change their perception ($-0.34 \text{ with } SE = 0.30$), a pattern generally consistent with the hedging hypothesis (5) but inconsistent with the heuristics hypothesis (3) and the backlash hypothesis (4).

### 3.6.4 Level

*Level* measures a subject’s preferred level of involvement for each war on a 3-point scale $\{1, 2, 3\}$, with greater values corresponding to higher involvement. Consider first the Level results for the hypothetical war in Figure 3.4a. For Democratic subjects, receiving separately either a Democrat support cue or a Republican opposition cue increases their preference ($+0.33^* \text{ with } SE = 0.12$ and $+0.29^* \text{ with } SE = 0.13$, respectively), consistent with the support hypothesis (1) and the opposition hypothesis
Figure 3.3. Right. For the given war, each figure plots the differences in the means for Right between subjects assigned to the control group and subjects assigned to a treatment group, for each of the treatment groups. Ninety percent confidence bands from a Welch unequal variance t-test, with Democrat subjects as “squares” and Republican subjects as “triangles.”

But, for Democratic subjects, receiving both cues together does not change their preference (+0.13 with SE = 0.14), a pattern generally consistent with the hedging hypothesis (5) but inconsistent with the heuristics hypothesis (3) and the backlash hypothesis (4). Nothing changes the preference of Republican subjects, inconsistent with all hypotheses.

Now consider the Level results for Libya in Figure 3.4b. Nothing changes the preference of Democratic subjects, inconsistent with all hypotheses. On the other hand, for Republican subjects, receiving only the Democrat support cue does not change their preference (−0.18 with SE = 0.18), inconsistent with the support hypothesis (1), but receiving only the Republican opposition cue decreases their preference (−0.37* with SE = 0.18), consistent with the opposition hypothesis (2). But, for Republican subjects, receiving both cues together does not change their preference (−0.01
with SE = 0.17), a pattern generally consistent with the hedging hypothesis (5) but inconsistent with the heuristics hypothesis (3) and the backlash hypothesis (4).

Figure 3.4. Level. For the given war, each figure plots the differences in the means for Level between subjects assigned to the control group and subjects assigned to a treatment group, for each of the treatment groups. Ninety percent confidence bands from a Welch unequal variance t-test, with Democrat subjects as “squares” and Republican subjects as “triangles.”

3.7 Discussion & Conclusion

The consistency of these results and the hypotheses differs across the four dependent variables as well as across the two experiments. However, these differences are not unexpected nor unexplainable. First, the four dependent variables may measure different dimensions of subjects’ war support contra the conventional wisdom of the literature on the use of force (see Berinsky and Druckman, 2007; Gelpi and Reifler, 2008). Second, one experiment involves a real war, while the other experiment involves a hypothetical war based on it. Cues come from real partisan elites in the former experiment but from hypothetical ones in the latter experiment (see Nicholson, 2012).
Further, subjects’ prior experiences contaminate the cues in the former experiment but not in the latter experiment (see Gelpi, 2010). Although these two explanations are reasonable, little is actually known about how and why the results of these two types of experiments diverge. Regardless of the differences, some findings emerge. Consider the consistency of the results and the hypotheses across the four dependent variables for the experiment involving the hypothetical war in Table 3.3 and for the experiment involving Libya in Table 3.4. Each experiment yields evidence or not for each of five hypotheses on each of four dependent variables (measures), or, that is, in twenty hypothesis-measure cases.

<table>
<thead>
<tr>
<th>Approval</th>
<th>Opposition</th>
<th>Support</th>
<th>Heuristics</th>
<th>Backlash</th>
<th>Hedging</th>
</tr>
</thead>
<tbody>
<tr>
<td>Handling</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Rightness</td>
<td>D</td>
<td>D</td>
<td>D</td>
<td>D</td>
<td></td>
</tr>
<tr>
<td>Level</td>
<td>D</td>
<td>D</td>
<td></td>
<td>D</td>
<td></td>
</tr>
</tbody>
</table>

Table 3.3. Evidence for the hypotheses from the hypothetical experiment. The hypothetical experiment tests each of the five hypotheses (columns) using each of the four measures (rows). A “D” in a cell indicates evidence among Democrats for the hypothesis given by the column using the measure given by the row, and an “R” in a cell indicates the same for Republicans. A blank cell indicates no evidence.

