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Political Perception in the Polarized Era

by

Douglas James Ahler

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Political Science

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Associate Professor Gabriel Lenz, Chair
Professor Leif Nelson
Associate Professor Laura Stoker
Associate Professor Rob Van Houweling

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Douglas James Ahler

Abstract

Political Polarization in the Polarized Era

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Doctor of Philosophy in Political Science

University of California, Berkeley

Associate Professor Gabriel Lenz, Chair

"Self-Fulfilling Misperceptions of Public Polarization"

Mass media convey deep divisions among citizens despite scant evidence for such ideological polarization. Do ordinary citizens perceive themselves to be more extreme and divided than they actually are? If so, what are the ramifications of such misperception? A representative sample from California provides evidence that voters from both sides of the state's political divide perceive both their liberal and conservative peers' positions as more extreme than they actually are, implying inaccurate beliefs about polarization. A second study again demonstrates this finding with an online sample and presents evidence that misperception of mass-level extremity can affect individuals' own policy opinions. Experimental participants randomly assigned to learn the actual average policy-related predispositions of liberal and conservative Americans later report opinions that are 8-13% more moderate, on average. Thus, citizens appear to consider peers' positions within public debate when forming their own opinions and adopt slightly more extreme positions as a consequence.

"The Parties in Our Heads: Misperceptions About Party Composition and Their Consequences" (co-authored with Gaurav Sood)

We document a consequential and heretofore unnoted perceptual phenomenon in American politics and public opinion: people considerably overestimate the share of party-stereotypical groups in the mass-level parties. For instance, people think that 32% of Democratic supporters are LGBT (6% in reality) and 38% of Republican supporters earn over $250,000 per year (2%). We demonstrate that these perceptions are genuine and party-specific, not artifacts of expressive responding, innumeracy, or ignorance of base rates. These misperceptions are relatively universal across partisanship and positively associated with political interest. With experimental and observational evidence, we document consequences of this perceptual bias: misperceptions are associated with partisan affect and attitudinal polarization, and when provided information about the actual share of various party-stereotypical

groups in the out-party, partisans see supporters of the out-party as less extreme and feel less socially distant from them. Thus, people's skewed mental images of the parties appear to fuel intense partisanship.

"Irresponsible Partisanship and Democratic Accountability: How Citizens Understand Party Conflict"

American citizens resent contemporary party conflict largely for its "process consequences." These include incivility, gridlock, and government dysfunction. This is puzzling because political science generally concludes that such "irresponsible partisanship" is strategic. That is, Democratic and Republican politicians manipulate and intensify conflict as an electoral and messaging strategy. I evaluate potential resolutions for this puzzle, namely that citizens perceive party conflict as affectively-driven rather than strategic—and, importantly, that their tendency to see their own party as motivated by in-group love and the out-party by out-group hate impedes their ability to hold elites accountable for its process consequences. With data from the 2015 IGS-California Poll, I find citizens see both parties as significantly more motivated by strategy than emotion, especially when conflict is presented in less abstract, more policy-related terms. However, I also show that citizens generally oppose or lack strong attitudes toward reforms that could potentially curb process consequences. This suggests that blindness to institutional externalities, rather than to elite strategy, sustains irresponsible partisanship.

After 21 years as a student in California public schools, I submit my final assignment with my deepest gratitude to all the wonderful teachers I've known along the way.

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Chapter 1

Introduction: Political Perception

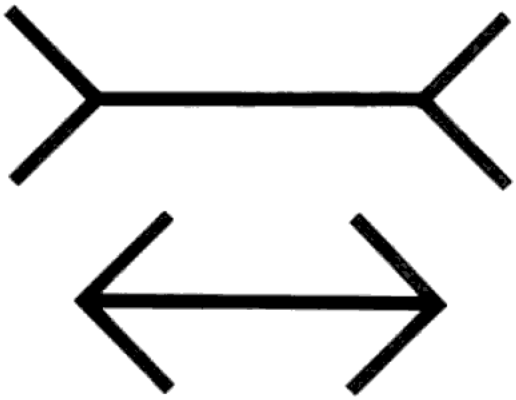
Perception, quite simply, involves making sense of sensory input. We are constantly bombarded with vast amounts of information—visual, auditory, tactile, olfactory, and gustatory—and we perceive something when we separate it "from the background" (Hinton, 2016, p. 7).

To identify something from the informational morass, we obviously must sense it. But perception is about more than the information available in the moment; it also relies on information stored in memory. Gregory (1970) thus describes perception as a "top-down" process by which people make implicit assumptions about the stimuli they receive. These assumptions stem from past experiences with related stimuli and generally help people to perceive things more accurately and quickly (Hinton, 2016). From time to time, however, memory can lead perception astray.

The Müeller-Lyer illusion, shown in Figure 1.1 is a famous example. Although the two parallel lines are of identical length, the line with the arrow tails appears longer than the line with the arrowheads, a perceptual tendency confirmed in numerous laboratory studies (e.g., Dewar, 1967). Recent scholarship suggests that top-down processing drives the Müeller-Lyer effect. Howe and Purves (2004) examine a large set of three-dimensional range images and observe: "The identical shafts or intervals in Müller-Lyer stimuli appear different in length because the probability distributions of the real-world sources of the lines or intervals, given the contexts provided by the arrowheads or arrow tails, are in fact different." That is, if the equal-length, two-dimensional images in Figure 1.1 were retinal projections of real-world, three-dimensional objects, the probability that the object projecting the top line is larger than the one projecting the bottom line would be greater than 0.5.[1] Although this information is not helpful for judging the relative lengths of actual two-dimensional lines on a piece of paper—like those in Figure 1.1—it is a perfect example of how prior beliefs (implicit, in this case) can cause biased perception of a seemingly related stimulus.

[1] Although quite different in methodology, Weidner and Fink (2007) reach a concordant conclusion. Using functional magnetic resonance imaging (fMRI), they find that exposure to the Müeller-Lyer illusion leads to activation of the right intraparietal sulcus, which is implicated in top-down visuospatial processing.

Figure 1.1: The Müeller-Lyer Illusion: The Parallel Lines are the Same Length



This dissertation examines how perceptual biases, similar in nature to the Müeller-Lyer illusion, may fuel contemporary political polarization in America. As such, I examine a very particular form of perception: *social perception*, the process by which we form beliefs and make inferences about individuals and groups in society. Social perception operates in an especially top-down fashion. As Gibson (1979) observes, all sentient species have some form of direct perception. That is, they have evolved the ability to parse the richness of the environment and respond accordingly to individual stimuli, even if they cannot name and categorize them as part of a broader representation of the world. However, social perception falls outside of this realm of perception, especially when it comes to perceiving collectives. As Lippman (1922) notes, "The real environment is altogether too big, too complex, and too fleeting for direct acquaintance." That is, one cannot literally meet "liberals" or "the Republican Party," so we have developed ways of *indirectly* perceiving these groups—making inferences about what is being perceived from information stored in memory (Hinton, 2016).

To perceive groups from the "social background," people construct mental representations of the sociopolitical world, relying on classification systems to place individuals into categories. These systems' primary purpose is heuristic: to help individuals draw sharp distinctions between important social categories and thus most easily classify individuals. For example, *prototypes* are classification heuristics in which "fuzzy sets of attributes" stand in for groups (Hogg, 2006). However, attributes become prototypical not primarily because they are highly common in group g, but because they sharply predict membership in g over membership in out-groups $\neg g$ (Rosch and Mervis, 1975). Similarly, schematic processing tends to preclude ambiguity in mental representations of groups. By rejecting information inconsistent with prior beliefs about social groups, people tend to preserve sharp distinctions between social categories (Bartlett, 1932). Henri Tajfel (1969) summarizes the effects of such indirect social perception ably: "Stereotypes arise from a process of categorization. They introduce simplicity and order where there is complexity and nearly random variation. They

help us to cope only if fuzzy differences between groups are transmitted into clear ones, or new differences created where none exist" (p. 82).

In short, we desire simplicity and clarity in the social world, so our mental representations of it tend to heighten inter-group distinctions. Left unchecked, our mental representations may thus give rise to perceptual biases when we attempt to make inferences about the actual social world. Just as our implicit beliefs about the lengths of three-dimensional objects resembling the Müeller-Lyer segments lead us to incorrectly see lines of different length in Figure 1.1, our classification heuristics—e.g., prototypes, exemplars, and associative networks—lead us to see sociopolitical groups as more distinct than they actually are.

The first paper in this dissertation illustrates this phenomenon. "Self-Fulfilling Misperceptions of Public Polarization" investigates Americans' beliefs about how ideologically polarized other citizens are. American citizens tend to be ideologically moderate. Even those who describe themselves as "liberal" or "conservative" often do so for social reasons (e.g., Conover and Feldman, 1981) and tend to hold mixed bags of liberal and conservative issue positions (Ahler and Broockman, 2016; Baldassarri and Gelman, 2008; Broockman, 2016), unlike political elites. However, I show that citizens draw sharper political distinctions between their self-described liberal and conservative peers than actually exist. Across three broad policy domains, citizens consistently overestimate how liberal liberals are and how conservative conservatives are—and as a consequence, overestimate the degree of polarization in the mass public.

The second paper explores a similar phenomenon. In "The Parties in Our Heads," a joint paper with Gaurav Sood, I investigate Americans' perceptions of the social composition of the two main political parties. A dominant perspective on partisanship holds that Americans consider the types of people who tend to be Democrats and Republicans, form opinions and even party identifications accordingly, and reason about politics from there (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Converse, 1964; Gerber and Green, 1998; Green, Palmquist and Schickler, 2002; Hetherington and Weiler, 2009). However, this explanation for why people identify so strongly with their parties is somewhat puzzling because the mass-level parties are not especially distinct in their composition. The typical Democrat and the typical Republican are both straight, white, middle-class, upper-middle-aged, Christian Americans. Moreover, the groups that people often identify as core to the parties' brands tend to comprise just a small fraction of party identifiers. (For example, just 11% of Democrats belong to a labor union.)

The solution to this puzzle lies in Americans' *perceptions* of the parties' composition. People see the parties as far more distinct than they actually are. For example, people estimate that 39% of Democrats are union members and that a similar proportion of Republicans (38%) belong to the economic 1%. (Just 2% do.) Relying on five studies, the paper demonstrates that people overestimate the degree to which party members have prototypical characteristics—consistent with the notion that our simplified mental representations of the world can systematically bias our perception of the world as it is.

Even more importantly, these two papers demonstrate that these perceptual biases hold consequences for the political climate. In "Self-Fulfilling Misperceptions of Public Polariza-

tion," I show that overestimating mass-level polarization leads individual citizens to adopt somewhat more extreme issue positions than they otherwise would. And "The Parties in Our Heads" shows that skewed social perceptions about the parties contribute to the very misperceptions documented in the first paper: experimentally correcting people's beliefs about party composition makes them less likely to see Democratic and Republican citizens as politically extreme. However, most significantly, "The Parties in Our Heads" presents experimental and observational evidence that skewed perceptions about party composition fuel the intense partisan animus that many have noted (Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2014; Mason, 2015).

Thus, mass-level polarization is partially illusory. The very classification heuristics we rely on to make sense of the political landscape lead us to draw sharper distinctions between liberals and conservatives, and Democrats and Republicans, than actually exist. The resultant perceptual biases exacerbate ideological and affective polarization at the mass level.

The third paper in the dissertation probes the limits of perceptual biases in explaining polarization and its ills. "Irresponsible Partisanship and Democratic Accountability" evaluates the claim that asymmetric motive attribution—the apparent tendency of partisans to attribute their own party's role in conflict to in-group love but their out-party's role to out-group hate—sustains intractable political conflict. Waytz, Young and Ginges (2014) document this pattern and assert that people's beliefs about the affect underlying conflict may lead people to support their own party's intransigent behavior. However, this fails to square with two other conclusions. First, citizens generally despise elite party conflict and its consequences for government, even if it benefits their own party (e.g., Flynn and Harbridge, Forthcoming). Second, a broad consensus exists that elite party conflict is motivated more by electoral strategy than inter-party affect. Thus, if this attribution bias sustains irresponsible partisanship at the elite level, it must be because citizens fail to recognize the strategic nature of conflict. The paper investigates this possibility and finds that citizens generally see strategic explanations for conflict as more plausible than affective ones. However, I also explore citizens' opinions toward reforms that could curb the most unpopular *process consequences* of elite polarization and find a mixture of opposition and lack-of-opinion. Thus, I conclude the dissertation by suggesting that political psychology not only investigate citizens' perceptions of other citizens and elite actors, but also their knowledge of the institutions and processes that actually sustain polarization and its consequences at the elite level.

Chapter 2

Self-Fulfilling Misperceptions of Public Polarization

Listening to many pundits today, one might think that America is on the verge of an ideological civil war. According to this narrative, the population is split into two deeply committed factions and engaged in a righteous struggle for no less than the soul and future of the nation. But this sensational account doesn't square with reality. Time and time again, political science has found that citizens form political attitudes with very little ideological constraint (Converse, 1964, 2000), vote according to incumbents' performance rather than their issue positions (Campbell et al., 1960; Lenz, 2012), and identify as "liberal" or "conservative" largely for social and symbolic reasons (Conover and Feldman, 1981). And, ultimately, there is little evidence that Americans today reject centrism in their issue positions more than in the past (Brownstein, 2007; Fiorina and Abrams, 2009).

But do citizens recognize their own moderateness? Bombarded from the left and right by messages that convey mass-level ideological strife, do ordinary citizens' perceptions of public opinion diverge from reality? In two studies, I present evidence that they do: citizens tend to overestimate the liberalism of self-described liberals and the conservatism of self-described conservatives.[1] As one might expect, the severity of these misperceptions is heterogeneous across ideological groups, but both self-described liberals and self-described conservatives significantly overestimate extremism on both sides. This pattern of misperception implies a phenomenon akin to pluralistic ignorance of public moderateness: citizens tend to be relatively centrist, but they also misperceive themselves as outliers in this regard.

What are the consequences of such misperception for individuals' own attitudes? In Study 2, I present experimental evidence that individuals who are informed of the true distribution of public opinion tend to subsequently report political attitudes that are significantly more moderate than participants whose misperceptions are either left uncorrected or reinforced. In this sense, misperception of public extremism is, to a certain degree, a self-fulfilling process.

[1]This chapter previously appeared in *The Journal of Politics*, Volume 76 (3), pages 607-620.

Citizens' Perceptions of Ideological Groups

Over the past two decades, the press has frequently relied on the narrative of a polarized nation to explain political outcomes (Fiorina and Abrams 2008; also see Fiorina, Abrams, and Pope 2005 for excellent primary source examples). But while "the year of the angry white male" and "values voters" provide for better copy than "retrospective voting using the economy as a heuristic," the evidence tends to support the latter narrative (Converse, 1964; Jacobson, 1996; Bartels, 2008; Lenz, 2012). Regardless of the truth, however, if citizens' sources of political information convey the existence of deep division, then their perceptions of liberal and conservative positions and their beliefs about mass-level polarization may be quite inaccurate. As Mutz (1998, 5) notes:

> "One might say that mass media may not be particularly influential in telling people what to think, or perhaps even what to think about, but media are tremendously influential in telling people what *others* are thinking about and experiencing. These perceptions, in turn, have important consequences for the political behavior of mass publics and political elites as well."

Brady and Sniderman (1985) present partial evidence for such misperception of ideological groups, finding that 1972 and 1976 ANES respondents perceive sociopolitical groups on the left side of the political divide (e.g., liberals, Democrats, and African-Americans) as significantly more liberal on specific issues than their average member actually is. On the other hand, contrary to this study's hypothesis of a general misperception of extremism and polarization, Brady and Sniderman find that their survey respondents identify the average conservative location on specific issues with precision.

Why might citizens today also misperceive self-described conservatives' positions? For a start, social movements and elite politics on the right may have affected citizens' beliefs about mass-level conservatism. The rise of the evangelical, neoconservative, and Tea Party movements in the years since 1976 may have produced a rightward shift in citizens' general perceptions of conservatives. Citizens may also perceive greater mass-level extremism on the right because of the increase in elite-level polarization, which has been driven primarily by Republican elites becoming more extreme (McCarty, Poole and Rosenthal, 2006; Hacker and Pierson, 2006).

Indeed, if elites serve as exemplars of sociopolitical groups, attentive citizens may attribute greater extremism to rank-and-file liberals and conservatives even absent the mass polarization narrative. The rise of both polarized party politics and partisan media yields an information environment in which citizens more frequently encounter extreme liberal and conservative exemplars. And while these exemplars tend to be elite, evaluations of exemplars can affect evaluations of group members linked to them, even if such linkages are tenuous (Gilovich, 1981). In sum, today's information environment may lead individuals to overestimate mass-level polarization multiple ways: through the use of a polarization narrative, through the role that partisan journalists play as exemplars of liberals and conservatives,

and by transmitting information that elite political exemplars are, indeed, becoming more polarized.

In addition to historical developments and the changing information environment, affective processes may also be at work. Individuals may attribute extremism to a group as a consequence of the low affect they hold for that group. Conservatives' relative disdain for liberals in the 1970s, compared to liberals' generally neutral feelings about conservatives at that time, produced the asymmetric misperception of liberals that Brady and Sniderman (1985) document. Today, strong liberal and conservative identifiers hold more intense feelings about these groups than they did in the 1970s (Fiorina, Abrams and Pope, 2005; Iyengar, Sood and Lelkes, 2012).[2] While this trend is primarily limited to strong identifiers, the increased animosity among strong identifiers and ideological activists could potentially trickle down to the weaker identifiers via the mass media, which gives ideological purists a bigger megaphone and a greater ability to affectively tint the news in the post-broadcast era (Prior, 2007; Mutz, 2007).

