DO FACTS SPEAK FOR THEMSELVES?
CAUSES AND CONSEQUENCES OF PARTISAN BIAS IN FACTUAL BELIEFS

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Abstract

Observers of contemporary U.S. politics lament the seeming inability of Democrats and Republicans to agree on objective truth, such as the unemployment rate, federal deficit, and other politically relevant matters of fact. These apparent divisions are troubling from a normative perspective. They cast doubt on citizens’ ability to form informed opinions, engage in productive debate with each other, and reward and punish elected officials.

This dissertation explores causes and consequences of these partisan gaps in the public’s factual beliefs. Using a combination of survey meta-analysis and original data collection, I measure the extent of partisan bias in beliefs and experimentally probe its proposed mechanisms. After building a database of national polling data on a variety of economic indicators from 1980 to the present, I find that partisan bias in perceptions of the economy is less severe than past work has suggested. However, the database reveals considerable heterogeneity in bias across various survey items and real-world conditions.

In a series of experiments, I test a prominent and troubling theory of partisan bias: selective learning of factual information. I expose partisans to actual facts with either positive (congenial) or negative (uncongenial) implications for their party. I find little evidence of selective learning across studies. Instead, partisans learn facts evenhandedly. While many forget factual information after several days, they are equally likely to recall congenial and uncongenial information, mitigating concerns about bias. I also find that partisans assimilate both congenial facts and uncongenial facts into relevant attitudes, such as incumbent evaluations and policy preferences. These attitudinal shifts persist several days after initial exposure to factual information.

Rather than revealing a deep-seated bias in how people learn, partisan gaps in surveys of factual beliefs are more likely to arise from selective reporting on the part of respondents with motivation to express partisan loyalty. Evidence of selective reporting comes from a series of survey experiments that nudge respondents to accurately report their beliefs in response to factual questions without providing respondents with any additional information. However, incentivizing respondents to counter their bias in reporting factual information can have adverse effects on their subjective attitudes.
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For Mama & Papa
“Politics is a battle of ideas. In the course of a healthy debate, we’ll prioritize different goals, and the different means of reaching them. But without some common baseline of facts, without a willingness to admit new information, and concede that your opponent is making a fair point, and that science and reason matter, we’ll keep talking past each other, making common ground and compromise impossible.”

-Barack Obama’s Farewell Address, January 10, 2017

With these parting words, President Obama echoed a widespread concern that has taken hold among academics, journalists, and laypersons alike: can people agree on basic facts anymore? Observers of contemporary politics, particularly in the United States, are increasingly alarmed by the seemingly vast gulf between citizens on the left and right of the political spectrum. Poll after poll suggests that Democrats and Republicans are seeing starkly different realities when it comes to factual matters of political relevance, such as global warming, the state of the economy, and the integrity of elections. Many political observers have resigned themselves to the idea that partisan division and disagreement over facts are unfortunate hallmarks of our current period of polarization.

These concerns are now a prominent part of the national zeitgeist. Dire headlines, such as “Why Facts Don’t Change Our Minds” (Kolbert, 2017), “It’s Time to Give Up on Facts” (Zimmerman, 2017) and “Your Facts or Mine” (Roller, 2016), warn of widespread disagreement about factual information, motivated resistance to new information, and the impotence of fact-based arguments to persuade citizens and voters on important issues. A lot of the current anxiety around these issues relates to Donald Trump’s egregious use of misleading and patently false claims during his successful candidacy and then from his bully pulpit as president (“Tracking
President Trump’s false claims”, 2019). Not long after the U.S. presidential election in 2016, Oxford Dictionaries declared “post-truth” international word of the year, citing a twentyfold increase in its use over the prior year – it is defined as, “relating to or denoting circumstances in which objective facts are less influential in shaping public opinion than appeals to emotion and personal belief” (Wang, 2016). The impression of factual discord has made its way to the lay public. When polled in the fall of 2016, eight in ten voters said that Clinton and Trump supporters disagree not only on plans and policy but also basic facts (“Voters Say ‘Basic Facts’ Are in Dispute”, 2016).

While generally more measured in their tone, political scientists and psychologists have issued similar warnings about the pernicious influence of partisan identities and, identity-based processes more generally, on people’s factual beliefs. These warnings are based on decades of survey research and experiments. As I will review in detail, the academic literature is replete with demonstrations of partisan divisions in factual beliefs and biased information processing among the public. In this chapter, I review theories of partisan bias, as well as related concepts, such as misinformation, factual corrections, and backfire effects – topics that sit within a broader literature on forms of mass polarization.

**Scholarly Work on Polarization**

This dissertation addresses an area of political behavior that falls under the larger umbrella of research on polarization among the mass public. Most of this research centers on subjective beliefs or feelings, and more specifically, on differences between members of the major parties in the U.S. with respect to subjective attitudes. Partisan differences have become more pronounced in recent years. For example, partisans have increasingly voted for their own party’s candidates over time (e.g., Bartels, 2000). Democrats and Republicans have become
increasingly divided in presidential approval ratings (e.g., Abramowitz & Stone, 2006; Lebo & Cassino, 2007; McAvoy & Enns, 2010; Donovan et al., 2019), racial attitudes (e.g., Valentino & Sears, 2005; Tesler, 2012), and self-reported left-right ideology (e.g., Levendusky, 2009). Partisan differences also emerge in which information sources partisans say they prefer (e.g., Garrett, 2009; Iyengar & Hahn, 2009; Stroud, 2010), what they think majority opinion is on a given issue (Nir, 2011), and even in how extreme they believe their political opponents to be (Ahler, 2014).

While some scholars have documented left-right polarization with respect to issue attitudes (e.g., Abramowitz & Saunders, 2008; Ura & Ellis, 2012), others argue that while partisans are increasingly sorted into likeminded groups of liberals or conservatives, they have not become markedly more extreme with respect to specific issues (e.g., Fiorina & Abrams, 2008; Hetherington, 2009). Hill and Tausanovitch (2015), for instance find that the public has become more ideologically sorted since 1980 without much divergence – that is, Democrats and Republicans have not moved away from each other on a left-right ideological scale. On the other hand, Democrats have become more consistently liberal and Republicans more consistently conservative in their policy preferences, particularly among partisans attuned to elite signals about the issues (e.g., Layman & Carsey, 2002; Levendusky, 2010).

One of the consequences of this partisan sorting is polarization along affective, or emotional, lines (Mason, 2015, 2016, 2018). Partisans tend to view opponents with more antipathy than in the past, as social distance between the parties’ rank-and-file has grown larger over time, heightened by contentious political campaigns (Iyengar et al., 2012; for a review of affective polarization, see Iyengar et al., 2019). Partisan animosity sometimes occurs

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1 Congress, on the other hand, has both exhibited substantial divergence over time and become better sorted than the public (e.g., McCarty et al., 2006; Hill & Tausanovitch, 2015).
automatically and implicitly, without people being consciously aware that they are feeling ill will or exhibiting prejudice toward the other side (Iyengar & Westwood, 2015).

A portrait emerges of a public firmly entrenched in partisan camps and bitterly divided about many issues of the day, including whose opinions they value and how they view the other side, though not very extreme ideologically. Perhaps we should not be too surprised to see these divisions, given the current period of elite polarization (McCarty et al., 2006), combined with the public’s propensity to follow elite cues (Zaller, 1992; Lenz, 2013) and a fragmented, high-choice media environment (Prior, 2007). What unites these forms of polarization is that they concern subjective attitudes, whether they are kneejerk, emotional reactions or deliberative judgments. Factual beliefs, on the other hand, concern objective matters. What surprises many contemporary observers and is less understood than political attitudes is the apparent mass polarization around matters of fact.

While studied less than subjective attitudes have been by political psychologists, factual beliefs have been the subject of decades of survey research and have had a recent resurgence in political science and psychology. One takeaway from the extant research is that there is widespread ignorance within the U.S. public – when quizzed about politics and public affairs, many people give incorrect answers or say they simply do not know (e.g., Bartels, 1996; Delli Carpini & Keeter 1997; Kuklinski et al., 2000; Luskin & Bullock 2011).

Compounding this bleak news, Democrats often report dramatically different factual beliefs than Republicans do. Partisans tend to report better conditions, like low crime and unemployment, when their party is in power than when the other party is in power (e.g., Bartels 2002). These days, for example, nine in ten Republicans report the national economy is good, while closer to six in ten Democrats say so, resulting in a gap of 30 points (Salvanto et al., 2019).

Figure 1.1 visualizes this pattern over time, showing that partisan gaps generalize to other periods of Republican control, and flip signs under Democratic administrations. (I later return to the utility of this particular survey item.)

**Figure 1.1. Positive Ratings of the Economy among Partisans in CBS News Poll**

Note: figure plots percent of Republicans (dashed line) and Democrats (solid line) saying national economy is somewhat or very good. Vertical lines indicate change in presidency. All polls conducted by phone with nationally representative samples of U.S. adults. Data is LOESS smoothed.
The general pattern is that partisans report *congenial* beliefs that reflect positively on their party. These responses often come at the expense of the *factually correct* beliefs, which may cast their party in a more negative light. This behavior has been documented across many topic areas, including foreign policy (e.g., Kull et al., 2003; Jacobson, 2010), global warming (e.g., McCright & Dunlap, 2011), health care (Nyhan, 2010; Berinsky, 2017), other social services (e.g., Kuklinski et al., 2000; Jerit & Barabas, 2012), and statewide ballot initiatives (Wells et al., 2009).

**Why Study Facts?**

Factual beliefs are qualitatively different the subjective dimensions of polarization described above. Factual beliefs concern what *is*, or in some cases, what *was*. What is the national unemployment rate? Is it higher or lower than it was one year ago? What percent of U.S. adults lack health insurance? How many electoral votes did Donald Trump win in 2016? Did he win more than Barack Obama did four years earlier? Did three million non-citizens illegally cast ballots in the presidential election? Though they may have political implications, all of these questions have objectively verifiable answers that are commonly understood to be the truth of the matter.

On the other hand, attitudes, policy preferences, issue importance, and values are essentially subjective and generally concern what *should be*. How should the government allocate the federal budget? What policies should we enact to help working class people? For that matter, should helping the working class be a priority? This distinction between descriptive and normative beliefs has been proposed since the enlightenment era. David Hume articulated it

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3 More generally, people are more likely to report having beliefs that do not threaten their preexisting beliefs and attachments, including ideological worldview and prior attitudes. While partisanship is one possible attachment that can bias factual beliefs, I focus on its role because of its primacy in contemporary politics.
in *A Treatise of Human Nature* (1739). More recently, the late Senator and statesman Daniel Patrick Moynihan gave a pithy summary of the is-ought problem in an often-quoted remark: “Everyone is entitled to his own opinion, but not to his own facts.”

In many cases, the latter is a prerequisite for the former. Subjective attitudes are often based on beliefs. Preferences about allocating federal funds, for example, may be based on factual beliefs about various agencies’ current funding levels. Attitudes about anti-poverty measures may depend on factual beliefs about the rate of poverty or about the efficacy of proposed policies. People are entitled to their opinions, because facts are of course just one of several possible inputs to political attitudes, which are also shaped by personal preferences and values. Even some putatively descriptive questions, such as whether a politician is honest or competent, are inherently subjective. While the may be influenced by one’s factual beliefs, they nonetheless require one to make judgments of personal qualities about which different observers can reasonably disagree.

Throughout this dissertation, I maintain this distinction between factual beliefs and subjective attitudes. As much as possible, I operationalize factual beliefs in such a way as to probe beliefs about purely objective matters. I also examine the complicated relationship between factual beliefs and attitudes. However, I focus on facts because their qualitatively different nature raises unique normative concerns and methodological issues for political scientists.

**Normative Concerns**

While we expect personal preferences and values to differ in a pluralistic society, we should not welcome disagreement over facts and objectively verifiable information. Why? Competent democratic citizenship requires factual information. Citizens must be able to discern
their interests and engage with the political system (e.g., Delli Carpini & Keeter, 1997). As alluded to before, factual disagreement casts doubt on people’s ability to form attitudes that are in line with their values and interests (e.g., Hochschild, 2001). For example, Gilens (2001) shows that when people learn policy-specific facts, such as the percent of the federal budget going to foreign aid, they significantly adjust their level of support for relevant policies. Bullock (2011) shows that providing people with policy-relevant information shape their policy preferences to a greater extent than even partisan cues do.

**Retrospective Voting and Democratic Accountability**

Factual information also plays a key role in prominent voting theories, in which citizens cast votes on the basis of economic conditions (e.g. Kramer, 1971; Fiorina, 1981; Kinder & Kiewiet, 1981). Retrospective voting, in which citizens take stock of conditions – or recent changes in conditions – in order to gauge incumbent performance, is thought to be an important mechanism of democratic accountability. Good performance is rewarded at the ballot box and poor performance punished by voting out the incumbents (e.g., Key, 1966). While there is some scholarly debate over exactly which economic indicators are the most relevant in this process, the general scholarly consensus that they impact both presidential favorability and voting decisions. In a review of economic voting literature, Duch and Stevenson (2008) find that normal changes in economic indicators, such as the rates of unemployment and inflation, move presidential approval by three to ten percentage points.

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4 Another form of accountability that has been proposed as an alternative to this reward-and-punish model is a selection model. Voters aim to elect competent officials and try to infer their competence from past economic conditions, which serve as a “noisy indicator” (Duch & Stevenson, 2013). Even in this alternative model, economic perceptions and other factual beliefs are critical, because they inform voters’ decision-making process.
However, retrospective voting becomes a dubious form of accountability when voters’ beliefs about real-world conditions are systematically incorrect or even missing. How can the public properly reward and punish incumbents without accurate beliefs about what has happened? Hetherington (1996) provides an instructive example of a breakdown in this process. In 1992, commonly used economic indicators suggested that the incumbent Republican Party held the advantage in the presidential election. If voters’ decisions at the ballot box were based solely on objective economic conditions, George H.W. Bush likely would have been reelected. Instead, voters appeared to believe that the economy was in worse shape than most indicators suggested, leading to a failure of economic voting and an out-party victory at the polls. It is of course possible that voters had other issues on their mind in 1992, but misperceptions nonetheless pose a serious threat to economic voting models.

The problem is deeper than just a lack of critical information, of course: factual beliefs vary systematically between each party’s rank and file. Bartels (2002) finds large partisan differences in beliefs about a variety of economic indicators. Analyzing panel data, Evans and Pickup (2010) turn the conventional wisdom about economic voting on its head: rather than shape political preferences, perceptions of the economy are heavily influenced by partisanship. The influence of partisanship on economic perceptions and other factual beliefs suggests that relevant political attitudes will not converge, instead perpetuating partisan divisions.

Many scholars have argued that even though bias and ignorance are common at the individual level, the public can overcome these limitations through heuristics and aggregation. Perhaps individuals short on relevant factual information use party cues and other cognitive shortcuts to behave as if they were fully informed (e.g., Popkin, 1991; Lupia, 1994). And even though partisan bias exists in individuals’ beliefs, perhaps aggregating data up to the mass level
would cancel out this bias, resulting in a more or less “rational” public (e.g., Page & Shapiro, 1992).

Unfortunately, a fair reading of the subsequent literature reveals that neither of these optimistic hypotheses have been fully borne out by the evidence. Heuristics do not enable ignorant or misinformed voters to behave as if they had the relevant information at hand (e.g., Bartels, 1996; Kuklinski & Quirk, 2000; Lau & Redlawsk, 2001). Moreover, a litany of studies demonstrate that aggregating perceptions does not solve the problem of individual-level partisan bias (see, e.g., Durr, 1993; De Boef & Kellstedt, 2004; Duch & Stevenson, 2011). For instance, Enns et al. (2012) analyze public opinion about national economic conditions using a variety of survey measures over four presidencies and find that partisan bias does not cancel out in the aggregate and produces distortions in relevant attitudes. Enns and McAvoy (2012) show that partisan bias in the aggregate slows down the impact of objective economic information on the public’s beliefs about the economy.

In sum, achieving accountability via retrospective voting generally requires more of citizens than they appear capable of doing. As Anderson (2007) summarizes the issue, “To properly judge the government’s record, citizens ideally should be well informed, unbiased consumers of accurate and plentiful information” (p. 289). The public appears to be neither well informed nor unbiased, and these limitations affect collective opinion.

While models of economic voting tend to be retrospective and sociotropic in nature, other variations exist in the literature. For example, voters may instead behave prospectively, basing their voting decisions on future expectations, rather than past conditions (e.g., MacKuen et al., 1992; Erikson et al., 2000). However, these expectations are themselves subject to partisan bias
(Freeman et al., 1998; Gerber & Huber, 2009). This is not surprising, given the subjective nature of guesses about future conditions.

Another form of accountability that does not require beliefs about the present or past economy are “pocketbook” theories, in which voters simply look at their household finances or other personal experiences to inform their vote decisions (e.g., Lewis-Beck, 1985). However, surveys show that partisan differences emerge even when people report their personal financial situation. A CBS News/YouGov poll last summer found that Republicans were twice as likely as Democrats to say that their family was better off financially than one year prior (Salvanto et al., 2018). Democrats, by contrast, were twice as likely as Republicans to say they were worse off than a year before. A similar item fielded by SurveyMonkey in the same time period produced even more extreme results: Republicans (65%) were over four times as likely as Democrats (14%) to say that their financial situation had improved over the previous year (Casselman & Tankersley, 2018). Partisan gaps this large are unlikely to be due to actual pocketbook differences between Democrats and Republicans. Even if Republicans tend to be wealthier than Democrats, it is unlikely that the groups’ fortunes diverged so dramatically over the course of a year.5

**Methodological Concerns**

Partisan bias in factual beliefs also poses unique methodological challenges to political scientists and scholars of public opinion. Most relevant to the rich literature on economic voting, the endogeneity of beliefs about the economy to partisanship and other political preferences results in biased estimates of the effect of economic perceptions on vote choice, presidential approval, and related variables. In an early demonstration of this issue, Kramer (1983) finds that

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5 Relatedly, Gerber and Huber (2010) find that partisan differences in economic ratings are not a result of correlations between partisanship and actual economic experiences.
the evidence for sociotropic economic voting is an artifact of endogeneity and that the evidence is consistent with voters acting out of self interest instead. Enns et al. (2012) find that partisan bias leads researchers to overstate the actual relationship between economic perceptions and ratings of the president’s job handling the economy. Any correlational work must take care to deal with this endogeneity problem.

Partisan gaps that emerge in surveys of factual beliefs pose additional challenges related to measurement. When Democrats and Republicans report dramatically different answers in response to easy questions about their personal finances or about the readily apparent size of an inauguration crowd in a photo (see Schaffner & Luks, 2018), it calls into question whether survey respondents are reporting their genuine beliefs. Chapter 4 will present evidence that partisans often answer factual questions on surveys as if they are opinion questions, sometimes giving answers they know to be untrue out of partisan loyalty. This behavior suggests that ordinary survey measures designed to probe factual beliefs may be contaminated by respondents’ opinions. Researchers must therefore take care in designing questions to mitigate this tendency.

These normative and methodological issues motivated the current inquiry into the degree, causes, and consequences of partisan bias in factual beliefs. Partisan bias impedes citizens’ ability to arrive at informed preferences and to reward and punish their elected officials effectively (e.g., Shapiro & Bloch-Elkon 2008; Hochschild & Einstein 2015). More generally, if we do not use facts to inform its evaluations of officials and policies, we risk getting bad officials and poor policies (Lavine et al., 2012). While party loyalty may be valuable for other reasons, factual disagreement harms democratic deliberation and thwarts compromise between those on the political left and right (Muirhead, 2013). With these concerns in mind, I turn to proposed explanations for why partisans report different factual beliefs from one another.
Proposed Explanations

The importance of party identification in structuring political beliefs is not a new idea. Campbell et al. (1960) describe partisanship as a “perceptual screen through which the individual tends to see what is favorable to his partisan orientation” (p. 133). Many subsequent studies have provided empirical support for the perceptual screen argument. For example, experimental studies show that partisans process information in a way that reinforces their preexisting attachments and beliefs, often while discounting contradictory information (e.g., Zaller, 1992; Taber & Lodge, 2006, Taber et al., 2009). While most of this work has dealt with attitudes, the information processing literature includes theories that explain partisan bias in factual beliefs. Below, I review the two possible mechanisms I empirically test in subsequent chapters.

Selective Learning

The motivated reasoning paradigm offers one explanation of why partisan gaps in factual beliefs emerge: partisans selectively learn facts. According to this process, when they encounter party-relevant information, partisans tend to uncritically learn facts that portray their party in a positive light, while rejecting or ignoring facts that look bad for their party. For example, when reading a jobs report during a Republican administration, a staunch Republican may readily learn the number of new jobs added but ignore the stagnant labor force participation rate, even though both figures are reported. The former fact reflects well on the Republican Party, while the latter is much less flattering, if not outright negative, to most observers. Importantly, selective learning is a hypothesis that is conditional on a given set of facts or piece of information. It predicts that partisans will be more likely to learn congenial facts than uncongenial facts.\(^6\)

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\(^6\) Some scholars refer to this phenomenon as “partisan perceptual bias” (Gerber & Green, 1999; Jerit & Barabas, 2012). I avoid this term, because it is close to partisan bias, which is employed
Selective learning is closely related to the theory of motivated reasoning, which posits that human reasoning and information processing is shaped by two differing and sometimes competing types of motivation or goals (Kunda, 1990). Accuracy motivation encourages us to arrive at the most accurate belief possible, given available information. These are the set of goals that tend to kick in when someone is taking a test and racking their brain for correct answers. Directional goals, on the other hand, push us toward some specific belief, often one that is internally consistent with deeply held prior beliefs or core attachments, such as political party, ideology, or cultural identity. This set of goals allows one to avoid potential cognitive dissonance when forming beliefs, evaluating evidence, or reasoning more generally (Frimer et al., 2017). I consider selective learning to be a form of motivated reasoning, which describes a wider set of behaviors.

Accuracy and directional motivation are occasionally at odds, for example when forming an accurate belief would conflict with our prior beliefs. People reach erroneous conclusions and form false beliefs when directional goals exert a greater influence than accuracy goals, which often depends on context (Druckman, 2012; Bolsen et al., 2014). Most research on motivated reasoning in politics probes the influence of directional goals on subjective attitudes, such as evaluations of political arguments (e.g., Taber & Lodge, 2006; Bolsen, et al., 2014), ratings of political leaders (e.g., Lebo & Cassino, 2007), and party cue taking (e.g., Slothuus & de Vreese, 2010; Petersen et al., 2013; Druckman et al., 2013).

To describe a range of behaviors. I use selective learning, because it describes a particular process (i.e., learning) by which partisan differences may emerge.

7 While my focus is on partisanship, individuals may experience other directional goals. For example, race and religion are powerful social identities that may independently give rise to consistency pressures. Issue publics, who care deeply about a particular issue, may also experience similar pressures.
While fewer studies examine how directional goals influence factual information processing, there is reason to believe that similar processes operate when people learn facts. When partisans process factual information, especially facts that threaten their partisan loyalty, they are likely to feel the dual pull of accuracy and directional goals. If the directional motivation outweighs the accuracy motivation, partisans may fail to learn facts that conflict with their preferred conclusions. They might fail to attend to such facts or expend enough cognitive effort to commit such facts to long-term memory. Or they may dismiss an uncongenial factual statement as not being credible and therefore forget the statement itself. Directional motivation may also encourage partisans to form factual beliefs that are incorrect but flattering for their party. For all these reasons, partisans may be more likely to learn congenial facts than uncongenial ones.

A related process is that when people are presented with factual information, they are more likely to deduce that the information supports a congenial conclusion than an uncongenial conclusion (Kahan, 2013; Kahan et al., 2017). Suppose, for instance, that there exist some data that support an unequivocal conclusion relevant to public policy. And we ask an individual with an existing opinion on the policy to look at the data and learn what conclusion it supports. If directional goals outweigh accuracy goals, then the individual will be more likely to learn a conclusion that is congenial to them, even if this conclusion is incorrect. This is the version of selective learning tested in Chapter 4.

I note here that learning a fact or a conclusion supported by factual information can be different from believing the fact or conclusion is correct. For example, imagine a study on raising wages that found wage increases led to job losses. One could correctly learn the conclusion of this particular study while still believing that wage hikes do not affect the
availability of jobs. Similarly, one could commit an unemployment figure to memory while simultaneously believing the figure is not credible. For the most part, I define learning as committing factual information to memory, leaving aside the issue of perceived credibility. I usually operationalize learning as correctly answering a factual question. However, in Chapter 4, I also address the question of perceived credibility of the information respondents are asked to learn.

Selective learning is not an entirely new idea, but it is a stronger version of older theories.\(^8\) In their description of the perceptual screen, for example, Campbell et al. (1960) remain agnostic about the precise mechanism by which information is filtered, as well as what types of beliefs or information can be distorted. For instance, the perceptual screen may operate through Republicans and Democrats getting their information from different sources, which would not constitute selective learning.

Zaller’s (1992) seminal model of public opinion offers us a framework that can encompass selective learning. In brief, his model describes the process by which people receive political messages and accept or reject messages based on their prior attitudes and predisposition. Both steps are contingent on their level of political sophistication. When asked for their opinion, individuals sample from the set of available considerations, with more recent considerations being more accessible in memory. Selective learning may be thought of as bias in the second step in Zaller’s receive-accept-sample (RAS) model. He explicitly states that “people tend to accept what is congenial to their partisan values and reject what is not” (p. 241). Applied to

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\(^8\) Seminal studies in political psychology, such as Hastorf and Cantril (1954) and Vallone et al. (1985), make an argument that is similar in spirit: even when partisans are exposed to the same information, they selectively perceive what is congenial to their side.
factual information, the resistance axiom implies that people will be more likely to accept congenial facts than uncongenial ones.

Selective learning is a particularly insidious form of partisan bias, adding another layer of normative concern. Selective learning suggests that even when people encounter the same information as one another, they nevertheless form very different beliefs. Such bias makes reducing gaps in factual beliefs quite thorny: providing accurate information to people is unlikely to be sufficient. In fact, media coverage is likely to exacerbate gaps, rather than reduce them, because partisans will have an even greater opportunity to learn congenial facts and ignore uncongenial ones (Jerit & Barabas, 2012).

**Empirical Evidence**

While most work on partisanship and factual beliefs is observational in nature, a dispositive test of the selective learning hypothesis requires an experimental or quasi-experimental design. For example, in a series of survey experiments on student samples, Nyhan and Reifler (2010) find that partisans resist factual information that contradicts their ideological worldview. Schaffner and Roche (2017) employ a natural experiment around a jobs report in the fall of 2012 to test for selective learning. The report contained the good news that the unemployment rate had decreased to below 8 percent nationally, reflecting well on the Obama administration. The authors find that Democrats are more likely than Republicans to accurately update their beliefs about the new unemployment number.

In an important study that was a point of departure for this dissertation, Jerit and Barabas (2012) argue that partisans engage in selective learning of facts about a range of issues. The authors present partisans with news stories containing facts that reflect either positively or negatively on Democrats or Republicans, as well as a control condition with no relevant
information. They find that partisans are more likely to learn congenial than uncongenial facts. For example, Democrats are more likely to learn about the success of the Troubled Asset Relief Program than the size of the trade deficit. This study is similar in spirit to the tests of selective learning that I employ in Chapter 3, but its experimental design is flawed in a way that precludes a clean test of the hypothesis. I will discuss the shortcomings of this and other selective learning studies in greater detail in Chapter 3.

The empirical evidence is not uniformly consistent with selective learning. To the contrary, several observational studies find some degree of convergence in factual beliefs between the left and the right, as the public receives new information (Gaines et al. 2007; Blais et al. 2010; Parker-Stephen 2013). These studies find that, rather than polarizing in response to factual information, people come to agree on the facts on the ground. This is more likely to occur when there is an unambiguous signal in the environment. Bisgaard (2015), for example, finds that while partisans in the UK initially disagreed on the state of the national economy, the economic recession of 2008 was an inescapable reality that prompted partisans to agree that conditions had indeed deteriorated. Partisans diverged in whom they blamed, instead of the cold facts. This line of research suggests that partisans can learn accurately, or at a minimum, that real-world conditions constrain partisan bias.

Selective Reporting

Selective reporting is another mechanism that may explain partisan differences that are observed in surveys. Selective reporting occurs when partisans hold the same underlying beliefs as one another but differ in their propensity to report these beliefs on a survey. In particular, it occurs when people with the same beliefs are more likely to give politically congenial answers
than uncongenial answers. The end result of this behavior is to produce partisan differences in surveys of factual beliefs that exaggerate the actual degree of difference between partisan groups.

People engage in selective reporting for a variety of reasons. Some deliberately misreport as a way to express their partisan loyalty. For example, a survey respondent who vehemently opposes President Trump may be reluctant to admit knowing that the national unemployment rate is lower today than it was at the end of President Obama’s presidency. The respondent may instead report a rise in unemployment to express their opposition. This kind of expressive self-presentation has been described as cheerleading (e.g., Gerber & Huber, 2009). Others may misreport just to be consistent within a survey, ensuring that later answers do not contradict their earlier ones (e.g., Sears & Lau, 1983; Lau et al., 1990; Wilcox & Wlezien, 1993; Palmer & Duch, 2001). Yet others may engage in selective reporting to indicate their disbelief in information. For instance, in Kahan et al. (2017), a respondent may pick the congenial answer even after figuring out that the data support an uncongenial conclusion as a way to express their disbelief in the putative data.