<table>
<thead>
<tr>
<th>Approval</th>
<th>Opposition</th>
<th>Support</th>
<th>Heuristics</th>
<th>Backlash</th>
<th>Hedging</th>
</tr>
</thead>
<tbody>
<tr>
<td>Handling</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Rightness</td>
<td>R</td>
<td>R</td>
<td></td>
<td>R</td>
<td></td>
</tr>
<tr>
<td>Level</td>
<td>R</td>
<td>R</td>
<td></td>
<td>R</td>
<td></td>
</tr>
</tbody>
</table>

Table 3.4. Evidence for the hypotheses from the Libya experiment. The Libya experiment tests each of the five hypotheses (columns) using each of the four measures (rows). A “D” in a cell indicates evidence among Democrats for the hypothesis given by the column using the measure given by the row, and an “R” in a cell indicates the same for Republicans. A blank cell indicates no evidence.

Consider a first finding. For Democratic subjects, receiving cues from partisan elites changes their position on the hypothetical war but does not change at all their position on Libya. For Republican subjects, receiving cues from partisan elites
changes their position on Libya but does not change their position that much on the hypothetical war. For Democratic subjects, the hypothetical experiment yields evidence in ten of the twenty hypothesis-measure cases, but the Libya experiment yields no evidence in any of them. On the other hand, for Republican subjects, the hypothetical experiment yields evidence in just two of the twenty hypothesis-measure cases, but the Libya experiment yields evidence in eight of them. This finding surprised me, and since it goes beyond this paper’s hypotheses, I leave the reader to conjecture. However, this rigorously shows for the first time in the literature that the results of experiments otherwise similar but different in their war context can diverge significantly. Future research should explore why this is the case.

Consider a second finding. The support hypothesis and the opposition hypothesis successfully predict changes in the positions of both Democratic and Republican subjects during one-sided information flows, taking into account the first insight. For Democratic subjects, the hypothetical experiment yields evidence in six of the eight hypothesis-measure cases in the “Support” and “Opposition” columns of Table 3.3. However, for Democratic subjects, the Libya experiment yields no evidence in any of the eight hypothesis-measure cases in the “Support” and “Opposition” columns of Table 3.4. For Republican subjects, the Libya experiment yields evidence in five of the eight hypothesis-measure cases in the “Support” and “Opposition” columns of Table 3.4. However, for Republican subjects, the hypothetical experiment yields evidence in just one of the hypothesis-measure cases in the “Support” or “Opposition” columns of Table 3.3. This experimentally corroborates the claims of the heuristics literature in the case of one-sided information flows, limited by the first finding above.

Consider a third finding. The general pattern of results is consistent with the hedging hypothesis successfully predicting changes in the positions of Republican subjects during two-sided information flows. However, no single hypothesis success-
fully predicts changes in the positions of Democratic subjects, although the hedging hypothesis seems to work best based on the general pattern of results. For Republican subjects, the Libya experiment yields evidence in three of the four hypothesis-measure cases in the “Hedging” column of Table 3.4 but no evidence in any of the cases in the “Heuristics” and “Backlash” columns. Further, for Republican subjects, the hypothetical experiment yields evidence in one of the four hypothesis-measure cases in the “Hedging” column of Table 3.3 but no evidence in any of the cases in the “Heuristics” and “Backlash” columns. For Democratic subjects, the hypothetical experiment yields evidence in two of the four hypothesis-measure cases in the “Hedging” column, one of the cases in the “Heuristics” column, and one of the cases in the “Backlash” column. Further, for Democrat subjects, the Libya experiment yields no evidence in any of the cases in the “Heuristics,” “Backlash,” and “Hedging” columns. This experimentally demonstrates some evidence for my claim about hedging, limited by the first finding above. I find little experimental support for the claims of the heuristics and backlash literatures.

