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Assessing Civic Competence Against The Normative Benchmark Of Considered Opinions

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Abstract
There is widespread skepticism about civic competence. Some question if citizens are informed enough to make considered decisions. Others doubt citizens' ability to rationally evaluate relevant evidence and update their opinions even when they have necessary information. The purpose of my dissertation is to critically evaluate this literature and its claims against a clearly defined normative benchmark of considered opinion.

In the first half of the dissertation, I revisit the benchmark of informed citizenship, arguing that seemingly knowledgeable citizens, as traditionally defined, may fail to consider a balanced range of arguments due to partisan biases. Accordingly, I draw a distinction between two dimensions of political knowledge; information—the classic definition and measurement; and what I call consideration—awareness of balanced sets of arguments. I empirically establish the discriminant validity of consideration as a separate and distinct dimension of political knowledge, and show that information and consideration have different consequences on policy opinions—a finding that calls for a better conceptualization of what it means to be well informed.

The importance of having political information ultimately hinges on a critical assumption that people are capable of using it effectively. In the second half, I test this assumption against even grimmer doubts over civic competence—namely that citizens lack the ability to think critically or that their conscious or unconscious desire to defend preordained political positions easily trumps the motivation to be accurate. I present three experiments that challenge these claims, in favor of a Bayesian model of information processing. Across the experiments, I find that people update their beliefs and attitudes in light of presented arguments. People did not mindlessly accept whatever arguments they encounter, nor did they categorically reject uncongenial arguments. Instead, they accounted for the (un)certainty of evidence as they form their posterior opinions, even when it disconfirms their prior opinions.

Taken together, the empirical evidence presented in this dissertation suggests that citizens' failure to act as competent decision makers is more likely due to the lack of necessary and balanced information, rather than their own unwillingness or inability to use such information.

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ASSESSING CIVIC COMPETENCE AGAINST THE NORMATIVE BENCHMARK OF CONSIDERED OPINIONS

To my mom,

Young Soon Cho (조영순)

ACKNOWLEDGEMENT

I began my dissertation with an idea that may sound a little inconsistent with my advisor Michael Delli Carpini's famous work on political knowledge. My point was that the more you know about something, the less willing you become to consider the possibility that you're wrong. And that can lead you to make a bad decision. Michael, who has studied this topic for decades, should have struck this idea down, according to my "theory." Fortunately for me, but not so fortunately for my "theory," he wasn't like that at all. He listened carefully to what I had to say, accepting my arguments when I made sense, and pushing me to be clearer when I didn't make much sense. And that's how my little challenge became my dissertation. I thank him for his guidance, patience, encouragement and enthusiasm for my research.

I am imbedded to my dissertation committee. Much of my dissertation would not have been possible without Joe Cappella, who generously shared the Health Dialogue Data with me. He offered valuable criticism and encouragement on my earlier papers that ended up in this dissertation after many rounds of revisions. Devra Moehler and Diana Mutz provided insightful comments that allowed me to see more clearly the implications of my work.

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Throughout my life, I have had the tremendous support of my loving family. My sister Yuri Kim has tolerated my weird sense of humor. My farther Wan Soo Kim has tolerated my political views. Still, Yuri laughs at my silly jokes, and my farther values my opinions. Our difference is never a match for the love and respect we have for one another.

The final stages of graduate life can be very stressful for anyone. But I have been able to relish the past few years because of my wife, partner and best friend Yunjoung Park. When I am having one of the better days, she gets as excited as I am. When I am not having one of those days, she would say "괜찮을 거야 (it's going to be fine)" and somehow make me believe her. Her ability to do so is a source of wonder. I especially thank her for Gina, our beautiful daughter, who makes cameo appearances in the dissertation.

But more than anyone else, I thank my mother Young Soon Cho, who passed away a few months after I had finished my second year at Annenberg. She didn't get as much education as I did, but I will always count what I learned from her as the most valuable lessons. I learned to love knowledge. I learned to believe in myself. And I learned about God's love. These lessons got me through many challenges I have faced at Annenberg and elsewhere.

For this and much more, I dedicate this dissertation to my mom.

ABSTRACT

ASSESSING CIVIC COMPETENCE AGAINST THE NORMATIVE BENCHMARK OF CONSIDERED OPINIONS

Jin Woo Kim

Michael X. Delli Carpini

There is widespread skepticism about civic competence. Some question if citizens are informed enough to make considered decisions. Others doubt citizens' ability to rationally evaluate relevant evidence and update their opinions even when they have necessary information. The purpose of my dissertation is to critically evaluate this literature and its claims against a clearly defined normative benchmark of considered opinion.

In the first half of the dissertation, I revisit the benchmark of informed citizenship, arguing that seemingly knowledgeable citizens, as traditionally defined, may fail to consider a balanced range of arguments due to partisan biases. Accordingly, I draw a distinction between two dimensions of political knowledge; *information*—the classic definition and measurement; and what I call *consideration*—awareness of balanced sets of arguments. I empirically establish the discriminant validity of consideration as a separate and distinct dimension of political knowledge, and show that information and

consideration have different consequences on policy opinions—a finding that calls for a better conceptualization of what it means to be well informed.

The importance of having political information ultimately hinges on a critical assumption that people are capable of using it effectively. In the second half, I test this assumption against even grimmer doubts over civic competence—namely that citizens lack the ability to think critically or that their conscious or unconscious desire to defend preordained political positions easily trumps the motivation to be accurate. I present three experiments that challenge these claims, in favor of a Bayesian model of information processing. Across the experiments, I find that people update their beliefs and attitudes in light of presented arguments. People did not mindlessly accept whatever arguments they encounter, nor did they categorically reject uncongenial arguments. Instead, they accounted for the (un)certainty of evidence as they form their posterior opinions, even when it disconfirms their prior opinions.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Chapter 1 Introduction

He who knows only his own side of the case knows little of that.

—John Stuart Mill, *On Liberty*

An essential part of navigating everyday life is making decisions based on information at hand. At least for important decisions, people gather and synthesize some information. The key word here is "some." Many things are unknowable within the limits of human capability. And even for readily available—i.e., "Googleable"—information, learning everything there is to know is impossible for all but the most trivial topics. Thus, we humans have to live with the choices we make with imperfect knowledge. It is the source of constant regret when new information makes it clear that we had made a bad decision. Even when this does not happen, uncertainty usually remains as to whether we made the right call.

Given this human condition, what decision-making strategy would at least increase the chance of making a good decision? The starting point of this dissertation is the normative assumption that giving due considerations to *both* pros *and* cons of a decision is an imperative element of sound decision making, even if the fact remains that an individual can cover only so much information. I call an opinion formed in this way a

"considered opinion" and use it as the benchmark against which to assess civic competence. Outside politics, at least, this point seems noncontroversial. That is why cancer patients undergoing chemotherapies receive explanations about the side effects *as well as* the intended treatment effects. That is also why an institutional review board requires researchers to specify the potential benefits *and* harms of conducting a particular study. Weighing pros and cons is a basic, although not perfect, guard against making a blatantly bad and avoidable choice.

The normative case for my argument can be somewhat more ambiguous in politics, due to the subjectivity involved in declaring a political decision "good," the prevalence of false information and conspiracy theories that presumably do not deserve close attention, and so on. But few would argue that taking account of diverse aspects of a potential political decision leads to more undesirable than desirable outcomes, as a matter of general principle. The importance of considering conflicting ideas is also a basic ingredient of most democratic theories. In one of the most memorable defenses of free speech, John Stuart Mill (1869) argued that even seemingly faulty claims are worthy of a fair hearing, because they ultimately provide the opportunity to perceive the truth more clearly.[1] In more recent years, deliberative theorists have similarly argued that cross-cutting political conversation is central to the democratic citizenship (Benhabib 1996; Fishkin 1995; Gastil 2008; Guttmann and Thompson 1996; Habermas 1989).[2]

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[1] Mill's argument would lose its ground, however, if people tend to ignore counterevidence to defend their prior beliefs, as the motivated reasoning hypothesis contends (Taber and Lodge 2006; Nyhan and Reifler 2010). Chapter 3 addresses this concern directly.

[2] For example, Habermas (1984, 25) holds that "[a]nyone participating in argument

There is also empirical evidence that an eclectic thinker who knows "many little things" outperforms a dogmatic thinker who knows "one big thing," as judged by the accuracy of political predictions (Tetlock 2005; Tetlock and Gardner 2015).

But even if drawing on a diverse informational base can be conducive to forming a better opinion in theory, it is entirely different matter as to how prevalent that exercise is in citizens' actual political decision-making processes. There is widespread skepticism about civic competence, underscoring the seeming implausibility of the expectation that the average citizen makes considered decisions about political matters. Some question if citizens are informed enough to make considered decisions. Others doubt citizens' ability to rationally evaluate relevant evidence and update their opinions even when they have necessary information. Raised in some of the seminal studies in political communication and public opinion (e.g., Converse 1964; Berelson et al, 1954), these concerns have been recurrent themes in the literatures on political knowledge (Delli Carpini and Keeter, 1996), public deliberation (Delli Carpini et al. 2004; Fishkin, 1991), and political information processing models (Taber and Lodge 2006; Zaller, 1992).

The purpose of my dissertation is to critically evaluate this literature, and provide empirical evidence that overcomes some of conceptual and/or methodological challenges of prior research, allowing for a better understanding of whether people fail to act as competent decision makers, and if so, in what way.

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shows his rationality or lack of it by the manner in which he handles and responds to the offering of reasons for or against claims. If he is 'open to argument,' he will either acknowledge the force of these of reasons or seek to reply to them. … If he is 'deaf to argument,' by contrast, he may either ignore contrary reasons or reply to them with dogmatic assertions."

1.1 Forming a Considered Opinion

Before getting into the specifics of my research, I begin by presenting a model that specifies the process of forming a "considered opinion." Throughout this dissertation, I use a Bayesian model as a standard procedure of integrating one's prior knowledge and new information.[3] Bayes' theorem is an equation that calculates a conditional probability of an event given another event, according to the law of probability. It is useful for studies in public opinion formation because it can be applied to generate a mathematically coherent prescription of how one *should* incorporate new evidence into one's prior belief. Under Bayes' rule, the updated conclusion (posterior belief) is supposed to be the precision-weighted average of the prior belief and new evidence. In other words, one should update one's belief such that one's conclusion leans more heavily toward new information to the extent that the evidence is clear and one's prior belief is uncertain.

For a more formal demonstration, consider the issue of the Affordable Care Act (ACA), also known as "Obamacare." Suppose that a Bayesian voter—let's call her Gina—will support the legislation to the extent that it is beneficial to her. Let θ be the unobserved true net benefit for Gina.

[3] Scholars disagree about whether certain patterns such as biased assimilation and attitude polarization constitute a violation of Bayes Theorem (Bartels 2002; Bullock 2009; Gerber and Green 1999; Taber and Lodge 2006; Taber et al. 2009; Tetlock 2005). This disagreement arises because the properties of a Bayesian model vary depending on the restriction on how people interpret information (i.e., likelihood functions). I address this issue in detail in Chapter 3.

The net benefit θ can be expressed as the weighted average of various benefits reduced to some common denominator (see Achen 1992; Downs 1957, 37; Gerber and Green 1998). I define these benefits broadly; they may include values, self-interests, public interests, altruistic motives, or even simple tastes (see Mansbridge 1983, 25-26).[4] The true net benefit is what Mansbridge (1983, 25) refers to as "'enlightened preferences' among policy choices, 'enlightened' meaning the preferences that people would have if their information were perfect, including the knowledge they would have in retrospect if they had a chance to live out the consequences of each choice before actually making a decision." Clearly, this definition "can never be put into practice" because "[n]o one can have perfect information." The best Gina can do is to educate herself with available information, though the quality of her opinion will be limited by the existing knowledge.

Now, let μ_0 be Gina's initial estimate of θ Following previous studies using the Bayesian framework (e.g., Bullock 2009; Gerber and Green 1999), I assume that this variable is normally distributed: $N\ (\mu_0, \sigma_0^2)$, where the mean is Gina's best guess, and the variance is the uncertainty of the initial belief. Given she has not received any information about the policy she would acknowledge that μ_0 may be far off the mark. So σ_0^2 is high.[5]

[4] The net benefit of a policy will vary across individuals, to the extent that it has differential effects for different people, and/or people use differential evaluative criteria to judge the importance of these effects.

[5] I follow Zaller's (1996, 21) distinction between exposure and reception in that the latter "involves actually 'getting' or taking in' or 'cognizing' the given message." Therefore, when I say Gina received an argument, I mean that she was exposed to the argument *and* she paid attention comprehending its key reasons and conclusion. Also note that I use "consider" and "receive" interchangeably.

Gina becomes *informed* about the policy as she encounters new information that suggests that the benefit (or harm) of the new policy is x_1. I assume that x_1 is normally distributed: $N\ (\theta, \sigma_1^2)$, where the variance σ_1^2 captures the precision of the new evidence in information *i*. For example, if information 1 provides overwhelmingly clear evidence, the variance σ_1^2 will be very small. Gina will update her belief by calculating a weighted average of her prior belief μ_0 and information x_1. Her posterior belief can be represented by a normal distribution with mean μ_1 and variance ρ_1, where

$$\mu_1 = \mu_0\left(\frac{\tau_0}{\tau_0 + \tau_1}\right) + x_1\left(\frac{\tau_1}{\tau_0 + \tau_1}\right) \qquad (1.1a)$$

$$\rho_1 = \frac{1}{\tau_0 + \tau_1}, \qquad (1.1b)$$

and where $\tau_0 = 1/\sigma_0^2$ and $\tau_1 = 1/\sigma_1^2$ are the precisions of the prior belief and the argument, which determine the weights on the prior belief and the new message. If Gina was initially unsure about the benefits of ACA (low τ_0), but then encounters new information with extremely precise evidence (high τ_1), the new message will be heavily weighted, virtually determining the mean of her updated beliefs μ_1. Given $\tau_1 > 0$, the precision of her belief becomes smaller ($\rho_1 < \rho_0$).

Now suppose Gina repeats this process for a stream of information $1, 2 \ldots, n$, where *n* is the number of arguments she receives, x_i is a draw from a normal distribution $N\ (\theta, \sigma_i^2)$, and σ_i^2 is the (apparent) uncertainty of information i. The posterior belief

given *n* number of messages is distributed $N\ (\mu_n, \rho_n)$, where μ_n and ρ_n are the mean and variance of the posterior distribution, and

$$\mu_n = \rho_n \left(\mu_0 \tau_0 + \sum_{i=1}^{n} x_i \tau_i \right), \text{and} \qquad (1.2a)$$

$$\rho_n = \frac{1}{\tau_0 + \cdots \tau_n}. \qquad (1.2b)$$

Again, the model implies that the posterior belief is the average of the initial belief and the collection of information, weighted by precisions, and that Gina will become even more certain about her belief after considering a range of new information ($\rho_n < \rho_{n-1}$, when σ_n^2 is finite). Now let η be the posterior belief given *all available* information, the *best possible* assessment of the true benefit. A "considered" opinion is a "good" decision because it is more likely to be closer to η than a random initial guess and an opinion of one "who knows only his own side of the case" (Mill 1869).[6]

One important strength of this model is that it parsimoniously expresses the informational conditions under which Gina's preference becomes a considered opinion. First, *n* in Equations 1.2a and 1.2b should be greater than zero and x_i should include both

[6] This term will tend toward θ, as the quality and quantity of available information goes into infinity. The correspondence between η and θ depends the quality of public discourse which, practically speaking, is not the responsibility of the average citizen, but instead of political elites and experts. This is clearly an important issue for democratic politics (e.g., Page and Shapiro 1992, Chap. 9), and although it is not my focus here, I do briefly return to this issue in the concluding section.

positive and negative values. Second, Equations 1.2a and 1.2b should hold; in other words, Gina should form her opinion by assessing the quality of information, and assigning appropriate weights to her prior beliefs and new messages. These two conditions have been the hallmarks of competent citizen decision making in the studies of political communication, and public opinion, and will respectively be the foci of Chapters 2 and 3 of this dissertation.

1.2 Preview of Chapters to Follow

The general public appears too uninformed about most aspects of politics to be able to form a "considered" opinion about a policy in question (Delli Carpini and Keeter 1996; for a recent review see Achen and Bartels 2016, Ch. 2). Concerns over low levels of political knowledge, at least as traditionally conceptualized and measured, tell only half the story, however. Even the knowledgeable might fail to serve as competent decision makers, if information selection is generally biased toward their partisan identity. To the extent that this concern holds true, the opinions of an "informed" citizenry (e.g., Althaus 1998; Bartels 1996; Delli Carpini and Keeter 1996; Gilens 2001) may differ from those of a "considered" citizenry. Chapter 2 examines this possibility. It presents evidence drawn from the *2004-2005 Health Dialogue Project*, underscoring the potential tension between two dimensions of political knowledge; *information*—the classic definition and measurement; and what I call *consideration*—awareness of balanced sets of arguments.

Even if citizens have unbiased information, one might doubt their ability to use it rationally, evaluating relevant evidence and updating their opinions as appropriate. According to empirical research on persuasion and information processing (e.g., Bartels 2002; Kunda 1990; Lord et al., 1979; Taber and Lodge 2006; Zaller 1992; see Mutz 2008, 534) how people *do*—instead of *should*—respond to persuasive arguments appears to be inconsistent with the democratic ideal. Some models suggest that people lack the ability to think critically (Zaller 1992) or that their desire to defend preordained political positions easily trumps the motivation to be accurate (Taber and Lodge 2006). Despite such widespread skepticism, how close to the democratic ideal the average citizen might reach *if* high quality and balanced information was given remains elusive because the benchmark against which to judge the quality of political reasoning is often a moving target (see Kuklinski and Quirk 2001). In Chapter 3, I present three survey experiments that challenge the models assuming people's internal failures of reason, in favor of a Bayesian model of information processing.

Finally, Chapter 4 discusses the implications of the results for relevant literatures, noting the limitations and caveats, and making suggestions for future research. This chapter concludes by reflecting on the normative implications of the findings for democracy.

Before moving on, let me acknowledge that the idealized the model of informed citizen who knows "what the issues are, what their history is, what the relevant facts are, what alternatives are proposed, what the party stands for, and what the likely consequences are" (Berelson et al. 1954, 308) is not uniformly accepted. Some scholars

note that this characterization is not well reflective of classical democratic theorists (Pateman 1970, Ch. 1) and the civic virtue of informed competence is a relatively new invention that took shape in the Progressive Era in reaction to the enthusiastic party politics of the Gilded Age (Schudson 1998).

Some scholars go further and question the usefulness of focusing on political knowledge to assess citizen competence. For one, it is unrealistic to expect citizens stay informed about political issues when information is costly and returns are often minimal (Downs 1957). The portrait of the informed voter underlying the "folk theory of democracy" is, it is argued, based on a fundamental misunderstanding of human nature (Achen and Bartels 2016). Others have suggested that the scholarly attention to political knowledge misses the points that voters can use information shortcuts to make rational decisions (e.g., Lupia and McCubbins 1998), and that each individual voter's errors stemming from the lack of detailed knowledge cancel out when aggregated into public opinions overall (Page and Shapiro 1992; but see Bartels [1996]; and Delli Carpini and Keeter [1996]).

I do not dispute the general point that people have better things to do than studying the minutiae of countless policy issues. Yet even the most skeptical observers of public opinion, who do not see much value in ordinary citizens' input into political decision making, do recognize that there is an essential democratic function in giving politicians "strong incentives to avoid doing what is widely despised" (Achen and Bartels 2016, 319; see also Zaller 1992, Ch.9). But this "modest victory for political accountability" (Achen and Bartels 2016, 319) requires that citizens stay informed when

evidence of an elected official's gross incompetence and/or corruption arises, and rationally update their beliefs about the official. Thus, even the minimalist defense of democracy appears to assume that citizens have *some* information about *some* political issues. Again, the key word here is "some." But how much is enough?

While not claiming to know the answer, I would argue (along with many other scholars) that the range of such issues need not stop at politicians violating ethical norms, but should also include some knowledge about policies that have the potential to significantly affect the individual and collective lives of the public (e.g., healthcare) and even the globe (e.g., climate change). Absent this, there would be little incentive for political elites to refrain from making decisions that can, unwittingly or not, do a disservice to their constituents' essential interests.

Chapter 2 Revisiting Political Knowledge

For many, a key element of good decision making is political information (e.g., Bartels 1996; Converse 1964; Delli Carpini and Keeter 1996). In the running example presented in Chapter 1, the importance of information is well reflected in the prediction that Gina's belief will be refined as n in Equations 2a and 2b increases. On the flipside, if uninformed, Gina's belief will mostly consist of random error, probably quite distant from η. This prediction accounts for two important consequences of widespread political ignorance (i.e., small n) that previous studies have identified. First, citizens' political opinions are usually incoherent and unstable, consisting of what Converse (1964) calls *nonattitudes*—a characteristic that is more common among less informed citizens (Delli Carpini and Keeter, 1996, 231-237). Second, voters hold policy positions that they would not have supported if they knew more about the issues, leading to collective conclusions that diverge from hypothetical "fully informed" public opinions (Althaus 1998; Bartels 1996; Delli Carpini and Keeter 1996, Ch. 6; Gilens 2001; but see Levendusky 2011 and Pierce 2015 for critiques on this literature).

In practice, even highly knowledgeable citizens are not expected to be "fully informed" in the strictest sense. As Berelson et al. (1954, 232) put it, "[v]oters cannot have contact with the whole world of people and ideals; they must sample them." So even if Gina was extremely well informed, the range of relevant information she brings to bear is likely a small subset *sampled* from the universe of all possible information of relevance. Like survey sampling, representativeness is a critical benchmark here; a biased

sample, no matter how large, cannot form a valid estimate of the unknown parameter of interest. But usually "the sampling is biased" (*ibid*, 232), and the most well known cause of such bias is partisanship, which "raises a perceptual screen through which the individual tends to see what is favorable to his orientation" (Campbell et al. 1960, 133). Since Larzarsfeld, Berelson and Gaudet's (1944, 89-90) seminal work, scholars have raised the concern that citizens fail to consider balanced sets of competing arguments because they selectively expose themselves and/or attend to information that is consistent with their partisan identity (e.g., Bennett and Iyengar 2008; Iyengar and Hahn 2009; Jamieson and Cappella 2008; Jerit and Barabas 2012; Sunstein 2001; Taber and Lodge 2006; see Hart et al. [2009] for a meta-analysis). And recent research indicates that well-informed citizens are less likely to expose themselves to opposing political viewpoints than others (Mutz 2006, 33; Taber and Lodge 2006).

Biased "sampling" of this kind (i.e., partisan selective exposure) can present a serious challenge to making rational political decisions. Suppose Gina is a Democrat who avoids uncongenial arguments, and thus the information Gina receives systematically underrepresents information about the undesirable consequences of the ACA. In this scenario, per Equation 1.2a, her estimate of θ is likely biased upward, possibly far off the mark of her real interests.[7]

[7] This is not to say there is no possibility that Gina "gets it right" without considering both sides of the arguments. Indeed, it is quite likely that after taking up oppositional information, Gina maintains her initial conclusion, if the new information turns out to be much less certain and/or relevant to her than congenial information that she had previously considered. My point, however, is that Gina cannot know if she "got it right" unless she hears the other side. Without the *complete* knowledge of his evaluative criteria, I (the researcher) cannot know if her decision is good either. While consideration of

An unbiased but relatively small sample of arguments can be more useful than a large but biased sample. For instance, suppose that there are *N* number of voters, who have identical interests and evaluative criteria as Gina, trying to determine whether the ACA serves their best interests. Assume they are unbiased Bayesians, although they consider just a few relevant arguments. For each individual, argument consideration results in only a slight improvement (e.g., a few percentage points increase) over a 50-50 chance, because of individual errors stemming from their random initial guess, and sampling variability in argument selection. But so long as these errors are *random*, they will cancel out when aggregated. So the *average* of Ian-like voters' opinions will quickly approach η even when individuals draw on small amounts of information—more rapidly so if the initial guess is given smaller weight than new information.

This idea may be reminiscent of Page and Shapiro (1992), who argue that collective public opinions are "rational" despite individual ignorance and uncertainty. A difficulty with this line of argument, however, is substantiating the assumption that individual errors are randomly distributed (see Bartels 1996; Delli Carpini and Keeter 1996). While Page and Shapiro do acknowledge misleading and manipulative information as one of the prominent threats to the validity of the random assumption, their discussion is focused on the biases of available information and of those who *produce* it—not those who *consume* it. Partisan bias on the part of the citizenry, however,

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competing arguments is neither the necessary nor sufficient condition for "getting it right," but it raises the chance of better decision making (see Tetlock 2005).

also undermines a crucial condition under which aggregation can mitigate individuals' lack of information.

2.1 Balanced Consideration as a Hallmark of Political Knowledge

My argument is in line with a central claim found in the scholarship on deliberation; that the quality of democratic decisions depends on consideration of diverse viewpoints (e.g., Fishkin, 1995; Gutmann and Thompson 1996; Mutz 2006, 7-10; Sunstein 2001). At the same time, it points out an important limitation in the classic conceptualization and measurement of political knowledge (and Equation 1.2a), which is that they do not take partisan bias into account.[8] In order to incorporate the possibility of biased reception of information, I categorize the n cases of received arguments into two groups: k cases of pro arguments ($x > 0$), and $n - k$ cases of con arguments ($x < 0$), where $n \geq k$.[9] I rewrite Equation 1.2a as

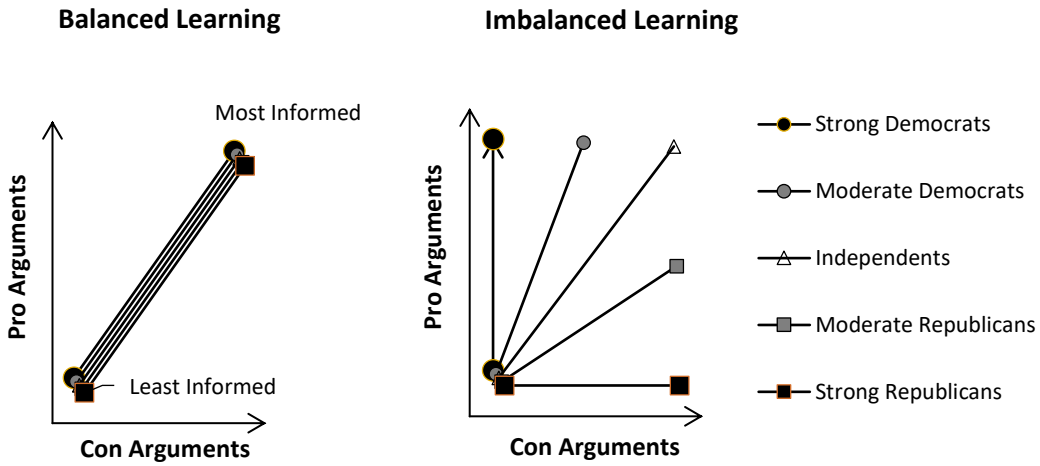
$$\mu_n = \rho_n \left(\mu_0 \tau_0 + \sum_{i=1}^{k} x_i \tau_i + \sum_{i=k+1}^{n} x_i \tau_i \right). \quad (2.1)$$

Assuming competing information flows in the public discourse are evenly balanced (e.g., Zaller 1992, 187), the set of pro and con arguments Gina considers should

[8] Delli Carpini and Keeter (1996, 10) define political knowledge as "the range of factual information about politics that is stored in long-term memory."
[9] See Gerber and Green (1999, 195) for a similar formulation.

be roughly balanced ($k \approx n - k$).[10] This requirement is violated, for example, when Democrats systematically learn more pros than cons ($k > n - k$), while Republicans do the opposite ($k < n - k$). The classic definition does not make this constraint explicit, leaving open the possibility that seemingly knowledgeable citizens in fact know only one side of the story.

Figure 1: Political Learning with and without Partisan Bias



For conceptual clarity, I draw a distinction between two dimensions of political knowledge. One is *information* about politics and public affairs—the classic definition—leading to what I call "informed opinion." The other is *consideration* of the central arguments in favor and against a political decision—leading to what I call "considered

[10] This doesn't imply everyone will converge to the middle because the weights associated with each piece of information are assumed to vary across individuals.

opinion."[11] Figure 1 visually demonstrates two hypothetical relationships between *information* and *consideration*, with and without partisan bias in political learning; coordinates in the first quadrant represent the number of pro (Y) and con (X) messages one has received. In the left panel of Figure 1, partisans are equally likely to learn evidence that supports the *opposing* party's position as evidence congenial to their own party. The right panel visualizes biased learning in its most extreme formulation where strong partisans ignore unfavorable arguments altogether.

The first goal of this chapter is to examine how citizens actually match up against these distinct possibilities, using an argument repertoire measure tapping people's awareness of pro and con arguments about a universal healthcare system (see Cappella et al. 2002 for validity and reliability tests of this measurement). This is essentially a discriminant validity test, establishing the (lack of) correspondence between information and consideration, which may call for better conceptualization and measurement of political knowledge.

[11] Zaller (1992, 40) defines "consideration" as the "reason that might induce an individual to decide a political issue one way or other," which is formed when political arguments are *accepted*. Unlike Zaller, however, my definition does not require acceptance of an argument, only careful contemplation. This conceptual difference leads to different operationalizations of consideration; unlike the survey item used in Zaller's study to measure considerations— "what idea came to mind as answering the question" — I draw on the open-ended questionnaires that explicitly ask the respondents to mention arguments for *and* against their own view, which enables me to capture the range of reasons a respondent could offer (Cappella et al. 2002).

2.2 Estimating Considered Opinions

If reality resembles the left panel of Figure 1, the issue of improving public opinions largely boils down to correcting for simple political ignorance. This is the condition under which one can treat "informed opinion"— the opinion one would hold if he/she was *informed in the traditional sense*—as the normative benchmark. Empirical research on the effects of factual knowledge (or ignorance) on public opinion and vote choice shares this assumption (e.g., Althaus 1998; Bartels 1996; Claassen and Highton 2006; Delli Carpini and Keeter 1996; Gilens 2001). These studies statistically impute a counterfactual "fully informed" public by comparing each respondent's actual preference with an opinion that a "fully informed" individual sharing the same combination of characteristics is predicted to hold. The gap between such hypothetical fully informed preferences and the respondents' observed preferences is taken to be evidence that information, or the lack thereof, matters for public opinion.

On the other hand, if political learning is closer to the right panel, "informed opinion" is likely to belie the benchmark. It is important to note that the methods that statistically impute informed opinion are essentially changing the observed opinions of lesser informed respondents to those held by well-informed individuals with similar demographic and social characteristics. Therefore, the estimates of "informed opinion" should reflect the characteristics of well-informed people, such as their political interest (Delli Carpini and Keeter 1996), their news media use (Price and Zaller 1993), their information processing strategies (Sniderman et al. 1991) and most importantly for my

purposes, the extent to which they consider conflicting perspectives (Mutz 2006; Taber and Lodge 2006). To the extent that the citizen learning process is not evenhanded, it will remain uncertain as to how much statistically simulated "fully informed" preferences reflect the undesirable effects of partisan bias in information reception (see Pierce 2015).

In keeping with the conceptual distinction between the two facets of political knowledge, I distinguish "considered opinion" from "informed opinion," and define a citizen as having the former if he/she has received a *balanced* set of competing arguments offered by the oppositional sides. Using two empirical strategies, I estimate considered opinions to gauge how balanced consideration affects public opinion. In doing so, I impute "informed" opinions as well, following previous research (e.g., Bartels 1996; Delli Carpini and Keeter 1996), to examine whether they converge to (or diverge from) "considered opinions."

The first strategy I use for observing considered opinions is statistical imputation methods, similar to Bartels (1996) and Delli Carpini and Keeter (1996). In my case, I use *consideration* instead of *information* to simulate what choice people would make if they had considered a *broad and balanced* range of pros and cons. Specifically, I estimate the effects of considering arguments *for* a universal healthcare system, and the effects of considering arguments *against*. And based on the estimated parameters, I simulate a "considered" public opinion by setting the numbers of pro and con arguments underlying people's opinion to be *equally high* for everyone and calculating the average of the imputed opinions.

A key difference between this approach and previous models of "informed" opinions is that it directly addresses the potential effects of imbalanced learning by *fixing* argument awareness to be balanced—a defining property of a considered opinion. That said, as with prior work (Bartels 1996), the imputation methods involve some strong modeling assumptions, and thus the estimates may be susceptible to a variety of specification errors. Perhaps the most prominent threat is an omitted variable bias (see Levendusky 2011); those who have considered more (pro or con) arguments about the healthcare issue may have idiosyncratic demographic and psychological traits, and these characteristics, not consideration per se, may produce what is observed to be considered opinions.

As a second empirical strategy, I draw on an experiment where individuals are randomly assigned to participate in public deliberation—an intervention that is designed to produce "refined" public opinion that "has been tested by the consideration of competing arguments and information conscientiously offered by others who hold contrasting views" (Fishkin 2009, 14; see also Barabas 2004; Cappella et al. 2002; Fishkin 1995; Fishkin and Luskin 2005; Luskin et al. 2002; for a review see Delli Carpini, Cook and Jacobs [2004]). Research based on experimentally induced deliberation implements this conceptual framework (Barabas 2004; Cappella et al. 2002; Fishkin 1995; Luskin et al. 2002) by creating carefully balanced argument pools, using trained moderators, and encouraging participants to be open-minded. Although participants still may not take opposing arguments seriously, it is safe to assume that deliberation can surmount at least one likely cause of imbalanced considerations—

selective exposure—as diversity of viewpoints is a minimal requirement that distinguishes it from other forms of political communication (Thompson 2008).

While statistical methods for imputing *informed* opinions and public deliberation are sometimes seen as addressing the same underlying phenomenon of political ignorance and its consequences (e.g., Althaus 1998, 547-548; see also Gilens 2001; Sturgis 2003), according to my conceptualization, they emphasize different elements of political decision making. Certain procedures in public deliberation are specifically designed to combat such problems as biased selection of information, which may creep into the estimates of "informed" opinion. Norms, such as equal exchange of diverse viewpoints and careful consideration of competing arguments are theorized to be the defining principles of successful deliberation (Benhabib 1996; Gastil 2008; Gutmanm and Thompson 1996; Habermas 1989). To the degree that the difference between the two research paradigms is substantial, "informed opinions" will *not* correspond to "deliberative opinion."

At the same time, the role of argument consideration in actual deliberative forums is debatable (Mutz 2008; Sanders 1997; Price et al. 2006; Wojcieszak 2011). Although deliberation by definition solves the selective exposure problem, it does not necessarily guarantee balanced *reception* (see footnote 2); people may *attend* selectively to congenial information. Or they may respond predominantly to superfluous information (e.g., partisan cues) without really processing the content of the messages (Petty and Cacioppo 1986; Zaller 1992). The difficulty comes from the fact that there are no acceptable grounds for declaring *a priori* what "considered opinions" would look like

(see Kuklinsiki and Qurik 2001; Price and Neijens 1997), which makes the prediction of opinion changes impossible in most cases. That said, there is evidence that formal deliberation and exposure to disagreement in other contexts enhances the understanding of opposing arguments (Cappella et al. 2002; Mutz 2006, 73; Price et al. 2006) and that opinion shifts during deliberation are driven by consideration of well-justified arguments (Westwood 2015).

One way to solve this dilemma may be to compare the results of the two approaches (deliberation and statistical imputation), which bring *different* (normative and methodological) assumptions to get at the *same* thing—collective opinions of a hypothetically more deliberative citizenry. The two approaches will generate similar results so long as both approaches successfully estimate considered public opinions. And to that degree, each may provide complementary evidence to the other. The imputation strategy can generate an expectation about how evenhanded argument consideration during deliberation affects opinion changes *ex ante*, without making an assumption about which collective opinion is "better" for the public. The experimental approach allows us to observe actual opinion changes that arise when people are exposed to diverse viewpoints, without modeling assumptions. Drawing on different methods can be seen as a kind of a convergent validity test: If inconsistent, at least one of them has to be relying on wrong assumptions. If consistent, one may have increased (though by no means certain) confidence in the results.

2.3 Data and Methods

Data for my analyses comes from the 2004-2005 *Healthcare Dialogue (HD) Project*, a multi-wave study that combined Internet surveys with online deliberation experiments.[12] Participants in the HD Project were drawn from a representative sample of adult American citizens (aged 18 or older), maintained by Knowledge Networks (now GfK).[13] Because of the software used for the online discussions, those without access to a personal computer were excluded from the sample. Nevertheless, the characteristics of the HD sample reasonably resembles the 2004 American National Election Study sample and the 2004 Current Population Survey benchmark on key demographics and partisanship (see Appendix A). At the screening survey, 3,429 respondents were invited to take part in the study, and 2,193 completed the baseline survey. The final response rate for the baseline survey was 33.5% (AAPOR RR3). Table 1 summarizes the timeline of the major events of the Healthcare Dialogue Study with respect to the analyses I conducted.

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[12] Data used in Chapter 2 of this dissertation comes from the Health Dialogue Project, supported by grants to Vincent Price and Joseph N. Cappella from The National Science Foundation (Grant EIA-0306801) and the Annenberg Public Policy Center at the University of Pennsylvania. I thank everyone previously involved in the Health Dialogue Project. They, not me, conducted surveys, experiments, and content analyses.

[13] The project included a purposive sample of healthcare policy elites including policy makers, experts and industry representatives (N = 314). Because the focus of this research is policy preferences of the general public, I use only the population sample.

Table 1: The Health Dialogue Project

Date	Event	Key Variables / Discussion Topics	N
7/14 to 8/12 (2004)	Baseline Survey	Political Information Argument Repertoire Opinion: Nationalized Universal Healthcare (1) Party Identification Control Variables	2,193
9/17 to 9/23	Deliberation 1	The most pressing problem	376
11/30 to 12/6	Deliberation 2	Policy proposals for the selected problem	355
2/4 to 3/1 (2005)	Post Survey	Opinion: Nationalized Universal Healthcare (2)	1,340

Experimental Design

A subset of participants in the baseline survey (N = 1,844) was asked to participate in subsequent pre- and post-discussion surveys. Among these individuals, 1,237 were randomly assigned to attend four rounds of online discussions that took place between September 2004 and May 2005 in addition to the surveys. Those in the treatment group were divided into 72 discussion groups. The first (September 2004) and second (November 2004) discussions pertained to issues surrounding health insurance. In the first discussion, participants deliberated about the most pressing problem related to health insurance, and each group identified one problem as a top priority. Twenty seven groups named the large number of Americans without insurance coverage, while 45 groups named the increasing costs of health insurance. In the second round, participants discussed the pros and cons of several policy proposals designed to address the specific problem selected by the group in the first discussion. Each discussion was held for an hour, facilitated by a moderator. Each deliberation participant made 18.6 (first round) and 20.2 (second round) statements on average at each event. Since I rely on open ended questions tapping awareness of arguments for and against a "universal, federally funded

health insurance," my analyses exclusively drew on the first two deliberation meetings and the two waves of surveys conducted before the first and after the second deliberations (see Table 1).[14] Appendix A shows that the treatment and control groups are balanced on pre-treatment covariates among those who responded to the baseline and post surveys, minimizing concerns that uneven attrition between groups is responsible for the results shown below.

Non-compliance.

Although substantial efforts were made to motivate participants to attend the discussions by accommodating their schedules, sending reminders and offering incentives (e.g., $15 in wave 2), among the 1,237 invitees, only about 30% were present in deliberative meetings (376 in the first and 355 in the second). I take three estimation strategies to deal with the non-compliance issue. First, I regress opinion change on random *assignment*, estimating the Intent-to-Treat Effect (ITE). Albeit valid, ITE is a smaller than average treatment effect (ATE) because many of those assigned to be treated never receive the treatments. Second, I use a two-stage-least-square (2SLS) estimator where random assignment is used as the instrument for attendance. This approach recovers the ATE on the Treated (ATET). One must assume, however, that compliers and non-compliers would be affected by the treatment in the same way in order to generalize findings to the population. To estimate the ATE, I use simple Difference-in-Differences (DD) estimators treating deliberation *attendance* as the independent variable, which *can*

[14] The third and fourth discussions and surveys were concerned with prescription drugs.

be generalized to non-compliers. The downside is that this strategy is essentially "observational"; since deliberation attendance is likely endogenous, it requires an additional identifying assumption that the overtime opinion change would have been the same for attendees and non-attendees in the absence of deliberation.

Measures

Support for universal health insurance. At the baseline survey, participants indicated their views on "a universal, single-payer system of national health insurance paid for by the federal government." The same question was asked in the post survey, which was fielded about two months after the round two deliberations. This variable was coded on a scale from 0 ("oppose strongly") to 1 ("favor strongly"), with the midpoint representing "don't know" responses.

Consideration of pro and con arguments. After being asked about one's opinion on universal health insurance, those who were favorable to the program were then asked to offer *all* the reasons they have for being in favor of a universal, national health insurance program. They were then asked to give reasons "other people" might have for being opposed to the program. Similarly, participants who were opposed to a universal health insurance program were asked to give rationales for their own and for others' views. Those who did not take a position were asked to name the reasons why other people might favor a universal health insurance, followed by a question asking about reasons for being against it. Thus, all respondents were invited to offer both pro and con arguments about a federally funded health insurance program, regardless of their opinion.

The number of pro and con arguments was counted by four independent coders, using the coding procedure developed and implemented by Cappella et al. (2002). Answers without evidence (e.g., "it makes sense"), restatement of opinion, repetition of the same ideas, and the like were not counted. Evidence coupled with stated or implied explanation as to why that evidence supports/opposes universal national health insurance was counted as a reason (argument). A subsample of 50 responses was randomly drawn and coded by all of the coders to test inter-coder reliability. Krippendorff's alpha value for reliability was .83 for pro reasons, and .85 for con reasons. Overall, people supplied similar numbers of pro arguments (M = 2.30, SD = 2.69) and con arguments (M = 2.17, SD = 2.74).[15]

Political information. The *Healthcare Dialogue Project* assessed general political knowledge as well as specific healthcare issue knowledge in the baseline survey. General knowledge was measured by an additive index of the number of correct answers to the five questions recommended by Delli Carpini and Keeter (1996, 306). The seven questions tapping healthcare issue knowledge included knowledge about provisions of the Medicare bill signed by President Bush; which organization was responsible for deciding whether drugs are ready for use; the political office held by Tommy Thompson; the amount of Medicare's fund; the Bush administration's position on stem cell research;

[15] Of course, I do not claim that there is a perfect correspondence between all the arguments one had previously encountered, and the arguments one provided during the survey. But it is not unreasonable to assume that those who were able to cite both pro and con arguments in some details are more *likely* to have taken uncongenial arguments seriously, than those who only cited arguments confirming their opinions (see Cappella et al. 2002 for validity tests of this measure).

eligibility requirements for Medicare; and the percentage of Americans without health insurance. I created an additive index of political information by counting the number of correct responses to all 12 items (M = 7.20, SD = 2.47, α = .72).[16]

Party identification. Conventional survey questions (e.g., ANES) were used to measure partisanship. Respondents were categorized into five (partisan) groups: Strong Republicans (16.1%), Moderate Republicans (16.5%), Independents (34.5%), Moderate Democrats (16.4%), and strong Democrats (16.5%).[17]

Control variables. Some of the statistical models to be reported below include a range of covariates. They include age (Mean = 44.7), years of education (Mean = 14.2), gender (Male = 46.6%), income (Median = $50,000), Black (8.4%), marital status (Married = 64%), political interest, political participation, political discussion, attention to news about healthcare issues, access to health insurance (insured = 87%), one's own health status (36.0% have a serious disease), and family health status (42.5% have a family member with a serious disease). See Appendix A for more detailed information about the measurement of the control variables. The appendices also provide survey question wordings of the independent, dependent and control variables, and their descriptive statistics. Unless noted otherwise, all variables are recoded to vary between 0 and 1.

[16] The results reported below are robust to using general political knowledge items only as well as using healthcare issue knowledge items only.
[17] Those who indicated "something else" (N = 170) were categorized as independents.