In addition to actively misreporting what they believe, selective reporting may take more passive forms. For example, a respondent may withhold their beliefs by giving a “don't know” answer or skipping a question. Alternately, respondents who don't know the correct answer may report a congenial answer as their best guess. Selective reporting may occur without conscious awareness. For example, when asked a factual question in a survey, respondents may scan their memory for a longer time to come up with examples of congenial beliefs than uncongenial beliefs. Again, Zaller’s RAS model offers a useful framework. Selective reporting concerns the last step of opinion formation, in which the survey respondent samples the set of available

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9 This phenomenon is sometimes referred to as “motivated responding” (Khanna & Sood, 2018) or “expressive responding” (Schaffner & Luks, 2018) the same meaning in mind.
considerations. While both congenial and uncongenial considerations may be stored in memory, the respondent may selectively sample the congenial ones when answering a survey question or coming up with a top-of-the-head answer to a factual question that they had not given much thought to previously.

Evidence of selective reporting comes from a pair of innovative experiments that nudge survey respondents to be more accurate when answer factual questions. Bullock et al. (2015) and Prior et al. (2015) randomly incentivize survey respondents to report their factual beliefs accurately, which cuts partisan bias by about half. These studies show that non-incentivized responses reveal a mix of what partisans believe and what they wish to be true. I review the key findings of Prior et al. in greater detail at the start of Chapter 4.

Schaffner and Luks (2018) also uncover evidence of selective reporting without the use of monetary incentives. In a cleverly designed experiment, they ask partisans to answer a factual question “where the answer is so clear and obvious to the respondents that nobody providing an honest response should answer incorrectly” – they ask about crowd sizes in a pair of photos of the respective inaugurations of President Obama and President Trump. This topic also has the advantage of being politicized, due to Trump’s boasting that his inauguration had a bigger audience. The authors find clear evidence of selective reporting among Republicans, which was moderated by political interest.

While both selective learning and selective reporting are related to the theory of motivated reasoning, there are two major differences. First, selective reporting pertains to the survey response process, as opposed to real learning.¹⁰ Selective reporting influences what

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¹⁰ It is possible, even likely, that selective reporting also operates outside survey contexts, explaining which beliefs people choose to reveal in discussions among social networks, for
people say they have learned, not what they have actually learned or know. Second, and relatedly, selective reporting is in part “cheap talk” that people engage in to publicly protect their core attachments and beliefs. And to the extent that these pronouncements are shallow, based not in what people deeply believe but what people are prepared to say publicly, these reported beliefs are unlikely to shape respondents’ attitudes and behavior. On the other hand, uncongenial beliefs, which respondents hold internally but are loath to admit, may nonetheless influence attitudes and behavior. It is therefore important to distinguish between genuine beliefs and instrumental or shallow responses.

These differences suggest that selective reporting is a less troubling phenomenon than selective learning. In fact, it may be welcome news from a perspective of democratic accountability. If observed partisan differences in factual beliefs are primarily caused by selective reporting, then typical surveys overestimate the severity of the problem. There may be a much greater deal of partisan agreement on factual matters than past work has led us to believe.

**Related Concepts**

Partisan bias in factual beliefs touches on many concepts in political psychology that are also discussed widely among laypeople. These related concepts include selective exposure (and “echo chambers”), misinformation (and “fake news”), as well as corrective information. While these topics are not the central focus of this project, I briefly review them to explain their connection to my area of inquiry and situate them in the broader literature.

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example. In this dissertation, I examine only on selective reporting in surveys, but selective reporting in more everyday domains is an important topic for future research.
Selective Exposure

Another prominent explanation of partisan gaps is selective exposure to factual information. Selective exposure describes a process by which people receive different facts from one another, depending on their partisanship or ideology. This process can be thought of as biasing the first step of the RAS model, in which people receive messages from the information environment. Instead of passively receiving messages from political elites and news media, partisans may actively seek out congenial information and avoid uncongenial information.

Selective exposure is generally thought to stem from individual preferences for certain kinds of information, which has been facilitated by increasing choice over media in recent decades (e.g., Iyengar & Hahn, 2009; Stroud, 2010). Partisans are free to choose information sources that reinforce their prior attitudes or minimize the chance of seeing conflicting views (e.g., Mutz, 2006; Garrett, 2009; Levendusky, 2013a). Distortions in the information environment itself may also result in selective exposure. For example, when reporting economic news, the media tend to emphasize negative information more than positive information, producing a negativity bias in public perceptions (Soroka, 2006). Partisan selective exposure may also occur involuntarily through informal networks or local information (Ansolabehere et al., 2011).

The end result of selective exposure is that Democrats and Republicans do not see the same sets of facts as each other. Thus, even without selective learning, partisan gaps in factual beliefs would occur because of a biased input process. Studies finding selective exposure have fueled worries about partisan echo chambers and ideological bubbles. On the one hand, this is normatively troubling. On the other, if partisan bias is mainly due to selective exposure, then simply exposing partisans to accurate information should reduce partisan bias significantly. In
this way, selective exposure is more of an institutional issue than selective learning, which is primarily a problem of individual-level motivations.

There have been important qualifications of the selective exposure hypothesis in recent years. In a review of the literature, Prior (2013) raises several questions about the polarizing nature of selective exposure. One of the issues is that it is very challenging to measure directly. Studies often rely on self-reported information consumption behavior. These estimates of selective exposure tend to be upwardly biased, often due to inaccurate recall in self-reported data (Prior, 2009). (Selective reporting may again be at play here.) Selective exposure likely occurs among the 10-15 percent of the electorate that consumes cable news today, but its substantive impact may be limited. For instance, the main effect of partisan media may be to further polarize partisan extremists (Levendusky, 2013b).

Advances in directly measuring information seeking in naturalistic settings have also qualified early evidence of selective exposure. Multiple studies passively track what information web users are seeing online. For example, Gentzkow and Shapiro (2011) find that ideological selective exposure is quite limited with respect to online news, though more biased than offline news consumption. Guess (2018) finds that most people have fairly balanced information diets online, and selective exposure is concentrated among a smaller group of ideologues. Generally speaking, behavioral data yield less evidence of partisan selective exposure than surveys and lab experiments do (Guess, Lyons, Nyhan, & Reifler, 2018).

I do not measure selective exposure in this dissertation. In the experiments that follow, I bracket the issue by holding information exposure constant across participants. Selective exposure is almost certainly *partly* responsible for the partisan gaps we observe; however, it is beyond the scope of this dissertation, which focuses on selective learning and reporting.
**Misinformation and Misperceptions**

Another active branch of research concerns misinformation and misperceptions among the public. Misinformation refers to false or inaccurate information that exist in the environment. Misperceptions refer to false or inaccurate factual beliefs, which are often (but not necessarily) the result of receiving misinformation. Flynn et al. (2017) offer a useful definition that specifies that misperceptions are beliefs that “contradict the best available evidence in the public domain” and may or may not be demonstrably false. Some examples are the false or unsubstantiated beliefs that Iraq was hiding weapons of mass destruction in 2003 (Nyhan & Reifler, 2010); that the Affordable Care Act would produce “death panels” (Nyhan, 2010); and that Barack Obama was not born in the United States (Berinsky, 2018).

While misinformation certainly threatens the quality of public opinion, recent work suggests that the concern about “fake news” is somewhat overblown. For example, Allcott and Gentzkow (2017) estimate that the average U.S. adult saw and remembered only one fake news story in the months before the 2016 presidential election. Combining self-reported and online behavioral data, Guess, Nyhan, and Reifler (2018) estimate that about one in four Americans visited a fake news website over a similar time frame. And consistent with previous finding on online selective exposure, they find that most visits to fake news sites occurred among a small group of right-leaning users (i.e., those in the 90th percentile or above, in terms of the conservative slant of their information diet).

An important feature of misperceptions is that they are held with greater certainty than other beliefs. In this way, misperceptions are qualitatively different than ignorance about a particular topic, which is marked not having any relevant beliefs (Kuklinski et al., 2000).

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11 As a matter of terminology, *disinformation* is purposefully inaccurate or deceptive, while misinformation is not necessarily so.
Misperceptions are more problematic from the perspective of citizen competence, as false, confidently held beliefs are harder to correct than ignorance or softly held beliefs are. Fortunately, surveys likely overstate the prevalence of confidently held false beliefs (Schuman & Presser, 1980; Luskin & Bullock, 2011; Graham, 2018; Luskin & Sood, 2018), and ignorance appears to be more common (Pasek et al., 2015). Susceptibility to fake news may also be due to cognitive laziness more than motivated reasoning (Pennycook & Rand, 2018).

Misperceptions are one possible source of the partisan gaps in factual beliefs, but not the only one. For example, when asked about the current unemployment rate, which is close to 4 percent, some Democrats might confidently believe that it is over 5 percent. This misperception would result in fewer Democrats than Republicans giving the correct figure, producing a partisan gap. However, ignorance could have the same effect. Many people may not have firm beliefs about the unemployment rate, and a partisan gap could also emerge from Republicans reporting the correct rate and Democrats overestimating it or saying that they do not know.

**Factual Corrections and Backfire Effects**

A related line of research explores whether misperceptions among the public can be corrected through the provision of accurate information. Most of this work has used a similar experimental paradigm, in which researchers instill false beliefs into experimental participants and subsequently treat them with corrective information.\(^\text{12}\) (This is of course a different process than giving people information about a topic on which they do not have firm beliefs.) The effectiveness of factual corrections can be gauged by measuring these participants’ post-

\(^{12}\) Nyhan and Reifler test partisan resistance to factual information in the context of two-sided information flows. They instill false factual beliefs in respondents through the use of misleading news stories and then try to reduce misperceptions with a corrective story. While this design feature lends external validity to their study, I conduct a stricter test of selective learning by using a one-sided information context without contradictory facts.
treatment beliefs and comparing them to beliefs among a control group. Moreover, partisan gaps in treatment and control groups can be compared to test whether corrections reduce gaps in factual beliefs.

Using this strategy, Nyhan and Reifler (2010) find substantial resistance to fact checks. For example, student participants inclined to believe that Iraq had weapons of mass destruction before the U.S. invasion do not update their beliefs when they read that weapons inspectors in fact found no evidence of this. In some cases, factual corrections backfired, causing participants to become even more convinced of their erroneous beliefs. This behavior is even more extreme than selective learning, which simply predicts that partisans are more likely to learn congenial facts than uncongenial ones. When backfire occurs, people update their beliefs in the opposite direction of the facts, a tendency noted early on by Lazarsfeld et al. (1944). If true, factual corrections would polarize the public even further.

Follow-up studies have often failed to replicate the backfire effect (e.g., Weeks, 2015; Hill, 2017) or only observe it under narrow conditions (e.g., Nyhan et al., 2013). Nyhan et al. (2017) find that correcting Donald Trump’s false claims about crime and unemployment reduced misperceptions during the 2016 campaign, even among his supporters. Porter et al. (2018) also find that correcting fake news stories can reduce misperceptions without backfire. Guess and Coppock (2018) conduct a series of survey experiments in which they present factual information about gun control, wage increases, and the death penalty to respondents; they find no evidence of backfire. Wood and Porter (2019) conduct the most comprehensive test of the backfire hypothesis to date, finding that among 52 issues, only one triggered backfire – Nyhan and Reifler’s (2010) original item on weapons of mass destruction in Iraq. On balance, the
evidence suggests that corrective information can indeed reduce misperceptions and that backfire occurs rarely, if at all. Backfire has only been replicated on a few politically salient issues.

Even though backfire effects are rare, much of the research above is consistent with selective learning. Porter et al. (2018) find that partisans are not as responsive to uncongenial corrections. Similarly, Wood and Porter (2019) find that across most of the issues they tested, corrective information was more successful when it was congenial. Some studies on corrective information measure belief change (e.g., Nyhan & Reifler, 2010), while others measure attitudinal change (e.g., Thorson, 2018), and others both (Guess & Coppock, 2018; Nyhan et al., 2019). In generally, corrective information is more likely to change factual beliefs than attitudes.

Figure 1.2 displays predicted patterns of results under various theories of factual learning. Panel A displays a stylized pattern consistent with selective learning. Compared to baseline, partisans’ factual beliefs change in response to congenial information more than they do in response to uncongenial information. (Greater values indicate correctly learning facts.) Though the effect of uncongenial information is null in Figure 1.2, it does not necessarily have to be – it must simply have a smaller effect than congenial facts to be consistent with the selective learning hypothesis. By contrast, Panel B indicates what evenhanded learning looks like: partisans learn congenial and uncongenial facts at about equal rates. Lastly, Panel C show hypothetical results consistent with backfire. Uncongenial information causes belief updating in the wrong direction.
Figure 1.2. Selective Learning, Evenhanded Learning, and Backfire Effect Hypotheses

Note: Figure plots hypothetical effect of congenial and uncongenial information on factual beliefs among partisans. Y-axis is scaled such that greater values indicate greater uptake of correct information, i.e., factual learning.
Source Cues and Perceived Credibility

Two other related concepts warrant mention. Source cues are an important feature of information that may affect which factual claims partisans find credible and therefore accept (e.g., Zaller, 1992). Many studies find that partisans rely heavily on source cues when processing information, including factual corrections. For example, Bisgaard and Slothuus (2018) find that party elites substantially influence partisan gaps in beliefs about the economy. Berinsky (2017) finds that factual corrections emanating from an in-party member are more effective than those from out-party sources (see, e.g., Nicholson, 2012 for a contrary finding). Swire et al. (2017) find that the original source of a misperception conditions the effectiveness of subsequent attempts to correct it. For instance, attributing a false statement to Donald Trump makes it harder to correct among Republicans than if it were presented without a source. Esberg and Mummolo (2018) find that officials can undermine credibility of official crime statistics, inhibiting learning among co-partisans.

One reason source cues matter is that they allow partisans to engage in heuristic processing (Rahn, 1993). Party cues serve as mental shortcuts for partisans to figure out the credibility of an information source. Such cues are especially important for the less politically aware (Kam, 2005; Anson, 2018). Political sophisticates, on the other hand, tend to exhibit more effort when processing information and may therefore rely on cues less or employ them in more sophisticated ways (e.g., Lodge & Hamill, 1986). Alt et al. (2014), for example, find that political sophisticates find information about the economy more credible when they are made by a party with incentives to state the opposite. Cues relating to political elites can also counteract partisan bias. For example, Druckman et al. (2013) find that signaling elite consensus around an issue inhibits motivated reasoning. There is some debate about whether the opinions of party elites or co-partisan peers are most influential on individuals (e.g., Toff & Suhay, 2018).
Because source cues are likely to interact with individual motivations, I generally try to keep the information source neutral in the experiments that follow. I present factual information that originates with non-partisan government agencies, generally speaking. I recognize that this is not a perfect solution, due to partisan differences in trusting various institutions, but a systematic examination of source cues it out of scope for this project.

**Partisan Asymmetries**

One final area of research that this dissertation speaks to is partisan asymmetries. Going at least as far back as Adorno et al. (1950), scholars have argued that the political right and left are associated with distinct personality traits that moderate their response to political information. Some scholars argue for the “rigidity of the right hypothesis” that conservatives are more likely than liberals to exhibit motivated resistance to information (e.g., Jost et al., 2003; Jost & Krochik, 2014). This left-right asymmetry is explained by individual differences in personality and cognitive style (Johnston et al., 2017). Other scholars simply document partisan asymmetries in information processing, even when asymmetry is not the main focus (e.g., Nyhan & Reifler, 2010; Bullock, 2011; Miller et al. 2016; Marietta & Barker, 2019). For example, McAvoy and Enns (2010) find that Republicans drove polarization in presidential approval ratings beginning in the George W. Bush presidency by no longer incorporating economic information into their ratings.

However, on balance, the empirical evidence on this point is decidedly mixed (see Kahan, 2016, for a review). In a meta-analysis of 51 experiments, Ditto et al. (2019) find that liberals and conservatives exhibit no difference in mean levels of bias across studies. Across several studies, Marietta and Barker (2019) only find a few instances of left-right asymmetry in misperceptions and resistance to corrective information. One concern with make generalizations
about conservative and liberal personality types is the possibility of substantially different intra-party dynamics (e.g., Ura and Ellis, 2012). The differences in liberal and conservative information environments may explain findings by Guess (2018) and others of ideological asymmetry in consuming information online. I do not explicitly test the asymmetry hypothesis. However, in the empirical work that follows, I note a few instances of asymmetries in selective learning and reporting.

**Factual Information and Attitude Change**

In addition to examining selective learning and reporting, I consider whether partisans respond to factual information by updating relevant attitudes appropriately. From a normative standpoint, one of the main reasons that we should be concerned about factual beliefs is that they are presumed to influence politically consequential attitudes, such as evaluations of leaders and policy preferences. There is some evidence for this proposition. For example, Gilens (2001) finds that giving people factual information about a specific policy powerfully shapes their support for that policy. It may also be the case that providing partisans with factual information about a policy, such as how it has performed, reduces partisan gaps in policy support. Along these lines, Bullock (2011) finds that giving partisans substantive information about a policy reduces their reliance on party cues in forming opinions about the policy.

The psychological literature yields starkly different theoretical expectations for attitudinal change in response to factual information. More specifically, motivated reasoning suggests that partisans are more likely to update their attitudes in response to congenial facts than uncongenial ones. When evaluating congenial claims, motivated reasoners tend to search for evidence only partially, seizing on confirmatory facts (e.g., Kruglanski & Webster, 1996; Nickerson, 1998). They are also more likely to evaluate the available evidence in a superficial manner (e.g.,
Chaiken & Maheswaran, 1994). These processes would cause partisan motivated reasoners to uncritically use information that reflects positively on their party to update their attitudes. Attitude change can be distinct from learning a piece of information itself. For instance, Democrats and Republicans may be equally likely learn about a decrease in unemployment under President Obama; however, Democrats may be more likely than Republicans to consider this fact to determine whether they approve of Obama’s handling of the economy.

Motivated reasoners tend to treat uncongenial claims with skepticism and become more likely to engage in effortful processing and counter-argue (e.g., Ditto & Lopez, 1992; Ditto et al., 1998; Dawson et al., 2002a). They may adopt various strategies to avoid updating their attitudes in an uncongenial direction when they learn an uncongenial fact. Consider Republicans who learn that national unemployment has decreased under President Obama. They may doubt the credibility of this fact (e.g., by questioning the impartiality of the Bureau of Labor Statistics). They may believe the fact but apply a more stringent standard of performance (e.g., by thinking unemployment is still too high). Or they may believe that the unemployment situation has indeed changed from bad to good, but attribute this change to causes outside of Obama’s administration. Since there is much room for differing interpretations of facts in politics (see Gaines et al., 2007), partisans may avoid updating attitudes in various ways if they learn uncongenial facts.

Thus, contrary to the normative benchmark of attitudinal change with factual information, the rich literature on motivated reasoning suggests that partisans selectively update attitudes. In a canonical study, Lord, Ross, and Lepper (1979) demonstrate that people are more likely to favor information in line with their prior attitude on an issue. In the aggregate, this behavior results in attitude polarization, whereby people view the same information (e.g., evidence on the efficacy of the death penalty), yet diverge in their attitudes (e.g., support for
capital punishment). Though this study has been critiqued on methodological grounds (see, e.g., Miller et al., 1993; Guess & Coppock, 2018), subsequent research replicates the prior attitude effect and attitude polarization in a variety of domains, such as candidate evaluations (Redlawsk, 2002) and framing (Taber & Lodge, 2006), that is exacerbated by elite polarization (Druckman et al., 2013).

Studies on the link between facts and political attitudes also yield mixed results. Observational studies, such as Jacobson (2010), usually lack clean identification. Some prominent experiments find significant effects of facts on attitudes (e.g., Gilens, 2001; Bullock, 2011; Anglin, 2019), while others find that attitudes are very resistant to change (e.g., Kuklinski et al., 2000; Berinsky, 2007; Druckman & Bolsen, 2011). For instance, across seven survey experiments over 11 years, Hopkins et al. (2019) find that factual information does not substantially affect immigration attitudes, though it does improve the accuracy of factual beliefs about the size of the foreign-born population in the U.S. Many other studies employing factual information to correct misperceptions find belief updating with little to no attitudinal change (e.g., Nyhan & Zeitzoff, 2017; Thorson, 2018; Nyhan et al., 2019).

The Bayesian Ideal

Many scholars explicitly or implicitly use a Bayesian ideal when studying attitudinal change among partisans. These scholars take Bayesian updating as a normative benchmark and examine whether real-world attitude change meets this standard. That is, when partisans receive facts, do they update their attitudes in a more or less Bayesian way? Much ink has been spilled debating whether partisans ought to converge in their attitudes when exposed to the same facts (see, e.g., Gerber & Green, 1998; Gerber & Green, 1999; Bartels, 2002; Bullock, 2009). I do not enter this debate, however, because the Bayesian framework is flexible enough to accommodate
a variety of behaviors that may not be normatively desirable, such as a prior attitude effect (e.g., Bullock, 2007; Lauderdale, 2016; Hill, 2017; Guess & Coppock, 2018) and even belief polarization. Because of its flexibility, Bayesian updating is not the most useful benchmark. As Bartels (2002, p.126) puts it:

“…it seems very hard to think of Bayesian consistency as a sufficient condition for rationality in the sense of plain reasonableness. Opinion change in accordance with Bayes’ rule may often be biased, and in extreme cases it may approach delusion, as long as it does not manifest internal contradictions.”

Instead of adopting the Bayesian ideal, I apply a commonsense understanding of “rational” attitude change. I assume that it is normatively desirable to update relevant attitudes in response to both congenial and uncongenial information. However, most of the psychological literature suggests that directionally motivated partisans will be more likely to update attitudes in response to congenial facts than uncongenial facts. I refer to this alternative as selective updating. In an extreme case, partisans will not respond to uncongenial facts at all or perhaps update in the opposite direction, causing a “backfire” effect (Nyhan & Reifler, 2010).

Figure 1.3 plots the expected pattern of attitude change under rational updating as defined above (left panel) and selective updating (right panel).

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13 For example, scholars have demonstrated that belief polarization is compatible with Bayesian updating by introducing background information as another variable affecting posterior beliefs (Jern et al., 2014), disagreement over the likelihood function specifying how new facts are combined with prior beliefs (Benoît & Dubra, 2016), and an additional interpretive step before new information can be combined with priors (Fryer et al., 2017).

14 Indeed, this standard is compatible with Bayesian updating, given a few reasonable assumptions: namely, that the facts on the ground are not changing dramatically (Bullock, 2009) and that partisans do not disagree wildly about the likelihood function (Guess & Coppock, 2018).
Figure 1.3. Expected Pattern of Results under “Rational” and Selective Updating

Note:

*Figure plots hypothesized effects of congenial and uncongenial facts on relevant attitudes among partisans. Attitude change is scaled such that greater values indicate updating in a more congenial direction.*

**Scope of Project**

Barack Obama’s quote from the beginning of this chapter touches on multiple topics I explore in this dissertation. Do people hold similar factual beliefs to each other (i.e., “some common baseline of facts”)? Do people learn politically relevant information in an evenhanded or selective manner (i.e., “admit new information”)? Finally, are people willing to revise their attitudes in response to uncongenial information (i.e., “concede that your opponent is making a fair point”)?

Much of the previous research attempting to answer these questions suffers from a few limitations that I try to address. For example, past work on selective learning rarely manipulates the congeniality of factual information cleanly and rarely measures learning in the long term. Research on partisan gaps in economic perceptions often suffers from a conceptual messiness, lumping together retrospective evaluations, qualitative judgments, and future expectations. I elaborate on each of these issues in the subsequent chapters, but for now I note that they prevent
us from achieving a more complete understanding of partisan bias in factual beliefs. This project attempt to address the three main questions: How prevalent is partisan bias in factual beliefs? What are the mechanisms that give rise to partisan gaps in surveys? (Here I focus on selective learning and selective reporting.) Finally, how do factual beliefs affect downstream political attitudes, if at all?

A Note on Terminology

The terms “factual belief” and “perception” are often used interchangeably in the existing literature. I use the term “factual belief” to refer to a belief about an objectively verifiable truth about the world. I generally prefer this term to “perception” because of the disparate ways the latter has been used in political science. Many scholars have used the term “perception” to refer to non-factual attitudes, such as subjective impressions of candidates (Lau, 1982; Sigelman et al., 1995), inferences about candidates’ issue positions (Feldman & Conover, 1983), perceived differences between the parties (Conover, 1984), subjective perceptions of media bias (Watts et al., 1999), and even expectations about which candidate would win an election (Nir, 2011). However, I use “perceptions” occasionally to refer to estimates of numerical figures and other beliefs about the economy. My general approach in the studies that follow is to operationalize factual beliefs as perceptions of common indicators, like the unemployment rate or percent of Americans without health insurance. I occasionally refer to such a factual belief as “knowledge” when it is correct.\(^{15}\) I distinguish factual beliefs from prospective beliefs, which also exhibit partisan bias and wishful thinking (e.g., Gerber and Huber 2009, 2010; Niemi et al., 2019). I do

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\(^{15}\) Even if one disagrees about the extent to which these indicators are truly knowable, or if they are merely estimates with inherent uncertainty, the public may still have relevant factual beliefs about them. It is instructive to see how these beliefs differ between groups.
not consider prospective beliefs to be factual, because they concern future conditions and therefore cannot be objectively verified.

**Operationalizing Bias**

One of my primary goals is measuring “partisan bias” in factual beliefs. I use more than one operationalization of bias in the pages that follow. In Chapter 2, I look at partisan gaps, which are the difference between Democrats and Republicans for a given belief. For example, I measure the difference between Democrats and Republicans in the propensity to report an unemployment rate that is higher than the true value (which is an incorrect factual belief). Occasionally, I measure the difference in the probability of reporting congenial and uncongenial beliefs, conditional on party identification. This quantity reflects a bias in what survey responses partisans tend to give, conditional on reporting an incorrect belief. And in the experiments in Chapters 3 and 4, I look at the difference in reporting a correct factual belief between congenial and uncongenial treatment conditions. This quantity reflects a bias in which facts partisans learn.

**Chapter Summaries**

In Chapter 1, I discuss theories of partisan bias in factual beliefs and evaluate supporting evidence in extant research. Along the way, I discuss related concepts, such as misinformation, backfire effects, and source cues, situating my research topic in the broader literature and clearly delineating its scope.

In Chapter 2, I measure the extent of partisan bias in beliefs about a variety of objective economic indicators. I build a database of economic perceptions by searching national polling data since 1980 for factual questions about the economy. I find that partisan bias is less severe than past work has suggested, but also that there is considerable heterogeneity in bias across different administrations and changing economic conditions.
In Chapter 3, I test a prominent and troubling theory of partisan bias: namely, selective learning of factual information. In a series of survey experiments, I expose partisans to facts with either positive (congenial) or negative (uncongenial) implications for their party. There is little evidence of selective learning across these studies. Instead, I find that partisans learn facts fairly evenhandedly. In a panel study, I find that while many respondents forget factual information over the course of several days, they are equally likely to recall congenial and uncongenial information.

In Chapter 4, I discuss recent evidence for an alternate explanation of partisan gaps observed in surveys: selective reporting of factual beliefs. This evidence comes from a set of experiments that nudge survey respondents to report their beliefs accurately in response to factual questions, without providing respondents with any additional information.

In Chapter 5, I examine the relationship between factual information and “downstream” attitudes, such as incumbent evaluations and policy preferences. These politically consequential attitudes are the focus of much of the extant literature on motivated reasoning. Contrary to theories of motivated reasoning, partisans assimilate congenial and uncongenial facts into relevant political attitudes, such as incumbent evaluations and policy preferences. Furthermore, attitudinal changes persist several days after initial exposure to factual information. However, incentivizing respondents to counter their bias in reporting factual information can have adverse effects on their subjective attitudes.

Chapter 6 offers concluding remarks, summarizing the main findings from the previous chapters and their implications. I note limitations of the empirical work and offer a few avenues for future research.
Chapter 2. Measuring Partisan Gaps in Reported Beliefs

If having the correct facts is important for a functioning democracy, we would benefit from a more complete understanding of the severity of partisan bias in factual beliefs and the conditions that exacerbate and mitigate the problem. As discussed in the previous chapter, inaccurate beliefs about the economy in particular may affect policy preferences and electoral decisions, and undermine democratic accountability (e.g., Holbrook & Garand, 1996).

In this chapter, I document the extent of partisan bias in perceptions of the U.S. economy. To do so, I construct a unique database of factual questions about the national economy that were administered in nationally representative surveys since 1980. I collected 78 factual questions from 50 surveys and approximately 80,000 respondents. I focus on factual questions about four economic indicators: the national unemployment rate, federal budget deficit, inflation rate, and Dow Jones Industrial Average. This large database gives a bird’s eye view of what partisans say about the U.S. economy, along with differences in factual beliefs between the left and right, over a long stretch of time.

I improve upon previous studies of partisan bias in the economy in several ways. First, I only analyze questions with correct and incorrect answers, comparing responses to the truth. Second, each question has ramifications for the health of the economy and the performance of the incumbent administration. This enables the categorization of incorrect responses as congenial either to the Democratic Party or Republican Party, which is a necessary for measuring partisan bias. Furthermore, since the database spans more than three decades, I include data from multiple presidential administrations and changing economic circumstances, from periods of high

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16 Material from this chapter was presented at the Midwest Political Science Association Annual Conference.
inflation and joblessness to periods of growing prosperity and shrinking deficits. By pooling questions from different surveys, I arrive at a more representative estimate of the severity of partisan bias than most studies, which focus on a single administration or shorter period time. Finally, the breadth of the data enables an exploration of contextual variables that may affect partisan bias, such as party control and issue ownership.