In this paper, I fulfill two objectives. First, I develop a theoretical extension of the elite-driven hypothesis that I call the hedging effect. The version of the elite-driven hypothesis with the hedging effect anticipates that polarized elites result in a less polarized public than unpolarized elites do in time of war. Again, I emphasize here that there are other possible theoretical extensions of the elite-driven hypothesis that might also explain this outcome, but I leave the task of empirically adjudicating among these possibilities for future research. Second, I test the version of the elite-driven hypothesis with the hedging effect against the original version and the version with the backlash effect using data I obtained from two original survey experiments in the context of the Libyan Civil War of 2011. Both of these other versions of the elite-driven hypothesis anticipate that polarized elites result in at least an equally
polarized public compared to the effect of unpolarized elites. The third finding above reveals that the version of the elite-driven hypothesis with the hedging effect seems to perform the best of the three based on the general pattern of empirical results. In fulfilling these two objectives, I discover that polarized elites do not necessarily lead to a polarized public, but at least sometimes seem to lead to an unpolarized public—and that hedging might be one explanation of this outcome.

3.A Hedging versus Resisting

In both the experiment using the hypothetical war and the one using the Libyan Civil War of 2011, all subjects received four questions in randomized sequence that measured their political knowledge. The first question was: “Who has a responsibility to decide if a law is Constitutional or not...is it the President, the Congress, or the Supreme Court?” The second question was: “And whose responsibility is it to nominate judges to Federal Courts...the President, the Congress, or the Supreme Court?” These first and second questions had three possible responses: “The President,” “The Congress,” and “The Supreme Court.” The third question was: “Which party has the most members in the House of Representatives in Washington?” The fourth question was: “Which party has the most members in the U.S. Senate?” These third and fourth questions had two possible responses: “Democrats” and “Republicans.” I code a variable that indicates a subject’s political knowledge. If the subject correctly answered all four of these questions that measure political knowledge, the variable Know takes the value 1 but 0 otherwise. Also recall from the Analysis section of this paper the coding of two indicator variables for a subject’s partisanship (Democrat, Republican) and the coding of three indicator variables for a subject’s treatment
group assignment (House, President, Both) as well as the coding of the four ordered dependent variables (Approval, Handling, Right, Level).

An estimation of the regression coefficients of the equation

\[ y = \beta_1 \text{House} + \beta_2 \text{President} + \beta_3 \text{Both} \]  

for \( y \in \{\text{Approval}, \text{Handling}, \text{Right}, \text{Level}\} \) for both Democratic subjects (Democrat = 1) and Republican subjects (Republican = 1) gives the respective causal impacts of the treatment group assignment. In the case of \( y = \text{Approval} \) and likewise for the other three dependent variables, \( \hat{\beta}_1 \) gives the impact of a Republican opposition cue on Approval, \( \hat{\beta}_2 \) gives the impact of a Democrat support cue on Approval, and \( \hat{\beta}_3 \) gives the impact of both cues on Approval. Table 3.5 and Table 3.6 present these results for both the hypothetical experiment and the Libya experiment, respectively, and they are necessarily equivalent to those results presented in the Results section of this paper.

As mentioned in the main text’s discussion, relative political knowledge indicated by the variable Know allows a cursory adjudication between the resistance axiom and the hedging mechanism. In particular, under the resistance axiom, the impact of receiving both cues (Both = 1) should be equal to or larger in magnitude than the impacts of receiving only the Republican opposition cue (House = 1) or only the Democrat support cue (President = 1) for those subjects with relatively more political knowledge (Know = 1) compared to those with relatively less political knowledge (Know = 0). But under the hedging mechanism, the impact of receiving both cues (Both = 1) should be smaller in magnitude than the impacts of receiving only the Republican opposition cue (House = 1) or only the Democrat sup-
port cue (President = 1) for those subjects with relatively more political knowledge (Know = 1) compared to those with relatively less political knowledge (Know = 0).