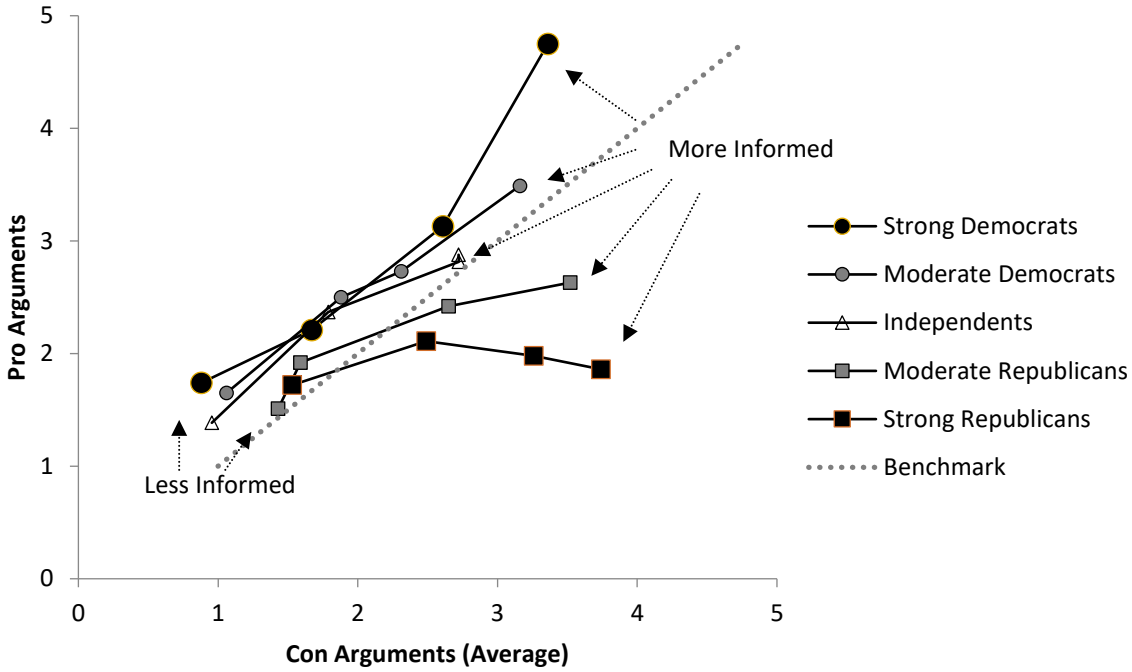
2.4 Distinguishing Information and Consideration

A limitation in the classic notions of political knowledge and informed opinion, I have argued, is that it ignores the possibility of biased sampling of incoming political information. In this section, I examine this possibility empirically, using the distinction I drew between two dimensions of political knowledge. If the partisan "perceptual screen" affects the sampling of received information, citizens will fail to receive both sides of an argument equally. And to the extent that some citizens are "better informed," their awareness of opposing arguments is likely to be slanted. If there is little partisan bias in information selection, "well informed" partisans (according to the classic definition) would have considered similar number of pro and con arguments, in which case information and consideration would be essentially the same thing. In Figure 1 (above) I presented two hypothetical scenarios, where such biased learning is completely absent (left panel) or extremely strong (right panel).

In Figure 2, I match the actual data against the two hypothetical possibilities outlined in Figure 1 (see Appendix A for specific numbers including number of observations and standard errors).[18] The solid lines plot the mean values of pro and con arguments provided by each partisan group, with the four markers in each group

[18] As a general principle, I start from simple (transparent) specifications, and then move onto more complicated models, checking the robustness of findings. In these "simple" models, I treat the key independent variables (e.g., PID and political information) as categorical, and compare group means.

Figure 2: Observed Relationship Between Information and Consideration



Note. N = 2,193. The mean values of pro and con arguments are plotted by PID and political information.

representing levels of political information.[19] As can be seen, partisans tend to diverge from the diagonal "benchmark" of balanced consideration, in the direction of the position most closely associated with their partisan identity. People were more aware of the arguments for their own side, and this pattern was largely driven by "knowledgeable" citizens. Well-informed strong Republicans were especially likely to have ignored pro-healthcare reform arguments, as they mention just about the same number of arguments

[19] I use a quartile split to divide the respondents into four levels of political information, with the following cutoffs: 42% or less, 50%, 67%, 83% correct answers, which roughly correspond to the quartiles.

for universal healthcare as their uninformed counterparts (1.86 vs 1.72), although they were able to articulate far more con arguments than less informed ones. Similar, albeit less pronounced, patterns can be found for moderate Republicans and strong Democrats. Independents and moderate Democrats, on the other hand, appear to exhibit fairly well balanced considerations on average.

Now, I provide a more formal examination of the tendencies highlighted in Figure 2: (1) partisan gaps in the awareness of pro and con arguments, and (2) the pattern where partisan gaps are widest among the well informed. To do so, I fit a set of regression models where the number of pros and cons are the dependent variable.[20] First, consider the following regression model:

$$AR_{i,j} = \alpha + \beta_1 Pro_j + \gamma_1 PID_i + \gamma_2 Pro_j \times PID_i + \delta_i + \varepsilon_{i,j} \quad (2.2)$$

where the *j* subscript indexes arguments (pro or con), the *i* subscript indexes each participant, $AR_{i,j}$ is the number of arguments, Pro_j is a within-individual factor dummy variable indicating argument position (pro =1; con=0), PID_i is a vector of dummy variables for partisan identities where strong Republicans serve as the reference category, δ_i is individual-specific random effects and $\varepsilon_{i,j}$ is the stochastic error term.[21] α is the

[20] In these regression models, there are two observations of the dependent variable (pros and cons) for each respondent.

[21] Four dummy variables for PID were included in the model, with the base category representing strong Republicans. I fit a Maximum Likelihood model to account for the individual random effects.

average number of cons articulated by strong Republicans (the reference category), β_1 is the difference between the number of pros and cons (for strong Republicans), γ_1 is the vectors of parameters for the PID dummies summarizing partisan differences in the number of cons, and γ_2 is the interaction between PID and argument position indicating the partisan differences in the gap between the number of pros and cons. If there is a partisan bias in consideration of pro and con arguments, the difference between pros and cons (i.e., the coefficient on Pro_j) will vary across partisan groups. This can be demonstrated by fitting Equation 2.2 where the value of the coefficient on argument position varies across PID, and comparing it against a reduced model restricting the interactions (γ_2 vector) to be zero. A likelihood-ratio test suggests that the model including the interaction terms fits significantly better than the restrictive one (Chi^2 (4) = 91.61, $p < 0.001$), providing strong evidence that the distributions of pros and cons that underlie people's opinion substantially differ by PID.

Table 2: Gap between Pros and Cons by PID

(1) Strong Rep	(2) Mod. Rep	(3) Independent.	(4) Mod. Dem	(5) Strong Dem
-1.077[ab]	-0.13[b]	0.33[a]	0.53[a]	0.77[ab]
(0.15)	(0.15)	(0.10)	(0.15)	(0.15)

Note. Entries are calculated by estimating Equation 2.2. Standard errors are in parentheses. N = 4,340. [a] $P < 0.1$ where $H_0: \beta = 0$. [b] $P < 0.1$ where $H_0: \beta = 0.33$.

Table 2 reports the estimates of $\beta_1 + \gamma_2$ (i.e., average gap between pros and cons for each partisan group), which ranges from -1.08 to 0.77 in the expected direction (see Appendix A for a full report on the estimates of Equation 2.2). How does each partisan

group measure up in terms of balanced awareness? One can think of two benchmarks here; assuming that the information flows about the issue in public discourse were evenly balanced, one may define unbiasedness as $Pro - Con = 0$. By this standard, every group appears to be biased beyond chance, except for moderate Republicans. But assuming that Independents lack a partisan slant, the fact that they also named more pro arguments may indicate the range of arguments brought to the citizenry overall is slightly slanted in favor of universal healthcare. If this were the case, Independents could serve as a benchmark in that they lack partisan bias. By this standard ($Pro - Con = 0.33$), all but moderate Democrats significantly deviate from the benchmark. In either case, strong partisans exhibit slanted distributions of considered reasons.

Next, to examine the correspondence between information and consideration I estimate a regression that extends Equation 2.2 by including political information, and its interactions with PID and argument position, which takes the form:

$$AR_{i,j} = \alpha + \beta_1 Info_i + \beta_2 Pro_j + \beta_3 Info_i \times Pro_i + \gamma_1 PID_i + \gamma_2 Info_i \times PID_i + \gamma_3 Pro_j \times PID_i + \gamma_4 Info_i \times Pro_j \times PID_i + \delta_i + \varepsilon_{i,j} \quad (2.3)$$

where $Info_i$ is the political information of respondent *i*, α is now the predicted number of cons mentioned by the least informed strong Republicans (who served as the reference category), $\alpha + \beta_2$ is the predicted number of pros for strong Republicans with the lowest political information score; $\alpha + \beta_1$ is the fitted value of cons for "fully informed" ($Info_i = 1$) strong Republicans, and $\alpha + \beta_1 + \beta_2 + \beta_3$ is their predicted number of pros,

β_1 is the difference between least informed and most informed strong Republicans in the number of *con* arguments, and $\beta_1 + \beta_3$ is the difference in number of *pros*. The difference ($\beta_1 + \beta_3 - \beta_1 = \beta_3$) is the asymmetry in the association between information and consideration of pros versus the association between information and consideration of cons. It captures the extent to which the slope representing strong Republicans in Figure 2 diverges from the diagonal benchmark, i.e., the extent to which information and consideration do *not* correspond. Alternatively, one can interpret β_3 as the extent to which the pro versus con difference is strongly pronounced for well-informed strong Republicans ($\beta_2 + \beta_3$), compared to the less informed (β_2).

γ_4 is the vector of coefficients on the triple interaction terms representing the extent to which the asymmetric correlations differ by PID. If the relationship between information and the range of considered reasons is unaffected by PID, a model that restricts the triple interactions to zero should perform as well as a flexible model that allows γ_4 to be non-zero. A likelihood-ratio test easily rejects this null hypothesis in favor of the possibility that partisanship influences the differential correlations between information and pro and con arguments (Chi^2 (4) = 23.89, $P < 0.001$).

The estimates of Equation 2.3 are summarized in Table 3, which shows the predicted difference in the number of pros between the least and most informed respondents in each partisan group ($\beta_1 + \beta_3 + \gamma_2 + \gamma_4$), as well as their predicted difference in the number of cons ($\beta_1 + \gamma_2$). Also reported is $\beta_3 + \gamma_4 = (\beta_1 + \beta_3 + \gamma_2 + \gamma_4) - (\beta_3 + \gamma_4)$, indicating the gap in these two correlations (see Appendix A for a full report on the estimates of Equation 2.3). As with the previous model, there may be two

benchmarks. One may presuppose that "well informed" citizens should do better, compared to less well informed ones, *in articulating both pros and cons to the same extent.* In this the case, $\beta_1 + \beta_3 + \gamma_2 + \gamma_4$ should not be significantly different from $\beta_1 + \gamma_2$. This "null" hypothesis ($\beta_3 + \gamma_4 = 0$) is tantamount to assuming that the slopes in Figure 2 will parallel with the diagonal. As demonstrated in Table 3, Republicans, especially strong ones, clearly fall short of this standard. The relationship between information and consideration for strong Democrats appears to be slanted in the opposite direction, although to a much lesser degree ($\beta_3 + \gamma_4 = 0.99$, $p = 0.18$). According to an alternative standard provided by Independents ($\beta_3 + \gamma_4 = -0.44$), all but leaning Democrats show asymmetric relationship between information and consideration.[22]

Table 3: Associations between Information and Consideration by PID

	(1) Strong Rep	(2) Mod. Rep	(3) Independent.	(4) Mod. Dem	(5) Strong Dem
$\beta_1 + \beta_3 + \gamma_2 + \gamma_4$ (Pros)	0.08	2.13	2.94	3.08	5.56
	(0.88)	(0.61)	(0.43)	(0.59)	(0.70)
$\beta_1 + \gamma_2$ (Cons)	4.22	4.00	3.38	3.48	4.57
	(0.88)	(0.61)	(0.43)	(0.59)	(0.70)
$\beta_3 + \gamma_4$ (Gap)	-4.15[ab]	-1.87[ab]	-0.44	-0.41	0.99[b]
	(0.98)	(0.61)	(0.48)	(0.60)	(0.74)

Note. Entries are calculated by estimating Equation 2.3. Standard errors are in parentheses. N = 4,340. [a] $P < 0.1$ where $H_0: \beta_3 = 0$. [b] $P < 0.1$ where $H_0: \beta_3 = -0.44$.

[22] The mismatch between information and consideration is particularly severe for Republicans. Although I did not hypothesize it, this finding is in fact consistent with studies showing that conservatives tend to avoid dissonance-arousing situations (Nam et al., 2013), including exposure to counter-attitudinal information more than liberals (Garrett and Stroud 2014; Mutz 2006, 33).

Taken together, the correspondence between being *informed* and being *considerate* is far from perfect, and therefore there is little ground for treating these two characteristics as the same. Thus even a well-informed public's decisions may not approximate the decisions that a well-considered public would make. I explore this possibility in the following sections.

2.5 Informed Opinions versus Considered Opinions

Table 4: Support for Universal Health Insurance by Political Information

Political Information	Support
Very Low	0.64
	(0.01)
Low	0.63
	(0.01)
High	0.58*
	(0.01)
Very High	0.49*
	(0.01)
F (3)	24.02*

Note. N = 2,193. Standard errors in parentheses. *$p < 0.1$. The p values on each level are for the mean comparisons against the baseline category (Very Low). The F statistic (with 3 degrees of freedom) tests the null hypothesis that the mean differences are jointly zero.

In this section I compare informed opinions and considered opinions. As mentioned above, I use statistical simulation (and an experiment) to estimate them. But before presenting the relatively complex models, I first outline how people's opinions differ across different levels of information and consideration using mean comparisons. Table 4 compares the average support for universal healthcare of better informed and less informed citizens using a quartile spilt. I find a significant (monotonic) negative correlation between political information and the view that universal health insurance is a

good idea. The difference between the most informed and least informed citizens is quite sizeable (mean difference = –0.16, $p < 0.01$), with the former being evenly divided and the latter being generally supportive.

Table 5: Support for Universal Health Insurance by Consideration

Consideration		Support	
Balanced	0.63*	0.70*	0.71*
	(0.02)	(0.03)	(0.06)
Rest	0.59	0.59	0.59
	(0.01)	(0.01)	(0.01)
Cutoff for Balanced Consideration	3 or more	5 or more	7 or more
Observations Passing Cutoff	358	105	35

Note. N = 2,187. Standard errors in parentheses. *$p < 0.1$. The p values are for the mean differences between Balanced and Rest.

Does the same result arise when using consideration as the measure of political knowledge? To examine this question, I compare the mean support for universal healthcare of those who are fairly "well considered" against the rest of the respondents in Table 5.[23] I find that those who are well versed in both sides of arguments are *more* supportive of universal healthcare than others, contrary to what was found for "well informed" respondents. For example, the respondents who articulated five or more pros *and* five or more cons were substantially more likely to approve the universal health insurance policy than the rest (mean difference = 0.11, $p < 0.01$). Taken together, the

[23] Given the uncertainty regarding how many arguments one should articulate to qualify as exhibiting high levels of consideration, I use various cutoffs: from three pro and three con arguments to seven pros and seven cons. Unsurprisingly, the number of respondents categorized to have balanced consideration decreases rapidly as the definition of balanced consideration becomes more restrictive (from about 16.3% to 1.6%).

mean comparisons reported in Tables 4 and 5 provide preliminary evidence that the opinions held by "knowledgeable" citizens differ depending on how one defines and measures political knowledge.

To be sure, the group means in Tables 4 and 5 are only crude measures of "informed" and "considered" opinions.[24] I next follow the imputation methods developed by Delli Carpini and Keeter (1996) and Bartels (1996) to generate estimates of the counterfactual collective opinions held by the "fully informed" public, and the "fully considered" counterpart, while accounting for the most likely confounders and heterogeneity in the effects of information and consideration (for similar approaches to simulating other counterfactuals, see Kuklinski et al. 2000; Zhang 2010). First, I estimate the effects of political information based on a regression model that takes the following form:

$$Y_i = \beta_0 + \beta_1 Info_i + \varepsilon_i. \qquad (2.4)$$

where Y_i is support for a universal health insurance for individual i, $Info_i$ is political information, β_0 is the average support among those who are lowest in political information, and β_1 is the effect of information.[25] Given that people with different political values or self-interests will (rationally) use different evaluative criteria, I allow these parameters to vary across individual differences by adding a set of moderators to Equation 2.4. To ensure that the results are not sensitive to the choice of individual

[24] They do not account for likely confounding factors such as PID, and implicitly assume that people will be equally affected by political information and considered arguments.
[25] $Info_i$ is now a continuous variable ranging from 0 to 1.

factors, I try alternative specifications. In a more restrictive (but simple) variation of Equation 2.4, I include only party identification and its interaction with political information. A second specification includes demographic characteristics that are likely to affect the evaluative criteria, in addition to party identification.[26] Based on the estimated parameters, I simulate an "informed" public opinion by setting $Info_i$ to be the highest possible value (100% correct) for everyone and calculating the average of imputed Y_i. Accordingly, the differences between the counterfactual and the observed opinions constitute the anticipated opinion change upon each individual becoming "fully informed." And the mean of these differences is taken to be the "information effect."

For considered opinions, I begin by measuring the effects of considering arguments *for* a universal healthcare system, and the effects of considering arguments *against* by fitting (an extended version of) the regression model:

$$Y_i = \beta_0 + \beta_p k_i + \beta_c (n_i - k_i) + \varepsilon_i. \qquad (2.5)$$

where β_p and β_c are the effects of pro and con considerations, and k_i and $n_i - k_i$ are again, the numbers of pros and cons underlying individual *i*'s opinion.[27] As with

[26] The demographic variables include a variety of stable characteristics pertinent to healthcare policy preferences (see Appendix A, for the full list). They are similar to those used in previous research (e.g., Althaus 1998), but also include several variables tapping personal health status, and healthcare conditions (e.g., whether insured or not). In the regression analysis, these variables and their interaction with political information are added to Equation 2.4.

[27] One can think of Equation 2.5 as a simplified version of Equation 2.1, which cannot be estimated as it is. The simplification is that for individual *i*, all pro arguments are equally effective, as are all con arguments.

Equation 2.4, I allow these effects to vary across individual differences using two different specifications; one including PID only and the other adding demographic factors.[28] The key difference from "informed" opinion is that this time I set both k_i and $n_i - k_i$ to be equally high numbers for everyone, thereby forcing a balance in pros and cons—a feature that is absent in the estimate of "informed" opinions.[29]

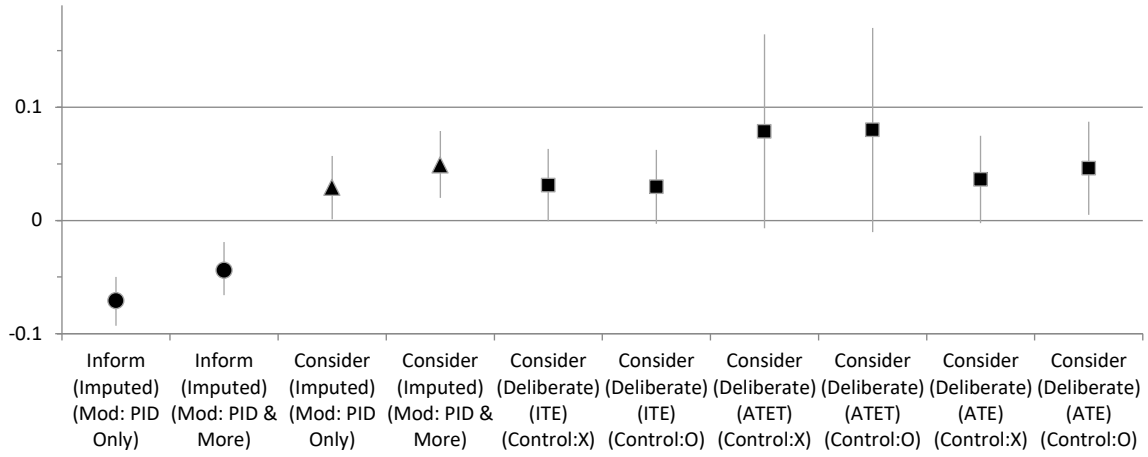
The results of these various models are presented in Figure 3.[30] The circles and triangles in the left half of the figure compare the imputed estimates of information effects against consideration effects (with and without demographic moderators). Each marker represents the point estimate of the difference between imputed informed or considered public opinion and the baseline of actual public opinion. The bars report the

[28] I also examined models that included interactions between pros and cons, allowing for the possibility that the effect of one side of arguments depends on awareness of the other side. The results were robust to this specification choice (See Appendix A). I report the results from the simpler specification without the interaction term here.

[29] Pro and con arguments had extremely skewed distributions; while the maximum values were 42 and 48 respectively, for the most part respondents articulated five or less arguments for each side (87.5% fall in this range for pros and 87.6% for cons). In fact, only one respondent listed more than 15 pros and 15 cons (see also Figure A1 in Appendix A). Given the sparseness of observations near the actual maximums, "fully considered opinions" fixed at these values will be mere extrapolations. Consistent with the mean comparison analyses (reported in Table 5), I employ more reasonable definitions of "full consideration"—5 pros and 5 cons, and set the maximum of number of arguments to be 5, by collapsing values greater than 6 into the maximum category. Admittedly this is an *arbitrary* choice in that there is no theoretical ground to say consideration of 5 reasons is enough, and 4 is not. Yet, the findings are generally robust to alterative specifications using different cutoffs—3 or 7 arguments on both sides—and using percentile values, which does not require (arbitrary) selection of cutoffs (See Appendix A).

[30] The confidence intervals and standard errors are calculated using bootstrapping. Each set of analyses—i.e., regression and simulation—are replicated on 2000 bootstrap samples drawn from the original sample with replacement. Thus the standard error and confidence interval for fully informed preference are the standard deviation, and 5th and 95th quintiles of its values from the 2000 samples.

Figure 3: Comparing Information Effects versus Consideration Effects



Note. The dependent variable is support for universal health insurance. Information Effects, denoted by the circle markers, represent the simulated estimates of public opinion change that is anticipated when the public is fully informed. Imputed Consideration Effects, denoted by the triangular markers, represent the simulated estimates of public opinion change anticipated to occur when the public considers a "full" range of competing arguments. Deliberation effects, denoted by the square markers, represent the estimated effect of actual deliberation on opinion change. Vertical lines represent 95% confidence intervals. See Appendix A for more information.

95% confidence intervals. As shown in the figure, imputed opinions significantly differ from actual public opinion. More importantly, considered and informed public opinions differ from actual opinion *in the opposite direction*. The estimated values suggest that while informed public opinion is *less* favorable to a nationalized health insurance program than actual collective opinion by 4.4 to 7.1 percentage points (both statistically significant at $p < .05$), considered public opinion is *more* favorable. The size of the "consideration effect" reported here varies somewhat—from 2.9 to 4.9 percentage points—but these estimates are consistently positive, and the lower bounds of the 95%

confidence intervals remain above zero with and without additional moderators in the model.

In sum, this analysis suggests that the consideration effect on collective opinions significantly diverges from the information effect, not only in magnitude, but also in direction. This discrepancy seems to imply that the estimates of "fully informed" public opinion (Althaus 1998; Bartels 1996; Delli Carpini and Keeter 1996) may reflect partisan bias in awareness by leaving open the possibility that well informed citizens' political knowledge is generally imbalanced.

It is important note that the statistical imputation approach still requires some strong modeling assumptions, particularly that there is no unobserved confounder. I now turn to a randomized experiment on deliberation as a means to measure the *causal* effect of consideration. If imputation and experimentation are both valid tools for identifying more considered opinions, I should find that these estimates correspond to one another. I fit the following regression model:

$$\Delta Y_i = \beta_0 + \beta_1 Del_i + \varepsilon_i \quad (2.6)$$

where $\Delta Y_i$is change respondent *i*'s support for universal healthcare system before and after deliberations, Del_i indicates deliberation, and β_1 is the causal effect of interest. As mentioned above I use three different specification approaches—(1) ITT, (2) ATET, and (3) ATE. The difference among the three is how deliberation is operationalized—(1)

assignment, (2) assignment as the instrument for attendance, and (3) attendance.[31] I fit each of the specifications with and without (pre-treatment) covariates, yielding a total of 6 estimates of deliberation effects (see Appendix A for full reports of these models including the list of covariates). The rectangular markers in the right half of Figure 3 report β_1 in Equation 2.6, across these specifications.

I first present the result of regressing opinion change on group assignment status (without controls), which shows a treatment effect that is consistent with the imputed estimates of *consideration* effect (β_1 = 0.031, p = 0.056). [32] The next model, shown in the second rectangle, adds the lengthy list of control variables, but still produces a remarkably similar figure (β_1=.030; p = 0.074). These two estimates capture the "intention-to-treat" effect, which represents the impact of being assigned to a treatment group, while ignoring the rate at which those in the treatment group actually receive it. To calculate the ATET, I turn to 2SLS regression (the third and fourth triangles in Figure 3).[33] I find that actual treatment (i.e., deliberative participation) increases support for universal healthcare system by 7.8 to 8.2 percentage points for those would receive treatment when assigned. Finally, comparing individuals on (endogenous) deliberation attendance, I find the effect of deliberating about healthcare corresponds to 3.6 percentage points greater support for universal healthcare system, compared to non-

[31] Attendance is coded as the following; 0 = none; 0.5= once; 1 = twice.
[32] All test statistics are calculated using robust cluster standard error that accounts for within-discussion-group correlations. I rescale the number of deliberations participated in to range between 0 (none) and 1 (twice). See Appendix A for more information on the models.
[33] ATET is identical to the ITT effect divided by the average rates of deliberative participation among the treatment group.

deliberators (p = 0.067). And holding fixed individual factors *increases* the size of the point estimate (β_1=.046; p = 0.028), implying that omission of some potential confounders, if anything, may have led to the *underestimation* of the deliberation effect.[34] Taken as a whole, Figure 3 makes a very straightforward point: across various specifications, deliberation moves public opinion in the direction predicted by my simulation of "considered opinion," which is at odds with "informed opinion."

2.6 Summary

To summarize, this chapter underscores the discrepancy between two dimensions of political knowledge—information and consideration—by providing the following evidence. First, the range of considered argument is generally slanted in the direction of partisan identities, and this tendency is far stronger among those who possess high levels of factual information. Second, the statistical simulations of considered and informed preferences indicate that they are not the same thing, significantly diverging on preferences for government-sponsored health insurance. Third, on average, deliberative intervention—an experimental method for producing considered opinions—pushed public opinion in the same direction predicted by the imputation method.

It is important to note that whether these estimates of considered opinions are normatively "better" than the baseline is still an open question. One may argue that

[34] Note that the latest deliberation took place about two months before the measurement of the post-test outcome variable (Table 1). Considering that communication effects in general tend to disappear rather rapidly (e.g., Gerber et al. 2011), this result is impressive.

competent decision making requires more than the reception of a broad range of messages. In particular, the Bayesian learning model dictates *how* citizens should revise their opinions upon receiving a stream of information: the most certain evidence should carry the greatest weight in opinion formation and revision. Importantly, the analyses thus far have ignored the question of whether the quality of argument matters as citizens form their policy opinions. Neither the certainty of arguments people had considered prior to the surveys nor the certainty of the messages they received during deliberation was measured. In the following chapter, I examine *how* people use received policy information more directly by testing the effect of evidence strength.

Chapter 3 The Force of the Better Evidence

In terms of multi-step models of attitude change (McGuire 1969; Zaller 1992), Chapter 2 focused (mostly) on the "reception" stage. It has been assumed that facilitating more considered public opinions is largely a matter of solving the problems of ignorance and partisan selective reception. But arguably the most damaging blow to citizen competence comes from the notion that it does not even matter whether people have substantive information about policy issues because they lack the ability to use information rationally anyway. While it is the central assumption of deliberative democracy that citizens *are* capable of forming meaningful opinions led by "the force of the better argument" in the marketplace of ideas (Habermas 1984; Fishkin 1995; Mercier and Landemore 2012), there has been little empirical evidence that directly supports it—and plenty that calls it into question (for reviews of such evidence see Delli Carpini et al 2004; Mendelsberg 2002; Mutz 2008).

From a theoretical standpoint, there are two important reasons to think that it does not matter whether citizens have detailed facts about the political issues of the day, and the prospects of deliberative democracy are hopelessly untenable.

1. The Uncritical Public Hypothesis: People do not reason for themselves. They defer readily to elite opinions (usually their party leaders' positions), so much so that they ignore substantive information about the merits of the policies at hand

(Broockman and Butler 2017; Cohen 2003; Lenz 2012; Rahn 1993; Zaller 1992; see Bullock 2011 for a review).

2. The Motivated Public Hypothesis: People are motivated to defend their partisan viewpoints. They vigorously discount uncongenial information when confronted, even to the point of bolstering prior attitudes (Kunda 1990; Lord et al. 1979; Taber and Lodge 2006; Nyhan and Reifler 2010).

But what empirical observation constitutes the evidence against the people can pick and choose good argument in the marketplace of ideas? For example, if people change their minds after hearing an argument, are they being "manipulated" or "educated" (Mansbridge 2003)? If they don't change their minds, are they exhibiting healthy skepticism or irrationality? Teasing out these normative interpretations requires defining *ex ante* the qualitative aspects of strong versus weak arguments that should (or should not) make them persuasive—an approach that few studies on political persuasion have taken. As such, I argue in the following section that prior evidence remains more ambiguous about how critically and rationally citizens respond to new information bearing on political issues than is often supposed. In this chapter, I fill this vacuum in the literature by presenting three experiments examining whether people take account of *evidence* strength, which permit more straightforward normative interpretation than existing studies that draw on the general (and underspecified) concept of argument strength.

Across the experiments drawing on highly contentious issues in American politics—the Affordable Care Act, and the economic performance of the two major parties— I find that people update their beliefs and attitudes in light of presented arguments. According to the results, people do not mindlessly accept whatever arguments they encounter, nor do they categorically reject uncongenial arguments. Instead, people account for the (un)certainty of evidence as they form their posterior opinions, even when it disconfirms their prior opinions. The patterns of belief and attitude changes imply that people sensibly evaluate the diagnostic values of presented evidence in more or less the same way across prior attitudes, partisan identity, and other individual traits. These findings make a straightforward case for a Bayesian model of persuasion (specified below), under which people revise their beliefs by calculating the probative values of new information.

3.1 The Uncritical Public Hypothesis

Zaller's (1992; 1996) RAS model provides a good starting point for the first claim. His model aims to explain the nature and origins of mass opinion as a function of elite discourse, and ideology while presuming no critical decision making on the part of citizens. The patterns in public opinion depend on the two elite discourse variables—relative balance between competing sides, and overall intensity of issue coverage—but not on how citizens critically evaluate these messages. While he notes that better informed citizens are more likely to resist incoming messages, even this is far from the

kind of critical reasoning that characterizes the democratic ideal in that these individuals are not assumed to scrutinize the content of messages in doing so. As Zaller (1992, 45) puts it, his "postulate makes no allowance for citizens to think, reason, or deliberate about politics: If citizens are well informed, they react mechanically to political ideas on the basis of external cues about their partisan implications, and if they are too poorly informed to be aware of these cues, they tend to uncritically accept whatever ideas they encounter."

It is important to note that Zaller (1992, 3) does not provide a direct *test* of this mechanical portrait of the populace. It is, rather, a simplifying assumption necessary for "a unified theory of its major empirical regularities," which after all proved useful. In his famous analyses on Americans' reactions to the Vietnam war, Zaller (1992; 1996) was able to explain complicated shifts in mass opinion even though (or because) his model did not take into account citizens' ability to think for themselves. For instance, even in the face of an extremely problematic situation in Vietnam in 1970, conservatives—especially the informed ones—maintained their pro-war position, which, according to Zaller (1996, 57-58), indicates that "independent thought among highly aware conservatives—or, for that matter highly aware liberals—is entirely absent" (see also Lenz 2012).

It is, however, difficult to know based on observational research (e.g., Lenz 2012; Zaller 1992) if citizens rely on party cues because they know little more than that, or they instead do not reason using the substantive policy information they possess (see Bullock 2011, 510-511). In fact, recent experimental studies that provide some detailed

facts about various political issues have documented information-based persuasion (e.g., Boudreau and MacKenzie 2014; Bullock 2011; Guess and Coppock 2015; Mérola and Hitt 2016; but see also Broockman and Butler 2017). In particular, a series of experiments by Chong, Druckman and their colleagues report that the strength of arguments/frames drives policy opinions (e.g., Chong and Druckman 2007; 2010; Druckman et al. 2013), even when participants are provided with party cues (Druckman et al. 2013).[35] In light of these findings, it may be tempting to draw the sanguine conclusion that citizens are able to make competent decisions guided by the actual merits of the arguments after all.

There is a major caveat though: the absence of a well-articulated definition of the elements of strong arguments (see Chong and Druckman 2007, 652; Druckman et al., 2013, 75). This is one of the longstanding difficulties in persuasion studies that has hampered the systematic understanding of the relationship between specific message variations and persuasion as well as the role of argument quality in persuasion in general (Areni and Lutz 1988; O'keefe and Jackson 1995). As unsatisfactory as it might be, argument quality has been defined in terms of perceived effectiveness, and operationalized based on pretests identifying strong versus weak arguments (e.g., Chong and Druckman 2007). The problem with this approach is that to derive a normative implication from such a finding is to make the *assumption* that those who pretest the arguments *can* (and do) evaluate them based on some normatively desirable standards

[35] But the effect of argument strength disappears when participants are told that Democratic and Republican politicians are polarized on the issue at hand (Druckman et al. 2013).

(e.g., evidentiary certainty), which is the very thing one should to test in order to assess the quality of political reasoning.

This issue is well understood and articulated by Chong and Druckman (2007, 652), who note: "strong frames that emerge from debate will reflect a political process in which the persuasiveness of a claim depends on more than its validity or relation to evidence. The elements of an argument that make it plausible or compelling seem to reside as much in its source and the cultural values and symbols as in its causal logic." To the extent that these extraneous factors are not held constant in operationalization, it opens up the possibility that the observed effect of argument strength is in fact confounded with, for example, the effects of ideological differences across arguments.

Suppose, for example, that in an experiment, a "strong" argument that the ACA will reduce the number of uninsured Americans was more persuasive than a "weak" argument that the free market can handle various problems. A normatively pleasing explanation may be that pre-testers successfully identified a better argument, which then generated opinion change in the experiment. An alternative interpretation, however, is that there is nothing inherently more valid about the pro-ACA argument, but that both pre-testers and experiment participants favored it anyway, simply because it was more consistent with their prior attitudes and values—a phenomenon called "biased assimilation" (Lord et al., 1979), "prior attitude effect," or "congruency bias" (Taber and Lodge 2006) in the motivated reasoning literature.

3.2 The Motivated Public Hypothesis

In RAS, people rely on partisan cues simply because they are unable to think critically (Zaller, 1992). According to the motivated reasoning theory, on the other hand, they hold onto partisan viewpoints because they have motivations to do so. The theoretical underpinnings of motivated reasoning are provided by Kunda (1990), who notes that two general classes of motivations guide how people reason: (1) accuracy goals, which lead people to consider a broad range of alternatives and carefully process relevant information; and (2) directional goals, which is activated when people desire to reach certain conclusions or defend their prior attitudes, usually leading to disconfirmation of uncongenial information. While the reasoning strategies under the accuracy motivations may be in much agreement with the democratic ideal (see Druckman 2012), the defensive partisan motivations are assumed to be the general default when it comes to political reasoning (Taber and Lodge 2006; 2012).

Classic evidence for the motivated reasoning hypothesis is found in the experiment by Lord and his associates (1979), which documents that proponents of the death penalty thought the studies showing pro-deterrent information were more "convincing" and "well-done," whereas opponents favored the studies demonstrating the opposite, even though the quality of methodology was essentially the same (similar findings are reported by Corner et al 2012; Lodge and Taber 2012; Miller et al. 1993; Munro and Ditto 1997; Taber and Lodge 2006; Taber et al., 2009). And "biased

assimilation" of evidence is likely to lead to attitude polarization.[36] "Such motivated reasoning," Taber et al. (2009, 138) argue, "is hard to square with normative standards for belief updating, which require some independence of new evidence from priors."

Unlike the Lord et al. (1979) study, the arguments used by Taber, Lodge and colleagues are rhetorical points (recurrent pro and con frames in controversies on affirmative action and gun control), rather than "hard" evidence regarding a particular aspect of an issue. Albeit advantageous in external validity, this approach leaves open the possibility that their findings simply reflect value differences instead of motivated biases or irrationality. Indeed, for the findings of "prior attitude effect" or "polarization" to be considered evidence of a *bias* or *irrationality* of any kind, one should rule out this alternative explanation. As an example, suppose that Democrats care about reducing disparities between rich and poor, and Republicans care about reducing the role of government, and they are exposed to a pro-ACA argument that it can help those who couldn't afford a decent healthcare, and a con argument that the federal government will impose more regulations under the ACA. It is not inconsistent with the democratic (or Bayesian) standards when Democrats find the pro argument more convincing, whereas Republicans favor the con. Nor is it problematic in any sense that they polarize after receiving the two arguments. Their "fully informed" preferences of a public policy (per Equation 2.1) can very well be divergent to the extent that Democrats and Republicans use different evaluative criteria. So if they were in (some) agreement due to insufficient

[36] Another component of motivated reasoning examined in Taber and Lodge (2006) is selective exposure. This is not my focus here because I aim to examine how people *use* information after exposure, not whether they *receive* it to begin with.

knowledge, it is expected that partisans who process the pro and con arguments in an unbiased fashion move in the opposite directions.

Therefore, *to the extent that values/frames are not held constant in research design*, the benchmark requiring evaluation and assimilation of new arguments to be *independent* of priors are unduly restrictive. To uphold this requirement is to suggest either that people with different prior attitudes and values should use the same evaluative criteria, or that there are no value differences in the populace, both of which seem untenable on normative as well as empirical grounds. In this regard, some of the most widely cited evidence from the literature on motivated reasoning (e.g., Edwards and Smith 1996; Taber and Lodge 2006; Taber et al., 2009) is not really a strong indictment of the citizenry. It may as well be a testament to value diversity in the American public.

What about evidence that seems to rule out the possibility that apparent "congruency biases" in argument assimilation simply reflect value differences? Most notably, in the experiment by Lord et al. (1979), both sides of evidence presented to participants address the same subtopic—deterrence effect—ensuring that the differential evaluations of congenial versus uncongenial information is not driven by differences in moral principles held by those who initially favor and oppose death penalty (for similar results, see Houston and Fazio 1989; Munro and Ditto 1997; Schuette and Fazio 1995; for a review of evidence from non-political issues see Kunda 1990). According to the motivated reasoning literature, this evidence shows that "judgments about the validity, reliability, relevance, and sometimes even the meaning proffered by evidence are biased" (Lord et al. 1979, 2099).

But while the observed correlation between priors and evaluation of incoming information would be a necessary condition for establishing people's tendency to selectively denigrate the diagnostic value of uncongenial information, it is not a sufficient one. This is because people's answers to the survey items probing the perceived quality/strength of information (or arguments) can be causally posterior to their updated conclusions. As an example consider a case of two unbiased Bayesians starting from different priors. Gina thought there was an 80% chance that the ACA will successfully "bend the cost curve." Ian thought there was only a 10% chance. Assume that they both received and understood the same piece of pro-ACA evidence, and also *interpreted the evidence in the same way*; both thought the likelihood of observing that particular data when the ACA actually controls cost is 2.25 times greater than when the opposite is true. Following Bayes' rule strictly, both revised their beliefs upward; Gina now thinks there is a 90% chance, and Ian says 20%. However, when asked whether they found the evidence "convincing," they give conflicting responses, despite the fact that they are in full agreement about the probative values of the evidence *per se*, because Ian, after all, was not "convinced" (Gerber and Green 1999, 197-199).

Without making the implausible assumption that their *responses* to evidence evaluation questionnaires—typically termed "convincing," "persuasive," "strong" and so on—are strictly independent of the posteriors, the only condition under which Gina and Hyoja will give the same answers to the survey items would be when they base their conclusions entirely on the evidence and completely discard their prior knowledge. Unless the evidence is indisputably clear, "openness" of this kind is an obvious violation

of Bayes' Theorem. Arguably, this is even more problematic than biased assimilation, because "[w]illingness to interpret new evidence in the light of past knowledge and experience is essential for any organism to make sense of, and respond adaptively to its environment" (Lord et al. 1979, 2107). This "bias" keeps people from being swayed by highly implausible claims (e.g., conspiracy theories)—an exercise that is probably more beneficial than harmful in general.

What is clearly *not* normative is to judge the plausibility of new information based on one's prior attitude *and,* as a result, push forward the very attitude that colored the perception of new information (Gerber and Green 1999; Lord et al., 1979; Ross 2012). The evidence for polarization of this kind is mixed, however. While Lord et al. (1979) found that participants strengthened their views after reading a set of mixed evidence, it is important to note that they did not directly observe opinion change with pre- and post-treatment measures, but instead relied on self-reported opinion change. Subsequent research tapping *actual* (instead of self-assessed) opinion changes failed to replicate Lord et al.'s (1979) findings on polarization (e.g., Guess and Coppock 2015; Miller et al. 1993; Munro and Ditto 1997), although there are important exceptions (e.g., Nyhan and Reifler 2010). [37]

[37] Let me reiterate that unless values/frames of arguments are held constant, it does not necessarily take defensive motivations for people with different political values to polarize after receiving a balanced set of pro and con argument (e.g., Taber and Lodge 2006; Taber et al., 2009). In these studies, polarization *per se* is a poor indicator of irrationality or partisan bias.

3.3 A (Restrictive) Bayesian Learning Model

In sum, the normative implications of the extant evidence on political information processing are more ambiguous than is often assumed. This of course is not to say that we have reason to believe that citizens meet any normative standard, but rather that it is surprisingly difficult to draw any normative conclusion, the standard itself is a moving target. In following section, I specify a *restrictive* Bayesian model that will be used as a benchmark against which to judge the results of the experiments presented below.

Suppose Gina, our Bayesian voter, receives an argument with some evidence that the ACA will slow down cost growth. The model states:

$$P(H|E) = \frac{P(E|H) \cdot P(H)}{P(E)} \qquad (3.1)$$

where *H* is a hypothesis about the Affordable Care Act (e.g., that the net benefit of the law is greater than zero); *E* is evidence relevant to the hypothesis; $P(H)$ is Gina's prior probability that *H* is true, before observing *E*; $P(H|E)$ is her posterior probability, revised in light of *E;* and $\frac{P(E|H)}{P(E)}$ is the likelihood function, representing her interpretation of how the evidence bears on the hypothesis at hand. This term can be rewritten as:

$$\frac{P(E|H)}{P(E|H) \cdot P(H) + P(E|{\sim}H) \cdot P({\sim}H)}$$

where $P(E|H)$ is her belief about the likelihood of observing *E* when *H* was true, and $P(E|{\sim}H)$ is the likelihood of observing E when the contrary was true.[38] The ratio between $P(E|H)$ and $P(E|{\sim}H)$—the likelihood ratio—dictates in what direction, and how much, Gina should update her belief about *H* after observing E. The likelihood ratio of completely uninformative evidence will be 1, in which case, the posterior will be the same as the prior. And the larger the difference the between the likelihoods, the more will Gina's posterior tend toward the evidence.

It is worth emphasizing that Bayes' Theorem does not tell Gina how to construct her likelihood function; it is up to her own judgments, which would reflect not only some *objective* properties of *E*, but also her *subjective* evaluations of *E*. To be sure, if the treatment of new evidence is left fully subjective (i.e., there is no constraints on the acceptable treatment of new evidence) a Bayesian model becomes tautological because it can account for any belief change (Taber et al. 2009). But then again, one should not conflate subjectivity *per se* with bias. Although "[h]ow we determine the boundary line between rational skepticism and irrational bias is a critical normative question, but one that empirical research may not be able to address," the fact remains that one somehow should draw the line "to resolve the controversy over the rationality of motivated reasoning" (Taber and Lodge 2006, 768). In order to make a Bayesian model falsifiable, it is necessary to put constraints on likelihood functions based on the properties of *E*.

[38] My discussion here focuses on a dichotomous parameter—the net effect of the ACA is either positive (*H*) or negative (~*H*)—, this Bayesian framework can be extended to a continuous parameter—the value of the net effect—using probability density functions.

In a recent study, Guess and Coppock (2015) address this dilemma by imposing the restriction that the difference between the likelihoods, $P(E|H) - P(E|{\sim}H)$, is "correctly" signed, which does not allow people to have an extreme likelihood function that would lead one to believe, for instance, that evidence of strong deterrent effects is good news for the anti-death penalty position. I build on their research by introducing an additional restriction that individuals' likelihood functions are constrained not just by the direction but also the strength of evidence. That is, Gina grasps the differences in the diagnostic values of an event that is *far* more likely to occur when *H* is true than is not (strong evidence), and an event that is *slightly* more likely to occur when *H* is true than is not (weak evidence). More formally, it means Gina's subjective likelihood ratios are lined up such that $\frac{P(E_{SP}|H)}{P(E_{SP}|{\sim}H)} > \frac{P(E_{WP}|H)}{P(E_{WP}|{\sim}H)}$, where E_{SP} and E_{WP} are strong and weak pro evidence for *H.* Likewise, $\frac{P(E_{Sc}|{\sim}H)}{P(E_{Sc}|H)} > \frac{P(E_{Wc}|{\sim}H)}{P(E_{Wc}|H)}$ where E_{SC} and E_{WC} are strong and weak con evidence against *H.*

Imposing this restriction requires defining the *objective* elements of evidence that would compel Gina toward the position it supports—i.e., what makes a piece of strong evidence strong. I define these elements as the commonly held standards of statistical, external, and construct validity, upon which social scientists readily draw to evaluate the quality of research findings (e.g., Shadish et al. 2002). Accordingly, in the experiments presented below, "strong" evidence describes, for instance, more sizeable effects of the ACA on health care costs, based on more externally valid, and more relevant, data.