Indeed, there exists evidence that citizens of all political stripes hold erroneous beliefs about both liberals and conservatives today. Citizens from all parts of the political spectrum tend to overestimate the difference in moral concerns between the two groups, and in particular, overestimate conservatives' stinginess and liberals' "bleeding-heartedness" (Graham, Nosek and Haidt, 2012; Farwell and Weiner, 2000). Furthermore, individuals believe themselves to be different from these stereotypes, a phenomenon similar to pluralistic ignorance.[3] While this concerns sociological stereotypes of liberals and conservatives rather than beliefs about their policy-related and ideological predispositions, it provides even more reason to investigate the latter. In particular, I suspect that citizens, and especially those who identify as liberals or conservatives, accept ideological extremity as a norm even if they would prefer centrist outcomes, on average (Fiorina and Abrams, 2009).

Hypothesis 1: Misperceived mass polarization Citizens tend to overestimate the extremity of their peers' political positions. More specifically, they overestimate the liberalism of self-described liberals and the conservatism of self-described conservatives.

Research on pluralistic ignorance has also shown that erroneous beliefs about the public can affect attitudes and behaviors by leading individuals to shift toward the perceived social norm (Miller, Monin and Prentice, 2000). In this case, I suspect that misperceptions of polarization have consequences for citizens' own political attitudes. Why might this be so? One reason is that citizens may use group cues as shortcuts in forming opinions. Under this view, citizens use perceptions of where their group stands to help them approximate the attitudes they would form were they to expend significant time and effort considering the

[2]This is measured by the difference in feeling thermometer ratings for "liberals" and "conservatives."

[3]Pluralistic ignorance is a social psychological concept that refers to "shared but erroneous beliefs about the attitudes and behaviors of other people" (Todorov and Mandisodza, 2004), or more simply, a state of affairs in which "no one believes, but everyone thinks that everyone believes" (Krech and Crutchfield, 1948).

issues (Lupia, 1994; Levendusky, 2010). Thus, when asked for an opinion on an unfamiliar issue, a respondent may satisfice by reporting what she perceives to be an opinion that a fellow group member would report.

A less sanguine view would argue that citizens are not rational cue-takers but rather blind followers of where they perceive their groups to be (Lenz, 2012; Mackie and Cooper, 1984). Under this view, "liberal" and "conservative" are more than sets of political orientations: they are also sociopolitical identities. As such, members of these groups "share some emotional involvement in this common definition of themselves, and achieve some degree of social consensus about the evaluation of their group and of their membership in it" (Tajfel and Turner, 2005). If individuals who identify as liberal or conservative find value in a common definition of what it means to be liberal or conservative, then it follows that they will more readily accept socially constructed, albeit inaccurate, definitions of what it means to hold that identity. In this case, being a member of the group involves reporting consonant views, even if those views are somewhat more extreme than those the citizen would normally hold. Perceptions of the other side would also be important according to this view, as group identities are partially defined in relation to outgroups (Citrin, Wong and Duff, 2001). Perceiving a high degree of mass-level polarization may heighten the perceived stakes of political conflict, thus leading citizens to perceive greater threat to their own group and place greater value on group solidarity. As a result, they may report views that are more consistent with perceptions of their in-groups' positions.

Most citizens are relatively unequipped with hard information for reporting opinions about politics and public policy, but they also tend to possess a handful of considerations for any given issue. In the absence of hard knowledge, these considerations may include heuristic or group-based cues (Converse, 1964; Zaller, 1992). The key point here, following the preceding discussion, is that perceived group cues—and, indeed, perceptions of the broader public debate—serve as considerations that affect public opinion. If citizens attribute greater extremism to groups on the left and right, such beliefs will factor into the opinions they form and report. But, we should note that group cues aren't the only considerations that citizens possess, in most cases. As a consequence, a false sense of polarization is not entirely self-fulfilling.[4] Rather, I expect that the public would be even more centrist in its opinions if citizens more accurately gauged mass-level opinion.

Hypothesis 2: The consequences of overestimating public polarization Perceptions of public debate color individuals' own opinions. As a consequence, overestimating the policy-related disagreement between self-described liberals and self-described conservatives leads citizens to report political opinions that are more extreme than they would with perfect information about where their peers stand.

[4]If it were, the public actually would be polarized and citizens would not hold erroneous perceptions as predicted in Hypothesis 1.

Study 1: Misperceived Extremism in California

Research Design

To test the hypothesis that citizens tend to overestimate self-described liberals' liberalism and self-described conservatives' conservatism, I relied on a population-representative survey of 2444 registered voters in California in April and May of 2013.[5] From this sample, I collected either respondents' own policy-related predispositions for two major policy domains in American politics, or their perceptions of self-described liberals' and conservatives' predispositions. The online sample was recruited through Survey Sampling International (SSI) and nearly perfectly matched the population of California voters on party registration, education, and race.[6]

Two important caveats on external validity are necessary. First, the sample contains only registered voters. Thus, any findings of perception-reality divergence can only be generalized to voters rather than citizens as a whole. Second, the sample is highly representative of registered voters in California, but not the nation as a whole. Nevertheless, while it is important to recognize these limitations, it is also important to recognize what we *can* say about the validity of any findings with this sample. Most significantly, by being able to generalize this study's findings to registered voters in California, we can show that the politically active class of citizens in the nation's largest state—a political entity unto itself—believe that the state's population is more polarized than it actually is.

I randomly asked half of the sample to place themselves on a sliding scale for two policy domains: the role of government in managing social welfare and the economy, and the tradeoff between protecting the environment and protecting jobs. Both scales ranged from 1 to 7, with the endpoints anchored by position statements.[7] Respondents could place themselves anywhere between 1 and 7 on these scales. I asked the other half of the sample to use the same sliding scales to denote where they thought "Californians who call themselves liberal" and "Californians who call themselves conservative" would place themselves. To test the hypothesis that citizens overestimate their peers' extremism, I compare the average perceptions of liberals and conservatives on these two policy dimensions, as reported by this latter random half of the sample, to the actual average of the positions reported by self-described liberals and conservatives in the first half of the sample.[8]

Why were these two policy domains chosen? I chose to include the ANES question about the proper role of government because it captures what we traditionally think of as the primary dimension of politics and the domain that serves as the sharpest cleavage between liberals and conservatives in American politics (Gerring, 1998; McCarty, Poole and

[5]This was a random subset of a sample of size n=5542, as allocated by the Institute of Governmental Studies.

[6]See SI section 1.

[7]These were taken from the traditional ANES 7-point scale questions for these policy domains. See SI section 2 for wording.

[8]I operationalize "liberals" as respondents located at 1-3 on the standard 7-point ideological scale, and "conservatives" as respondents located at 5-7.

Rosenthal, 2006). By contrast, I chose to ask about the environment because it has at times been a cross-cutting issue in California politics. Given that "green" is not solely associated with liberalism in California politics, and that Californians have prominent green conservative archetypes (including Arnold Schwarzenegger), we should expect this domain to be a tough test of the hypothesis that Californians over-attribute policy-related extremism on the basis of ideological identity.

Results

Do the California voters overestimate liberals' liberalism and conservatives' conservatism? To answer this question, Table 1 compares the average positions held by liberal and conservative Californians to the average perceptions of these groups. As the table shows, both liberal and conservative respondents significantly overestimate both groups' extremism on the proper role of government and environmental issues. Consistent with Brady and Sniderman (1985), conservatives' misperceptions regarding liberals are the largest. Unlike those findings, however, liberal and conservative respondents in this study both overestimate extremism within their own ranks.

Table 2.1: Liberal and Conservative Positions, as Perceived by Respondents in Study 1

	Role of Government		Environmentalism	
	Liberals	Conservatives	Liberals	Conservatives
Actual Mean Position	3.71	5.34	3.55	4.74
	(0.08)	(0.08)	(0.07)	(0.08)
	(n=444)	(n=342)	(n=342)	(n=445)
Mean Estimate by Liberal Respondents	3.45**	5.52**	3.32**	5.39***
	(0.08)	(0.08)	(0.07)	(0.07)
	(n=434)	(n=434)	(n=436)	(n=435)
Mean Estimate by Conservative Respondents	2.74***	5.64**	2.81***	5.12***
	(0.10)	(0.08)	(0.10)	(0.08)
	(n=305)	(n=306)	(n=305)	(n=306)
Mean Estimate by Moderate Respondents	3.63	5.16**	3.60	4.89*
	(0.08)	(0.08)	(0.08)	(0.08)
	(n=431)	(n=431)	(n=432)	(n=432)

NOTE: Standard errors in parentheses. Two-sided t-tests of the hypothesis that the mean estimate equals the actual mean position. Asterisks denote levels of statistical significance: * = $p < .10$; ** = $p < .05$; *** = $p < .001$.

To better understand this apparent phenomenon, Figure 1 plots kernel density estimates of liberal and conservative Californians' self-placements on these two policy dimensions against the estimated distributions of perceptions of these self-placements. For three of the four comparisons, the target group's actual modal position is well over a point less extreme

than the group's own modal perception of that position, and outgroup members' perceptions are even more divorced from reality.[9] Most significantly, Figure 1 shows that strange distributions of preferences cannot be responsible for citizens' misperceptions of their peers. With the exception of conservatives' self-placements on the role of government—the policy dimension/group dyad for which respondents tended to be most accurate—self-placements within ideological groups tended to be relatively unimodal with means (denoted in the figures with dotted vertical lines) roughly equal to modes. This rules out the possibility that respondents used "modal liberal" and "modal conservative" as heuristics for the groups' "average" identifiers.

Interestingly, one group is more accurate than the others in its perceptions of liberals and conservatives: the staunch moderates. Whereas both liberal and conservative identifiers believe liberal Californians to be significantly more liberal than they actually are, the moderate respondents who claim to not lean either way hold perceptions of liberals that are not significantly different from the truth. Moderates actually tend to underestimate conservatives' conservatism on economic issues. And while their perceptions of conservatives' environmental views are slightly more extreme than the truth, these perceptions are significantly better than those held by conservatives themselves. While unexpected, this finding is noteworthy, particularly when considering the information flows these individuals are likely to receive vis-a-vis other citizens. Moderate citizens may be less politically engaged (Abramowitz, 2010) and less likely to receive messages about mass polarization from the media and other elites as a result. Compounding this, ideological identifiers are more likely than true moderates to turn to the ideological and partisan media outlets that most heavily exaggerate polarization (Stroud, 2008). However, like partisans, ideological identifiers may simply enjoy political competition more than true moderates (Green, Palmquist and Schickler, 2002) and may overestimate conflict between the groups as a result. As discussed in the final section of this paper, future work should examine this unexpected result more systematically.

Study 2: The Effect of Misperceived Extremism on Political Attitudes

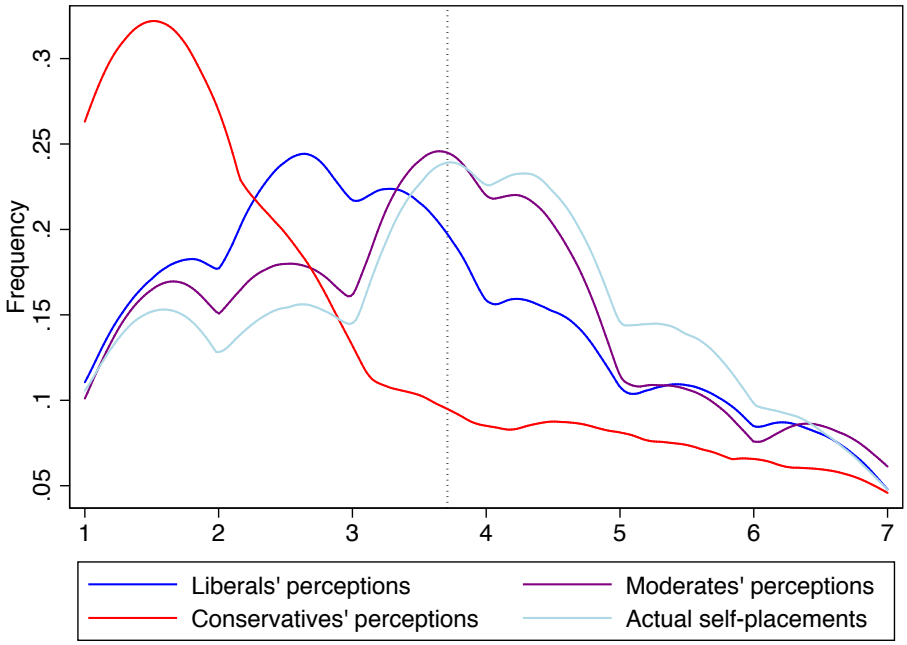
Study 1 demonstrates that voters in California who identify as liberal or conservative, or at least lean one way or the other, tend to overestimate the degree of polarization within the state's mass public. What are the consequences of this perceptual error? More specifically, do these erroneous beliefs about peers lead individuals to develop and report attitudes that are more extreme than they otherwise would? Study 2 relies on a survey experiment conducted on Amazon's Mechanical Turk (MTurk) online labor market to answer this question.[10]

[9] As Figure 1(b) shows, the sole exception is conservatives' position on the proper role of government. In this case, modal perceptions match up perfectly with the modal position of conservatives, but not with the mean position of conservatives.

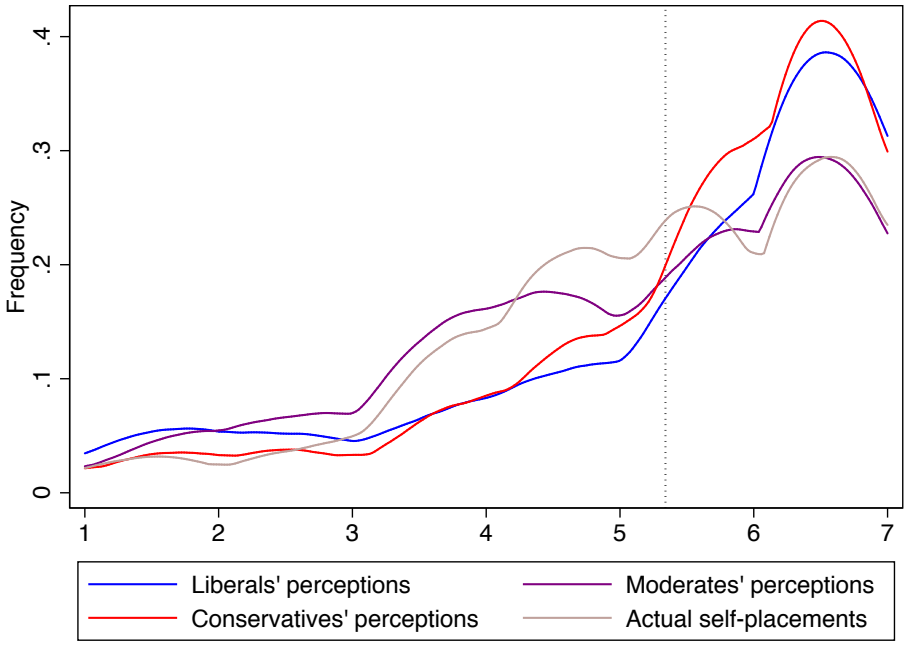
[10] As Berinsky, Huber and Lenz (2012) note, Amazon's online labor market is more representative of the United States population than the student samples traditionally used in psychology experiments, but it

Figure 2.1: Perceptions of Liberals and Conservatives in California, Compared with Actual Positions (Kernel Density Estimates)

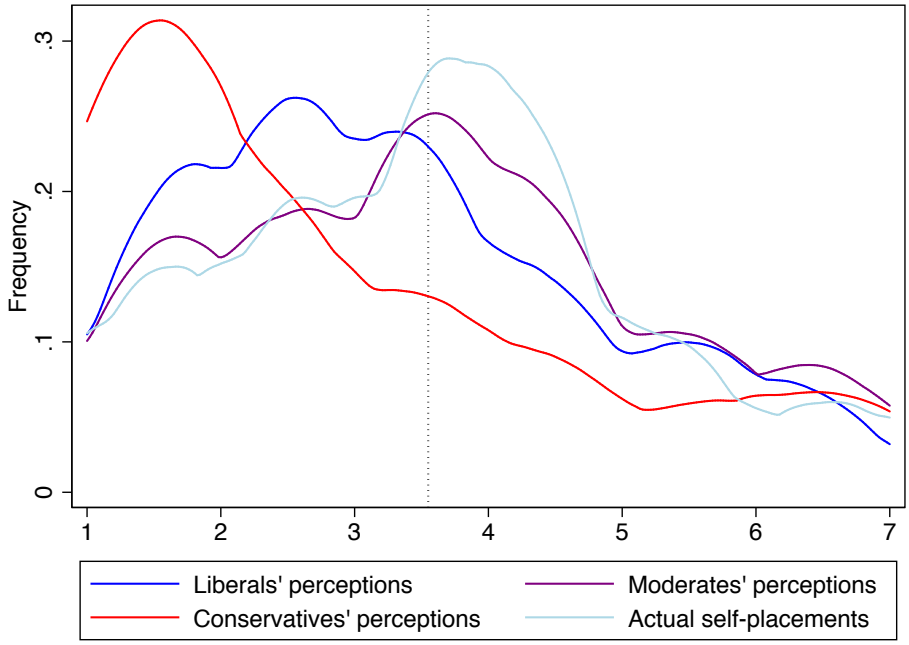
(a) Perceived and Actual Positions of CA Liberals on the Role of Government