I do not adjudicate among the multiple mechanisms that may give rise to partisan perceptual bias in this chapter. My aim in this chapter is descriptive: I use observational data to establish the degree of partisan bias in a politically important factual beliefs. It is thus analogous to the control condition in most experimental inquiries into selective learning. I next discuss past work on partisan bias in economic perceptions and then describe my database and method for measuring bias in greater detail.

**Canonical Examples of Partisan Bias**

One of the most frequently cited examples of partisan bias in factual beliefs is Bartels’ (2002) analysis of economic perceptions under Presidents Reagan and Clinton. Bartels finds that most Democrats incorrectly reported rising unemployment and inflation under Reagan, while most Republicans reported a worsening economy under Clinton. For example, survey respondents were asked whether the level of unemployment had gotten better, stayed the same, or worsened from 1980 to 1988. In truth, unemployment had decreased nationally during this period. However, only 30 percent of Democrats correctly answered the question, while the vast majority of Republicans correctly reported an improvement in the unemployment situation. The opposite was true under President Clinton, when Republicans were less likely than Democrats to report favorable changes in the economy. These dramatic findings suggest that partisans do not agree on basic information about what has happened.
The partisan gaps that Bartels reports, especially the pattern of responses under Reagan, are substantively large; however, how representative or these data of partisan gaps more generally? Bartels speculates that the survey instrument may have primed partisan loyalty by measuring feelings about Reagan prior to the economic knowledge questions, which may have induced greater bias. The severity of partisan bias in factual beliefs may also depend on the actual state of the world, including the objective state of the economy, levels of mass and elite polarization, and which party is in power. In order to achieve a broader understanding of partisan bias in factual knowledge, one must take a broader view of different surveys, topics, and time periods.  

Jerit and Barabas (2012) conduct one of the most comprehensive tests of partisan bias to date. Combining public opinion data from 1993 to 2006, they find partisan bias in factual beliefs about health care, foreign policy, social services, and a variety of other issues. However, Jerit and Barabas’ analysis suffers from two major issues. First, they measure knowledge among Democrats and Republicans using two different sets of items. This decision may lead to an inaccurate measure of partisan bias if one set of items is inherently more difficult than the other. In order to measure partisan bias, one must compare Democrats and Republicans on the same items. Second, most of the items Jerit and Barabas collected do not have positive or negative implications for the major parties, thereby precluding a test of partisan perceptual bias. Instead, most of the items in their database reflect information that is consistent or inconsistent with party stereotypes. Their main finding thus reflects partisans’ tendency to hold beliefs that are

\[\text{17}\]\n
Jones (2019) provides an updated analysis, including more recent ANES survey items. However, by examining only retrospective evaluations, this analysis is similarly limited. Moreover, the analysis pools economic and non-economic items (e.g., about America’s standing in the world) that are somewhat subjective in nature.
consistent with the positions and actions they expect parties to take (Lodge & Hamill, 1986; Rahn, 1993). This behavior is distinct from the tendency to hold congenial beliefs at the expense of uncongenial beliefs.

In addition to issues with existing work, there is reason to believe that the case for partisan bias is overstated. Some studies find that partisans on the left and right actually converge in their factual beliefs, particularly when there is an unambiguous signal from the information environment, such as a severe economic downturn (Gaines et al., 2007; Blais et al., 2010; Parker-Stephen, 2013; Bisgaard, 2015).

In order to assess the degree of partisan bias across different issues and over a long period of time, I bring as much data to bear on the question as possible. I searched publicly available survey data for all factual questions about the U.S. economy since 1980. I focus on perceptions of the economy, because of their importance in economic voting and forming opinions about government officials and policies. In this chapter, I analyze questions about four objective indicators that are commonly used to evaluate the state of the economy and incumbent performance: the unemployment rate, the federal budget deficit, the inflation rate, and the Dow Jones Industrial Average. Importantly, public perceptions of each indicator can be compared with the objective truth.

Following Bartels (2002), I assume that information about these indicators reflects either positively or negatively on the party controlling the executive branch. This assumption is in line with much of the economic voting literature, as well as research demonstrating the public’s tendency to attribute economic responsibility to the president (Stein, 1990), particularly among those lower in political sophistication (Gomez & Wilson, 2001). Thus, declining unemployment from 1993 to 1996 was a congenial fact to Democrats and uncongenial to Republicans, because it
reflected positively on President Clinton. Similarly, declining inflation from 1982 to 1983 is congenial for Republicans and uncongenial for Democrats. I measure whether partisans are more likely to report congenial beliefs and less likely to report uncongenial beliefs than members of the out-party. Were Democrats more likely than Republicans to report high unemployment in 1992? Were Republicans more likely than Democrats to report an increasing deficit in 2010?

As previously discussed, the economic perceptions database enables a broader understanding of the public’s knowledge of the economy than much past work. In addition to analyzing perceptions of multiple indicators over many years, I look at a variety of survey questions asking about the same indicator. Thus, I can be more confident that the level of bias I measure does not depend on a particular survey or question wording. Furthermore, I specifically analyze the difference between two question formats: asking for current estimates of economic indicators and asking for retrospective evaluations. Bartels (2002) only analyzed retrospective evaluations, which generally ask whether conditions have improved, worsened, or stayed the same over a given period of time. Current estimates, on the other hand, may result in less bias because it may be less clear to respondents which responses reflect favorably on their party and which do not, particularly if the question is open-ended.

**Conceptual Messiness in Economic “Perceptions”**

As mentioned in the previous chapter, scholars of public opinion have used the term “perception” to mean many different things. Moreover, when it comes to economic perceptions, scholars have used many different types of survey measures over the years. For example, De Vries et al. (2018) measure economic perceptions with an item asking whether “the general economic situation” had gotten better or worse in the past year. Enns et al. (2012) and Enns and McAvoy (2012) use qualitative ratings from public polls, combining questions
about levels and change to measure perceptions. These items tend to use language like “good” and “bad” (CBS News), “better” and “worse” (ABC News). Such items come close to measuring belief about verifiable fact, but they nevertheless let respondents off the hook. Respondents are free to rely on whatever indicators they want when thinking about the economy, rather than being specific, and therefore, objective.

In this chapter, I eschew qualitative language such as this, unless it can be clearly translated to a numerical change in an indicator. For example, retrospectively asking if the unemployment rate got “better” or “worse” clearly translates to asking whether the rate became “lower” and “higher”. I consider such questions to be factual.

Table 2.1 includes several other examples of survey questions used to measure economic perceptions. Questions differ along several dimensions, including time frame: some items ask about the current state of the economy or current level of an indicator, while others are retrospective or even prospective. Questions also differ by specificity: some ask about general conditions, while other relate to a particular indicator. Finally, some items are qualitative, while others are quantitative. I focus on the latter and note which types of items I use in this chapter in the first column.
Table 2.1. Common Measures of Economic Perceptions in Extant Literature

<table>
<thead>
<tr>
<th>A</th>
<th>B</th>
<th>C</th>
<th>D</th>
<th>Studies</th>
</tr>
</thead>
<tbody>
<tr>
<td>✓</td>
<td>Current</td>
<td>Spec.</td>
<td>Quant.</td>
<td>“Do you happen to know if the Dow Jones Industrial Average is currently closer to 3,000, 6,000, 10,000, or 16,000?” (Pew Research, Oct 2009)</td>
</tr>
<tr>
<td>✓</td>
<td>Retrospective</td>
<td>Spec.</td>
<td>Quant.</td>
<td>“Compared to five years ago, has the federal budget deficit increased, decreased, or stayed about the same?” (Pew Research Center, Aug 1997)</td>
</tr>
<tr>
<td>✓</td>
<td>Retrospective</td>
<td>Spec.</td>
<td>Qual.*</td>
<td>Would you say that over the past year, the level of unemployment in the country has gotten better, stayed about the same, or gotten worse?” (American National Election Studies)</td>
</tr>
<tr>
<td></td>
<td>Retrospective</td>
<td>Gen.</td>
<td>Qual.</td>
<td>“Now thinking about the economy in the country as a whole, would you say that over the past year the nation’s economy has gotten worse, stayed about the same, or gotten better?” (American National Election Studies)</td>
</tr>
<tr>
<td></td>
<td>Retrospective</td>
<td>Gen.</td>
<td>Qual.</td>
<td>“Would you say at the present time business conditions are better or worse than a year ago?” (Michigan Survey of Consumer Attitudes)</td>
</tr>
<tr>
<td></td>
<td>Prospective</td>
<td>Spec.</td>
<td>Quant.</td>
<td>“By what percent do you expect prices to go (up/down) on the average during the next 12 months?” (Survey of Consumer Attitudes)</td>
</tr>
<tr>
<td></td>
<td>Prospective</td>
<td>Gen.*</td>
<td>Quant.</td>
<td>“During the next 12 months, do you think that prices in general will go up, or go down, or stay about where they are now?” (Michigan Survey of Consumer Attitudes)</td>
</tr>
<tr>
<td></td>
<td>Prospective</td>
<td>Gen.</td>
<td>Qual.</td>
<td>“About a year from now, do you expect that in the country as a whole business conditions will be better, or worse, than they are at present, or just about the same?” (Michigan Survey of Consumer Attitudes)</td>
</tr>
</tbody>
</table>

Note: Column A indicates whether item is used here. B indicates if items ask for current estimate or are retrospective or prospective; C indicates if items are specific or general. D indicates whether items are quantitative or qualitative. Asterisks indicate items that are qualitative or general but can be translated to quantitative or specific measures.
Method

Economic Perceptions Database

In order to construct the economic perceptions database, I collected data from the American National Election Studies (ANES) and the iPoll Databank maintained by the Roper Center for Public Opinion Research. I identified 78 items from 50 nationally representative surveys conducted from 1980 to 2013. The surveys were administered by a variety of organizations, including Gallup, Pew Research Center, CBS News, The Los Angeles Times, The New York Times, and The Washington Post. ANES data consists of face-to-face surveys, with a relatively recent addition of an online sample. Of the other surveys I collected, most were conducted by phone (85 percent), while a minority was conducted in person (12 percent) or online (2 percent). The database consists of a total of 80,376 respondents, some of whom were asked about multiple indicators.

In order to identify questions to include in the database, I searched the iPoll in various ways. To reiterate, the goal was to collect as many factual questions about the economy as possible, with a focus on objective economic indicators. Most of the survey questions in the iPoll Databank are subjective in nature and therefore not included. In order to narrow the field of possible questions, I used keyword searches to identify potentially relevant questions and then carefully read each question to check if it was strictly factual in nature. If a question had an objectively verifiable answer, I kept it in the database. In addition to using keyword searches, I also looked at all surveys conducted by organizations that tend to ask about specific topics repeatedly. Additionally, once I identified a relevant survey question, I examined the rest of the

items in that survey, because surveys containing one factual question often contain others. I examined over 2,000 questions in all. Appendix Table A1 provides additional details about the keyword searches I employed.

Figure 2.1 presents a visual overview of the 78 survey questions, which vary in important ways. First and foremost, questions vary by topic: there are 31 questions about unemployment, 17 questions about the deficit, 20 questions about inflation, and 10 questions about the Dow Jones. Questions also differ in their wording and format. Most questions (68) are closed-ended in nature, providing respondents multiple response options to choose from, while the rest (10) were open-ended. Such questions generally ask respondents for their best guess about the value of a particular indicator and usually do not give any benchmarks to anchor respondents’ guesses. Moreover, some items ask for estimates of the current level of an indicator (32), while other questions are retrospective in nature (46). Retrospective items ask respondents to compare the current level of an indicator with its level some specified period of time ago. Finally, most questions (60) are quantitative in nature, asking about the current level or change in a numerical value. Quantitative questions allow a comparison of respondents’ estimates with the true values. Some questions (18) are worded in a slightly more qualitative way, asking whether conditions improved or worsened (rather than asking whether a numerical value increased or decreased). Answers to such questions can also be compared to objective changes in the four indicators. (For example, a response that unemployment has worsened over time corresponds to an increase in the unemployment rate.) Illustrative questions about each of the four indicators are included in the appendix, and all survey questions are listed chronologically by topic in Appendix Tables A2 through A5.
Figure 2.1. Overview of Database Items by Topic

Note: Figure plots number of current and retrospective survey questions included in the database for each year from 1980 through 2013. Background shading indicates Democratic (blue) and Republican (red) presidential administrations. The database includes a total of 78 questions, including 32 current items and 46 retrospective items.

Measure of Partisan Bias

I define partisan bias here as a greater propensity to report congenial beliefs than uncongenial beliefs when asked about an objective economic indicator. I operationalize this concept as the difference between Democrats and Republicans in the probability of giving a particular type of response. For instance, consider the 1988 ANES survey items that Bartels
examines. Democrats were 28 points more likely than Republicans to report rising unemployment and 27 points more likely to report rising inflation under Reagan. I calculate a comparable quantity for each survey item in the database.

Coding Responses

In order to compare partisan bias across different survey questions, I recode responses so that they are on a common scale. I first code each response as a correct answer, an underestimate, or an overestimate. This is straightforward for questions about current levels. For retrospective items, reporting a numerical change in an indicator that is greater than the true change is an overestimate. The reported numerical change may be explicit in the case of a quantitative item or implicit in the case of a qualitative question. For example, reporting that inflation got worse (i.e. increased) or stayed the same, when inflation in fact got better (i.e. decreased), is an overestimate. By the same logic, reporting a numerical change less than the true change is an underestimate.

Recoding responses in this way enables me to compare responses to different types of questions. Furthermore, it solves the problem of dealing with extreme responses. A downside to this approach is a loss of variation in responses to open-ended questions. However, most items are closed-ended (87 percent) and retrospective (59 percent).

Results

The presentation of the results proceeds as follows. I first present descriptive results to provide a rough measure of partisan bias. I next run separate regressions for each item in the database, controlling for respondent characteristics, and then explore heterogeneity across items.
I then calculate the average level of bias for each indicator and look at differences between Democratic and Republican administrations.

**Descriptive Results**

I begin by presenting descriptive results about respondents’ beliefs about each of the four economic indicators. The data suggest that there is partisan bias in perceptions of most indicators, but the results are not consistent across items on a given topic. While these analyses do not control for variables that may be correlated with partisanship and lead individuals to give congenial responses, they nonetheless provide an initial, rough measure of bias.\(^\text{19}\)

Figure 2.2 displays the percentage of Democrats (blue points), Republicans (red points), and independents (purple points) overestimating the current unemployment rate in response to fifteen survey items, which are labeled along the vertical axis. The partisan gap—defined as the percentage of Democrats overestimating minus the percentage of Republicans overestimating—is indicated on the far right of the figure, with standard errors in parentheses.

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\(^{19}\) Independents are included as a benchmark, because they are less likely than partisans to report party-congenial beliefs. I treat independents that lean toward one of the parties as independents rather than partisans, because not all surveys asked a follow-up question to distinguish party leaners from pure independents.
Figure 2.2. Percentage of Respondents Overestimating Current Unemployment Rate

<table>
<thead>
<tr>
<th>Date</th>
<th>Gap: D-R</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pew 5/2012</td>
<td>3.3 (2.2)</td>
</tr>
<tr>
<td>Pew 9/2011</td>
<td>13 (3.4)</td>
</tr>
<tr>
<td>Pew 3/2011</td>
<td>0.8 (2.3)</td>
</tr>
<tr>
<td>Pew 7/2010</td>
<td>1.8 (4.3)</td>
</tr>
<tr>
<td>Pew 1/2010</td>
<td>6.7 (4)</td>
</tr>
<tr>
<td>Pew 10/2009</td>
<td>5.5 (4.4)</td>
</tr>
<tr>
<td>UConn 4/2009</td>
<td>7.8 (4.6)</td>
</tr>
<tr>
<td>Pew 3/2009</td>
<td>7.8 (3.8)</td>
</tr>
<tr>
<td>FSRA 6/2008</td>
<td>5.5 (4.4)</td>
</tr>
<tr>
<td>Rutgers 8/1998</td>
<td>16.7 (4)</td>
</tr>
<tr>
<td>Washington Post 10/1996</td>
<td>3.8 (3.7)</td>
</tr>
<tr>
<td>Kaiser 7/1996</td>
<td>-5.1 (3.2)</td>
</tr>
<tr>
<td>LA Times 8/1982</td>
<td>12.5 (4.5)</td>
</tr>
<tr>
<td>CBS/NYT 4/1980</td>
<td>5.5 (4.4)</td>
</tr>
</tbody>
</table>

Note: Figure displays percentage of Democrats (blue), Republicans (red), and Independents (purple) overestimating current unemployment rate. Partisan gap is Democratic percentage minus Republican percentage with standard errors in parentheses. Background shading indicates party of president.
The evidence for partisan bias is mixed. Past work suggests that the partisan gap will be negative under Democratic presidents (1996-1998 and 2009-2012) and positive under Republican presidents (1980-1982 and 2008). However, with the exception of one year (1996) the partisan gap is consistently positive (range: 1 to 17 points), indicating that Democrats are more likely than Republicans to overestimate the current unemployment rate. While generally positive, the partisan gap is only significantly different from zero for 5 out of the 15 questions. As expected, independents usually fall between Democrats and Republicans.

Retrospective items on unemployment provide clearer evidence of partisan gaps. These questions generally ask respondents to compare the current unemployment rate to the rate one year previous.

Figure 2.3 displays the percentage of partisans overestimating changes in the unemployment rate over time, which means the percentage of respondents who said the unemployment rate increased when it in fact decreased or stayed the same.\textsuperscript{20} Seven of the 11 questions yield significant partisan gaps. All of statistically significant gaps are in the predicted direction, indicating that partisans overestimate changes in unemployment when the out-party is in power. The gaps are substantial in magnitude, reaching almost 40 points in some cases. The exceptions to this pattern are four questions asked during the Clinton administration, when Democrats and Republicans were equally likely to overestimate changes in unemployment.

\textsuperscript{20} In some cases, the percentage displayed is the percentage of respondents saying the unemployment rate increased or stayed the same when it in fact decreased.
Figure 2.3. Percentage of Respondents Overestimating Changes in Unemployment

Note: Figure displays percentage of Democrats (blue dots), Republicans (red dots), and Independents (purple dots) overestimating change in unemployment rate, i.e. reporting increasing unemployment when unemployment had in fact decreased or stayed the same.

Figure 2.4 provides further evidence of partisan gaps, this time plotting the percentage of respondents underestimating unemployment in response to retrospective questions.\textsuperscript{21} Because all five of these questions occurred during Republican presidencies (1983, 1992, 2003, and 2008), I would expect Republicans to underestimate unemployment more than Democrats. This is exactly

\textsuperscript{21} I plot percentage underestimating only for those closed-ended items in which an overestimate was impossible, e.g., because the actual unemployment rate increased.
what I find with all five items yielding significantly negative partisan gaps. The gaps are again substantial in magnitude, ranging from 7.5 to 33 percentage points.

**Figure 2.4. Percentage of Respondents Underestimating Changes in Unemployment**


*Note: Figure displays percentage of Democrats (blue dots), Republicans (red dots), and Independents (purple dots) underestimating change in unemployment rate, e.g. reporting decreasing unemployment when unemployment had in fact increased or stayed the same. Poll marked with an asterisk did not include measure of partisanship, so I used a measure of presidential approval instead.*

I present similar descriptive results for the survey questions about the deficit, inflation, and the Dow Jones in Appendix Figures A1-A5. These results reveal some interesting asymmetries between Democratic and Republican administrations. I observe partisan gaps in perceptions of the federal deficit under Democratic administrations. For instance, Republicans were 7 to 18 points more likely than Democrats to overestimate the deficit during Democratic administrations. However, the difference between Republicans and Democrats is essentially null during Republican administrations. The inflation data also yield mixed results with respect to the
partisan bias. Under Democratic presidents, there are no significant partisan gaps in the probability of overestimating inflation. However, all of the items assessing perceptions of inflation under Republican presidents yield large partisan gaps in the expected direction (range: 12 to 35 points). Finally, items asking about the Dow Jones Industrial Average yield consistent but unexpected results. With the exception of one item, Democrats are always more likely than Republicans to underestimate the Dow Jones, regardless of which party controls the White House (difference range: 1 to 14 points).

**Item-Level Regressions**

The descriptive analyses uncovered considerable heterogeneity in the size of partisan gaps across survey items. For a given topic, some items elicited substantial partisan bias, while others did not. In order to explore item-level heterogeneity in partisan bias, I run a separate ordered probit regression for each survey question, controlling for respondent education, income, race, and gender.\(^{22}\)

The ordered probit framework is appropriate given the ordinal outcome variable, which can take one of three values: overestimates, correct answers, and underestimates. In order to compare responses across Democratic and Republican presidents, I take an additional step in coding responses. I simply reverse code the dependent variable under Democratic administrations and leave it as is under Republican administrations. The end result is a variable with the following three categories: Republican-congenial errors (0), correct answers (1), and Democratic-congenial errors (2).

\(^{22}\) A few polls did not contain each of these respondent-level variables, in which case I controlled on the variables that were available.
Consider responses to survey questions about unemployment, which are depicted in Table 2.2 below. As depicted in the first row, underestimating unemployment is a Republican-congenial response when Republicans control the presidency, because lower unemployment reflects positively on the party. However, the opposite is true when Democrats are in power: underestimating unemployment would now be congenial to Democrats. Therefore, in order to compare Democratic and Republican respondents on the same scale, responses must be reverse coded under Democratic administrations.23

Table 2.2. Coding Correct Responses and Party Congenial Errors

<table>
<thead>
<tr>
<th>Original Response</th>
<th>Recoded Response under Republican Administrations</th>
<th>Recoded Response under Democratic Administrations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Underestimate</td>
<td>Rep-Congenial Error (0)</td>
<td>Dem-Congenial Error (2)</td>
</tr>
<tr>
<td>Correct Answer</td>
<td>Correct Answer (1)</td>
<td>Correct Answer (1)</td>
</tr>
<tr>
<td>Overestimate</td>
<td>Dem-Congenial Error (2)</td>
<td>Rep-Congenial Error (0)</td>
</tr>
</tbody>
</table>

I use this recoded variable as the outcome in an ordered probit regression and use party identification as the main independent variable. I operationalize party identification as a dummy variable for Democrats (1) with Republicans serving as the reference category (0).24 Because of the way the variables are coded, a positive effect of party identification on the response variable indicates partisan bias: that is, Democrats should be more likely than Republicans to make Democratic-congenial errors and less likely than Republicans to make Republican-congenial errors. By controlling for respondent demographics and combining data on overestimates and underestimates, this analysis produces a more reliable measure of partisan bias for each question.

23 Items about the Dow Jones constitute an exception. They are instead reverse coded for respondents under Republican administrations. The reason is that, unlike the other three indicators, overestimating the Dow Jones reflects positively on the current administration. 24 I exclude independents from the present analysis, because partisan bias does not apply. Strength of party identification was not measured in all surveys, so I do not distinguish strong and weak party identifiers in the analysis.
Figures 2.5 and 2.6 plot the item-level ordered probit results for perceptions of each of the four indicators. Each plot displays the marginal effect of party identification on reporting party-congenial perceptions – these effects are not always positive. These results bolster the findings from the earlier descriptive analyses. We see partisan gaps in perceptions of unemployment and inflation mostly during Republican presidencies, with small to null findings during Democratic presidencies (see Figure 2.5). Partisan gaps in perceptions of the deficit, on the other hand, tend to occur during the 1990s and post-2010, during the Clinton and Obama administrations (Figure 2.6, panel A). The Dow Jones results, on the other hand, indicate a complete lack of partisan bias in perceptions of the stock market after controlling for respondent demographics (Figure 2.6, panel B). None of these marginal effects are statistically significant.

Figures 2.5 and 2.6 also make it clear that retrospective items drive partisan bias in perceptions of unemployment, the deficit, and inflation. For example, 14 out of the 16 retrospective evaluations of unemployment result in significantly positive partisan gaps, while 14 of the 15 current estimates of unemployment result in gaps that are essentially zero. Similarly, 10 of the 12 retrospective questions about the deficit and 15 of the 17 retrospective questions about inflation resulted in significant gaps in the hypothesized direction, while no significant gaps arose from current estimates of these two indicators.

This observation raises the question of whether the different patterns of partisan bias observed during Democratic and Republican presidencies are in fact due to differential implementation of retrospective and current-level items in these periods. Unfortunately, there are not enough current-level items to rigorously test this hypothesis. For example, current estimates of the deficit were only asked in Republican years, and current estimates of inflation only in Democratic years.
Figure 2.5. Effect of Partisanship on Responding Congenially to Questions about Unemployment and Deficit

A. Unemployment

B. Inflation Rate

Note: Figure plots marginal effects of partisanship on congenial responding separately for each item on unemployment (panel A) and inflation (panel B), where y-axis is change in probability of a congenial response. Each effect is estimated from item-level ordered probit regression controlling for education, income, race, and gender whenever possible. Variables are coded such that the expectation is that all effects are positive.
Figure 2.6. Effect of Partisanship on Responding Congenially to Questions about Inflation and Stock Market

A. Federal Budget Deficit

B. Dow Jones

Note: Figure plots marginal effects of partisanship on congenial responding separately for each item on the federal deficit (panel A) and Dow Jones (panel B), where y-axis is change in probability of a congenial response. Each effect is estimated from item-level ordered probit regression controlling for education, income, race, and gender whenever possible. Variables are coded such that the expectation is that all effects are positive.
However, I cautiously offer a few interpretations of the available data. First, I observe partisan gaps in some retrospective items but not others, suggesting that partisan bias is not driven solely by question format. Second, I collected current estimates of the unemployment rate in both Democratic and Republican years. These items yielded gaps in the expected direction under Republican but not Democratic administrations (Figure 2.5, panel A). This finding suggests that contextual variables other than item wording, such as the party of the president and issue at hand, also affect the level of partisan bias.

**Average Partisan Gaps by Indicator**

I next pool survey questions and estimate the average partisan gap for each of the four economic indicators. I fit separate models for unemployment, the federal budget deficit, inflation, and the Dow Jones. As discussed previously, items differed in response format and wording, not to mention difficulty. Moreover, I compare items from different polls conducted at different point in time.\(^{25}\) Therefore, I include item fixed effects in each model. Doing so essentially estimates the difference between Democrats and Republicans for each item and then averages these differences to calculate the overall effect of party identification.

Figures 2.7 and 2.8 display predicted probabilities generated from these models for Republican congenial, Democratic congenial, and correct responses. Probabilities are calculated for Democrats and Republicans, and the partisan gap is displayed at the top of each figure. Partisan gaps in perceptions of unemployment are about 14 points on average (Figure 2.7, panel A). Republicans are 14 points more likely than Democrats to give a Republican congenial response, while Democrats are 6 points more likely to give a Democratic congenial response and

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\(^{25}\) I also adjusted for respondent education and income in each model, as these variables were available in every poll. Adjusting for respondent race and gender (where available) does not substantively affect results, which are presented in full in Appendix Table A6.
8 points more likely to give a correct response. Partisan gaps in perceptions of the deficit tend to be smaller. Republicans are 5 points more likely than Democrats to give a Republican congenial response (Figure 2.7, panel B). Partisan gaps in perceptions of the rate of inflation are similar in magnitude to the gaps in perceptions of unemployment. Democrats are 11 percentage points more likely than Republicans to give a Democratic congenial answer (Figure 2.8, panel A). Finally, the average partisan gap in perceptions of the Dow Jones is close to zero (Figure 2.8, panel B).

Figure 2.9 breaks down these estimates by party of the president. We again see that the average effects mask asymmetries depending on which party is in power. The partisan gap in unemployment items is approximately 17 points under Republicans, but only 8 points under Democrats (panel A). Partisan gaps in perceptions of the deficit are nonexistent under Republicans and about 7 points under Democrats (panel B). Partisan gaps in perceptions of inflation show the most variation, ranging from close to zero under Democrats to 14 points under Republicans (panel C).
Figure 2.7. Predicted Probabilities of Correct and Party-Congenial Responses

A. Unemployment Rate

B. Federal Budget Deficit

Note: Figure displays the predicted probabilities of giving Republican congenial, correct, and Democratic congenial responses to questions about unemployment and the deficit. Blue and red points indicate point estimates for Democrats and Republicans, respectively. Lines indicate 95 percent confidence intervals. Predicted probabilities were generated from ordered probit regressions with item fixed effects and controls for education and income. The partisan gaps are indicated at the top of the plot.
Figure 2.8. Predicted Probabilities of Correct and Party-Congenial Responses

A. Inflation Rate

Note: Figure displays the predicted probabilities of giving Republican congenial, correct, and Democratic congenial responses to questions about inflation and the Dow Jones. Blue and red points indicate point estimates for Democrats and Republicans, respectively. Lines indicate 95 percent confidence intervals. Predicted probabilities were generated from ordered probit regressions with item fixed effects and controls for education and income. The partisan gaps are indicated at the top of the plot.

B. Dow Jones Industrial Average

Note: Figure displays the predicted probabilities of giving Republican congenial, correct, and Democratic congenial responses to questions about inflation and the Dow Jones. Blue and red points indicate point estimates for Democrats and Republicans, respectively. Lines indicate 95 percent confidence intervals. Predicted probabilities were generated from ordered probit regressions with item fixed effects and controls for education and income. The partisan gaps are indicated at the top of the plot.
Figure 2.9. Partisan Gaps in Perceptions by Administration Type

A. Unemployment Rate

B. Federal Budget Deficit

C. Inflation Rate
Discussion

The economic perceptions database yields somewhat mixed support for partisan bias. The descriptive analyses uncovered partisan bias in perceptions of the deficit and inflation, but only under Democratic and Republican presidencies, respectively. These descriptive results also suggest that Democrats are consistently more pessimistic than Republicans about unemployment and the Dow Jones, contrary to partisan bias. Partisan gaps shrink after controlling for respondent demographics. When survey items are pooled for each indicator, I find partisan bias in perceptions of unemployment, inflation, and the deficit, but not the stock market. However, there is a great deal of variation in bias across survey items within each indicator.