No one thinks that the average citizen is as skilled in making such assessments of evidence as trained researchers (Niesbett and Ross 1980). But I would argue that people

routinely calculate the probative values of various kinds of information they encounter, guided by common sense and logic, which allow them to notice the difference between strong versus weak evidence. The theoretical expectation of the (restrictive) Bayesian model specified here is that the following equation holds, regardless of prior attitudes:

$$P(H|E_{SC}) < P(H|E_{WC}) < P(H) < P(H|E_{WP}) < P(H|E_{SP}).$$

If, alternatively, people accept whatever arguments they encounter, without accounting for the uncertainty of the evidence, one will find $P(H|E_{SC}) = P(H|E_{WC}) < P(H) < P(H|E_{WP}) = P(H|E_{SP})$. If this holds true, political persuasion is possible, but even to the point where politicians can get away with claiming whatever they want citizens to believe. If people categorically reject counterevidence to defend their priors, one will find $\mathrm{P}(H|E_{SC}) = P(H|E_{WC}) = P(H)$ for proponents (or Democrats), and $\mathrm{P}(H) = P(H|E_{WP}) = P(H|E_{SP})$ for opponents (or Republicans). If people backlash against counterevidence, which is the most extreme form of motivated reasoning, one will find $\mathrm{P}(H|E_{SC}) > P(H)$ and $\mathrm{P}(H|E_{wC}) > P(H)$ for supporters (or Democrats), and $\mathrm{P}(H|E_{SP}) < P(H)$ and $\mathrm{P}(H|E_{WP}) < P(H)$ for opponents (or Republicans). If this holds true, political persuasion would not be generally possible.

In what follows, I present three experiments that test these competing hypotheses. To the extent that motivated reasoning overwhelms evidence-based opinion revision, "hot button" issues should provide the hardest context under which to detect the effect of evidence (Taber and Lodge 2006, 757). With that in mind, the experiments draw on two

of arguably the most contentious issues in American politics today—the Affordable Care Act (Experiments 1 and 2), and the economic performance of the two major parties (Experiment 3). The experiments examine how participants' posterior beliefs and attitudes vary in response to arguments coupled with varying degrees of evidence certainty.

3.4 Experiment 1: A Pilot Test

The purpose of Experiment 1 was to pilot-test the materials—theoretical explanations and factual evidence—that would then be used to construct the arguments for Experiment 2. The dependent variable was people's guesses about the ACA's impact on health care costs, conditional on a range of evidence that varies in position (good or bad news for the ACA) and strength. These characteristics of presented evidence constitute the key independent variables of Experiment 1. It thus represents a preliminary test of how people would interpret the diagnostic values of various pieces of information, and update their beliefs. The data were gathered from 246 participants recruited via Amazon's Mechanical Turk in March 2016. Each participant was paid $0.75.

Research Design

As noted earlier, it is important to hold "frames" constant to compare people's responses to political arguments against a normative standard. Among several economic and social dimensions of the health care debate, I chose health care costs as the focus of

the Experiment 1 (and Experiment 2) because prior research shows it to be the most prevalent frame in public discourse about this topic, and used in arguments made by both sides (Druckman et al. 2012, 434). It also seemed plausible that both supporters and opponents of the law would consider it an important evaluative criterion, unlike some of the other considerations about the ACA (e.g., the government's involvement in health decisions).[39] Throughout Experiments 1 and 2, "pro evidence" refers to the ones that suggest the ACA is successfully "bending the cost curve", and "con evidence" refers to the ones that suggest the law makes health care even more expensive.

To maximize the realism of experimental stimuli, I draw on actual facts published in research articles and reports (a full list of evidence and sources is offered in Appendix B). I selected four "pairs" of pro evidence, and four "pairs" of con evidence, each pair consisting of a strong version and a weak version. The strong versions are "quasi-experimental" findings that describe (a) noticeable differences (b) at the national level (c) in relevant statistical data. I created their weaker counterparts by manipulating some of these dimensions—(a) smaller differences and/or (b) local level evidence, and/or (c) arguably irrelevant statistics—as permitted by the information that I was able to find. I also included six pieces of evidence—two strong and four weak (as judged by the same criteria). In total, 22 pieces of evidence that vary in direction (11 pros and 11 cons) and certainty (10 strong and 12 weak) were pre-tested. While I did not attempt to predict the rank order of the likelihood ratios among all 22 pieces of information, I did expect that

[39] This decision makes it easier to rule out value differences as a convincing explanation for a partisan difference in treatment effects. But the tradeoff is that the lack of variation in importance of the sub topic makes it difficult to detect its moderating effects.

evidence classified as strong *ex ante* would have higher likelihood ratios on average, in people's judgments. Testing this expectation was the point of Experiment 1.

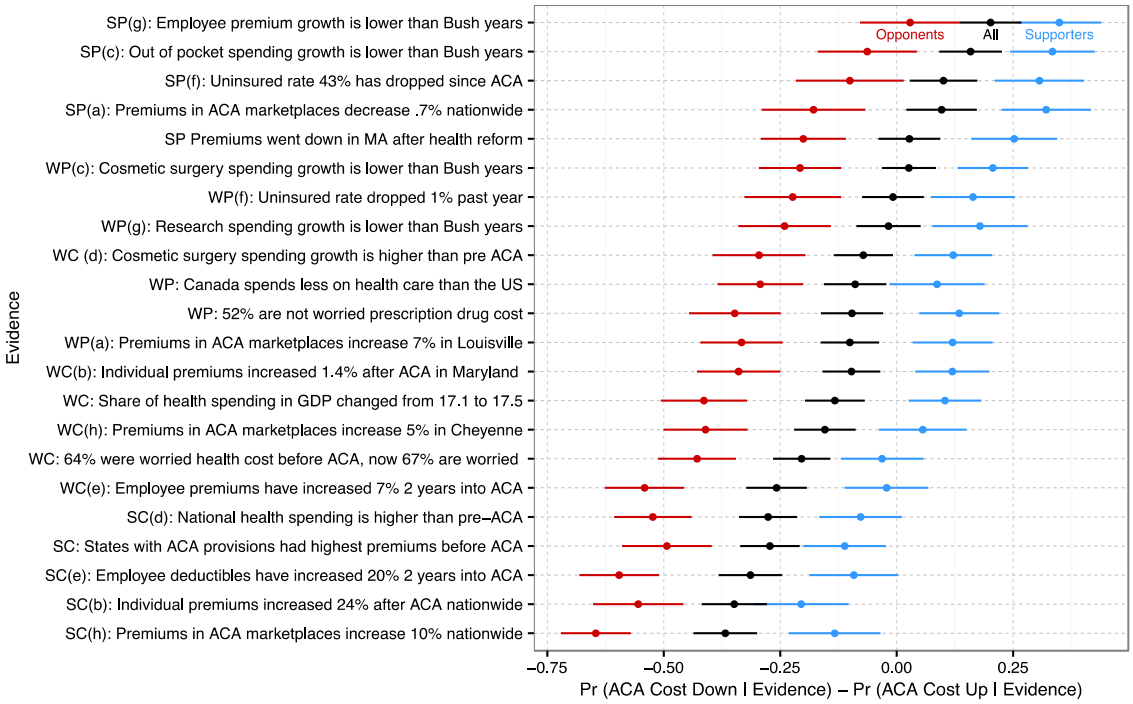
The experiment was conducted as follows. After answering pre-treatment questionnaires, participants read a message summarizing "health economists' theory" about the ACA's impact on health care costs. Participants were randomly assigned to one of two conditions. Half of the participants were told the ACA would slow down growth in health care costs because Obamacare would solve the "adverse selection problem" where those buying insurance were sicker than average (the pro condition). The other half were told the ACA would accelerate cost growth because community rating would *create* the adverse selection problem, where medical costs for healthy people would rise, leading some of them to exit the insurance market (the con condition). Texts of the messages are provided in Appendix B.

The dependent variable in this experiment is the conditional probability that the ACA restrains (or drives up) health care costs, in light of each piece of evidence drawn from the pool of information described above. Participants assigned a conditional probability on a scale varying from "extremely low (5% chance)" to "extremely high (95% chance)," given that a hypothetical new study found that "during the first two quarters after the reform, premiums in the individual health insurance market increased by 24.4% over what they would have been without Obamacare across all states." This task was repeated for the 22 facts in random order, making for a 2 (between-subject) by 22 (within-subject) factorial design.

Results

The mean difference between the two experimental groups represents the conditional probability that the ACA controls cost growth given each piece of evidence (scaled to vary between 0.05 to 0.95) minus the conditional probability the ACA drives up costs given the same evidence. If a piece of information makes every respondent believe that Obamacare will definitely bend the cost curve, the group difference given this evidence would be 0.95-0.05=0.9. The main interest here is how this statistic varies across the 22 pieces of evidence.

Figure 4: Conditional Probabilities Given Evidence



Note: OLS estimates. Horizontal lines are 95% confidence intervals. Standard errors are clustered at the respondent level. “S” indicates “Strong.” “W” indicates “Weak.” “P” indicates “Pro.” “C” indicates “Con.” Lower case letters in parentheses pair a piece of strong evidence with its weaker version. For example, SP(a) and WP(a) are a pair.

Figure 4 reports the main results from regression models where conditional probabilities were regressed on (1) experimental condition, (2) dummy variables indicating each piece of evidence, and (3) the interactions between condition and the dummies.[40] To closely examine whether people interpret evidence differently across their prior attitudes, I also present the model that draws on only opponents of the law (87 respondents) as well as the model drawing on only supporters (100 respondents). The estimates are ordered on the vertical axis so that the evidence that drives conditional probabilities to the pro-ACA direction the most is located on the top.

Several patterns stand out at a glance. First, the facts predetermined to be strong pro evidence appear in the first five rows, and those predetermined to be strong con are located at the bottom. This means that participants do take account of evidence strength, and their interpretations are lined up as intended. Second, none of the evidence is powerful enough to eliminate the gap between opponents and supporters. This indicates people would reach different conclusions based on the same evidence. But more importantly, there is a direct correspondence between how proponents' conditional probabilities vary across evidence, and how opponents' vary. This means people starting

[40] Across all experiments, I embedded a screener question right before the treatments take place (Berinsky et al. 2014). In Experiment 1 participants were instructed to "ignore the question and select the Drudge Report and none of the above as your two answers," before being asked to identify the news website they visit. 71.5% of participants correctly marked the two options, whereas 15.5% marked only one of them—mostly failing to notify the "none of the above" part of the instruction. I dropped the remaining 13%, who would most likely add noise, and underweighted those who marked only one option correctly by 0.5. The same approach was taken for Experiments 2 and 3 as well. The results remain almost identical when low-attention participants are kept in the analyses and given the same weight as high-attention participants. Also note that non U.S. citizens were also excluded from all analyses.

from different priors are in good agreement with one another about the diagnostic values (i.e., the likelihood ratios) of each piece of information. There is no clear indication that they undervalue counterevidence. Overall, the results suggest that people match very nicely to the *restrictive* Bayesian standards specified above.

Table 6: Conditional Probabilities Pooled by the Type of Given Evidence

	(1)	(2)	(3)	(4)	(5)	(6)
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Strong Con	-0.316	-0.563	-0.124	-0.327	-0.580	-0.127
	(0.028)	(0.034)	(0.038)	(0.029)	(0.034)	(0.039)
Weak Con	-0.153	-0.404	0.058	-0.141	-0.396	0.081
	(0.027)	(0.035)	(0.033)	(0.027)	(0.037)	(0.033)
Weak Pro	-0.047	-0.275	0.148	-0.024	-0.255	0.171
	(0.025)	(0.033)	(0.032)	(0.026)	(0.036)	(0.033)
Strong Pro	0.117	-0.103	0.313	0.139	-0.078	0.328
	(0.029)	(0.042)	(0.038)	(0.030)	(0.045)	(0.038)
F (3)	83.36*	50.42*	39.20*	81.96*	46.44*	38.38*
N (Evidence)	22	22	22	16	16	16
N (Respondent)	214	87	100	214	87	100

* $p < 0.1$ (two-tailed). OLS estimates with standard errors in parentheses. Entries are marginal group differences, or the between the probability that the ACA controls cost growth minus the probability that the ACA accelerates cost growth, by type of given evidence. Standard errors are clustered at the respondent level.

In Table 6, I pool the results in Figure 4 by the four evidence types, and provide F-statistics that test the null hypothesis that the differences in conditional probabilities do not vary across the kind of given evidence.[41] Column 1 draws on all participants, and

[41] The estimates are based on OLS modes that regress the dependent variable on (1) experimental condition, (2) three dummy variables indicating evidence type, and (3) the interactions between condition and the dummies. The F-statistics test if the three interaction terms are jointly zero—i.e., the group difference is constant across evidence types.

Columns 2 and 3 subset the data by prior attitudes toward the ACA. As indicated by the large F-values, the null hypothesis is easily rejected regardless of prior attitudes. Again, while supporters and opponents never agree about the conditional probabilities, the magnitudes of the within-subject differences across different types of evidence are very similar (-0.103 + 0.563 = 0.460 for opponents; 0.313 + 0.124 = 0.447 for proponents).

I repeat similar analyses in Columns 4 to 6, but this time using the 8 pairs of evidence that have a stronger/weaker counterpart (the ones with a lower case letter in Figure 4). The results are virtually identical to the previous specification. To maximize the comparability between the strong and weak arguments, only these 8 "pairs" of evidence were used in Experiment 2.

3.5 Experiment 2: The Affordable Care Act

There are two important caveats to Experiment 1. First, participants did not *actually* update their beliefs, unlike a typical experiment that tests the effects of a political argument. Instead, they just indicated how they *would* assess the likelihood of the health economists' prediction being correct, "if and when" a *hypothetical* study presents a piece of information as its finding. They might respond differently when the facts are presented as actual findings from real data. In addition, there is an important distinction to make between a belief about a specific consideration (cost) about the ACA, and overall attitudes toward the law. As conventionally defined, the latter would be the weighted average of multiple beliefs (Fishbein 1963). Even if strong counterevidence can change

people's beliefs, it may ultimately fail to change attitudes, if people search their memories to muster up reasons to draw the desired conclusion based on other beliefs about the law (see Kunda 1990, 483). Experiment 2 addresses these concerns by having participants read an argument coupled with the strong or weak evidence identified in Experiment 1, and answer questions measuring beliefs and attitudes. The key dependent variables are beliefs about the impacts of the ACA's on health cost increases as well as overall attitudes toward the law. The independent variable is exposure to one of four arguments that differ in terms of position (pro versus con), and evidence strength.

Research Design

Experiment 2 was conducted in four waves. In the first wave, pre-treatment covariates were measured, including the baseline values of the dependent variables, and prior knowledge about the ACA.[42] Survey responses were gathered from 2,029 participants recruited via Mechanical Turk in May 2016. Each participant was paid $0.35. Following a week of "washout period," 1,800 of the initial respondents were invited to participate in the main experiment. Those who failed a screener question and non-U.S. citizens were not invited. Among the invited respondents, 1,514 completed the second wave. Initially, participants were paid $0.75 for Wave 2, though I increased the payment amount to $1 and then to $1.5 to maximize the completion rate.

[42] Some covariates were measured in the second wave. This decision was made based on (1) whether answering the questions would plausibly affect how participants respond to to the treatments, or (2) whether the treatments would plausibly affect the answers to the questions.

Participants were randomly assigned to one of five conditions: strong con, weak con, weak pro, strong pro, and placebo. Those assigned to one of the first four conditions received an argument about the ACA's impact on health costs, constructed from the materials pre-tested in Experiment 1 (see Appendix C for the full texts of these arguments). In these cases, the arguments began by stating "health experts have been keeping close taps on how the law is changing America's health care system." In the con conditions, the arguments went on to say "[D]espite some successes, Obamacare [the ACA] is failing to address one of the most serious problems of America's health care system: the rising costs," and then presented the health economists' theoretical explanation that was used in Experiment 1 as well. In the pro conditions, the messages said "the ACA is successfully addressing the rising costs," and then provided an explanation. In all four cases, participants were told "now, there is mounting evidence that that the health economists were right," followed by four pieces of evidence pretested in Experiment 1. In the strong pro condition, for example, participants were provided with the following facts:

1. Confirming health economists' key assumption about expanding the insurance pool, an analysis by the Department of Health and Human Services indicates that the uninsured rate for non-elderly adults has decreased by 43%, over the two years after the reform.

2. According to a report by the Kaiser Family Foundation, the overall premium

for employer-sponsored health insurance rose by 3.8% each year since the reform. By contrast, during the Bush years, premiums increased on average by 8.8 % each year.

3. According to the latest data released by the Centers for Medicaid and Medicare Services, the annual growth in national out-of-pocket spending was 1.3% in 2014. Again this is lower than the annual growth rates under Bush (5.6% on average).

4. Earlier this year, Kaiser Family Foundation released an analysis showing premiums in the Obamacare Marketplaces decreased by 0.7% nationwide, after accounting for tax credits. In comparison, the national individual health insurance premium had increased by 10 to 12% per year before the reform, according to an analysis by an MIT economist.

The arguments then concluded by emphasizing the importance of the "positive [warning] signs." Across these four conditions, the wordings were kept as close as possible.[43] In the placebo condition, participants read a message about the debt crisis in Greece.[44] The five messages were similar in length (between 416 to 462 words).

[43] In each of the four treatment conditions, the law was referred to as "the Affordable Care Act" (and then as "the ACA") for half of participants, and as "Obamacare" for the other half, by random assignments. I found no evidence that this wording choice altered the treatment effects, so I pool them.

[44] In Experiment 3, I added a control condition where participants did not read any

The second wave of Experiment 2 was conducted as follows. First, participants answered general political knowledge questions,[45] followed by a screener question.[46.] The next step was the core part of this experiment. Participants were provided with one of the five arguments to which they were randomly assigned, and asked to read the arguments carefully.[47] Then participants evaluated the strength of the argument and answered the post-treatment measures of attitudes and belief about the law. Subsequently, participants answered questions for potential moderators—e.g., need for cognition, demographic characteristics, etc. Finally, they were asked to recall the contents of the argument.[48]

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message to address the (unlikely) concern that the placebo had impacts on participants' attitudes on American political issues. I found no evidence for this possibility.

[45] General political knowledge questions were asked at the beginning as a "warm up" exercise. I assumed that they are unlikely to have priming effects.

[46] Similar to Experiment 1, participants were instructed to "ignore the question and select the Drudge Report and the NPR website and none of the above as your three answers and none of the above as your two answers," before being asked to identify the news website they visit. 71.1% of participants correctly marked the two options, whereas 22.2% marked only one or two of them—mostly failing to notify the "none of the above" part of the instruction. I dropped the remaining 6.7%, who would most likely add noise, and underweighted those who marked only one or two options correctly by 0.5. Appendix B provides the results that treat low attention respondents in the same was high attention respondents. The findings are virtually identical.

[47] Participants were told that there would be recall questions toward the end of the survey. The time that each participant spent on the message page was recorded. Its 99%-trimmed mean was 143 seconds. The median was 113 seconds. Most participants (93.4 %) spent at least 30 seconds reading the presented article. I did not drop those spent less time from the analyses below, because doing so may compromise the comparability across experimental conditions.

[48] Two recall questions were used. The first one was choosing the factual evidence included provided in the message one read. 78.9% of those in the treatment groups gave correct answers. The second one was choosing the economic term that the argument used to explain the health economists' theory. 47.5% of the treated respondents correctly identified "adverse selection," 7.8% chose one of the incorrect answers, and the rest said they were not sure.

Those who took part in the main experiment were invited to take two follow-up surveys. Wave 3 was launched about 80 days after Wave 2 (between late July and early August) in order to assess the longevity of the treatment effects. Wave 3 was completed by 899 participants. In the wake of the widely-covered story of premium increases that broke in late October, I re-contacted the participants to examine if (1) they received this information, and if (2) they changed their beliefs as a result.[49] According to my classification, this story provides a piece of "strong con" evidence. The final survey was completed by 850 participants.

Measurement

Table 7: Measurement of Beliefs and Attitudes about the ACA

Belief in cost saving effect (Cronbach's alpha = 0.83; scaled 0-1, with 1 indicating the pro-ACA side)	
1.	In terms of health care costs, the reform law is changing the American health care system... (for the better/worse)
2.	The probability that the law slows down growth in health care costs over the next ten years is...(high/low)
3.	The probability that the law accelerates growth in health care costs over the next ten years is... (high/low)
Attitudes toward ACA (Cronbach's alpha = 0.96; scaled to 0-1, with 1 indicating the pro-ACA side)	
1.	Do you support or oppose the health care reform law passed in 2010?
2.	Would you vote to repeal the health care reform law, if you were in Congress today?
3.	The health care reform law is changing the American health care system... (for the better/worse)
4.	The overall impacts of the health care reform law on the American people will be...(good/bad)
5.	The overall impacts of the health care reform law on you and your family will be... (good/bad)

The key dependent variables are (1) belief in the ACA's impact on health costs, and (2) attitudes toward the ACA. Appendix C reports additional analyses focusing on other dependent variables such as the intention to vote Democratic, support for President

[49] The story broke on October 24th, 2016, and survey responses were collected between October 26th and November 3rd (but mostly in the first three days).

Obama, and so on. All dependent variables were coded so that higher values indicate the pro-ACA (and the Democratic) side. Table 7 lists the survey items used to measure beliefs and attitudes. See Appendix C for survey wordings and more information about measurements of other variables.

Results

The (immediate) treatment effects were estimated using the following regression model:

$$Y_i = \beta_0 + \beta_1 SC_i + \beta_2 WC_i + \beta_3 WP_i + \beta_4 SP_i + \varepsilon_i \quad (3.2)$$

where Y_i is the dependent variable, and SC_i, WC_i, WP_i, SP_i are dummy variables marking the four treatment conditions to which participant *i* was assigned—e.g., *SC* indicates the strong con condition—, β_0 is the control group mean, and β_1 to β_4 are the treatment parameters, and ε_i is an error term. To minimize error variance, and thus improve statistical precisions, I estimate the model with a set of pre-treatment covariates that are thought to be highly predictive of the dependent variables, including their lagged values.[50] Reported from here on out are all covariate-adjusted estimates. Participants exhibiting low attention were underweighted.

[50] When estimated without covariates, the point estimates are very similar, though standard errors are larger. For the results without pre-treatment covariates and additional robustness checks see Appendix C.

The expectation under the (restrictive) Bayesian model is $\beta_1 < \beta_2 < 0 < \beta_3 < \beta_4$ across priors. This model will be rejected if either of the skeptics' hypotheses hold. Specifically, one will have strong evidence against the Bayesian model in favor of the uncritical public hypothesis when one finds $\beta_1 = \beta_2 < 0 < \beta_3 = \beta_4$. One will have strong evidence for the motivated reasoning hypothesis if $\beta_1 = \beta_2 = 0$ for proponents (or Democrats), and $\beta_3 = \beta_4 = 0$ for opponents (or Republicans).[51] If there is a backfire effect, one will find $\beta_1 > 0$ and $\beta_2 > 0$ for supporters (or Democrats), and $\beta_3 < 0$ and $\beta_4 < 0$ for opponents (or Republicans).

Table 8 reports Experiment 2's main results (see Panel A). The first three columns demonstrate the treatment effects on belief in the ACA's cost saving effect. Column 1 for all participants, and Columns 2 and 3 for opponents and supporters respectively. The results are remarkably consistent with the Bayesian model. The average effect of the strong con message is a 13.0 percentage point decrease. The weak con message's effect is -9.2 percentage points. The pro-ACA argument coupled with weak evidence increase people's belief in the ACA's cost preserving effect by 8.4 percentage points, and the strong pro evidence raises the effect to 12.6 percentage points. The estimate of β_2 is significantly higher than β_1 ($p < 0.01$); β_3 is significantly higher than β_2 ($p < 0.001$); and β_4 is significantly higher than β_3 ($p < 0.01$). To put the sizes of these effects in context, the gap between the strong con evidence condition and the strong pro evidence condition

[51] I subset the data by prior attitudes toward the ACA in the analyses presented below. Similar analyses subsetting the data by partisan identity is provided in Appendix C. I reach the same general conclusion using this alternative specification.

($\beta_4 - \beta_1 = 0.130 + 0.126 = 0.256$) is comparable to the average difference between Democrats and Republicans at the baseline (0.256).

Table 8: Treatment Effects on ACA Opinions (Experiment 2)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Panel A						
Strong Con (β_1)	-0.130*	-0.089*	-0.156*	-0.082*	-0.030*	-0.109*
	(0.014)	(0.024)	(0.017)	(0.011)	(0.017)	(0.015)
Weak Con (β_2)	-0.092*	-0.061*	-0.105*	-0.038*	-0.029*	-0.041*
	(0.014)	(0.025)	(0.016)	(0.010)	(0.015)	(0.013)
Weak Pro (β_3)	0.084*	0.088*	0.082*	0.035*	0.072*	0.013
	(0.014)	(0.024)	(0.016)	(0.010)	(0.018)	(0.012)
Strong Pro (β_4)	0.126*	0.137*	0.119*	0.060*	0.101*	0.032*
	(0.014)	(0.025)	(0.017)	(0.011)	(0.019)	(0.012)
AIC	-1077	-301	-786	-1711	-586	-1119
Panel B						
Linear Slope	0.275*	0.242*	0.295*	0.143*	0.147*	0.135*
	(0.013)	(0.023)	(0.015)	(0.011)	(0.018)	(0.013)
AIC	-1073	-304	-783	-1715	-584	-1111
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1412	563	829	1412	563	829

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are underweighted. The coefficients on the intercepts and the covariates are not shown. See Appendix C.3 for the full reports.

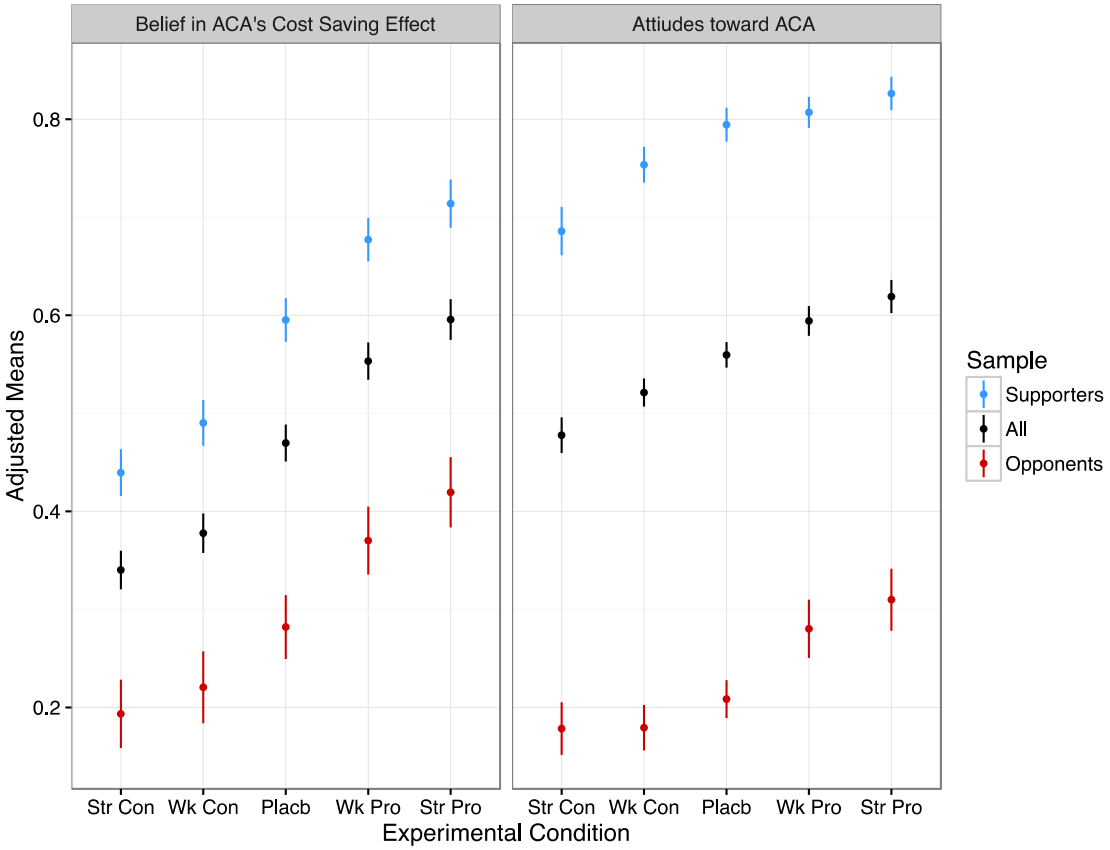
In columns 2 and 3, I demonstrate that the treatment effects are fairly consistent across prior attitudes. While the overall differences across experimental conditions are somewhat larger for proponents, this heterogeneity is small considering the overall effect sizes. Presumably, it reflects a floor effect for opponents, whose baseline belief in ACA's cost saving effect is lower (0.309) than proponents' belief is high (0.584), as measured by

the control group's means. Another noticeable difference is that con arguments produce *larger* changes for proponents, and pro arguments produce *larger* changes for opponents, again probably reflecting ceiling and floor effects. Importantly, to the extent there *is* difference in the treatment effects, it is in the *opposite* direction of what should have happened had people been motivated to eschew counterevidence. Indeed, the heterogeneous treatment effects *reduce* the pre-existing belief gap.[52]

In Columns 4 to 6, I show that the treatments shifted people's attitudes toward the ACA (scaled 0-1, with 1 indicating most favorable attitudes) in a manner consistent with the Bayesian model. In particular, the strong pro argument increases support for the law by 6.0 percentage points on average, doing so by 10.1 points for opponents and by 3.0 percentage points for proponents. This means that the argument decreases the pre-existing difference by 7.1 percentage points. The strong con argument also undercuts support for the law by 8.0 percentage points on average, and by 10.9 points for supporters. The effects on this dependent variable are not as large as the effects on belief about costs. Conceiving an attitude as the summary of various beliefs (Fishbein 1963; see also Zaller 1992), this is not surprising; health costs would be one of many things people consider as they form their opinion about the law.

[52] In Appendix C, I show that even those who exhibited strong attitudes toward the ACA at the baseline survey are influenced in a similar way. They, too, adjusted their beliefs and attitudes in response to counter arguments. I also show in the appendix that the treatment effects are similar for those who supported different presidential candidates in the 2016 election cycle (e.g., Clinton versus Trump).

Figure 5: Adjusted Means across Experimental Conditions



Note: OLS estimates based on Table 8. Vertical lines are 95% confidence intervals. Estimates are adjusted for pretreatment covariates including the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are underweighted. Unadjusted means are similar, though confidence intervals wider.

Figure 5 summarizes the results by presenting the adjusted group means across the experimental conditions. As can be seen at a glance, the treatments shifted people's beliefs and attitudes in a more or less linear fashion. This means that a single coefficient of the "linear slope" may summarize the results more efficiently with greater statistical power, though doing so involves restricting a change from each condition to the next

(e.g., strong con from weak con) to be the same across the spectrum. Panel B of Table 8 provides the estimates of these "slopes."[53] The overall treatment effects are estimated to be between 24.2 to 29.5 percentage points on belief changes, and between 13.5 to 14.7 points on attitude changes.

By and large, the patterns shown above are consistent with what would happen when people have similar ideas about the certainty of evidence—i.e., the likelihoods of observing the "data" under the competing hypotheses about the ACA's effects on health care costs—regardless of their priors. This apparently goes against the general findings of the motivated reasoning literature that people's interpretation of new information is colored by their prior attitudes. As I have argued, the previous findings of "biased assimilation"—i.e., correlations between prior attitudes and argument evaluations—can reflect the remaining disagreements between people starting from different priors, instead of their tendency to discount counterevidence—i.e., biased likelihood functions. The fact that people unfavorably evaluated arguments that challenged their prior attitudes in ostensible perceived argument strength measures does not necessarily mean that people refuse to recognize the "force" of counterevidence.

To make this point more precise, I calculated the likelihood ratios that correspond to the average belief changes summarized in the left panel of Figure 5—i.e., "the force of an argument, namely its general capacity of changing degrees of beliefs" (Hahn and Oaksford 2007, 710). The likelihood ratio of a pro argument is: $LR_{pro} = \frac{P\,(E_{pro}|H)}{P\,(E_{pro}|{\sim}H)} =$

[53] Experimental conditions were coded as a continuous variable running from 0 (strong con) to 0.25 (weak con), 0.5 (placebo), 0.75 (weak pro), and to 1 (strong pro).

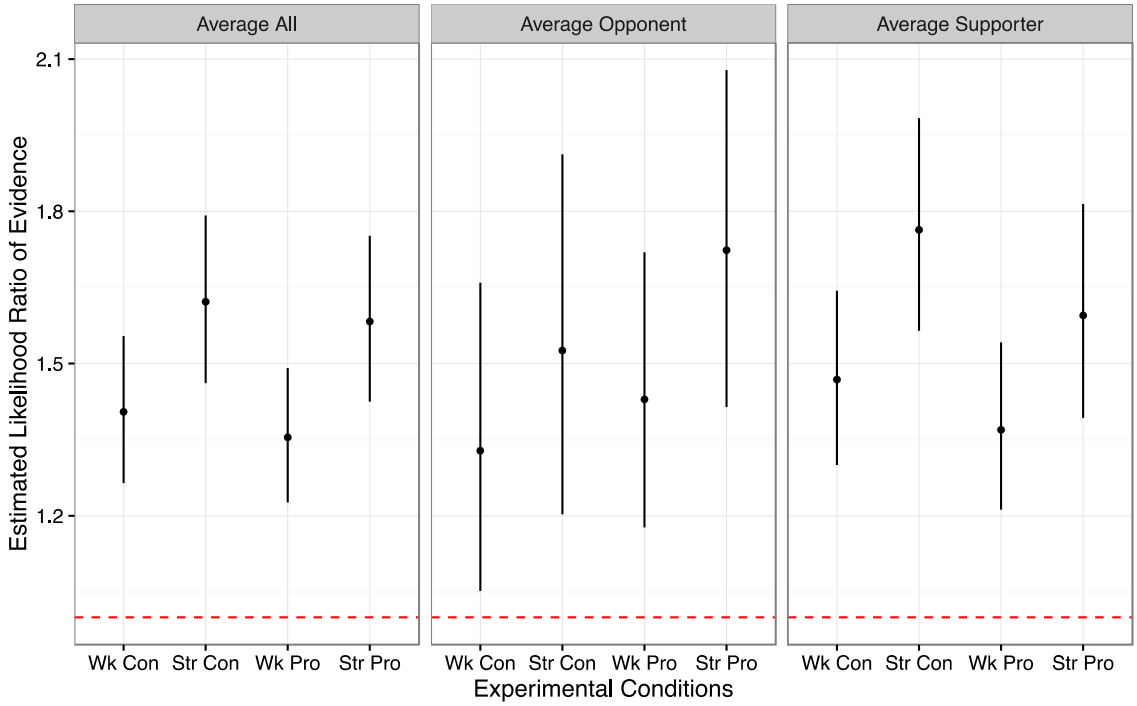
$\frac{P\,(H|E_{pro})}{P\,(\sim H|E_{pro})} \cdot \frac{P\,(\sim H)}{P\,(H)}$, where *H* is the hypothesis that the ACA slows down cost growth, and ~*H* is the contrary hypothesis. The likelihood ratio of a con argument is: $LR_{con} = \frac{P\,(E_{con}|\sim H)}{P\,(E_{con}|H)} = \frac{P\,(\sim H|E_{con})}{P\,(H|E_{con})} \cdot \frac{P\,(H)}{P\,(\sim H)}$. Defining the average respondent's prior as the control group's mean belief in *H* and her posterior given each argument as the treatment groups' mean beliefs in *H*, the estimated value of the average respondent's prior belief in *H* would be $\widehat{\beta_0} = 0.47$ and posterior belief given the strong pro argument would be $\widehat{\beta_0 + \beta_4} = 0.60$.[54] The corresponding likelihood ratio is $LR_{pro} = \frac{0.60}{(1-0.60)} \cdot \frac{(1-0.47)}{0.47} \approx 1.66$; that is, a change from 0.47 and 0.60 is what would happen if one interprets that the events described in the strong pro argument are 1.66 times more likely if *H* was true, than if it was false (under Bayes' theorem). This framework was used to calculate the likelihood ratios of the four types of argument, as judged in the eyes of the average opponent and supporter of the law. Figure 6 provides the point estimates and 95% confidence intervals, obtained via bootstrapping.

The figure shows that the likelihood ratios—i.e., "the force of an argument" (see Hahn and Oaksford 2007, 710)—differ significantly by argument type, but little by prior attitudes. The results suggest that people assigned higher diagnostic values to the arguments with stronger evidence (1.66 to 1.88) than weaker evidence (1.40 to 1.50). The figure also suggests that the likelihood ratios assigned by the average opponent and supporter are more or less the same across the four types of argument (all p >.10).[55] If

[54] I take these values as probability statements. That is, a respondent who scored 0.60 in the belief scale is taken to have given a 60% chance that the ACA controls costs.
[55] P-values are calculated via bootstrapping.

anything, the likelihood ratios associated with uncongenial arguments are higher than congenial arguments.

Figure 6: Likelihood Ratios Corresponding to the Average Treatment Effects



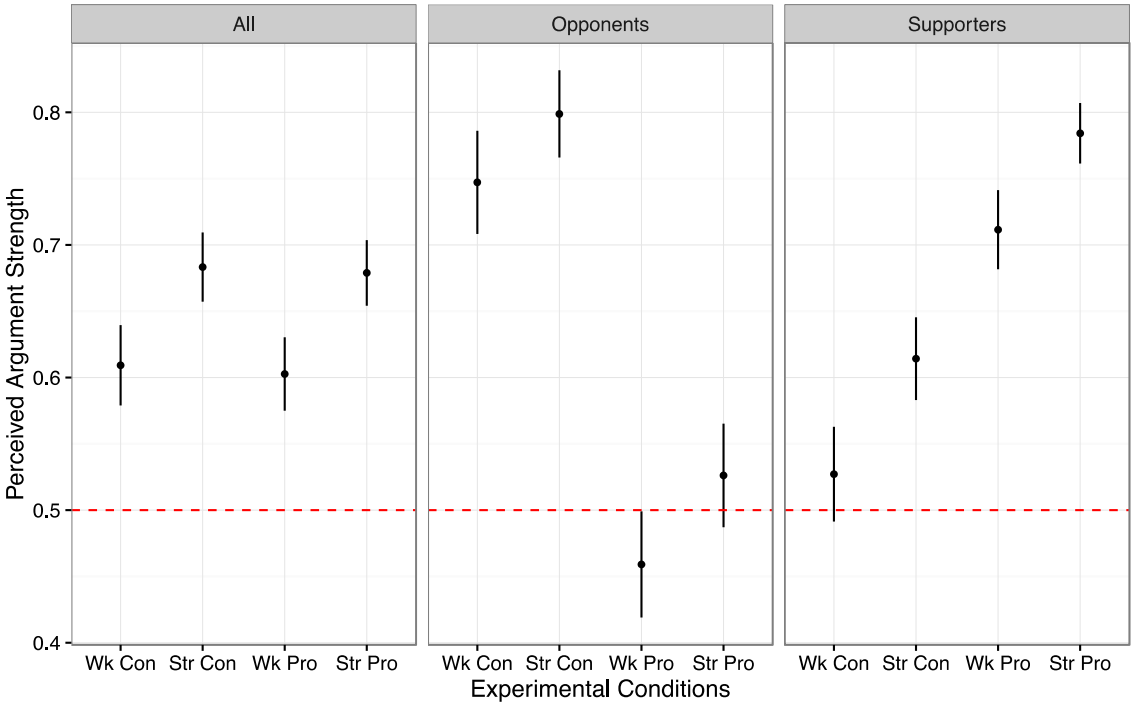
Note. The estimates are calculated based on the group means shown in the left panel of Figure 5. Vertical lines are 95% confidence intervals.

In contrast, people's *stated* evaluations of arguments are correlated positively and substantially with prior attitudes for both proponents and opponents, as shown in Figure 7.[56] The discrepancy between Figures 6 and 7 underscores the need to differentiate between people's responses to the conventional measures of perceived argument strength

[56] See Appendix C for more information about the measurement of perceived argument strength, as well as the regression models, from which the figure is drawn.

and their actual interpretation of how probative the given evidence is—i.e., likelihood functions. As long as people have similar likelihood functions, no matter how they ostensibly evaluate the arguments, Bayes' theorem predicts people's beliefs will eventually converge to the available evidence as the clarity and amount of (received) information increases (see Tetlock 2005, 310 for an example of a formal demonstration of the latter point).

Figure 7: Perceived Argument Strength by Argument Type and Prior Attitudes



Note: Based on regression estimates reported in Appendix C. Dots are marginal means, adjusted for pretreatment covariates including lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Vertical lines are 95% confidence intervals.

I have looked closely at the potential difference in treatment effects by prior attitudes (and by party ID in Appendix C). Now I examine treatment effect heterogeneity by other respondent characteristics. These factors include several direct and indirect indicators of the importance of health care (costs), political knowledge, need for cognition, and demographics. This exercise serves two purposes. From an empirical standpoint, it allows me to examine the generalizability of the findings of the current experiment that draws on a convenience sample from Mechanical Turk. Compared to the general U.S population, MTurk respondents are known to be more Democratic and liberal, more politically informed, high on need for cognition, less wealthy, younger, more educated and so on (Berinsky et al. 2012). In the presence of *substantial* treatment effect heterogeneity along these factors, one should be wary of the possibility that the impacts of the arguments are overstated (or understated for that matter) here, due to the peculiar sample attributes.

Exploring moderating effects can also have theoretical implications, because the hypotheses discussed above can be extended to specify the boundary conditions under which new information does (and does not) make an impact. For example, issues deemed personally important can trigger defensive reactions, thereby attenuating the treatment effects, according to the motivated reasoning hypothesis (see Leeper 2014). In contrast, under the Bayesian framework, incoming information will be weighted more heavily when it is personally relevant than when it is not, even if it is not consistent with one's prior beliefs.[57] Since I have been examining the effects of four types of arguments on two

[57] This is because people are expected to construct less discriminant likelihood functions

outcome variables, introducing 14 moderating variables means that there are $4 \times 2 \times 14 = 112$ interaction terms to estimate and interpret. To reduce the complexity, I focus on the differences in the "linear slopes" instead. I estimate 28 regression models that allow the slopes to vary by each individual trait, by interacting it with experimental conditions (operationalized as a continuous variable), and pre-treat covariates. Table 9 summarizes the results.

As shown in Column 1, the treatment effects on the belief in ACA's effect on cost growth are highly consistent across a variety of individual factors. Some "significant" differences do appear, but they are fairly small in substantive terms, and mostly fail to survive the Holm correction for multiple (14) comparisons. Given this consistency, it appears highly implausible that the treatment effects on beliefs would completely disappear or even flip signs when examined with a different sample that is more representative of the U.S. population.

Even if an argument creates similar changes in the beliefs about an objective attribute of a policy—whether it raises health care costs—its impact on overall attitudes can vary across individual differences in evaluative criteria. In the second column, I report more noticeable interaction effects on attitudes toward the ACA. Six out of the 14 variables moderate the treatment effects at the $p < 0.1$ level, three of which remain statistically significant even after correcting for multiple comparisons. Interestingly, I

(i.e., likelihood ratio close to 1) when the presented information is not relevant to their concerns. For example, someone who does not care at all health care costs but cares deeply about something else (e.g., the size of the federal government) is less likely to be influenced by the arguments, even if the evidence is strong, compared to someone for whom the rising costs are of greater concern.