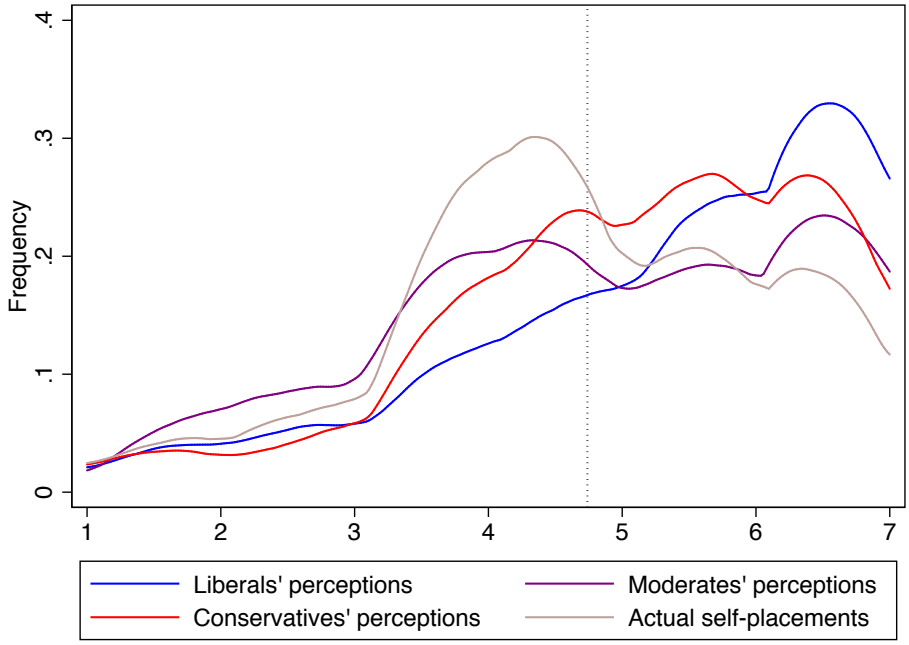
(b) Perceived and Actual Positions of CA Conservatives on the Role of Government



(c) Perceived and Actual Positions of CA Liberals on Environmentalism



(d) Perceived and Actual Positions of CA Conservatives on Environmentalism



Research Design

The goals of this study are twofold: to again demonstrate Study 1's findings with a different population and to determine whether these misperceptions affect individuals' political attitudes. As such, a "tell-ask" experimental research design is an ideal strategy because it allows us to do both of these things in cases in which widespread ignorance exists in the population of interest. In this design, the true positions of liberals and conservatives are provided to subjects assigned to the "tell" condition, thus manipulating beliefs by *removing* the ignorance.[11]

The procedures for the "tell" and "ask" conditions were identical but for the manipulation itself, which occurred over a series of three screens on an Internet survey.[12] I presented participants assigned to the "ask" condition with two manipulable sliding seven-point scales on each of these three screens. Each screen contained the text from an American National Election Study (ANES) issue scale question and asked participants to estimate the "average positions taken by people who call themselves liberal and people who call themselves conservative." In an attempt to capture three different, salient, dimensions of political conflict in America, I asked participants to estimate the average liberal and conservative positions on: 1) whether or not the government should guarantee each person a job and a certain standard of living, 2) how much they favor or oppose the U.S. government paying for all necessary medical care for all Americans, and 3) the importance of environmental protection versus protecting jobs and standard of living.[13] After identifying their perceptions of the average liberal and average conservative stance on these policy domains, participants moved on to the survey content in which the dependent variables were measured.

I presented participants randomly assigned to the "tell" condition with graphics that

is "less representative than subjects in Internet-based panels or national probability samples." The more often expressed concern about MTurk regards validity. Many fear that the economics of MTurk incentivize participants to take surveys and experiments less-than-seriously (Paolacci, Chandler and Ipeirotis, 2010). Both studies cited in this footnote, however, have replicated results conducted in the laboratory, suggesting that Mechanical Turk can be used to make inferences that are at least as valid as those made using traditional convenience samples.

[11]Similar information-providing designs have been implemented to test whether the correction of widely-held misinformation affects political attitudes. See Kuklinski et al. (2000), Gilens (2001), Todorov and Mandisodza (2004), and Howell and West (2009).

[12]Since the nature of the study made the collection of party and ideological self-identification problematic—collecting these data prior to the experiment would introduce priming, but collecting them after the experiment exposes them to the effects of the treatments—I used a two-wave panel design. Participants reported party identification, ideological self-placement, education, and political knowledge in the first wave. Participants completed the second wave, in which the experiment was embedded, five to nine days after the first. Attrition was high (49.3%), albeit typical for Mechanical Turk panels, and a potential concern is that participants in the second wave were unusual, thus severely limiting external validity. However, no differences emerge to trigger concerns about nonresponse bias. (See SI section 3.)

[13]The first and third questions are analogous to the "role of government" and "environment-economy tradeoff" questions used in Study 1. Study 2 was actually conducted prior to Study 1, in 2012, and the Supreme Court had not yet ruled on the Constitutionality of the Patient Protection and Affordable Care Act. For this reason, I removed all questions and manipulations related to health care from Study 1.

looked identical to the sliding scales used in the "ask" condition, but these graphics were not manipulable. Instead, they showed participants liberals' and conservatives' true average positions on these three policy dimensions. (See SI section 4 for a comparison of the experience across the conditions.) The average liberal position shown to subjects in "tell" was the mean self-placement on each of the issue scales by 2008 ANES respondents who identified as liberal, while the average conservative position was the mean self-placement on the same issue scales by ANES respondents who identified as conservative.[14]

However, these types of manipulations present a compound treatment problem: the "tell" treatment manipulates not only participants' beliefs, but also the certainty with which they hold those beliefs. By comparing the "tell" and "ask" groups on the dependent variable, we estimate an average treatment effect of being *fully informed* of the true state of the world. While participants in the "ask" condition might only be taking a best guess about the state of the world, participants in the "tell" condition believe with certainty that the information they possess is correct, provided that the manipulation works as intended. This certainty, rather than the information itself, could affect outcomes on dependent variables of interest. If, indeed, respondents use beliefs about groups' positions as heuristic considerations when reporting policy opinions, these considerations may become more important as certainty about the groups' positions increases. On the other hand, if respondents believe the groups hold extreme positions but hold such beliefs with uncertainty, they may discount these beliefs and instead rely more heavily on other considerations. Thus, at best, the compound nature of the "tell" treatment vis-a-vis the "ask" condition renders causal inference murky, and, at worst, could bias results.

I solve this problem with a treatment that differs only from the "tell" condition on the information presented. Whereas the "tell" condition informs participants that self-described liberals and conservatives are relatively moderate, the third condition (called "distort") attempts to impart (false) knowledge that the public is, in fact, relatively more polarized along ideological lines than it actually is. While results from the "tell" and "ask" conditions can determine whether misperceptions affect attitudes, comparing the "tell" and "distort" conditions can determine whether misinformation about polarization, decoupled from certainty, affects attitudes. The average positions given to participants assigned to this condition were the mean self-placement on each of the issue scales by ANES respondents who self-identified as "extremely liberal" and "extremely conservative."[15]

On the same screen, I asked participants in "tell" and "distort" whether or not the locations depicted on the issue scales surprised them. This question serves three purposes. First,

[14]Similar to Study 1, ANES respondents who self-placed at 1, 2, or 3 on the seven-point ideology scale were considered liberal, and ANES respondents who self-identified as 5, 6, or 7 on the seven-point ideology scale were considered conservative.

[15]These were the respondents who self-placed at 1 and 7 on the ANES 7-point scale. On the healthcare issue scale, though, the mean location of "conservative" respondents was actually more conservative than that of "very conservative" respondents. Because the goal of the "distort" condition is to create the illusion of mass-level polarization as the true state of the world, I chose to use the average response of the former group.

providing interactive content slows the survey experience on the manipulation screen, thus decreasing the probability that quick survey-takers miss the manipulation. Second, without such a question, participants in these two conditions might wonder why they were shown the information and begin to suspect their role as experimental subjects rather than survey respondents, thus opening the door for noncompliance or demand characteristics. Finally, the question about whether or not the locations are surprising serves as a manipulation check. Responses to this question can indicate whether the "tell" condition presented novel information, and similarly, whether the "distort" condition conformed to participants' prior beliefs. The manipulation check confirms the effectiveness of the treatments. Additionally, randomization checks confirm that random assignment produced groups that were balanced on key covariates. [16]

The primary dependent concept of interest is the degree of extremity of policy opinions reported post-treatment. I asked for opinions on six specific policies, each of which was related to one of the broader policy dimensions used in the treatments. These policy questions included five-point scales related to Social Security privatization, the 2009 economic stimulus, deficit reduction via raising taxes versus cutting spending, the Keystone XL pipeline, cap-and-trade, and a three-point question on the Patient Protection and Affordable Care Act. Each item presented a neutral response (e.g., "Neither favor nor oppose"), and such responses were always located at the midpoint of the response set. I measure response extremism, the dependent variable, as the absolute distance of the reported opinion from the neutral response. As per Hypothesis 2, I expect that subjects in the "tell" condition will report policy opinions that are significantly closer to the midpoints of the policy questions than subjects assigned to the "ask" or "distort" conditions. Because responses to individual survey items tend to be highly noisy byproducts of multiple considerations (Zaller, 1992; Ansolabehere, Rodden and Snyder, 2008), I construct an index to measure the average extremism of responses to all questions (with extremity for each component item rescaled 0-1). This index, ranging from 0-1, serves as the primary outcome variable.[17]

Results

I turn first to presenting a similar pattern of results as those from Study 1. As Figure 2 shows, participants asked about their perceptions of the typical liberal and conservative positions on the three policy dimensions reported beliefs that were significantly different from the truth. These average misperceptions were large and in the expected direction: participants' perceptions of liberals and conservatives were far closer to the positions held by the most extreme identifiers (those shown to participants assigned to the "distort" condition) than to the true average positions of all group members (the positions shown in the "tell" condition).[18]

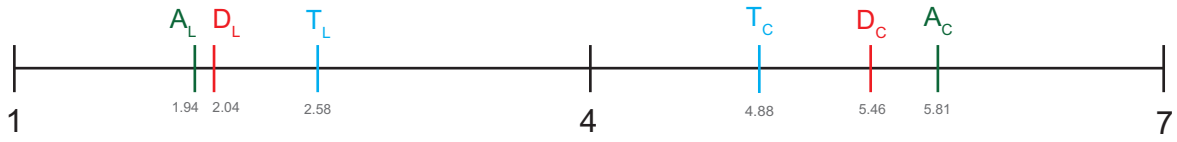
[16] See SI sections 7-8.

[17] See SI section 6 for details on the index and component items.

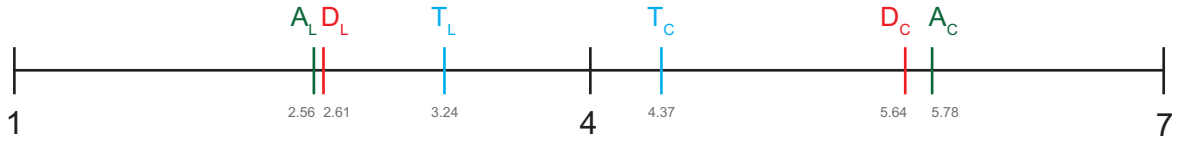
[18] As the confidence intervals indicate, the perceptions reported by participants assigned to "ask" tend not to be significantly different from the positions held by the most extreme liberals and conservatives. See

Figure 2.2: Spatial Representation of Perceptions of Liberal and Conservative Policy-Related Predispositions, Compared to Information Given in "Tell" and "Distort"

(a) Role of Government in Economic Welfare



(b) Healthcare



(c) Environmental Policy/Protection

NOTE: A_L and A_C are the average liberal and conservative positions given by subjects assigned to the "ask" condition. Thick black bars represent 95% confidence intervals. T_L and T_C are the average positions reported by all self-described liberals and self-described conservatives on the 2008 ANES, and subsequently used for the "tell" condition in this study. D_L and D_C are the average positions reported by the most extreme liberal and conservative identifiers on the 2008 ANES, used for this study's "distort" condition.

Table 2.2: Liberal and Conservative Positions, as Perceived by Liberal, Conservative, and Moderate Participants in Study 2

	Role of Government		Universal Healthcare		Environmentalism	
	Liberals	Conservatives	Liberals	Conservatives	Liberals	Conservatives
True Position	3.64	5.13	2.58	4.88	3.24	4.37
Mean Estimate by Liberal Participants	2.67***	5.91**	2.01**	5.82***	2.78**	5.92***
	(0.23)	(0.23)	(0.17)	(0.25)	(0.20)	(0.17)
	(n=41)	(n=41)	(n=41)	(n=41)	(n=41)	(n=41)
Mean Estimate by Conservative Participants	2.17***	5.65*	1.70***	5.96***	2.08***	5.64***
	(0.29)	(0.27)	(0.20)	(0.21)	(0.18)	(0.21)
	(n=16)	(n=16)	(n=16)	(n=16)	(n=16)	(n=16)
Mean Estimate by Moderate Participants	2.43**	5.71*	2.04	5.57	2.42**	5.48**
	(0.33)	(0.32)	(0.31)	(0.42)	(0.32)	(0.32)
	(n=12)	(n=12)	(n=12)	(n=12)	(n=12)	(n=12)

NOTE: Standard errors in parentheses. Two-sided t-tests of the reported estimates against the true positions taken by the average liberal or average conservative on the 2008 ANES. * = $p < .10$; ** = $p < .05$; *** = $p < .001$.

As in Study 1, these misperceptions are not limited to beliefs about the outgroup. As Table 2 shows, both liberal and conservative participants tend to overestimate the extremism of both groups. Thus, we again observe a tendency to overestimate the degree of polarization between citizens who identify as liberal and citizens who identify as conservative, rather than a simple attribution of extremity to the other side.

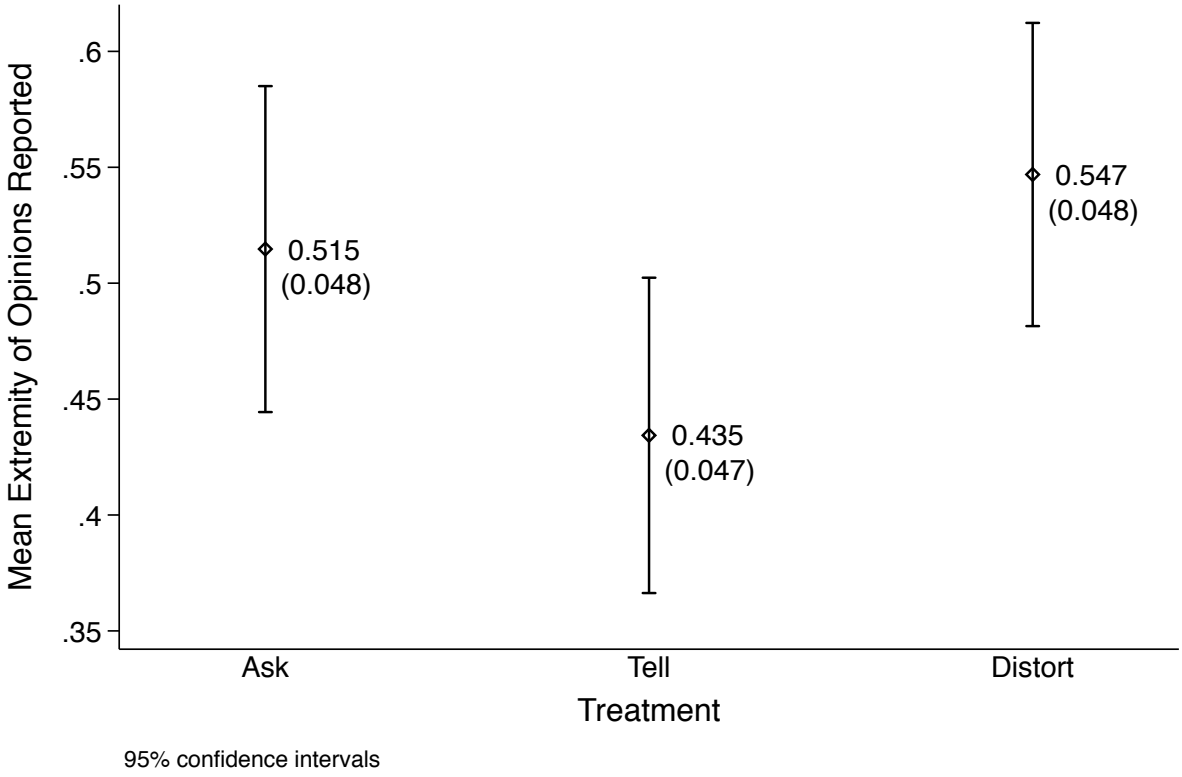
Evidence of moderates' better perceptual accuracy is less clean in Study 2. The sample included just 12 non-leaning moderates, rendering meaningful inferences difficult to obtain. Further, the evidence from this small subsample of moderates is mixed. On the one hand, they report the most accurate perceptions, on average, for three of the six issue-group pairings, better than any other group. On the other three issues for which they are not more accurate than the target group itself, moderates' perceptions are consistently better than those of the target group's outgroup (e.g., conservatives beliefs about liberals' positions). On the other hand, the differences between moderates' average perceptions and those of the other groups are not significant, while moderates' perceptions are significantly different from reality for five of the six issue-group pairings. But again, we should interpret these results with caution: 95% confidence intervals for moderates' average perceptions were large, ranging from 1.35 to 1.84 points in width on the 7-point scale.

Having found that participants overestimate their peers' political extremity, I turn to testing the effect of the "tell" condition. Does clearing up these misperceptions reduce participants' own extremity? Consistent with Hypothesis 2, the experimental results suggest that it does. Informing participants of the public's moderateness appears to decrease the average extremity of opinions participants report in the battery of policy questions. As

SI section 5 for a full table corresponding to Figure 2.