So an estimation of the regression coefficients of the equation

\[
y = \beta_1 \text{House} + \beta_2 \text{President} + \beta_3 \text{Both} \\
+ \gamma_1 \text{House} \times \text{Know} + \gamma_2 \text{President} \times \text{Know} + \gamma_3 \text{Both} \times \text{Know} \\
+ \gamma_4 \text{Know} \tag{3.2}
\]

for \(y \in \{\text{Approval, Handling, Right, Level}\}\) for both Democratic subjects (Democrat = 1) and Republican subjects (Republican = 1) gives the respective causal impacts of the treatment group assignment conditional on political knowledge (Know = 1 and Know = 0). In the case of \(y = \text{Approval}\) and likewise for the other three dependent variables, \(\hat{\beta}_1\) gives the impact of a Republican opposition cue on \(\text{Approval}\) for unknowledgeable subjects, \(\hat{\beta}_2\) gives the impact of a Democrat support cue on \(\text{Approval}\) for unknowledgeable subjects, and \(\hat{\beta}_3\) gives the impact of both cues on \(\text{Approval}\) for unknowledgeable subjects. And further, \(\hat{\beta}_1 + \hat{\gamma}_1\) gives the impact of a Republican opposition cue on \(\text{Approval}\) for knowledgeable subjects, \(\hat{\beta}_2 + \hat{\gamma}_2\) gives the impact of a Democrat support cue on \(\text{Approval}\) for knowledgeable, subjects and \(\hat{\beta}_3 + \hat{\gamma}_3\) gives the impact of both cues on \(\text{Approval}\) for knowledgeable subjects. Table 3.5 and Table 3.6 also present these results for both the hypothetical experiment and the Libya experiment, respectively, which differ from the results presented in the Results section of this paper.