Table 9: Heterogeneous Treatment Effects by 14 Individual Traits (Experiment 2)

		(1)		(2)		
	Moderating Variables	Beliefs		Attitudes		N
1	ACA Opponent	0.242*	(0.023)	0.147*	(0.018)	563
	ACA Supporter	0.295*	(0.015)	0.135*	(0.014)	892
	Difference in Treatment Effect	0.053*	(0.028)	-0.012	(0.023)	
2	Republican	0.216*	(0.025)	0.121*	(0.020)	396
	Democrat	0.300*	(0.016)	0.148*	(0.014)	842
	Difference in Treatment Effect	0.084**	(0.030)	0.027	(0.025)	
3	Cost Importance < 1	0.280*	(0.013)	0.136*	(0.012)	1120
	Cost Importance = 1 (Highest)	0.273*	(0.036)	0.176*	(0.027)	292
	Difference in Treatment Effect	-0.007	(0.038)	0.040	(0.030)	
4	Healthcare Importance Low-mid	0.275*	(0.018)	0.136*	(0.015)	769
	Healthcare Importance High	0.279*	(0.019)	0.156*	(0.017)	643
	Difference in Treatment Effect	0.004	(0.026)	0.020	(0.022)	
5	No Problems with Medical Bills	0.273*	(0.017)	0.116*	(0.015)	795
	Had Problems with Medical Bills	0.272*	(0.020)	0.177*	(0.016)	617
	Difference in Treatment Effect	-0.001	(0.026)	0.061**	(0.022)	
6	Insured	0.289*	(0.014)	0.143*	(0.012)	1187
	Uninsured	0.180*	(0.037)	0.149*	(0.033)	207
	Difference in Treatment Effect	-0.109*	(0.040)	0.007	(0.035)	
7	Middle or Upper Class	0.267*	(0.017)	0.117*	(0.014)	886
	Working or Poor Class	0.291*	(0.020)	0.182*	(0.017)	621
	Difference in Treatment Effect	0.024	(0.026)	0.065**	(0.022)	
8	Income ≥ $50,000	0.302*	(0.018)	0.119*	(0.015)	694
	Income < $50,000	0.254*	(0.018)	0.169*	(0.016)	718
	Difference in Treatment Effect	-0.048*	(0.026)	0.050*	(0.022)	
9	High Political Knowledge	0.274*	(0.019)	0.105*	(0.015)	664
	Low Political Knowledge	0.275*	(0.018)	0.175*	(0.016)	748
	Difference in Treatment Effect	0.002	(0.026)	0.070**	(0.022)	
10	High Need for Cognition	0.280*	(0.019)	0.121*	(0.015)	682
	Low Need for Cognition	0.265*	(0.017)	0.163*	(0.016)	730
	Difference in Treatment Effect	-0.015	(0.026)	0.042*	(0.022)	
11	4 Year College or higher	0.293*	(0.017)	0.120*	(0.014)	751
	2 Year College or lower	0.255*	(0.020)	0.166*	(0.017)	661
	Difference in Treatment Effect	-0.038	(0.026)	0.046*	(0.022)	
12	Age ≥ 35	0.267*	(0.019)	0.133*	(0.016)	604
	Age < 35	0.279*	(0.018)	0.148*	(0.015)	808
	Difference in Treatment Effect	0.012	(0.026)	0.015	(0.022)	
13	Male	0.278*	(0.018)	0.135*	(0.015)	696
	Female	0.274*	(0.019)	0.153*	(0.015)	716
	Difference in Treatment Effect	-0.005	(0.026)	0.018	(0.022)	
14	White	0.271*	(0.015)	0.141*	(0.013)	1066
	Non-White	0.287*	(0.025)	0.153*	(0.022)	346
	Difference in Treatment Effect	0.016	(0.029)	0.013	(0.026)	
	Covariates	Yes		Yes		
	Moderator ×Covariates	Yes		Yes		

* p < 0.1 (No correction). ** p <0.1 (Holm corrections for 14 comparisons; reported for the interaction terms only). OLS estimates with robust standard errors in parentheses. The set of covariates is the same as Table 8.

find that the effects are more pronounced for those who have had problems with medical bills (by 6.1 percentage points), and those who identify themselves as the working/poor class (by 6.5 points)—for whom, presumably, the rising costs would be of greater importance. Personal relevance appears to lead people to give more relative weights to incoming information than prior—a finding that is compatible with the Bayesian model, but not with the motivated reasoning theory.[58] Nevertheless, even these differences are not big enough to make the treatment effects disappear for certain individuals. Across all subsets of the sample, the treatment effects on attitudes are in the double digits, again demonstrating a great deal of treatment effect homogeneity.

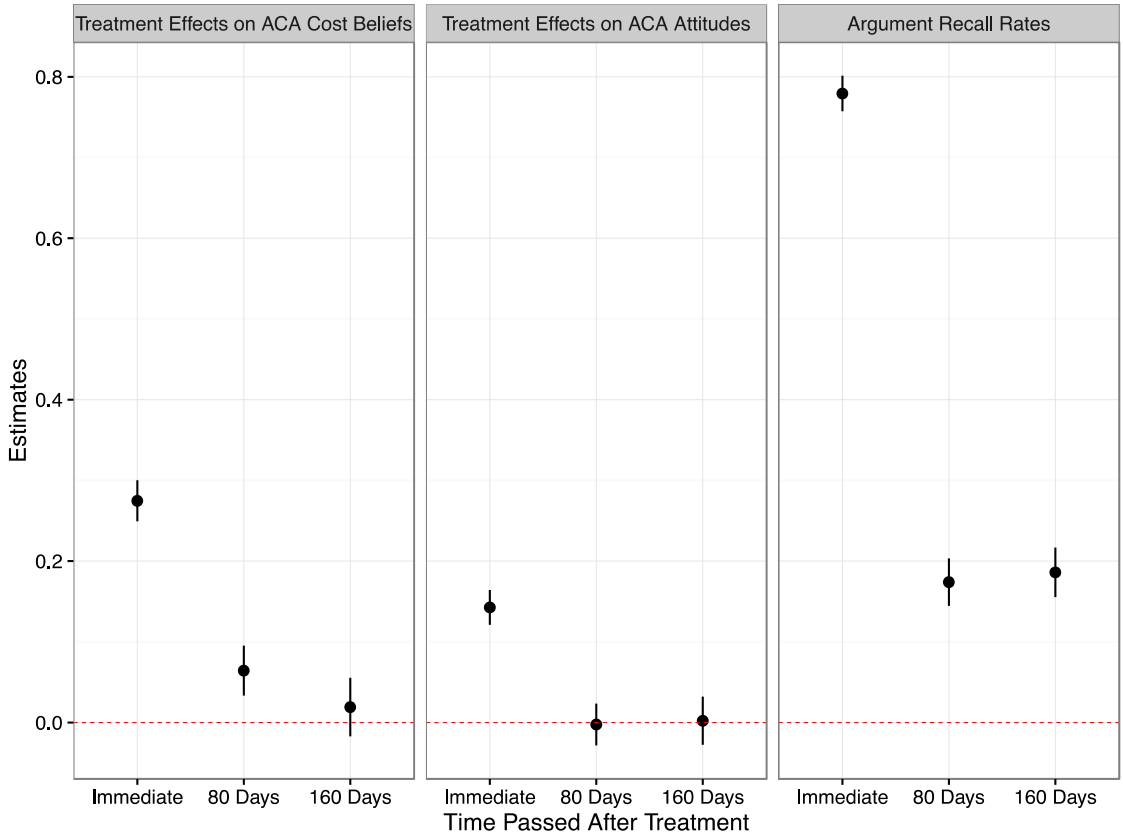
Longevity of Treatment Effects

Having documented immediate treatment effects, I now examine whether they persist for an extended period of time. Figure 8 reports the overall treatment effects (i.e., linear slopes) as measured about 80 days Wave 3), and 160 days after the main experiment (Wave 4). The effects did remain observable even a couple of months later, but decayed in the long run. On average, the overall effects on beliefs about health costs had diminished to about one-fourth of the original estimates (0.064/0.275) by the third wave (though remaining statistically significant at $p < 0.001$), and to less than one-tenth by the final survey ($p = 0.301$). As for attitudes toward the law, the treatment effects had disappeared entirely by Wave 3 and remained so until Wave 4. Perhaps the eventual

[58] As is often the case, a strong causal interpretation of the interaction effects is not warranted, because they may reflect spurious correlations.

decay in the treatment effects is not surprising because, as shown in third panel of the figure, most respondents in the treatment groups were (understandably) not able to recall whether the message argued that Obamacare will increase/decrease healthcare costs in the follow-ups.

Figure 8: Long-Run Effects on ACA Opinions (Experiment 2)



Note: Based on regression estimates reported in Table A21 in Appendix C. Dots are overall treatment effects, adjusted for pretreatment covariates including baseline values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Vertical lines are 95% confidence intervals.

3.6 Observational Replication of the ACA Experiment

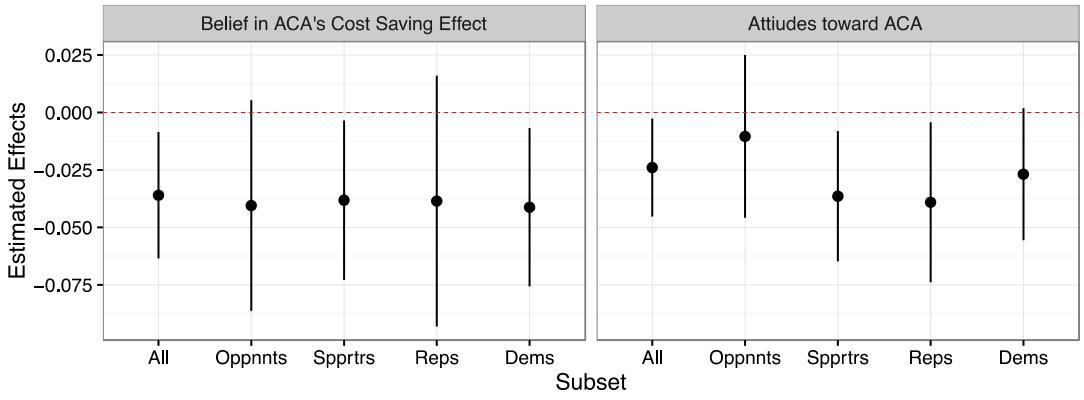
The main experiment is designed to measure the effects of new information in a closely controlled setting. While it allows for a strong causal inference, it may be questionable as to how far the findings can be generalized to how people update their political opinions in light of new information in other contexts (see Shadish et al. 2002, Chap. 3). First, people may have paid unusually high attention to the messages because they were informed that recall questions would follow. Second, the estimates reported above may be specific to the particular characteristics of the experimental stimuli; for example, the effects might have been smaller, had the messages been less civil and/or more partisan. Third, participants may have guessed the purpose of the experiment, and tried to give responses that would confirm the hypothesis.

Wave 4 of the ACA study, which was fielded two days after news broke that ACA premiums are increasing by 22% in 2017, provides an excellent opportunity to addresses these concerns and strengthen the external validity of this research by shedding light on how people respond to new information received in a non-artificial "real world" setting. To do so, I asked participants if they heard or seen any news about health insurance rates in 2017 over the past few days,[59] and compared those who did and did not receive the information. Admittedly, this particular analysis is not experimental, and thus requires an identifying assumption. To identify the effect of the new information, I

[59] 56.5% responded yes, and most of them (98.1%) correctly indicated that the news reports suggest premiums will increase next year.

regressed the dummy variable indicating whether one received the news, the lagged dependent variables measured in the prior three waves, as well as other political variables highly predictive of ACA beliefs and attitudes (e.g., PID).[60] The identifying assumption was that those who had the same opinions at prior waves would have had the same opinions in the absence of exposure to the premium increase story (Angrist and Pischke 2009, 244). Consistent with this assumption, the exposed and unexposed groups did *not* differ significantly in their attitudes and beliefs measured in Wave 3, conditional on Wave 1 and Wave 2 covariates. Likewise, their Wave 2 opinions were statistically indistinguishable, conditional on Wave 1 covariates.

Figure 9: Effects of Exposure to the Coverage of 2017 ACA Premium Increases



Note. Based on regression estimates reported in Table A22 in Appendix C. Dots are the effects of exposure to the premium increase story. Vertical lines are 95% confidence intervals. "Oppnnts" stands for "Opponents." "Spprtrs" stands for "Supporters." All estimates are adjusted for pre-treatment covariates including lagged dependent variables.

[60] Appendix B reports similar findings using a specification that controls only for lagged dependent variable at Wave 3. It also reports consistent negative effects when the respondents are subset by party ID instead of prior ACA attitudes.

As shown in Figure 9, receiving the news appears to have lowered people's belief in ACA's cost saving effect by 3.6 percentage points, and overall support for the law by 2.4 points on average. Importantly, the results show that supporters of the law (and Democrats) moved in the negative direction upon exposure to the unwelcoming information, instead of ignoring it outright or backlashing. Again, this is consistent with the findings of the main experiment, and not with the motivated reasoning hypothesis.

3.7 Experiment 3: The Economy

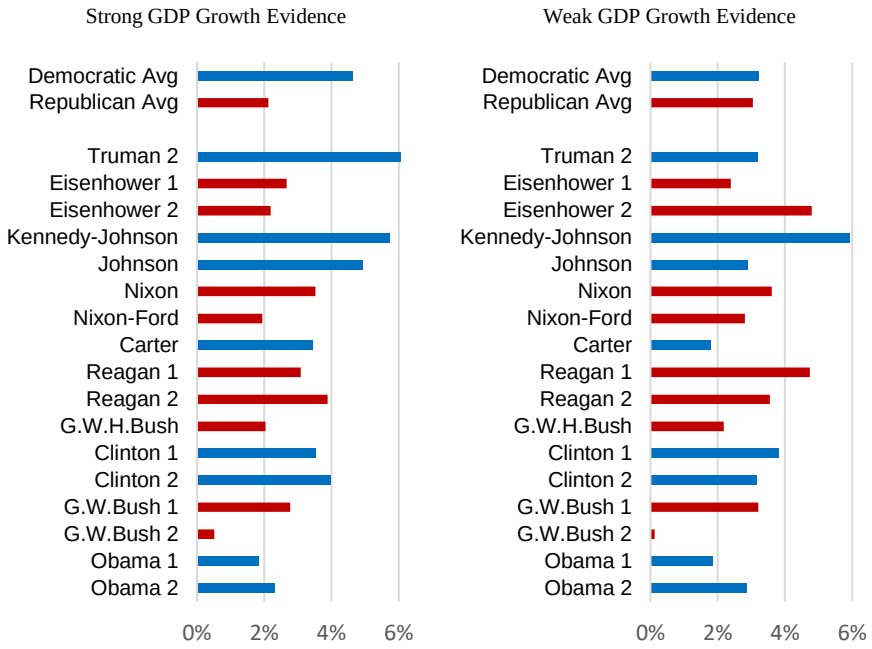
While the Affordable Care Act is obviously one of the most polarized political issues of the day, one may point out that the law is only two years old. Few people know about the provisions of the law in detail, and fewer still have a comprehensive knowledge of the tangible changes that the law has introduced to the American health care system, which might have attenuated the effects of motivated bias (Taber and Lodge 2006). Experiment 3 replicates the main results of Experiment 2, focusing on another contentious, but much older, issue in American politics—which party is strongest at improving the nation's economic performance.

Research Design

Unlike Experiment 2, all pre-treatment covariates were measured within the same wave as the treatment. Twelve hundred (1200) respondents were recruited via Mechanical Turk and completed the survey in April 2016. Each participant was paid

$1.25. Participants were randomly assigned to one of six conditions: strong income evidence, weak income evidence, strong GDP evidence, weak GDP evidence, placebo, and control.

Figure 10: Charts in the Strong (Left) vs. Weak (Right) Conditions



Note: Modified replications of Blinder and Watson (2016).

The evidence presented to the first experimental group (strong income growth evidence) is a slightly modified replication of Bartels' analysis (2008), which shows that incomes grew generally faster under Democratic Presidents than Republican ones, especially for the working class.[61] To create its weaker counterpart (the second treatment

[61] I also acknowledge that parts of the arguments used in the experiment are taken directly from Bartels' (2012) article, published on Salon.com.

group), I unfaithfully replicated Bartels' (2008) findings, searching for a specification that makes the partisan performance gaps appear much smaller, though not completely absent. I took the same approach for the third and fourth treatment groups (strong and weak GDP growth evidence groups), based on a study by Blinder and Watson (2016), which shows that GDP grew much faster under Democratic Presidents than Republican ones. Each argument contained a chart that summarizes mean differences. Figure 10 provides the examples from the third (strong GDP evidence) and fourth (weak GDP evidence) conditions. Appendix C provides more information about these "replications."

Those assigned to one of the first four conditions then received an argument about the historic differences in economic performance under Democratic and Republican Presidents since the 1950s. The first two focused on income growth, and the other two focused on GDP growth. The arguments all claimed that Democratic Presidents historically handled the economy better than Republican Presidents.[62] All facts presented in these conditions are generated based on my calculations, using the official data sources cited in the messages; the U.S. Census Bureau in the first two and the National Bureau of Economic Research in the other two.

By analyzing the income data and GDP data firsthand, I was able to calculate the p-values, and incorporate that information in manipulating the uncertainty of the evidence. For example, those assigned to the strong income evidence condition were told "a regression analysis (a standard statistical technique) indicates that there is about a 1 in 1000 chance that a difference like this would pop up, simply because Democrats got

[62] In other words, there was no pro-Republican argument in this experiment.

lucky, when there's nothing inherently better about their policies." Those in the corresponding weak condition were told "there is less than a 3 in 7 chance." In Bayesian terms, these quantities represent the probabilities of observing the data under the null hypothesis, or *P (E|~H)*, which comprise the denominators of the likelihood ratios. All of the four arguments concluded by suggesting that past performance provides important information about the future as well. The placebo group read a message about debt crisis in Greece, which also included a chart. The control group did not read any argument. The full texts can be found in Appendix C.

Experiment 3 was conducted as follows. The experiment began with questions measuring various pre-treatment covariates, including prior retrospective evaluations of the two major parties, followed by a screener question. Then, those assigned to the treatment and the placebo conditions were provided with one of the five messages. After reading the messages, these participants were asked to evaluate the argument.[63] Those in the control group skipped these steps. Then all participants answered questions measuring the dependent variable—their retrospective and prospective economic evaluations of the major parties. Finally, they were asked to recall the contents of the argument.[64]

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[63] The time that each participant spent on the message page was recorded. Its 99%-trimmed mean was 145 seconds. The median was 113 seconds. Most participants (89.7 %) spent at least 30 seconds reading the presented article. I did not drop those spent less time from the analyses below, because doing so might compromise the comparability across experimental conditions.

[64] Two recall questions were used. The first one asked respondents to choose the main claim of the message. 78.7% of those in the treatment groups gave correct answers. The second one involved choosing the chart they saw in the message. 80.8% of the treated respondents correctly identified the chart included in each argument.

Measurement

Table 10: Measurement of Retrospective and Prospective Economic Evaluations

Retrospective Beliefs (Cronbach's alpha = 0.88; scaled 0-1, with 1 indicating the pro-Democratic side)	
1.	Which party has done a better job of handling the national economy
2.	People like you have been better off under…
3.	Which party has done a better job of handling income inequality
Prospective Beliefs (Cronbach's alpha = 0.93; scaled to 0-1, with 1 indicating the pro-Democratic side)	
1.	Which party will do a better job of handling the national economy
2.	Expect the national economy to get better, if a Democratic candidate wins
3.	Expect the national economy to get better, if a Republican candidate wins
4.	Between Democratic and Republican economic policies, which will generate faster economic growth
5.	People like you will be better off financially under
6.	Expect the total income of your household will go up or down, if a Democratic candidate wins
7.	Expect the total income of your household will go up or down, if a Republican candidate wins
8.	Between Democratic and Republican economic policies, which will make the your income grow faster
9.	Which party will do a better job of handling income inequality
10.	Expect income distribution to become more equal, or less equal than now, if a Democratic candidate wins
11.	Expect income distribution to become more equal, or less equal than now, if a Republican candidate wins
12.	Between Democratic and Republican economic policies, which will reduce income inequality more

The key dependent variables in Experiment 3 are retrospective and prospective evaluations of the economic performance of the two major parties.[65] Table 10 lists the 15 survey items that were used to measure economic evaluations along with reliability statistics (for additional information about the measurements, see Appendix D).

Results

To simplify the analyses and presentation of the findings, I collapse the six conditions into three: (1) strong income/GDP evidence, (2) weak income/GDP evidence,

[65] See Appendix D for additional analyses examining the "carryover" treatment effects on vote choice and so on.

and (3) control/placebo.[66] As with Experiment 2, I also estimate the "linear slope" of treatment effects by operationalizing the experiment conditions as a single continuous variable representing a scale of evidence strength (0 = control/placebo, 0.5 = weak; 1=strong). In short, the treatment effects are estimated using the following regression equations:

$Y_i = \beta_0 + \beta_1 Weak_i + \beta_2 Strong_i + \varepsilon_i$ (3.3) and

$Y_i = \gamma_0 + \gamma_1 Evidence_i + \epsilon_i$ (3.4).

Table 11 reports Experiment 3's main results. Notably, the effects appear to be much more modest than the belief changes observed in Experiment 2.[67] But the central expectations of the restrictive Bayesian model generally hold up. As shown in Columns 1 and 4, the arguments with strong evidence had significant, though incremental, effects on both retrospective (5.5 percentage points) and prospective beliefs (3.4 percentage points)

[66] In Appendix D, I report additional analyses estimating the effects of each of the four treatment conditions. Appendix D also provides results based on alternative operationalization of the dependent variables, by dividing them into three sub-elements: sociotropic evaluations; pocketbook evaluations; income inequality evaluations. Although it was not my primary concern, I examined whether the GDP arguments had more pronounced effects on socio-tropic evaluations; and whether the income arguments on evaluations of the handling of income inequality in the appendix. This indeed was the case.

[67] There are several possibilities as to why this was the case. The reason may be something substantive—people had more certain prior beliefs about the economy; or methodological—responses to pre-treatment questionnaires had stronger impacts because they were measured within the same wave; or something artificial—the arguments were not as well written as Experiment 2.

for the participants as a whole. Meanwhile, the effects of weak evidence were about half the size of the strong one (3.7 and 1.7 percentage points). The estimates of β_2 are significantly higher than β_1 for both retrospective (p = 0.07) and prospective evaluations (p = 0.02). Columns 2 and 3 (and Columns 5 and 6) compare the treatment effects for Republicans and Democrats. I do not find that Republicans ignored the uncongenial arguments—in which case both β_1 and β_2 should have been zero—nor do I find that they backlash—in which case both β_1 and β_2 should have taken on negative values. If anything, the treatments had larger impacts on Republicans, though the difference between strong versus weak arguments were pronounced only for Democrats.

Table 11: Treatment Effects on Economic Evaluations (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
Weak	0.037*	0.070*	0.017	0.017*	0.040*	0.007
	(0.010)	(0.021)	(0.012)	(0.007)	(0.014)	(0.009)
Strong	0.055*	0.074*	0.037*	0.034*	0.033*	0.031*
	(0.009)	(0.018)	(0.011)	(0.007)	(0.013)	(0.009)
AIC	-1449	-346	-919	-2065	-569	-1252
Panel B						
Linear Slope	0.055*	0.077*	0.037*	0.034*	0.035*	0.031*
	(0.009)	(0.019)	(0.011)	(0.007)	(0.013)	(0.009)
AIC	-1450	-344	-921	-2067	-567	-1253
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * p < 0.1 (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. The coefficients on the intercepts and covariates are not shown. See Appendix D for the full reports.

Table 12: Heterogeneous Treatment Effects by 11 Individual Traits (Experiment 3)

		(1)		(2)		
Model	Moderating Variables	Retrospective		Prospective		N
1	Republican	0.078*	(0.018)	0.037*	(0.013)	319
	Democrat	0.038*	(0.011)	0.032*	(0.009)	654
	Difference in Treatment Effect	-0.040*	(0.021)	-0.005	(0.016)	
2	Economy Importance Low	0.051*	(0.014)	0.031*	(0.012)	476
	Economy Importance High	0.058*	(0.012)	0.036*	(0.009)	624
	Difference in Treatment Effect	0.007	(0.019)	0.005	(0.015)	
3	Equality Importance Low	0.057*	(0.011)	0.031*	(0.009)	646
	Equality Importance High	0.052*	(0.014)	0.039*	(0.011)	458
	Difference in Treatment Effect	-0.005	(0.018)	0.008	(0.015)	
4	Middle or Upper Class	0.045*	(0.012)	0.024*	(0.009)	465
	Working or Poor Class	0.066*	(0.014)	0.047*	(0.012)	625
	Difference in Treatment Effect	0.021	(0.018)	0.023	(0.015)	
5	Income ≥ $50,000	0.045*	(0.013)	0.030*	(0.009)	562
	Income < $50,000	0.062*	(0.013)	0.037*	(0.010)	538
	Difference in Treatment Effect	0.017	(0.018)	0.007	(0.014)	
6	High Knowledge	0.028*	(0.011)	0.023*	(0.009)	540
	Low Knowledge	0.084*	(0.014)	0.048*	(0.011)	560
	Difference in Treatment Effect	0.056**	(0.018)	0.025*	(0.014)	
7	High Need for Cognition	0.039*	(0.012)	0.036*	(0.010)	540
	Low Need for Cognition	0.075*	(0.014)	0.036*	(0.011)	560
	Difference in Treatment Effect	0.038*	(0.018)	0.000	(0.015)	
8	4 Year College or higher	0.037*	(0.012)	0.025*	(0.009)	517
	2 Year College or lower	0.075*	(0.014)	0.042*	(0.011)	583
	Difference in Treatment Effect	0.038*	(0.018)	0.017	(0.014)	
9	Age ≥ 35	0.066*	(0.012)	0.050*	(0.010)	604
	Age < 35	0.047*	(0.013)	0.026*	(0.010)	496
	Difference in Treatment Effect	-0.019	(0.018)	-0.024*	(0.014)	
10	Male	0.061*	(0.013)	0.046*	(0.010)	542
	Female	0.048*	(0.013)	0.023*	(0.010)	558
	Difference in Treatment Effect	-0.014	(0.018)	-0.023	(0.014)	
11	White	0.061*	(0.010)	0.033*	(0.008)	872
	Non-White	0.034*	(0.018)	0.039*	(0.014)	228
	Difference in Treatment Effect	-0.027	(0.021)	0.006	(0.016)	
	Covariates	Yes		Yes		
	Moderator ×Covariates	Yes		Yes		

* p < 0.1 (No correction). ** p <0.1 (Holm corrections for 14 comparisons; reported for the interaction terms only). OLS estimates with robust standard errors in parentheses. The set of covariates is the same as Table 11.

In Table 12, I explore whether the treatment effects interact with individual characteristics. The treatment effects are fairly consistent across a variety of individual factors. All but one interaction term fails to remain statistically significant, after correcting for multiple comparisons. The treatment effects remain statistically significant for all subsets of the sample. Overall, the key findings of Experiment 2 are replicated fairly well.

Table 13: Long-run Effects on Economic Evaluations (Experiment 3)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Linear Slope	0.028*	0.033	0.023*	0.012	0.022	0.010
	(0.011)	(0.022)	(0.014)	(0.009)	(0.018)	(0.012)
AIC	-1079	-265	-669	-1386	-361	-827
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	837	251	492	837	251	492

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses.

Finally, Table 13 measure the treatment effects 1 or 2 weeks later. The estimated effects on retrospective evaluations retained about 50% of immediate effects ($p < 0.05$), whereas the effects on prospective evaluations were diminished to about 35% of the original size ($p = 0.170$).

3.8 Summary

Despite widespread skepticism, it remains elusive as to whether people can critically evaluate political information and revise opinions, because the benchmark itself

is often a moving target. In this chapter, I present three experiments that strongly support a Bayesian model of information processing. Across the experiments, people updated their beliefs and attitudes in light of the presented arguments. They did not mindlessly accept whatever arguments they encounter, nor did they reject uncongenial arguments outright. Instead, people account for the uncertainty of evidence as they form their posterior opinions, regardless of the congeniality of the incoming information. To answer the title question of this chapter, the quality of evidence matters.

An encouraging implication of these findings is that people will eventually be able to know if a policy is in their interests, *if* they are exposed to a stream of information. On a related note, and contrary to prior research rooted in the motivated reasoning hypothesis (e.g., Nyhan and Reifler 2010), it also suggests that people will move away from political misbeliefs—defined as those inconsistent with the best available evidence—*if* sufficiently informed with facts where one side produces clearly better evidence than the other. A big caveat to this counterfactual prediction, however, is that most people do not consume much political information (Delli Carpini and Keeter 1996), and those who do follow politics more regularly tend to be more aware of arguments that confirm their partisan viewpoints, as demonstrated in Chapter 2 (see also Iyengar and Hahn 2009; Jerit and Barabas 2012; Taber and Lodge 2006).

The broad implications of this dissertation should, therefore, reflect and integrate the findings of both Chapters 2 and 3—a task set out for the following chapter.

Chapter 4 General Discussion

In American politics, controversies surrounding policy proposals rarely come down to a simple black and white choice. Almost every policy option entails trade-offs, with legitimate rationales for supporting or opposing them. In this regard, democratic decision making necessitates taking potential advantages *and* disadvantages into consideration as one forms his or her opinion. Obviously, those who do not possess *any* knowledge about politics are not likely to make political decisions in such a fashion. They may not have any meaningful opinions at all (Converse 1964). A substantial body of theory and research has argued that "informed citizens are demonstrably better citizens, as judged by the standards of democratic theory and practice underpinning the American system" (Delli Carpini and Keeter 1996, 272). Such citizens, it was found, are more engaged, better able to translate their interests into opinions, more likely to support democratic norms, and more likely to have stable opinions.

However, more recent research has counterbalanced this optimism, suggesting that politically knowledgeable citizens are more likely to develop systematically biased empirical beliefs; to align their opinions with their previously held values (Wells Reedy Gastil and Lee 2009); to respond to partisan polarization among elites in ways consistent with their own partisanship (Abramowitz 2010; Claassen and Highton 2009); and to engage in even more biased information processing and be more likely to polarize as a result (Taber and Lodge 2006). All of these tendencies seem to suggest that seemingly well informed citizens fall short by the standard of having "considered opinions." The

first half of the current research (Chapter 2) represents a long overdue attempt resolve this discrepancy in the literature with a more nuanced conceptualization and measurement of political knowledge.

But even assuming citizens are provided with diverse information, the question remains as to whether they are capable of effectively using it, and thereby making competent decisions. Dishearteningly, the bulk of empirical research has called this assumption into question (see Achen and Bartels 2016, Chap. 2 for a recent review of this literature). To the extent that citizens encounter political messages, they tend to mechanically and even blindly rely on contextual cues and adopt elite positions instead of thinking critically (Broockman and Butler 2017.; Cohen 2003; Lenz 2012; Rahn 1993; Zaller 1992; 1996). And they tend to use a modicum of information they receive to rationalize and strengthen their partisan viewpoints (Lodge and Taber 2013; Taber and Lodge 2006).

But the case against evidence-based reasoning in a (hypothetically) informed electorate is not settled. While the examples of political persuasion have been documented in many experimental studies (see Druckman et al. 2013; Guess and Coppock 2015 for recent examples), few studies manipulate arguments in such a way that the observed persuasion can be considered clear evidence for (or against) citizens' ability to critically evaluate the quality of information. In addition, fully rational Bayesians may appear to exhibit "biased assimilation"—the empirical bedrock of a second line of skepticism (e.g., Taber and Lodge 2013)—under some plausible conditions such as that people holding different values use differential evaluative criteria, and/or that their

responses to argument evaluation questionnaires are causally posterior to their conclusions. The second half of the current research (Chapter 3) represents an attempt to measure the extent to which evidence matters, directly pitting Bayesian rationality against the skeptics' hypotheses.

4.1 Main Findings and Their Implications for the Literature

There are several important findings reported in the previous chapters. In Chapter 2, I showed that the range of considered argument is generally slanted in the direction of partisan identities, and that this tendency is far stronger among those who possess high levels of factual information. This raises important issues regarding the civic competence of the knowledgeable, and even what one means by concepts such as "informed opinions" or "political knowledge." To be clear, nothing in these findings questions the beneficial role of factual information for democratic decision making. It does suggest, however, that the ways in which citizens receive and process political information do not necessarily lead them to "fully consider" the merits and perils of the choices they make.

Given the skewed distribution of argument repertoires among the well informed, it is not surprising that "informed" and "considered" opinions diverge on the support for a universal healthcare program—the second takeaway of this dissertation. Taken together, these findings call for a conceptual distinction between "informed" opinion (e.g., Althaus 1998; Gilens 2001), and "considered" opinion (e.g., Luskin et al. 2002), concepts which have been used interchangeably in the literature (e.g., Fishkin 1995; Price and Neijens

1998). Citizens could have a large base of information about politics, and still hold ill-considered opinions. On a related note, they help explain why "informed" citizens exhibit arguably undesirable tendencies, such as holding incorrect beliefs aligned with their partisan identity (Achen and Bartels 2006; see also Taber and Lodge 2006). My research suggests that the limitations in conceptualization and measurement that have failed to purge political knowledge of biased awareness may be the reason for some of the apparent perils of "sophistication."

But no matter how one defines adequate political knowledge, the importance of being informed about politics ultimately depends on the assumption that people refine their opinion in light of what they have learned. Some find this assumption unrealistic. One concern is that people can easily be manipulated, often failing to account for the strength of empirical evidence as they form their opinions. Another concern is that they can be irrationally defensive about their partisan viewpoints often not budging an inch even when faced with strong counterevidence. Neither was the case in the experiments reported in Chapter 3. Instead, I found that people update their beliefs and attitudes in response not only to the position of the argument, but also the (un)certainty of evidence. Pro-ACA arguments shifted people's mind in the expected direction, and more so when the evidence was strong. Perhaps more important, I found no evidence of defensive reaction to counter arguments, which is especially impressive considering that the experiments drew on highly contentious issues in American politics. The patterns of belief and attitude changes shown in the chapter imply that people evaluated the

diagnostic values of presented evidence in more or less the same way across prior attitudes, partisan identity, and other individual traits.

These findings contribute to the long-standing controversy about citizens' critical ability to use information and complement recent research demonstrating the effects of substantive information and arguments on policy opinions (e.g., Boudreau and MacKenzie 2014; Bullock 2011; Chong and Druckman 2010; Druckman et al. 2013; Guess and Coppock 2015; Mérola and Hitt 2016), by demonstrating particularly that evidentiary (un)certainty puts constraints on belief and attitude updating. This is rare evidence that people account for the differential probative values of strong versus weak evidence when they update their opinions—even when the evidence challenges prior opinions about highly contentious issues.

My findings are seemingly inconsistent with two important strands of prior research: (1) partisan disagreements and misperceptions about factual matters at the macro-level (e.g., Bartels 2002; Berinsky 2012a; 2012b; 2015; Flynn et al. 2017; Jerit and Barabas 2012; Kuklinski et al. 2000; but see Bullock et al. 2015; Prior et al. 2015); (2) and the micro-level findings of motivated reasoning whereby people reject uncongenial information when confronted, sometimes doubling down on their prior attitudes (e.g., Lord et al. 1979; Nyhan and Reifler 2010; Taber and Lodge 2006). I will now attempt to reconcile my findings with the large body of evidence documenting partisan bias.

With respect partisan gaps in factual beliefs and misperceptions, the question is: how can such differences persist *if* partisans indeed interpret information in more or less the same way? A plausible explanation may be that people inevitably forget the specific

content of the new information over time, and as they do so, fall back on prior opinions and party identification. While some previous studies found persuasion effects lasting at least for a few weeks (e.g., Guess and Coppock 2015; but see Hill et al. 2013 and Gerber et al. 2011 for examples showing more rapid decay), rarely do researchers attempt to measure communication effects that last for several months, leaving open the possibility that communication effects become minimal in a longer stretch of time. Indeed, the treatment effects of new information found in Experiment 2 substantially decayed (though partially surviving) 80 days into the post-treatment period, and almost entirely disappeared in 160 days. Although hardly surprising, the long-run decay shown in my experiments has an important implication: even if new evidence matters momentarily, at the end of the day, public opinion would converge toward partisan viewpoints most of the time. As such, the force of the better evidence is itself transitory, often failing to upset the deeper partisan bedrock.

Of course, partisans may converge toward the best available evidence *if* partisans continue to receive a stream of similar information doubling down on the same conclusion repeatedly. But this scenario seems unlikely, given citizens' lack of interest in political issues and tendency to seek out information that confirms their pre-existing attitudes to the extent that they consume political information at all (Bullock 2009)—a point I return to below.

How then can my findings be squared with the motivated reasoning literature (e.g., Kunda 1990; Lord et al. 1979; Miller et al. 1993; Taber and Lodge 2006; Nyhan and Reifler 2010; Nyhan et al. 2014)? First, it is worth re-emphasizing the point that people's

response to ostensible argument evaluation items can differ across prior attitudes or partisan leaning *even when* they are in full agreement about the diagnostic values of the presented information. While such findings (conflicting evaluations about the same information among partisans) have been taken to show that the likelihood function of evidence is "biased" toward prior attitudes (e.g., Lord et al. 1979), they might also reflect the fact that people starting from different priors fail to converge in the end because the presented evidence is not clear enough (Gerber and Green 1999). Indeed, I would have concluded that the participants in the ACA experiments also engaged in biased assimilation, had I relied solely on how their stated evaluations of the arguments were dependent on their priors—a misguided conclusion, given the parallel (and sometimes convergent) belief updating patterns outlined throughout Chapter 3. This, of course, is not to say motivated reasoning never occurs, but to question its predominance as *the* default mode of political reasoning.

Importantly, there exists a considerably smaller but more convincing body of evidence demonstrating "backfire" effects whereby people strengthen their prior opinions in face of counterevidence (Nyhan and Reifler 2010; 2015; Nyhan et al. 2014). This is more directly at odds the findings reported in Chapter 3 than other studies open to alternative explanations (e.g., Lord et al. 1979; Taber and Lodge 2006). But the prevalence of such backlash is also in dispute, as more recent studies found little to mixed evidence (Bishin et al. 2016; Guess and Coppock 2015; Wood and Porter 2016). Taken as a whole, there emerges an intriguing puzzle: under what conditions, does the force of evidence trump the power of denial? I examine this question in terms of whether my

findings can be generalized to different contexts, different issues, and different features of persuasive messages.

With respect to the context, the online experiments presented in Chapter 3 were designed to isolate the effect of a single piece of argument. This "sterilized" setup is admittedly less than ideal, as it resembles neither the usual information environments in which citizens are situated nor the normative speech situation designed by deliberative theorists. Democratic competition leads (or forces) citizens to have access to arguments from competing sides (Chong and Druckman 2007), and I have argued that it is critical that they do so in a balanced fashion to make considered decisions. One important possibility is that the effects of multiple arguments become interactive, rather than additive, such that the force of counterevidence arises only in the absence of a challenge, and recedes into null in a competitive environment. However, the evidence for this possibility is mixed in the previous literature.

On the one hand, studies on the inoculation theory (McGuire 1961) have found that warning people about future persuasive attempts and preemptively discrediting the argument helps people resist them (e.g., Pfau et al. 2001), even when the "preemption" does not provide specific evidence against the forthcoming message (see Banas and Rains 2010 for a meta-analysis). In the "real-world" context, calling out the "mainstream" media's dishonesty and bias is one of the tactics used most frequently by conspiracy theorists, commentators in partisan media and (some) politicians (see Jamieson and Cappella 2008). Doing so may lead people to think that contradictory information simply proves the other side is biased, and thus reinforce their prior opinions. Two well-known

cases of misbeliefs (about vaccines' effect on autism and the presence of WMD in Iraq) that "backfired" (Coppock 2016; Nyhan and Reifler 2010; 2015; Wood and Porter 2016) were, perhaps, of this variety.

On the other hand, Chong and Druckman (2007, 651) found "competing frames tend to stimulate individuals to deliberate on the merits of alternative interpretations" making the effect of argument strength *more* pronounced. It is also worth noting that the observational replication of the ACA experiment did show that people sensibly shifted their opinions in the wake of the premium increase story that broke right toward the end of the 2016 presidential campaign—arguably one of the most fiercely combative moments in the modern history of American politics. A potentially fruitful avenue for future research is to examine the effects of evidence in competitive environments where participants receive both pro and con arguments, but one side offers clearly better evidence. The understanding of voter rationality will be further sharpened by exploring whether people start ignoring even strong counterevidence in favor of much more dubious, or irrelevant evidence supporting congenial arguments, and if so, under what conditions.

Secondly, the generalizability of the experiments presented in Chapter 3 should depend on the extent to which the effects of evidence certainty are moderated by the characteristics of the political issue in question. However, to the extent that there is heterogeneity across issues, the evidence provided in Chapter 3 would be closer to the lower bound. People are expected to have strong priors as well as the motivation to defend the priors for polarized and salient issues such as the Affordable Care Act and the

economic performance of the parties—conditions that are likely to attenuate the treatment effects of new information, according to the motivated reasoning hypothesis (Taber and Lodge 2006). Indeed, the two issues were chosen precisely for this reason. And it seems unlikely that new evidence would be less consequential when people update their beliefs about non-partisan issues for which people possess even less baseline knowledge, and less desire to maintain their pre-existing opinions.

That is not to deny the possibility that, for some issues, empirical evidence would not matter much. Importantly, my experiments focused on "valence issues" for which the end goals are relatively unambiguous. Most people want better and cheaper healthcare that covers all. And no one would want to see slower income growth. The disagreement is largely about the means—e.g., can the Affordable Care Act make health care less expensive? One can at least *imagine* some piece of strong empirical evidence that can provide a straightforward answer to questions like this. But when it comes to "position issues" such as the pro-life/pro-choice controversy, the core disagreement between the competing sides comes down to the normative question of what's right and what's wrong. It is more difficult to fathom what "strong" evidence would create the kind of opinion shifts observed in Chapter 3 for such value-laden issues. But at this point, we have moved passed the scope of this dissertation. To the extent that people fail to build consensus about moral issues after much deliberation, it would be extremely difficult (and perhaps impossible) to tease out whether the remaining disagreement reflects motivated bias or value differences.

Third, certain features of the treatment messages may have made them amenable to finding support for the Bayesian model. The messages were probably outside the norm of the usual political discourse than what people are accustomed to, in that statistical facts were put in the forefront of the arguments instead of partisan talking points mixed with emotional appeals and ad hominem attacks. One can imagine a different version of the treatment messages that could have made people's partisan identity more salient, triggering more defensive motivated reasoning. Perhaps consistent with this possibility is the finding that the effect of argument strength disappears when participants are told that Democratic and Republican politicians are polarized on the issue at hand (Druckman et al. 2013).

On the other hand, the partisan nature of the issues themselves (Obamacare and the economic performance of the parties) must have been unambiguous to most participants, and it is doubtful that making the messages *more* partisan would have made much difference. Tellingly, in the ACA experiment, calling the law "Obamacare" versus "the Affordable Care Act" in the treatment messages—the former presumably being the more partisan label—made no discernable difference in how people responded to the messages. Also, Coppock's (2016) survey experiment shows that insulting language did *not* keep people from rationally updating their opinions in light of new information. Taken together, to the extent that the treatment messages were unique in their rather reserved and analytical tone, prior research does not provide enough evidence to think that the patterns of opinion updating would have been much different had the messages been designed to instigate "hot cognition" (Redlawsk 2002; Taber and Lodge 2006).

To be sure, participants in the experiments did receive a wealth of statistical evidence that is more detailed and probably better explained than they typically would find in other contexts. Political persuasion in the "real world" is likely to be less dramatic than what was reported above. But that was precisely the point of the three experiments to begin with—examining how people behave in a counterfactual condition that gives them a better chance to see evidence clearly and make up their minds. The fact that under these conditions they do follow a rational belief updating model has important normative implications about citizen competence and political environments, which I further elaborate in my concluding remarks.

All in all, while there are issues with the external validity of the findings shown in Chapter 3 vis-à-vis the motivated reasoning hypothesis, they could cut both ways. One can think of a situation where people become more defensive about their priors, as well as a situation where people give even more weight to new information. This means more research is needed to establish the boundary conditions of evidence-based reasoning versus motivated reasoning. Previous understanding has been that people would be motivated to defend their opinions about polarized partisan issues. But this was simply not the case in my experiments. Identifying what else is needed to trigger motivated reasoning remains an important avenue for future research.

Finally, Chapters 2 and 3 provide evidence that has important implications for the growing scholarship on public deliberation. While expanding the range of arguments and counterarguments that people are exposed to is often offered as a central mechanism for the positive benefits of deliberation on opinion formation (Fishkin 1995; 2009), this

proposition has remained unsubstantiated, largely because it requires strong and often problematic normative assumptions to determine a priori what collective opinions are to be held by the "considered" or "enlightened" public (see Kulinski and Quirk 2001; Price and Neijens 1998). The approach taken in Chapter 2 helps to resolve this issue by advancing hypotheses that do not depend on such normative assumptions. Additionally, the supporting findings suggest that deliberation does lead to more "considered preferences," not only in the sense that the procedural requirements such as balanced argument pools are met, but also in that the outcomes closely resemble the hypothetical collective opinions that correct for the absence and imbalance of considerations.

Perhaps more importantly, Chapter 3 provides evidence for the central assumption of deliberative democracy that citizens are capable of weighing different ideas, and forming meaningful public opinion led by "the force of the better argument" (Habermas 1984; Fishkin 1995; Mercier and Landemore 2012). Despite the importance of this assumption for the theoretical framework, there has been little empirical evidence that directly supports it—and plenty that calls it into question (Delli Carpini et al 2004; Mendelsberg 2002; Mutz 2008). The key difficulty again is a normative one; that is, the subjectivity involved in predetermining what is "the better argument" toward which public opinion should converge after deliberation. I avoided making strong, and potentially problematic normative assumptions, by focusing on evidence certainty, a sub-element of the umbrella concept of argument strength stripped of the value-laden components. My research, thereby, provides the empirical evidence to believe in "the force of the better *evidence*" at least.

Overall, I believe that these findings represent an important step forward in the long process of resolving the controversy around how to set the benchmark of civic competence, and how people measure up. Nonetheless, it is only a step. There are several important limitations that require future research.