It is clear that looking at many different types of survey questions across many years results in a very different picture of partisan bias than looking at a particular survey or point in time. For example, the estimates of bias in perceptions of unemployment and inflation presented here are substantially smaller than those reported by Bartels (2002). Bartels finds that Democrats were 28 percentage points more likely than Republicans to give a Democratic-congenial response to a factual question about unemployment. Pooling unemployment questions over a thirty-year period, I estimate a 14-point gap between Democrats and Republicans for Republican-congenial responses, or half the size of Bartels’ finding (Figure 2.7, panel A). The partisan gap is even smaller for Democratic congenial responses (6 points). Bartels also finds that Democrats were 27 percentage points more likely than Republicans to give Democratic-congenial responses on inflation. On the other hand, I find an average partisan gap of 11 points (Figure 2.8, panel A). As my descriptive results immediately illustrated, limiting the analysis to particular years can result in substantially larger or smaller partisan gaps. Broadening the
analysis to include data from different presidencies and across changing economic circumstances results in a more complete picture of partisan bias.

More work is needed to understand why partisan perceptual bias is particularly pronounced in certain surveys. There are several possibilities. One is that certain item-level characteristics affect the observed level of bias. For example, Ansolabehere et al. (2013) find that quantitative items result in less bias than qualitative items. I find that retrospective evaluations are substantially more likely to result in bias than questions asking for current estimates. One reason for this difference may be that it is easier to understand the implications of retrospective evaluations for the parties than it is when estimating current levels of economic indicators. Retrospective evaluations inherently involve a comparison of how things are now with how they used to be. The timespan over which one is asked to retrospect may cover both in- and out-party administrations, further strengthening partisan motivations. Other features of surveys may affect observed levels of bias, for example the inclusion of questions that prime party identification prior to the factual question of interest. It is interesting that ANES items, for example, often yield larger partisan gaps (see Figure 2.3).

The salience of economic indicators in the public mind is likely to vary by media coverage, elite communication, and the actual state of the economy. Conover et al. (1986) find that the American public responds more quickly to changes in unemployment than to changes in inflation. Partisan bias also increases when elite communication about a topic is polarized (Druckman et al., 2013). In a comprehensive analysis of ANES survey data from 1956 to the present, Jones (2019) finds that the magnitude of partisan differences in retrospective evaluations of national conditions is primarily a function of the degree of elite polarization, as well as respondents’ political awareness of elite cues, bearing little relation to actual conditions. Since
economic conditions, elite discourse, and media coverage all varied during decades of data examined, observed heterogeneity in bias is likely due to a combination of these factors.\(^{26}\)

Another possible explanation for item-level heterogeneity is that Republicans and Democrats differ in their sensitivity to certain issues. For example, the major parties have each come to “own” certain issues, which are more central to their platforms than others (Petrocik, 1996). In turn, the public can more easily assign credit or blame to the given party in these issue areas. For example, Democrats tend to own unemployment, and Republicans own the budget deficit. Partisan bias may be more likely when partisans are asked about issues their party owns, as partisans may feel more is at stake in these areas. Indeed, I find greater partisan bias in beliefs about unemployment during Republican presidencies, which may follow from the public’s greater trust in Democrats on the issue. I also find greater bias in perceptions of the deficit under Democrats, whom the public is less likely to trust on this issue. The exception to this pattern is perceptions of inflation. Here, I find more bias under Republicans, even though they traditionally own this issue.

Finally, the partisan relevance of economic information may vary both by the indicator examined and time period. In order for partisan bias to occur, partisans must view information as reflecting positively or negatively on their party or the out-party. In this study, partisans had to understand that their responses to economic questions might reflect positively or negatively on the party in power. This connection may not be equally strong for all types of information. For example, individuals may perceive the president to have more control over the deficit than the Dow Jones, and therefore the deficit may have greater partisan relevance than the Dow Jones.

\(^{26}\) For example, I find a great deal of partisan bias in perceptions of inflation during the 1980s, which was indeed a volatile period with respect to inflation. However, because the bulk of questions about inflation in the database were asked in this period, it is difficult to disentangle this explanation from other variables.
In using cross-sectional survey data to estimate partisan gaps in factual beliefs, I am of course limited in the inferences I can draw. But by analyzing a large set of surveys conducted over many years, I limit the possibility that these results are dependent on any specific item, survey, or time period. Indeed, variation in survey items, actual economic conditions, and media coverage likely each explain a part of the heterogeneity in partisan bias across the indicators I examined. Unfortunately, these contextual variables are inter-correlated, complicating the task of quantifying their relative impact on partisan gaps in surveys.

That said, my pooled estimates of partisan bias are substantially different than those reported in the past. The main takeaway from my construction and analysis of the economic perceptions database is that partisan bias in economic perceptions is less severe than past research suggests. It is only by collecting a comprehensive database of survey items, paying careful attention to wording and format, that we can confidently say the picture of partisan bias that emerges here is more representative – and smaller – than it is in other studies.
Chapter 3. Does Selective Learning Increase Partisan Gaps?27

The preceding chapter estimated the severity of partisan bias in beliefs about the economy without speaking to its potential causes. As reviewed in the first chapter, there are different potential mechanisms that may give rise to the partisan gaps we observe in surveys. While Chapter 2 indicates that these gaps are not as severe as past work has suggested, partisan gaps are still clearly present and exhibit a great deal of variation in magnitude, depending on the survey. In this chapter, I offer a rigorous test of the selective learning hypothesis, which suggests that partisans learn congenial facts more readily than uncongenial ones. I test the hypothesis in a pair of survey experiments, including one with a panel design.

These experiments make several contributions to the literature on information processing. I am careful to use strictly objective information to test whether partisans learn selectively or evenhandedly. I also employ a crucial design feature that is absent from most studies that use factual information: I exogenously vary the congeniality of facts, while holding constant their other attributes, such as subject matter and difficulty. Moreover, I build on one-shot surveys to consider learning and attitude change over the course of days. Studies of factual learning rarely consider multiple points in time, even though doing so may lead to substantively different inferences (Chong & Druckman 2010). I therefore examine whether factual beliefs persist or decay several days after information exposure, and whether information congeniality affects recall at this later point in time.

I find little evidence for the selective learning hypothesis. Instead, partisans learn and remember congenial and uncongenial facts at more or less equal rates. Across these two

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27 Material in this chapter was presented at the annual meetings of the American Political Science Association, Midwest Political Science Association, and International Society of Political Psychology.
experiments, my findings push back against the image of partisans as rigid motivated reasoners, at least with respect to factual information.

The rest of this chapter is organized as follows. I discuss theoretical reasons to expect partisans to selectively learn facts, as well as some evidence for and against this proposition. I next elaborate on limitations of past work and how I address them in my research design. I then describe the method and results of each experiment: Study 1 on the unemployment rate and Study 2 on the Affordable Care Act. I end by discussing my results together, how they reflect on the quality of public opinion, and avenues for future work.

Selective Learning Hypothesis

As discussed in Chapter 1, the selective learning hypothesis predicts that partisans will learn congenial facts more readily than uncongenial facts (see theoretical pattern of results in Figure 1.2).

Selective Learning Hypothesis: partisans will be more likely to learn congenial facts than uncongenial facts.

Prior work suggests that partisan bias is likely more severe among the politically knowledgeable, because they tend to experience greater partisan loyalty and have more counterarguments at their disposal (Zaller 1992; Achen & Bartels 2006; Shani 2006). Therefore, I conduct additional analyses to examine whether this group is more likely to engage in selective learning than the less knowledgeable.

Limitations of Past Studies

Prior studies on selective learning of facts suffer from two major limitations. First, a clean test requires exogenous variation in information congeniality. Observational work is
obviously handicapped in this regard. For example, Schaffner and Roche (2017) use an actual jobs report to test selective learning; however, the congeniality of the information tested was fixed, as the Bureau of Labor Statistics does not randomly release congenial and uncongenial reports. Even with experiments, it is challenging to address this issue, because it is difficult to manipulate fact congeniality without also influencing subject matter, difficulty, and other attributes that may influence learning, particularly without resorting to deception.

As mentioned previously, Jerit and Barabas (2012) provide an illuminating case study. The authors present experimental participants with factual stories about politically salient issues. They manipulate congeniality by using stories about four different topics. The problem is that their experimental conditions differ along dimensions other than just congeniality. For example, some topics are inherently more difficult than others, which is evident from differences in baseline knowledge across topics in a non-informative control condition. Furthermore, respondents may be differentially responsive to new information across topics, perhaps because of their prior beliefs. The upshot of all this is that observed differences in learning across conditions cannot be attributed solely to the congeniality of the facts provided. Therefore, I design an experiment that solely manipulates the congeniality of the facts provided, while holding other variables constant.

Lack of specificity also affects post-treatment variables. For example, De Vries et al. (2018) treat British respondents with statistics about growth or the unemployment rate. However, their post-treatment variable is a general retrospective evaluation and therefore does not measure whether respondents learned the specific facts they were exposed to, but rather whether they assimilate the fact into a generalized evaluation of the economy.
Second, past work rarely considers the effect of factual information in the long term. Instead, most studies consist of one-shot surveys (see, however, Dowling et al., 2019). However, it is important to consider learning both at the time of information exposure and after a substantial amount of time. Partisan goals may operate when memories are first formed, causing partisans to encode congenial facts at the expense of uncongenial ones. Alternatively, or additionally, partisan motivation may operate over a longer time horizon, causing partisans to selectively recall congenial facts while letting uncongenial ones slip through the cracks (Hastie & Park 1986; McDonald & Hirt 1997; Pizarro et al. 2006). It is also important examine learning over the course of several days to understand whether survey respondents remember facts in any lasting sense. For these reasons, I conduct a panel survey with multiple days between waves.

In summary, most past studies on selective learning of facts are limited by failing to cleanly manipulate information congeniality or to measure learning over a sufficiently long period of time. Given these methodological limitations, as well as some evidence that partisans learn facts fairly accurately in Chapter 1, it is important to design a clean test of the selective learning hypothesis. Such a test should examine whether information congeniality affects learning at the individual level, both in the short term and long term.

**Study 1: Learning the Unemployment Rate**

I test selective learning hypothesis initially by conducting a survey experiment in which I present the national unemployment rate to respondents. I manipulate whether this fact was congenial to Democrats, Republicans, or neither party. I measure respondents’ factual beliefs about unemployment at the end of the survey.
Experimental Design and Participants

The survey took approximately ten minutes to complete and began with factual information in the form of questions. I administered three yes-or-no questions asking respondents if they had heard a recent news story. The first and third questions were merely distractor items, while the second concerned national unemployment. I randomly assigned respondents to read one of five versions of the unemployment question: a non-informative control version and four informative versions. The control version read as follows: “The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. Have you heard this story?” This condition was designed to provide a baseline assessment of knowledge of the current unemployment rate, in the absence of factual information.

The four informative versions of the question included the actual unemployment rate at the time of the survey, thereby providing respondents with an opportunity to learn. These conditions varied the congeniality of the information, reflecting positively on the Democratic Party, Republican Party, or neither party. The first presented the information in a neutral way. The other three versions used extra text to frame the unemployment rate positively, negatively, or positively and negatively. The Democratic Congenial condition noted the decline in the rate since Barack Obama took office. The Republican Congenial condition noted that the rate was higher than the average under George W. Bush. The final condition included both frames. Table 3.1 displays the text in all five conditions.

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28 The distractor items concerned iPhone sales and the National Football League’s handling of domestic violence.
29 The order of the two experimental clauses in the Both condition was randomized. I also varied the information source in the treatment conditions, but do not present those results.
Table 3.1. Information Treatments in Study 1

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>No Information (Control)</td>
<td>“The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. Have you heard this story?”</td>
</tr>
<tr>
<td>Neutral</td>
<td>“The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. It was reported that the unemployment rate is 5.9 percent. Have you heard this story?”</td>
</tr>
<tr>
<td>Democratic Congenial</td>
<td>“The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. It was reported that the unemployment rate is 5.9 percent, which is the lowest it has been since Barack Obama took office. Have you heard this story?”</td>
</tr>
<tr>
<td>Republican Congenial</td>
<td>“The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. It was reported that the unemployment rate is 5.9 percent, which is higher than the average unemployment rate under George W. Bush. Have you heard this story?”</td>
</tr>
<tr>
<td>Both</td>
<td>“The jobs situation has been in the news lately. A news story recently came out with the current national unemployment rate. It was reported that the unemployment rate is 5.9 percent, which is the lowest it has been since Barack Obama took office, but higher than the average unemployment rate under George W. Bush. Have you heard this story?”</td>
</tr>
</tbody>
</table>

I conducted an out-of-sample pretest to check whether my treatments have the intended effect. In short, I asked respondents to rate how positively or negatively each statement reflects on Democrats and Republicans. The pretest confirmed that these treatments significantly alter the partisan congeniality of the unemployment rate. For example, 47% of respondents rated the Neutral condition as positive for Democrats, while the Democratic Congenial text increases this
percentage to 77%, and the Republican Congenial text brings it down to only 11%. The percentage in the Both condition falls in the middle at 39%.  

I measured respondents’ factual beliefs in a series of post-treatment questions at the end of the survey (see Appendix for wording). I first asked for an open-ended estimate of the current national unemployment rate. This item serves as my primary dependent variable. I consider the percentage of respondents who correctly report the unemployment rate (within a very small margin of error) in each condition. I estimate learning by comparing this value between the non-informative control condition and the four informative conditions. I also test the differences across the informative conditions to assess the impact of information congeniality on learning.  

Sample Considerations  

I recruited 603 survey respondents via Amazon’s Mechanical Turk (mTurk) to participate in Study 1 in November 2014. In between the information treatment and post-treatment questions, respondents completed a demographic questionnaire and short political knowledge scale, which served as buffer tasks. The survey concluded with questions about political interest, ideology, and party identification. The sample consists of 61 percent Democrats and 23 percent

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30 In each condition, Democrats are likelier than Republicans to report that facts reflect positively on the Democratic Party, which is indicative of partisan bias. However, I find substantial treatment effects among both Democrats and Republicans, indicating that treatments alter congeniality among both groups. The Appendix describes the pretest methodology in greater detail and contains the full results (see Appendix Table A8).

31 I use three other factual questions to further probe the selective learning hypothesis. I asked respondents for open-ended estimates of the average unemployment rate under Barack Obama and George W. Bush. I use these items to assess whether respondents learn that average unemployment was lower under Bush than under Obama, which they could learn in the Republican Congenial and Both conditions. I also asked respondents whether unemployment had gotten better or worse in the past year, which they could learn in the Democratic Congenial and Both conditions.

32 Amazon’s mTurk is a micro-task market: workers complete small tasks, such as surveys, for money. For details of how samples are recruited on MTurk and general characteristics of the market, see Buhrmester et al. (2011) and Berinsky et al. (2012).
Republicans (both including leaners). As is common with mTurk samples, the average respondent is more likely to be a Democrat, male, young, white, and educated than a representative sample of U.S. adults (full sample characteristics in Appendix Table A7).

In both Studies 1 and 2, I recruited respondents via mTurk in order to administer a low-cost survey of U.S. partisans, which was particularly helpful in conducting the panel study. Two concerns about the sample raise questions about my study’s generalizability. First, mTurk workers may experience greater accuracy motivation than other participants do, because workers are often rewarded for completing tasks attentively. Consistent with this proposition, over 90 percent of respondents passed an attention check I embedded in the survey. Second, analyses relying on partisanship as a moderator may differ between mTurk and samples of the broader population (Krupnikov & Levine, 2014). It would be of course be informative to replicate these experiments on diverse national samples.

That said, we can learn a great deal from mTurk samples. Multiple studies find that they yield high-quality data and are more diverse and nationally representative than other common convenience samples, such as college students (Buhrmester et al., 2011; Berinsky et al., 2012; Paolacci & Chandler, 2014). Moreover, I do not expect partisans on mTurk to differ from partisans nationally with respect to factual information processing, and I therefore expect that any treatment effects I find in this sample would be similar in a national sample. Indeed, Mullinix et al. (2015) find similar treatment effects on mTurk and population-based samples among a wide swath of experiments. Several other studies find partisan motivated reasoning among mTurk, indicating that workers indeed succumb to partisan directional goals in certain contexts. Workers exhibit partisan bias in stored knowledge (e.g., Ahler, 2014; Chambers et al., 2014; Chambers et al., 2015; Bullock et al. 2015; Ahler & Sood, 2018). Partisanship also
influences political judgments among mTurk study participants (e.g., Arceneaux & Vander Wielen, 2013; Lyons & Jaeger, 2014; Crawford et al., 2015; Crawford & Xhambazi, 2015; Thibodeau et al., 2015).

**Results**

In order to summarize responses, I calculate the percent of respondents learning the correct unemployment rate, separately among Democrats and Republicans, including leaners. If the selective learning hypothesis is correct, the starkest difference in percent correct should occur between the Democratic and Republican Congenial conditions. The percent correct in the Both condition should fall somewhere in between. Before analyzing the percent correct, I examine raw estimates of unemployment in the baseline condition to get a sense of respondents’ prior beliefs about the number. Figure 3.1 shows a wide distribution of estimates that is skewed to the right, due to many overestimates, but centered at approximately the correct answer of 5.9 percent.

**Figure 3.1. Distribution of Open-Ended Estimates of Unemployment Rate**

![Figure 3.1](image)

*Note: Figure plots distribution of open-ended estimates of current unemployment rate in No Info condition (N=60). Smoothing uses .89 bandwidth. Dashed vertical line indicates correct answer.*
In order to summarize the data in a useful way and deal with extreme values, I recode respondents’ estimates to be either overestimates, underestimates, or correct. Figure 3.2 displays the proportion of each type of response by experimental condition. The results show a clear increase in the percent reporting the correct unemployment rate in each of the informative conditions. It is over twice as high as in the No Info condition. Disaggregating the data by party reveals Democrats and Republicans both exhibit a big difference between the No Info condition and the four informative conditions. Partisans tend to enter the study ignorant of the unemployment rate, but by the end of the survey, the majority report the correct figure. Therefore, upon first glance, it appears that many respondents learn the relevant fact.

In order to test for selective learning, I examine the effect of the congeniality manipulation on the percent correct in the informative conditions: Neutral, Democratic Congenial, Republican Congenial, and Both. Figure 3.3 plots the percent correct among Democrats by condition to probe the selective learning hypothesis. (Unfortunately, this analysis lacks sufficient power to subset Republicans.) Percent correct is quite stable across these four conditions. It ranges from approximately 68 to 76 percent, but it does not change vary by condition ($\chi^2_{23} = 1.64, p = .65$). Most importantly, the difference between the Democratic and Republican Congenial conditions is essentially zero (one-tailed $p$-value is .41). Thus, information congeniality has no discernible effect on learning the current unemployment rate.

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33 I coded responses as correct if they were within one-tenth of a percentage point of the correct answer (i.e., 5.8 – 6.0). Using a stricter or more lenient window does not change the substantive results (see Table A3 in Appendix 1.5.1).
Figure 3.2. Proportion of Correct Answers, Underestimates, and Overestimates by Condition (Study 1)

Note: Figure displays proportion of underestimates (white), correct answers (black), and overestimates (gray) by experimental condition, among all respondents.
Figure 3.3. Percent of Democrats Correctly Reporting Unemployment Rate by Condition (Study 1)

Note: Figure plots percent of Democrats correctly reporting current unemployment rate (and standard error) with 95 percent confidence intervals, by condition in Study 1.

In supplementary results, I subset respondents by general political knowledge, finding suggestive evidence of selective learning only among high-knowledge partisans (see Appendix Figure A5). Moreover, I examine two other post-treatment measures of factual beliefs, including a retrospective evaluation and comparison of unemployment under Obama and Bush. While partisan gaps exist in prior knowledge of these two facts, selective learning does not exacerbate these gaps (see Appendix Figures A6 and A7).

Overall, I find little evidence of selective learning in Study 1. Partisans learn the current unemployment rate fairly accurately, regardless of whether it is congenial or uncongenial (or both). These findings push back against the idea that partisans stubbornly cling to their beliefs and attitudes in the face of factual information.
I have discussed several reasons why my experimental design enables a cleaner test of selective learning than past studies, (e.g., Jerit & Barabas 2012); however, there might be other reasons why I do not see selective learning. One possibility is that it may have been very easy for respondents to remember the facts they were given. The survey took approximately ten minutes to complete, so respondents did not have to hold information in memory for very long before they were asked to recall it. Moreover, respondents were extremely attentive, passing two attention checks embedded in the survey at a rate of 91%. It is possible that selective learning is unlikely when facts are presented to attentive respondents in an easily digestible manner. In Study 2, I address these potential issues by requiring respondents to expend greater effort to learn facts initially and by testing respondents’ memories after several days.

**Study 2: Learning about the Affordable Care Act**

I conduct a more rigorous test of the selective learning and updating hypotheses in Study 2, which differs from Study 1 in three important ways. First, I changed the topic to the performance of the 2010 Patient Protection and Affordable Care Act (ACA). I chose this issue because the law is both highly politicized and a hallmark achievement of the Obama administration.\(^{34}\) For these reasons, positive news about the ACA’s performance is congenial for Democrats and negative news congenial for Republicans – I verified both in the pretest (see Appendix). Second, I presented factual information in the form of a brief article, which is a more naturalistic way of exposing partisans to politically relevant facts than the question-as-treatment method used in Study 1. I cited non-partisan sources for key facts to lend the article greater credibility. The article also contained extraneous information, making it more difficult to learn specific facts. Third, I measured partisans’ post-treatment beliefs twice: a few minutes after

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\(^{34}\) A Pew Research Center poll in 2016 found a large partisan gap in views of the policy: eight in ten Democrats approved of it, while nine in ten Republicans disapproved (Gramlich, 2016).
information exposure and again several days later. This design allows me to rule out the possibility that the experimental task is trivially easy for respondents, while also enabling me to measure learning over a longer time horizon than experiments typically consider.

**Experimental Design and Participants**

Study 2 also consisted of a randomized experiment, which was described as a brief survey on news and politics. Respondents began by answering questions about their party identification, political interest, and ideology. I invited only partisans and leaners to proceed to the main survey, which began with the factual information treatment, a brief article on the ACA. Respondents were randomly assigned to read one of three versions of the article: a Democratic Congenial (i.e., pro-ACA) version, Republican Congenial (i.e., anti-ACA) version, or control (i.e., balanced) version. Each condition shared a common stem describing the purpose of the law, the concerns of its critics, and the major changes the law made, such as the insurance exchanges and individual mandate. The article then took a position on the success of the law, citing key facts along the way. The Appendix contains the three versions in full. For now, I focus on key experimental manipulations.35

Table 3.2 summarizes facts included in each version of the article. Each version mentioned that 12 percent of U.S. adults currently lack health insurance. Each also said that the uninsured rate had decreased by 5 points since the ACA started requiring people to be insured. However, I used a subtle wording change in the Republican Congenial condition to increase the congeniality of this fact for Republicans and decrease it for Democrats. The control and pro-ACA articles said that the uninsured rate had decreased by “about 5 percentage points,” while the

35 Median screen completion time was 70 seconds in the control condition, 74 seconds in the Democratic Congenial condition, and 70 seconds in the Republican Congenial condition. Screen completion times did not differ by respondent partisanship.
Republican Congenial version said that it had decreased by “only 5 percentage points.” All three versions also noted average annual premiums of nearly $17,000 for employer-sponsored family coverage in 2014. The anti-ACA and control conditions further mentioned that “average premiums increased by 3 percent” relative to the previous year, while the Democratic Congenial condition said “only 3 percent, which is a record low”, thereby making this fact more congenial for Democrats and less congenial for Republicans.

**Table 3.2. Key Facts by Condition in Study 2**

<table>
<thead>
<tr>
<th>Control (Balanced)</th>
<th>Democratic Congenial (Pro-ACA)</th>
<th>Republican Congenial (Anti-ACA)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Uninsured rate is 12%</td>
<td>Uninsured rate is 12%</td>
<td>Uninsured rate is 12%</td>
</tr>
<tr>
<td>Uninsured rate down 5 points since ACA</td>
<td>Uninsured rate down 5 points since ACA</td>
<td>Uninsured rate down only 5 points since ACA</td>
</tr>
<tr>
<td>Family premiums went up 3 percent</td>
<td>Family premiums went up only 3 percent, which is a record low</td>
<td>Family premiums went up 3 percent</td>
</tr>
<tr>
<td>(No additional fact)</td>
<td>6 million young adults gained insurance</td>
<td>5 million insurance policies canceled</td>
</tr>
</tbody>
</table>

*Note: experimental text is underlined in each condition.*

I confirm that these subtle manipulations had the intended effect in the out-of-sample pretest. For example, the wording change for the decrease in the uninsured rate shifted the percentage of respondents reporting that this fact reflected positively on the Democratic Party from a majority (69 percent) to a minority (42 percent). The wording change for the increase in family premiums changed this same percentage from 15 to 59 percent. Large treatment effects hold among both Democrats and Republicans (see full results in Appendix Table A8).

Finally, the Democratic Congenial and Republican Congenial articles each randomly included an additional sentence with an extra fact. The extra fact in the Democratic Congenial condition stated that, “almost 6 million young adults have gained coverage over the past five years.” The extra fact in the Republican Congenial condition said, “almost 5 million people across 31 states lost their health insurance, because their policies did not meet the standards of
the new law.” Half of respondents in these two conditions (but not in the control condition) were given the extra fact.

I administered several post-treatment items to measure factual learning about the ACA. I asked two questions about the change in the uninsured rate: a closed-ended retrospective evaluation and an open-ended estimate of the magnitude of the change (in percentage points). I also asked for an open-ended estimate of the percentage increase in family premiums, as well as closed-ended questions about the two extra facts. The article contained answers to all of the factual questions (see Appendix for full list of questions).

I administered the exact same items in a follow-up survey (Wave 2), which took place several days after the initial survey (Wave 1). I invited everyone who completed Wave 1 to participate in Wave 2 about two days later. Respondents were not alerted to the possibility of a follow-up survey beforehand and varied in exactly when they began Wave 2. The modal period of time between waves was four days (mean: 4.7 days). This lag enables me to test two types of learning: learning a fact in Wave 1 and reporting it back a few minutes later (short-term learning), as well as committing a fact to memory in Wave 1 and recalling it multiple days later (long-term learning).

I recruited a total of 1,594 respondents on mTurk to participate in Wave 1 in June 2015. Out of these respondents, 1,296 also participated in Wave 2, yielding a re-interview rate of 81%. In both waves, the sample is 75% Democrats and 25% Republicans (detailed sample characteristics can be found in Appendix Table A7).
Results

Figure 3.4 plots the percentage of partisans who correctly report two key facts about the ACA: the sizes of the decrease in the uninsured rate (panel A) and increase in family premiums (panel B). The horizontal dashed lines indicate the percentage in the control condition of Wave 1 (upper dashed lines) and Wave 2 (lower dashed lines).³⁶

Before examining evidence for or against selective learning, I consider the general pattern of learning and forgetting in Study 2. In Wave 1, 61% of Democrats and 51% of Republicans correctly report the uninsured rate decline in the control condition. And the probability of reporting the correct premium increase is about 50% among both groups. Thus, learning each of these facts from a balanced article is imperfect in Wave 1. As expected, these percentages decrease quite a bit in Wave 2, as many respondents forget the information they learned. The percent correct for each fact declines by approximately one-half in Wave 2. For example, only 24% of Democrats and 21% of Republicans assigned to the control condition report the correct premium increase in Wave 2.

³⁶ To ensure that there was no attrition bias, I regressed participation in Wave 2 on the experimental treatments in Wave 1 ($F=.38, p=.68$). I then regressed Wave-2 participation on the Wave-1 treatments, party identification, and their interaction ($F=.72, p=.61$).
Figure 3.4. Treatment Effects on Learning Facts about the ACA (Study 2)

A) Percent Correct: Uninsured Change

B) Percent Correct: Premium Increase

Note: Dashed horizontal lines indicate percentage correctly reporting fact in control condition. Points indicate effects of Democratic Congenial and Republican Congenial treatments on percent correct, relative to control condition. Vertical lines indicate 95 percent confidence intervals of treatment effects.
The crucial test of selective learning is whether partisans are more likely to forget uncongenial facts than congenial facts. As the treatment effects in Figure 3.4 show, this is decidedly not the case. Consider the percent of respondents learning the decrease in the uninsured rate (Panel A). The Democratic Congenial treatment has a significant effect on learning, but not in a manner consistent with selective learning. In Wave 1, Democrats are 9.4 points less likely to learn this fact when it is congenial. They are also 8.0 points less likely to remember the congenial fact in Wave 2 (relative to the control condition). Thus, contrary to the selective learning hypothesis, a congenial story makes Democrats more likely to forget the good news about the policy, both initially and after a few days. This condition also increased learning among Republicans by 6.2 points, although this effect is not significant. On the other hand, the Republican Congenial treatment has no effect on learning among either Democrats or Republicans in either wave. This result indicates that framing the ACA as unimpressive does not inhibit learning among Democrats, nor does it facilitate learning among Republicans.