There are only two cases in which there is some partial evidence of the resistance axiom over the hedging mechanism. The first of these cases is on the variable \(\text{Approve}\) for Republican subjects in the Libya experiment. Neither cue separately nor in combination impacts \(\text{Approve}\) for politically unknowledgeable Republicans in
Libya. But both cues separately and in combination impact *Approve* for politically knowledgeable Republicans in Libya in a statistically indistinguishable way. This is partial evidence of the resistance axiom over the hedging mechanism: the impact of receiving both cues is equal to the magnitude the impacts of receiving only the Republican opposition cue or only the Democrat support cue for knowledgeable Republicans compared to unknowledgeable Republicans. The second of these cases is on the variable *Handling* for Republican subjects in the Libya experiment—and this second case is almost completely parallel to the first. Neither cue separately nor in combination impacts *Handling* for politically unknowledgeable Republicans in Libya. But both cues separately and in combination impact *Handling* for politically knowledgeable Republicans in Libya in a statistically indistinguishable way. This is partial evidence of the resistance axiom over the hedging mechanism for the same reason as in the first case. Of course, I caution that while there is only partial evidence for the resistance axiom over the hedging mechanism in only two cases—suggesting that the hedging mechanism is at least a plausible alternative to the resistance axiom—future research should investigate this issue further, especially with a larger sample size that would allow a more confident exploration of subgroup impacts from elite cues.
Table 3.5. Estimates for the causal impacts of elite cues from the hypothetical experiment. Columns give four estimated regression equations for dependent variables Approval, Handling, Right, and Level from the hypothetical experiment with independent variables corresponding to rows. Models both exclude and include an interaction for incorrectly (Know = 0) or correctly (Know = 1) answering all political knowledge questions and and include both Democratic subjects (Democrat = 1) or include Republican subjects (Republican = 1). Coefficient estimates in cells correspond to standard errors in parentheses and the presence of asterisk indicating 90% statistical significance.
<table>
<thead>
<tr>
<th></th>
<th>Democrat = 1</th>
<th>Democrat = 1</th>
<th>Republican = 1</th>
<th>Republican = 1</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Approval</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>House</td>
<td>0.05 (0.21)</td>
<td>0.03 (0.30)</td>
<td>-0.19 (0.30)</td>
<td>0.32 (0.38)</td>
</tr>
<tr>
<td>President</td>
<td>0.11 (0.20)</td>
<td>0.16 (0.26)</td>
<td>-0.30 (0.30)</td>
<td>0.27 (0.42)</td>
</tr>
<tr>
<td>Both</td>
<td>0.00 (0.20)</td>
<td>0.03 (0.27)</td>
<td>-0.05 (0.29)</td>
<td>0.32 (0.37)</td>
</tr>
<tr>
<td>House × Know</td>
<td>0.02 (0.42)</td>
<td></td>
<td>-1.16* (0.56)</td>
<td></td>
</tr>
<tr>
<td>President × Know</td>
<td>-0.11 (0.42)</td>
<td></td>
<td>-0.93* (0.57)</td>
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</tr>
<tr>
<td>Both × Know</td>
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<td></td>
<td>-1.05* (0.56)</td>
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</tr>
<tr>
<td>Know</td>
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<td></td>
<td>0.08 (0.41)</td>
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</tr>
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<td><strong>Handling</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>House</td>
<td>-0.03 (0.20)</td>
<td>-0.02 (0.29)</td>
<td>-0.53* (0.28)</td>
<td>-0.10 (0.36)</td>
</tr>
<tr>
<td>President</td>
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<td>0.02 (0.25)</td>
<td>-0.80* (0.28)</td>
<td>-0.02 (0.40)</td>
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<tr>
<td>Both</td>
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<td>-0.06 (0.26)</td>
<td>-0.36 (0.28)</td>
<td>0.06 (0.34)</td>
</tr>
<tr>
<td>House × Know</td>
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<td>-0.97* (0.52)</td>
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</tr>
<tr>
<td>President × Know</td>
<td>-0.13 (0.41)</td>
<td></td>
<td>-1.28* (0.53)</td>
<td></td>
</tr>
<tr>
<td>Both × Know</td>
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<td></td>
<td>-1.16* (0.52)</td>
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<tr>
<td>Know</td>
<td>0.16 (0.27)</td>
<td></td>
<td>0.17 (0.39)</td>
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<tr>
<td><strong>Right</strong></td>
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<tr>
<td>House</td>
<td>0.31 (0.22)</td>
<td>0.28 (0.31)</td>
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<td>-0.44 (0.41)</td>
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<tr>
<td>President</td>
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<td>0.26 (0.27)</td>
<td>-0.52* (0.31)</td>
<td>-0.17 (0.46)</td>
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<tr>
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<td>0.00 (0.28)</td>
<td>-0.34 (0.30)</td>
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<td>-0.33 (0.60)</td>
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<tr>
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<td>-0.52 (0.61)</td>
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<tr>
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<td>-0.42 (0.60)</td>
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<tr>
<td>Know</td>
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<td>-0.17 (0.45)</td>
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<tr>
<td>House</td>
<td>0.00 (0.12)</td>
<td>0.01 (0.17)</td>
<td>-0.37* (0.18)</td>
<td>-0.40* (0.24)</td>
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<tr>
<td>President</td>
<td>0.07 (0.11)</td>
<td>0.19 (0.15)</td>
<td>-0.18 (0.18)</td>
<td>-0.02 (0.27)</td>
</tr>
<tr>
<td>Both</td>
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<td>-0.03 (0.15)</td>
<td>-0.01 (0.17)</td>
<td>0.06 (0.23)</td>
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<tr>
<td>House × Know</td>
<td>-0.01 (0.24)</td>
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<td>0.04 (0.35)</td>
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</tr>
<tr>
<td>President × Know</td>
<td>-0.36 (0.24)</td>
<td></td>
<td>-0.23 (0.36)</td>
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</tr>
<tr>
<td>Both × Know</td>
<td>0.01 (0.23)</td>
<td></td>
<td>-0.28 (0.35)</td>
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</tr>
<tr>
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<td>0.03 (0.16)</td>
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<td>-0.17 (0.26)</td>
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</table>

Table 3.6. Estimates for the causal impacts of elite cues from the Libya experiment. Columns give four estimated regression equations for dependent variables Approval, Handling, Right, and Level from the Libya experiment with independent variables corresponding to rows. Models both exclude and include an interaction for incorrectly (Know = 0) or correctly (Know = 1) answering all political knowledge questions and and include both Democratic subjects (Democrat = 1) or include Republican subjects (Republican = 1). Coefficient estimates in cells correspond to standard errors in parentheses and the presence of asterisk indicating 90% statistical significance.
Bibliography


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