Figure 3 shows, participants assigned to the "tell" condition report attitudes that are 8.0 percentage points more moderate, on average, than those reported by participants assigned to the "ask" condition ($p = .09$). This suggests that misperception of public polarization induces attitudes that are more extreme than they otherwise would be. This apparent effect translates into a poleward movement of .24 on a seven-point scale. While this may seem relatively small, it is important to note that if the "ask" condition most accurately represents the state of the world, polarization on these issues is close to half a point greater on a seven-point scale than it would be were participants fully informed about where liberals and conservatives stand.

Figure 2.3: Mean Extremity of Political Opinion Reported by Study 2 Participants, by Experimental Condition



NOTE: Extremity is measured as the average of all policy opinions reported, folded. Standard errors in parentheses. The difference in extremity between the "ask" and "tell" conditions is -0.80 ($p < 0.10$). The difference in extremity betwen the "distort" and "tell" conditions is -0.113 ($p < 0.05$). The difference in extremity between the "ask" and "distort" conditions is 0.032, which is not statistically significant. All p-values are based on two-sided t-tests. See SI section 9 for regression tables.

Similarly, reinforcing misperceptions of mass-level extremity produces policy opinions

that are significantly more extreme, on average, than those reported by participants whose incorrect perceptions were dispelled. Figure 3 shows that participants assigned to the "distort" condition reported opinions that were 11.3 percentage points more extreme, on average, than those reported by participants assigned to the "tell" condition ($p = .02$). Again translating this into spatial terms, the average treatment effect of the "distort" condition is equivalent to a movement of .34 toward the appropriate anchor of a seven-point scale. Note, however, that while participants assigned to "distort" reported attitudes slightly more extreme than those assigned to "ask," this difference is not statistically significant. The major difference is found between "tell" and the other two treatments. Correcting participants' ignorance of the public's centrism thus appears to lead participants to report policy positions roughly 8–11 percentage points (or 0.48–0.68 scale points) more moderate, on average, than they otherwise would for these issues.

The estimates reported in Figure 3 are average treatment effects of information, generated by comparing the mean extremity of opinions reported by participants from the three groups. However, we know that some participants (17.9%) assigned to the "tell" condition reported being unsurprised by the relatively centrist positions held by self-described liberals and conservatives. In addition to the average effect of this information, we might be interested in the effect of such information being novel or surprising. We can estimate such an effect via instrumental variables. This procedure is relatively straightforward for a randomized experiment with one-way noncompliance, which we have in the case of comparing the "ask" group to the "tell" group.[19] By using assignment to the "tell" condition as an instrument for becoming informed about other citizens' relative moderateness, we can estimate a *complier average causal effect* ($CACE$) (Bloom, 1984; Angrist and Pischke, 2009). This is a local average treatment effect for individuals whose beliefs about their peers' extremism would be changed by the information provided in the "tell" condition.

According to such an analysis, as expected, the effect of the treatment on compliers is larger than the corresponding average treatment effect depicted in Figure 3. Compared to those assigned to the "ask" condition, participants who learn that liberals and conservatives are actually more centrist than they had previously thought report opinions that are 13 percentage points more moderate, on average (s.e. $= 0.07$, p $= 0.08$, two-sided t-test, $n = 67$).[20] Information about fellow citizens' relative moderateness does not affect everyone equally. But when this treatment does alter perceptions of public polarization, its effect on the political opinions that individuals subsequently report is quite apparent.

The results presented thus far address the moderating effect of information about the public's relative centrism on individuals' *average* extremity, measured by an index of policy opinion questions. This is the most reasonable way to gauge such an effect, as responses to individual survey items reflect a sample of disparate considerations and are prone to measurement error (Zaller, 1992; Ansolabehere, Rodden and Snyder, 2008). Given this tendency,

[19] That is, there is no way that participants assigned to the "ask" group could have received the treatment, but participants assigned to the "tell" group could have been—and were, in 17.9% of cases—unaffected by it.

[20] See SI section 10 for full details on the analysis and regression tables.

we should be surprised to find a significant effect of the "tell" treatment on all of the individual policy opinion items, and, indeed, very few of these tests reach statistical significance.[21] However, the coefficients associated with the "ask" and "distort" treatments are positive for 10 out of the 12 individual items, which we would only expect with a probability of .045 if the sign of these coefficients was determined by the flip of a fair coin.[22] This outcome indicates the robustness of the main finding and implies that no single opinion item is driving the results reported in the pooled analyses.

Discussion

The studies here provide evidence that citizens believe their peers to be more polarized than they actually are. Study 2 also provides evidence that such misperceptions can affect individuals' own political attitudes. When misperceived extremism is cleared up, participants report attitudes of their own that are more moderate, on average, than those of participants whose prior beliefs about liberals and conservatives are simply primed or are reinforced. In this sense, misperceptions of polarization appear to be partially self-fulfilling: erroneous beliefs about the extreme views of liberal and conservative citizens lead individuals to adopt views that are slightly more extreme in and of themselves.

The finding that liberals and conservatives tend to view members of both groups as more extreme than they actually are is distinct in comparison to previous studies finding that citizens are accurate with respect to their own groups' positions but overattribute extremism to the outgroup (Brady and Sniderman, 1985; Linville and Jones, 1980). Indeed, Study 1 provides evidence that the most perceptually accurate participants are the moderates—those who explicitly choose not to identify with either group when given the chance to lean one way or the other. The reason for this noteworthy finding is beyond the scope of this research but certainly worth future exploration. Past research has often concluded that choosing sides correlates with civic engagement and political knowledge (Keith et al., 1986; Abramowitz, 2010). In the case of accurate perception of public opinion, this does not appear to be true.

Interestingly, a similar pattern of across-the-board misperception emerges in Farwell and Weiner's (2000) lab study of stereotypes of liberals and conservatives: members of both groups believe that ingroup members as well as outgroup members will behave in a more stereotype-confirming fashion than they actually do. Similarly, Robinson et al. (1995) find that partisans are likely to believe their fellow group members are more ideologically-motivated and less politically pragmatic than they truly are. The point is that erroneous perceptions of other citizens are not restricted to beliefs about the outgroup, but instead appear to follow a more general pattern of overestimated polarization.

[21] See SI section 6 for these individual tests and survey item details.

[22] When we drop the analysis of the "distort" condition on health care because respondent perceptions of liberals and conservatives on this issue were actually *more* extreme than the positions reported in "distort," we are left with positive coefficients on 10 out of 11 individual-item analyses ($p = .013$).

Why do we observe a discrepancy between the pattern of results found in this study and the others described above when compared to Brady and Sniderman's seminal finding of one-sided misperception?[23] While this question is outside the purview of this study, a possible avenue for future research concerns the information flows to which citizens are exposed today compared to past decades. The proliferation of partisan media via cable television, talk radio, and the Internet is a relatively recent phenomenon and one that has shaped the way that citizens learn about politics. Gone are the days of the dominance of dry and relatively unslanted nightly news broadcasts. Instead, the most politically interested can choose from any number of ideologically congruent media outlets, while the least interested can opt out altogether (Prior, 2007). And as the late James Q. Wilson (2006) argues, the increased competition in the media landscape has produced greater sensationalism and combativeness in political coverage. Even in traditional news outlets, however, citizens today encounter more extreme exemplars of liberals and conservatives due to elite polarization. This, in and of itself, could affect citizens' perceptions of the groups at the mass-level. Future experimental research could manipulate the types of media to which individuals are exposed, as well as the elite exemplars covered therein, to better identify the effect of information flows on perceptions of mass-level polarization.

Additionally, citizens have become increasingly sorted into liberal and conservative enclaves in recent decades (Bishop, 2009). Residential sorting tends to produce social networks that are more homogenous with respect to politics, which tends to encourage political discussion within networks (Mutz, 2002). One might conclude that increased discussion would lead citizens to hold more accurate perceptions. However, when citizens think about sociopolitical groups, they may be most likely to recall the most ideologically devoted members. Ideologues are most likely to engage in campaign activities, try to persuade other citizens to adopt similar beliefs, and undertake the types of actions that make them easily accessible as liberal or conservative archetypes (Allport and Hartman, 1925; Fiorina, Abrams and Pope, 2005). Thus, the effect of residential sorting on how citizens perceive their own sociopolitical groups is another avenue of research worth exploring.

The psychological mechanisms underpinning this phenomenon should be fully explored, but so too should potential cures. The "tell" treatment very rarely exists in the real world, if at all, but the "distort" treatment is available with the click of a mouse or the tuning of a dial. This holds consequences for democratic accountability. A public that believes itself to be divided into two extreme factions is one that can be more easily manipulated by an elite political class that is, by many accounts, more extreme than the citizens it informs and represents (Fiorina and Abrams, 2009; Bafumi and Herron, 2010). The research presented

[23] A potential reason pertains to samples. Brady and Sniderman rely on the ANES, a representative sample of the American population, while the studies discussed and cited here rely on a representative sample of Californian voters and a variety of convenience samples. The results from California in Study 1 should cast doubt on this explanation. The distribution of ideology in California is not unlike that of the entire United States (see SI section 1.) More importantly and contrary to popular belief, Californians tend not to be isolated in liberal and conservative havens where they rarely encounter the other side of a debate (Kousser, Phillips and Shor, 2013; Ahler, Citrin and Lenz, Forthcoming).

here provides evidence that such erroneous beliefs do not simply exist, but that citizens consider these beliefs when forming opinions and subsequently veer from the political center themselves. As such, finding a way to broadcast the true distribution of public opinion may constitute a start toward improving the political climate and re-engaging citizens in constructive political discussion.

Chapter 3

Supporting Information for "Self-Fulfilling Misperceptions of Public Polarization"

3.1 Description of Study 1 Sample

The sample used for Study 1 was an online quota sample provided by Survey Sampling International (SSI). Although it was not a probability sample, as is true of many online samples, it represented California voters well. As the table below details, the sample was a near-perfect match on education and party registration. It also was a near-perfect match on race, although it significantly underrepresents Latinos. One possible reason for this is that the survey only asked one race/ethnicity question, and some Latinos may have chosen "white" rather than "Latino." However, it is also worth noting that the sample overrepresents voters born in the US.

SI Table 1 compares the demographics of the SSI sample used in Study 1 to a probability sample of California voters conducted by Field Poll and UC Berkeley's Institute of Governmental Studies in April and May of 2013, as well as to California Census data and party registration data from the California Secretary of State.

The non-probability nature of the sample may be of concern to some readers. Indeed, Study 1 would be of little use if it did not accurately reflect a population of interest. In particular, some may worry that the opt-in nature of the sample will yield respondents who are more politically interested and knowledgeable. In this case, however, such concerns are largely unfounded. I compare the SSI sample used for Study 1 to the 2012 ANES subsample of Californians, the 2012 CCES subsample of Californians, and an IGS/Field Poll probability sample of Californians on various knowledge and ideology measures and show that the SSI sample comports quite well with the ANES and Field Poll results, and appears to represent the population of interest better than the CCES.

Table 3.1: Representativeness of SSI Sample for Study 1

	SSI Sample (April/May 2013)	IGS/Field Poll (September 2012)	CA Census (2010)	CA Sec. of State
Sampling frame	registered voters quota	registered voters probability	population	registered voters population
Age				
18-19	4%	2%	4%	
20-24	11%	9%	10%	
25-34	17%	15%	19%	
35-49	20%	19%	28%	
50-64	30%	24%	24%	
65 & over	18%	30%	15%	
Gender				
Male	43%	49%	50%	
Female	57%	51%	50%	
Race				
White/Caucasian	82%	82%	75%	
African American	8%	8%	8%	
Asian	11%	11%	17%	
Ethnicity				
Hispanic or Latino	15%	21%	38%	
Not Hispanic or Latino	85%	79%	62%	
Education				
Some high school	2%	4%	12%	
High school/GED	13%	15%	24%	
Some college/2-year degree	37%	33%	34%	
4-year college degree	30%	26%	18%	
Graduate/professional degree	17%	21%	11%	
Birthplace				
United States	93%	83%	73%	
Outside the US	7%	17%	27%	
Parents' birthplace				
Both born in US	73%	78%		
One in US, one outside US	10%	10%		
Both born outside US	17%	11%		
Not sure	1%	1%		
Marital status				
Single, never married	34%	29%		
Married	43%	56%		
Separated/divorced/widowed	19%	14%		
Domestic partnership	5%	NA		
No answer	NA	1%		
Party registration				
Democratic	47%	46%	43%	44%
Republican	28%	31%	30%	29%
No party preference	16%	18%	21%	21%
Other	9%	5%	5%	6%
Ideology				
Liberal (inc. leaners)	37%	35%		
Moderate	34%	22%		
Conservative (inc. leaners)	29%	42%		
Response to question: "Direction of California?"				
Right direction	40%	41%		
Wrong direction	60%	59%		

Ideology question in SSI survey was a 7-point scale, while it was an 11-point scale (0-10) for the Field Poll survey, which was conducted over the telephone.

Political Knowledge

The sample used in Study 1 was asked a series of questions particular to knowledge of California politics and government.[1] Respondents were randomly assigned to answer three of these questions. On average, they answered 49.6% correctly.

Most importantly, three of these questions (party in control of the governorship, party in control of the State Assembly, and party in control of the State Senate) appear on the 2012 CCES. The figures below compare Study 1 respondents' performance on these questions to 2012 CCES respondents' performance by plotting the distributions of correct response rates from the two studies.[2] The figures demonstrate that the samples are quite similar in terms of their knowledge of party control of Californian political institutions. Slightly fewer respondents answer all questions correctly in the Study 1 sample. More interestingly, while 96.7% of CCES respondents can name the governor's party, just 76.6% of the Study 1 sample can. At least in terms of knowledge of the partisan-institutional landscape of California politics, the SSI sample used in Study 1 appears to be less politically knowledgeable than the CCES sample.

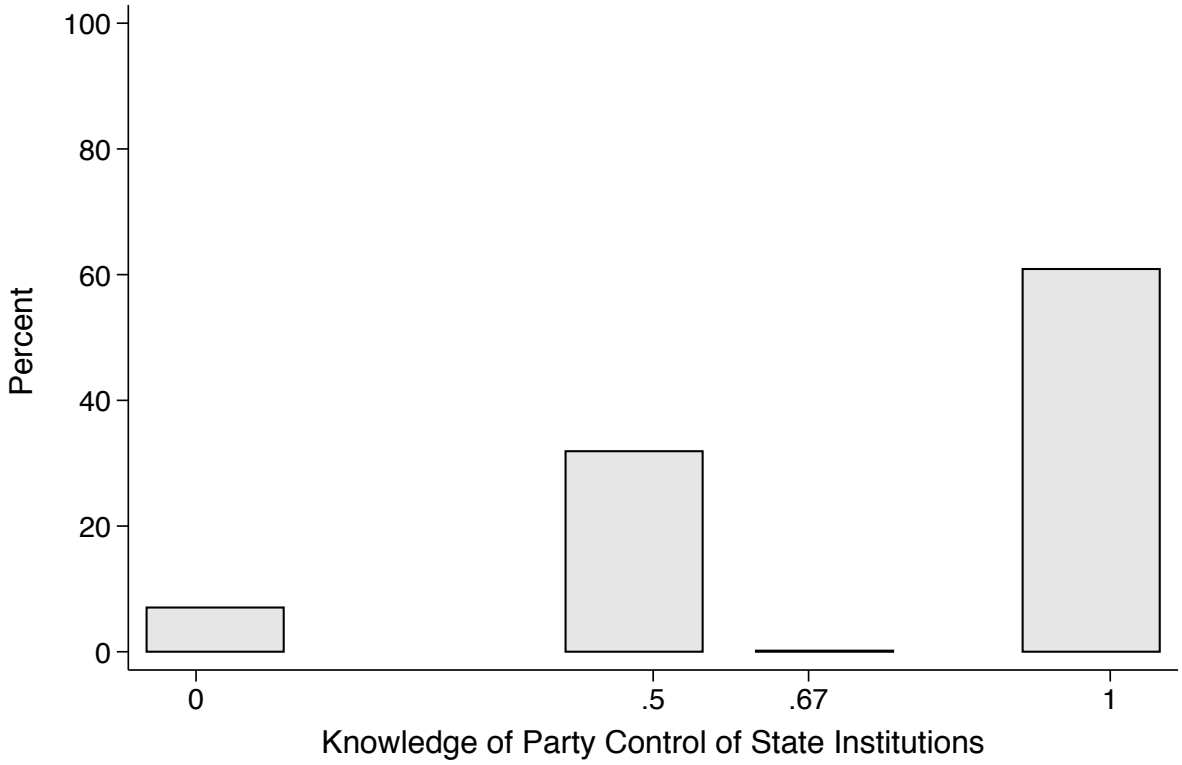
Similarly, co-authors and I relied on another SSI sample of California voters for a study conducted in May 2012. (A final version of this SI will include a citation to the study, but in the interest of preserving the integrity of the double-blind review process, I have redacted it here.) This study included more knowledge items that can be compared to the CCES and ANES. One of these is the knowledge that the Democratic Party is more liberal than the Republican Party, measured by comparing 7-point ideological placements of the two parties. In the SSI sample, 64.9% of respondents correctly identified the Democratic Party as more liberal. In the 2012 ANES sample, 76.2% of Californians correctly placed the Democratic Party to the left of the Republican Party. By contrast, in the CCES sample, an incredible 97.6% of respondents correctly placed the two parties in relation to each other.

On political knowledge, the evidence seems clear that the SSI respondents were less knowledgeable than CCES respondents, and similar to (and likely no more knowledgeable than) respondents to the ANES, the gold standard of survey research in political science. While knowledge and interest are two distinct concepts, they covary highly, making it doubtful that the opt-in sampling design used by SSI introduces sampling bias on political interest. (Certainly, we should be more concerned about the opt-in sampling procedures used by the CCES on this front.)