First, the approach taken in Chapter 2 to revisit the classic definition and measurement of political knowledge should not be considered the only way to go. While prior research provides theoretical justification of using "argument repertoire" as a measure of opinion quality, and for the empirical tests of its construct validity and reliability (Cappella et al. 2002; see also Mutz 2006 for an application of similar measures), I recognize that it has its limitations as an open-ended measure. For one, it can be confounded with irrelevant factors such as one's engagement with the survey itself. On a more fundamental level, it may be a deficient proxy for *accurate* knowledge of "inconvenient" facts, because accuracy was not a part of the coding scheme. Future research should develop empirical strategies to measure being "well informed" in this particular sense—accurate understanding of congenial and uncongenial information—and examine its causes and effects.

Another limitation of this dissertation has to do with the fact that only two issues (health care and the economy) were used across the studies. Particularly with respect to the discrepancy between information and consideration outlined in Chapter 2, it is important to keep in mind that one may reach a very different conclusion when looking at an issue that is less divided than the health care issue in the United States. This is because the discrepancy is assumed to arise when people fail to learn information that runs

counter to their *partisan* viewpoints. The match between the two dimensions of political knowledge, therefore, can very well be issue-dependent. But at least when it comes to "salient" issues of the day, the results may be more the rule than the exception because many public debates are often fueled by vehement disagreements among partisans. Nonetheless, more replications would be essential to better understand the potential heterogeneity across various issues.

Moreover, the generalizability of findings from the Chapter 3 experiments can also be questioned due to the characteristics of the samples drawn from MTurk. It is well established from prior research (e.g., Berinsky et al. 2012) that MTurk respondents are less representative of U.S. population compared to national probability samples, although more so than in-person convenience samples. The estimates presented above, therefore, are the *sample* average treatment effect (SATE), which may or may not be close to the target estimates of the population average treatment effect (PATE). The correspondence between the two depends on (1) the observed and unobserved differences between the sample and the population, and (2) the treatment effect heterogeneity across such differences.

However, it is important to note that while the PATEs can very well be smaller or larger than the SATEs reported above, it is highly implausible that they approach zero, or even flip signs. The condition under which the worst case scenarios come true would be that some segments of population unrepresented in the sample have extremely deviant likelihood functions, which lead them to update their belief downward substantially in

face of strong pro arguments, and upward substantially in face of strong con arguments.[68] It is very difficult to imagine who these people might be. It is telling in that regard that the moderating effects of various individual factors that *were* examined above turned out to be mostly modest in size and statistically insignificant. In addition, recent studies have found considerable similarity between the results from online convenience samples and those from national samples (Berinsky et al. 2012; Coppock 2016; Krupnikov and Levine 2014; Mullinix et al 2015), somewhat allaying the concerns about the generalizability of the results in Chapter 3. Even so, I recognize the possibility that some unobserved features of the samples may have led me to overstate (or understate) the treatment effects, and also the fact that it is not possible to quantify the uncertainty associated with this potential issue. Only future research replicating the results with more representative sample can put this concern to rest.

On a more fundamental level, one may argue that the treatments employed in the previous chapters do not really resemble how people learn about policy issues on a daily basis. People do not usually get together with those holding different viewpoints to refine their opinions (Chapter 2). And the political messages they do consume are often rhetorical points devoid of facts and substance (Chapter 3). What, then, do the findings say about democratic politics?

[68] Recall that some treatment effects were above 20 percentage points.

4.2 Implications for Democratic Politics

The overarching conclusion of this dissertation is that citizens usually don't—but can—form considered opinions about political issues. From a normative standpoint, the discouraging side of my argument is that there is little guarantee that even "informed" citizens—let alone less attentive citizens—would make well considered decisions. Of course, my study showed that some had considered both sides of arguments carefully enough to articulate the rationales for the competing perspectives in some detail. But the fact that their opinions significantly differed from other citizens (including informed ones), in conjunction with the imputed and experimental estimates of considered opinions, ultimately suggests the public as a whole may be holding policy opinions that they would not have held if they had a chance to go beyond their usual diet of political information consumption, and consider more carefully the competing arguments.

Arguably the most damaging blow to the prospects of deliberative democracy comes from the notion that it does not even matter whether people have balanced substantive information about policy issues because they are too uncritical and/or too biased to use it rationally anyway. But this skepticism, at least according to my argument and evidence provided in Chapter 3, is misplaced and exaggerated. People do not simply believe whatever they want to believe. Nor do they simply believe whatever politicians want them to. Rather, given the chance people appear capable of accounting for evidence and its (un)certainty, suggesting that public opinion would tend toward the best available evidence as the amount and clarity of information that people receive increase. It offers

some reason to believe that public opinion reflects (or *can* reflect) rational thinking on the part of the citizenry, at least for salient, widely covered, politically significant issues. This claim runs against the tide of much scholarly and popular thinking, but I would argue that it is consistent with some important observations from previous research on public opinion and voter behavior, such as that an incumbent party's reelection prospects diminish dramatically in a failing economy (e.g., Kramer 1971; Hibbs 2000; Lewis-Beck 1988; but see Healy and Lenz 2014); or that partisan disagreement about economic conditions recedes in face of indisputably clear signals (Parker-Stephen 2013; Stanig 2013). These studies and my research here seem to suggest that, everything else being equal, citizens would give less credence to politicians' claims when the evidence isn't simply there—a reason to be at least cautiously optimistic about the prospect of democratic accountability. In short, the findings reported in Chapter 3 add (a hint of) optimism to the literature on public opinion that has tilted toward documenting voter irrationality (e.g., Bartels 2002; Campbell et al 1960; Berelson et al. 1954, Ch. 14; Taber and Lodge 2006; Zaller 1992) over rationality (e.g., Fiorina 1980; Gerber and Green 1999; Key 1966; Page and Shapiro 1992).

To the extent that citizens fail to act as competent decision makers, it is more likely so because they do not have the necessary (and necessary mix of) information (Chapter 2), than because they are unable to use information even if they have it (Chapter 3). According to the findings, some of the more troubling tendencies observed in public opinion research, such as partisan differences in factual beliefs (e.g., Bartels 2002) and political misbeliefs (e.g., Berinsky 2015; Kuklinski et al. 2000), are likely to arise

because partisans infer these beliefs based on the talking points of their party leaders and not much else (Achen and Bartels 2016, 280)—but not because partisans refuse to recognize clear factual evidence that goes against their priors. There are two ways in which people exhibit partisan biases, failing to give due considerations to oppositional arguments: they may not receive them at all, or they may reject them outright upon reception. This dissertation confirms the former possibility, but not the latter. It thus underscores that improving the quality of public opinion, after all, comes down more to solving the problems of ignorance and partisan bias at the "reception" stage—encompassing both exposure to and comprehension of information—, than at the "acceptance" stage.

I realize that it may seem that I am proposing even loftier goals than the traditional model of informed citizenship—i.e., knowing "what the issues are, what their history is, what the relevant facts are, what alternatives are proposed, what the party stands for, and what the likely consequences are" (Berelson et al. 1954, 308)—by introducing an additional requirement that the mix of information be balanced. No one thinks that citizens have the time and energy to know every aspect of a political decision, let alone politics in general. But, as Delli Carpini and Keeter (1996, 14) put it, "the value of political information is … relative and collective." The point is that all else being equal, one's opinion is more likely to be reflective of one's interests when one takes uncongenial information more seriously. Perhaps more importantly, the collective opinions of a group of citizens with similar interests, and ultimately of the citizenry as a

whole are *far* more likely to be reflective of their collective interests when individuals take uncongenial information more seriously.

Up to this point, I have focused almost exclusively on the civic competence of the public. But the best citizens can do is to form opinions reflective of available information; information which, for the most part, is supplied by political and media elites. Things can go awry on "the supply side." Elites can intentionally or unintentionally "mislead" the public with biased, selective, and/or erroneous information and interpretations (Page and Shapiro 1992, Chap. 9). They can present competing arguments in ways that make it hard to take the other side seriously—for example, by yelling at each other simultaneously, or denigrating each other's motives or values. They can also avoid communicating in ways that make clear the degree of certainty or uncertainty of their evidence and the implicit assumptions underlying their claims, instead sticking to partisan talking points. All these forms and styles of elite discourse—unfortunately and increasingly standard fare in political communication—may make it difficult for citizens to base their views on appropriate and appropriately balanced information, even if they wanted to do so.

It is well beyond the purview of this dissertation to explicate the necessary features of political environments that would help citizens make decisions that are reflective of their individual and collective best interests in great detail. But these data and findings point at least indirectly to the features of political environments that would help citizens develop considered opinions. Specifically, the counterfactual situations simulated in the experiments, where people talked about policy options with those with

whom they disagreed (Chapter 2), and evaluated the strength of available evidence based on factual information (Chapter 3), provide good starting points. As such, I briefly discuss the implications of my findings for those envisioning an information environment that is more conducive to better political decision making than the status quo.

The research presented in this dissertation identified deficits in *what* people know about political issues, but not in *how* people use information once they have it. The fact that the key challenge to competent decision making is at the "reception" stage underscores the importance of better civic education. And the challenge is not just that people are generally underinformed, but also that even when they do learn, they tend to learn particular sets of information that support their partisan opinions. This means that people may miss the opportunity to see why their party's policy position is not in their best interests or in the larger public interest—even if they do follow politics and learn about the issues. While formal and ongoing public deliberation *per se* would not be a practical alternative for most citizens, there can be some (modest) changes to the information environment that may increase the likelihood of citizens considering a wider range of viewpoints.

One possible way would be for columns, op-eds, and the growing number of partisan or ideological media outlets to regularly introduce its readers/viewers to the "other side's" perspectives; not, as is often currently the case, to counter-argue, criticize, or make fun of these opposing viewpoints, but to give the audience the opportunity to understand why they disagree. For example, the left leaning *Guardian* launched the publication of a new weekly column called "Burst Your Bubble" that provides "five

conservative articles worth reading to expand your thinking each week" with some background information about the author and the story to its (liberal) readers. According to Wilson, the author of Burst Your Bubble, the column has become quite popular, and perhaps other press or networks can learn from its success.

Another approach that the major social networking services such as Facebook and Twitter can take may be to tweak their news feed algorithms, so that their users' information feeds contain *more* cross-cutting viewpoints than they would otherwise. While algorithmically ranked news feeds have been blamed for creating echo chambers by selecting ideologically similar content to share with their users, they could just as easily include dissimilar content to users' information feeds in ways that parallel the *Guardian's* approach. In the meantime, citizens need not wait for the social media services to rescue themselves from their echo chambers. New smartphone applications (e.g., *Read Across The Aisle*) and browser extensions (e.g., *Escape Your Bubble*) have been developed to help people monitor their "media diet" in terms of balance and receive more cross-cutting content. Such services make it easier for citizens to check if they are getting a healthy diet of quality information and well considered arguments. One can also go to the Wall Street Journal's "Blue Feed, Red Feed" site to see stories shared by liberal and conservative Facebook users side by side.

Perhaps more importantly, the willingness to consider and understand uncongenial arguments should be emphasized more emphatically as a hallmark of civic virtue and responsibility. The fact that people have been developing new tools to burst "filter bubbles" seems to suggest that the motivation to learn from the other side has been

growing lately—an encouraging development. But this change may be too modest to counterbalance the tides of partisan fragmentation and affective polarization that have swept political discourse and public opinion over the past decade (e.g., Iyengar et al. 2012). It should be communicated more forcefully—in public education, by the media, and by public officials and other elites—that the more people fall deeply into their echo chambers, the more difficult it becomes to build consensus around common ground, or to understand if a political decision may ultimately backfire against one's individual and collective interests.

Much of scholarly work and conventional wisdom has focused on how the deliberative, informed citizen model fails. Much less is known about how it works at all, and less still about how to make it work better. I have sketched a few preliminary ways to give people better opportunities to form considered opinions. The analyses presented in this dissertation suggest that citizens are both willing and able to take advantage of such an information environment, and to do so in ways conducive to the practice of democratic politics.

Appendix

Appendix A The Health Dialogue Data (Chapter 2)

Appendix A.1. Sample Characteristics

Table A1. Health Dialogue Sample Characteristics Compared to ANES and CPS

Variables	Health Dialogue N = 2,193	2004 ANES N = 1,211	2004 CPS N = 226,672
Age (Average)	44.7	47.3	43.7
Female (%)	53.4	53.3	51.7
Bachelor's Degree (%)	34.7	29.9	23.7
HH Income $ 50,000 or Higher (%)	52.5	48.0	52.3
Access to Health Insurance (%)	86.8	85.9	84.2
White (%)	78.7	72.7	81.7
Black (%)	8.3	15.0	11.7
Married (%)	64.2	51.6	53.2
Northeast (%)	18.9	18.0	19.0
Midwest (%)	22.2	25.9	22.6
South (%)	35.2	34.4	35.8
West (%)	23.4	21.7	22.6
Strong Democrat (%)	16.5	17.0	NA
Moderate Democrat (%)	16.4	15.0	NA
Independent (%)	34.5	39.0	NA
Moderate Republican (%)	16.4	12.9	NA
Strong Republican (%)	16.5	16.2	NA

Note. CPS results were derived from CPS Table Creator.[2]

This section compares characteristics of the Health Dialogue Project to 2004 American National Election Study (ANES) against the American adult population as

[2] http://www.census.gov/cps/data/cpstablecreator.html

documented by the 2004 Current Population Survey (CPS) from the U.S. Census Bureau. Overall, as shown in Table A3, the characteristics of HD sample were fairly similar to the ANES sample, and the CPS benchmark. While those in the HD sample were somewhat more educated, and more likely to be married than the ANES and CPS data, the HD sample resembled the CPS benchmark more closely than the ANES sample on age, income, and race (White).

Appendix A.2. Balance Check

In this section, I provide balance statistics. Treatment and control groups are balanced on pre-treatment covariates among those responded to the baseline, and post surveys, minimizing the concern that differential attrition introduced imbalances. As shown in Table A4, the coefficients on each variable and the F-statistics are insignificant (at $p < 0.05$) and the *R*-squares are very small. In addition, Table A4 shows that group assignment did not significantly affect attrition, further mitigating the concern that differential attrition is responsible for the results.

Table A2: Balance Check

	DV= Assigned to Deliberate		DV= Assigned to Deliberate (If Completed Post Survey)		DV= Competed Post Survey	
	Coef	SE	Coef	SE	Coef	SE
Support for Universal Healthcare (0-1)	0.01	0.04	-0.04	0.05		
Pro arguments (0-42)	0.01+	0.00	0.01	0.01		
Con arguments (0-48)	0.00	0.00	0.00	0.01		
Political Information (0-1)	0.02	0.07	0.02	0.08		
Party ID (0-1, 1=strong Dem)	-0.04	0.04	-0.01	0.05		
Age (0-1, 0 = 18, 1=94)	0.04	0.06	0.00	0.08		
Years of Education (0-1)	0.04	0.06	0.06	0.08		
Gender (1=male)	0.00	0.02	-0.02	0.03		
Income (0.02-1, percentile)	0.00	0.04	0.03	0.05		
Black (1=Black)	0.02	0.04	-0.02	0.05		
Marital status (1=married)	-0.01	0.02	-0.03	0.03		
Political Interest (0-1)	0.11+	0.06	0.08	0.08		
Political participation (0-1)	-0.02	0.06	-0.12	0.08		
Political discussion (0-1)	-0.08	0.05	0.02	0.07		
Attention to political news (0-1)	-0.09	0.06	-0.02	0.08		
Attention to healthcare news (0-1)	0.06	0.06	0.02	0.07		
Access to health insurance (1=insured)	0.01	0.04	0.00	0.04		
Own serious disease (1=yes)	-0.01	0.02	0.00	0.03		
Family serious disease (1=yes)	-0.03	0.02	-0.02	0.03		
Healthcare satisfaction (0-1)	-0.01	0.05	0.03	0.06		
Assigned to deliberate	0.48	0.06	0.61	0.08	-0.01	0.02
Intercept	0.48*	0.06	0.61*	0.08	0.73*	0.02
Observations	2,080		1,276		1,844	
R^2	0.010		0.009		0.000	
F	1.08		0.60		0.30	
p	0.36		0.92		0.59	

Note. OLS estimates with standard errors. + $p < 0.1$ * $p < 0.05$. Second model excludes those who did not take post-treatment surveys. Third model excludes those uninvited (via random assignment) to take the post survey. F tests fail to reject the null hypothesis that the coefficients are jointly zero.

Appendix A.3. Measurement and Survey Items

This section provides information on the measurement of key variables and covariates. Since the measurement of the key independent and dependent variables is described in the main text, for these variables we simply provide the survey items here.

Some basic demographic variables used in our analyses were provided by Knowledge Networks from the respondent profiles. These include *age, education, gender, income, race,* and *marital status*. *Age* was rescaled to vary between 0 and 1. *Education* was initially measured based on seven categories. We calculated years of education by assigning the following numbers to each category: less than high school (8 years), some high school (10), high school graduate (12), some college (14), BA (16), master's degree (18), doctorate (21). I rescaled this to 0-1. (Household) *Income* was measured on 19 categories, ranging from less than $5,000 to $175,000 or more. We took the percentile values, with 1 representing the highest income level. Dummy variables were created *gender* (male), *race* (i.e., Black), and *marital status*.

Political interest was measured by combining the two items, which were coded on a 0-1 scale, with 1 indicating the highest interest level and averaged (Cronbach's α=0.45).

Political participation. I counted the number of participatory activities that respondents indicate that they engage in from the list of nine activities (Cronbach's α=0.72), which was then rescaled to 0-1.

Political discussion was measured based on two items, which was averaged and rescaled to 0-1 (Cronbach's α=0.64).

Attention to political was measured based on three items, which was averaged and rescaled to 0-1, with 1 indicating "very closely" (Chronbach's α=0.73).

Attention to news about healthcare issues was constructed from a single item, which was scaled to 0-1, with 1 indicating "very closely."

Access to health insurance was measured based on a single item, asking if the respondent was covered by health insurance.

Own serious disease was measured by asking if the respondent had a serious health condition. Those who noted "Yes" to both were coded 1, indicating having a serious disease or chronic health condition, and 0 otherwise.

Family serious disease was measured by asking if "anyone living in the same household" had one of the serious health condition. Those who noted "Yes" to both were coded 1, indicating having a family member with serious disease or chronic health condition, and 0 otherwise.

Survey Item Wordings

Support for universal health insurance.

> **Do you favor or oppose** a universal, single-payer system of national health insurance, paid for by the federal government (that is, a publicly financed, but privately delivered heath care system)**?**
>
> 1) Favor strongly
> 2) Favor somewhat
> 3) Oppose somewhat
> 4) Oppose strongly
> 5) Don't know

Consideration of pro and con arguments (argument repertoire)

> (If favorable) What are the reasons you have for being in favor of a universal, national health insurance program? (Please list *all* the reasons that come to mind)

What reasons do you think other people might have for being opposed to a universal, national health insurance program? (Please list *all* the reasons that come to mind)
(If unfavorable) What are the reasons you have for being opposed to a universal, national health insurance program? (Please list *all* the reasons that come to mind)
What reasons do you think other people might have for being in favor of a universal, national health insurance program? (Please list *all* the reasons that come to mind)
(If don't know) What are the reasons you think other people might have for being in favor of a universal, national health insurance program? (Please list *all* the reasons that come to mind)
What reasons do you think other people might have for being opposed to a universal, national health insurance program? (Please list *all* the reasons that come to mind)

Political information

Which one of the parties would you say is more conservative than the other at the national level?

1) Democrats
2) Republicans
3) Don't know

Do you happen to know what job or political office is currently held by Dick Cheney?

1) U.S. Senator
2) U.S. Vice President
3) Energy Secretary
4) Don't know

Which party has the most members in the United States House of Representatives?

1) Democrats
2) Republicans
3) Don't know

Who has the final responsibility to decide whether a law is Constitutional or not?

1) President
2) Congress
3) Supreme Court
4) Don't know

How much of a majority is needed for the U.S. Senate and House to override a presidential veto?

1) Simple majority (one more than half the votes)
2) Two-thirds majority
3) Three-fourths majority
4) Don't know

Healthcare issue information

President Bush has signed a Medicare bill to help senior citizens with prescription drugs. Which of the following is among the provisions of the new act offers a prescription drug discount program for Medicare beneficiaries

1) Allows Medicare beneficiaries to buy prescription drugs from Canada
2) Permits the federal government to negotiate with drug companies to get lower prices for those in Medicare
3) Don't know

Which of the following has responsibility for deciding when drugs are ready for use by the public?

1) FTC, Federal Trade Commission
2) FDA, Food and Drug Administration
3) APHA, American Public Health Association
4) Don't know

Do you happen to know what job or political office is currently held by Tommy Thompson?

1) State Governor
2) U.S. Surgeon General
3) U.S. Health and Human Services Secretary
4) Don't know

Many experts say that Medicare's main fund, the hospital insurance fund, will run out of money if there are no changes in the way it works. According to Medicare's public trustees, when is the fund now expected to run out of money?

1) Within about 5 years
2) In the next 10 to 30 years
3) In the next 40 to 60 years
4) Later than 70 years from now
5) Don't know

Which of the following best characterizes the Bush administration's policy concerning research on stem cells derived from human embryos?

1) Federal funds <u>cannot</u> be used to support any research on stem cells derived from human embryos
2) Federal funds <u>can</u> be used to support any research on stem cells derived from human embryos
3) Federal funds <u>can</u> be used to support research on stem cells, but only on a limited number of cell lines derived from embryos before August 2001
4) Don't know

The Medicare program provides health care coverage to the disabled and to older Americans who paid enough in payroll taxes while they worked. Which of the following best describes who is generally eligible for Medicare?

1) Age 55 or older, and earning less than $25,000 in income
2) Age 65 or older, and earning less than $25,000 in income
3) Age 65 or older, regardless of income
4) Age 70 or older, regardless of income
5) Don't know

The percentage of Americans who do not have health care insurance is closest to …

1) 5 percent
2) 15 percent
3) 33 percent
4) 50 percent
5) Don't know

Party identification

Do you generally think of yourself as a …

1) Republican
2) Democrat
3) Independent
4) Something else

(If Republican) Do you consider yourself …

1) A strong Republican
2) Not a very strong Republican

(If Democrat) Do you consider yourself …

1) A strong Democrat
2) Not a very strong Democrat

Political interest

Generally speaking, how much do you care which party wins the 2004 presidential election?

1) A great deal
2) Somewhat
3) Not very much
4) Not at all

Some people seem to follow what is going on in government and public affairs most of the time, whether there is an election or not. Others are not that interested, or are interested in other things. How much would you say you follow what is going on in government and public affairs?

1) Most of the time
2) Some of the time
3) Only now and then
4) Hardly at all

Political participation

In the last 12 months, have you personally done any of the following?

1) Contacted or written a public official about an issue that concerned you
2) Attended a public hearing or town meeting
3) Talked to anyone and tried to show them why they should vote for or against a political candidate

4) Attended any political meetings, rallies, speeches, dinners or similar events in support of a particular candidate
5) Done any other work for a candidate
6) Given money to a candidate
7) Worn a candidate's campaign button, put a campaign sticker on your car or placed a sign in your window or in front of your house
8) **Contacted a newspaper or television station about an issue that concerned you**
9) **Tried to get another person to sign a petition**

Political discussion

How many days in the past week did you discuss politics with your family or friends?

How many days in the past week did you discuss politics with acquaintances or people at work?

Attention to political news

How much attention did you pay to news stories about each of the following last week?

National politics

Local news and events

International affairs

1) very closely
2) fairly closely
3) not too closely
4) not at all

Attention to news about healthcare issues

How closely have you been following news about health care issues?

5) very closely
6) fairly closely
7) not too closely
8) not at all

Access to health insurance

Many Americans do not have health insurance. Are you now covered by any form of health insurance, including any private insurance or a government program such as Medicare or Medicaid?

1) Yes
2) No

Own serious disease

In the past 10 years, have you been told by a doctor or nurse that you have a serious or chronic health condition or disease, such as any one of the following?

High blood pressure or high cholesterol
A heart condition or heart disease
A stroke
Emphysema or other chronic respiratory condition
Cancer or malignancy of any kind
Diabetes or high sugar levels
A liver or kidney condition
Serious chronic neck, back, or joint pain
Parkinson's disease or other neurological disorder
Lasting depression or other mental health condition
Other serious or chronic health condition

1) Yes
2) No

Do you <u>now</u> have a serious or chronic health condition or disease?

1) Yes
2) No

Family serious disease

Does anyone else now living in your household have a serious or chronic health condition or disease, such as any one of the following?
High blood pressure or high cholesterol
A heart condition or heart disease
A stroke
Emphysema or other chronic respiratory condition
Cancer or malignancy of any kind
Diabetes or high sugar levels
A liver or kidney condition
Serious chronic neck, back, or joint pain
Parkinson's disease or other neurological disorder
Lasting depression or other mental health condition
Other serious or chronic health condition

1) Yes
2) No

Appendix A.4. Descriptive statistics

Table A3 reports descriptive statistics for our key variables and covariates. All variables are scaled to 0-1, except for pro and con arguments. As mentioned in the main text, pro and con arguments have skewed distribution. Table A2 shows frequencies and Figure A1 plots distributions. Majority of respondents named less than three pro arguments, and few named more than seven. Very similar patterns were found for con arguments. The distributions of pros and cons look nearly identical.

Table A3: Descriptive statistics for key variables and covariates

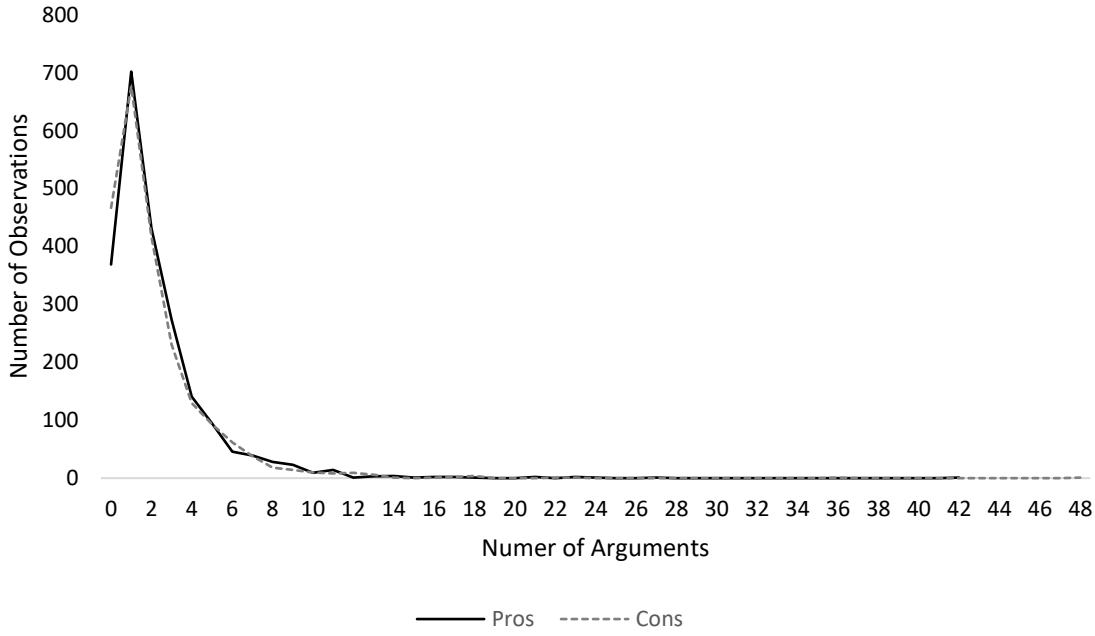
Variables (range)	Mean	SD	N
Support for Universal Healthcare (0-1)	0.59	0.33	2187
Pro arguments (0-42)	2.30	2.69	2193
Con arguments (0-48)	2.17	2.74	2193
Political Information (0-1)	0.59	0.21	2193
Party ID (0-1, 0=strong Rep, 1=strong Dem)	0.50	0.32	2170
Age (0-1, 0 = 18, 1=94)	0.35	0.20	2193
Years of Education (0-1)	0.47	0.20	2193
Gender (1=male)	0.47	0.50	2193
Income (0.02-1, percentile)	0.56	0.29	2193
Black (1=Black)	0.08	0.28	2182
Marital status (1=married)	0.64	0.48	2193
Political Interest (0-1)	0.79	0.23	2193
Political participation (0-1)	0.17	0.20	2192
Political discussion (0-1)	0.28	0.25	2188
Attention to political news (0-1)	0.62	0.22	2173
Attention to news about healthcare issues (0-1)	0.57	0.24	2186
Access to health insurance (1=insured)	0.87	0.34	2187
Own serious disease (1=yes)	0.36	0.48	2190
Family serious disease (1=yes)	0.42	0.49	2178
Healthcare satisfaction (0-1)	0.52	0.23	2164

Table A4: Frequencies of Pro and Con Arguments

	Pro	Con	Total
0	16.8%	21.3%	12.9%
1	32.0%	30.9%	6.8%
2	19.7%	19.0%	18.3%
3-4	18.9%	16.5%	26.3%
5-6	6.4%	7.1%	15.0%
7 or more	6.1%	5.3%	16.5%

N = 2,193

Figure A1: Distributions of Pro and Con Arguments



Note: The maximum of pro arguments is 42. The maximum of con arguments is 48.

Appendix A.5. Additional Information on Results in Chapter 2

This Appendix supplements the results reported in Chapter 2 by providing additional information that was omitted in the main text.

Table A5 supplements Figure 2 in the main text. In addition to what was shown in the figure, it describes specific group means, standard errors, and number of observations in each group.

Table A6 reports the estimates of Equation 2.2 in the main text. The quantities in Table 2 in the main text are derived from this model.

Table A7 reports the estimates of Equation 2.3 in the main text. The quantities in Table 3 in the main text are derived from this model.

Tables A8 to A12 supplement Figure 3 in the main text.

Table A8 reports the the estimates of Equation 6 in the Main Text, which were used to impute "fully informed" opinions. Four models are specified. All models allow the effect of information to vary by party ID. Models 3 and 4 allow the effect of information to vary by other demographic factors (e.g., access to health insurance), which may affect evaluative criteria. Models (1) and (3) use the original political knowledge index. Models 2 and 4 use the percentile values.

Table A9 reports the the estimates of Equation 7 in the Main Text, which were used to impute "fully considered" opinions. A total of 16 models are specified. All models allow the consideration of pros and cons to vary by party ID. In addition, Models 9 to 16 allow the effect of information to vary by other demographic factors (e.g., access

to health insurance), which may affect evaluative criteria. Also, Models 5 to 8, and 13 to 16 allow the effect of one side of argument to be heterogeneous by the awareness of the other side. In Models 1, 5, 9, 13, the number of pro (or con) arguments larger than three was recoded to three so that the maximum can be three, which was then divided by three. In Models 2, 6, 10, 14 values greater than five were recoded (to five). In Models 3, 7, 11, 15 the collapsing cutoff was 7. In Models 4, 8, 12, 16, percentile values of pros and cons were used without collapsing.

Table A10 reports four estimates of information effects, imputed in accordance with the 4 models on in Table 8. Among these, the estimates from Models 1 and 3 were shown in Figure 3, but the results are similar other specifications.

Table A11 reports 16 estimates of consideration effects, imputed in accordance with the 16 models on in Table 9. Among these, the estimates from Models 2 and 10 were shown in Figure 3, but the results are similar in other specifications.

Table 12 reports the estimates of Equation 8 in the Main Text, the experimental approach to estimating consideration effects based on deliberations. Six models are specified. Models 1 and 2 estimate the "intent-to-treat" effect by regressing opinion change on random assignment. Models 3 and 4 estimate the average treatment effect on the treated by using random assignment as instrument for attending deliberation meetings. Models 5 and 6 estimates average treatment effect by regressing opinion change on deliberation attendance. The identifying assumption for Models 5 and 6 are that the potential change over time in the outcome variable for attendees and absentees would have been without the treatment. We report the results with and without control

variables for each approach. Models 1, 3 and 5 do not make covariate adjustments. Models 2,4 and 6 do. The results are robust.

Table A5: Pros and Cons by Political Information and Party Identification

	Total			Strong Rep			Moderate Rep		
Pol Info	Pro	Con	N	Pro	Con	N	Pro	Con	N
Low	1.51	1.07	586	1.72	1.53	39	1.51	1.43	81
	(0.82)	(0.06)		(0.35)	(0.36)		(0.22)	(0.23)	
Lower	2.24	1.85	576	2.11	2.49	89	1.92	1.59	104
	(0.09)	(0.09)		(0.20)	(0.33)		(0.19)	(0.15)	
Higher	2.65	2.74	613	1.98	3.26	112	2.42	2.65	100
	(0.13)	(0.14)		(0.21)	(0.41)		(0.23)	(0.25)	
High	2.95	3.29	418	1.86	3.74	110	2.63	3.52	73
	(0.14)	(0.15)		(0.13)	(0.36)		(0.35)	(0.34)	
	Independent			Moderate Dem			Strong Dem		
Pol Info	Pro	Con	N	Pro	Con	N	Pro	Con	N
Low	1.44	0.94	165	1.44	0.94	165	1.44	0.94	165
	(0.13)	(0.08)		(0.13)	(0.08)		(0.13)	(0.08)	
Lower	2.35	1.84	140	2.35	1.84	140	2.35	1.84	140
	(0.21)	(0.16)		(0.21)	(0.16)		(0.21)	(0.16)	
Higher	2.79	2.65	165	2.79	2.65	165	2.79	2.65	165
	(0.31)	(0.20)		(0.31)	(0.20)		(0.31)	(0.20)	
High	2.84	2.54	109	2.84	2.54	109	2.84	2.54	109
	(0.22)	(0.21)		(0.22)	(0.21)		(0.22)	(0.21)	

Entries are average number of pro and con arguments listed by each partisan group across different levels of political information, with standard errors in parentheses. Figure 2 in the main text is generated based on these estimates.

Table A6. Partisan bias in consideration of pro versus con arguments

	Coef.	SE	*p*
Pro	-1.08	0.15	0.00
Moderate Republican	-0.78	0.20	0.00
Independent	-1.11	0.17	0.00
Moderate Democrat	-1.19	0.20	0.00
Strong Democrat	-0.90	0.20	0.00
Pro × Moderate Republican	0.95	0.22	0.00
Pro × Independent	1.41	0.19	0.00
Pro × Moderate Democrat	1.61	0.22	0.00
Pro × Strong Democrat	1.84	0.22	0.00
Constant	3.03	0.14	0.00

N = 4340 responses (i.e., 2,170 individuals). This regression model tests Equation 2.2 in the main text. Estimates reported in Table 2 in the main text are derived from this model. "Pro" is a within-individual factor indicating whether the response is the number of pros (or cons). Dummies for PID were created and used, with the base category representing Strong Republicans. Entries are maximum likelihood estimates. Individual-specific random effects are accounted for.

Table A7. Political information and partisan bias in consideration of pro versus con arguments

	Coef.	SE	*p*
Information	4.23	0.78	0.00
Pro	1.77	0.61	0.00
Info × Pro	-4.15	0.85	0.00
Moderate Republican	-0.31	0.71	0.66
Independent	-0.10	0.61	0.87
Moderate Democrat	-0.16	0.67	0.81
Strong Democrat	-0.82	0.71	0.25
Info × Moderate Republican	-0.22	1.04	0.83
Info × Independent	-0.84	0.88	0.34
Info × Moderate Democrat	-0.74	1.02	0.47
Info × Strong Democrat	0.35	1.03	0.74
Pro × Moderate Republican	-0.77	0.78	0.32
Pro × Independent	-1.19	0.67	0.07
Pro × Moderate Democrat	-1.03	0.73	0.16
Pro × Strong Democrat	-1.61	0.77	0.04
Info × Pro × Moderate Republican	2.28	1.14	0.05
Info × Pro × Independent	3.71	0.97	0.00
Info × Pro × Moderate Democrat	3.74	1.12	0.00
Info × Pro × Strong Democrat	5.14	1.13	0.00
Constant	0.12	0.55	0.82

N = 4340 responses (i.e., 2,170 respondents). This regression model tests Equation 2.3 in the main text. Estimates reported in Table 3 in the main text are derived from this model. "Pro" is a within-individual factor indicating whether the response is the number of pros (or cons). Dummies for PID were created and used, with the base category representing Strong Republicans. Entries are maximum likelihood estimates. Individual-specific random effects are accounted for.

Table A8: Regression models for imputation of informed opinions

	Model (1)		Model (2)		Model (3)		Model (4)	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Intercept	0.84	0.04	0.68	0.03	0.70	0.09	0.64	0.06
Political Information	-0.68	0.06	-0.50	0.04	-0.21	0.14	-0.15	0.11
PID	-0.24	0.07	-0.01	0.05	-0.19	0.07	0.02	0.05
Information×PID	0.97	0.10	0.69	0.07	0.84	0.11	0.60	0.08
Age					0.14	0.11	0.10	0.08
Education					0.00	0.11	0.01	0.08
Male					0.02	0.04	0.01	0.03
Income					0.07	0.07	0.06	0.05
Black					-0.08	0.07	-0.06	0.04
Married					-0.06	0.04	-0.06	0.03
Insured					0.02	0.06	0.00	0.04
Satisfied					-0.02	0.08	-0.09	0.06
Own Health					0.14	0.04	0.11	0.03
Family Health					-0.03	0.04	-0.01	0.03
Information×Age					-0.20	0.17	0.60	0.08
Information×Education					0.05	0.17	-0.14	0.12
Information×Male					-0.04	0.06	0.05	0.12
Information×Income					-0.09	0.12	-0.03	0.05
Information×Black					0.14	0.12	-0.08	0.09
Information×Married					0.05	0.07	0.13	0.09
Information×Insured					-0.11	0.10	0.05	0.05
Information×Satisfied					-0.32	0.13	-0.09	0.08
Information×Own Health					-0.18	0.07	-0.24	0.10
Information×Family Health					0.09	0.06	-0.13	0.05
Information measure	Original		Percentile		Original		Percentile	
N	2165		2165		2109		2109	

Note. All variables were scaled to 0-1. All variables are scaled to 0-1. In Models 1 and 3, the original political information score (% correct) was used. In Models 2 and 4, percentile values of political information was used.

Table A9: Regression models for imputation of considered opinions

	Model (1)		Model (2)		Model (3)		Model (4)		Model (5)		Model (6)		Model (7)		Model (8)	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Intercept	0.51	0.02	0.48	0.02	0.47	0.02	0.54	0.03	0.52	0.02	0.49	0.02	0.47	0.02	0.55	0.03
Pro	0.29	0.04	0.40	0.04	0.50	0.05	0.46	0.05	0.27	0.05	0.38	0.05	0.48	0.06	0.44	0.06
Con	-0.45	0.03	-0.54	0.04	-0.61	0.04	-0.66	0.05	-0.47	0.04	-0.55	0.04	-0.62	0.05	-0.67	0.06
PID	0.11	0.04	0.18	0.03	0.22	0.03	0.07	0.04	0.11	0.04	0.18	0.03	0.22	0.03	0.07	0.04
Pro×PID	-0.01	0.06	-0.09	0.07	-0.13	0.08	-0.08	0.08	0.00	0.06	-0.08	0.07	-0.12	0.08	-0.08	0.08
Con×PID	0.43	0.06	0.45	0.07	0.47	0.08	0.59	0.08	0.42	0.06	0.44	0.07	0.46	0.08	0.58	0.08
Pro×Con									0.03	0.05	0.04	0.06	0.04	0.07	0.03	0.08
	Model (9)		Model (10)		Model (11)		Model (12)		Model (13)		Model (14)		Model (15)		Model (16)	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE
(Intercept)	0.54	0.05	0.56	0.05	0.55	0.04	0.53	0.06	0.54	0.05	0.56	0.05	0.55	0.04	0.54	0.06
Pro	0.36	0.09	0.43	0.10	0.49	0.12	0.47	0.12	0.34	0.09	0.41	0.10	0.48	0.12	0.44	0.12
Con	-0.26	0.09	-0.31	0.10	-0.31	0.12	-0.32	0.12	-0.29	0.09	-0.33	0.10	-0.33	0.12	-0.36	0.12
PID	0.11	0.07	0.04	0.06	0.02	0.05	0.08	0.05	0.12	0.07	0.05	0.06	0.03	0.05	0.08	0.05
Age	-0.02	0.07	-0.01	0.06	0.00	0.05	0.11	0.08	-0.01	0.07	0.00	0.06	0.00	0.05	0.12	0.08
Education	0.03	0.02	0.01	0.02	-0.01	0.02	0.00	0.07	0.03	0.02	0.01	0.02	-0.01	0.02	0.02	0.08
Male	0.02	0.05	0.02	0.04	0.02	0.04	0.03	0.03	0.02	0.05	0.02	0.04	0.02	0.04	0.03	0.03
Income	-0.03	0.04	-0.03	0.04	-0.04	0.04	0.03	0.05	-0.04	0.04	-0.04	0.04	-0.04	0.04	0.03	0.05
Black	-0.03	0.03	-0.02	0.02	-0.03	0.02	-0.05	0.05	-0.03	0.03	-0.02	0.02	-0.03	0.02	-0.06	0.05
Married	-0.02	0.04	-0.02	0.03	-0.02	0.03	-0.03	0.03	-0.02	0.04	-0.02	0.03	-0.02	0.03	-0.03	0.03
Insured	-0.11	0.05	-0.15	0.05	-0.17	0.04	-0.01	0.04	-0.11	0.05	-0.15	0.05	-0.17	0.04	-0.01	0.04
Satisfied	0.04	0.03	0.04	0.02	0.04	0.02	-0.09	0.06	0.04	0.03	0.05	0.02	0.04	0.02	-0.09	0.06
Own Health	-0.01	0.03	-0.01	0.02	0.00	0.02	0.05	0.03	-0.01	0.03	-0.01	0.02	0.00	0.02	0.05	0.03
Family Health	0.12	0.04	0.18	0.03	0.22	0.03	-0.02	0.03	0.12	0.04	0.18	0.03	0.22	0.03	-0.02	0.03
Pro×PID	-0.01	0.07	-0.07	0.07	-0.10	0.08	-0.06	0.08	0.00	0.07	-0.06	0.07	-0.09	0.09	-0.05	0.08
Pro×Age	-0.19	0.11	-0.22	0.12	-0.28	0.14	-0.26	0.14	-0.21	0.11	-0.23	0.12	-0.28	0.14	-0.28	0.14
Pro×Education	-0.01	0.10	-0.09	0.11	-0.15	0.13	-0.03	0.13	-0.03	0.10	-0.12	0.12	-0.16	0.13	-0.06	0.13
Pro×Male	-0.03	0.04	-0.01	0.04	0.02	0.05	-0.02	0.05	-0.03	0.04	-0.01	0.04	0.02	0.05	-0.02	0.05
Pro×Income	0.01	0.07	0.02	0.08	-0.01	0.10	0.03	0.10	0.02	0.07	0.03	0.08	-0.01	0.10	0.03	0.10
Pro×Black	0.01	0.07	0.01	0.08	0.02	0.09	-0.01	0.09	0.01	0.07	0.02	0.08	0.03	0.09	0.00	0.09
Pro×Married	0.01	0.04	0.01	0.05	0.02	0.06	0.02	0.06	0.01	0.04	0.01	0.05	0.02	0.06	0.02	0.06
Pro×Insured	-0.02	0.06	0.03	0.07	0.11	0.07	0.00	0.08	-0.02	0.06	0.03	0.07	0.11	0.07	0.00	0.08
Pro×Satisfied	0.08	0.09	0.14	0.10	0.17	0.11	0.18	0.11	0.07	0.09	0.13	0.10	0.16	0.11	0.17	0.11
Pro×Own Health	-0.01	0.04	0.00	0.05	-0.02	0.06	0.00	0.06	-0.01	0.04	0.00	0.05	-0.02	0.06	0.00	0.06
Pro×Family Health	-0.06	0.04	-0.07	0.05	-0.07	0.05	-0.08	0.05	-0.06	0.04	-0.08	0.05	-0.07	0.05	-0.08	0.05
Con×PID	0.37	0.06	0.37	0.07	0.36	0.08	0.49	0.08	0.36	0.06	0.36	0.07	0.35	0.08	0.47	0.08
Con×Age	-0.03	0.10	0.05	0.12	0.13	0.14	0.01	0.14	-0.03	0.10	0.05	0.12	0.13	0.14	0.00	0.14
Con×Education	0.03	0.10	0.09	0.11	0.12	0.13	0.02	0.13	0.02	0.10	0.09	0.11	0.11	0.13	0.00	0.13
Con×Male	-0.04	0.04	-0.02	0.04	-0.02	0.05	-0.05	0.05	-0.03	0.04	-0.02	0.04	-0.02	0.05	-0.05	0.05
Con×Income	-0.08	0.07	-0.09	0.08	-0.08	0.10	-0.10	0.10	-0.08	0.07	-0.09	0.08	-0.08	0.10	-0.10	0.10
Con×Black	0.07	0.08	0.08	0.10	0.11	0.12	0.12	0.10	0.08	0.08	0.08	0.10	0.11	0.12	0.13	0.10
Con×Married	0.01	0.04	-0.02	0.05	-0.03	0.06	-0.02	0.06	0.01	0.04	-0.02	0.05	-0.02	0.06	-0.01	0.06
Con×Insured	0.01	0.06	-0.07	0.07	-0.17	0.08	-0.03	0.08	0.01	0.06	-0.06	0.07	-0.17	0.08	-0.03	0.08
Con×Satisfied	-0.28	0.09	-0.27	0.10	-0.27	0.11	-0.40	0.11	-0.28	0.09	-0.27	0.10	-0.27	0.11	-0.40	0.11
Con×Own Health	-0.02	0.04	-0.06	0.05	-0.08	0.06	-0.05	0.06	-0.03	0.04	-0.06	0.05	-0.08	0.06	-0.05	0.06
Con×Family Health	0.11	0.04	0.15	0.05	0.15	0.05	0.16	0.05	0.11	0.04	0.15	0.05	0.15	0.05	0.15	0.05
Pro×Con									0.07	0.05	0.07	0.06	0.06	0.08	0.10	0.08
Cutoffs for Pro/Con	3 or more		5 or more		7 or more		NA		3 or more		5 or more		7 or more		NA	
Consideration measure			# of Arguments				Percentile				# of Arguments				Percentile	

Note. N = 2165 for Models 1 through 6. N = 2019 for Models 7 through 12. All variables, including Pro and Con, are scaled to 0-1. In Models 1, 5, 9, 13, the number of pro (or con) arguments larger than three was recoded to three so that the maximum can be three, which was then divided by three. In Models 2, 6, 10, 14 values greater than five were recoded (to five). In Models 3, 7, 11, 15 the collapsing cutoff was 7. In Models 4, 8, 12, 16, percentile values of pros and cons were used without collapsing.