Turning to the premium increase (Figure 3.4, Panel B), we see a result consistent with selective learning. Democrats are 9.8 points more likely to learn the size of the hike in family premiums if it is congenial in Wave 1. However, Republicans exhibit a similar increase in learning when the fact is congenial to Democrats (10.1 points, \( p < .1 \)). These effects persist in Wave 2 but decrease in magnitude. The sign is consistent with selective learning among Democrats but not Republicans. The Republican Congenial treatment has no discernible effect on Democrats or Republicans in either wave. It is possible that the Democratic Congenial condition made this fact more memorable among both parties, because framing the premium hike as positive is novel or counter-stereotypical. At any rate, the pattern of learning on the whole is

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37 Analyzing the percentage of respondents who correctly learn the current uninsured rate yields similar results (see Appendix Figure A9).
not selective: across all these tests, I observe just one statistically significant treatment effect that is consistent with selective learning.

I next examine the pattern of learning for the two extra facts randomly included in the articles. Recall that only half of respondents in each treatment condition (Democratic Congenial and Republican Congenial) saw an extra fact, so the baseline conditions here is not informative. In this analysis, I subset on one of the conditions and simply test whether treating respondents with an extra fact increases the percent answering the corresponding factual question correctly. Figure 3.5 plots treatment effects on the percentage correctly reporting the extra facts congenial to Democrats (Panel A) and to Republicans (Panel B) by party and wave.\textsuperscript{38}

Consider the extra fact that is congenial for Democrats: the six million young adults gaining coverage under the ACA. Selective learning suggests that, all else equal, Democrats will be more likely than Republicans to learn this fact. In Panel A, I plot the treatment effect on the percentage correctly reporting this fact in each wave. Contrary to selective learning, the effects are very close among Democrats and Republicans in Wave 1 (26.3 and 27.2 points, respectively). In Wave 2, however, the effect is greater among Democrats than Republicans (9.7 vs. -10.9 points, one-tailed $p = .03$). This is consistent with selective learning in the long term, as Republicans are more likely to forget this fact.

\textsuperscript{38} Note that this observable implication is slightly different than what I have previously tested. Instead of examining the effect of congeniality on learning facts conditional on party identification, I now look at differences in learning between parties.
Figure 3.5. Treatment Effects on Learning Extra Facts about the ACA (Study 2)

Note: Points indicate effect of providing extra fact on percent correct among Democrats (blue, left) and Republicans (red, right) by survey wave (with standard errors of effects in parentheses). Vertical lines indicate 95 percent confidence intervals of treatment effects.
In Panel B of Figure 3.5, I plot the same quantities for the Republican Congenial extra fact: five million canceled insurance policies. Here, selective learning implies that treatment effects will be greater among Republicans. Once again, the effects in Wave 1 are quite similar among Democrats and Republicans (30.6 and 39.7 points, respectively), indicating symmetric learning in the short term. Treatments effects in Wave 2 are indeed different between Democrats and Republicans (28.9 vs. 11.1 points, one-tailed p = .03); however, the sign of the difference indicates that Democrats are more likely to remember the number of canceled policies than Republicans, which is selectivity in an unexpected (i.e., uncongenial) direction. Considering the two extra facts’ results together, Democrats and Republicans are equally likely to learn each fact in the short term, but Democrats are more likely than Republicans to remember each fact in the long term, irrespective of the fact’s partisan congeniality.

**Discussion**

This chapter explores one mechanism thought to give rise to partisan gaps in factual beliefs: selective learning. I subject the hypothesis to rigorous tests using two complementary experiments, which produce largely consistent results. On balance, I do not find much evidence of selective learning. None of the congeniality manipulations in Study 1 significantly influenced learning or the size of partisan gaps in knowledge. The learning task of was significantly more difficult in Study 2, both because the treatments were more complex and fact recall was tested several days after information exposure. While many respondents in Wave 2 did not recall facts that they had learned in Wave 1, they were no more likely to forget uncongenial facts than congenial ones. In the short and long term, partisans learned (and forgot) congenial and uncongenial facts at more or less equal rates.
Together, these findings serve as an important corrective to past work that finds selective learning and mitigate normative concern around partisan bias. One reason these findings differ from past work on motivated reasoning is because of the research design. I test strictly factual information and carefully isolate the effect of congeniality, avoiding an important pitfall of past research in this area. I have a great deal of confidence in the internal validity of my experiments, because the out-of-sample pretest suggests that the experimental treatments successfully change congeniality for partisan respondents.

A recent study by Dowling et al. (2019) also tests long-term factual learning about the ACA. The authors reinterview participants over four weeks but do not experimentally test information congeniality. Similar to the randomized inclusion of extra facts in Study 2, their treatments instead consist of textual information about specific provisions of the law, while a control condition contains no information. Despite these design differences, their substantive findings with respect to factual beliefs are largely consistent with mine: respondents learned both congenial and uncongenial facts with some decay over time.
Chapter 4. Selective Reporting of Factual Beliefs

If selective learning of factual information is an unlikely culprit for partisan gaps that we observe in public opinion surveys, then what else could be contributing to them? As discussed in Chapter 1, differences in survey reports of factual beliefs may not reflect differences in what people believe. Instead, they may reflect bias in the survey response process. Respondents may give congenial but inaccurate answers in response to factual questions even when they have accurate but uncongenial facts at hand. In other instances, respondents are ignorant, having no relevant beliefs, and they offer a congenial response as a best guess (e.g., Luskin & Sood, 2018). In either case, the survey response process inflates estimates of bias in factual beliefs. Study 3 extends this line of thinking to survey estimates of learning, the process that produces factual beliefs. As with studies of stored beliefs, studies of learning may overstate bias without accounting for selective reporting.

“You Cannot be Serious”39

A pair of studies demonstrate that partisans engage in selective reporting on factual questions with political implications. Bullock et al. (2015) and Prior et al. (2015) find that partisans incorrectly report congenial beliefs, even when they know or could have inferred a more accurate answer. Both studies demonstrate selective reporting by experimentally boosting respondents’ accuracy motivation. Bullock et al. (2015) do so by offering respondents bonus payments for correct answers and admitting their ignorance, Prior et al. (2015) also test the effect of bonus payments for correct answers, and in a follow-up study, textual appeals.40 Importantly,

39 Figures and tables from Prior et al. (2015) are reproduced here with the permission of my co-authors.
40 Prior et al.’s textual appeal simply read, “In order for your answers to be most helpful to us, it is really important that you answer these questions as accurately as you can.”
none of the treatments provide any additional information, so any change in responses can be attributed to a change in respondents' motivation, rather than a change in their knowledge. Both studies find that, consistent with selective reporting, accuracy incentives substantially reduce partisan bias in respondents’ reported beliefs, across a variety of factual questions, such as the unemployment rate, poverty rate, and national debt.

Figure 4.1 shows the key results that Prior et al. observe, with respect to both treatments considered. Similar to the approach taken in Chapter 2, they operationalize partisan bias as the difference in the probabilities of offering congenial and uncongenial responses to factual questions. For example, in the control condition, partisans were 11.8 points likelier to report congenial, incorrect answers than uncongenial, incorrect answers (see Figure 4.1a). When offered monetary incentives, bias was reduced by almost half to just 6.3 points. The follow-up experiment shows that both the non-monetary treatment (‘Accuracy Appeal’) and monetary treatment significantly reduce bias, by almost the same amount (Figure 4.1b).
Figure 4.1. Percent Uncongenial and Congenial Responses, by Experimental Condition (Prior et al., 2015)

a) Studies 1 & 2

Note: Graphs plot predicted probabilities from Table 2, columns (1) and (2a), as well as 95 percent confidence intervals. Probabilities are multiplied by 100 to make them comparable to percentages reported in Table 1.
Study 3: Learning about Gun Control and Minimum Wage\textsuperscript{41}

Study 3 builds on previous studies of selective reporting by applying the paradigm to studies of learning. Prior et al. (2015) measure selective reporting of \textit{stored} beliefs. The present study considers selective reporting of about information that has just been learned. We reassess the extent to which people learn selectively by measuring selective learning and reporting simultaneously in three experiments. This study does not attempt to disentangle the various mechanisms that may undergird selective reporting. Instead, our aim is to merely estimate the bias that selective reporting produces in estimates of selective learning derived from ordinary survey instruments.

Building on the research design of Kahan et al. (2017), we present people with data from a social scientific study and ask them to report the conclusion supported by the data. This is a different form of learning than that considering previous chapter. Studies 1 and 2 tested uptake of fairly isolated facts about the economy and healthcare. Learning in this study is a more inferential and cognitively demanding process. We offer a random set of respondents incentives to honestly report the conclusion they think the data supports. We then test whether doing so reduces bias in what respondents report having learned.

\textbf{Kahan et al. on Motivated Numeracy}

Many psychological studies find that people evaluate congenial and uncongenial claims differently, using different evidentiary standards and investing different amounts of effort in processing the information. When evaluating congenial claims, people tend not to be as thorough in searching for evidence (e.g., Kruglanksi & Webster, 1996; Nickerson, 1998), and evaluate...

\textsuperscript{41} This study was conducted jointly with Gaurav Sood and previously published (Khanna & Sood, 2018). It is included here with the permission of the co-author and publisher.
available evidence more superficially and less skeptically (e.g., Chaiken & Maheswaran, 1994). On the other hand, when evaluating uncongenial claims, people are more skeptical and invest greater processing effort (e.g., Ditto & Lopez, 1992; Ditto et al., 1998; Dawson et al., 2002a). In covariance detection tasks, respondents are more likely to only partially consider the data if doing so leads them to a congenial conclusion (Dawson et al., 2002b).

Building on this psychological literature, Kahan et al. (2017) argue that a natural tendency to learn heuristically results in bias. For instance, when presented with tabular data, people tend to only consider the most salient datum in the table, for instance, the largest number, to deduce the conclusion supported by the data (Gilovich, 1991). When heuristic processing yields a congenial answer, people tend to stop processing further, concluding that the data supports their beliefs. If heuristic processing instead yields an uncongenial result, people tend to look at the data more carefully to make sure they are correct. For instance, activating directional goals can motivate people to overcome the common error of neglecting “cell D” in a two-by-two table (Mata et al., 2015a; Mata et al. 2015b). Imbalance in scrutiny produces a congeniality effect: people learn congenial facts more readily than uncongenial facts. Kahan et al. argue that this effect will increase with numeracy, because only respondents with sufficient numerical ability are capable of learning the correct result by considering all four cells.

As discussed previously, a major challenge in motivated reasoning studies is cleanly manipulating information congeniality, while holding constant other attributes, such as topic and difficulty. In order to so, Kahan et al. (2017) cleverly repurpose a “covariance detection task” (see Gilovich, 1991). Respondents see a two-by-two table with data on the relationship between

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42 In trying to learn without expending much effort, people may also misread data in ways that are shaped by prior beliefs (e.g., Bechlivanidis & Lagnado, 2013; Kahneman, 2013). For instance, when reading a two-by-two contingency table, people may misread column or row labels, or even the numbers themselves, in a way that suggests a congenial result.
banning concealed carry and rates of crime. The table's column headings are manipulated so that the data either support the conclusion that banning concealed reduces crime or the conclusion that banning concealed carry increases crime. When asked what result the data support, respondents are more likely to answer correctly when the data support a congenial claim than when the data support an uncongenial claim. That is, liberal Democrats are more likely to report the correct result when banning concealed carry reduces crime than when banning concealed carry increases crime, while the reverse is true among conservative Republicans.\footnote{Kahan et al. alternately use the terms \textit{motivated numeracy} and \textit{motivated cognition} to describe this phenomenon.}

Because studies of knowledge and learning rely on similar survey instruments, discovery of selective reporting on questions about stored knowledge suggests that estimates of selective learning may also be inflated. To uncover selective reporting, we borrow a design feature of Prior and Lupia (2008), Bullock et al. (2015), and Prior et al. (2015), offering a random set of respondents small monetary incentives to accurately report their beliefs. To minimize the possibility that incentives affect processing of information, we present information about incentives after respondents have seen the information and can no longer revisit it. These incentives should increase respondents' motivation to honestly answer which conclusion they think the data support. Respondents who would ordinarily offer a congenial answer as their best guess or as an act of political expression may be nudged to think more carefully or be more truthful. We hypothesize that this treatment will attenuate the congeniality effect that Kahan et al. observe.

More specifically, we hypothesize that incentives will increase the probability of answering correctly when the answer is uncongenial. If respondents learn the uncongenial result, and then knowingly report an incorrect answer, incentives should encourage some of them to
reveal their true belief. If respondents offer an incorrect, congenial response as their best guess, incentives should nudge some of them to guess more neutrally. In both scenarios, incentives should increase correctness. We do not expect incentives to boost correctness in the congenial condition, because in this case, the congenial answer is also correct.

Figure 4.2 visualizes the hypothesized pattern of results, where the vertical axis represents probability of answering correctly, and the dashed lines indicate the size of the congeniality effect. Incentives reduce the congeniality effect by increasing the probability of answering correctly in the uncongenial condition.

**Figure 4.2. Hypothesized Pattern of Results**

![Probability of Answering Correctly](image)

*Note: Figure displays hypothesized probability of correctly reporting study's result by experimental condition. Dashed vertical lines indicate congeniality effect, i.e., difference in probability of answering correctly between congenial and uncongenial conditions. We hypothesize that accuracy incentives will reduce the congeniality effect by increasing the probability of answering correctly in the uncongenial, but not the congenial, condition.*
Experimental Design

To distinguish between selective learning and selective reporting, we conduct a series of experiments that build upon the original design of Kahan et al. In particular, we add an orthogonal manipulation, offering participants a small monetary incentive to accurately report what they have learned. The two-by-two design enables testing whether incentives reduce the congeniality effect, which has been interpreted as evidence for selective learning. In addition to measuring the outcome used by Kahan et al., we also measure subjective ratings of the study to examine whether the study's congeniality influences its perceived credibility.

In Study 3a, we asked respondents to read a summary of a hypothetical study on gun control. A preface to the study described its purpose: a city government is trying to decide whether or not to ban private citizens from carrying concealed weapons and wants to know if doing so would increase or decrease crime. After the preface, the study was briefly summarized: researchers compared changes in annual crime rates in cities that had banned concealed carry with changes in annual crime rates in cities that had not banned concealed carry. A two-by-two contingency table with the putative results came next.

Following Kahan et al., we manipulated the conclusion supported by the study by switching column labels. In Table 4.1, cities that banned concealed carry experienced crime decreases more often than cities that did not ban concealed carry. This result can be learned by comparing the ratios of cities in the first row (75:223) and the second row (21:107). Table 4.2 flips the column labels to produce the opposite result: the data now indicate that cities with bans saw more increases in crime than cities without bans.
Table 4.1. Experimental Stimulus Undermining Concealed Carry

<table>
<thead>
<tr>
<th></th>
<th>Increase in crime</th>
<th>Decrease in crime</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cities that did ban carrying concealed handguns in public</td>
<td>223 cities</td>
<td>75 cities</td>
</tr>
<tr>
<td>Cities that did not ban carrying concealed handguns in public</td>
<td>107 cities</td>
<td>21 cities</td>
</tr>
</tbody>
</table>

Table 4.2. Experimental Stimulus Supporting Concealed Carry

<table>
<thead>
<tr>
<th></th>
<th>Decrease in crime</th>
<th>Increase in crime</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cities that did ban carrying concealed handguns in public</td>
<td>223 cities</td>
<td>75 cities</td>
</tr>
<tr>
<td>Cities that did not ban carrying concealed handguns in public</td>
<td>107 cities</td>
<td>21 cities</td>
</tr>
</tbody>
</table>

After presenting the summary, we asked respondents whether cities with a ban were more likely to experience an *increase* or *decrease* in crime than cities without a ban. This question serves as our primary dependent variable. Note that it is strictly factual in nature. It simply asks which of two descriptions is consistent with the data. The question does not ask the respondents to assess a causal claim, evaluate gun control, or indicate their faith in the study. Respondents could not access the study description and table when picking which of the conclusions were supported by the data. At the end of the survey, respondents were debriefed and informed that the data were not real.

To measure selective reporting, we independently manipulated respondents’ motivation to give the answer they thought was correct. We offered a random set of respondents a small nudge, an additional $0.10 for the correct answer. Though this is a very small amount, Prior et al. (2015) uncover about same amount of selective reporting without any extra money as they do by offering another $1 for the correct answer. To ensure that incentives did not affect how respondents processed the contingency table in the treatment condition, we withheld any information about the incentive until after they had seen the table and could no longer return to
it. (A control group was not offered the accuracy incentive.) We also asked respondents to recall the numbers in the contingency table at the end of the survey, in order to test whether respondents are more likely to remember congenial data than uncongenial data. To minimize respondent disengagement, we offered an additional $0.05 for each number recalled correctly.

In Study 3b, we re-administered the concealed carry task and added another task. In this follow-up task, respondents were presented with a study on the impact of raising the minimum wage. Again, respondents were asked to indicate its result based on tabular data. The minimum wage task was very similar to the concealed carry task in design, with two important differences, aside from the change in topic. First, with the intention of making it easier to learn the correct result, we replaced cell frequencies with percentages in the table. The data suggest that the change had the intended effect, as there was a large increase in the frequency of correct responses. Second, we manipulated congeniality by switching the row labels instead of the column labels in the table. This is a cleaner manipulation as it holds constant the increase-to-decrease ratio in each row, and simply changes the policy associated with each ratio. While lowering the task’s difficulty might change the congeniality effect observed, we do not expect the changes to affect the degree of selective reporting. Lastly, randomization in the second task was conducted independently of the first, but the sequence of the two tasks was fixed.

In Study 3c, we re-administered both the concealed carry and minimum wage tasks on a more diverse sample. Following our hypothesis that incentives influence responses in the uncongenial condition, we presented all respondents with an uncongenial version of the concealed carry task (based on their pre-treatment attitudes) and randomized incentives as in Studies 3a and 3b. This simpler design allows us to conserve resources while testing our central theoretical claim that incentives increase correctness in the uncongenial condition. Additionally,
we replicated the full two-by-two minimum wage task. The purpose of Study 3c was to gather confirmatory evidence and probe the generalizability of the estimates in Studies 3a and 3b.

In order to identify respondents that would find each study’s result congenial or uncongenial, we measured attitudes toward banning concealed carry and raising the federal minimum wage before the tasks in each study. We expect respondents who oppose concealed carry to find a decrease in crime to be congenial, and an increase in crime uncongenial. We expect the opposite among respondents who support concealed carry. A similar logic applies in the minimum wage task. We measured partisanship, ideology, and demographics prior to the experiments in each study.44

Participants

In Studies 3a and 3b, we recruited respondents from mTurk to complete a short survey on “how people learn.” To assess whether our findings generalize beyond samples recruited via mTurk, we recruited respondents via Qualtrics in Study 3c. The Qualtrics sample is more representative of the U.S. general population, and respondents appear to be less attentive and detail-oriented than mTurk workers. Study 3a was fielded from December 2013 to January 2014, Study 3b in March and April 2015, and Study 3c in August 2016. (For details of the recruited samples and how they compare to national benchmarks, see Appendix Table A10.)

Given the theoretical expectation that we should only observe selective learning among respondents with sufficient numerical ability to complete the covariance detection task, we screened for high-numeracy respondents using a numeracy quiz in Study 3a. The numeracy quiz was composed of the five easiest questions in Weller et al. (2012). We invited respondents

44 The appendix contains a complete description of the three studies and the full wording of each task and question. In Studies 3b and 3c, we omitted recall questions and ratings of the minimum wage study due to concerns about the length of the survey.
answering four or more items correctly to participate in the full study. We use a threshold of four because Kahan et al. (2017) find that the median respondent answers four items correctly on the full nine-item scale. In Studies 3b and 3c, we invited all respondents to complete the main task, irrespective of numeracy, to ensure that our findings in Study 3a were not contingent on the relatively numerate mTurk sample.45

In Study 3a, we recruited 1,207 respondents and invited 785 (65% of sample) who passed the numeracy quiz to participate in the full survey. Our main analyses include 686 respondents (87% of screened sample) reporting a position on a concealed carry ban, which is necessary to code congeniality. Of them, 34% opposed concealed carry (i.e., favored ban) and 66% supported concealed carry (i.e., opposed ban). In Studies 3b and 3c, we recruited another 947 and 1,062 respondents, respectively. Of those indicating a position, similar percentages to those in Study 3a opposed concealed carry: 36% in Study 3b and 40% in Study 3c. The vast majority of respondents were in favor of raising the federal minimum wage: 85% in Study 3b and 65% in Study 3c.46

**Results**

We begin by presenting results from the concealed carry task, first pooling data from Studies 3a and 3b, followed by results from Study 3c. We separate out Study 3c because the concealed carry task only included the uncongenial condition. We follow it with results from the

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45 We find that low- and high-numeracy respondents are similar in terms of partisanship, ideology, and demographics (see Appendix Table A10), but we also present their results separately in the appendix.

46 Attitudes in our study are similar to public opinion in two national polls. A CBS News/New York Times poll in January 2013 finds 34% of Americans favor “a federal law requiring a nationwide ban on people other than law enforcement carrying concealed weapons” (including 19% of Republicans a 52% of Democrats). An Associated Press/GfK Poll in January 2015 finds that 77% favor raising the federal minimum wage (from $7.25/hour).
minimum wage task, and end with describing impact of treatment on respondents' subjective study ratings.

If people learn in a motivated manner, the percentage of respondents answering correctly when the study's result is congenial should be greater than when the study's result is uncongenial. Respondents who oppose concealed carry should be more likely to answer correctly if the data support the conclusion that crime is more likely to decrease in cities with concealed carry bans than in cities without such bans. Among respondents who support concealed carry, the reverse should be true. We thus examine whether the congeniality manipulation increases the probability of answering correctly.

Kahan et al. (2017) define congeniality on the basis of party identification and ideology. However, overlap between a composite of partisanship and ideology and attitude toward concealed carry is considerably short of 100%. Across the three studies, 47% of self-described liberal Democrats oppose a ban on concealed carry, and 15% of self-described conservative Republicans favor such a ban. We therefore opt for coding congeniality in terms of the attitude most directly related to the data being. Recoding congeniality on the basis of party identification and ideology, following Kahan et al., results in a substantively similar congeniality effect (see Appendix Figure A10).

Before analyzing data from the covariance detection tasks, we check to see if partisanship, ideology, and demographics are balanced across experimental conditions. The average \( p \)-value of cross-condition comparisons is .42 in Study 3a, .47 in Study 3b, .56 in Study 3c, and .48 overall (see Appendix Table A11). We also confirm that the first experimental task did not affect behavior in the second task (see Appendix Table A12).
Figure 4.3 plots the percentage of respondents answering correctly in the concealed carry task by experimental condition across Studies 3a and 3b.\textsuperscript{47} We first consider the percentage correct among concealed carry supporters in the absence of incentives (Panel A, left). When the result is uncongenial (i.e., pro-ban), only 42.6\% of respondents mark the right answer. When the result is congenial (i.e., anti-ban), the percentage increases to 54.6\%. Thus, simply changing the result from uncongenial to congenial (by swapping column headers) increases the probability of answering correctly by 12.0 percentage points (plotted in Panel B). The pattern is similar among concealed carry opponents without incentives (Panel C, left). When the result is uncongenial (i.e., anti-ban), 41.0\% of the respondents answer correctly. When it is congenial (i.e., pro-ban), the number increases to 55.5\%. The congeniality effect is 14.4 percentage points (see Panel D). Thus, in the absence of incentives, the congeniality effects among both concealed carry supporters and opponents is statistically significant.

We next examine the extent to which incentives reduce these congeniality effects, which Kahan et al. (2017) and take as evidence of selective learning.\textsuperscript{48} Examining Panel B of Figure 4.3, we see that offering incentives to concealed carry supporters does not reduce bias. The congeniality effect is 13.9 percentage points with incentives, which is almost indistinguishable from the congeniality effect without incentives (difference-in-differences is 1.9). Since a

\textsuperscript{47} We subset high-numeracy respondents in Study 3b to ensure commensurability with Study 3a. For concealed carry task results by study, see Appendix Figures A11 and A12.

\textsuperscript{48} As we note earlier, the incentive treatment was administered in such a way to minimize its influence on how respondents (initially) processed the data – incentives were revealed after the respondents had seen the data and could not go back to it. If we were successful in administering the incentive treatment in the way we intended to, respondents should be as good at recalling data in the No Incentives condition as in Incentives condition, which is indeed what we find (see Appendix Tables A13 and A14). Thus, it is unlikely that any treatment effects we see are explained by respondents paying greater attention to the task.
congeniality effect remains substantial regardless of incentive condition, it appears that concealed carry supporters indeed learn in a motivated manner.

Data from opponents of concealed carry, however, tell quite a different story (Figure 4.3, Panel D). Here, it appears that selective reporting masquerades as selective learning. Incentives lower the congeniality effect from 14.4 percentage points to an insignificant -3.9 percentage points. The difference-in-differences is -18.3 percentage points and statistically significant (s.e. = 9.3, \( p = .05 \)). Incentives completely wipe out the bias in answering the question about the study's result. Moreover, consistent with our hypothesis, the reduction in bias is entirely due to an increase in correctness in the uncongenial condition (16.0 percentage points, s.e. = 6.4, \( p < .05 \)), rather than any change in the congenial condition.

Results from Study 3c are similar to results from Studies 3a and 3b for both concealed carry supporters and opponents. Recall that Study 3c only included the uncongenial version of the concealed carry task. Figure 4.4 displays percent correct among concealed carry supporters (Panel A) and opponents (Panel B) by incentive condition. Overall, Study 3c respondents had more trouble correctly identifying the study's result than respondents in Studies 3a and 3b. For instance, only 33.6\% of concealed carry supporters correctly identified the uncongenial study's results without incentives, while 42.6\% did so in Studies 3a and 3b. More relevant for our purposes, we see that concealed carry supporters are again essentially immune to the incentive treatment. The percentage correct among this group is almost identical when we offer accuracy incentives (33.3\%). There is no evidence of selective reporting here.
Figure 4.3. Concealed Carry Task Results (Studies 3a and 3b Pooled)

Note: Panels on the left display percentage of concealed carry opponents (Panel A) and supporters (Panel C) correctly indicating study result by experimental condition. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among concealed carry opponents (Panel B) and supporters (Panel D). Vertical lines indicate 95 percent confidence intervals. Only respondents who passed the numeracy screener and indicated a position on concealed carry are included (686 in Study 3a and 604 in Study 3b).
Figure 4.4. Concealed Carry Task Results (Study 3c)

![Graph showing concealed carry results](image)

Note: Panels display percentage of concealed carry opponents (Panel A) and supporters (B) answering correctly by condition. Vertical lines indicate 95 percent confidence intervals.

Concealed carry opponents, on the other hand, once again exhibit a pattern of selective reporting. While only 25.6% answer correctly without incentives, 32.5% answer correctly when offered incentives, resulting in a treatment effect of 6.9 percentage points (s.e. = 4.4, $p < .06$). While the magnitude of the effect is smaller than in Studies 3a and 3b, it is still non-trivial. In all, the evidence suggests that selective reporting introduces substantial amounts of bias in estimates of selective learning.

We now turn our attention to the minimum wage task, which we included in Studies 3b and 3c, to probe the degree of selective learning and reporting on a different issue, using a slightly different experimental design. The overall percent correct was high in this task (87% in Study 3b and 77% in Study 3c), which is unsurprising given that we replaced frequencies with
percentages to decrease task difficulty. Figure 4.5 summarizes the results from this experiment, pooling respondents in Studies 3b and 3c.49

Results from the minimum wage task are more consistent with selective reporting than selective learning, on balance. Among opponents of raising the minimum wage, we see the familiar pattern that opponents of concealed carry display in the first task. The percentage of these respondents correctly identifying the result of the minimum wage study is 62% in the uncongenial condition and 89% in the congenial condition (Panel A). This dramatic congeniality effect is significantly reduced by incentives. Specifically, it decreases from 27 to 16 points, which is a 40% reduction (see Panel B). Again, this reduction is due to an increase in correctness in the uncongenial condition. Incentives increase the percent correct by 8.4 points in the uncongenial condition, but their effect is null in the congenial condition.

Supporters of raising the minimum wage do not behave in a manner consistent with selective learning (Panel C). In fact, the congeniality effect is a significant -10.7 points without incentives, indicating that respondents are actually less likely to correctly report a congenial result than an uncongenial result. A theoretical expectation for incentives is unclear here, because data in the control condition is neither consistent with selective learning nor responding. We do not expect incentives to significantly alter behavior if there is no bias to reduce. Indeed, incentives do little to affect responses in either the uncongenial or congenial condition, so the congeniality effect remains negative with incentives (Panel D).50

49 In Study 3b, only 129 respondents (15%) oppose raising the minimum wage. Pooling them with opponents in Study 3c yields a large enough sample to analyze. Analyzing each study separately yields substantively similar results (see Appendix Figures A13 and A14).
50 One possible explanation for this finding is that behavior in this task was affected by the previous task on concealed carry, since we did not randomize task order. We explore this possibility in the Appendix but find little support for it (see Appendix Table A12).
Figure 4.5. Minimum Wage Task Results (Studies 3b and 3c)

Note: Panels on the left display percentage of opponents (Panel A) and supporters (Panel C) of raising the federal minimum wage correctly indicating study result by experimental condition. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among opponents (Panel B) and supporters (Panel D). Vertical lines indicate 95 percent confidence intervals.
These findings as a whole suggest that selective learning is not very common. Furthermore, an analysis of cell recall in Study 3a also reveals that respondents do not selective remember the numbers in the table (see Appendix Tables A13 and A14). Across the two tasks in these three studies, we find much more evidence for selective reporting.