[1] These were multiple choice questions about: California governor's party, party in control of the State Assembly, party in control of the State Senate, the party that is more favorable toward extending citizenship to undocumented immigrants, the issue Proposition 13 concerned, whether the California constitution can be changed by popular vote, the size of a legislative majority needed to change tax rates in California, and whether photos of candidates ever appear on ballots in American elections.

[2] The analysis for Study 1 only examines respondents randomly assigned to answer at least two questions about institutional control.

Figure 3.1: Knowledge of Party Control of California Institutions, Study 1 Respondents (SSI)

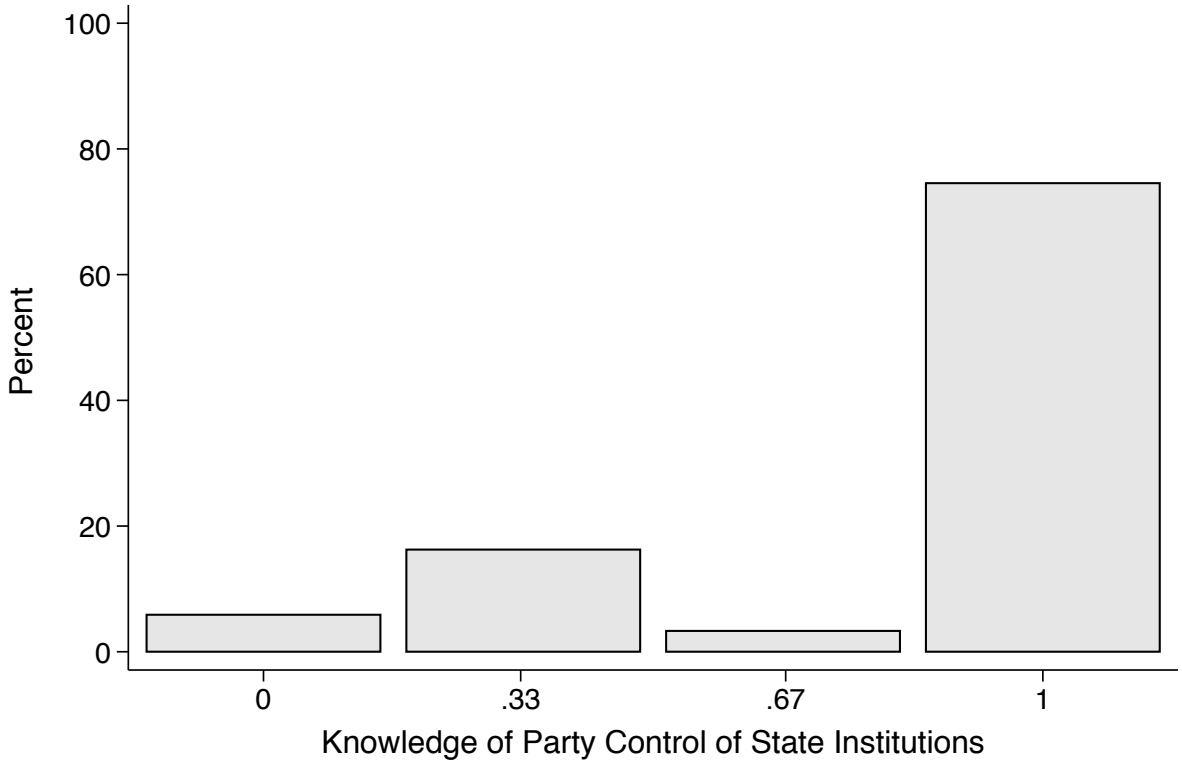


Ideology

Ideology is also related to political interest, with engagement tending to increase with stronger liberal or conservative leanings (Abramowitz 2010). We might be concerned about the findings from Study 1 if the sample were more ideologically motivated than the population of interest. I again rely on ANES and CCES data to dispel these concerns, as well as a probability sample of Californians recruited for a 2012 Field Poll (also referenced in Table SI-1).

As the histograms below show, the SSI sample used in Study 1 paints a similar picture of the distribution of ideology in California as do the 2012 ANES and the probability sample from the Field Poll. In particular, the distribution of ideology in the SSI sample and the ANES sample are nearly identical, but even the 11-point question used for the Field Poll generates a similarly shaped distribution. Each of these figures indicates that strict moderateness is the clear modal ideological self-placement in California. By contrast, the CCES data is unique in that the most moderate option is not the modal self-placement and that

Figure 3.2: Knowledge of Party Control of California Institutions, 2012 California CCES Respondents



Sample weights used in the analysis. Results are nearly identical without weights.

much more density is concentrated among more extreme positions.

In sum, nothing in these analyses should lead us to believe that the SSI sample introduces sampling bias by yielding a more ideological or politically interested group of respondents. Indeed, the unweighted SSI data appears much more representative of California than the weighted CCES data.

Figure 3.3: Ideological Self-Placement, Study 1 Respondents (SSI)

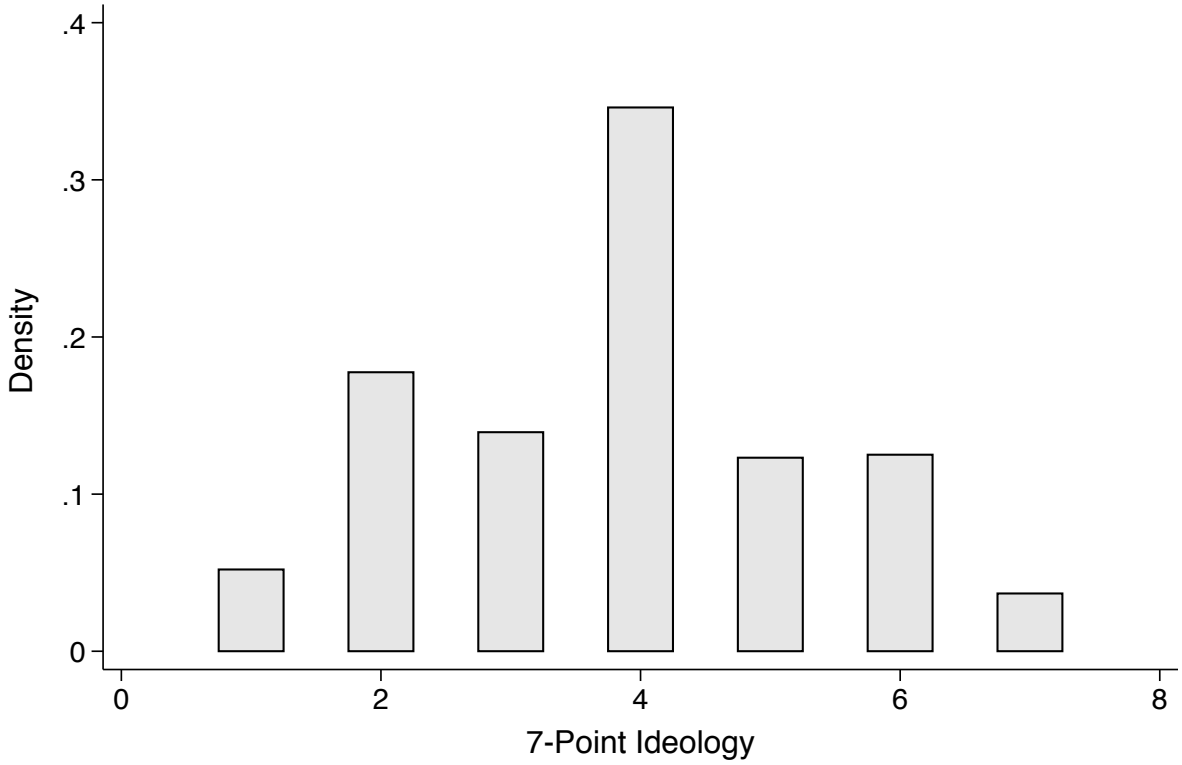
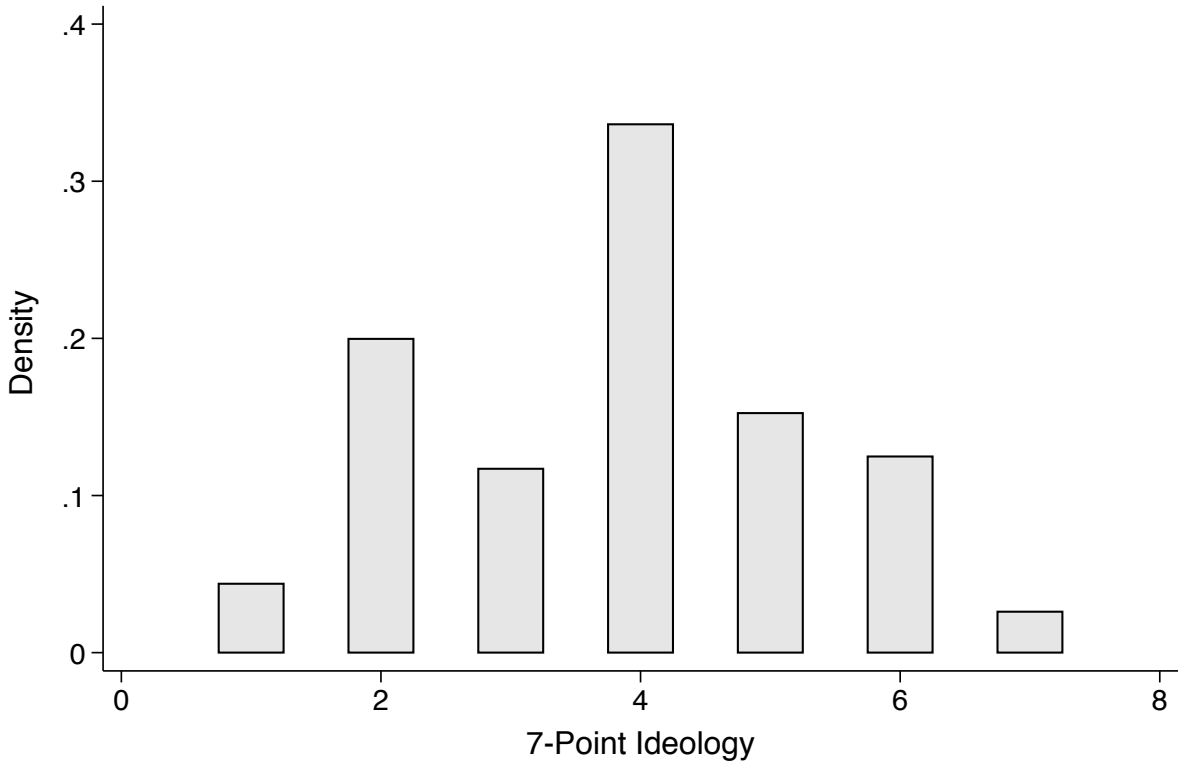


Figure 3.4: Ideological Self-Placement, California Respondents to the 2012 ANES



Sample weights used in the analysis. Results are nearly identical without weights.

Figure 3.5: Ideological Self-Placement, September 2012 Field Poll (RDD Probability Sample)

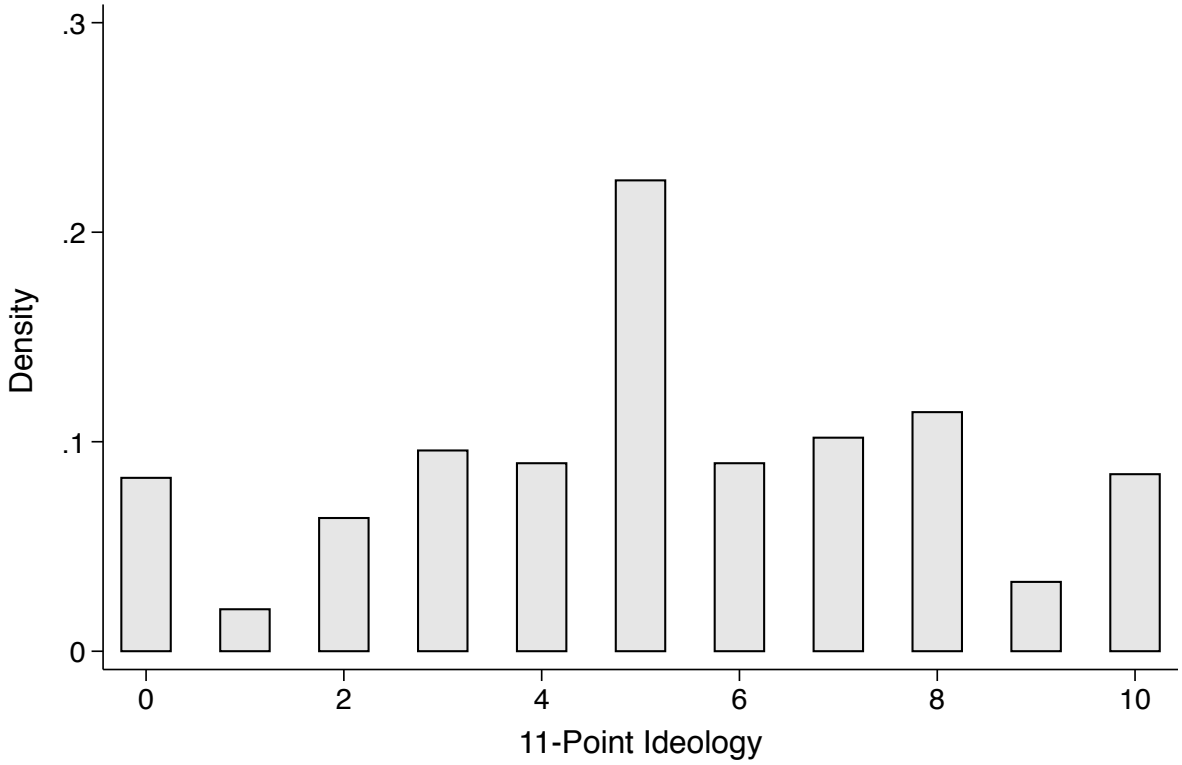
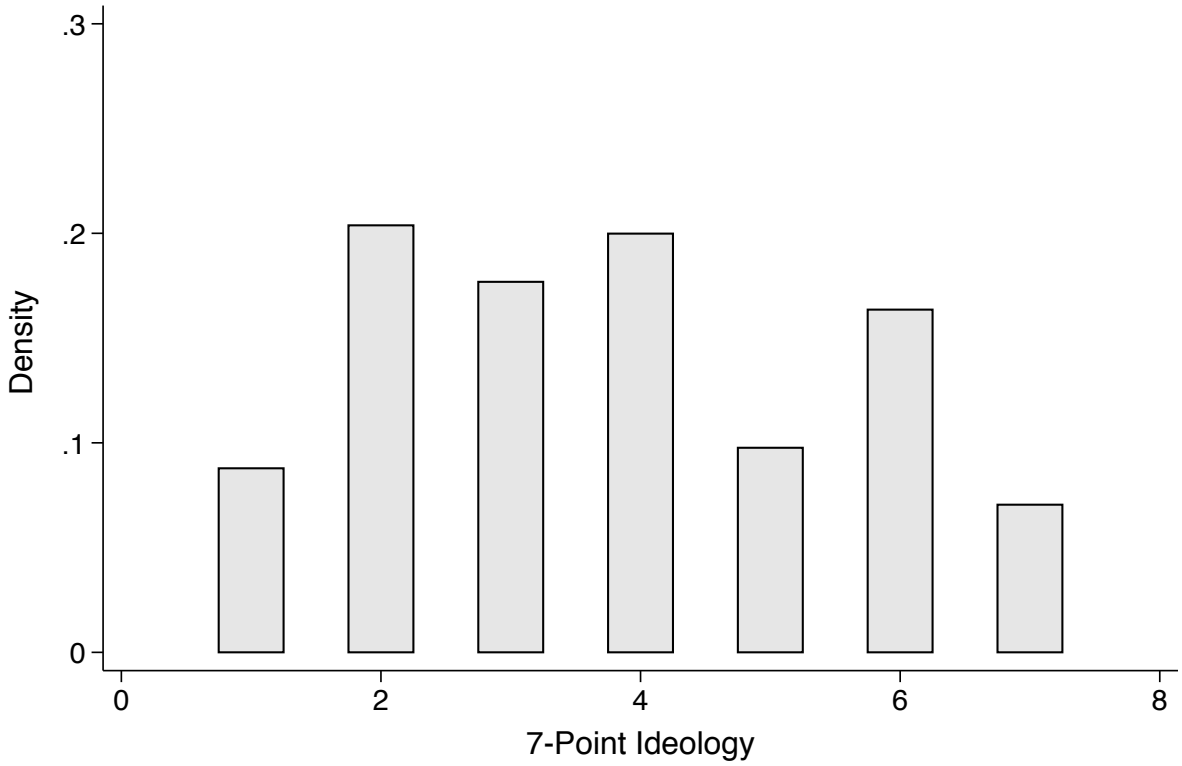
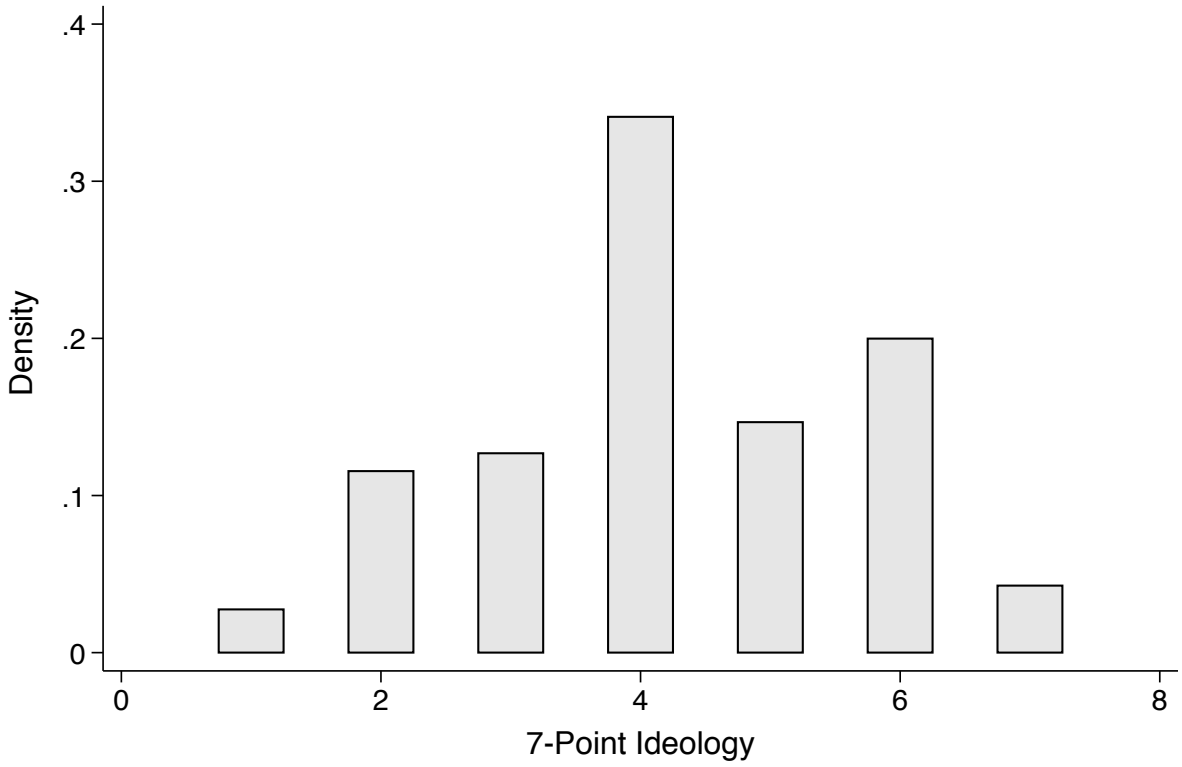


Figure 3.6: Ideological Self-Placement, 2012 California CCES Respondents



Sample weights used in the analysis. Density of strict moderates decreases while density of liberals and conservatives increases without weighting.