Table A10: Estimates of Information Effects

	Model (1)	Model (1)	**Model (3)**	Model (4)
Point Estimate	**-0.071**	-0.068	**-0.044**	-0.041
Standard Error	**0.011**	0.011	**0.013**	0.013
95% CI (Upper bound)	**-0.050**	-0.045	**-0.019**	-0.017
90% CI (Upper bound)	**-0.053**	-0.049	**-0.023**	-0.020
90% CI (Lower bound)	**-0.090**	-0.086	**-0.066**	-0.063
95% CI (Lower bound)	**-0.093**	-0.089	**-0.070**	-0.067
Moderators	PID		PID and demographics	
Information measure	Original	Percentile	Original	Percentile

Note. Entries are the estimates of (imputed) informed opinion minus (observed) baseline. Estimates in bold are reported in the main text. The confidence intervals and standard errors are calculated using bootstrapping. Each set of analyses—i.e., regression and imputation—are replicated on 2000 bootstrap samples drawn from the original sample with replacement. Thus, the standard error and confidence interval for fully informed preference are standard deviation, and 5^{th} and 95^{th} quintiles of its values from the 2000 samples. Models 1 and 3 are reported in Figure 3 in the main text.

Table A11: Estimates of Consideration Effects

	(1)	**(2)**	(3)	(4)	Moderators
Point Estimate	0.015	**0.029**	0.038	0.031	PID
Standard Error	0.009	**0.014**	0.019	0.012	PID
95% CI (Upper bound)	0.032	**0.057**	0.076	0.053	PID
90% CI (Upper bound)	0.030	**0.053**	0.059	0.050	PID
90% CI (Lower bound)	0.0001	**0.006**	0.008	0.011	PID
95% CI (Lower bound)	-0.003	**0.001**	0.002	0.007	PID
	(5)	(6)	(7)	(8)	
Point Estimate	0.020	0.040	0.053	0.038	PID and Demographics
Standard Error	0.013	0.023	0.033	0.021	PID and Demographics
95% CI (Upper bound)	0.044	0.085	0.117	0.081	PID and Demographics
90% CI (Upper bound)	0.041	0.077	0.106	0.073	PID and Demographics
90% CI (Lower bound)	-0.001	0.004	-0.002	0.002	PID and Demographics
95% CI (Lower bound)	-0.005	-0.004	-0.012	-0.004	PID and Demographics
	(9)	**(10)**	(11)	(12)	Moderators
Point Estimate	0.027	**0.049**	0.070	0.048	PID
Standard Error	0.010	**0.015**	0.022	0.013	PID
95% CI (Upper bound)	0.047	**0.079**	0.112	0.073	PID
90% CI (Upper bound)	0.044	**0.074**	0.105	0.069	PID
90% CI (Lower bound)	0.016	**0.025**	0.036	0.027	PID
95% CI (Lower bound)	0.009	**0.020**	0.027	0.023	PID
	(13)	(14)	(15)	(16)	
Point Estimate	0.038	0.068	0.091	0.068	PID and Demographics
Standard Error	0.013	0.025	0.034	0.023	PID and Demographics
95% CI (Upper bound)	0.065	0.118	0.160	0.112	PID and Demographics
90% CI (Upper bound)	0.061	0.110	0.148	0.107	PID and Demographics
90% CI (Lower bound)	0.016	0.026	0.035	0.069	PID and Demographics
95% CI (Lower bound)	0.012	0.020	0.025	0.025	PID and Demographics
Pro×Con Term Included	Yes	Yes	Yes	Yes	
Cutoffs for collapsing	3	5	7	NA	
Consideration measure	# of arguments			Percentile	

Note. Entries are the estimates of (imputed) informed opinion minus (observed) baseline. Estimates in bold are reported in the main text. The confidence intervals and standard errors are calculated using bootstrapping. Each set of analyses—i.e., regression and imputation—are replicated on 2000 bootstrap samples drawn from the original sample with replacement. Thus, the standard error and confidence interval for fully informed preference are standard deviation, and 5^{th} and 95^{th} quintiles of its values from the 2000 samples. Estimates in bold are reported in Figure 3 of the main text.

Table A12: Deliberation Effect on Support for Universal Healthcare System

	Intend-to-treat effect				Average treat. effect on treated				Average treat. effect			
	Assign.				Assign. as IV for Attend.				Attend.			
	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Deliberation	0.03+	0.02	0.03+	0.02	0.08+	0.04	0.08+	0.04	0.04+	0.02	0.05*	0.02
Covariates												
Pros			-0.01	0.00			-0.01	0.00			-0.01	0.00
Cons			0.01	0.00			0.01	0.00			0.01	0.00
Info			0.00	0.05			-0.02	0.05			-0.01	0.05
PID			-0.04	0.03			-0.04	0.03			-0.04	0.03
Age			0.01	0.04			0.01	0.04			0.01	0.04
Education			0.04	0.04			0.03	0.04			0.03	0.04
Male			0.02	0.02			0.03	0.02			0.02	0.02
Income			-0.01	0.03			-0.01	0.03			-0.01	0.03
Black			0.03	0.03			0.03	0.03			0.03	0.03
Married			0.00	0.02			0.00	0.02			0.00	0.02
Pol. interest			-0.02	0.04			-0.02	0.04			-0.02	0.04
Participation			-0.10	0.05			-0.10	0.05			-0.10	0.05
Pol. Discussion			0.07	0.04			0.08	0.04			0.08	0.04
pol. news			0.02	0.05			0.02	0.05			0.02	0.05
HC. news			-0.05	0.04			-0.05	0.04			-0.05	0.04
Insured			-0.01	0.03			-0.01	0.03			-0.01	0.03
Own disease			-0.01	0.02			-0.01	0.02			-0.01	0.02
Family disease			0.03	0.02			0.03	0.02			0.03	0.02
HC satisfaction			0.06	0.04			0.06	0.04			0.06	0.04
Intercept	-0.06	0.01	-0.06	0.05	-0.06	0.01	-0.04	0.05	-0.04	0.01	-0.04	0.05
N	1,325		1,266		1,325		1,266		1,325		1,266	
Estimator	OLS		OLS		2SLS		2SLS		OLS		OLS	

+ $p < 0.1$ * $p < 0.05$. P-values are omitted for covariates.

Appendix B Experiment 1 (Chapter 3)

Appendix B.1. Message Wordings

Appendix B.1.1. Con Argument

Many health economists argue that Obamacare will accelerate growth in health care costs. Here's why: Obamacare requires health insurance providers to offer health insurance policies at the same price to all persons, regardless of their health conditions. This regulation will almost certainly increase medical costs for healthy people and lead some of them to exit the insurance market, causing costs to rise even more. It is extremely important to have enough healthy people in the market to control the rising health care prices, but Obamacare does just the opposite. Because of this "adverse selection" problem, the health economists think that the law will make medical care even more expensive than would have been the case otherwise, and we will see much faster growth in health spending over the next ten years.

Appendix B.1.2. Pro Argument

Many health economists argue that Obamacare will slow down growth in health care costs. Here's why: Obamacare provides several incentives for previously uninsured healthy people to join the insurance pool. Before Obamacare, medical costs were so high mainly because those buying insurance were sicker than average, which in turn led some healthy people to exit the insurance market, causing costs to rise even more. It is extremely important to have enough healthy people in the market to control the rising health care prices, and Obamacare does just that. Since Obamacare solves this “adverse selection” problem, the health economists think that the law will make medical care less expensive than would have been the case otherwise, and we will see much slower growth in health spending over the next ten years.

Appendix B.2 List of Evidence and Sources[69]

- In 2016, premiums in the Obamacare Marketplaces decrease by 0.7% nationwide, whereas the national average premium in the individual health insurance market had increased by 10 to 12% per year before the reform (Kaiser Family Foundation 2015).
- In 2016, premiums in the Obamacare Marketplace increase by 7.1% in Louisville, Kentucky (Kaiser Family Foundation and HRET 2015), whereas the national average premium in the individual health insurance market had increased by 10 to 12% per year before the reform (Gruber 2014).
- During the first two quarters after the reform, premiums in the individual health insurance market increased by 24.4% than what they would have without Obamacare across all states (Kowalski 2014).
- During the first two quarters after the reform, premiums in the individual health insurance market increased by 1.4% than what they would have without Obamacare in Maryland (Kowalski 2014).
- The latest annual growth in national out-of-pocket spending (1.3%) is lower than the Bush years (5.6% on average) (Centers for Medicare and Medicaid Services 2015; my calculation).
- The latest annual growth in spending on cosmetic surgeries (8.5%) is lower than the Bush years (9.8% on average) (American Society for Aesthetic Plastic Surgery 2015; my calculation)
- The latest annual growth in national spending on health insurance (6.2%) is higher than the year prior to the reform (3.0%) (Centers for Medicare and Medicaid Services 2015; my calculation).
- The latest annual growth in spending on cosmetic surgeries (8.5%) is higher than the year prior to the reform (7.5%) (American Society for Aesthetic Plastic Surgery 2015; my calculation).
- The average deductible for employer-sponsored health insurance has increased by 22% over the two years after the reform (Kaiser Family Foundation and HRET 2015).
- The average premium for employer-sponsored health insurance has increased by 7.3% over the two years after the reform (Kaiser Family Foundation and HRET 2015).
- The uninsured rate for non-elderly adults (ages 18 to 64) has decreased by 43%, over the two years after the reform (Department of Health and Human Services 2016).
- The uninsured rate for U.S. adults (ages 18 or older) has decreased by 1% over the preceding year (Gallup 2016).

[69] The sources shown here were not provided to the participants of Experiment 1.

- The latest annual growth in premiums for employer-sponsored health insurance (3.8% on average) is lower than the Bush years (8.8% on average) (Kaiser Family Foundation and HRET 2015).
- The latest annual growth in private sector spending on medical research (5.2% on average) is lower than the Bush years (7.2% on average) (American Society for Aesthetic Plastic Surgery 2015; my calculation).
- In 2016, premiums in the Obamacare Marketplaces increase by 10.1%, nationwide and above 30% in some states (Kaiser Family Foundation 2015).
- In 2016, premiums in the Obamacare Marketplaces increase by 4.7%, in Cheyenne, Wyoming (Kaiser Family Foundation 2015).
- Before the reform, the five U.S. states with similar regulations as Obamacare were among the states with the highest insurance premiums (Rosenbaum and Gruber 2010).
- Premiums for individuals decreased by 10.5% in Massachusetts when it implemented a health care reform similar to Obamacare, while premiums increased by 12.8% in neighboring states at the same period of time (between 2004 and 2010) (Hackmann et al. 2015).
- In a recent national survey, 52% of Americans say they are not worried about not being able to afford prescription drugs while 48% say they are worried (Kaiser Family Foundation 2016).
- Before the reform, 64% of Americans were worried about having to pay more for health care, and now 67% are worried (Kaiser Family Foundation 2016).
- Canada spends less on health care than the US.
- US national health spending accounted for 17.1% of GDP before the reform, and 17.5% after the reform (Centers for Medicare and Medicaid Services 2015; my calculation).

Appendix B.3. Survey Wordings

Attitudes toward the ACA (pre-treat)

In general, do you support or oppose the health care reform law passed in 2010? This law is called the Affordable Care Act, and sometimes referred to as Obamacare.

1) Strongly support
2) Moderately support
3) Neither support nor oppose
4) Moderately oppose
5) Strongly oppose

Conditional Probabilities was measured by the following 22 questions and was scaled 0.05-0.95 where 0.95 indicates a 95% probability that the health economists' theory turns out to be right given the presented evidence. The questions were presented in the random order.

Now let's assume that a group of independent researchers conduct a new study. Its findings may or may not be useful in refining your long-term predictions, depending on how you interpret the implications. Please indicate how you would assess the probability that the health economists' prediction (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years, if and when the new study finds each of the followings.

If the new study finds the following...

In 2016, premiums in the Obamacare Marketplaces decrease by 0.7% nationwide, the national average premium in the individual health insurance market had increased by 10 to 12% per year before the reform

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is…

1) Extremely high (95% chance)
2) Fairly high (80% chance)
3) A little high (65% chance)
4) Neither high nor low (50/50)
5) A little low (35% chance)
6) Fairly low (20% chance)
7) Extremely low (5% chance)

If the new study finds the following...

In 2016, premiums in the Obamacare Marketplaces decrease by 0.7% nationwide, the national average premium in the individual health insurance market had increased by 10 to 12% per year before the reform

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

In 2016, premiums in the Obamacare Marketplace increase by 7.1% in Louisville, Kentucky, whereas the national average premium in the individual health insurance market had increased by 10 to 12% per year before the reform

I will say probability that the health economists' theory (Obamacare accelerating cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

During the first two quarters after the reform, premiums in the individual health insurance market increased by 24.4% than what they would have without Obamacare across all states

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

During the first two quarters after the reform, premiums in the individual health insurance market increased by 1.4% than what they would have without Obamacare in Maryland

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in national out-of-pocket spending (1.3%) is lower than the Bush years (5.6% on average)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in spending on cosmetic surgeries (8.5%) is lower than the Bush years (9.8% on average)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in national spending on health insurance (6.2%) is higher than the year prior to the reform (3.0%).

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in spending on cosmetic surgeries (8.5%) is higher than the year prior to the reform (7.5%)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The average deductible for employer-sponsored health insurance has increased by 22% over the two years after the reform

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The average premium for employer-sponsored health insurance has increased by 7.3% over the two years after the reform

I will say probability that the health economists' theory (Obamacare accelerating c[slowing down] ost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The uninsured rate for non-elderly adults (ages 18 to 64) has decreased by 43%, over the two years after the reform

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The uninsured rate for U.S. adults (ages 18 or older) has decreased by 1% over the preceding year

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in premiums for employer-sponsored health insurance (3.8% on average) is lower than the Bush years (8.8% on average)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

The latest annual growth in private sector spending on medical research (5.2% on average) is lower than the Bush years (7.2% on average)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

In 2016, premiums in the Obamacare Marketplaces increase by 10.1%, nationwide and above 30% in some states

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

In 2016, premiums in the Obamacare Marketplaces increase by 4.7%, in Cheyenne, Wyoming

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

Before the reform, the five U.S. states with similar regulations as Obamacare were among the states with the highest insurance premiums

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

Premiums for individuals decreased by 10.5% in Massachusetts when it implemented a health care reform similar to Obamacare, while

premiums increased by 12.8% in neighboring states at the same period of time (between 2004 and 2010)

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

In a recent national survey, 52% of Americans say they are not worried about not being able to afford prescription drugs while 48% say they are worried

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

Before the reform, 64% of Americans were worried about having to pay more for health care, and now 67% are worried

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

Canada spends less on health care than the US

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

If the new study finds the following...

US national health spending accounted for 17.1% of GDP before the reform, and 17.5% after the reform

I will say probability that the health economists' theory (Obamacare accelerating [slowing down] cost growth) turns out to be right over the next ten years is… (same multiple choice options as above)

Message Recall

Which of the followings is consistent with the message you read above? (If you are not sure, you can mark that too)

1) Many health economists argue that Obamacare will accelerate growth in health care costs
2) Many health economists argue that Obamacare will slow down growth in health care costs
3) Not sure

Which of the following terms did the message use to explain the health economists' theory? (If you are not sure, you can mark that too)

1) The free rider problem
2) Adverse selection
3) Risk aversion
4) Not sure

Screener (Those who failed this were underweighted)

When a big news story breaks people often go online to get up-to-the-minute details on what is going on. We want to know which websites people trust to get this information. We also want to know if people are paying attention to the question. To show that you've read this much, please ignore the question and select the Drudge Report and none of the above as your two answers.

1) New York Times website (1)
2) Huffington Post (2)
3) Washington Post website (3)
4) Cnn.com (4)
5) FoxNews.com (5)
6) MSNBC.com (6)
7) The Associated Press website (7)
8) Reuters website (8)
9) New York Post online (9)
10) The Drudge Report (10)
11) Google News (11)
12) ABC News website (12)
13) CBS News website (13)
14) NBC News website (14)
15) Yahoo! News (15)
16) NPR website (16)
17) USA Today website (17)
18) None of the above (18)

Appendix B.4. Additional Information on Results from Experiment 1

In Columns 1, 3, and 5 of Table A13, I report the regression models on which Figure 4 is based. The dependent variable (conditional probability that the health economists' prediction turns out to be correct) was regressed on the position of the presented argument (between-subject factor), the dummies for each piece of evidence (22 within-subject factors), and the interactions between the argument and evidence factors.[70] Standard errors were clustered at the respondent level. The table shows the coefficients on each of these terms. As noted in the main text, those who exhibited low attention by failing the screener question (see Appendix B.3) were underweighted. Those who chose only one of the two correct options were weighted by 0.5, and those who choose none of the two were weighted by 0 (i.e., dropped).

Column 2, 4, and 6 of Table A13 repeats the same model, but this time without underweighting the low-attention respondents. As can be seen, the results are very similar across this specification choice.

[70] The reference category for the evidence factor was "Premiums in ACA marketplaces increase 10% nationwide (SC[h])."

Table A13-1: Conditional Probabilities Given Evidence

	(1)	(2)	(3)	(4)	(5)	(6)
Subset	All		Opponents		Proponents	
Pro Argument	-0.368*	-0.317*	-0.646*	-0.591*	-0.133*	-0.113*
	(0.035)	(0.033)	(0.038)	(0.044)	(0.050)	(0.044)
SC(b): Individual premiums increased 24% after ACA nationwide	-0.010	-0.012	-0.018	-0.009	0.026	0.013
	(0.022)	(0.021)	(0.028)	(0.030)	(0.034)	(0.030)
SC(e): Employee deductibles have increased 20% 2 years into ACA	-0.036*	-0.036*	-0.034	-0.018	-0.026	-0.046
	(0.021)	(0.021)	(0.027)	(0.028)	(0.036)	(0.033)
SC(d): National health spending is higher than pre-ACA	-0.043*	-0.032*	-0.063*	-0.048*	-0.023	-0.013
	(0.019)	(0.017)	(0.024)	(0.025)	(0.032)	(0.026)
SC: States with ACA provisions had highest premiums before ACA	-0.053*	-0.044*	-0.080*	-0.078*	-0.016	-0.005
	(0.022)	(0.020)	(0.036)	(0.032)	(0.031)	(0.027)
WC(e): Employee premiums have increased 7% 2 years into ACA	-0.056*	-0.044*	-0.063*	-0.048*	-0.046	-0.041
	(0.018)	(0.017)	(0.019)	(0.023)	(0.034)	(0.028)
WC: 64% were worried health cost before ACA, now 67% are worried	-0.105*	-0.089*	-0.146*	-0.129*	-0.058	-0.058
	(0.024)	(0.022)	(0.026)	(0.027)	(0.042)	(0.036)
WC(h): Premiums in ACA marketplaces increase 5% in Cheyenne	-0.140*	-0.131*	-0.143*	-0.117*	-0.138*	-0.147*
	(0.024)	(0.023)	(0.030)	(0.033)	(0.041)	(0.038)
WC: Share of health spending in GDP changed from 17.1 to 17.5	-0.154*	-0.135*	-0.137*	-0.102*	-0.168*	-0.160*
	(0.019)	(0.021)	(0.030)	(0.034)	(0.030)	(0.034)
WP(a): Premiums in ACA marketplaces increase 7% in Louisville	-0.192*	-0.162*	-0.224*	-0.192*	-0.176*	-0.145*
	(0.025)	(0.024)	(0.034)	(0.036)	(0.040)	(0.034)
WC(b): Individual premiums increased 1.4% after ACA in Maryland	-0.145*	-0.125*	-0.186*	-0.147*	-0.127*	-0.122*
	(0.022)	(0.021)	(0.032)	(0.033)	(0.038)	(0.033)
WP: 52% are not worried prescription drug cost	-0.154*	-0.130*	-0.168*	-0.132*	-0.145*	-0.132*
	(0.023)	(0.022)	(0.035)	(0.035)	(0.037)	(0.035)
WP: Canada spends less on health care than the US	-0.158*	-0.145*	-0.217*	-0.192*	-0.110*	-0.119*
	(0.025)	(0.024)	(0.028)	(0.029)	(0.046)	(0.042)
WC (d): Cosmetic surgery spending growth is higher than pre ACA	-0.179*	-0.161*	-0.224*	-0.192*	-0.150*	-0.147*
	(0.025)	(0.024)	(0.039)	(0.039)	(0.038)	(0.035)
WP(g): Research spending growth is lower than Bush years	-0.203*	-0.188*	-0.251*	-0.222*	-0.175*	-0.170*
	(0.029)	(0.026)	(0.043)	(0.041)	(0.044)	(0.039)
WP(f): Uninsured rate dropped 1% past year	-0.224*	-0.204*	-0.296*	-0.252*	-0.165*	-0.160*
	(0.027)	(0.025)	(0.041)	(0.040)	(0.039)	(0.036)
WP(c): Cosmetic surgery spending growth is lower than Bush years	-0.227*	-0.205*	-0.277*	-0.240*	-0.171*	-0.165*
	(0.025)	(0.024)	(0.038)	(0.037)	(0.039)	(0.035)
SP Premiums went down in MA after health reform	-0.202*	-0.192*	-0.242*	-0.216*	-0.190*	-0.196*
	(0.024)	(0.023)	(0.036)	(0.036)	(0.041)	(0.036)
SP(a): Premiums in ACA marketplaces decrease .7% nationwide	-0.258*	-0.230*	-0.325*	-0.288*	-0.209*	-0.188*
	(0.028)	(0.027)	(0.045)	(0.043)	(0.042)	(0.039)
SP(f): Uninsured rate 43% has dropped since ACA	-0.269*	-0.225*	-0.336*	-0.285*	-0.237*	-0.198*
	(0.028)	(0.028)	(0.046)	(0.047)	(0.042)	(0.039)
SP(c): Out of pocket spending growth is lower than Bush years	-0.302*	-0.257*	-0.360*	-0.309*	-0.241*	-0.201*
	(0.028)	(0.027)	(0.045)	(0.044)	(0.038)	(0.036)
SP(g): Employee premium growth is lower than Bush years	-0.335*	-0.298*	-0.432*	-0.375*	-0.257*	-0.239*
	(0.028)	(0.027)	(0.042)	(0.044)	(0.040)	(0.036)
Low Attention Participants Underweighted	Yes	No	Yes	No	Yes	No

Table A13-2: Conditional Probabilities Given Evidence (Continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Subset	All		Opponents		Proponents	
Pro Argument × SC(b): Individual premiums increased 24% after ACA nationwide	0.019	0.004	0.091*	0.081*	-0.072	-0.079*
	(0.031)	(0.031)	(0.047)	(0.047)	(0.046)	(0.042)
Pro Argument × SC(e): Employee deductibles have increased 20% 2 years into ACA	0.054*	0.036	0.050	0.021	0.041	0.036
	(0.030)	(0.029)	(0.040)	(0.042)	(0.048)	(0.044)
Pro Argument × SC(d): National health spending is higher than pre-ACA	0.092*	0.057*	0.122*	0.096*	0.056	0.010
	(0.027)	(0.026)	(0.038)	(0.038)	(0.043)	(0.039)
Pro Argument × SC: States with ACA provisions had highest premiums before ACA	0.096*	0.079*	0.153*	0.163*	0.021	-0.007
	(0.028)	(0.027)	(0.043)	(0.041)	(0.041)	(0.039)
Pro Argument × WC(e): Employee premiums have increased 7% 2 years into ACA	0.110*	0.084*	0.105*	0.089*	0.111*	0.074*
	(0.027)	(0.026)	(0.034)	(0.035)	(0.047)	(0.042)
Pro Argument × WC: 64% were worried health cost before ACA, now 67% are worried	0.164*	0.139*	0.217*	0.194*	0.102*	0.089*
	(0.031)	(0.029)	(0.038)	(0.040)	(0.053)	(0.047)
Pro Argument × WC(h): Premiums in ACA marketplaces increase 5% in Cheyenne	0.214*	0.207*	0.236*	0.206*	0.189*	0.207*
	(0.031)	(0.030)	(0.039)	(0.042)	(0.052)	(0.047)
Pro Argument × WC: Share of health spending in GDP changed from 17.1 to 17.5	0.235*	0.202*	0.232*	0.204*	0.237*	0.198*
	(0.027)	(0.029)	(0.037)	(0.042)	(0.044)	(0.046)
Pro Argument × WP(a): Premiums in ACA marketplaces increase 7% in Louisville	0.267*	0.232*	0.313*	0.284*	0.253*	0.212*
	(0.033)	(0.032)	(0.044)	(0.046)	(0.052)	(0.047)
Pro Argument × WC(b): Individual premiums increased 1.4% after ACA in Maryland	0.271*	0.236*	0.307*	0.266*	0.252*	0.224*
	(0.032)	(0.031)	(0.043)	(0.045)	(0.050)	(0.046)
Pro Argument × WP: 52% are not worried prescription drug cost	0.272*	0.219*	0.298*	0.255*	0.267*	0.204*
	(0.033)	(0.032)	(0.046)	(0.045)	(0.054)	(0.050)
Pro Argument × WP: Canada spends less on health care than the US	0.279*	0.248*	0.353*	0.332*	0.220*	0.193*
	(0.036)	(0.036)	(0.046)	(0.050)	(0.063)	(0.059)
Pro Argument × WC (d): Cosmetic surgery spending growth is higher than pre ACA	0.296*	0.259*	0.350*	0.315*	0.254*	0.221*
	(0.034)	(0.032)	(0.051)	(0.050)	(0.051)	(0.047)
Pro Argument × WP(g): Research spending growth is lower than Bush years	0.350*	0.319*	0.405*	0.379*	0.312*	0.277*
	(0.037)	(0.035)	(0.055)	(0.054)	(0.055)	(0.051)
Pro Argument × WP(f): Uninsured rate dropped 1% past year	0.360*	0.315*	0.423*	0.378*	0.297*	0.255*
	(0.036)	(0.034)	(0.051)	(0.050)	(0.053)	(0.049)
Pro Argument × WP(c): Cosmetic surgery spending growth is lower than Bush years	0.394*	0.355*	0.438*	0.414*	0.339*	0.294*
	(0.036)	(0.035)	(0.050)	(0.052)	(0.057)	(0.050)
Pro Argument × SP Premiums went down in MA after health reform	0.395*	0.360*	0.445*	0.417*	0.385*	0.351*
	(0.037)	(0.034)	(0.052)	(0.052)	(0.058)	(0.050)
Pro Argument × SP(a): Premiums in ACA marketplaces decrease .7% nationwide	0.464*	0.412*	0.467*	0.445*	0.454*	0.371*
	(0.042)	(0.039)	(0.063)	(0.062)	(0.062)	(0.056)
Pro Argument × SP(f): Uninsured rate 43% has dropped since ACA	0.468*	0.391*	0.545*	0.500*	0.440*	0.341*
	(0.042)	(0.040)	(0.062)	(0.062)	(0.064)	(0.058)
Pro Argument × SP(c): Out of pocket spending growth is lower than Bush years	0.526*	0.448*	0.583*	0.541*	0.467*	0.363*
	(0.039)	(0.038)	(0.060)	(0.058)	(0.055)	(0.053)
Pro Argument × SP(g): Employee premium growth is lower than Bush years	0.569*	0.499*	0.675*	0.627*	0.482*	0.401*
	(0.042)	(0.040)	(0.062)	(0.062)	(0.063)	(0.055)
Intercept	0.704*	0.680*	0.838*	0.788*	0.582*	0.589*
	(0.021)	(0.021)	(0.022)	(0.031)	(0.029)	(0.028)
N	4708	5412	1914	2068	2200	2684
Low Attention Participants Underweighted	Yes	No	Yes	No	Yes	No

Note. OLS estimates. Standard errors in parentheses are clustered at the respondent level. The reference category for the evidence factor is "Premiums in ACA marketplaces increase 10% nationwide (SC[h])." "S" indicates "Strong." "W" indicates "Weak." "P" indicates "Pro." "C" indicates "Con." Lower case letters in parentheses pair a piece of strong evidence with its weaker version. For example, SP(a) and WP(a) are a pair.

Appendix C Experiment 2 (Chapter 3)

Appendix C.1. Message Wordings

Appendix C.1.1 Strong Con

A checkup for Obamacare reveals a warning sign

Since the passage of Obamacare, health experts have been keeping close taps on how the law is changing America's health care system.

What is the prognosis, almost three years into implementation? Despite some successes, Obamacare is failing to address one of the most serious problems of America's health care system: the rising costs.

This isn't surprising to many health economists who had argued that Obamacare would accelerate growth in health care costs from the beginning.

Here's why: Obamacare requires health insurance providers to offer health insurance policies at the same price to all persons, regardless of their health conditions. This regulation would almost certainly increase medical costs for healthy people and lead some of them to exit the insurance market, causing costs to rise even more.

It is extremely important to have enough healthy people in the market to control the rising health care prices, but Obamacare does just the opposite.

Because of this "adverse selection" problem, the health economists have said that the law would make medical care even more expensive than would have been the case otherwise, and we would see much faster growth in health spending over the next ten years.

Now, there is mounting evidence that that the health economists were right.

1. Earlier this year, the Kaiser Family Foundation released an analysis showing that premiums in Obamacare Marketplaces were rising by **10.1%** on average nationwide, and above **30%** in some states.

2. A new study by a Yale economist finds that, during the first two quarters after the reform, premiums in the individual health insurance market increased by **24.4%** than what they would have without Obamacare across all states.

3. According to the 2015 annual health benefits survey by the Kaiser Family Foundation, the average deductible for employer-sponsored health insurance has increased by **22.0%** over the two years after the reform.

4. The latest national health expenditure data released by the Centers for Medicaid and Medicare Services shows that the annual growth in national spending on health insurance was **6.2%** in 2014. This was much higher than the annual growth rate before the reform (**3.0%**).

So no matter how you look at the data, the evidence is clear. Obamacare is not sustainable, and is likely to implode down the road if the current trend continues.

Then, what should be done? This question continues to frustrate many legislators and experts. What is unhelpful, though, is the unwillingness to recognize the warning signs.

Appendix C.1.2 Weak Con

A checkup for Obamacare reveals a warning sign

Since the passage of Obamacare, health experts have been keeping close taps on how the law is changing America's health care system.

What is the prognosis, almost three years into implementation? Despite some successes, Obamacare is failing to address one of the most serious problems of America's health care system: the rising costs.

This isn't surprising to many health economists who had argued that Obamacare would accelerate growth in health care costs from the beginning.

Here's why: Obamacare requires health insurance providers to offer health insurance policies at the same price to all persons, regardless of their health conditions. This regulation would almost certainly increase medical costs for healthy people and lead some of them to exit the insurance market, causing costs to rise even more.

It is extremely important to have enough healthy people in the market to control the rising health care prices, but Obamacare does just the opposite.

Because of this "adverse selection" problem, the health economists have said that the law would make medical care even more expensive than would have been the case otherwise, and we would see much faster growth in health spending over the next ten years.

Now, there is mounting evidence that that the health economists were right.

1. Earlier this year, the Kaiser Family Foundation released an analysis showing that premiums in Obamacare Marketplaces were rising by **4.7%** in Cheyenne, Wyoming.

2. A new study by a Yale economist finds that, during the first two quarters after the reform, premiums in the individual health insurance market increased by **1.4%** than what they would have without Obamacare in Maryland.

3. According to the 2015 annual health benefits survey by the Kaiser Family Foundation, the average premium for employer-sponsored health insurance has increased by **7.3%** over the two years after the reform.

4, The latest data released by the American Society for Aesthetic Plastic Surgery shows that the annual growth in spending on cosmetic surgeries was **8.5%** in 2015. This was higher than the annual growth rate before the reform (**7.5%**).

So no matter how you look at the data, the evidence is clear. Obamacare is not sustainable, and is likely to implode down the road if the current trend continues.

Then, what should be done? This question continues to frustrate many legislators and experts. What is unhelpful, though, is the unwillingness to recognize the clear warning signs.

Appendix C.1.3. Strong Pro

A checkup for Obamacare reveals a positive prognosis

Since the passage of Obamacare, health experts have been keeping close taps on how the law is changing America's health care system.

What is the prognosis, almost three years into implementation? Despite some problems, Obamacare is successfully addressing one of the most serious problems of America's health care system: the rising costs.

This isn't surprising to many health economists who had argue that Obamacare would slow down growth in health care costs from the beginning.

Here's why: Obamacare provides several incentives for previously uninsured healthy people to join the insurance pool. Before the ACA, medical costs were so high mainly because those buying insurance were sicker than average, which in turn led some healthy people to exit the insurance market, causing costs to rise even more.

It is extremely important to have enough healthy people in the market to control the rising health care prices, and Obamacare does just that.

Since Obamacare solves this "adverse selection" problem, the health economists have said that the law would make medical care less expensive than would have been the case otherwise, and we would see much slower growth in health spending over the next ten years.

Now, there is mounting evidence that that the health economists were right.

> 1. Confirming the health economists' key assumption about expanding the insurance pool, an analysis by the Department of Health and Human Services indicates that the uninsured rate for non-elderly adults has decreased by **43%**, over the two years after the reform.
>
> 2. According to a report by the Kaiser Family Foundation, the overall premium for employer-sponsored health insurance rose by **3.8%** each year since the reform. By contrast, during the Bush years, premiums increased on average by **8.8 %** each year.
>
> 3. According to the latest data released by the Centers for Medicaid and Medicare Services, the annual growth in national out-of-pocket spending was **1.3%** in 2014. Again this is lower than the annual growth rates under Bush (**5.6%** on average).

4. Earlier this year, Kaiser Family Foundation released an analysis showing premiums in the Obamacare Marketplaces decreased by **0.7%** nationwide, after accounting for tax credits. In comparison, the national individual health insurance premium had increased by **10 to 12%** per year before the reform, according to an analysis by an MIT economist.

So no matter how you look at the data, the evidence is clear. Obamacare is fixing the problem that would have made the system far worse than it is now.

Of course, the law is not perfect by any means and there should be debates about how to make more progresses. What is unhelpful, though, is the unwillingness to recognize the positive signs.

Appendix C.1.4. Weak Pro

A checkup for Obamacare reveals a positive prognosis

Since the passage of Obamacare, health experts have been keeping close taps on how the law is changing America's health care system.

What is the prognosis, almost three years into implementation? Despite some problems, Obamacare is successfully addressing one of the most serious problems of America's health care system: the rising costs.

This isn't surprising to many health economists who had argue that Obamacare would slow down growth in health care costs from the beginning.

Here's why: Obamacare provides several incentives for previously uninsured healthy people to join the insurance pool. Before Obamacare, medical costs were so high mainly because those buying insurance were sicker than average, which in turn led some healthy people to exit the insurance market, causing costs to rise even more.

It is extremely important to have enough healthy people in the market to control the rising health care prices, and Obamacare does just that.

Since Obamacare solves this "adverse selection" problem, the health economists think that the law would make medical care less expensive than would have been the case otherwise, and we would see much slower growth in health spending over the next ten years.

Now, there is mounting evidence that that the health economists were right.

> 1. Confirming the health economists' key assumption about expanding the insurance pool, a recent Gallup poll indicates that the uninsured rate for U.S. adults (ages 18 or older) has decreased by **1% point** over 2015.
>
> 2. According to the data released by the Centers for Medicaid and Medicare Services, private sector spending on medical research rose by **5.2%** since the reform. By contrast, during the Bush years, private sector spending on medical research increased on average by **7.2%** each year.
>
> 3. According to the 2015 survey by the American Society for Aesthetic Plastic Surgery, the annual growth in spending on cosmetic surgeries was **8.5%** in 2015. Again this was lower than the annual growth rates under Bush (**9.8%** on average).

4. Earlier this year, the Kaiser Family Foundation released an analysis showing premiums in the Obamacare Marketplaces increased by **7.1%** in Louisville, Kentucky. In comparison, the national individual health insurance premium had increased by **10 to 12%** per year before the reform, according to an analysis by an MIT economist.

So no matter how you look at the data, the evidence is clear. The Obamacare is fixing the problem that would have made the system far worse than it is now.

Of course, the law is not perfect by any means and there should be debates about how to make more progresses. What is unhelpful, though, is the unwillingness to recognize the positive signs.

Appendix C.1.5. Placebo

Germans Forget Postwar History Lesson on Debt Relief in Greece Crisis

It is not hard to notice hypocrisy in Germany's insistence that Athens must agree to more painful austerity before any sort of debt relief can be put on the table: The main creditor demanding that Greeks be made to pay for past profligacy benefited not so long ago from more lenient terms.

But beyond serving as a reminder of German hypocrisy, the history offers a more important lesson: These sorts of things have been dealt with successfully before. The 20th century offers a rich road map of policy failure and success addressing sovereign debt crises.

The good news is that by now economists generally understand the contours of a successful approach. "I've seen this movie so many times before," said Carmen M. Reinhart, a professor at the Kennedy School of Government at Harvard who is perhaps the world's foremost expert on sovereign debt crises. "It is very easy to get hung up on the idiosyncrasies of each individual situation and miss the recurring pattern."

The recurring, historical pattern? Major debt overhangs are only solved after deep write-downs of the debt's face value. The longer it takes for the debt to be cut, the bigger the necessary write-down will turn out to be. Nobody should understand this better than the Germans. It's not just that they benefited from the deal in 1953, which underpinned Germany's postwar economic miracle.

Twenty years earlier, Germany defaulted on its debts from World War I, after undergoing a bout of hyperinflation and economic depression that helped usher Hitler to power. It is a general lesson about the nature of debt from the World War I defaults of more than a dozen countries in the 1930s to the Brady write-downs of the early 1990s.

Both of these episodes were preceded by a decade or more of negotiations and rescheduling plans that — not unlike Greece's first bailout programs — extended the maturity of debts and lowered their interest rate. But crises ended and economies improved only after the debt was cut. A new study found sharp economic rebounds after the 1934 defaults — which cut debtors' foreign indebtedness by at least 43 percent, on average — and the Brady plan, which sliced debtors' burdens by an average of 36 percent.

The crisis exit in both episodes came only after deep face-value debt write-offs had been implemented. Softer forms of debt relief, such as maturity extensions and interest rate reductions, are not generally followed by higher economic growth or improved credit

ratings.

Yet Policy makers have yet to get this. It took a decade or more from the onset of the Latin American debt crisis to the Brady deal. Brazil alone had six debt restructurings. Similarly, the generalized defaults of 1934 followed more than a decade of failed half-measures. Does Greece have to wait that long, too?

Appendix C.2. Measurement and Survey Wordings

Appendix C.2.1. Pre-treat Covariates (Control Variables)

Attitudes toward the ACA (Wave 1) was constructed by combining the five items, and scaled to 0-1 where 1 indicates the strongest support (Cronbach's alpha = 0.96).

In general, do you support or oppose the health care reform law passed in 2010? This law is called the Affordable Care Act, and sometimes referred to as Obamacare.

1) Support
2) Oppose
3) Neither support nor oppose

Is your support [opposition] strong or not so strong?

1) Strong
2) Not so strong

Do you lean toward supporting or opposing the health care reform law, or do you not lean either way?

1) Supporting
2) Opposing
3) Do not lean either way

Would you vote to repeal the health care reform law, if you were in Congress today?

1) Definitely yes
2) Probably yes
3) Might or might not
4) Probably not
5) Definitely not
6) Not sure

On the whole, the health care reform law is changing the American health care system...

1) Definitely for the better
2) Probably for the better
3) Neither for the better nor worse
4) Probably for the worse
5) Definitely for the worse

In the long run, the overall impacts of the health care reform law on the American people will be...

1) Definitely good
2) Probably good
3) Neither good nor bad
4) Probably bad
5) Definitely bad

In the long run, the overall impacts of the health care reform law on you and your family will be...

1) Definitely good
2) Probably good
3) Neither good nor bad
4) Probably bad
5) Definitely bad

Belief that the ACA saves costs (Wave 1) was measured by combining the following three items, and scaled to run from 0 to 1 where 1 indicates the strongest belief that the ACA saves health care costs (Cronbach's alpha = 0.77). "Not sure" responses were coded 0.5.

In terms of health care costs, the reform law is changing the American health care system...

1) Definitely for the better
2) Probably for the better
3) Neither better nor worse
4) Probably for the worse
5) Definitely for the worse
6) Not sure

Based on what you have heard, the probability that the law slows down growth in health care costs over the next ten years is...

1) Extremely high (95% chance)
2) Fairly high (80% chance)
3) A little high (65% chance)
4) Neither high nor low (50/50)
5) A little low (35% chance)
6) Fairly low (20% chance)
7) Extremely low (5% chance)
8) Not sure

Based on what you have heard, the probability that the law accelerates growth in health care costs over the next ten years is...

1) Extremely high (95% chance)
2) Fairly high (80% chance)
3) A little high (65% chance)
4) Neither high nor low (50/50)

5) A little low (35% chance)
6) Fairly low (20% chance)
7) Extremely low (5% chance)
8) Not sure

Vote Choice 1 (Wave 1) was measured by the following questionnaire. When controlling for this pre-treatment covariate, I estimated the fixed effects terms of each choice, treating "The Democratic Party" as the reference category.

> If you had to choose now, for which party would you vote in the 2016 Presidential Election?
> 1) The Democratic Party (whoever wins the nomination among the current candidates)
> 2) The Republican Party (whoever wins the nomination among the current candidates)
> 3) Either Party (depending on the primary results)
> 4) Neither Party
> 5) Will not vote
> 6) Not sure

Vote Choice 2 (Wave 1) was measured by the following questionnaire. When controlling for this pre-treatment covariate, I estimated the fixed effects terms of each choice, treating "Hillary Clinton" as the reference category.

> If Hillary Clinton were the Democratic Party's candidate and Donald Trump were the Republican Party's candidate, who would you vote for?
> 1) Hillary Clinton
> 2) Donald Trump
> 3) Someone else
> 4) Will not vote
> 5) Not sure

Party Identification (Wave 1) was measured by the following questionnaire, and scaled 0-1 where 1 indicates "Strong Democrat"

> Generally speaking, do you think of yourself as a Democrat, a Republican, an Independent or something else?
> 1) Democrat
> 2) Republican
> 3) Independent
> 4) Other
>
> Would you call yourself a strong Democrat or a not very strong Democrat [Republican]?
> 1) Strong Democrat [Republican]

2) Not very strong Democrat [Republican]

Do you think of yourself as closer to the Democratic Party, the Republican Party, or neither?

1) Closer to the Democratic Party
2) Closer to the Republican Party
3) Neither

Approval of Obama (Wave 1) was measured by the following questionnaire, and scaled 0-1 where 1 indicates "strongly approve."

Do you approve or disapprove of the way Barack Obama is handling his job as President?

1) Strongly approve
2) Somewhat approve
3) Neither approve nor disapprove
4) Somewhat disapprove
5) Strongly disapprove

Approval of Obama's Handling of Healthcare (Wave 1) was measured by the following questionnaire, and scaled 0-1 where 1 indicates "strongly approve."

Do you approve or disapprove of the way Barack Obama is handling health care?

1) Strongly approve
2) Somewhat approve
3) Neither approve nor disapprove
4) Somewhat disapprove
5) Strongly disapprove

Appendix C.2.2. Dependent Variables

Attitudes toward the ACA (wave 2) was measured in the same way as the pre-treatment measurement (see above for survey wordings; Cronbach's alpha = 0.96).

Belief that the ACA saves costs (wave 2) was measured in the same way as the pre-treatment measurement (see above for survey wordings; Cronbach's alpha = 0.83).