**Discussion**

Our findings confirm that selective learning occurs in some cases but also suggest that estimates of selective learning are upwardly biased. The results are consistent with selective learning among supporters of concealed carry. Changing congeniality affects their probability of reporting the correct answer. And accuracy incentives fail to change this tendency. Two other pieces of evidence suggest that selective learning occurs less than conventional estimates suggest. First, respondents recall data in an unbiased way. Second, among those who support increasing the minimum wage, the congeniality effect is negative. Respondents are more likely to report the correct result in the uncongenial condition than the congenial condition – the opposite of what selective learning implies.

A portion of what is thought to be selective learning is really selective reporting. When respondents are offered a mere ten cents to report their beliefs accurately, estimates of selective learning decline sharply in some cases. And given that incentives could not have affected how respondents initially processed the information, incentives very likely identify the artifactual component of evidence for selective learning.

However, there are other potential explanations for why money may reduce estimates of selective learning. The lure of making additional money may cause respondents to choose the answer that they believe the experimenter favors, rather than the one they think is right. Or, respondents may take monetary incentives as a cue that the congenial answer is incorrect. In both
cases, the decline would be artifactual. We explore both possibilities, finding little empirical support for either (see Appendix Tables A15 and A16). On balance, the data suggest that incentives reduced bias in estimates of selective learning, rather than increase it.

It is possible that the data, even accounting for incentives, still overstate the extent to which people learn selectively. It is likely that a non-trivial proportion of respondents simply tune out because they find the task too complex, or because they are disinterested in the question. Such respondents may pick an answer by taking a blind guess or by going with a congenial answer, while being aware that they have not really learned the result. Other respondents may use cheap heuristics to deduce the correct result and downgrade their certainty in what they have learned. A simple correct/incorrect scoring does not capture either of these concerns, instead treating each answer as evidence of learning a particular result. We asked people how confident they were about the answers they gave after they had selected their answers in Study 3c. Only 13% of respondents are certain of their answer in the concealed carry task without incentives. Even fewer, 10%, are certain and incorrect. This result suggests that becoming confidently misinformed due to selective learning – the gravest concern – does not happen very often.51

Lastly, data from the minimum wage task suggest that when the task is made easier, selective learning all but disappears. This may happen because when the truth is transparent and easy to grasp, even people who are prone to motivated reasoning have trouble denying it. The finding is consistent with bounded rationality: when little effort is required, selective learning is perhaps not as much an issue. This also suggests that treatments designed to teach people how to infer data correctly from the contingency table ought to prove efficacious. So should treatments that give people more time to learn, and incentivize attention.

51 In the minimum wage task, 23% of respondents are certain of their answer, and only 4% are certain and incorrect. These results are presented fully in Appendix Table A17.
Chapter 5. Not Just the Facts: Attitudinal Change

Having rigorously tested the selective learning and selective reporting hypotheses, I turn now to the final topic I explore with respect to factual information. As discussed in Chapter 1, the literature on the connection between factual information and attitudes is somewhat murky. Some prominent studies find that factual information shapes policy preferences and other related attitudes (e.g., Gilens, 2001; Bullock, 2011). Moreover, several studies have found that even uncongenial information can move partisan attitudes in the “correct” direction (Bardolph et al., 2017; Guess & Coppock, 2018; Anglin, 2019). Others find factual belief updating without much attitudinal change (e.g., Thorson, 2018; Dowling et al., 2019; Hopkins et al., 2019; Nyhan et al., 2019).

Selecting Updating Hypothesis

In this chapter, I test whether or not factual information reduces partisan gaps in subjective attitudes, such as presidential approval and policy preferences. Specifically, I test whether partisans update attitudes in response to congenial and uncongenial facts. The motivating reasoning literature predicts that partisans will be more likely to change their attitudes in response to congenial information than uncongenial information (see Figure 1.3 for a stylized representation). Even if partisans update their factual beliefs in response to uncongenial information, direction goals may inhibit updating of relevant attitudes or perhaps even cause them to update in the opposite direction (i.e., backfire).

Selective Updating Hypothesis: congenial factual information will cause partisans to update relevant attitudes more so than uncongenial factual information.
I address another shortcoming of past studies by examining attitude change over the course of multiple days. Most extant work on motivated reasoning consists of one-shot information treatments with attitudes measured shortly thereafter (for exceptions, see Guess & Coppock, 2018; Dowling et al., 2019). The panel design in Study 2 enables me to test whether any effects of factual information on political attitudes persist, even after the facts themselves have been forgotten (McGraw et al., 1990; Lodge et al., 1995). If partisans use relevant facts to update their attitudes in an online manner, there is less reason to be worried when they fail political knowledge quizzes several days later.

Attitudinal Measures

Each of the experimental studies I conducted included post-treatment measures of relevant political attitudes, which are summarized in Table 5.1 below. In each study, the attitudinal measures were administered after measures of factual learning, both of which followed the information treatments. In Study 1, I rely primarily on presidential approval. Using standard ANES wording, I asked respondents to rate President Obama’s handling of his job overall and also his job handling health care, using a standard approval four-point scale in each item. I rescaled both items to range from 0 to 1, and averaged them together to reduce measurement error.

In Study 2, I measured respondents’ approval ratings of President Obama and the ACA. Like the factual questions, these attitudinal measures were administered twice: first at the time of information exposure (Wave 1) and then again several days later (Wave 2). In both waves, I asked respondents to rate President Obama’s job overall and also his job handling health care, using a standard approval four-point scale in each item. I employed a similarly worded item to measure ACA approval, substituting “the health reform law of 2010” for the Obama reference. The approval ratings of President Obama overall and on health care are highly correlated ($r =$
As in Study 1, I average them to produce a presidential approval scale. I also average together the two items measuring support for the ACA itself, in order to produce a single measure of ACA approval ($r = .72$). I rescale both approval measures so they range from 0 to 1.

Directional motivations affect not just what people learn and report but also how credible people think a study or evidence is (e.g., Druckman & McGrath, 2019). Previous work suggests that people are more likely to question a study's credibility when its results are uncongenial than when they are congenial (e.g., Lord et al., 1979; Kunda, 1990; Ditto & Lopez, 1992). This phenomenon likely stems from the more general tendency to spend greater time and effort scrutinizing and refuting uncongenial claims than congenial ones, known as disconfirmation bias (e.g., Edwards & Smith, 1996; Taber & Lodge, 2006).

For these reasons, we might expect respondents in Study 3 to rate the evidence on concealed carry as more credible when its result is congenial than when it is uncongenial. To test this hypothesis, Study 3 asked respondents to rate how “convincing” and “well done” they judged the concealed carry study to be. Both items were administered at the end of the study (after the primary dependent variable). They were each measured on a 0-10 scale and averaged together to form a single rating (Cronbach’s $\alpha = .82$).

Table 5.1. Attitudinal Measures Analyzed in Studies 1 through 3

<table>
<thead>
<tr>
<th>Study</th>
<th>Attitudinal Measure</th>
<th>Range</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study 1</td>
<td>Presidential Approval Scale</td>
<td>0 to 1</td>
</tr>
<tr>
<td>Study 2</td>
<td>Presidential and ACA Approval Scales</td>
<td>0 to 1</td>
</tr>
<tr>
<td>Study 3</td>
<td>Rating of Concealed Carry Study</td>
<td>0 to 10</td>
</tr>
</tbody>
</table>

52 While I group these ratings with other attitudes, they are qualitatively different than presidential approval and policy preferences. These ratings were not incentivized, because unlike the question about study's result, they are inherently subjective.
Results

Presidential Approval (Study 1)

I first examine whether factual information about the unemployment rate shifts attitudes toward the president. Figure 5.1 plots the combined measure of presidential approval in Study 1. The dashed horizontal lines show approval in the No Info and Neutral conditions (pooled), among Democrats (blue, top) and Republicans (red, bottom). Unsurprisingly, there is a large difference in baseline approval level between Democrats and Republicans. However, approval varies across experimental conditions among both Democrats and Republicans. For instance, the Republican Congenial treatment results in a marginal decrease in approval among Democrats from a baseline of .70 to .65 ($p < .1$). Moreover, the Republican Congenial treatment decreases Democratic approval by .07, relative to the Democratic Congenial condition ($p = .04$). Thus, uncongenial facts about unemployment cause Democrats to downgrade their approval ratings. Interestingly, the Both condition also produces a decline of .06, relative to the Democratic Congenial condition ($p = .06$).

The results among Republicans are less crisp, but there is indeed variation in approval across conditions. The Democratic Congenial condition increases it from a baseline of .14 to .19, and the Republican Congenial decreases it by the same amount, though neither effect is statistically significant. As with Democrats, the largest difference is between the Democratic and Republican Congenial conditions (.09, $p = .05$). Thus, Republican approval is also somewhat responsive to facts. Taken as a whole, the results among Democrats and Republicans are not consistent with selective attitude updating.

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53 All p-values reported here are one-tailed, because the hypotheses regarding attitudinal change are directional.
Figure 5.1. Presidential Approval by Experimental Condition (Study 1)

Note: Dashed horizontal lines indicate baseline approval level of Democrats (blue) and Republicans (red), rescaled between 0 (strongly disapprove) to 1 (strongly approve). Baseline approval pools No Info and Neutral conditions. Points indicate effects of Democratic Congenial, Republican Congenial, and 'Both' treatments, relative to baseline. Vertical lines indicate 95 percent confidence intervals of treatment effects.

Incumbent Evaluations and Policy Attitudes (Study 2)

I next examine whether partisans update relevant attitudes in response to the facts about the ACA in Study 2. Figure 5.2 plots the effects of congenial and uncongenial facts on approval of President Obama and the ACA by survey wave. Democrats update both ratings positively in response to congenial (i.e., pro-ACA) facts and negatively in response to uncongenial (i.e., anti-ACA) facts in both waves. For example, presidential approval is .68 among Democrats in the control condition of Wave 1 (Panel A). Approval increases to .75 in the Democratic Congenial
condition ($p < .01$), and decreases to .63 in the Republican Congenial condition ($p = .01$). Both effects persist in Wave 2 (Panel B). Democrats remain significantly more supportive after receiving congenial facts multiple days earlier ($p = .001$), and marginally less supportive after receiving uncongenial facts ($p = .05$). ACA approval exhibits similar patterns of responsiveness and persistence among Democrats (see Panels C and D).

Republicans, on the other hand, only update their approval ratings in response to congenial (i.e., anti-ACA) information. In Wave 1, Republicans start out disapproving strongly of both President Obama (Panel A) and the ACA (Panel C). And they do not budge in response to pro-ACA facts, which is evident from the Democratic Congenial null treatment effect. They do, however, update approval ratings in response to anti-ACA facts. In the Republican Congenial condition, presidential approval decreases from .21 to .14 ($p = .005$), and ACA approval decreases from .26 to .19 ($p = .03$). These effects not only persist in Wave 2, but they even increase in magnitude (see Panels B and D). Thus, while Democrats appear to assimilate factual information about the ACA into relevant attitudes fairly evenhandedly, Republicans appear to update their attitudes selectively.
Figure 5.2. Attitudinal Change among Partisans in Response to Congenial and Uncongenial Facts (Study 2)

Note: Dashed horizontal lines indicate approval level of Democrats (blue) and Republicans (red) in control condition, rescaled to range from 0 (strongly disapprove) to 1 (strongly approve). Points indicate effects of Democratic Congenial and Republican Congenial treatments, relative to control condition. Vertical lines indicate 95 percent confidence intervals of treatment effects.
Biased Evaluations of Concealed Carry Study (Study 3)

Does the congeniality of the hypothetical study’s result affect how positively respondents rate the study? In Figure 5.3, I plot the effect of the congeniality treatment on ratings among concealed carry supporters (Panel A) and opponents (Panel B). I estimate the overall effect first and then disaggregate it by incentive condition. Among concealed carry supporters, the overall congeniality effect is a marginally significant .25 ($t = 1.53, p = .06$). Among concealed carry opponents, the overall effect is .20, which is in the hypothesized direction but not significant.

Drilling down, we see that congeniality only changes study ratings in the presence of incentives. Among concealed carry supporters, the congeniality effect is null without incentives and a significant .41 with incentives ($t = 1.78, p = .04$). Among opponents too, a congeniality effect only appears in the Incentive condition ($t = 1.41, p = .08$). It may be the case that when incentivized respondents to admit having learned an uncongenial fact, they express their displeasure via study ratings. If so, we should observe a greater congeniality effect among respondents who correctly report the study’s result than among those who are incorrect. Indeed, the congeniality effect only occurs among respondents who answered correctly. Among correct concealed carry supporters, the incentivized effect is 1.05 ($t = 3.31, p < .001$). Among correct opponents, the incentivized effect is 1.39 ($t = 3.43, p < .001$). These findings are only suggestive, because correctness is of course endogenous.

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54 Pooling concealed carry supporters and opponents, we find that ratings increase from 4.79 if the study is uncongenial to 5.02 if the result supported by the study is congenial ($t = 1.79, p = .04$). With incentives, the effect is .42 ($t = 2.30, p = .01$), and in the absence of incentives, the effect is only .08 ($t = .46, p = .32$). All $t$-tests reported here are one-tailed.
Figure 5.3. Congeniality Effect on Study Ratings

Note: Figure displays effect of congeniality manipulation on average study ratings (0-10 scale), among concealed carry opponents (A) and supporters (B). Data are pooled from Studies 3a and 3b. Effect is calculated overall and disaggregated by incentive condition. Vertical lines indicate 90 percent confidence intervals.
Chapter 6. Conclusion

My findings are cause for cautious optimism about the quality of public opinion. Comprehensive estimates of partisan bias in beliefs about the economy suggest that it is not as severe as past work implied and that it varies considerably by issue and over time, which was the main takeaway from Chapter 2. Furthermore, partisan bias in surveys of factual beliefs do not always reflect deep-seated differences in what people truly believe (Bullock et al., 2015; Prior et al., 2015). Estimates of selective factual learning are due in part to selective reporting, a bias in the survey response process rather than the learning process (Chapter 4). While such bias may itself be normatively troubling, this is a much different scenario than a world in which partisans confidently hold dramatically different beliefs from one another. To the contrary, small nudges reveal that people actually know more than they might let on, or at the very least, are capable of making educated guesses.

It is also good news that partisans learn evenhandedly when exposed to the same facts as one another. In Study 1 in Chapter 3, I find that Democrats accurately learn the unemployment rate, irrespective of whether it is framed as positive or negative for their party. Moreover, Democrats and Republicans learn about an increase in health insurance coverage (good news) or a hike in family premiums (bad news) under the new health care law, whether or not these facts are framed as successes or failures.

These findings on learning echo several recent studies on factual information that push back against the idea of strong motivated reasoning, at least with respect to factual learning (e.g., Hill, 2017; Guess & Coppock, 2018; Porter et al., 2018; Bisgaard, 2019; Dowling et al., 2019; Wood & Porter, 2019). These studies, which consist primarily of randomized experiments, are careful about both internal validity and measurement. On the whole, they find receptivity to
factual information on the part of partisans, and very few instances of backfire. This dissertation adds several more data points in support of evenhanded, as opposed to selective, learning.

**What to Make of Null Effects**

Several of the experimental manipulations in Chapter 3 produced null effects, raising the question of whether the treatments had the intended effect on participants. While some of the manipulations were quite subtle – adding just a word in one case – they did produce the intended effects in terms of perceived congeniality of the factual information being presented. The evidence for this point comes from the out-of-sample pretest I conducted (see Appendix Table A8). For instance, in Study 2, the subtle wording tweak to cast the decrease in the uninsured rate in a negative light substantially affected the fact’s perceived congeniality: both Democrats and Republicans became less likely to say that this fact reflected positively on the Democratic Party. Similarly, a small wording change to cast the increase in family premiums in a more positive light made respondents across partisan lines more likely to say the fact reflected positively on the Democratic Party. These results serve as a manipulation check, lending greater validity to the null results with respect to learning. We can confidently say that information congeniality indeed changed in respondents’ minds, but this change did not affect their learning.

Moreover, some of the factual information tested did not rely on subtle wording manipulations. For example, one of the extra facts in Study 2 was the large increase in the number of young adults with health insurance. This extra fact was included because it is unambiguously positive, a sentiment that respondents almost universally agreed with: according to the pretest, 87% reported that this fact reflected positively on Democrats. Comparing the inclusion of the fact to a non-informative baseline condition yielded substantively similar results to the wording manipulations discussed above: Democrats and Republicans learned this
Democratic congenial fact at similar rates to one another. The second extra fact in Study 2 concerned the canceled insurance policies after the ACA took effect. Again, Democrats and Republicans both learned the unambiguously negative fact. Moreover, Democrats were more likely than Republicans to recall this information in the second wave. In summary, the null effects with respect to factual learning are more likely to reflect evenhandedness among partisans than failure to manipulate congeniality.

These findings suggest that learning and recall of factual information may not depend heavily on whether it threatens core attachments. To the contrary, providing factual information to the public can reduce partisan differences observed in surveys, particularly if ignorance, rather than misinformation, is the main culprit for differences. Study 2 makes an important contribution by measuring whether respondents recall or assimilate factual information beyond the initial study context. While forgetfulness is characteristic of the public’s grasp of political information, partisans appear to be quite evenhanded in the facts they remember. This result is consistent with other recent work that measures learning in the long term (Guess & Coppock, 2018; Dowling et al., 2019). Replicating these findings in other issue areas and with a broader array of information treatments would of course lend greater confidence to this idea.

One possibility is that partisans process factual information in an online manner (see, e.g., McGraw et al., 1990; Lodge et al., 1995). For example, people may learn facts about the economy, assimilate new information into their overall assessment of the issue, and then forget the facts themselves. When asked about their factual beliefs on a survey, they may provide a combination of their best guess (e.g., “the unemployment rate last year was 5 percent, so it’s probably a bit lower than that now”) and a top-of-the-head inference based on their general sentiment (e.g., “the economy is generally good, so unemployment rate must be pretty low” or
“Trump is president, so unemployment rate must be pretty low”). They may also use the survey response as an opportunity to satisfy express goals (e.g., “I support Trump and this pollster should know about it”).

**Accuracy Incentives and Bias**

Whatever the exact response process, minor nudges can improve respondents’ accuracy and reduce partisan gaps. This result has important implications for survey methodology. Small incentives – whether monetary or non-monetary – are likely to produce better measures of certain types of beliefs, which survey respondents would otherwise selectively report. If a researcher’s goal is to precisely measure firm beliefs, then incentives may be the best way to obtain unbiased estimates. These incentives can be implemented in a relatively costless way (Prior et al., 2015). However, in some cases, researchers may want to measure a mix of firm beliefs and more symbolic considerations, in which case ordinary survey items are more suitable.

However, other attempts to improve accuracy via incentives have been less successful. Berisnky (2012) finds little evidence of selective reporting using incentives to correct misperceptions about a variety of issues, including Barack Obama’s birthplace. Further research is necessary to understand the conditions under which selective reporting occurs. One possibility is simply that correcting firmly held misperceptions is harder than increasing accuracy in learning new information or reporting less confident factual beliefs, like educated guesses about economic indicators. A similar explanation may help reconcile seemingly contradictory findings on attitude change in past work – experiments that instill false beliefs may have greater difficult in subsequently moving opinions in the opposite direction.

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55 For example, Kraus et al. (2017) show that the U.S. public systematically overstates economic equality between races; however, these beliefs are quite soft and malleable.
However, incentivizing respondents to accurately report factual information can have the adverse effect of increasing bias elsewhere. In Study 3, respondents rate the hypothetical study as less well done and less convincing when incentivized to report an uncongenial result. One possible explanation for this finding is that respondents who admit an uncongenial factual result use a more subjective question as an opportunity to satisfy partisan directional goals.

**Partisan Asymmetries**

I observed partisan asymmetries in both Studies 2 and 3. In Study 2, Democrats respond to congenial and uncongenial facts by updating attitudes, while Republicans are more responsive to congenial facts than uncongenial ones. Republicans appear to ignore uncongenial facts when determining their level of support for the President and the ACA, for instance, confirming worries that policy preferences are not responsive to facts on the ground. In Study 3, incentives reduced the congeniality effect among those who oppose concealed carry, and not among those who support it.

One possible moderator of these effects is issue importance. It is possible that Republicans feel more strongly about the ACA than Democrats, and are therefore more likely to resist changing their mind than their less emotionally invested counterparts. On a similar note, supporters of concealed carry have a stronger affective reaction to the issue of gun control and experience a greater directional pull toward their preferred conclusion. Explicitly measuring issue importance, as well as employing a variety of issues that range in perceived importance, could shed light on this possibility. In addition, measuring traits that are thought to be correlated with motivated reasoning would shed light on whether these asymmetric findings stem more from individual differences or from contextual variables.
Moving Beyond the Facts

Agreeing on the facts is one thing, but agreeing on opinions is quite another. On this point, Daniel Patrick Moynihan’s quip about facts versus opinions has more than one interpretation. He is often interpreted to mean that everyone should agree on the truth-value of a particular factual statement, e.g., “the earth is round”. Alternatively, he could mean that everyone should be working with a common set of facts. A charitable reading of presidential advisor Kellyanne Conway’s use of the term “alternative facts” is that in practice people bring different sets of facts to bear on political issues and debates. People may simply disagree about which facts are relevant to a particular issue, while agreeing on the truth-value of each fact itself.

Even when partisans agree are working with the same set of facts, there is still plenty of room for differing opinions. Partisans may interpret them differently from one another and thus fail to update relevant evaluations. For example, Gaines et al. (2007) find that while Democrats and Republicans came to form relatively accurate factual beliefs about the Iraq War, they continued to diverge in interpretations of these facts. As discussed previously, there are potentially several steps between learning a congenial or uncongenial fact and revising attitudes. Institutional design and contextual conditions can make it easier or harder for voters to assign credit and blame for the facts on the ground (e.g., Anderson, 2007; Duch & Stevenson, 2011).

By interpreting the same facts in different ways, partisans may avoid updating their attitudes. Indeed, Tilley and Hobolt (2011) and Bisgaard (2015, 2019) demonstrate that partisan attributions of responsibility play a critical role in linking perceptions of the economy and evaluations of government. Biased attributions enable partisans to escape blaming their side for poor real-world conditions and avoid crediting the other side for good conditions.

This line of research may explain my findings with respect to attitudinal change, which are somewhat mixed and less rosy about the quality of public opinion. In Study 2, Democrats
readily incorporate congenial and uncongenial facts into their attitudes toward President Obama and support for the ACA, and these attitudinal shifts persist for several days. This is welcome news from the perspectives of voter competence and democratic accountability. However, Republicans exhibit a markedly different pattern, updating their attitudes in response to congenial information while resisting uncongenial information. It is possible that Republicans avoided revising their opinions in response to positive news about the uninsured rate and family premiums by attributing these changes to something other than the Obama administration or its new health care law. And in Study 3, inducing respondents to agree on the conclusion supported by the gun control research, produced bias in their evaluations of the quality of the research. From a normative standpoint, the actual conclusion should have little relation with the quality of the study. These results suggest that when respondents are forced to admit an unflattering truth, bias emerges in other ways.

Combining the attitudinal results in Studies 2 and 3, I cannot rule out that the process of incorporating new facts into relevant attitudes occurs in a motivated manner, protecting partisan loyalty. One strength of the research designs employed here is the careful experimental design, in which any changes in attitudes can be attributed to the congeniality of the factual information provided. We can be sure that facts are driving opinions in these studies and not vice versa. However, one limitation is the breadth of post-treatment attitudes measured. It is possible that respondents would have revised other opinions, such as their support for gun control in Study 3, more than the attitudes tested here. Unfortunately, these findings do not go too far in clarifying the already muddy literature on the link between facts and opinions. Much work remains to be done to understand more fully the different steps that partisan directional goals interfere with among the public, between learning new facts and truly changing one’s mind about an issue.
References
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of reported attitude change. *Journal of Personality and Social Psychology*, 64(4), 561-574.


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Chapter 2 Supplementary Information

Items Included in Database

Here is a sample of illustrative items from the database. A comprehensive list of items analyzed in Chapter 2 is included in the following pages and sorted by indicator.

“Just your best guess: What percentage of all American adults are unemployed and looking for work?”

Closed-Ended/Current/Quantitative (Pew Research Center, October 2009):
“Do you happen to know if the Dow Jones Industrial Average is currently closer to 3,000, 6,000, 10,000, or 16,000?”

Closed-Ended/Retrospective/Quantitative (Pew Research Center, August 1997):
“Compared to five years ago, has the federal budget deficit increased, decreased, or stayed about the same?”

Closed-Ended/Retrospective/Qualitative (Los Angeles Times, August 1982):
“Some people say that the prices we pay today for merchandise and for services are not going up as fast as they did a year ago. Other people say that prices are going up even faster than they used to, and still other people say that prices we pay today for merchandise and for services continue to rise at about the same rate that they did last year. What do you think? Has inflation gotten worse, has it gotten better, or has inflation stayed about the same as it was a year ago?”
### Table A1. Keyword Searches to Identify Database Items

<table>
<thead>
<tr>
<th>All Polls</th>
<th>Most Recent 1,000 Polls</th>
<th>Most Recent 700 Polls</th>
<th>Most Recent 300 Polls</th>
<th>All ABC News/ Washington Post Polls</th>
<th>All Gallup and Pew Polls</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;unemployment&quot; AND &quot;best guess&quot;</td>
<td>&quot;unemployed&quot;</td>
<td>&quot;inflation&quot;</td>
<td>&quot;deficit&quot;</td>
<td>&quot;unemployed&quot;</td>
<td>&quot;unemployed&quot;</td>
</tr>
<tr>
<td>&quot;looking for work&quot; AND &quot;best guess&quot;</td>
<td>&quot;unemployment&quot;</td>
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<td></td>
<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td>&quot;unemployment&quot;</td>
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<td>&quot;stock market&quot;</td>
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<td></td>
<td></td>
<td></td>
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<td></td>
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<td>&quot;deficit&quot; AND &quot;smaller&quot;</td>
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<td>&quot;Dow Jones&quot;</td>
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<tr>
<td>&quot;stock market&quot; AND &quot;best guess&quot;</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>&quot;stock market&quot; AND &quot;value&quot;</td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>

*Note: Column headings identify the set of polls examined for each keyword search. For instance, the second column indicates that the 1,000 most recent polls containing the terms “unemployed” or “unemployment” were examined for relevant items.*

### Table A2. Survey Questions about Unemployment Rate
<table>
<thead>
<tr>
<th>Year</th>
<th>Month</th>
<th>Firm</th>
<th>Response Format</th>
<th>Current vs. Retrospective (Time Period)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1980</td>
<td>Apr</td>
<td>CBS/New York Times</td>
<td>Open</td>
<td>Current</td>
</tr>
<tr>
<td>1982</td>
<td>Aug</td>
<td>LA Times</td>
<td>Open*</td>
<td>Current</td>
</tr>
<tr>
<td>1983</td>
<td>May</td>
<td>ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (Getting Better/Worse)</td>
</tr>
<tr>
<td>1983</td>
<td>Jun</td>
<td>ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (Getting Better/Worse)</td>
</tr>
<tr>
<td>1983</td>
<td>Dec</td>
<td>LA Times</td>
<td>Closed</td>
<td>Retrospective (Three Years)</td>
</tr>
<tr>
<td>1985</td>
<td>Jul</td>
<td>ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>1988</td>
<td>Sep-Nov</td>
<td>ANES</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>1992</td>
<td>Jan</td>
<td>Gallup</td>
<td>Closed</td>
<td>Retrospective (Three Years)</td>
</tr>
<tr>
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<td>Sep-Nov</td>
<td>ANES</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>1996</td>
<td>Jan</td>
<td>Gallup</td>
<td>Closed</td>
<td>Retrospective (Three Years)</td>
</tr>
<tr>
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<td>Jul</td>
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<td>Closed</td>
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<td>1996</td>
<td>Oct</td>
<td>Washington Post</td>
<td>Open</td>
<td>Current</td>
</tr>
<tr>
<td>1997</td>
<td>Aug</td>
<td>Pew</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
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<td>Aug</td>
<td>Rutgers University</td>
<td>Open</td>
<td>Current</td>
</tr>
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<td>ANES</td>
<td>Closed</td>
<td>Retrospective (Five Years)</td>
</tr>
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<td>2003</td>
<td>Oct</td>
<td>Gallup</td>
<td>Closed</td>
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<td>Closed</td>
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<td>Jun</td>
<td>PSRA/Newsweek</td>
<td>Closed</td>
<td>Current</td>
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<td>Sep-Nov</td>
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<td>Pew</td>
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<td>Pew</td>
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<td>Current</td>
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<td>Sep-Nov</td>
<td>ANES</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
</tbody>
</table>

*Response options initially offered with follow-up probe asking for point estimate.
Table A3. Survey Questions about Federal Budget Deficit

<table>
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<tr>
<th>Year</th>
<th>Month</th>
<th>Firm</th>
<th>Response Format</th>
<th>Current vs. Retrospective (Time Period)</th>
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<td>Nov</td>
<td>Open</td>
<td>Current</td>
</tr>
<tr>
<td>3.</td>
<td>1983</td>
<td>Nov</td>
<td>Open</td>
<td>Current</td>
</tr>
<tr>
<td>4.</td>
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<td>Apr</td>
<td>Closed</td>
<td>Retrospective (Five Years)</td>
</tr>
<tr>
<td>5.</td>
<td>1989</td>
<td>Jan</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>6.</td>
<td>1989</td>
<td>Jan</td>
<td>Closed</td>
<td>Retrospective (Ten Years)</td>
</tr>
<tr>
<td>7.</td>
<td>1995</td>
<td>Nov</td>
<td>Closed</td>
<td>Retrospective (Three Years)</td>
</tr>
<tr>
<td>8.</td>
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<td>Jul</td>
<td>Closed</td>
<td>Retrospective (Five Years)</td>
</tr>
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<td>Closed</td>
<td>Retrospective (Five Years)</td>
</tr>
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<td>Closed</td>
<td>Retrospective (Three Years)</td>
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### Table A4. Survey Questions about Inflation Rate

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<th>Firm</th>
<th>Response Format</th>
<th>Current vs. Retrospective (Time Period)</th>
</tr>
</thead>
<tbody>
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<td>Open</td>
<td>Current</td>
</tr>
<tr>
<td>2.</td>
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<td>Jun NBC/Associated Press</td>
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<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>3.</td>
<td>1982</td>
<td>Jul LA Times</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>4.</td>
<td>1982</td>
<td>Aug LA Times</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
<td>5.</td>
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<td>Jan ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
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<td>Feb ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
</tr>
<tr>
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<td>Apr ABC News/Washington Post</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
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<td>Closed</td>
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<td>Aug Time</td>
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<td>Retrospective (One Year)</td>
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<td>Sep-Nov ANES</td>
<td>Closed</td>
<td>Retrospective (One Year)</td>
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<td>Closed</td>
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### Table A5. Survey Questions about Dow Jones Industrial Average

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<th>Firm</th>
<th>Response Format</th>
<th>Current vs. Retrospective (Time Period)</th>
</tr>
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<td>Aug Pew</td>
<td>Closed</td>
<td>Retrospective (Past Few Months)</td>
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<td>Apr Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>3.</td>
<td>2007</td>
<td>Jun PSRA/Newsweek</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>4.</td>
<td>2007</td>
<td>Aug Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
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<td>Feb Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>6.</td>
<td>2008</td>
<td>Dec Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>7.</td>
<td>2009</td>
<td>Mar Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>8.</td>
<td>2009</td>
<td>Oct Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>9.</td>
<td>2010</td>
<td>Jan Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
<tr>
<td>10.</td>
<td>2011</td>
<td>Sep Pew</td>
<td>Closed</td>
<td>Current</td>
</tr>
</tbody>
</table>
Additional Analyses

Figure A1. Percentage of Respondents Overestimating Federal Budget Deficit

*Note: Questions marked by an asterisk asked for current estimates of the deficit, while the rest were retrospective questions asking about change in the deficit over time.*
Figure A2. Percentage of Respondents Underestimating Federal Budget Deficit

Note: Questions marked by an asterisk asked for current estimates of the deficit, while the rest were retrospective questions asking about change in the deficit over time.
Figure A3. Percentage of Respondents Overestimating Inflation Rate

Note: Questions marked by an asterisk ask for current estimates of inflation, while the rest ask about changes in the inflation rate over time.
Figure A4. Percentage of Respondents Underestimating Inflation Rate

Figure A5. Percentage of Respondents Underestimating Dow Jones Average

Note: Question marked by an asterisk asks about the change in the Dow Jones over time, while the rest ask for current estimates of the Dow Jones.
**Full Ordered Probit Regression Results**

Table A6 displays the full ordered probit results for each economic indicator, where the coefficient of interest (Democrat) is in the first row. Consistent with partisan bias, the effect of partisanship is significantly positive for three indicators: unemployment (columns 1 and 2), the federal budget deficit (columns 3 and 4), and inflation (columns 5 and 6). However, the effect of partisanship on perceptions of the Dow Jones is indistinguishable from zero (columns 7 and 8).