Figure 3.7: Ideological Self-Placement, 2012 ANES, Entire US Sample (for Comparison)



Sample weights used in the analysis. Results are nearly identical without weights

3.2 Description of Study 1 Policy Scales

In Study 1, participants were randomly assigned to either answer two 7-point policy scales or give their perceptions of how liberals and conservatives in California would respond to those two policy scales. The "role of government" slider was anchored by "The government should guarantee jobs and a standard of living" (1) and "The government should let each person get ahead on their own" (7) while the "environment-economy" slider was anchored by "Protect environment, even if it costs jobs and standard of living" (1) and "Jobs and standard of living are more important than the environment" (7).

3.3 Description of Study 2 Sample

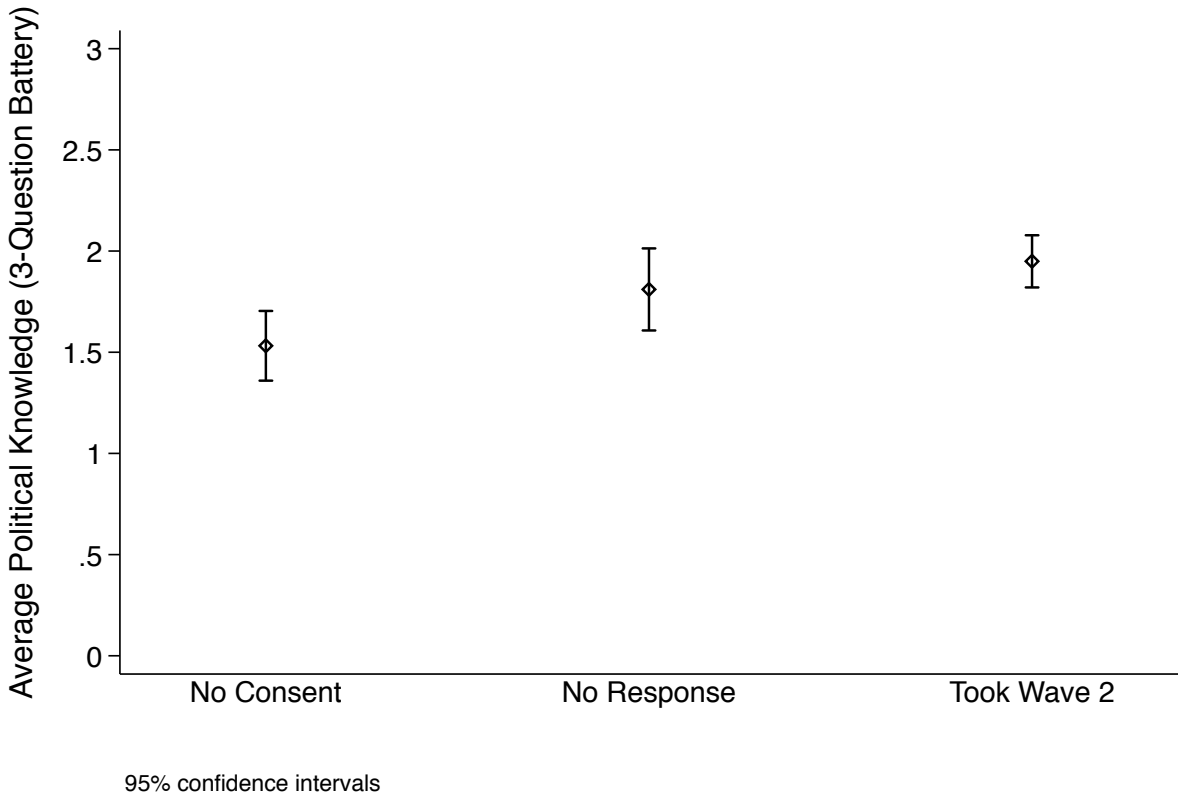
Berinsky, Huber, and Lenz (2012) present evidence that an MTurk sample is likely to be more representative of the population as a whole than a convenience sample. In terms of education, for example, the sample used in this study is obviously more similar to the nation as a whole than a sample of college sophomores would have been. Still, this is a convenience sample and does not represent any electorate in the US well. Presented below are details about the educational achievement and political leanings of the MTurk sample used in Study 2, compared to 2012 Census CPS data (for education) and 2012 ANES data (for politics).

This study made use of a two-wave survey by recontacting participants. Attrition occurs at a relatively high rate with Mechanical Turk panels, and it occurs two ways. First, recontacting subjects requires obtaining their consent in the first wave, and refusal to be recontacted accounted for the attrition of 26.4% of subjects from the first wave. Second, some subjects do not respond to the request to participate in the second wave. This happened with 33.0% of recontactees in this study, or 24.3% of all participants in the first wave. This loss of 50.7% of the initial subject pool suggests that researchers planning to use Mechanical

Table 3.2: Demographics of MTurk Sample for Study 2

	MTurk Sample (June 2012)	CPS (2012)	ANES (October 2012)
Education			
< High school diploma	0%	13%	
High school diploma/GED	11%	30%	
Some college/2-year degree	44%	29%	
4-year college degree	33%	18%	
Graduate/professional degree	12%	10%	
Partisanship			
Democratic (inc. leaners)	44%		51%
Republican (inc. leaners)	37%		37%
No party preference	14%		11%
Other	4%		1%
Ideology			
Liberal (inc. leaners)	66%		29%
Moderate	12%		29%
Conservative (inc. leaners)	22%		42%

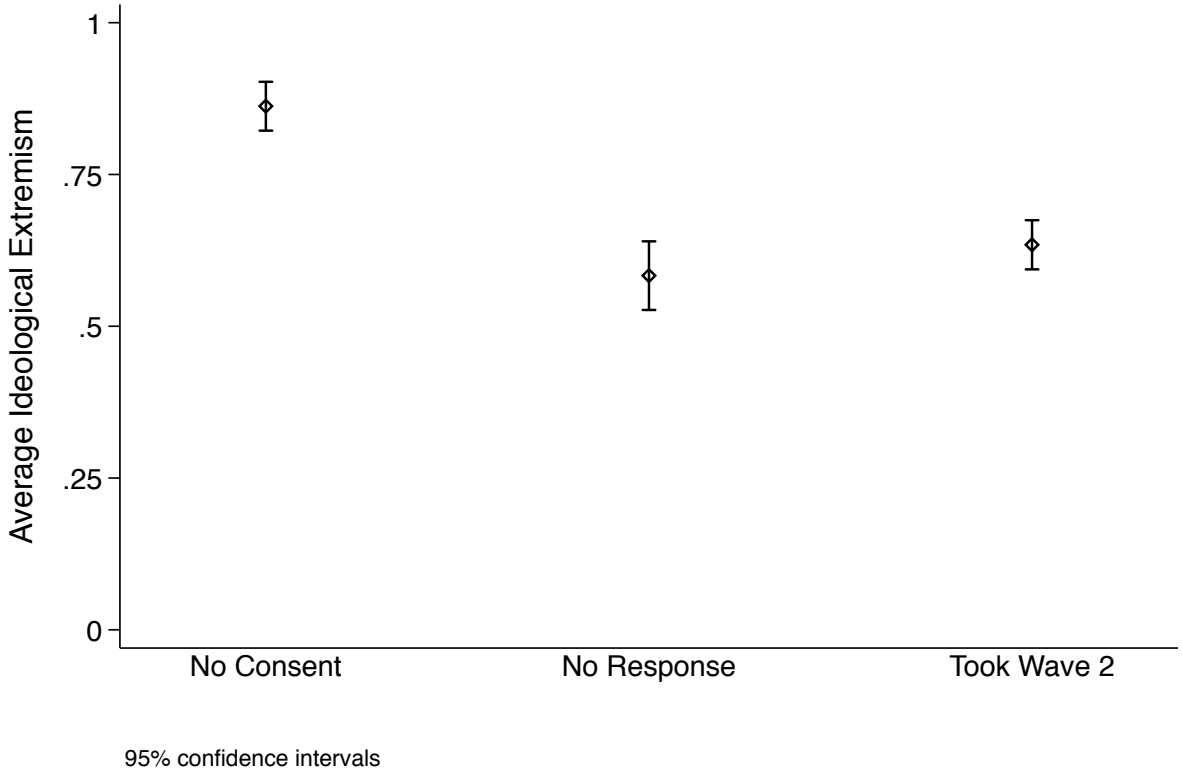
Figure 3.8: Wave 2 Takers and Non-Takers Compared on Knowledge



Turk to conduct a two-wave panel study should empanel Turkers liberally in the first wave.

Some may worry about nonresponse bias given the high rate of attrition. In particular, we might fear that only the most politically interested participants returned for the second wave, thus producing a sample that is more ideological, more plugged in to political media, and more likely to overestimate extremism as a result. And, indeed, participants who took Wave 2 were more politically knowledgeable than those who attritted: On average, they correctly answered .28 more of the 3 knowledge questions than participants who dropped out after Wave 1 ($p < .01$). However, counter to concerns, they were also more moderate on average by .09 points on a folded ideology scale, rescaled to 0-1 from the folded 7-point scale ($p < .001$). Interestingly, both of these differences stem from differences among those participants who refused to consent to recontact. When we compare participants who took part in Wave 2 to those who were invited but did not respond, we observe no significant differences.

Figure 3.9: Wave 2 Takers and Non-Takers Compared on Extremism



3.4 Study 2: The Manipulation

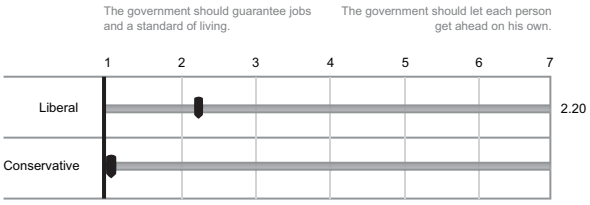
The image below depicts the manipulation screens for each of the three experimental conditions. The positions shown to participants in the "tell" and "distort" conditions were depicted as though they were actually on sliders to minimize compound treatment problems.

Figure 3.10: Example of Treatments: First Screen of Survey Experiment as Seen by Respondents Randomly Assigned to "Ask," "Tell," and "Distort"

ASK

Respondents to a recent national survey were asked the following question: "Some people feel the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of a scale, at point 1. Others think the government should just let each person get ahead on their own. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between, at points 2, 3, 4, 5, or 6. Where would you place yourself on this scale?"

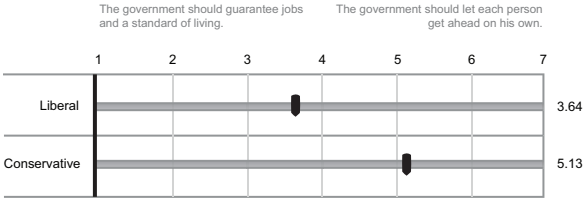
What do you think were the average positions taken by people who call themselves "liberal" and people who call themselves "conservative"?



TELL

Respondents to a recent national survey were asked the following question: "Some people feel the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of a scale, at point 1. Others think the government should just let each person get ahead on their own. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between, at points 2, 3, 4, 5, or 6. Where would you place yourself on this scale?"

The average positions taken by people who call themselves "liberal" and people who call themselves "conservative" are shown below:



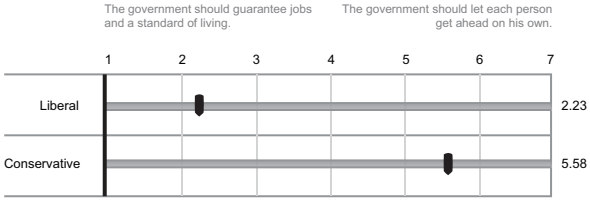
Do these locations suprise you?

- ○ Yes - at least one group is more moderate than I thought.
- ○ Yes - at least one group is more extreme than I thought.
- ○ Yes - one group is more moderate and the other is more extreme than I thought.
- ○ No.

DISTORT

Respondents to a recent national survey were asked the following question: "Some people feel the government in Washington should see to it that every person has a job and a good standard of living. Suppose these people are at one end of a scale, at point 1. Others think the government should just let each person get ahead on their own. Suppose these people are at the other end, at point 7. And, of course, some other people have opinions somewhere in between, at points 2, 3, 4, 5, or 6. Where would you place yourself on this scale?"

The average positions taken by people who call themselves "liberal" and people who call themselves "conservative" are shown below:



Do these locations suprise you?

- ○ Yes - at least one group is more moderate than I thought.
- ○ Yes - at least one group is more extreme than I thought.
- ○ Yes - one group is more moderate and the other is more extreme than I thought.
- ○ No.

3.5 Study 2: Table Corresponding to Figure 2

Figure 2 in the paper presents 95% confidence intervals for mean responses to the "ask" condition's questions about liberals and conservatives. These confidence intervals show that, on average, respondents' perceptions of liberals and conservatives deviated significantly from the truth (operationalized by responses to the 2008 ANES, also shown to respondents assigned to the "tell" condition), but not from the positions held by the most extreme liberal and conservative identifiers (operationalized by responses to the 2008 ANES by 1s abd 7s on the 7-point ideology scale, also shown to respondents assigned to the "distort" condition). The table below presents the same information, but as point estimates with standard errors reported rather than 95% confidence intervals.

Table 3.3: Perceptions of Policy-Related Predispositions (Using 7-Point Issue Scales) of Liberals and Conservatives, as Estimated by Subjects in "Ask" and Compared to Information Given in "Tell" and "Distort"

Role of Government	Ask	Tell	Distort
	Mean perception	From 2008 ANES	From 2008 ANES
Liberal position/estimate	2.51	3.64	2.23
Conservative position/estimate	5.82	5.13	5.58
Ask vs. Tell t-test (lib.)	$t = -6.94$	$p < .001$	
Ask vs. Distort t-test (lib.)	$t = 1.75$	$p = .08$	
Ask vs. Tell t-test (con.)	$t = 4.35$	$p < .001$	
Ask vs. Distort t-test (con.)	$t = 1.50$	$p = .14$	
Universal Healthcare	Ask	Tell	Distort
Liberal position/estimate	1.94	2.58	2.04
Conservative position/estimate	5.81	4.88	5.46
Ask vs. Tell t-test (lib.)	$t = -5.20$	$p < .001$	
Ask vs. Distort t-test (lib.)	$t = -0.95$	$p = .35$	
Ask vs. Tell t-test (con.)	$t = 5.49$	$p < .001$	
Ask vs. Distort t-test (con.)	$t = 2.07$	$p = .04$	
Environmentalism	Ask	Tell	Distort
Liberal position/estimate	2.56	3.24	2.61
Conservative position/estimate	5.78	4.37	5.64
Ask vs. Tell t-test (lib.)	$t = -4.88$	$p < .001$	
Ask vs. Distort t-test (lib.)	$t = -0.38$	$p = .70$	
Ask vs. Tell t-test (con.)	$t = 11.24$	$p < .001$	
Ask vs. Distort t-test (con.)	$t = 1.12$	$p = .26$	

T-tests are two-sided, one-sample tests. For "ask vs. tell," they test the null hypothesis that the average estimates from "ask" are equal to the positions reported in "tell"—the true average positions of liberals and conservatives on these policy dimensions. For "ask vs. distort," the test the null hypothesis that the estimates from "ask" are equal to the positions reported in "distort"—the positions held by the most extreme liberals and conservatives.