Vote Choice (wave 2) was measured the same survey item as the pre-treatment measurement (see above the survey wordings). In the analyses presented below in this appendix, "The Democratic Party" was coded 1, and other choices were coded 0.

Approval of Obama (Wave 2) was measured in the same way as the pre-treatment measurement (see above for survey wording).

Approval of Obama's Handling of Healthcare (Wave 2) was measured in the same way as the pre-treatment measurement (see above for survey wording.

Perceived Argument Strength was measured using the following six items, and scaled to vary between 0-1 where 1 indicates the highest score (Cronbach's alpha = 0.94). Similar questions were asked for the placebo group, although their responses were not included in the analyses.

> [Treatment Groups] Please indicate how much you agree or disagree with the following statements about the argument about the 2010 health care reform law called the Affordable Care Act, sometimes referred to as Obamacare.
>
> The argument provided evidence that seems certain to me.
>
> 1) Strongly agree
> 2) Agree
> 3) Somewhat agree
> 4) Neither agree nor disagree
> 5) Somewhat disagree
> 6) Disagree
> 7) Strongly disagree
> 8) Not sure
>
> The argument provided clear evidence (same multiple choices as above).
>
> The argument provided strong evidence (same multiple choices as above).

The argument provided a convincing reason for its position (same multiple choices as above).

The argument provided an important reason for me to consider when I form my opinion about the health care reform law (same multiple choices as above).

The argument would help me form my opinion about the health care reform law (same multiple choices as above).

[Placebo Group; not part of analysis] Please indicate how much you agree or disagree with the following statements about the argument about Greece's debt crisis.

The argument provided evidence that seems certain to me.

The argument provided clear evidence.

The argument provided strong evidence.

The argument provided a convincing reason for its position.

The argument would help me form my opinion about Greece's debt crisis.

The argument put thoughts in my mind about supporting debt relief for Greece.

Appendix C.2.3. Moderating Variables

Importance of Health care issue (Wave 1) was measured by the following questionnaire. In the moderation analyses, "not at all" to "one of the five" were coded 0; and "one of the two or three" and "single most important" was coded 1.

> As compared to your feelings on other political issues, how important is health care to you?
> 1) It is the single most important political issue to me
> 2) One of the two or three most important political issues
> 3) One of the five most important political issues
> 4) Not among the issues I consider important
> 5) Not at all important

Importance of Health care cost (Wave 1) was measured by the following questionnaire. In the moderation analyses, "not at all" to "very important" were coded 0; and "single most important" was coded 1.

> Please rate the importance of each of the following considerations in evaluating the health care reform law.
> Its impacts on health care costs
> 1) Single most important
> 2) Very important
> 3) Moderately important
> 4) Slightly important
> 5) Not at all important

Political Knowledge was measured by counting the percentage of correct answers to the following questionnaires (Cronbach's alpha = 0.59). In the moderation analyses 0% to 67% were coded 0; and 68% to 100% were coded 1.

> Please answer the following yes/no questions based on what you have heard about the health care reform law. If you don't know the answer, you can mark that too.
> Did some people get their health care policies cancelled due to the new regulations implemented by the law? (Wave 1)
> 1) Yes
> 2) No
> 3) Not sure
>
> Has the number of uninsured Americans declined since the implementation of the law? (Wave 1)

1) Yes
2) No
3) Not sure

Does the law require that if a U.S. citizen does NOT have health insurance, that person will have to pay a fine on his or her federal income taxes? (Wave 1)

1) Yes
2) No
3) Not sure

Does the law allow health insurance companies to refuse to sell health insurance to U.S. citizens who have a serious medical problem? (Wave 1)

1) Yes
2) No
3) Not sure

Which one of the parties would you say is more conservative than the other at the national level? (If you don't know the answer, you can mark that too) (Wave 1)

1) Democrats (1)
2) Republicans (2)
3) Not sure (3)

Do you happen to know what job or political office is currently held by John Kerry? (If you don't know the answer, you can mark that too) (Wave 2; before treatment)

1) U.S. Senator (1)
2) U.S. Vice President (2)
3) U.S. State Secretary (3)
4) U.S Energy Secretary (4)
5) Not sure (5)

Do you happen to know what job or political office is currently held by Ernest Moniz? (If you don't know the answer, you can mark that too) (Wave 2; before treatment)

1) U.S. Senator (1)
2) U.S. Vice President (2)
3) U.S. State Secretary (3)
4) U.S Energy Secretary (4)
5) Not sure (5)

Which party has the most members in the United States House of Representatives? (If you don't know the answer, you can mark that too) (Wave 2; before treatment)

1) Democrats (1)

2) Republicans (2)
3) Not sure (3)

Who has the final responsibility to decide whether a law is Constitutional or not? (If you don't know the answer, you can mark that too) (Wave 2; before treatment)

1) President (1)
2) Congress (2)
3) Supreme Court (3)
4) Not sure (4)

How much of a majority is needed for the U.S. Senate and House to override a presidential veto? (If you don't know the answer, you can mark that too) (Wave 2; before treatment)

1) Simple majority (one more than half the votes) (1)
2) Two-thirds majority (2)
3) Three-fourths majority (3)
4) Not sure (4)

Need for cognition (wave 2; after treatment) was measured by combining the following items, and scaled 0-1 where 1 indicates highest score (0.86). In the moderation analyses 0 to 0.7 were coded 0; and 0.71 to 1 were coded 1.

Please indicate whether or not you agree with each of the following statements about you or your beliefs. (Wave 2; after treatment)

1) Strongly agree
2) Somewhat agree
3) Neither agree nor disagree
4) Somewhat disagree
5) Strongly disagree

I prefer complex to simple problems (same multiple choices as above).

Thinking is NOT my idea of fun (same multiple choices as above).

I find satisfaction in deliberating hard and for long hours (same multiple choices as above).

I like to have the responsibility of handling a situation that requires a lot of thinking (same multiple choices as above).

Income (Wave 2; after treatment) was measured by the following question. In the moderation analyses, "less than 30,000" to "49,999" were coded 0; and "50,000" or higher were coded 1.

What is your combined annual household income?

1) Less than 30,000 (1)
2) 30,000 – 39,999 (2)
3) 40,000 – 49,999 (3)
4) 50,000 – 59,999 (4)
5) 60,000 – 69,999 (5)
6) 70,000 – 79,999 (6)
7) 80,000 – 89,999 (7)
8) 90,000 – 99,999 (8)
9) 100,000 or more (9)

Age (Wave 2; after treatment) was measured by the following question. In the moderation analyses, 18 to 34 were coded 0; 35 or higher were coded 1. There was no respondent under 18.

How old are you?

1) Under 18 (1)
2) 18-25 (2)
3) 26-34 (3)
4) 35-44 (4)
5) 45-54 (5)
6) 55-64 (6)
7) 65 or older (7)

Gender (Wave 2; after treatment) was measured by the following question.

What is your gender?

1) Male (1)
2) Female (2)

Race (Wave 2; after treatment) was measured by the following question. In the moderation analyses, "White/Caucasian" was coded 0; other choices were coded 1.

What is your race?

1) White/Caucasian (1)
2) African American (2)
3) Hispanic (3)
4) Asian (4)
5) Native American (5)
6) Pacific Islander (6)

7) Multiracial (7)
8) Other (8)

Problems with medical bills (Wave 2; after treatment) was measured by the following two questions. In the moderation analyses, those who indicated "Yes" to either question were coded 1; those who indicated "No" to both were coded were coded 0. "Not sure" was treated as "No."

In the past 12 months, did you or another family member in your household have any problems paying medical bills?

1) Yes (1)
2) No (2)
3) Not sure (3)

In the past 12 months, have you or someone else in your household ever put off or postponed seeking health care you felt you needed but could not afford?

1) Yes (1)
2) No (2)
3) Not sure (3)

Insured (Wave 2; after treatment) was measured by the following question

Are you covered by any form of health insurance?

1) Yes (1)
2) No (2)

Appendix C.2.4. Message Recalls

Evidence Recall

Which of the followings is consistent with the message you read above? (If you are not sure, you can mark that too)

1) Premiums in the ACA Marketplaces rose by 10.1% on average nationwide in 2016.
2) Premiums in the ACA Marketplaces rose by 4.7% in Cheyenne, Wyoming in 2016.
3) The uninsured rate has decreased by 43%, over the two years after the reform.
4) The uninsured rate has decreased by 1% point over 2015.
5) In the 1930s, the German economy improved after debt relief
6) Not sure

Key Term Recall

Which of the following terms did the message use to explain the health economists' theory? (If you are not sure, you can mark that too)

1) The free rider problem
2) Adverse selection
3) Risk aversion
4) I din't read anything about the health economist' theory
5) Not sure

Appendix C.2.5. Screeners

Wave 1 Attention Screener (Those who failed this question were not invited to Wave 2)

> Recent research on decision making shows that choices are affected by context. 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. To show that you have read the instructions, please ignore the question below about how you are feeling and instead check Interested, Bored and none of the above option as your three choices.
>
> Please check all words that describe how you are currently feeling.
> 1) Excited
> 2) Distressed
> 3) Interested
> 4) Upset
> 5) Bored
> 6) Guilty
> 7) Scared
> 8) Hostile
> 9) Enthusiastic
> 10) None of the above

Citizenship (Wave 1; non-US citizens were not invited to Wave 2)

> Are you a U.S. citizen? (Your answer will NOT affect HIT approval)
> 1) Yes
> 2) No

Wave 2 Attention Screener (Before Treatment; those who failed this question were underweighted in the analyses)

> When a big news story breaks people often go online to get up-to-the-minute details on what is going on. We want to know which websites people trust to get this information. We also want to know if people are paying attention to the question. To show that you've read this much, please ignore the question and select the Drudge Report and the NPR website and none of the above as your three answers.
>
> When there is a big news story, which is the one news website you visit first? (Please only choose one)
> 1) New York Times website (1)

2) Huffington Post (2)
3) Washington Post website (3)
4) Cnn.com (4)
5) FoxNews.com (5)
6) MSNBC.com (6)
7) The Associated Press website (7)
8) Reuters website (8)
9) New York Post online (9)
10) The Drudge Report (10)
11) Google News (11)
12) ABC News website (12)
13) CBS News website (13)
14) NBC News website (14)
15) Yahoo! News (15)
16) NPR website (16)
17) USA Today website (17)
18) None of the above (18)

Appendix C.2.4. Exposure to the 2017 Premium Increase Story

Exposure to the Premium Increase Story (Wave 4) was measured by the following questionnaire. In the moderation analyses, "No" and "Not Sure" was coded 0; "yes" was coded 1.

> The Obama administration released the prices for Obamacare health plans. Over the past few days, have you seen or heard any news about the 2017 health insurance rates?
>
> 1) Yes
> 2) No
> 3) Not Sure

Follow-up Question

> According to the news reports, the 2017 premiums for health plans under the Affordable Care Act will…
> 1) Decrease
> 2) Stay about the same
> 3) Increase
> 4) I don't remember the specifics

Appendix C.3. Additional Information, Robustness Checks, and Additional Analyses

This section provides additional information, and robustness checks and additional analyses for the results from Experiment 2, reported in Chapter 2 of this dissertation.

Tables A14 and A15 describes the full report on the regression models reported in Panels A and B Table 8. in the main text. In Table A14, the dependent variables (belief and attitudes) are regressed on the dummies for the experimental condition, for which the control group serves as the reference, and pre-treatment covariates. Covariates include prior attitudes toward ACA, prior belief about the ACA's effect on health care costs, approval of Obama's performance as the President, approval of Obama's handling of health care, and dummies for vote choice. As noted in the main text, those who exhibited low attention by failing the screener question (see Appendix C.2.5) were underweighted. Those who chose one or two of the three correct options were weighted by 0.5, and those who choose none of the two were weighted by 0 (i.e., dropped).

In Table A15, the experiment conditions were operationalized as a single continuous variable representing a scale of evidence direction and strength (0 = strong con; 0.25 = weak con; 0.5 = control; 0.75= weak pro 1=strong pro).

Tables A16 and A17 demonstrate that the results are robust across two important model specification choices. In Table A16, the same analyses as those reported in Table 8. are repeated, but this time without underweighting those who failed to choose the tree correct options in the screener question. In Table A17, I estimate the treatment effects

without controlling for the treatment covariates. As can be seen, the results are largely similar to those reported in Table 8.

In Table A18, I report the regression models summarizing the treatment effects on perceived argument strength. Figure 7 is based on these models. Perceived argument strength scales were regressed on experimental conditions, and the same set of pre-treatment covariates as prior analyses. Also, low-attention participants were underweighted. Control group was excluded from this analysis.

Table A19 provides supplementary analyses examining the "carry-over" treatment effects on additional outcome variables, other than the key dependent variables used in the analyses reported in the main text. Specifically, I focus on vote choice in Coumns1 to 3, and overall support for Obama in Columns 4 to 6, and support for Obama's handling of healthcare in columns 7 to 9. All models included the same set of pre-treatment covariates as prior analyses; and low-attention participants were underweighted. As the table shows, the treatment effects on vote choice, and overall approval of Obama were fairly small. The coefficients on each experimental condition were not statistically significant, although correctly signed for the most part (see Columns 1 to 6 of Panel A). While the effects of the experimental conditions as a whole, operationalized as a single continuous variable, were significant for these dependent variables, the effects were modest in size. This presumably reflects the fact that the Affordable Care Act as a political issue accounts for only a part of people's evaluations of the president, and their voting decisions. In Columns 7 to 9, I report more sizable effects on approval of Obama's handling of health care, to which the contents of the arguments more relevant.

To address the concern that those with treatment effects may be weaker for those with strong prior opinions, I repeat the main analyses this time using strong proponents and opponents (Table A20-1). Strong proponents were defined as those scored 0.75 or higher in the baseline measure of attitudes toward the ACA. Strong opponents are those scored 0.25 or lower. As the table shows, I find no evidence that those with strong opinions ignore or backlash against uncongenial arguments.

In Table A20-2, I show similar treatment effects across candidate support (during the 2016 primary season), in contrast to the conventional wisdom that the supporters of a certain presidential candidate tend to ignore uncongenial facts through motivated reasoning (e.g., Ignatius 2016).

In Table A20-3, I show the treatment effects for each partisan group. Consistent with the analyses presented in the main text (Table 8), I find that the treatments had similar effects for both Republicans and Democrats, for both outcome variables. Treatment effect heterogeneity was fairly small in comparison to the overall effect sizes. And to the extent there is heterogeneity, people were influenced *more* by the arguments that challenge their party's positions.

In Table A21, I report the regression models summarizing the "long-run" effects of the treatments measured 80 days (Wave 3) and 160 days (Wave 4) after the main experiment. Figure 8 is based on these models. In these analyses, the experiment conditions were operationalized as a single continuous variable representing a scale of evidence direction and strength (0 = strong con; 0.25 = weak con; 0.5 = control; 0.75= weak pro 1=strong pro). The estimates were adjusted for covariates include baseline

attitudes toward ACA, baseline belief about the ACA's effect on health care costs, approval of Obama's performance as the President, approval of Obama's handling of health care, and dummies for vote choice. As noted in the main text, those who exhibited low attention by failing the screener question (see Appendix C.2.5) were underweighted. Low-attention participants were underweighted.

In Table A22, I report the estimated effect of the premium increase story. I regressed beliefs and attitudes measured in Wave 4 on a dummy variable indicating whether one received the news and the lagged dependent variables measured in the prior three waves. In the upper panel, I show results unadjusted for other covariates. Reported in the lower panel are estimates adjusted for other covariates including approval of Obama's performance as the President, approval of Obama's handling of health care, and dummies for vote choice. Receiving the news lowered people's belief in ACA's cost saving effect and overall support for the law, across prior attitudes and party ID—a finding that remains robust to the inclusion of additional controls.

Table A14: Full Reports on Panel A of Table 8 (Treatment Effects [Experiment 2])

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Strong Con (β_1)	-0.130*	-0.089*	-0.156*	-0.082*	-0.030*	-0.109*
	(0.014)	(0.024)	(0.017)	(0.011)	(0.017)	(0.015)
Weak Con (β_2)	-0.092*	-0.061*	-0.105*	-0.038*	-0.029*	-0.041*
	(0.014)	(0.025)	(0.016)	(0.010)	(0.015)	(0.013)
Weak Pro (β_3)	0.084*	0.088*	0.082*	0.035*	0.072*	0.013
	(0.014)	(0.024)	(0.016)	(0.010)	(0.018)	(0.012)
Strong Pro (β_4)	0.126*	0.137*	0.119*	0.060*	0.101*	0.032*
	(0.014)	(0.025)	(0.017)	(0.011)	(0.019)	(0.012)
Prior Attitudes toward ACA	0.214*	0.236*	0.340*	0.689*	0.698*	0.735*
	(0.037)	(0.086)	(0.058)	(0.033)	(0.072)	(0.051)
Prior Belief in Cost Saving Effects	0.418*	0.391*	0.421*	0.079*	0.075*	0.060*
	(0.030)	(0.049)	(0.039)	(0.024)	(0.038)	(0.030)
Party ID	0.053*	0.016	0.066*	0.032	0.025	0.049*
	(0.026)	(0.042)	(0.033)	(0.022)	(0.033)	(0.029)
Obama Support	0.042	0.031	0.026	0.042	0.104*	-0.041
	(0.028)	(0.044)	(0.034)	(0.027)	(0.040)	(0.033)
Obama Healthcare Support	-0.027	0.075	-0.041	0.065*	0.058	0.092*
	(0.035)	(0.072)	(0.038)	(0.033)	(0.063)	(0.038)
Vote Choice 1 (Republican)	0.017	-0.078*	0.066	-0.003	-0.010	-0.004
	(0.027)	(0.042)	(0.042)	(0.022)	(0.035)	(0.040)
Vote Choice 1 (Either Party)	0.036*	-0.011	0.054*	-0.003	-0.004	-0.004
	(0.020)	(0.035)	(0.025)	(0.015)	(0.028)	(0.020)
Vote Choice 1 (Someone else)	-0.010	-0.091*	0.038	-0.004	-0.012	0.008
	(0.022)	(0.040)	(0.025)	(0.018)	(0.032)	(0.024)
Vote Choice 1 (Will not vote)	-0.007	-0.077	0.007	0.007	0.008	-0.009
	(0.029)	(0.048)	(0.040)	(0.029)	(0.044)	(0.042)
Vote Choice 1 (Not sure)	0.005	-0.077*	0.053*	-0.001	-0.008	0.013
	(0.026)	(0.044)	(0.032)	(0.022)	(0.034)	(0.029)
Vote Choice 2 (Trump)	-0.015	0.078*	-0.072*	-0.023	0.003	-0.049*
	(0.021)	(0.033)	(0.027)	(0.018)	(0.029)	(0.026)
Vote Choice 2 (Someone else)	-0.004	0.057*	-0.019	-0.014	-0.007	-0.017
	(0.015)	(0.032)	(0.017)	(0.012)	(0.028)	(0.013)
Vote Choice 2 (Will not vote)	-0.020	0.072*	-0.054*	-0.036	-0.019	-0.044
	(0.023)	(0.042)	(0.030)	(0.025)	(0.038)	(0.036)
Vote Choice (Not sure)	0.013	0.064	0.006	0.007	0.031	-0.013
	(0.027)	(0.045)	(0.034)	(0.018)	(0.036)	(0.021)
Constant	0.120*	0.109*	0.036	0.074*	0.021	0.095*
	(0.027)	(0.046)	(0.045)	(0.021)	(0.034)	(0.038)
N	1412	563	829	1412	563	829

Note. OLS estimates with robust standard errors in parentheses. Low attention participants are underweighted. The reference categories for Vote Choice 1 and Vote Choice 2 are respectively "the Democratic Party" and "Hilary Clinton."

Table A15: Full Reports on Panel B of Table 8 (Treatment Effects [Experiment 2])

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Experimental Condition	0.275*	0.242*	0.295*	0.143*	0.147*	0.135*
	(0.013)	(0.023)	(0.015)	(0.011)	(0.018)	(0.013)
Prior Attitudes toward ACA	0.213*	0.242*	0.341*	0.687*	0.706*	0.731*
	(0.038)	(0.086)	(0.058)	(0.033)	(0.073)	(0.051)
Prior Belief in Cost Saving Effects	0.418*	0.391*	0.415*	0.078*	0.073*	0.056*
	(0.030)	(0.049)	(0.039)	(0.024)	(0.038)	(0.030)
Party ID	0.057*	0.014	0.072*	0.034	0.022	0.045
	(0.026)	(0.042)	(0.033)	(0.022)	(0.033)	(0.029)
Obama Support	0.039	0.028	0.024	0.041	0.101*	-0.041
	(0.028)	(0.044)	(0.034)	(0.027)	(0.040)	(0.033)
Obama Healthcare Support	-0.025	0.074	-0.038	0.066*	0.055	0.097*
	(0.035)	(0.072)	(0.038)	(0.033)	(0.063)	(0.037)
Vote Choice 1 (Republican)	0.017	-0.079*	0.061	-0.003	-0.012	-0.010
	(0.027)	(0.042)	(0.043)	(0.022)	(0.034)	(0.040)
Vote Choice 1 (Either Party)	0.037*	-0.012	0.056*	-0.002	-0.007	-0.004
	(0.020)	(0.035)	(0.025)	(0.015)	(0.028)	(0.019)
Vote Choice 1 (Someone else)	-0.007	-0.088*	0.041	-0.004	-0.010	0.005
	(0.022)	(0.040)	(0.025)	(0.018)	(0.033)	(0.024)
Vote Choice 1 (Will not vote)	-0.004	-0.078	0.012	0.008	0.005	-0.010
	(0.029)	(0.048)	(0.040)	(0.029)	(0.043)	(0.042)
Vote Choice 1 (Not sure)	0.005	-0.084*	0.056*	-0.000	-0.018	0.014
	(0.026)	(0.045)	(0.034)	(0.021)	(0.035)	(0.030)
Vote Choice 2 (Trump)	-0.015	0.079*	-0.072*	-0.023	0.004	-0.051*
	(0.021)	(0.033)	(0.027)	(0.018)	(0.029)	(0.027)
Vote Choice 2 (Someone else)	-0.004	0.058*	-0.019	-0.014	-0.006	-0.018
	(0.015)	(0.032)	(0.017)	(0.012)	(0.028)	(0.014)
Vote Choice 2 (Will not vote)	-0.021	0.075*	-0.056*	-0.037	-0.014	-0.046
	(0.022)	(0.042)	(0.029)	(0.025)	(0.037)	(0.036)
Vote Choice (Not sure)	0.012	0.064	0.002	0.007	0.031	-0.015
	(0.027)	(0.045)	(0.034)	(0.018)	(0.037)	(0.021)
Constant	-0.022	0.005	-0.126*	-0.003	-0.028	0.012
	(0.026)	(0.046)	(0.044)	(0.020)	(0.033)	(0.037)
N	1412	563	829	1412	563	829

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Low attention participants are underweighted. The reference categories for Vote Choice 1 and Vote Choice 2 are respectively "the Democratic Party" and "Hilary Clinton."

Table A16: Robustness Check on Table 8 (No Underweighting of Low-attention Participants)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponents	Supporters	All	Opponents	Supporters
Panel A						
Strong Con (β_1)	-0.123*	-0.083*	-0.152*	-0.085*	-0.039*	-0.111*
	(0.013)	(0.023)	(0.016)	(0.011)	(0.017)	(0.014)
Weak Con (β_2)	-0.090*	-0.061*	-0.104*	-0.044*	-0.027*	-0.053*
	(0.013)	(0.024)	(0.015)	(0.010)	(0.015)	(0.012)
Weak Pro (β_3)	0.084*	0.089*	0.082*	0.030*	0.066*	0.008
	(0.013)	(0.023)	(0.015)	(0.010)	(0.018)	(0.011)
Strong Pro (β_4)	0.128*	0.133*	0.123*	0.053*	0.088*	0.030*
	(0.013)	(0.023)	(0.016)	(0.010)	(0.018)	(0.011)
Panel B						
Linear Slope	0.272*	0.233*	0.295*	0.140*	0.140*	0.137*
	(0.012)	(0.021)	(0.015)	(0.010)	(0.017)	(0.013)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1514	602	890	1514	602	890

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are underweighted.

Table A17: Robustness Check on Table 8 (No Covariates)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponents	Supporters	All	Opponents	Supporters
Panel A						
Strong Con (β_1)	-0.125*	-0.111*	-0.137*	-0.072*	-0.043*	-0.097*
	(0.020)	(0.027)	(0.021)	(0.027)	(0.024)	(0.021)
Weak Con (β_2)	-0.099*	-0.110*	-0.089*	-0.042	-0.066*	-0.031
	(0.021)	(0.029)	(0.021)	(0.029)	(0.023)	(0.020)
Weak Pro (β_3)	0.066*	0.055*	0.093*	0.011	0.040	0.022
	(0.021)	(0.029)	(0.020)	(0.027)	(0.026)	(0.017)
Strong Pro (β_4)	0.110*	0.109*	0.126*	0.035	0.079*	0.028
	(0.021)	(0.029)	(0.020)	(0.028)	(0.028)	(0.018)
Panel B						
Linear Slope	0.254*	0.243*	0.284*	0.107*	0.141*	0.123*
	(0.018)	(0.026)	(0.018)	(0.024)	(0.025)	(0.018)
Covariates	No	No	No	No	No	No
N	1414	563	831	1414	563	831

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates are not. Low attention participants are underweighted.

Table A18: Perceived Argument Strength by Experimental Condition and Prior Attitudes (Experiment 2)

	(1)	(2)	(3)
Subset	All	Opponents	Supporters
Weak Con	-0.074*	-0.052*	-0.087*
	(0.020)	(0.026)	(0.024)
Weak Pro	-0.081*	-0.340*	0.097*
	(0.019)	(0.026)	(0.022)
Strong Pro	-0.004	-0.273*	0.170*
	(0.018)	(0.026)	(0.019)
Covariates	Yes	Yes	Yes
N	1137	454	666

* $p < 0.1$ (two-tailed). Figure 7 in the main text is based on these results. OLS estimates with robust standard errors in parentheses. The reference category is the Strong Con condition. Entries report the mean differences between the reference category the other conditions. Covariates include the lagged values of beliefs and attitudes about the ACA, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are not underweighted.

Table A19: Treatment Effects on Other Outcomes (Experiment 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(89)	(9)
DV	Vote Democratic			Obama Approval			Obama Healthcare Approval		
Subset	All	Opponents	Supporters	All	Opponents	Supporters	All	Opponents	Supporters
Panel A									
Strong Con (β_1)	-0.033	-0.031	-0.047	-0.010	-0.004	-0.015	-0.046*	0.005	-0.073*
	(0.023)	(0.031)	(0.032)	(0.011)	(0.021)	(0.013)	(0.014)	(0.021)	(0.018)
Weak Con (β_2)	-0.036	-0.018	-0.042	-0.008	-0.029	0.003	-0.028*	-0.027	-0.029*
	(0.024)	(0.031)	(0.033)	(0.010)	(0.018)	(0.012)	(0.012)	(0.019)	(0.016)
Weak Pro (β_3)	0.006	-0.022	0.014	0.011	0.010	0.011	0.059*	0.078*	0.045*
	(0.023)	(0.032)	(0.030)	(0.010)	(0.017)	(0.012)	(0.013)	(0.022)	(0.015)
Strong Pro (β_4)	0.009	0.010	0.016	0.008	0.003	0.009	0.057*	0.054*	0.052*
	(0.024)	(0.033)	(0.033)	(0.012)	(0.022)	(0.013)	(0.014)	(0.023)	(0.016)
Panel B									
Linear Slope	0.050*	0.032	0.074*	0.022*	0.021	0.023*	0.117*	0.084*	0.131*
	(0.022)	(0.027)	(0.031)	(0.011)	(0.020)	(0.012)	(0.013)	(0.020)	(0.016)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1514	602	890	1514	602	890	1514	602	890

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are not underweighted.

Table A20-1: Treatment Effects for Strong Supporters and Strong Opponents of the ACA

	(1)	(2)	(3)	(4)
Subset	Strong Supporters	Strong Opponents	Strong Supporters	Strong Opponents
Panel A				
Strong Con (β_1)	-0.088*	-0.143*	-0.013	-0.096*
	(0.031)	(0.020)	(0.019)	(0.017)
Weak Con (β_2)	-0.052*	-0.105*	-0.008	-0.030*
	(0.031)	(0.021)	(0.018)	(0.013)
Weak Pro (β_3)	0.087*	0.082*	0.078*	0.003
	(0.029)	(0.019)	(0.020)	(0.012)
Strong Pro (β_4)	0.145*	0.120*	0.090*	0.017
	(0.032)	(0.020)	(0.023)	(0.014)
Panel B				
Linear Slope	0.244*	0.286*	0.121*	0.105*
	(0.028)	(0.018)	(0.022)	(0.015)
Covariates	No	No	No	No
N	394	590	394	590

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates are not included. Low attention participants are underweighted. Strong proponents are defined as those scored 0.75 or higher in the baseline measure of attitudes toward the ACA. Strong opponents scored 0.25 or lower.

Table A20-2: Treatment Effects by Favorite Candidates

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Candidate	Clinton	Sanders	Trump	Clinton	Sanders	Trump
Panel A						
Strong Con (β_1)	-0.139*	-0.140*	-0.115*	-0.114*	-0.099*	-0.040
	(0.037)	(0.021)	(0.044)	(0.036)	(0.017)	(0.029)
Weak Con (β_2)	-0.137*	-0.088*	-0.036	-0.035	-0.050*	-0.017
	(0.033)	(0.022)	(0.046)	(0.028)	(0.014)	(0.030)
Weak Pro (β_3)	0.059*	0.099*	0.049	0.020	0.031*	0.050
	(0.031)	(0.020)	(0.045)	(0.022)	(0.015)	(0.034)
Strong Pro (β_4)	0.072*	0.145*	0.116*	0.027	0.062*	0.071*
	(0.034)	(0.021)	(0.043)	(0.025)	(0.015)	(0.042)
Panel B						
Linear Slope	0.251*	0.303*	0.217*	0.133*	0.162*	0.117*
	(0.034)	(0.019)	(0.039)	(0.032)	(0.017)	(0.034)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	208	631	190	208	631	190

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are underweighted. Support for the presidential candidate was measured at Wave 1.

Table A20-3: Treatment Effects by Partisan Identity

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Republicans	Democrats	All	Republicans	Democrats
Panel A						
Strong Con (β_1)	-0.130*	-0.098*	-0.153*	-0.082*	-0.028	-0.110*
	(0.014)	(0.029)	(0.017)	(0.011)	(0.018)	(0.015)
Weak Con (β_2)	-0.092*	-0.029	-0.104*	-0.038*	-0.011	-0.046*
	(0.014)	(0.031)	(0.017)	(0.010)	(0.019)	(0.012)
Weak Pro (β_3)	0.084*	0.063*	0.090*	0.035*	0.062*	0.016
	(0.014)	(0.028)	(0.016)	(0.010)	(0.021)	(0.012)
Strong Pro (β_4)	0.126*	0.125*	0.122*	0.060*	0.086*	0.043*
	(0.014)	(0.029)	(0.017)	(0.011)	(0.024)	(0.012)
Panel B						
Linear Slope	0.275*	0.216*	0.300*	0.143*	0.121*	0.148*
	(0.013)	(0.025)	(0.016)	(0.011)	(0.020)	(0.014)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1412	396	842	1412	396	842

* $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Low attention participants are underweighted.

Table A21: Long-Run Effects on Obamacare Opinions (Experiment 2)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Linear Slope (Wave 3)	0.064*	0.022	0.096*	-0.002	-0.026	0.016
	(0.016)	(0.028)	(0.019)	(0.013)	(0.022)	(0.016)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	845	344	488	845	344	488
Linear Slope (Wave 4)	0.019	-0.004	0.038*	0.002	-0.031	0.022
	(0.018)	(0.031)	(0.023)	(0.015)	(0.025)	(0.019)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	808	332	461	808	332	461

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables, party ID, evaluations of Obama (overall and healthcare), and vote choice. Support for the presidential candidate was measured at Wave 1.

Table A22-1: Effects of Exposure to the Coverage of 2017 ACA Premium Increases on Beliefs

	(1)	(2)	(3)	(4)	(5)
DV	Belief in Cost Saving Effect				
Subset	All	Opponent	Supporter	Rep	Dem
Exposure to Story	-0.043*	-0.061*	-0.031*	-0.036	-0.036*
	(0.014)	(0.023)	(0.018)	(0.028)	(0.018)
Lagged DV (W3)	Yes	Yes	Yes	Yes	Yes
Other Covariates	No	No	No	No	No
Exposure to Story	-0.036*	-0.040*	-0.038*	-0.039	-0.041*
	(0.014)	(0.023)	(0.018)	(0.028)	(0.018)
Lagged DV (W3)	Yes	Yes	Yes	Yes	Yes
Other Covariates	Yes	Yes	Yes	Yes	Yes
N	685	281	391	202	394

Note. * p < 0.1 (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables measured Wave 1 through Wave 3, party ID, evaluations of Obama (overall and healthcare), and vote choice. Support for the presidential candidate was measured at Wave 1.

Table A22-1: Effects of Exposure to the Coverage of 2017 ACA Premium Increases on Attitudes

	(1)	(2)	(3)	(4)	(5)
DV	Attitudes toward ACA				
Subset	All	Opponent	Supporter	Rep	Dem
Exposure to Story	-0.037*	-0.043*	-0.033*	-0.040*	-0.029*
	(0.011)	(0.017)	(0.014)	(0.017)	(0.015)
Lagged DV (W3)	Yes	Yes	Yes	Yes	Yes
Other Covariates	No	No	No	No	No
Exposure to Story	-0.024*	-0.010	-0.036*	-0.039*	-0.027*
	(0.011)	(0.018)	(0.014)	(0.018)	(0.015)
Lagged DV (W3)	Yes	Yes	Yes	Yes	Yes
Other Covariates	Yes	Yes	Yes	Yes	Yes
N	685	391	281	202	394

Note. * p < 0.1 (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include the lagged values of the dependent variables measured Wave 1 through Wave 3, party ID, evaluations of Obama (overall and healthcare), and vote choice. Support for the presidential candidate was measured at Wave 1.

Appendix D Experiment 3 (Chapter 3)

Appendix D.1. Message Wordings

Appendix D.1.1. Income Strong

On income inequality, Republicans have a data problem

According to The Washington Post's Philip Rucker and Dan Balz, "Economic mobility, and the feeling of many Americans that they are being shut out from the nation's prosperity, will be a defining theme of the 2016 campaign."

Republican presidential hopefuls have signaled their interest in addressing the issue of stagnating working-class incomes. "[O]nly conservative principles can solve it by removing the barriers to upward mobility," as one conservative Super PAC puts.

How, exactly? Republicans offer the familiar recipe of smaller government, significant tax cuts across the board and fewer regulations — a strategy that would help business, boost growth, create jobs and ultimately boost working-class incomes.

Democrats, of course, have different solutions — more active government, higher taxes on the rich, lower taxes on the poor, and higher minimum wages. They promise that these policies will raise incomes for the working class, which will in turn create a larger number of consumers and a more dynamic economy overall.

Voters called upon to bet on these promises often find themselves much in the dark. Americans looking for skillful economic leadership from the White House would be better served by considering the long-term economic performance of Democratic and Republican presidents.

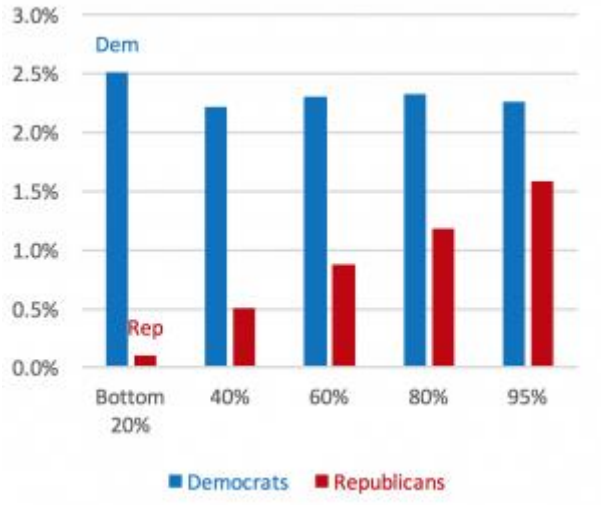
People readily grasp the diagnostic value of long-term performance in other walks of life. A judge has ruled that poker is more skill than luck; though luck may beat skill in any given hand, the better player is very likely to win in the long run. And on this front, Republicans have a data problem.

Economists and political scientists have demonstrated that, since the 1950s, low-income families have been much better off when a Democrat is president than when a Republican

is, in just about every aspect (faster income growth, lower unemployment, and so forth).

The chart below is a prime example. It shows average income growth under Democratic presidents has been higher than Republican ones across the income spectrum, but especially for families at the bottom 20%. The average incomes of these families have grown more than 24 times as fast under Democratic presidents (2.512%) as they have under Republican presidents (0.103%).

Annual income growth by income level under Democratic and Republican presidents (1949-2014)



Source: Census Bureau Historical Income Tables

Some may be tempted to attribute all this to random luck. But the odds are very slim; when a football team beats its "rival" team by wide margins, and do so again and again, over an extended period time, it becomes harder and harder to think it's just a coincidence.

More formally, a regression analysis (a standard statistical technique) indicates that there is about a 1 in 1000 chance that a difference this big would pop up simply because the Democrats got lucky, when there's nothing inherently better about their policies.

Of course, as investment advisers always say, "Past performance does not guarantee future results." Nevertheless, for low-income voters making a bet on their financial future, the past performance of Democrats and Republicans in the White House provides important evidence about how a Democratic or a Republican president would be likely to play whatever cards they are dealt over the next four years.

Appendix D.1.2. Income Weak

On income inequality, Republicans have a data problem

According to The Washington Post's Philip Rucker and Dan Balz, "Economic mobility, and the feeling of many Americans that they are being shut out from the nation's prosperity, will be a defining theme of the 2016 campaign."

Republican presidential hopefuls have signaled their interest in addressing the issue of stagnating working-class incomes. "[O]nly conservative principles can solve it by removing the barriers to upward mobility," as one conservative Super PAC puts.

How, exactly? Republicans offer the familiar recipe of smaller government, significant tax cuts across the board and fewer regulations — a strategy that would help business, boost growth, create jobs and ultimately boost working-class incomes.

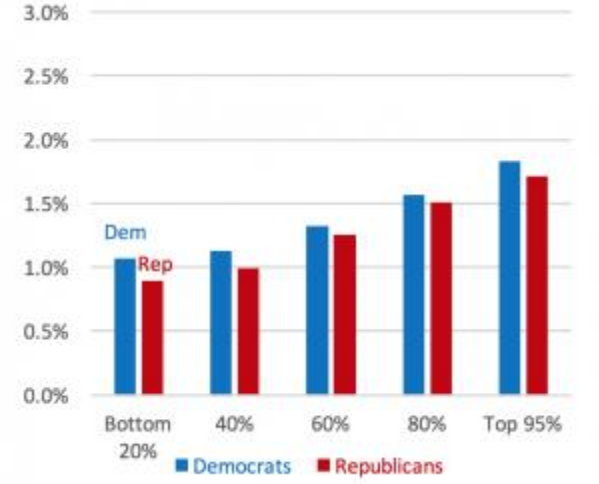
Democrats, of course, have different solutions — more active government, higher taxes on the rich, lower taxes on the poor, and higher minimum wages. They promise that these policies will raise incomes for the working class, which will in turn create a larger number of consumers and a more dynamic economy overall.

Voters called upon to bet on these promises often find themselves much in the dark. Americans looking for skillful economic leadership from the White House would be better served by considering the long-term economic performance of Democratic and Republican presidents.

People readily grasp the diagnostic value of long-term performance in other walks of life. A judge has ruled that poker is more skill than luck; though luck may beat skill in any given hand, the better player is very likely to win in the long run. And on this front, Republicans have a data problem.
2
Economists and political scientists have demonstrated that, since the 1950s, l0w-income families have been slightly better off when a Democrat is president than when a Republican is.

The chart below is a prime example. It shows average income growth under Democratic presidents has been higher than Republican ones across the income spectrum, but especially for families at the bottom 20%. The average incomes of these families have grown more than 1.2 times as fast under Democratic presidents (1.066%) as they have under Republican presidents (0.895%).

Annual income growth by income level under Democratic and Republican presidents (1953-2014)



Source: Census Bureau Historical Income Tables

Some may be tempted to attribute all this to random luck. But the odds are arguably slim. When a football team beats its "rival" team more than half the time over an extended period of time, it may no longer be a coincidence.

More formally, a regression analysis (a standard statistical technique) indicates that there is less than a 3 in 7 chance that a difference like this would pop up simply because the Democrats got lucky, when there's nothing inherently better about their policies.

Of course, as investment advisers always say, "Past performance does not guarantee future results." Nevertheless, for low-income voters making a bet on their financial future, the past performance of Democrats and Republicans in the White House provides important evidence about how a Democratic or Republican president would be likely to play whatever cards they are dealt over the next four years.

Appendix D.1.3. GDP Strong

On the economy, Republicans have a data problem

Primary season far from over. Both major parties will be doing a lot of politicking, voting, and arm-twisting between now and the conventions in July.

But, when all is said and done, the presidential race is likely to come down to the economy as usual. Republicans and Democrats will make their pitch to claim superior economic know-how over the opponent using the generic talking points many voters have heard all too much.

Republican presidential hopefuls offer the familiar recipe of smaller government, significant tax cuts across the board and fewer regulations — a strategy that would help business, boost growth, and create jobs.

Democrats, of course, have different solutions —more active government, higher taxes on the rich, lower taxes on the poor, and higher minimum wages. They promise that these policies will strengthen the middle class and create a more dynamic economy overall.

Voters called upon to bet on these promises often find themselves much in the dark. Americans looking for skillful economic leadership from the White House would be better served by considering the long-term economic performance of Democratic and Republican presidents.

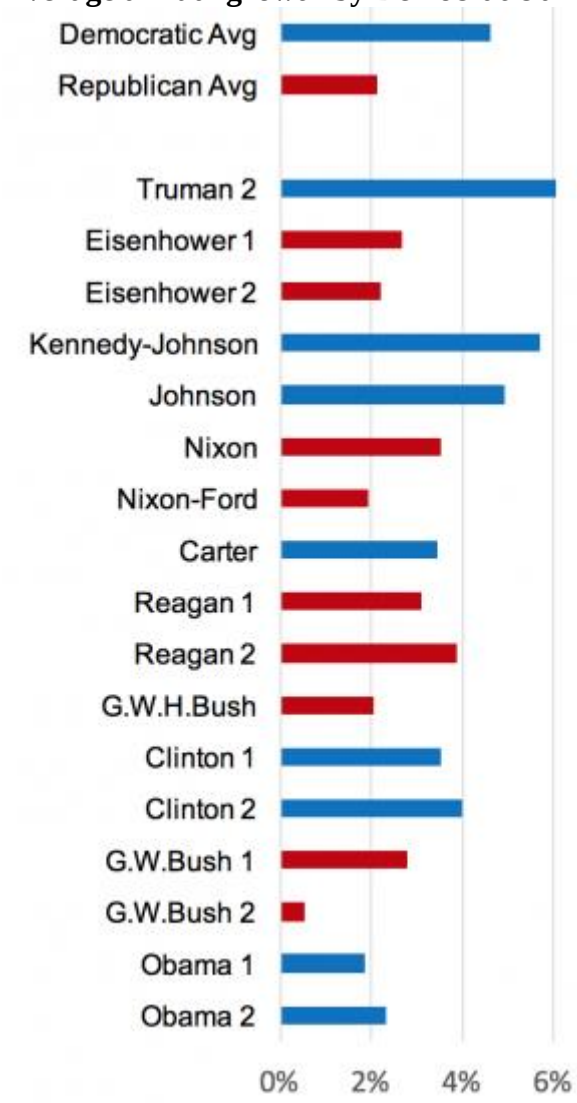
People readily grasp the diagnostic value of long-term performance in other walks of life. A judge has ruled that poker is more skill than luck; though luck may beat skill in any given hand, the better player is very likely to win in the long run. And on this front, Republicans have a data problem.

Economists and political scientists have demonstrated that, since the 1950s, the U.S. economy has performed much better when a Democrat is president than when a Republican is in just about every aspect (faster growth, lower unemployment, higher stock market returns and so forth).

The chart below is a prime example. It shows that real GDP has grown twice as fast under Democrats (4.624%) as it has under Republicans (2.123%), over a span of six decades. It is clear at a glance that GDP growth rises when Democrats get elected and falls when Republicans do, across all 8 cases of party changes in the White House. There

are no exceptions.

Average annual growth by Democratic and Republican terms (1949-2015)



Source: National Bureau of Economic Research

Some may be tempted to attribute all this to random luck. But the odds are very slim; just like winning 8 hands in a row in a one-on-one poker game is a very unlikely thing to happen, unless the winning player has better skills.

More formally, a regression analysis (a standard statistical technique) indicates that there is less than 1 in 1000 chance that a difference like this would pop up, simply because Democrats got lucky, when there's nothing inherently better about their policies.