### Table A6. Ordered Probit Regression Results Pooling Survey Items by Indicator

<table>
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<tr>
<th>Variable</th>
<th>(1) Unemployment</th>
<th>(2) Unemployment</th>
<th>(3) Deficit</th>
<th>(4) Deficit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrat</td>
<td>.437 (.017)***</td>
<td>.495 (.019)***</td>
<td>.253 (.024)***</td>
<td>.214 (.026)***</td>
</tr>
<tr>
<td>Cutpoint 1</td>
<td>-.489 (.117)</td>
<td>-.492 (.136)</td>
<td>.606 (.050)</td>
<td>.554 (.052)</td>
</tr>
<tr>
<td>Cutpoint 2</td>
<td>1.409 (.186)</td>
<td>1.487 (.194)</td>
<td>3.611 (.060)</td>
<td>3.570 (.062)</td>
</tr>
<tr>
<td># Items</td>
<td>27</td>
<td>23</td>
<td>15</td>
<td>14</td>
</tr>
<tr>
<td># Respondents</td>
<td>22,167</td>
<td>19,481</td>
<td>14,480</td>
<td>13,590</td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>-17,429</td>
<td>-15,009</td>
<td>-7,947</td>
<td>-7,581</td>
</tr>
<tr>
<td>Full Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Variable</th>
<th>(5) Inflation Rate</th>
<th>(6) Inflation Rate</th>
<th>(7) Dow Jones</th>
<th>(8) Dow Jones</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrat</td>
<td>.468 (.021)***</td>
<td>.452 (.023)***</td>
<td>-.054 (.039)</td>
<td>-.070 (.041)†</td>
</tr>
<tr>
<td>Cutpoint 1</td>
<td>-2.286 (.057)</td>
<td>-2.121 (.060)</td>
<td>-.371 (.072)</td>
<td>-.387 (.076)</td>
</tr>
<tr>
<td>Cutpoint 2</td>
<td>-.301 (.052)</td>
<td>-.105 (.055)</td>
<td>2.133 (.082)</td>
<td>2.142 (.088)</td>
</tr>
<tr>
<td># Items</td>
<td>19</td>
<td>18</td>
<td>9</td>
<td>8</td>
</tr>
<tr>
<td># Respondents</td>
<td>16,035</td>
<td>14,864</td>
<td>4,159</td>
<td>3,871</td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>-10,936</td>
<td>-9,994</td>
<td>-2,960</td>
<td>-2,742</td>
</tr>
<tr>
<td>Full Controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
</tbody>
</table>

*Note: Significance denoted by *** p < .001, ** p < .01, * p < .05, † p < .10. All models include item fixed effects, education, and income. Full controls add race and gender.*
Chapter 3 Supplementary Information

Study 1 Question Wording

Factual Questions

Please answer the following questions to the best of your ability. If you are not sure, please give us your best guess.

1. What is the current national unemployment rate? Please enter a percent between 0 and 100.

2. What has been the average national unemployment rate under Barack Obama? Please enter a percent between 0 and 100.

3. What was the average national unemployment rate under George W. Bush? Please enter a percent between 0 and 100.

4. Would you say that over the past year, the level of unemployment in the country has gotten better, stayed about the same, or gotten worse?

Attitudinal Measures

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

2. Do you [approve/disapprove] strongly or not strongly?

3. Do you approve or disapprove of the way Barack Obama is handling the economy?

4. Do you [approve/disapprove] strongly or not strongly?

5. How responsible is Barack Obama for the current condition of the nation's economy? (Not at all responsible, somewhat responsible, very responsible, completely responsible)

6. How responsible is George W. Bush for the current condition of the nation's economy? (Not at all responsible, somewhat responsible, very responsible, completely responsible)

7. For you personally, which of the following do you consider the best measure of how the national economy is doing?
   - The stock market index
   - The unemployment rate and jobs reports
   - The prices of goods and services you buy
   - Your personal finances and those of people you know
Study 2 Experimental Stimuli

Control Condition

Health Reform at Five Years Old

Five years ago marked the passage of the Patient Protection and Affordable Care Act, commonly known as Obamacare. The law was designed to increase access to health insurance and to make the health care system more efficient. Critics worried that it would increase costs without significantly improving coverage. Congress passed the health reform law on March 23, 2010.

Here is a summary of the major changes the law made:

- Established minimum standards for health insurance;
- Allowed children to stay on their parents’ insurance plans until the age of 26;
- Created insurance exchanges to buy policies;
- Provided insurance subsidies to low-income Americans; and
- Required most people to be insured or pay a penalty.

Five years later, by any reasonable and objective criteria, Obamacare has had successes and failures. According to Gallup, a non-partisan polling organization, the percentage of U.S. adults who are uninsured is currently 12 percent. It has decreased by about 5 percentage points since the law started requiring people to be insured.

According to the Kaiser Family Foundation, the average annual premiums for employer-sponsored health insurance were $16,834 for family coverage in 2014. Compared to the previous year, average family premiums increased by 3 percent.

In 2015, it has become increasingly clear that Obamacare has accomplished some things President Obama promised but not others. By expanding coverage, it has left Americans better off. However, by increasing costs, it has left others behind.

Note: Underlined text is unique to this experimental condition.
Democratic Congenial (i.e., Pro-ACA) Condition

Obamacare at Five Years Old

Five years ago marked the passage of the Patient Protection and Affordable Care Act, commonly known as Obamacare. The law was designed to increase access to health insurance and to make the health care system more efficient. Critics worried that it would increase costs without significantly improving coverage. Congress passed the health reform law on March 23, 2010.

Here is a summary of the major changes Obamacare made:

- Established minimum standards for health insurance;
- Allowed children to stay on their parents insurance plans until the age of 26;
- Created insurance exchanges to buy policies;
- Provided insurance subsidies to low-income Americans; and
- Required most people to be insured or pay a penalty.

Five years later, by any reasonable and objective criteria, Obamacare has been a huge success. Millions of Americans now have health insurance, many for the first time. According to Gallup, a non-partisan polling organization, the percentage of U.S. adults who are uninsured is currently 12 percent. It has decreased by about 5 percentage points since the law started requiring people to be insured.

(Randomize:) As a result of allowing young adults to stay on their parents insurance plans, almost 6 million young adults have gained coverage over the past five years.

According to the Kaiser Family Foundation, the average annual premiums for employer-sponsored health insurance were $16,834 for family coverage in 2014. Compared to the previous year, average family premiums increased by only 3 percent, which is a record low.

In 2015, it has become increasingly clear that Obamacare has accomplished what President Obama promised. By significantly expanding coverage and slowing down costs, it has left Americans much better off.

Note: Underlined text is unique to this experimental condition.
Republican Congenial (i.e., Anti-ACA) Condition

Obamacare at Five Years Old

Five years ago marked the passage of the Patient Protection and Affordable Care Act, commonly known as Obamacare. The law was designed to increase access to health insurance and to make the health care system more efficient. Critics worried that it would increase costs without significantly improving coverage. Congress passed the health reform law on March 23, 2010.

Here is a summary of the major changes Obamacare made:
- Established minimum standards for health insurance;
- Allowed children to stay on their parents insurance plans until the age of 26; 
- Created insurance exchanges to buy polices;
- Provided insurance subsidies to low-income Americans; and
- Required most people to be insured or pay a penalty.

Five years later, by any reasonable and objective criteria, Obamacare has been a huge failure. Millions of Americans are still uninsured. According to Gallup, a non-partisan polling organization, the percentage of U.S. adults who are uninsured is currently 12 percent. It has decreased by only 5 percentage points since the law started requiring people to be insured.

(Randomize:) After the law went into effect, almost 5 million people across 31 states lost their health insurance, because their policies did not meet the standards of the new law.

According to the Kaiser Family Foundation, the average annual premiums for employer-sponsored health insurance were $16,834 for family coverage in 2014. Compared to the previous year, average family premiums increased by 3 percent.

In 2015, it has become increasingly clear that Obamacare has not accomplished what President Obama promised. By increasing costs without significantly increasing coverage, it has left Americans much worse off.

Note: Underlined text is unique to this experimental condition.
Study 2 Question Wording

Factual Questions

Please answer the following questions to the best of your ability. If you are not sure, please give us your best guess.

1. In which year was the Patient Protection and Affordable Care Act, also known as Obamacare, signed into law?
   - 2009
   - 2010 (Correct)
   - 2011
   - 2012

2. Which of the following are features of the 2010 health reform law, also known as Obamacare? Check all that apply.
   - Required everybody to be insured or pay a penalty (True)
   - Provided insurance subsidies to low-income Americans (True)
   - Eliminated minimum standards for health insurance (False)
   - Created health insurance exchanges to buy policies (True)
   - Provided insurance subsidies to undocumented immigrants (False)

3. Until what age can children stay on their parents’ health insurance plans under the 2010 health reform law?
   - 18 years old
   - 21 years old
   - 26 years old (Correct)
   - 31 years old

4. What percentage of U.S. adults does not currently have health insurance? Please enter a value between 0 and 100. (Answer: 12 percent)

5. Has the percentage of U.S. adults without health insurance increased, stayed about the same, or decreased since the law started requiring individuals to be insured?
   - Increased
   - Stayed about the same
   - Decreased (Correct)

6. How much did the percentage of U.S. adults without health insurance change since the law started requiring people to be insured? Please enter a value between 0 and 100. (Answer: 5 percentage points)

7. How much did average family premiums for employer-sponsored coverage increase in 2014? Please enter a value between 0 and 100. (Answer: 3 percent)
8. Roughly how many young adults have gained health insurance coverage over the past five years?
   - 3 million
   - 6 million (Correct)
   - 9 million
   - 12 million

9. Roughly how many people lost their health insurance across 31 states after the law went into effect?
   - 1 million
   - 3 million
   - 5 million (Correct)
   - 7 million

Attitudinal Measures

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

2. Do you [approve/disapprove] strongly or not strongly?

3. Do you approve or disapprove of the way Barack Obama is handling health care in the U.S.?

4. Do you [approve/disapprove] strongly or not strongly?

5. Do you generally approve or disapprove of the health reform law of 2010?

6. Do you [approve/disapprove] strongly or not strongly?

7. What would you like to see Congress do when it comes to the 2010 health reform law?
   - Expand what the law does
   - Move forward with implementing the law as it is
   - Scale back what the law does
   - Repeal the entire law
   - None of these/something else
Sample Characteristics

Table A7 compares the party identification and demographic characteristics of the Study 1 and Study 2 samples with a representative sample of U.S. adults. Like most AMT samples, both the Study 1 and Study 2 samples are more Democratic, male, young, white, and educated than a national sample. National percentages are based on U.S. Census Bureau data, with the exception of party identification, for which 2012 American National Election Study data is used. Democrat and Republican percentages include partisan leaners.

Table A7. Sample Characteristics by Study Compared to National Benchmarks

<table>
<thead>
<tr>
<th></th>
<th>Study 1</th>
<th>Study 2: Wave 1</th>
<th>Study 2: Wave 2</th>
<th>National Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrat</td>
<td>61%</td>
<td>74%</td>
<td>75%</td>
<td>46%</td>
</tr>
<tr>
<td>Republican</td>
<td>23%</td>
<td>26%</td>
<td>25%</td>
<td>39%</td>
</tr>
<tr>
<td>Pure Independent</td>
<td>16%</td>
<td>n/a</td>
<td>n/a</td>
<td>15%</td>
</tr>
<tr>
<td>Female</td>
<td>46%</td>
<td>47%</td>
<td>48%</td>
<td>52%</td>
</tr>
<tr>
<td>Male</td>
<td>54%</td>
<td>53%</td>
<td>52%</td>
<td>48%</td>
</tr>
<tr>
<td>18-29 years old</td>
<td>43%</td>
<td>48%</td>
<td>48%</td>
<td>22%</td>
</tr>
<tr>
<td>30-39 years old</td>
<td>29%</td>
<td>30%</td>
<td>31%</td>
<td>17%</td>
</tr>
<tr>
<td>40-49 years old</td>
<td>14%</td>
<td>12%</td>
<td>11%</td>
<td>17%</td>
</tr>
<tr>
<td>50+ years old</td>
<td>14%</td>
<td>10%</td>
<td>10%</td>
<td>44%</td>
</tr>
<tr>
<td>White</td>
<td>76%</td>
<td>73%</td>
<td>74%</td>
<td>62%</td>
</tr>
<tr>
<td>Black</td>
<td>8%</td>
<td>6%</td>
<td>6%</td>
<td>13%</td>
</tr>
<tr>
<td>Latino</td>
<td>6%</td>
<td>9%</td>
<td>8%</td>
<td>17%</td>
</tr>
<tr>
<td>Asian</td>
<td>7%</td>
<td>10%</td>
<td>10%</td>
<td>5%</td>
</tr>
<tr>
<td>Other</td>
<td>2%</td>
<td>2%</td>
<td>2%</td>
<td>3%</td>
</tr>
<tr>
<td>HS or Less</td>
<td>15%</td>
<td>9%</td>
<td>8%</td>
<td>45%</td>
</tr>
<tr>
<td>Some College</td>
<td>39%</td>
<td>37%</td>
<td>35%</td>
<td>18%</td>
</tr>
<tr>
<td>College Degree</td>
<td>36%</td>
<td>41%</td>
<td>42%</td>
<td>27%</td>
</tr>
<tr>
<td>Post-Graduate</td>
<td>9%</td>
<td>14%</td>
<td>14%</td>
<td>9%</td>
</tr>
</tbody>
</table>
Out-of-Sample Pretest

I conducted an out-of-sample pretest in May 2015. I recruited 1,680 mTurk workers to participate in the pretest, including 1,027 Democrats and 387 Republicans. Each respondent was presented a series of factual statements, and for each, was asked “How positively or negatively does this statement reflect on the [Democratic/Republican] Party?” Thus, each respondent provided a separate rating for each party. The response options were “Very Positively”, “Somewhat Positively”, “Neither”, “Somewhat Negatively”, and “Very Negatively”. Importantly, I implemented a between-subjects randomized design such that each respondent only rated one version of each fact. Table A8 displays the percentage of respondents rating each fact as somewhat or very positive for the Democratic Party.

Table A8. Percent Reporting that Fact Reflects Positively on Democratic Party in Pretest

<table>
<thead>
<tr>
<th>Unemployment Rate (Study 1)</th>
<th>All Respondents</th>
<th>Democrats</th>
<th>Republicans</th>
</tr>
</thead>
<tbody>
<tr>
<td>Neutral: unemployment rate is 5.5 percent</td>
<td>47% (n = 335)</td>
<td>55% (n = 217)</td>
<td>30% (n = 80)</td>
</tr>
<tr>
<td>Democratic Congenial: lowest since Obama</td>
<td>77% (n = 338)</td>
<td>88% (n = 201)</td>
<td>55% (n = 85)</td>
</tr>
<tr>
<td>Republican Congenial: higher than Bush</td>
<td>11% (n = 333)</td>
<td>13% (n = 194)</td>
<td>9% (n = 75)</td>
</tr>
<tr>
<td>Both: lowest since Obama and higher than Bush</td>
<td>39% (n = 333)</td>
<td>48% (n = 204)</td>
<td>27% (n = 75)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Uninsured Rate Decrease (Study 2)</th>
<th>All Respondents</th>
<th>Democrats</th>
<th>Republicans</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control/Pro-ACA: “about 5 percentage points”</td>
<td>69% (n = 335)</td>
<td>78% (n = 208)</td>
<td>57% (n = 74)</td>
</tr>
<tr>
<td>Anti-ACA: “only 5 percentage points”</td>
<td>42% (n = 336)</td>
<td>54% (n = 205)</td>
<td>22% (n = 79)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Family Premium Increase (Study 2)</th>
<th>All Respondents</th>
<th>Democrats</th>
<th>Republicans</th>
</tr>
</thead>
<tbody>
<tr>
<td>Control/Anti-ACA: “3 percent”</td>
<td>15% (n = 328)</td>
<td>17% (n = 205)</td>
<td>12% (n = 75)</td>
</tr>
<tr>
<td>Pro-ACA: “only 3 percent, which is a record low”</td>
<td>59% (n = 328)</td>
<td>68% (n = 207)</td>
<td>41% (n = 70)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Young Adult Coverage (Study 2, Pro-ACA)</th>
<th>All Respondents</th>
<th>Democrats</th>
<th>Republicans</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>87% (n = 339)</td>
<td>96% (n = 212)</td>
<td>72% (n = 79)</td>
</tr>
</tbody>
</table>

Additional Analyses

Robustness of Study 1 Results to Correct Answer Window

I first examine the robustness of my results in Study 1 to my coding of the dependent variable. In Table A9, I use three different correct answer windows to calculate the percentages of Democrats and Republicans reporting the correct unemployment rate, and treatment effects of the Democratic Congenial, Republican Congenial, and Both conditions (relative to the Neutral condition). In addition to the 5.8–6.0 percent window that I use in the main analysis, I examine a strict window of 5.9 percent only and a lenient window of 5.0–6.9 percent. Unsurprisingly, the percent correct in the non-informative condition varies dramatically by correct answer window. Almost nobody reports the exact answer of 5.9 percent, unless they receive this information.
However, the general pattern of null treatment effects occurs with each correct answer window. In particular, under each windows, percent correct is very similar in the Democratic Congenial and Republican Congenial conditions, which should yield the starkest difference if selective learning were at work. Therefore, my main finding of evenhanded learning of the unemployment rate does not depend on my coding of the dependent variable is not responsible.

Table A9. Percent Correct and Treatment Effects under Various Correct Answer Windows

<table>
<thead>
<tr>
<th>Partisanship</th>
<th>Window</th>
<th>No Info</th>
<th>Neutral</th>
<th>Dem Cong</th>
<th>Rep Cong</th>
<th>Both</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrats</td>
<td>5.9 only</td>
<td>3.0 (3.0)</td>
<td>66.7 (8.2)</td>
<td>63.2 (4.7)</td>
<td>65.4 (4.7)</td>
<td>46.8 (5.1)</td>
</tr>
<tr>
<td></td>
<td>5.8 – 6.0</td>
<td>33.3 (8.2)</td>
<td>69.7 (8.0)</td>
<td>75.5 (4.2)</td>
<td>74.0 (4.3)</td>
<td>68.1 (4.8)</td>
</tr>
<tr>
<td></td>
<td>5.0 – 6.9</td>
<td>45.5 (8.7)</td>
<td>84.8 (6.2)</td>
<td>93.4 (2.4)</td>
<td>88.5 (3.1)</td>
<td>85.1 (3.7)</td>
</tr>
<tr>
<td>Republicans</td>
<td>5.9 only</td>
<td>0.0 (0.0)</td>
<td>50.0 (13.4)</td>
<td>50.0 (8.6)</td>
<td>47.1 (8.6)</td>
<td>62.5 (7.7)</td>
</tr>
<tr>
<td></td>
<td>5.8 – 6.0</td>
<td>26.7 (11.4)</td>
<td>50.0 (13.4)</td>
<td>64.7 (8.2)</td>
<td>64.7 (8.2)</td>
<td>65.0 (7.5)</td>
</tr>
<tr>
<td></td>
<td>5 – 6.9</td>
<td>33.3 (12.2)</td>
<td>78.6 (11.0)</td>
<td>85.3 (6.1)</td>
<td>88.2 (5.5)</td>
<td>87.5 (5.2)</td>
</tr>
</tbody>
</table>

Note: For each correct answer window, first row displays percent of respondents reporting correct unemployment rate (and standard error), and second row displays change in percent correct relative to Neutral condition (i.e., treatment effect).

**Study 1 Results by General Political Knowledge**

I next examine whether selective learning occurs among partisans with greater general political knowledge. I used a four-item political knowledge scale that asked respondents to identify the U.S. Secretary of Defense, Chair of the Federal Reserve Board, President of Iran, and number of Republican Senate seats. In this analysis, I classify respondents answering two or more items correctly as high knowledge (49.5 percent) and the remainder as low knowledge (50.5 percent).

In Figure A6, I plot the difference in percentage correct in the Democratic Congenial condition minus the percentage correct in the Republican Congenial condition. Selective learning implies that this difference will be positive for Democrats and negative for Republicans. Among low-knowledge Republicans, the difference is 20 points (s.e. = 17 points), which is inconsistent with selective learning. Among low-knowledge Democrats, the difference is a mere -3 points (s.e. = 9) points, indicating no significant difference between conditions. Among high-knowledge partisans, the differences are in the expected direction but not statistically significant:
17 points (s.e. = 16 points) among Republicans and 7 points (s.e. = 8 points) among Democrats. These results suggest that selective learning may occur among the politically knowledgeable and may be worth replicating with a larger sample.

**Figure A6. Congeniality Effect on Proportion Correct by Partisanship and Political Knowledge (Study 1)**

Note: Figure plots differences in proportion correctly reporting unemployment rate between Democratic Congenial and Republican Congenial conditions with 95 percent confidence intervals.
Additional Factual Beliefs in Study 1

I next test for selective learning by using the other post-treatment factual items in Study 1. I consider the percentage of respondents who correctly report that the average unemployment rate was higher under Obama than under Bush. Only two conditions contained this information: Republican Congenial and Both. Selective learning implies that percent correct should be greater among Republicans than Democrats, because this information is congenial to Republicans. Therefore, I measure the partisan gap in factual beliefs, which is simply the difference in percent correct between Democrats and Republicans. The key question is whether the providing this fact – in the Republican Congenial and Both conditions – increases the size of the preexisting gap.

Figure A7 displays the partisan gaps for this fact by condition and general political knowledge. I pool the Neutral and Democratic Congenial conditions, because both are non-informative with respect to the Obama-Bush comparison. Among low-knowledge respondents, the partisan gap is 15 percentage points, indicating that Republicans are significantly more likely than Democrats to correctly make the Obama-Bush comparison. The gap barely changes in the informative conditions, suggesting that low-knowledge Republicans and Democrats learn this information at approximately equal rates. High-knowledge partisans tell a somewhat different story. Surprisingly, there is almost no partisan gap in the absence of information. A positive gap emerges in the Republican Congenial condition, but it is not statistically distinguishable from zero. Thus, contrary to expectation, it is less knowledgeable partisans who exhibit bias in their knowledge of this fact, and this bias does not increase due to selective learning.
Figure A7. Partisan Gaps in Proportion Correctly Making Obama-Bush Comparison

![Graph](image)

Note: Plots differences in proportion correct between Democrats and Republicans with 95 percent confidence intervals. No Info refers to Neutral and Democratic Congenial conditions, and Info refers to Republican Congenial and Both conditions.

Figure A8 displays the partisan gaps in the percent of respondents making the correct retrospective evaluation. I asked respondents whether unemployment had gotten better, stayed about the same, or gotten worse in the past year. The correct answer could be learned only in the Democratic Congenial and Both conditions, which I pool here. Among low-knowledge partisans, the results are similar to those for the Obama-Bush comparison. There is a partisan gap in the expected direction which does not change much (from 15 to 18 percentage points), suggesting evenhanded learning. Among high-knowledge partisans, the partisan gap is 44 percentage points without information and decreases to 25 percentage points with information, indicating greater learning among Republicans. This result is again inconsistent with selective learning, which would imply that Democrats learn this fact more readily than Republicans.
Figure A8. Partisan Gaps in Proportion Correctly Making Retrospective Evaluation (Study 1)

Note: Plots differences in proportion correct between Democrats and Republicans with 95 percent confidence intervals. No Info refers to Neutral and Republican Congenial conditions, and Info refers to Democratic Congenial and Both conditions.
Additional Factual Belief in Study 2

In Figure A9, I plot the treatment effects on the percentage of respondents reporting the correct current uninsured rate in Study 2. Among Democrats, information congeniality has no effect on the probability of learning this fact correctly in Wave 1. In Wave 2, both treatment conditions decrease percent correct among Democrats, indicating that Democrats tend to forget this fact after a few days. However, this finding is inconsistent with selective learning in the long term, because Democrats are equally likely to forget the congenial fact and uncongenial fact. Among Republicans, both treatment effects are null in both waves.

Figure A9. Treatment Effects on Learning Current Uninsured Rate (Study 2)

Note: Dashed horizontal lines indicate percentage correctly reporting current uninsured rate in control condition. Points indicate effects of Democratic Congenial and Republican Congenial treatments on percentage correct, relative to control condition. Vertical lines indicate 95 percent confidence intervals of treatment effects.
Chapter 4 Supplementary Information

Numeracy Screener

The numeracy scale employed in Study 3 is below. It is a shortened version of the scale used by Kahan et al. (2017) and developed by Weller et al. (2012). It includes the five easiest items that Weller et al. find, in order to identify low-numeracy respondents. Two concerns vitiate commensurability of our scores and those of Kahan et al.: guessing and item sampling. Research suggests that lucky guessing is a minor concern on open-ended items (Luskin and Bullock 2011). And if we assume that the items are Guttman scaled, i.e., a person who answers a more difficult item correctly will also answer easier items correctly, our threshold will be exactly the same as the median score in Kahan et al. We take Guttman scaling on numeracy items to not only be plausible but likely.

1. If we roll a fair, six-sided die 1,000 times, on average, how many times would the die come up as an even number? (Answer: 500)

2. There is a 1% chance of winning a $10 prize in the Megabucks Lottery. On average, how many people would win the $10 prize if 1,000 people each bought a single ticket? (Answer: 10)

3. If the chance of getting a disease is 20 out of 100, this would be the same as having a ____% chance of getting the disease. (Answer: 20)

4. If there is a 10% chance of winning a concert ticket, how many people out of 1,000 would be expected to win the ticket? (Answer: 100)

5. In the PCH Sweepstakes, the chances of winning a car are 1 in a 1,000. What percent of PCH Sweepstakes tickets win a car? (Answer: .1)
Minimum Wage Task (Studies 3b and 3c Only)

A city government is trying to decide whether to pass a law increasing the minimum wage. Government officials were unsure whether the law would be more likely to decrease jobs or increase jobs.