3.6 Study 2: Individual Policy Opinion Questions

Question text and response options

These six policy opinion questions were presented to participants in random order and used to construct the dependent variable in Study 2:

1. Emissions trading, also called "cap-and-trade," is a proposed environmental policy in which the government sets a maximum limit on certain kinds of pollution. The right to emit these pollutants is sold in the form of a permit by the government, and permits can be traded among companies. Would you favor or oppose cap-and-trade legislation in the United States?

    - Strongly favor
    - Moderately favor
    - Neither favor nor oppose
    - Moderately oppose
    - Strongly oppose

2. You may have heard about the controversy surrounding the proposed Keystone XL pipeline, which would be used to transport synthetic crude oil from Canada to multiple destinations in the U.S. if constructed. Some have argued that the pipeline would be highly harmful to the environment, while others have argued that it would be a major step forward for economic development and energy independence in North America. Would you favor or oppose the development of the Keystone XL pipeline?

    - Strongly favor
    - Moderately favor
    - Neither favor nor oppose
    - Moderately oppose
    - Strongly oppose

3. Would you favor or oppose a plan to change the Social Security system that includes gradually reducing the amount of money that people receive as their guaranteed Social Security benefit in exchange for allowing workers to invest some of their Social Security taxes in the stock market?

    - Strongly favor
    - Moderately favor
    - Neither favor nor oppose
    - Moderately oppose

- Strongly oppose

4. The national debt has been a major concern for many people in the past year. As you may know, Congress can reduce the federal budget deficit by cutting spending, raising taxes, or a combination of the two. Ideally, how would you prefer to see Congress attempt to reduce the federal budget deficit?

    - Only with spending cuts
    - Mostly with spending cuts
    - Equally with spending cuts and tax increases
    - Mostly with tax increases
    - Only with tax increases

5. Turning to the health care law passed last year, what is your opinion of the law?

    - It should be repealed
    - It may need small modifications, but we should see how it works.
    - It should be left alone

6. You may remember the $800 billion economic stimulus package passed in 2009, passed as an attempt to save and create jobs during the worst part of the recession. In retrospect, do you approve or disapprove of that legislation?

    - Approve strongly
    - Approve somewhat
    - Neither approve nor disapprove
    - Disapprove somewhat
    - Disapprove strongly

Index Construction

The index constructed from these six items yields a Cronbach's alpha reliability coefficient of $\alpha = 0.69$. While this is relatively low by conventional standards, it is surprisingly high given what we know about survey response instability (Zaller 1992). Further, since we are constructing an index of overall political opinion across multiple domains (environment and role of government), we should not expect alpha to reach the usual benchmark of 0.8, given citizens' relatively low level of ideological constraint, which holds true among Mechanical Turk workers (Converse 1964; 2000; Broockman 2013).

The Effect of the Manipulations on the Extremity of Responses to the Individual Items

As stated in the paper, participants in the "tell" treatment tended to report individual policy opinions that were more moderate than those reported by participants in the other two treatments. While these apparent effects are in the expected direction, they also tended not to reach statistical significance. A possible explanation is that many considerations, and not just the information presented in the manipulation, factor into an individual's response to a survey question, so these apparent effects are necessarily noisy. These analyses are presented below, in SI Table 1.

Table 3.4: Effect of Treatments on Individual Policy Responses

(a) Regression of "Distance of Opinion on Healthcare Reform from Neutral Midpoint" on Treatment Condition"

	Coefficient	Std. Error	t	$p > \|t\|$
Ask	.137	.115	1.19	.236
Distort	-.115	.117	-0.98	.329
Constant	.333	.082	4.07	.000

(b) Regression of "Distance of Opinion on Social Security Privatization from Neutral Midpoint" on Treatment Condition"

	Coefficient	Std. Error	t	$p > \|t\|$
Ask	.147	.083	1.78	.078
Distort	.094	.084	1.12	.267
Constant	.500	.059	8.49	.000

(c) Regression of "Distance of Opinion on the 2009 Stimulus from Neutral Midpoint" on Treatment Condition"

	Coefficient	Std. Error	t	$p > \|t\|$
Ask	.015	.079	0.19	.849
Distort	.125	.081	1.55	.125
Constant	.485	.056	8.58	.000

(d) Regression of "Distance of Opinion on Deficit Reduction Strategies from Neutral Midpoint" on Treatment Condition"

	Coefficient	Std. Error	t	$p > \|t\|$
Ask	.109	.077	1.42	.159
Distort	.103	.077	1.33	.187
Constant	.303	.055	5.56	.000

(e) Regression of "Distance of Opinion on Emissions Trading from Neutral Midpoint" on Treatment Condition"

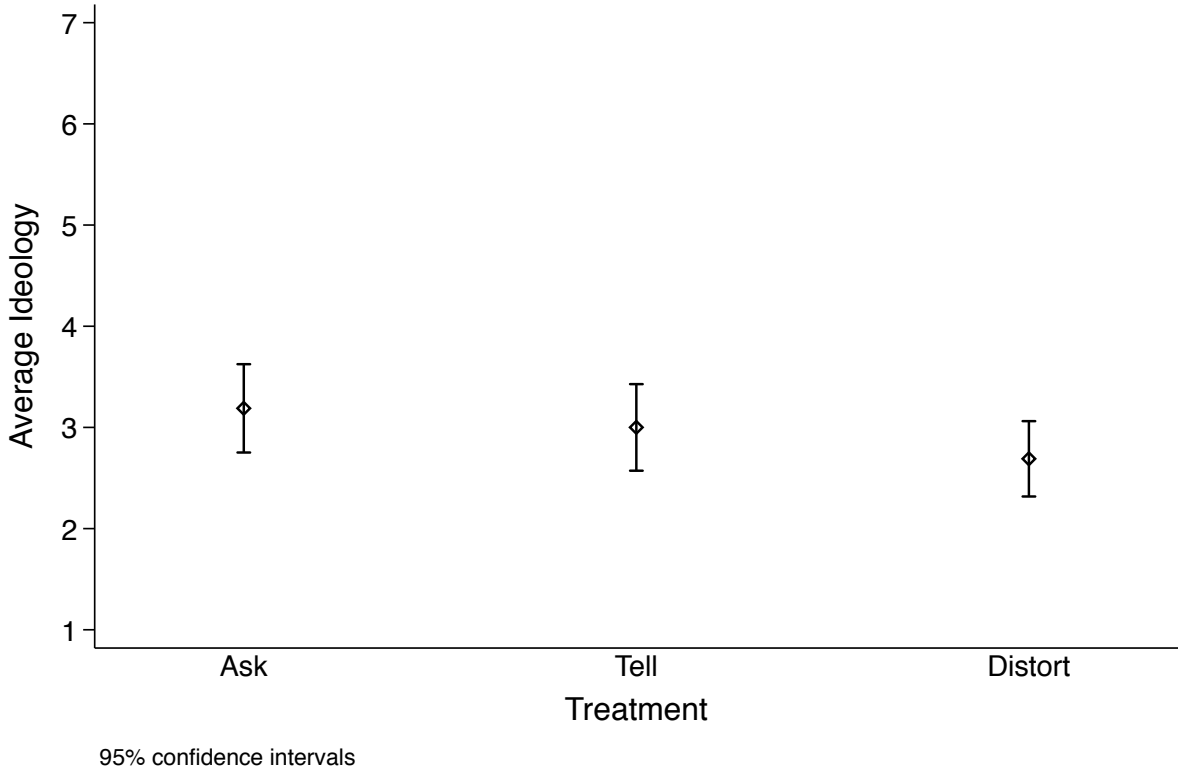
	Coefficient	Std. Error	t	$p > \|t\|$
Ask	-.091	.077	-1.18	.242
Distort	.018	.078	0.24	.814
Constant	.591	.055	10.74	.000

(f) Regression of "Distance of Opinion on the KeystoneXL Pipeline from Neutral Midpoint" on Treatment Condition"

	Coefficient	Std. Error	t	$p > \|t\|$
Ask	.165	.088	1.87	.065
Distort	.122	.090	1.36	.177
Constant	.394	.063	6.27	.000

"Tell" serves as the baseline condition, meaning that the positive coefficients for "Ask" and "Distort" are consistent with the study's hypotheses. "Extremity of opinion" is scaled from 0 to 1. $n = 99$.

Figure 3.11: Randomization Check: 7-Point Ideology



3.7 Study 2: Randomization Checks

A potential concern is that the randomly constructed groups in Study 2 could lack balance on key covariates, and that such imbalance could bias results. For example, if the "tell" group were more moderate, on average, than the other groups, we might worry that this characteristic, rather than the treatment, would lead the group to exhibit more moderate policy opinions. To allay concerns like this, I present randomization checks on five key covariates: ideological 7-point placement, ideological extremity, 7-point party identification, strength of party identification, and political knowledge. As we would expect given the number of randomization checks conducted, one significant difference emerges at the level of p ¡ 0.10: the "distort" group is significantly more liberal than the "ask" group. Fortunately, overall extremity is not significantly different between groups, and if anything, the "tell" group is *more* extreme initially than the "ask" group, which should bias the results against Hypothesis 2.