Of course, as investment advisers always say, “Past performance does not guarantee future results.” Nevertheless, for voters making a bet on America’s economic future, the past performance of Democrats and Republicans in the White House provides important evidence about how a Democratic or Republican president would be likely to play whatever cards they are dealt over the next four years.

Appendix D.1.3. GDP Weak

On the economy, Republicans have a data problem

Primary season is far from over. Both major parties will be doing a lot of politicking, voting, and arm-twisting between now and the conventions in July.

But, when all is said and done, the presidential race is likely to come down to the economy as usual. Republicans and Democrats will make their pitch to claim superior economic know-how over the opponent using the generic talking points many voters have heard all too much.

Republican presidential hopefuls offer the familiar recipe of smaller government, significant tax cuts across the board and fewer regulations — a strategy that would help business, boost growth, and create jobs.

Democrats, of course, have different solutions —more active government, higher taxes on the rich, lower taxes on the poor, and higher minimum wages. They promise that these policies will strengthen the middle class and create a more dynamic economy overall.

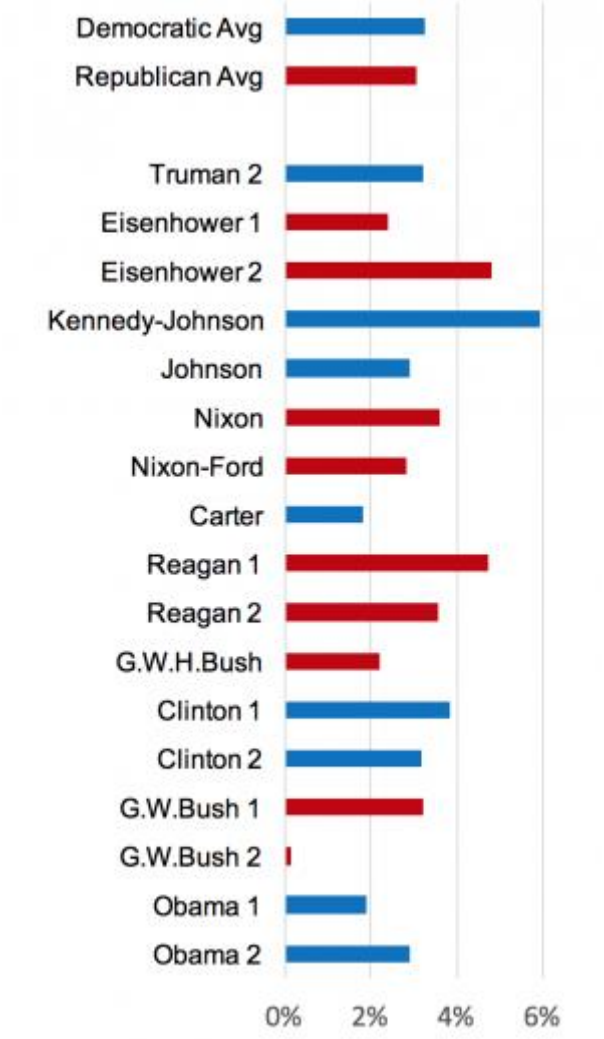
Voters called upon to bet on these promises often find themselves much in the dark. Americans looking for skillful economic leadership from the White House would be better served by considering the long-term economic performance of Democratic and Republican presidents.

People readily grasp the diagnostic value of long-term performance in other walks of life. A judge has ruled that poker is more skill than luck; though luck may beat skill in any given hand, the better player is very likely to win in the long run. And on this front, Republicans have a data problem.

Economists and political scientists have demonstrated that, since the 1950s, the U.S. economy has performed slightly better when a Democrat is president than when a Republican is.

The chart below is a prime example. It shows that real GDP has grown 1.06 times as fast under Democrats (3.233%) as it has under Republicans (3.043%), over a span of six decades. It is may be noticeable at a glance that GDP growth usually rises when Democrats get elected (with the exception of Carter) and falls when Republicans do (with the exceptions of Nixon and Regan). Across all 8 cases of party changes in the White House there are are only 3 exceptions.

Average annual GDP growth by Democratic and terms (1951-2015)



Source: National Bureau of Economic Research

Some may be tempted to attribute all this to random luck. But the odds are arguably slim; just like winning 5 hands out of 8 in a one-on-one poker game could be an unlikely thing to happen, unless the winning player has better skills.

More formally, a regression analysis (a standard statistical technique) indicates that there is about 2 in 5 chance that a difference like this would pop up, simply because Democrats got lucky, when there's nothing inherently better about their policies.

Of course, as investment advisers always say, "Past performance does not guarantee future results." Nevertheless, for voters making a bet on America's economic future, the past performance of Democrats and Republicans in the White House provides important evidence about how a Democratic or Republican president would be likely to play whatever cards they are dealt over the next four years.

Appendix D.1.5. Placebo

Germans Forget Postwar History Lesson on Debt Relief in Greece Crisis

It is not hard to notice hypocrisy in Germany's insistence that Athens must agree to more painful austerity before any sort of debt relief can be put on the table: The main creditor demanding that Greeks be made to pay for past profligacy benefited not so long ago from more lenient terms.

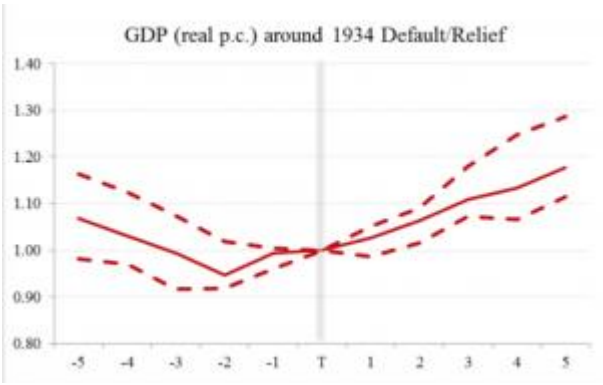
But beyond serving as a reminder of German hypocrisy, the history offers a more important lesson: These sorts of things have been dealt with successfully before. The 20th century offers a rich road map of policy failure and success addressing sovereign debt crises.

The good news is that by now economists generally understand the contours of a successful approach. "I've seen this movie so many times before," said Carmen M. Reinhart, a professor at the Kennedy School of Government at Harvard who is perhaps the world's foremost expert on sovereign debt crises. "It is very easy to get hung up on the idiosyncrasies of each individual situation and miss the recurring pattern."

The recurring, historical pattern? Major debt overhangs are only solved after deep write-downs of the debt's face value. The longer it takes for the debt to be cut, the bigger the necessary write-down will turn out to be. Nobody should understand this better than the Germans. It's not just that they benefited from the deal in 1953, which underpinned Germany's postwar economic miracle.

Twenty years earlier, Germany defaulted on its debts from World War I, after undergoing a bout of hyperinflation and economic depression that helped usher Hitler to power. It is a general lesson about the nature of debt from the World War I defaults of more than a dozen countries in the 1930s to the Brady write-downs of the early 1990s.

Both of these episodes were preceded by a decade or more of negotiations and rescheduling plans that — not unlike Greece's first bailout programs — extended the maturity of debts and lowered their interest rate. But crises ended and economies improved only after the debt was cut. The chart below shows sharp economic rebounds after the 1934 defaults — which cut debtors' foreign indebtedness by at least 43 percent, on average — and the Brady plan, which sliced debtors' burdens by an average of 36 percent.



The crisis exit in both episodes came only after deep face-value debt write-offs had been implemented. Softer forms of debt relief, such as maturity extensions and interest rate reductions, are not generally followed by higher economic growth or improved credit ratings.

Yet Policy makers have yet to get this. It took a decade or more from the onset of the Latin American debt crisis to the Brady deal. Brazil alone had six debt restructurings. Similarly, the generalized defaults of 1934 followed more than a decade of failed half-measures. Does Greece have to wait that long, too?

Appendix D.2. Measurement and Survey Wordings

Appendix D.2.1. Pre-treatment Covariates (Control Variables)

Retrospective Evaluation (National Economy) was measured using the following questionnaire, and was rescaled to 0-1, where 1 indicates "Definitely Democrats."

> Over the past few decades, which party do you think has done a better job of handling the national economy?
> 1) Definitely Democrats
> 2) Probably Democrats
> 3) Not much difference between them
> 4) Probably Republicans
> 5) Definitely Republicans
> 6) Not sure

Retrospective Evaluation (Income Inequality) was measured using the following questionnaire, and was rescaled to 0-1, where 1 indicates "Definitely Democrats."

> Over the past few decades, which party do you think has done a better job of handling income inequality?
> 1) Definitely Democrats
> 2) Probably Democrats
> 3) Not much difference between them
> 4) Probably Republicans
> 5) Definitely Republicans
> 6) Not sure

Retrospective Evaluation (Pocketbook) was measured using the following questionnaire, and was rescaled to 0-1, where 1 indicates "Definitely Democrats Presidents."

> Over the past few decades, people like you have been better off under...
> 1) Definitely Democratic Presidents
> 2) Probably Democratic Presidents
> 3) Not much difference between them
> 4) Probably Republican Presidents
> 5) Definitely Republican Presidents
> 6) Not sure

Party Identification was measured by the following questionnaire, and scaled 0-1 where 1 indicates "Strong Democrat"

Generally speaking, do you think of yourself as a Democrat, a Republican, an Independent or something else?

1) Democrat
2) Republican
3) Independent
4) Other

Would you call yourself a strong Democrat or a not very strong Democrat [Republican]?

1) Strong Democrat [Republican]
2) Not very strong Democrat [Republican]

Do you think of yourself as closer to the Democratic Party, the Republican Party, or neither?

1) Closer to the Democratic Party
2) Closer to the Republican Party
3) Neither

Approval of Obama was measured by the following questionnaire, and scaled 0-1 where 1 indicates "strongly approve."

Do you approve or disapprove of the way Barack Obama is handling his job as President?

1) Strongly approve
2) Somewhat approve
3) Neither approve nor disapprove
4) Somewhat disapprove
5) Strongly disapprove

Approval of Obama's Handling of Economy was measured by the following questionnaire, and scaled 0-1 where 1 indicates "strongly approve."

Do you approve or disapprove of the way Barack Obama is handling the national economy?

1) Strongly approve
2) Somewhat approve
3) Neither approve nor disapprove
4) Somewhat disapprove
5) Strongly disapprove

Approval of Obama's Handling of Inequality was measured by the following questionnaire, and scaled 0-1 where 1 indicates "strongly approve."

Do you approve or disapprove of the way Barack Obama is handling income inequality?

1) Strongly approve
2) Somewhat approve
3) Neither approve nor disapprove
4) Somewhat disapprove
5) Strongly disapprove

Favorite Candidate was measured using the following question, and coded into five categories (Clinton; Sanders; Trump; Cruz; Kasich; all else). When controlling for this pre-treatment covariate, I estimated the fixed effects terms of each category, treating "all else" as the reference category.

In the current election cycle, which presidential candidate do you support the most?

1) Hilary Clinton
2) Bernie Sanders
3) Donald Trump
4) Ted Cruz
5) John Kasich
6) Someone else
7) None of them
8) Not sure

2012 Vote Choice was measured using the following question, and coded into three categories (Romney; Obama; all else). When controlling for this pre-treatment covariate, I estimated the fixed effects terms of each category, treating "all else" as the reference category.

For which candidate did you vote in the 2012 Presidential Election?

1) Barack Obama
2) Mitt Romney
3) Someone else
4) Didn't vote
5) Not sure

Appendix D.2.2. Dependent Variables

Retrospective Evaluation was measured based on the following three items, and rescaled to 0-1 where 1 indicates the pro-Democratic side.

> Over the past few decades, which party do you think has done a better job of handling the national economy?
> 1) Definitely Democrats
> 2) Probably Democrats
> 3) Not much difference between them
> 4) Probably Republicans
> 5) Definitely Republicans
> 6) Not sure
>
> Over the past few decades, which party do you think has done a better job of handling income inequality?
> 1) Definitely Democrats
> 2) Probably Democrats
> 3) Not much difference between them
> 4) Probably Republicans
> 5) Definitely Republicans
> 6) Not sure
>
> Over the past few decades, people like you have been better off under...
> 1) Definitely Democratic Presidents
> 2) Probably Democratic Presidents
> 3) Not much difference between them
> 4) Probably Republican Presidents
> 5) Definitely Republican Presidents
> 6) Not sure

Retrospective Evaluation was measured based on the following twelve items, and rescaled to 0-1 where 1 indicates the pro-Democratic side.

> Which party do you think will do a better job of handling the national economy over the next four years?
> 1) Definitely Democrats
> 2) Probably Democrats
> 3) Not much difference between them
> 4) Probably Republicans
> 5) Definitely Republicans
> 6) Not sure

Do you expect the national economy to get better, stay about the same, or get worse than now, if a Democratic candidate wins the 2016 Presidential Election?

1) Definitely better
2) Probably better
3) About the same
4) Probably worse
5) Definitely worse
6) Not sure

Do you expect the national economy to get better, stay about the same, or get worse than now, if a Republican candidate wins the 2016 Presidential Election?

1) Definitely better
2) Probably better
3) About the same
4) Probably worse
5) Definitely worse
6) Not sure

Between Democratic and Republican economic policies, which do you think will generate faster economic growth?

1) Definitely Democratic
2) Probably Democratic
3) Not much difference between them
4) Probably Republican
5) Definitely Republican
6) Not sure

Which party do you think will do a better job of handling income inequality over the next four years?

1) Definitely Democrats
2) Probably Democrats
3) Not much difference between them
4) Probably Republicans
5) Definitely Republicans
6) Not sure

Do you expect income distribution to become more equal, or less equal than now, if a Democratic candidate wins the 2016 Presidential Election?

1) Definitely more equal
2) Probably more equal
3) About the same
4) Probably less equal
5) Definitely less equal
6) Not sure

Do you expect income distribution to become more equal, or less equal than now, if a Republican candidate wins the 2016 Presidential Election?

1) Definitely more equal
2) Probably more equal
3) About the same
4) Probably less equal
5) Definitely less equal
6) Not sure

Between Democratic and Republican economic policies, which do you think will reduce income inequality more?

1) Definitely Democratic
2) Probably Democratic
3) Not much difference between them
4) Probably Republican
5) Definitely Republican
6) Not sure

Thinking about the next President, people like you will be better off financially under...

1) Definitely a Democrat
2) Probably a Democrat
3) Not much difference between them
4) Probably a Republican
5) Definitely a Republican
6) Not sure

Do you expect the total income of your household will go up or down, if a Democratic candidate wins the presidential election?

1) Definitely up
2) Probably up
3) About the same
4) Probably down
5) Definitely down
6) Not sure

Do you expect the total income of your household will go up or down, if a Republican candidate wins the presidential election?

1) Definitely up
2) Probably up
3) About the same
4) Probably down
5) Definitely down

6) Not sure

Between Democratic and Republican economic policies, which do you think will make the total income of your household grow faster?

1) Definitely Democratic
2) Probably Democratic
3) Not much difference between them
4) Probably Republican
5) Definitely Republican
6) Not sure

Appendix D.2.3. Moderating Variables

Importance of the National Economy was measured by the following questionnaire. In the moderation analyses, "not at all" to "one of the five" were coded 0; and "one of the two or three" and "single most important" was coded 1.

> As compared to your feelings on other political issues, how important is the national economy to you?
> 1) It is the single most important political issue to me
> 2) One of the two or three most important political issues
> 3) One of the five most important political issues
> 4) Not among the issues I consider important
> 5) Not at all important

Importance of the Income Inequality was measured by the following questionnaire. In the moderation analyses, "not at all" to "one of the five" were coded 0; and "one of the two or three" and "single most important" was coded 1.

> As compared to your feelings on other political issues, how important is income inequality to you?
> 1) It is the single most important political issue to me
> 2) One of the two or three most important political issues
> 3) One of the five most important political issues
> 4) Not among the issues I consider important
> 5) Not at all important

Political Knowledge was measured by counting the percentage of correct answers to the following questionnaires (Cronbach's alpha = 0.67). In the moderation analyses 0% to 80% were coded 0; and 100% was coded 1.

> Which one of the parties would you say is more conservative than the other at the national level? (If you don't know the answer, you can mark that too)
> 1) Democrats (1)
> 2) Republicans (2)
> 3) Not sure (3)
>
> Do you happen to know what job or political office is currently held by John Kerry? (If you don't know the answer, you can mark that too)
> 1) U.S. Senator (1)
> 2) U.S. Vice President (2)
> 3) U.S. State Secretary (3)
> 4) U.S Energy Secretary (4)

5) Not sure (5)

Which party has the most members in the United States House of Representatives? (If you don't know the answer, you can mark that too)

1) Democrats (1)
2) Republicans (2)
3) Not sure (3)

Who has the final responsibility to decide whether a law is Constitutional or not? (If you don't know the answer, you can mark that too)

1) President (1)
2) Congress (2)
3) Supreme Court (3)
4) Not sure (4)

How much of a majority is needed for the U.S. Senate and House to override a presidential veto? (If you don't know the answer, you can mark that too)

1) Simple majority (one more than half the votes) (1)
2) Two-thirds majority (2)
3) Three-fourths majority (3)
4) Not sure (4)

Need for cognition (wave 2; after treatment) was measured by combining the following items, and scaled 0-1 where 1 indicates highest score (0.86). In the moderation analyses 0 to 0.7 were coded 0; and 0.71 to 1 were coded 1.

Please indicate whether or not you agree with each of the following statements about you or your beliefs. (Wave 2; after treatment)

1) Strongly agree
2) Somewhat agree
3) Neither agree nor disagree
4) Somewhat disagree
5) Strongly disagree

I prefer complex to simple problems (same multiple choices as above).

Thinking is NOT my idea of fun (same multiple choices as above).

I find satisfaction in deliberating hard and for long hours (same multiple choices as above).

I like to have the responsibility of handling a situation that requires a lot of thinking (same multiple choices as above).

Income (Wave 2; after treatment) was measured by the following question. In the moderation analyses, "less than 30,000" to "49,999" were coded 0; and "50,000" or higher were coded 1.

What is your combined annual household income?

1) Less than 30,000 (1)
2) 30,000 – 39,999 (2)
3) 40,000 – 49,999 (3)
4) 50,000 – 59,999 (4)
5) 60,000 – 69,999 (5)
6) 70,000 – 79,999 (6)
7) 80,000 – 89,999 (7)
8) 90,000 – 99,999 (8)
9) 100,000 or more (9)

Age (Wave 2; after treatment) was measured by the following question. In the moderation analyses, 18 to 34 were coded 0; 35 or higher were coded 1. There was no respondent under 18.

How old are you?

1) Under 18 (1)
2) 18-25 (2)
3) 26-34 (3)
4) 35-44 (4)
5) 45-54 (5)
6) 55-64 (6)
7) 65 or older (7)

Gender (Wave 2; after treatment) was measured by the following question.

What is your gender?

1) Male (1)
2) Female (2)

Race (Wave 2; after treatment) was measured by the following question. In the moderation analyses, "White/Caucasian" was coded 0; other choices were coded 1.

What is your race?

1) White/Caucasian
2) African American
3) Hispanic
4) Asian
5) Native American

6) Pacific Islander
7) Multiracial
8) Other

Appendix D.2.4. Message Recalls

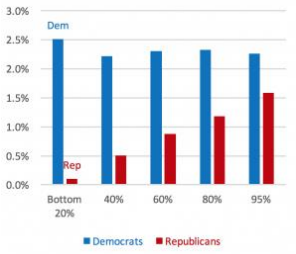
Argument Recall

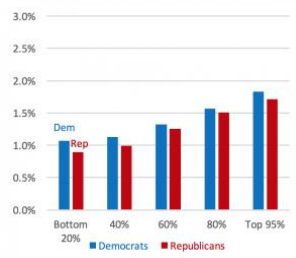
Thinking about the message you read and evaluated above, which of the following did it say?

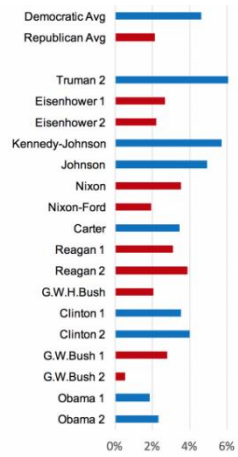
1) Working class incomes have grown faster under Democrats than Republicans.
2) GDP has grown faster under Democrats than Republicans.
3) In the 1930s, the German economy improved after debt relief.
4) I didn't read a message that says any of the above.
5) Not sure

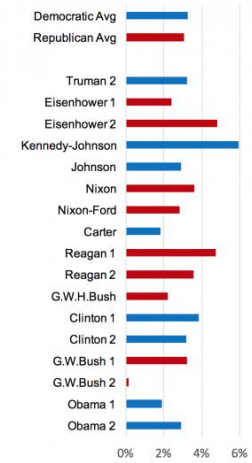
Chart Recall

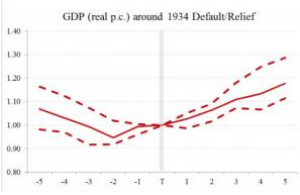
Which of the following charts did you see in the message above?

1) 

2) 

3) 

4) 

5) 

6) I didn't see any of these charts.
7) Not sure

Appendix D.2.5. Screener

Attention Screener (Before Treatment). Those who exhibited low attention by failing this screening test were underweighted. Those who chose one or two of the three correct options were weighted by 0.5, and those who choose none of the two were weighted by 0 (i.e., dropped).

> When a big news story breaks people often go online to get up-to-the-minute details on what is going on. We want to know which websites people trust to get this information. We also want to know if people are paying attention to the question. To show that you've read this much, please ignore the question and select the Drudge Report and the NPR website and none of the above as your three answers.
>
> When there is a big news story, which is the one news website you visit first? (Please only choose one)
>
> 1) New York Times website (1)
> 2) Huffington Post (2)
> 3) Washington Post website (3)
> 4) Cnn.com (4)
> 5) FoxNews.com (5)
> 6) MSNBC.com (6)
> 7) The Associated Press website (7)
> 8) Reuters website (8)
> 9) New York Post online (9)
> 10) The Drudge Report (10)
> 11) Google News (11)
> 12) ABC News website (12)
> 13) CBS News website (13)
> 14) NBC News website (14)
> 15) Yahoo! News (15)
> 16) NPR website (16)
> 17) USA Today website (17)
> 18) None of the above (18)

Appendix D.3. Replications of Bartels (2008) and Blinder and Watson (2016) for the Economy Arguments

Table A23-1: Replications of Bartels (2008)

	(1) Strong	(2) Weak
Income: 20%	0.024*	0.003
	(0.008)	(0.007)
Income: 40%	0.004*	0.002
	(0.002)	(0.006)
Income: 60%	0.008*	0.004
	(0.003)	(0.006)
Income: 80%	0.011*	0.007
	(0.003)	(0.006)
Income: Top 95%	0.015*	0.009
	(0.005)	(0.006)
Democratic President × Income: 40%	-0.007	-0.002
	(0.004)	(0.009)
Democratic President × Income: 60%	-0.010*	-0.002
	(0.005)	(0.009)
Democratic President × Income: 80%	-0.013*	-0.002
	(0.006)	(0.009)
Democratic President × Income: Top 95%	-0.017*	-0.001
	(0.008)	(0.009)
Year (linear)	-0.001*	-0.001*
	(0.000)	(0.000)
DV (1 Year Lag)	0.038	0.099*
	(0.125)	(0.058)
DV (2 Year Lag)	-0.202*	-0.209*
	(0.088)	(0.059)
House Majority		-0.076*
		(0.033)
Senate Majority		0.054*
		(0.032)
Intercept	0.028*	0.048*
	(0.009)	(0.018)
Year	1949-2014	1953-2014
Clustered Standard Errors (by Year)	Yes	No
Presumed Lag between Election and Responsibility	No	1 year
N	330	310

Note. * $p < 0.1$ (two-tailed). OLS estimates. Column 1 is close, although not identical, to Bartels' (2008) analyses. Column 2 is an intentionally bad reproduction of Bartels' (2008) results that makes the evidence seem weaker than the closer replication. Figures in Appendices D.1.1, and D.1.2 are based on these models. The data is from U.S. Census Bureau's Income Limits for Each Fifth and Top 5 Percent of Families (All Races): 1947 to 2014.[71]

[71] https://www2.census.gov/programs-surveys/cps/tables/time-series/historical-income-fa

Table A23-2: Replications of Blinder and Watson (2016)

	(1) Strong	(2) Weak
Democratic President	0.025*	0.002
	(0.007)	(0.007)
Year	-0.004*	
	(0.002)	
$Year^2$	0.000*	
	(0.000)	
$Year^3$	-0.000*	
	(0.000)	
DV (1 year lag)	-0.194	
	(0.121)	
DV (2 year lag)	-0.243*	
	(0.117)	
DV (2 ear lag)	-0.183	
	(0.115)	
Intercept	0.083*	0.030*
	(0.021)	(0.004)
Presumed Lag between Election and Responsibility	3 Months	15 Months
N	65	64

Note. * $p < 0.1$ (two-tailed). OLS estimates. Column 1 is close, although not identical, to Blinder and Watson's (2016) analyses. Column 2 is an intentionally bad reproduction of Blinder and Watson (2016) that makes the evidence seem weaker than the closer replication. The bars representing Democratic and Republican averages in Figure 10 in the main text (and the figures in Appendices D.1.3, and D.1.4) are based on these models. The bars associated with each presidential term also use 3 months and 15 months as the presumed lags between the election of a new president, and the time at which GDP growth is attributed to the newly elected president's responsibility. The data are from U.S. Bureau of Economic Analysis' Current-Dollar and "Real" Gross Domestic Product.[72]

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milies/f01ar.xls.

[72] https://www.bea.gov/national/xls/gdplev.xls.

Appendix D.4. Additional Information, Robustness Checks, and Additional Analyses

This section provides additional information, and robustness checks and additional analyses for the results from Experiment 3, reported in Chapter 2 of this dissertation.

Tables A24 and A25 describes the full report on the regression models reported in Panels A and B Table 11. in the main text. In Table A24, the dependent variables (retrospective and prospective economic evaluations) are regressed on the dummies for the experimental condition, for which the control group serves as the reference, and pre-treatment covariates. Covariates include prior retrospective evaluations, approval of Obama's performance as the President, approval of Obama's handling of the economy, approval of Obama's handling of the income inequality, and dummies for vote choice and favorite candidates. As noted in the main text, those who exhibited low attention by failing the screener question (see Appendix D.2.4) were underweighted. Those who chose one or two of the three correct options were weighted by 0.5, and those who choose none of the two were weighted by 0 (i.e., dropped).

In Table A25, the experiment conditions were operationalized as a single continuous variable representing a scale of evidence strength (0 = control; 0.5= weak 1= pro).

Tables A26 and A27 demonstrate that the results are robust across two important model specification choices. In Table A26, the same analyses as those reported in Table 11. are repeated, but this time without underweighting those who failed to choose the tree

correct options in the screener question. In Table A27, I estimate the treatment effects without controlling for the treatment covariates. As can be seen, the results are largely similar to those reported in Table 11.

Tables A28 and A29 provide supplementary analyses examining the "carry-over" treatment effects on additional outcome variables, other than the key dependent variables used in the analyses reported in the main text. Specifically, I focus on vote choice in Table A28, and preferences for redistributive fiscal policies that are often associated with the Democratic Party. All models included the same set of pre-treatment covariates as prior analyses; and low-attention participants were underweighted.

As Table A28 shows, the treatment effects on vote choice are statistically significant, and larger than those observed in the ACA experiment. The argument with weak evidence increased the intention to vote for the Democratic Party by 5.9 percentage points on average, and the argument with stronger evidence by 7.6 points. Interestingly, I find no evidence that the arguments changed Republicans' voting intentions, despite the fact that their economic evaluations were affected by the arguments. Presumably, this reflects the fact that convincing people to vote for the out-party is more difficult than changing their beliefs about policies. As shown in Table A29, I find little evidence that the treatments changed people's opinions about redistributive policies. Apparently, people did not attribute the Democratic edge in the economic performance to these policy options.

In the main text (Table 11), I reported the findings based on my preferred specification that collapsed experimental conditions into three categories. In Table A30, I

offer the estimates of the effects of each experimental condition by regression the dependent variables on the four dummies indicating each argument type (Weak GDP Evidence; Strong GDP Evidence; Weak Income Evidence; Strong Income Evidence), and the same set of covariates as previous analyses. While the estimates are noisier than the collapsed models, due the decrease in sample size in each category, they remain correctly signed and statistically significant. Despite some exceptions, I also find that the arguments with stronger evidence produced larger impacts than their weaker counterparts by and large.

I focused on overall economic evaluations as the dependent variable in the main text. Table A31 to A33 present an alternative specification that further categorizes economic evaluations into sub-elements; socio-tropic (the national economy), pocketbook, and income inequality. In each of these tables, Panel A presents the models using the four dummies for each treatment condition, Panel B presents the models using two (collapsed) dummies, and Panel C presents the models using a single continuous variable. While there are some exceptions, the key findings—significant and positive effects of arguments, especially those with stronger evidence—hold up here. Interestingly, I find that the GDP arguments were more effective for socio-tropic evaluations (Table A31), and the Income arguments were more effective for evaluations of the handling of the income inequality issue (Table A33). This is consistent with what should happen, when the arguments are assigned with more discriminating likelihood functions for more pertinent outcome variables.

Table A24: Full Reports on Panel A of Table 11 (Treatment Effects on Economic Evaluations)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Evaluations			Prospective Evaluations		
Subset	All	Republicans	Democrats	All	Republicans	Democrats
Weak Evidence	0.037*	0.070*	0.017	0.017*	0.040*	0.007
	(0.010)	(0.021)	(0.012)	(0.007)	(0.014)	(0.009)
Strong Evidence	0.055*	0.074*	0.037*	0.034*	0.033*	0.031*
	(0.009)	(0.018)	(0.011)	(0.007)	(0.013)	(0.009)
Party ID	0.110*	0.234*	0.061	0.117*	0.127*	0.153*
	(0.035)	(0.102)	(0.043)	(0.021)	(0.062)	(0.033)
Favorite Candidate (Clinton)	0.013	-0.036	0.040*	0.028*	-0.024	0.050*
	(0.017)	(0.058)	(0.024)	(0.012)	(0.031)	(0.016)
Favorite Candidate (Sanders)	0.037*	0.058	0.056*	0.040*	0.012	0.059*
	(0.015)	(0.038)	(0.022)	(0.011)	(0.028)	(0.015)
Favorite Candidate (Trump)	-0.043*	-0.054*	0.010	-0.081*	-0.103*	-0.066*
	(0.019)	(0.027)	(0.061)	(0.014)	(0.019)	(0.038)
Favorite Candidate (Cruz)	-0.048*	-0.055*	-0.019	-0.018	-0.040*	-0.012
	(0.019)	(0.026)	(0.044)	(0.014)	(0.019)	(0.026)
Favorite Candidate (Kasich)	0.005	-0.008	0.023	-0.001	-0.032	0.035
	(0.022)	(0.030)	(0.042)	(0.014)	(0.019)	(0.026)
2012 Vote Choice (Obama)	0.014	0.011	0.003	0.020*	0.023*	0.018
	(0.014)	(0.018)	(0.055)	(0.011)	(0.013)	(0.042)
2012 Vote Choice (Romney)	0.020	0.077*	-0.010	0.014	0.012	0.014
	(0.017)	(0.035)	(0.055)	(0.012)	(0.022)	(0.042)
Obama Approval	-0.012	-0.089*	0.021	0.018	-0.024	0.035
	(0.028)	(0.051)	(0.034)	(0.023)	(0.041)	(0.027)
Obama Economy Approval	0.060*	0.093*	0.044	0.047*	0.069*	0.035
	(0.025)	(0.052)	(0.033)	(0.020)	(0.035)	(0.027)
Obama Inequality Approval	-0.025	-0.007	-0.033	-0.018	0.029	-0.044*
	(0.017)	(0.038)	(0.021)	(0.013)	(0.026)	(0.018)
Retrospective Evaluation (Economy)	0.169*	0.246*	0.133*	0.084*	0.095*	0.077*
	(0.027)	(0.051)	(0.034)	(0.018)	(0.035)	(0.023)
Retrospective Evaluation (Inequality)	0.166*	0.139*	0.145*	0.113*	0.120*	0.088*
	(0.024)	(0.046)	(0.032)	(0.018)	(0.030)	(0.026)
Retrospective Evaluation (Pocketbook)	0.262*	0.177*	0.315*	0.183*	0.172*	0.211*
	(0.029)	(0.048)	(0.038)	(0.021)	(0.034)	(0.028)
Constant	0.148*	0.134*	0.200*	0.244*	0.248*	0.211*
	(0.020)	(0.034)	(0.070)	(0.014)	(0.024)	(0.050)
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Low-attention participants are underweighted. All covariates were measured before treatment. Reference categories for Favorite Candidate and 2012 Vote Choice are other options.

Table A25: Full Reports on Panel B of Table 11 (Treatment Effects on Economic Evaluations)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Belief in Cost Saving Effect			Attitudes toward ACA		
Subset	All	Opponent	Supporter	All	Opponent	Supporter
Experimental Conditions (0-1)	0.055*	0.077*	0.037*	0.034*	0.035*	0.031*
	(0.009)	(0.019)	(0.011)	(0.007)	(0.013)	(0.009)
Party ID	0.109*	0.222*	0.061	0.116*	0.118*	0.154*
	(0.035)	(0.102)	(0.043)	(0.021)	(0.061)	(0.033)
Favorite Candidate (Clinton)	0.015	-0.026	0.040*	0.028*	-0.017	0.047*
	(0.017)	(0.058)	(0.024)	(0.012)	(0.031)	(0.016)
Favorite Candidate (Sanders)	0.038*	0.070*	0.056*	0.040*	0.021	0.057*
	(0.015)	(0.039)	(0.021)	(0.010)	(0.029)	(0.015)
Favorite Candidate (Trump)	-0.042*	-0.050*	0.010	-0.081*	-0.100*	-0.067*
	(0.019)	(0.027)	(0.061)	(0.014)	(0.019)	(0.038)
Favorite Candidate (Cruz)	-0.048*	-0.054*	-0.019	-0.018	-0.039*	-0.014
	(0.019)	(0.025)	(0.044)	(0.014)	(0.019)	(0.026)
Favorite Candidate (Kasich)	0.005	-0.004	0.023	-0.001	-0.029	0.036
	(0.022)	(0.030)	(0.042)	(0.014)	(0.019)	(0.026)
2012 Vote Choice (Obama)	0.014	0.012	0.003	0.020*	0.024*	0.020
	(0.014)	(0.018)	(0.055)	(0.010)	(0.013)	(0.041)
2012 Vote Choice (Romney)	0.020	0.075*	-0.010	0.014	0.010	0.016
	(0.017)	(0.035)	(0.054)	(0.012)	(0.022)	(0.041)
Obama Approval	-0.012	-0.084*	0.021	0.018	-0.021	0.034
	(0.028)	(0.050)	(0.034)	(0.023)	(0.040)	(0.027)
Obama Economy Approval	0.060*	0.088*	0.044	0.047*	0.066*	0.034
	(0.025)	(0.052)	(0.033)	(0.020)	(0.035)	(0.027)
Obama Inequality Approval	-0.026	-0.008	-0.033	-0.018	0.028	-0.044*
	(0.017)	(0.038)	(0.021)	(0.013)	(0.026)	(0.018)
Retrospective Evaluation (Economy)	0.168*	0.248*	0.133*	0.084*	0.096*	0.078*
	(0.028)	(0.052)	(0.035)	(0.018)	(0.035)	(0.023)
Retrospective Evaluation (Inequality)	0.166*	0.146*	0.145*	0.113*	0.125*	0.088*
	(0.024)	(0.044)	(0.032)	(0.018)	(0.029)	(0.026)
Retrospective Evaluation (Pocketbook)	0.260*	0.173*	0.315*	0.183*	0.168*	0.213*
	(0.029)	(0.050)	(0.038)	(0.021)	(0.035)	(0.028)
Constant	0.151*	0.140*	0.199*	0.244*	0.252*	0.205*
	(0.020)	(0.034)	(0.068)	(0.014)	(0.024)	(0.049)
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Low-attention participants are underweighted. All covariates were measured before treatment. Reference categories for Favorite Candidate and 2012 Vote Choice are other options.

Table A26: Robustness Check on Table 11 (No Underweighting of Low-attention Participants)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
Weak	0.030*	0.063*	0.010	0.016*	0.034*	0.006
	(0.009)	(0.019)	(0.011)	(0.007)	(0.014)	(0.008)
Strong	0.050*	0.065*	0.034*	0.033*	0.027*	0.033*
	(0.008)	(0.017)	(0.010)	(0.007)	(0.012)	(0.009)
Panel B						
Linear Slope	0.050*	0.067*	0.034*	0.033*	0.029*	0.033*
	(0.008)	(0.017)	(0.010)	(0.007)	(0.012)	(0.009)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Low Attention Participants Underweighted	No	No	No	No	No	No
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. The coefficients on the intercepts and covariates are not shown.

Table A27: Robustness Check on Table 11 (No Pre-treatment Covariates)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
Weak	0.022	0.065*	-0.006	0.008	0.035*	-0.011
	(0.020)	(0.028)	(0.016)	(0.015)	(0.021)	(0.013)
Strong	0.068*	0.058*	0.028*	0.046*	0.022	0.022*
	(0.020)	(0.027)	(0.016)	(0.016)	(0.020)	(0.013)
Panel B						
Linear Slope	0.068*	0.061*	0.029*	0.045*	0.024	0.022*
	(0.020)	(0.027)	(0.016)	(0.016)	(0.020)	(0.013)
Covariates	No	No	No	No	No	No
Low Attention Participants Underweighted	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates are not included. Low-attention participants are underweighted. The coefficients on the intercepts are not shown.

Table A28: “Carryover” Treatment Effects on Vote Choice

	(1)	(2)	(3)	(4)
Subset	All	Republicans	Independent	Democrats
Panel A				
Weak	0.059*	0.039	-0.089	0.097*
	(0.023)	(0.032)	(0.071)	(0.031)
Strong	0.076*	0.007	0.167*	0.081*
	(0.023)	(0.029)	(0.079)	(0.032)
Panel B				
Linear Slope	0.077*	0.010	0.170*	0.080*
	(0.023)	(0.029)	(0.080)	(0.032)
Covariates	Yes	Yes	Yes	Yes
N	1100	319	127	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

Table A29-1: "Carryover" Treatment Effects on Redistributive Policy Preferences (1)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Minimum Wage			Rich Tax		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
Weak	-0.014	0.037	-0.020	0.003	0.029	-0.010
	(0.019)	(0.033)	(0.023)	(0.018)	(0.036)	(0.023)
Strong	-0.030*	-0.032	-0.017	0.038*	0.034	0.049*
	(0.017)	(0.033)	(0.021)	(0.018)	(0.037)	(0.022)
Panel B						
Linear Slope	-0.030*	-0.028	-0.017	0.038*	0.035	0.049*
	(0.017)	(0.033)	(0.021)	(0.018)	(0.037)	(0.022)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1075	311	644	1100	319	654

Table A29-2: "Carryover" Treatment Effects on Redistributive Policy Preferences (2)

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Income Gap			Government Responsibility		
Subset						
Panel A						
Weak	0.004	-0.004	-0.003	0.000	0.051	-0.024
	(0.017)	(0.027)	(0.023)	(0.020)	(0.040)	(0.022)
Strong	-0.012	-0.050*	0.004	-0.006	-0.003	-0.003
	(0.016)	(0.026)	(0.023)	(0.019)	(0.041)	(0.022)
Panel B						
Linear Slope	-0.012	-0.048*	0.004	-0.006	0.001	-0.002
	(0.016)	(0.026)	(0.023)	(0.019)	(0.041)	(0.022)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

Table A30: The Effects on Economic Evaluations by Each Treatment Condition

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
GDP Weak	0.070*	0.151*	0.031	0.035*	0.085*	0.018
	(0.016)	(0.032)	(0.020)	(0.011)	(0.023)	(0.013)
GDP Strong	0.093*	0.181*	0.033*	0.056*	0.070*	0.039*
	(0.016)	(0.035)	(0.016)	(0.012)	(0.025)	(0.014)
Income Weak	0.061*	0.087*	0.044*	0.028*	0.042*	0.022
	(0.014)	(0.032)	(0.015)	(0.011)	(0.021)	(0.014)
Income Strong	0.055*	0.097*	0.028	0.045*	0.052*	0.043*
	(0.015)	(0.030)	(0.019)	(0.011)	(0.022)	(0.014)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

Table A31: The Effects on Sociotropic Evaluations (the National Economy) by Each Treatment Condition

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
GDP Weak	0.070*	0.151*	0.031	0.035*	0.085*	0.018
	(0.016)	(0.032)	(0.020)	(0.011)	(0.023)	(0.013)
GDP Strong	0.093*	0.181*	0.033*	0.056*	0.070*	0.039*
	(0.016)	(0.035)	(0.016)	(0.012)	(0.025)	(0.014)
Income Weak	0.061*	0.087*	0.044*	0.028*	0.042*	0.022
	(0.014)	(0.032)	(0.015)	(0.011)	(0.021)	(0.014)
Income Strong	0.055*	0.097*	0.028	0.045*	0.052*	0.043*
	(0.015)	(0.030)	(0.019)	(0.011)	(0.022)	(0.014)
Panel B						
Weal	0.065*	0.116*	0.038*	0.031*	0.061*	0.020*
	(0.012)	(0.026)	(0.015)	(0.009)	(0.018)	(0.011)
Strong	0.074*	0.139*	0.030*	0.051*	0.061*	0.041*
	(0.012)	(0.026)	(0.014)	(0.009)	(0.019)	(0.012)
Panel C						
Slope	0.074*	0.143*	0.030*	0.051*	0.063*	0.041*
	(0.012)	(0.026)	(0.014)	(0.009)	(0.019)	(0.011)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

Table A32: The Effects on Pocketbook Evaluations by Each Treatment Condition

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
GDP Weak	0.026	0.109*	-0.010	0.023*	0.080*	0.006
	(0.017)	(0.031)	(0.021)	(0.012)	(0.023)	(0.015)
GDP Strong	0.034*	0.038	0.013	0.018	0.002	0.019
	(0.015)	(0.030)	(0.018)	(0.012)	(0.025)	(0.015)
Income Weak	0.020	0.039	0.008	-0.001	0.012	-0.007
	(0.015)	(0.033)	(0.018)	(0.012)	(0.023)	(0.016)
Income Strong	0.051*	0.061*	0.041*	0.029*	0.022	0.035*
	(0.014)	(0.031)	(0.017)	(0.012)	(0.024)	(0.015)
Panel B						
Weal	0.023*	0.070*	-0.001	0.011	0.042*	-0.000
	(0.013)	(0.027)	(0.015)	(0.010)	(0.018)	(0.013)
Strong	0.042*	0.049*	0.027*	0.024*	0.011	0.027*
	(0.012)	(0.025)	(0.014)	(0.010)	(0.019)	(0.012)
Panel C						
Slope	0.042*	0.053*	0.027*	0.024*	0.014	0.027*
	(0.012)	(0.025)	(0.014)	(0.010)	(0.019)	(0.012)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

Table A33: The Effects on Performance on the Handling of Income Inequality by Each Treatment Condition

	(1)	(2)	(3)	(4)	(5)	(6)
DV	Retrospective Beliefs			Prospective Beliefs		
Subset	All	Rep	Dem	All	Rep	Dem
Panel A						
GDP Weak	0.017	0.028	0.017	0.006	0.033	-0.002
	(0.017)	(0.037)	(0.022)	(0.014)	(0.032)	(0.016)
GDP Strong	0.034*	0.012	0.040*	0.017	0.016	0.007
	(0.016)	(0.033)	(0.019)	(0.012)	(0.028)	(0.015)
Income Weak	0.028*	0.022	0.015	0.014	0.006	0.005
	(0.016)	(0.037)	(0.019)	(0.012)	(0.026)	(0.015)
Income Strong	0.064*	0.058	0.070*	0.040*	0.040	0.041*
	(0.016)	(0.036)	(0.020)	(0.012)	(0.026)	(0.015)
Panel B						
Weal	0.022	0.025	0.016	0.010	0.018	0.001
	(0.014)	(0.031)	(0.017)	(0.010)	(0.023)	(0.013)
Strong	0.049*	0.035	0.055*	0.028*	0.027	0.025*
	(0.013)	(0.028)	(0.016)	(0.010)	(0.021)	(0.012)
Panel C						
Slope	0.049*	0.035	0.055*	0.028*	0.028	0.025*
	(0.013)	(0.028)	(0.016)	(0.010)	(0.021)	(0.012)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
N	1100	319	654	1100	319	654

Note. * $p < 0.1$ (two-tailed). OLS estimates with robust standard errors in parentheses. Covariates include party ID, favorite Presidential candidate (among Clinton, Sanders, Trump, Cruz and Kasich), vote choice in 2012, evaluations of Obama (overall, economy and inequality), pre-treatment retrospective evaluations of the two parties. Low-attention participants are underweighted. The coefficients on the intercepts and covariates are not shown.

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