Researchers completed a study of two groups of states to answer that question. The study involved comparing changes in jobs for one group of states that had increased the minimum wage with changes in jobs for a second group of states that had not increased the minimum wage.

In each group, the percentage of states in which jobs decreased and the percentage of states in which jobs increased are recorded in the table below. The exact number of states in each group is not the same, but this does not prevent assessment of the results.

We would like to know whether states that increased the minimum wage were more likely to have a decrease or increase in jobs than states without minimum wage increases.


<table>
<thead>
<tr>
<th>States that did not increase minimum wage</th>
<th>Increase in jobs</th>
<th>Decrease in jobs</th>
</tr>
</thead>
<tbody>
<tr>
<td>States that did increase minimum wage</td>
<td>37%</td>
<td>63%</td>
</tr>
</tbody>
</table>

What result does the study support? [Incentive Treatment:] We will give you a bonus of $0.10 for the correct answer.

- States that increased the minimum wage were more likely to have a decrease in jobs than states without minimum wage increases.

- States that increased the minimum wage were more likely to have an increase in jobs than states without minimum wage increases.
Sample Characteristics

To shed light on the quality of our respondent samples, Table A10 compares marginals on key variables. The 2012 American National Election Study is used for benchmarks data on political variables, and the U.S. Census 2015 American Community Survey for demographic benchmarks. Like other mTurk samples, the Study 3a and 3b samples are more young, white, male, and liberal than the general population. Study 3c is more representative in terms of party and ideology, but more educated, female, and white than the general population. In Studies 3b and 3c, we compare high-numeracy respondents with the entire sample. In Study 3b, there are no appreciable differences by numeracy, but in Study 3c, high-numeracy respondents are more educated, white, and male than low-numeracy respondents.

Table A10. Sample Characteristics in Study 3

<table>
<thead>
<tr>
<th></th>
<th>Study 1</th>
<th>Study 2</th>
<th>Study 2 High-Num.</th>
<th>Study 3 High-Num.</th>
<th>Study 3</th>
<th>Study 3</th>
<th>National Sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrat</td>
<td>65%</td>
<td>60%</td>
<td>62%</td>
<td>51%</td>
<td>50%</td>
<td>46%</td>
<td></td>
</tr>
<tr>
<td>Republican</td>
<td>21%</td>
<td>24%</td>
<td>22%</td>
<td>35%</td>
<td>39%</td>
<td>39%</td>
<td></td>
</tr>
<tr>
<td>Liberal</td>
<td>51%</td>
<td>50%</td>
<td>52%</td>
<td>23%</td>
<td>22%</td>
<td>27%</td>
<td></td>
</tr>
<tr>
<td>Conservative</td>
<td>16%</td>
<td>17%</td>
<td>16%</td>
<td>29%</td>
<td>28%</td>
<td>39%</td>
<td></td>
</tr>
<tr>
<td>HS or Less</td>
<td>9%</td>
<td>12%</td>
<td>10%</td>
<td>25%</td>
<td>17%</td>
<td>41%</td>
<td></td>
</tr>
<tr>
<td>Some College</td>
<td>40%</td>
<td>41%</td>
<td>41%</td>
<td>39%</td>
<td>37%</td>
<td>31%</td>
<td></td>
</tr>
<tr>
<td>College Degree</td>
<td>40%</td>
<td>39%</td>
<td>39%</td>
<td>26%</td>
<td>33%</td>
<td>18%</td>
<td></td>
</tr>
<tr>
<td>Post-Graduate</td>
<td>11%</td>
<td>9%</td>
<td>10%</td>
<td>10%</td>
<td>13%</td>
<td>10%</td>
<td></td>
</tr>
<tr>
<td>18-29 years old</td>
<td>57%</td>
<td>55%</td>
<td>55%</td>
<td>10%</td>
<td>11%</td>
<td>22%</td>
<td></td>
</tr>
<tr>
<td>30-44 years old</td>
<td>33%</td>
<td>33%</td>
<td>34%</td>
<td>34%</td>
<td>33%</td>
<td>25%</td>
<td></td>
</tr>
<tr>
<td>45-64 years old</td>
<td>9%</td>
<td>11%</td>
<td>10%</td>
<td>44%</td>
<td>47%</td>
<td>34%</td>
<td></td>
</tr>
<tr>
<td>65+ years old</td>
<td>1%</td>
<td>1%</td>
<td>1%</td>
<td>13%</td>
<td>10%</td>
<td>19%</td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>32%</td>
<td>38%</td>
<td>34%</td>
<td>66%</td>
<td>57%</td>
<td>52%</td>
<td></td>
</tr>
<tr>
<td>White</td>
<td>75%</td>
<td>73%</td>
<td>73%</td>
<td>76%</td>
<td>84%</td>
<td>62%</td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>5%</td>
<td>6%</td>
<td>5%</td>
<td>10%</td>
<td>5%</td>
<td>12%</td>
<td></td>
</tr>
<tr>
<td>Hispanic/Latino</td>
<td>5%</td>
<td>7%</td>
<td>7%</td>
<td>7%</td>
<td>4%</td>
<td>18%</td>
<td></td>
</tr>
<tr>
<td>Asian</td>
<td>11%</td>
<td>10%</td>
<td>11%</td>
<td>3%</td>
<td>4%</td>
<td>5%</td>
<td></td>
</tr>
<tr>
<td>Other/Mixed</td>
<td>4%</td>
<td>5%</td>
<td>5%</td>
<td>3%</td>
<td>3%</td>
<td>3%</td>
<td></td>
</tr>
</tbody>
</table>
Additional Analyses

**Covariate Balance**

Table A11 presents estimates of the extent to which covariates were balanced across conditions in the concealed carry task. Each row reports the $F$-statistic and associated $p$-value from a linear regression of the indicated covariate on the congeniality treatment, incentive treatment, and interaction between the two treatments. In Study 3c, which only contained the uncongenial version of the concealed carry task, the only regressor is the accuracy treatment.

**Table A11. Covariate Balance across Experimental Conditions in Concealed Carry Task**

<table>
<thead>
<tr>
<th>Covariate</th>
<th>Study 1 (N=785)</th>
<th>Study 2 (N=947)</th>
<th>Study 3 (N=1,062)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Partisanship (7-point)</td>
<td>$F = .45, p = .72$</td>
<td>$F = .88, p = .45$</td>
<td>$F = .04, p = .85$</td>
</tr>
<tr>
<td>Ideology (5-point)</td>
<td>$F = .30, p = .83$</td>
<td>$F = 1.29, p = .28$</td>
<td>$F = .002, p = .96$</td>
</tr>
<tr>
<td>Education (4-point)</td>
<td>$F = 2.42, p = .07$</td>
<td>$F = .78, p = .50$</td>
<td>$F = 2.51, p = .11$</td>
</tr>
<tr>
<td>Age (continuous)</td>
<td>$F = 1.34, p = .26$</td>
<td>$F = .56, p = .64$</td>
<td>$F = 1.00, p = .32$</td>
</tr>
<tr>
<td>Gender (binary)</td>
<td>$F = 1.53, p = .20$</td>
<td>$F = .47, p = .71$</td>
<td>$F = 1.04, p = .31$</td>
</tr>
<tr>
<td>Non-White (binary)</td>
<td>$F = .64, p = .42$</td>
<td>$F = 1.40, p = .24$</td>
<td>$F = .06, p = .81$</td>
</tr>
</tbody>
</table>

**Assessing Spillover**

Because the concealed carry task always preceded the minimum wage task in Studies 2 and 3, we assess the extent to which randomization in the former task affected behavior in the latter task. We estimate treatment effects in both tasks simultaneously via logistic regression. The outcome variable indicates correctness in the minimum wage task ($1$=correct). Regressors are congeniality and incentive treatments, and their interaction, in both tasks. We allow the minimum wage treatments to vary by concealed carry condition.

Table A12 displays the results for supporters of raising the minimum wage in Studies 2 and 3, and opponents in Study 3. In each regression, none of the coefficients reach significance, except for the congeniality treatment in the minimum wage task. Importantly, the concealed carry task treatments all have null effects, confirming that the first task did not spill over into the second task. Moreover, the null interactive effects indicate that the concealed carry treatments did not alter the effects of the minimum wage treatments in any of the samples.
Table A12. Logistic Regression of Minimum Wage Correctness on Experimental Treatments in Both Tasks

| Study 2: Supporters of Raising Minimum Wage (n=818) | Coefficient (SE) | z value | Pr(>|z|) |
|---------------------------------------------------|------------------|---------|---------|
| Intercept                                         | 1.556 (.389)     | 4.006   | .000    |
| Concealed Carry Congeniality                     | .611 (.656)      | .932    | .352    |
| Concealed Carry Incentives                        | .777 (.719)      | 1.081   | .280    |
| CC Congeniality × CC Incentives                   | .717 (1.350)     | .532    | .595    |
| Minimum Wage Congeniality                         | .927 (.716)      | 1.295   | .195    |
| Minimum Wage Incentives                           | 1.045 (.830)     | 1.259   | .208    |
| MW Congeniality × MW Incentives                   | -.667 (1.256)    | -.531   | .595    |
| CC Congeniality × MW Congeniality                 | -.1767 (.965)    | -1.830  | .067    |
| CC Incentives × MW Congeniality                   | -.1443 (1.035)   | -1.394  | .163    |
| CC Congeniality × CC Incentives × MW Congeniality | -.157 (1.627)    | -.096   | .923    |
| CC Congeniality × MW Incentives                   | .450 (.412)      | .319    | .750    |
| CC Incentives × MW Incentives                     | -.1534 (1.117)   | -1.374  | .170    |
| CC Congeniality × CC Incentives × MW Incentives   | -.1545 (1.964)   | -1.786  | .432    |
| CC Congeniality × MW Congeniality × MW Incentives | -.735 (1.789)    | -.411   | .681    |
| CC Incentives × MW Congeniality × MW Incentives   | 1.209 (1.618)    | .747    | .455    |
| CC Congeniality × CC Incent. × MW Congen. × MW Incent. | 2.856 (2.458) | 1.162   | .245    |

| Study 3: Supporters of Raising Minimum Wage (n=918) | Coefficient (SE) | z value | Pr(>|z|) |
|---------------------------------------------------|------------------|---------|---------|
| Intercept                                         | 1.655 (.303)     | 5.466   | .000    |
| Concealed Carry Incentives                        | -.060 (.421)     | -.141   | .888    |
| Minimum Wage Congeniality                         | -.961 (.376)     | -2.56   | .011    |
| Minimum Wage Incentives                           | -.032 (.414)     | -.077   | .939    |
| MW Congeniality × MW Incentives                   | .452 (.531)      | .852    | .394    |
| CC Incentives × MW Congeniality                   | .155 (.535)      | .290    | .772    |
| CC Incentives × MW Incentives                     | -.032 (.577)     | -.055   | .956    |
| CC Incentives × MW Congeniality × MW Incentives   | -.348 (.750)     | -.464   | .643    |

| Study 3: Opponents of Raising Minimum Wage (n=262) | Coefficient (SE) | z value | Pr(>|z|) |
|---------------------------------------------------|------------------|---------|---------|
| Intercept                                         | .211 (.326)      | .648    | .517    |
| Concealed Carry Incentives                        | .482 (.462)      | 1.043   | .297    |
| Minimum Wage Congeniality                         | 2.519 (.679)     | .679    | .0002   |
| Minimum Wage Incentives                           | .792 (.480)      | 1.649   | .099    |
| MW Congeniality × MW Incentives                   | -1.443 (9.090)   | -1.602  | .109    |
| CC Incentives × MW Congeniality                   | -1.015 (.920)    | -1.103  | .270    |
| CC Incentives × MW Incentives                     | -.846 (.647)     | -.1308  | .191    |
| CC Incentives × MW Congeniality × MW Incentives   | .889 (1.182)     | .752    | .452    |

Note: CC and MW indicate concealed carry and minimum wage treatments, respectively.
Does Numeracy Condition Treatment Effects?

We confined Study 3a to high-numeracy respondents based on the theoretical argument and main result of Kahan et al. (2017) that polarization only occurs among this group. However, in Studies 3b and 3c, we invited all respondents to participate fully, regardless of numeracy, to compare the behavior and background characteristics of high- and low-numeracy respondents. In theory, respondents may differ by numeracy in multiple ways. First, high-numeracy respondents are probably more educated, which we see in Study 3c (see Table A10). Second, they may hold different attitudes. For example, high-numeracy respondents may exhibit greater political sophistication, ideological constraint, party identification, or other traits that predispose them to selective learning, selective responding, or both.

Therefore, in Table A13, we compare behavior of low- and high-numeracy respondents in the concealed carry experiment. Note that our numeracy scale is truncated, so our ability to fully answer this question is limited. We find that the substantive pattern of results is similar among both groups, though the magnitudes of treatment effects change. Each cell in Table A13 displays the incentive treatment effect in percentage points, which is percent correct in the Incentive condition minus the percent correct in the No Incentive condition (standard errors in parentheses). Each row contains a type of respondent, and each column contains a different study and congeniality condition. Among concealed carry supporters, neither high- nor low-numeracy respondents exhibit significant treatment effects, in either the uncongenial or congenial condition. (The effects are positive among low-numeracy respondents in Study 3b, but have large standard errors.) Among concealed carry opponents, we see the familiar pattern of positive treatment effects in the uncongenial, but not the congenial, condition. These effects occur among high-numeracy respondents in Study 3b, and both high- and low-numeracy respondents in Study 3 (though they are only statistically significant among the latter group).
Table A13. Incentive Effects in Concealed Carry Task by Study, Condition, and Numeracy

<table>
<thead>
<tr>
<th>Respondents</th>
<th>Study 1 Uncongenial</th>
<th>Congenial</th>
<th>Study 2 Uncongenial</th>
<th>Congenial</th>
<th>Study 3 Uncongenial</th>
</tr>
</thead>
<tbody>
<tr>
<td>CC Supporters</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High-Numeracy</td>
<td>-1.5 (6.3)</td>
<td>-3.3 (6.9)</td>
<td>-4.2 (7.3)</td>
<td>2.8 (7.1)</td>
<td>2.9 (5.8)</td>
</tr>
<tr>
<td></td>
<td>n=248</td>
<td>n=207</td>
<td>n=180</td>
<td>n=200</td>
<td>n=261</td>
</tr>
<tr>
<td>Low-Numeracy</td>
<td>-</td>
<td>-</td>
<td>13.3 (11.3)</td>
<td>13.3 (11.0)</td>
<td>-2.4 (4.9)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>n=72</td>
<td>n=78</td>
<td>n=378</td>
</tr>
<tr>
<td>Combined</td>
<td>-</td>
<td>-</td>
<td>.6 (6.2)</td>
<td>5.9 (6.0)</td>
<td>-0.3 (3.7)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>n=252</td>
<td>n=278</td>
<td>n=639</td>
</tr>
<tr>
<td>CC Opponents</td>
<td>9.1 (9.1)</td>
<td>-8.0 (9.5)</td>
<td>25.4 (9.4)</td>
<td>3.0 (9.4)</td>
<td>4.5 (7.6)</td>
</tr>
<tr>
<td>High-Numeracy</td>
<td></td>
<td></td>
<td>n=108</td>
<td>n=116</td>
<td>n=159</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>n=123</td>
<td>n=108</td>
<td></td>
</tr>
<tr>
<td>Low-Numeracy</td>
<td></td>
<td></td>
<td>-10.1 (16.0)</td>
<td>-11.7 (17.9)</td>
<td>8.0 (5.4)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>n=38</td>
<td>n=32</td>
<td>n=257</td>
</tr>
<tr>
<td>Combined</td>
<td></td>
<td></td>
<td>17.1 (8.1)</td>
<td>-0.3 (8.2)</td>
<td>6.9 (4.4)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>n=146</td>
<td>n=148</td>
<td>n=416</td>
</tr>
</tbody>
</table>

Note: ‘CC Supporters’ and ‘CC Opponents’ refer to concealed carry supporters and opponents, respectively.

Since numeracy is not a requisite to understand which result is more congenial, we also checked whether low-numeracy respondents tended to pick the congenial answer more often than the uncongenial one. We term this behavior “response bias” and operationalize it as the odds of choosing the congenial answer. Low-numeracy respondents indeed exhibit such bias. In Study 2, response bias is 1.25 without incentives, meaning respondents are 25% more likely to pick the congenial answer. High-numeracy response bias is a very similar 1.22. Where the two groups differ is in their responsiveness to incentives. Low-numeracy respondents continue to exhibit a response bias of 1.13 with incentives, while bias among high-numeracy respondents reverses itself to .82, indicating that as expected, they become more likely to pick the uncongenial answer.

In Study 3, incentives reduce response bias among both low- and high-numeracy respondents alike, paralleling the results in the last column of Table A13. Incentives reduce low-numeracy response bias from 3.68 to 2.41 and high-numeracy response bias from 2.04 to 1.68. Interestingly, it is low-numeracy respondents who exhibit more response bias overall in Study 3.
**Subsetting Respondents by “Ideological Worldview”**

Kahan et al. (2017) subset respondents by “ideological worldview” instead of their view of concealed carry. They conceive of ideological worldview as a combination of partisanship and ideology, and measure it by multiplying the two. Conservative Republicans are at one end of the scale, and liberal Democrats are on the other. We build a similar subsetting variable, categorizing respondents as either conservative Republicans or liberal Democrats, based on their self-reported partisanship and ideology. Using this variable, we re-analyze the concealed carry data in Studies 3a and 3b. Views on concealed carry are weakly related to this new variable, especially among low-numeracy respondents. In Study 3b, the correlation is .41 among high-numeracy respondents and .16 among low-numeracy respondents. This weak connection may explain why Kahan et al. do not observe bias among low-numeracy respondents. We prefer to subset respondents by issue position, precisely because overlap between attitudes and ideological worldview is not 100%.

With the caveats above, we rerun our main analyses using this new variable to subset respondents, presenting the results in Figure A10. The substantive pattern is very similar to our main results. Conservative Republicans exhibit congeniality effects with and without incentives. The magnitudes are substantial, but the effects are marginally significant due to the small size of this group. Liberal Democrats, on the other hand, exhibit a significant congeniality effect of 10.9 points without incentives, which is reduced to an insignificant 3.8 points with incentives. While we view this conditioning variable as indirectly related to our outcome of interest, it is reassuring that we are able to replicate the main result of Kahan et al. Again, incentives only work on respondents on the political left.
Figure A10. Concealed Carry Task Results by Ideological Worldview (Studies 3a and 3b)

A) Percent Answering Correctly: Conservative Republicans (N=181)

B) Congeniality Effects and DiD: Conservative Republicans (N=181)

C) Percent Answering Correctly: Liberal Democrats (N=598)

D) Congeniality Effects and DiD: Liberal Democrats (N=598)

Note: Panels on the left display percentage of conservative Republicans (Panel A) and liberal Democrats (Panel C) of raising the minimum wage correctly indicating study result by experimental condition. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among conservative Republicans (Panel B) and liberal Democrats (Panel D). Vertical lines indicate 95 percent confidence intervals.
No Evidence of Biased Recall

We next examine whether respondents exhibited congeniality bias in recalling specific pieces of information. To test this hypothesis, we examine how well respondents did across various conditions in recalling the numbers in the contingency table (in Study 3a only). Past studies using similar tasks suggest that errors are most common in the upper-left cell of the contingency table (Gilovich, 1991; Dawson et al., 2002b). We therefore first examine whether the congeniality treatment changed recall of the number of cities in this cell. Since the incentive treatment occurred only after respondents saw the contingency table, we focus on the effect of congeniality in the absence of incentives.

There were 223 cities in the upper-left cell in both conditions (see Tables 4.1 and 4.2). If respondents misremember this value such that the study result is more congenial, they would overshoot it more in the uncongenial condition than the congenial condition. However, there is no difference: the mean number of cities recalled was 213.9 (s.e. = 5.5) in the uncongenial condition and 212.5 (s.e. = 4.9) in the congenial condition.

We next check whether recalling the 107 cities in the lower-left cell varied by condition. Here we would expect responses to be lower in the uncongenial condition than in the congenial condition. However, recall was again fairly consistent across conditions: 103.1 (s.e. = 2.7) and 105.2 (s.e. = 4.7) in the uncongenial and congenial conditions, respectively. Recalling the other two cells in the table were also unaffected by the congeniality manipulation. In summary, we do not find a congeniality effect on the recall task in the absence of incentives.

Finally, we simultaneously test the effect of congeniality, incentives, and their interaction on recall. We look at recall estimates of the number of cities in the upper-left and lower-left cells of the contingency table. As Tables A14 and A15 below show, there are no significant effects for either cell. We conclude that respondents do not perceive the data selectively. This analysis also serves as a placebo test, indicating that incentives affect responses without affecting recall.

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56 Overestimating the upper-left cell inflates the crime increase-to-decrease ratio for cities that banned concealed handguns in the anti-gun version of the table. Overestimating this cell inflates the decrease-to-increase ratio in the pro-gun version.
Table A14. Linear Regression: Upper-Left Cell Recall Estimates  

| Coefficient (SE) | t value | Pr(>|t|) |
|------------------|---------|---------|
| Intercept        | 213.92 (5.58) | 38.31   | .00 |
| Congeniality     | -1.39 (8.08)  | -.17    | .86 |
| Incentive        | -1.92 (7.97)  | -.24    | .81 |
| Congeniality X Incentive | 2.66 (11.79) | .23    | .82 |

Table A15. Linear Regression: Lower-Left Cell Recall Estimates  

| Coefficient (SE) | t value | Pr(>|t|) |
|------------------|---------|---------|
| Intercept        | 103.09 (3.40) | 30.34   | .00 |
| Congeniality     | 2.10 (4.91)  | .43     | .67 |
| Incentive        | 1.08 (4.85)  | .22     | .82 |
| Congeniality X Incentive | 1.92 (7.17) | .27    | .79 |

Assessing Experimenter Demand Effects

Do respondents report what they think the experimenter believes is true instead of what they believe to be true? To answer that, we need to conjecture about respondents’ beliefs about the experimenter conducting the study. We consider two hypotheses about what respondents may think in this regard. The first is that respondents believe that most social scientists are liberal, and therefore infer that the result favoring gun control will always be treated as correct. Another is that respondents interpret financial incentives as a cue that the uncongenial option is the right one. We investigate both possibilities.

If respondents believe the experimenter is liberal, incentives should encourage them to select the anti-gun (i.e., pro-ban) answer, irrespective of the study’s congeniality. To investigate this, we pooled opponents of concealed carry from Studies 3a and 3b as they respond appreciably to the incentive treatment. In Table A16 below, we display the percentage of these respondents who give the congenial (i.e., anti-gun/pro-ban) and uncongenial response (i.e., pro-gun/anti-ban) by condition. Without incentives, they are more likely to give the congenial response than the uncongenial response (59% vs. 41%). Here we quantify “response bias” as the odds of selecting the anti-gun response option. Response bias is 1.44 in the uncongenial condition and 1.25 in the congenial condition. When respondents are offered incentives, response bias reverses itself in the uncongenial condition, indicating that respondents are now more likely to select the pro-gun answer. Thus, incentives do not cause respondents to blindly choose either answer. Instead, they become less likely to pick the congenial answer and more likely to answer correctly.
Among concealed carry supporters, incentives have no discernible effect on response bias. These respondents are always more likely to pick the congenial answer than the uncongenial one. Bias is a bit greater in the uncongenial condition than in the congenial condition, resulting in poorer performance in the uncongenial condition. Without incentives, response bias is 1.35 in the uncongenial condition and 1.20 in the congenial condition. More importantly, incentives hardly change response bias in either condition. Thus, contrary to the scenario outlined above, incentives do not encourage supporters of concealed carry to select any particular response.

Do respondents interpret the incentive as a cue that the uncongenial answer is correct, regardless of which table they see? It doesn’t appear so. Table A16 shows that incentives only change behavior in the uncongenial condition. In the congenial condition, they are no more likely to pick the uncongenial (incorrect) answer with incentives. And Table A17 shows that concealed carry supporters always exhibit a response bias, which incentives do not significantly reduce. These findings contradict the idea that incentives introduce demand effects whereby respondents falsify their factual beliefs to try to get extra money. A more plausible interpretation of our results is that incentives reduce the prevalence of motivated responding, increasing correctness in the uncongenial condition.

Table A16. Response Bias among Concealed Carry Opponents (Studies 3a and 3b)

<table>
<thead>
<tr>
<th>Experimental Condition</th>
<th>N</th>
<th>% Selecting Anti-Gun Answer</th>
<th>% Selecting Pro-Gun Answer</th>
<th>Odds of Anti-Gun Answer</th>
</tr>
</thead>
<tbody>
<tr>
<td>No Incentives, Uncongenial</td>
<td>117</td>
<td>59.0%</td>
<td>41.0%</td>
<td>1.44</td>
</tr>
<tr>
<td>No Incentives, Congenial</td>
<td>128</td>
<td>55.5%</td>
<td>44.5%</td>
<td>1.25</td>
</tr>
<tr>
<td>Incentives, Uncongenial</td>
<td>114</td>
<td>43.0%</td>
<td>57.0%</td>
<td>.75</td>
</tr>
<tr>
<td>Incentives, Congenial</td>
<td>96</td>
<td>53.1%</td>
<td>46.9%</td>
<td>1.13</td>
</tr>
</tbody>
</table>

Table A17. Response Bias among Concealed Carry Supporters (Studies 3a and 3b)

<table>
<thead>
<tr>
<th>Experimental Condition</th>
<th>N</th>
<th>% Selecting Anti-Gun Answer</th>
<th>% Selecting Pro-Gun Answer</th>
<th>Odds of Pro-Gun Answer</th>
</tr>
</thead>
<tbody>
<tr>
<td>No Incentives, Uncongenial</td>
<td>216</td>
<td>42.6%</td>
<td>57.4%</td>
<td>1.35</td>
</tr>
<tr>
<td>No Incentives, Congenial</td>
<td>207</td>
<td>45.4%</td>
<td>54.6%</td>
<td>1.20</td>
</tr>
<tr>
<td>Incentives, Uncongenial</td>
<td>212</td>
<td>40.1%</td>
<td>59.9%</td>
<td>1.49</td>
</tr>
<tr>
<td>Incentives, Congenial</td>
<td>200</td>
<td>46.0%</td>
<td>54.0%</td>
<td>1.17</td>
</tr>
</tbody>
</table>
**How Confident are Respondents in Their Answers? (Study 3c)**

In Study 3c, after the respondents had marked their answer about the conclusion supported by the study, we asked the respondents “how confident are you of your answer to this question.” The respondents could pick between the following options: not confident at all, not very confident, moderately confident, very confident, and certain. Table A18 below displays the percentage of incorrect and correct answers by respondents’ subjective confidence in their answer. Few people are certain of their answers, particularly in the concealed carry task. Only 13% of respondents report that they are certain of their answer, and the vast majority of these certain answers are incorrect. Still, only 10% of respondents overall report being certain of an incorrect answer, mitigating the concern that motivated learning leads to being misinformed. Probably because the minimum wage task was relatively much easier, average certainty about the answer is substantially higher in the minimum wage task. One in five answers are certain and correct, and only 4% are certain and incorrect.

**Table A18. Percentage of Incorrect and Correct Answers by Confidence (Study 3c)**

<table>
<thead>
<tr>
<th>Confidence Rating</th>
<th>Concealed Carry Task</th>
<th>Minimum Wage Task</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Incorrect</td>
<td>Correct</td>
</tr>
<tr>
<td>Not at all confident</td>
<td>2%</td>
<td>1%</td>
</tr>
<tr>
<td>Not very confident</td>
<td>8%</td>
<td>5%</td>
</tr>
<tr>
<td>Moderately confident</td>
<td>27%</td>
<td>3%</td>
</tr>
<tr>
<td>Very confident</td>
<td>22%</td>
<td>9%</td>
</tr>
<tr>
<td>Certain</td>
<td>10%</td>
<td>3%</td>
</tr>
</tbody>
</table>

*Note: Cells display overall percentages, so cells within a task add up to 100%.*
Figure A11. Concealed Carry Task Results from Study 3a Only

Note: Panels on the left display percentage of concealed carry supporters (Panel A) and opponents (Panel C) correctly indicating study result by experimental condition in Study 3a only. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among concealed carry supporters (Panel B) and opponents (Panel D). Vertical lines indicate 95 percent confidence intervals.
Figure A12. Concealed Carry Task Results from Study 3b Only

Note: Panels on the left display percentage of concealed carry supporters (Panel A) and opponents (Panel C) correctly indicating study result by experimental condition in Study 3b only. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among concealed carry supporters (Panel B) and opponents (Panel D). Vertical lines indicate 95 percent confidence intervals.
Figure A13. Minimum Wage Task Results from Study 3b Only

Note: Panel A displays percentage of minimum wage raise supporters in Study 3b only correctly indicating study result by experimental condition. Panel B displays congeniality effect by incentive condition, as well as difference-in-differences (DiD), among minimum wage raise supporters. Vertical lines indicate 95 percent confidence intervals.
Figure A14. Minimum Wage Task Results from Study 3c Only

Note: Panels on the left display percentage of minimum wage raise supporters (Panel A) and opponents (Panel C) correctly indicating study result by experimental condition in Study 3c only. Panels on the right display congeniality effect by incentive condition, as well as difference-in-differences (DiD), among supporters (Panel B) and opponents (Panel D). Vertical lines indicate 95 percent confidence intervals.