Figure 3.12: Randomization Check: Ideological Extremity

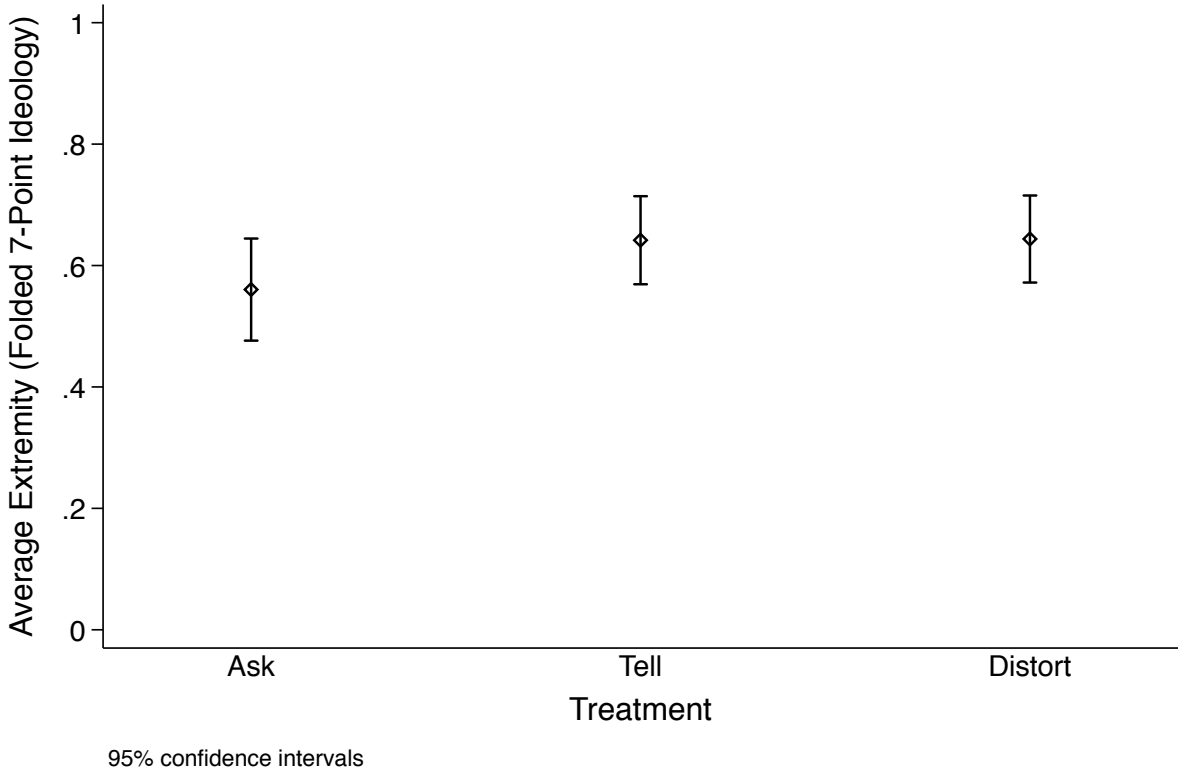


Figure 3.13: Randomization Check: 7-Point Party ID

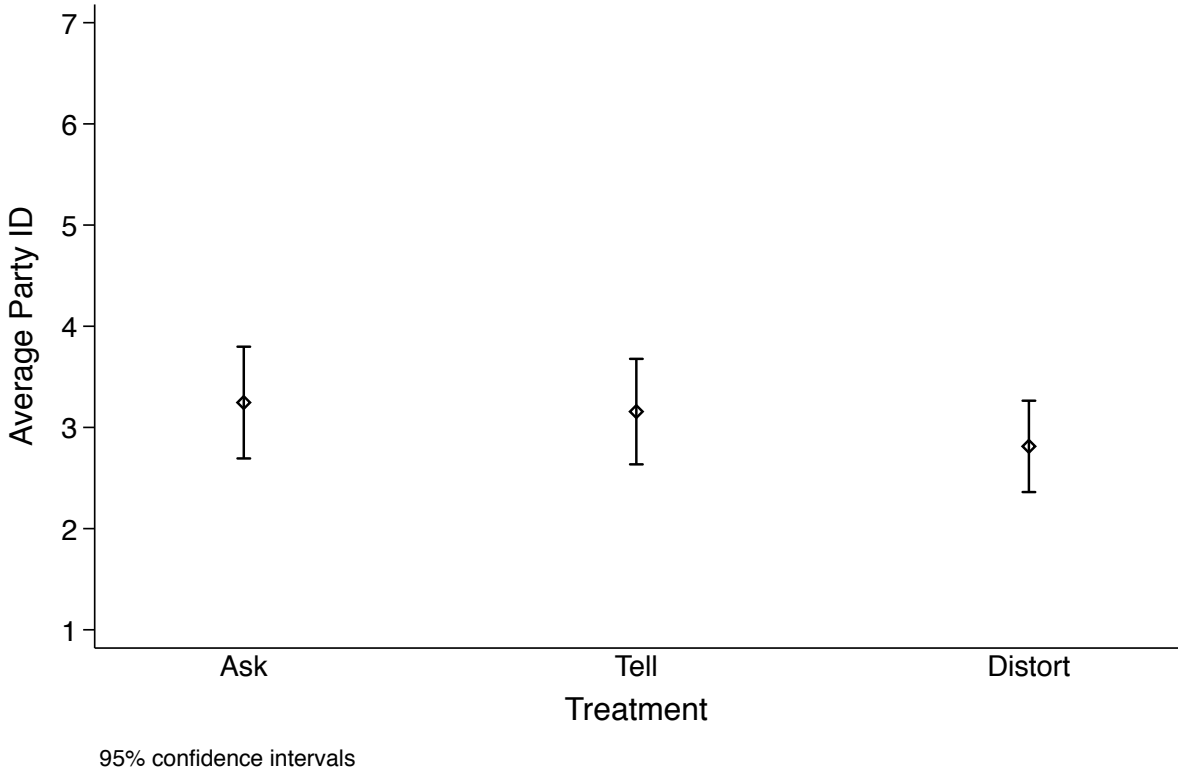


Figure 3.14: Randomization Check: Strength of Party ID

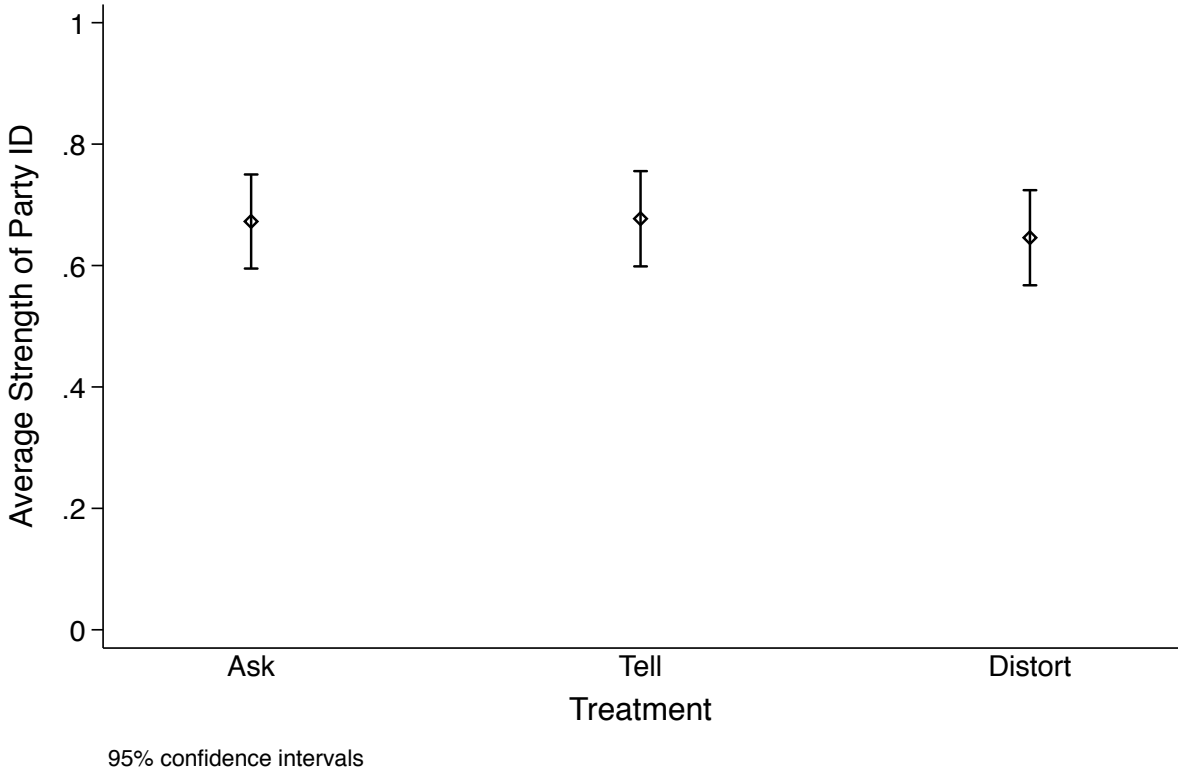
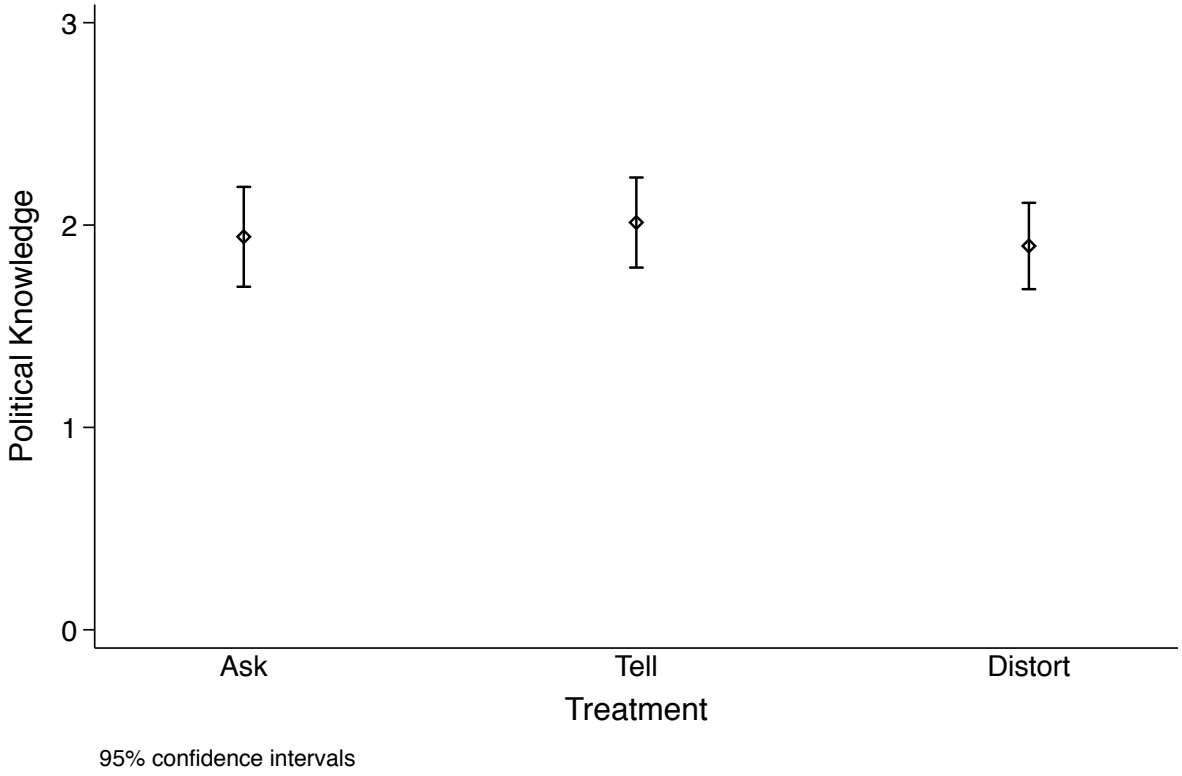


Figure 3.15: Randomization Check: Political Knowledge



3.8 Study 2: Manipulation Check

As discussed in the paper, each of the issue scales presented in the "tell" and "distort" manipulations also included a question asking participants whether they were surprised by the positions they were shown, and if so, the reason for their surprise. There were three possible reasons for surprise: at least one group was more moderate than expected, at least one group was more extreme than expected, or one group was more moderate and the other more extreme than expected.[3] Given the hypothesis that participants overestimate the extremity of liberals' and conservatives' average positions—a hypothesis confirmed from data gathered in the "ask" condition—we should expect that participants in the "tell" condition would be surprised by the relative moderateness of the positions they were shown, while subjects in the "distort" condition would be unsurprised by the positions they were shown. Some participants assigned to the "distort" condition may have also been genuinely surprised by the extremity of the positions they saw, since the mean estimated positions in "ask" were, for the most part, marginally more moderate than those reported to participants in "distort." Therefore, we consider reporting either no surprise, or surprise at both groups' extremity, as successful manipulation in the "distort" condition.

We would also expect that if the manipulations failed, subjects would, at best, randomly choose from the four response options provided for the "surprise" questions. In this case, we would expect that subjects would select the option indicating proper manipulation 25% of the time in the "tell" condition and 50% of the time in the "distort" condition. From these expectations, we can perform t-tests for each of the six individual manipulations from the "tell" and "distort" conditions (two conditions times three sets of issue scale locations). As shown in Table 4, which presents the results of these t-tests, we can be relatively certain that the manipulation worked in five of the six cases, as the mean rate of "proper" responses to the "surprise" question is significantly greater than we would expect if respondents were randomly choosing responses.[4] The manipulation appears to have failed in the case of the healthcare issue scale in the "distort" condition. This is not surprising in light of the results from the "ask" condition showing that subjects estimated the public to be more polarized on healthcare than depicted by the issue scale used for the "distort" condition.

[3]This last option is always considered a failure to properly treat.

[4]Some may cast doubt on the effectiveness of the "tell" manipulation, as it only appears to have worked for 40-44% of subjects on any given issue dimension. These readers may take heart in the fact that 82.9% of subjects assigned to "tell" expressed the proper form of surprise toward at least one of the sets of positions they were shown.

Table 3.5: T-Tests to Determine the Effectiveness of "Tell" and "Distort" Manipulations

Manipulation	H_0	Proportion Successfully Manipulated	t	p
Tell: Role of Government	$\mu = 0.25$	0.40	2.72	.01
Tell: Healthcare	$\mu = 0.25$	0.41	2.93	$< .001$
Tell: Environment	$\mu = 0.25$	0.44	3.36	.001
Distort: Role of Government	$\mu = 0.5$	0.70	4.08	$< .001$
Distort: Healthcare	$\mu = 0.5$	0.56	1.18	.24
Distort: Environment	$\mu = 0.5$	0.69	3.80	$< .001$

The null hypotheses capture the idea that respondents were randomly choosing from the response options for the manipulation check question. One of the four responses in the "tell" condition corresponds with successful manipulation, while two of the four responses in the "distort" condition correspond with successful manipulation. (See footnote 7.)

3.9 Study 2: Regression Table Corresponding to Figure 3

Table 3.6: Regression of Mean Extremity of Reported Policy Opinions on Treatment Condition

(a) With "ask" as baseline condition

	Coefficient	Std. Error	t	$p > \|t\|$
Tell	-.080	.047	-1.71	.090
Distort	.032	.048	0.68	.498
Constant	.515	.033	15.64	.000

(b) With "distort" as baseline condition

	Coefficient	Std. Error	t	$p > \|t\|$
Tell	-.113	.047	-2.36	.020
Ask	-.032	.048	-0.68	.498
Constant	.547	.034	16.12	.000

$n = 99$

3.10 Study 2: Full Accounting of Instrumental Variables Analysis

As detailed in the paper, one might be interested in the effect of uptake of the "tell" condition rather than an average treatment effect of being assigned to the "tell" condition. While the former addresses the actual effect of change in perceptions of where liberals and conservatives stand on extremity of opinion, the ATE estimand compares *all* individuals assigned to "tell" to those assigned to "ask," regardless of whether their perceptions changed. A natural instinct is to drop all participants who fail the manipulation check in the "tell" condition and compare them to the "ask" condition, but this breaks the expectation of equivalence between the two groups that we gain from a randomized, controlled experiment, and likely introduces selection bias. Instead, however, because we can estimate a causal average causal effect ($CACE$) with an instrumental variables approach.

This holds true for all cases of one-way noncompliance. In the context of survey experiments, one-way noncompliance means participants assigned to treatment reject the manipulation, and there is no way for participants in control to take the manipulation. The "ask" and "tell" conditions in this experiment fit this description. In this case, we are interested in the effect of the treatment itself (uptake of the information in "tell"), rather than assignment to treatment, on the outcome. Since only those assigned to treatment ($T_i = 1$) have access to it, T is randomly assigned, and any effect that T has on outcomes is through the treatment because $T_i \perp Y_{i1}, Y_{i0}$, T_i (the participant's treatment assignment) can be used as a clean instrument for the effect of treatment uptake, which we'll call C_i. According to Bloom (1984), we can use this instrument to estimate a local average treatment effect on the treated:

$$\hat{CACE} = E(Y_{i1} - Y_{i0}|C_i = 1) = \frac{E(Y_i|T_i = 1) - E(Y_i|T_i = 0)}{Pr(C_i = 1|T_i = 1)} \tag{3.1}$$

That is, with one-way noncompliance, we can estimate the local average treatment effect for compliers by dividing the ITT estimator by the rate of compliance. We can test for the significance of effects with a Wald test.

The first- and second-stage regressions used for the paper are presented in SI Table 7. "Ask" serves as the baseline condition, meaning that the negative coefficient for compliance with the "Tell" condition is consistent with the study's hypotheses. "Extremity of opinion" is scaled from 0 to 1. $n = 67$.

Table 3.7: 2-Stage Least Squares Regression of Uptake of the "Tell" Manipulation (Instrumented with Assignment to "Tell") on Extremity of Reported Opinions

(a) First-Stage Regression: Treatment Assignment on Treatment Uptake

	Coefficient	Std. Error	t	$p > \|t\|$
Assignment to "Tell"	.636	.084	7.60	.000

(b) Second-Stage Regression: Treatment Uptake on Extremity of Opinion (Complier Average Causal Effect)

	Coefficient	Std. Error	z	$p > \|z\|$
Treatment Uptake	-.126	.072	-1.75	.079
Constant	.515	.032	16.01	.000

Chapter 4

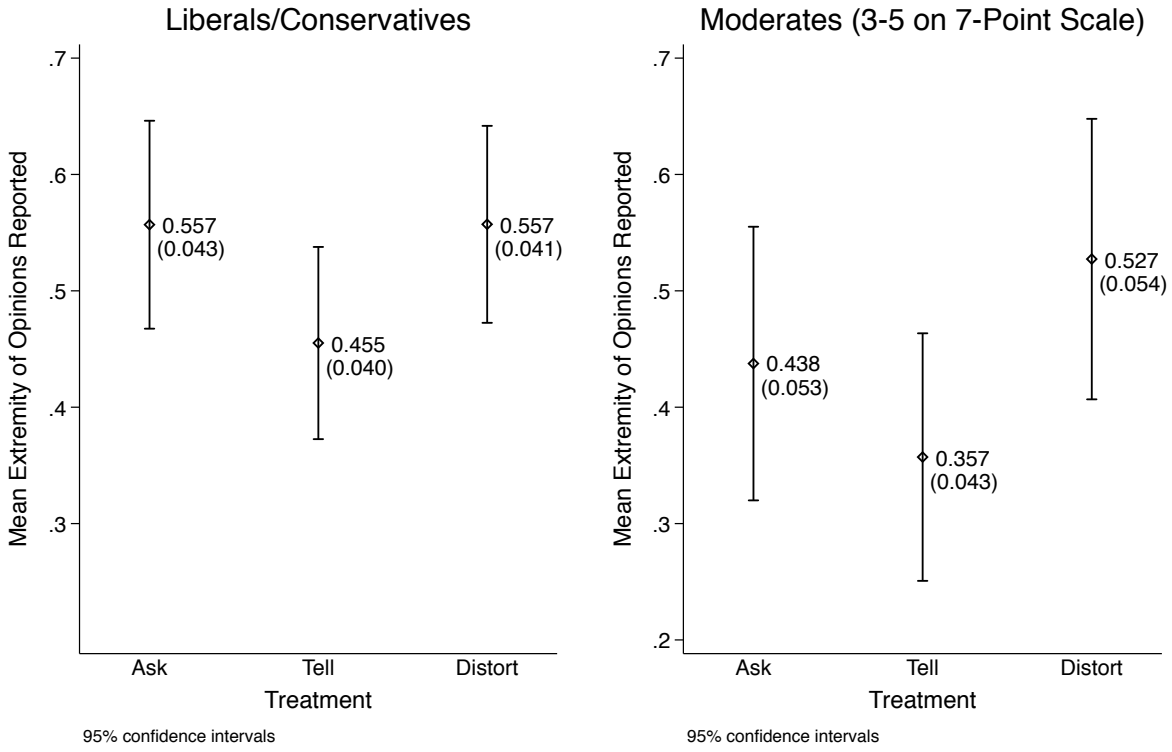
Are Perceptions of Mass Polarization Self-Fulfilling? A Brief Research Note on Paper 1

Following the publication of "Self-Fulfilling Misperceptions of Public Polarization," Levendusky and Malhotra (2016) documented the "polarization narrative" more systematically than any previous study. Their content analysis, including hundreds of news articles from the 21st century, demonstrates that media's discussion of mass polarization has increased markedly since 2000 and that this narrative emphasizes polarization on a range of issues, as well as affective polarization between rank-and-file Democrats and Republicans.

Levendusky and Malhotra (2016) also conduct an experiment similar to the one in "Self-Fulfilling Misperceptions," in which they randomly assign survey respondents to read one of two vignettes about political conflict, crafted to appear like newspaper articles. These vignettes aim to manipulate respondents' sense of mass polarization, with one vignette presenting Democratic and Republican citizens expressing extreme opinions in an uncivil way (the "polarization condition") and the other presenting the same citizens expressing moderate opinions in a civil way. Contrary to Ahler (2014), Levendusky and Malhotra (2016) find that the polarization condition leads respondents to become more *moderate* in the positions they take. Levendusky and Malhotra (2016) provide two possible explanations for the discrepancy. I address each of these in turn.

First, Ahler (2014) relies on a sample of Mechanical Turk workers rather than a representative sample of Americans. Levendusky and Malhotra (2016) suggest that the opt-in nature of the sampling may have yielded a sample that is more extreme and interested in politics than the typical American, and that the experimental results thus lack external validity. This explanation fails. As Druckman and Kam (2012) note, perfect correspondence on observable covariates is not necessary for experimental results to generalize. Rather, *variation* on critical covariates is necessary. With such variation, one can assess the degree to which those covariates moderate the treatment effect—as Levendusky and Malhotra (2016)

Figure 4.1: Self-Fulfilling Misperceptions Across Ideological Groups



suspect ideological centrism does for the treatment effect in Ahler (2014). Figure 4.1 plots the treatment effect separately for ideologues and self-identified moderates. Moderates and strong ideologues experience similar treatment effects, contrary to Levendusky and Malhotra's 2016 supposition. Thus, the descrepancy between the two studies is very unlikely to reflect an external validity concern related to subjects.

Second, Levendusky and Malhotra (2016) assert: "Ahler (2014) presented information about polarization in a direct, quantitative manner, whereas we embedded exemplification within a newspaper article. Consequently, it is possible that he is observing demand effects, or respondents seeking to conform to information provided directly by the researcher." Unfortunately, I lack direct evidence to assess this criticism. However, I would note that the approach in Ahler (2014) precludes ambiguous interpretation of treatment effects, a problem facing Levendusky and Malhotra (2016), whose vignettes presented a *compound treatment* (Campbell and Stanley, 1963) manipulating perceptions of extremity and perceptions of incivility concurrently.

As such, I offer a third explanation for the divergent results: the treatments are fundamentally different. Levendusky and Malhotra (2016) present a treatment that primes respondents to think about multiple facets of mass polarization in a manner consistent with

the mass media's "polarization narrative." By contrast, Ahler (2014) estimates the effect of the erroneous beliefs people generally hold about others' opinions, and it does so by *clearing up* those misperceptions. The incivility coupled with extreme views in Levendusky and Malhotra's 2016 "polarization" treatment may lead participants to moderate their own views so as to distance themselves from unpleasant people—just as citizens increasingly eschew party labels out of disdain for incivility and conflict (Klar and Krupnikov, 2016). However, absent such information about incivility, people's beliefs about others' opinions may lead them to adopt the opinions that they think characterize "their side," and thus, to be more extreme than they otherwise are.

These discrepant results suggest a promising avenue for future research. By manipulating perceptions of affective polarization and ideological polarization independently of each other, we may learn more about the distinct effects of these two types of perceptions on actual mass polarization.

Chapter 5

The Parties in Our Heads: Misperceptions About Party Composition and Their Consequences

Partisanship is arguably the most fundamental identity in American political life. It exerts a strong influence on voters' choices (e.g., Ansolabehere, Rodden and Snyder, 2008; Bartels, 2000) while remaining remarkably stable itself (e.g., Schickler and Green, 1997; Johnston, 2006). Partisanship colors how citizens process political communications and even factual information (Bartels, 2002; Druckman, Peterson and Slothuus, 2013; Lodge and Taber, 2013). And socially, partisans dislike and distrust the out-party and its supporters (Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2014).

What fuels such intense partisanship? Some (e.g., Achen, 2002) suggest that it could be a natural consequence of rational considerations. Others contend partisanship is a social identity (Campbell et al., 1960; Huddy, Mason and Aaroe, 2015; Iyengar, Sood and Lelkes, 2012). Still others think its power stems from persistent disagreement about policy and values—though surprisingly, partisans who espouse moderate issue positions hold about as much ill-will toward the out-party as those who hold more extreme views (Mason, 2015). Finally, people's tendency to think about parties in terms of other, longer-standing groups may explain intense partisanship at the mass level (Berelson, Lazarsfeld and McPhee, 1954; Campbell et al., 1960; Converse, 1964; Gerber and Green, 1998; Green, Palmquist and Schickler, 2002; Hetherington and Weiler, 2009). According to this view, people primarily conceive of the parties as *sociopolitical brands*, and how they feel about the parties' constituent groups affects their feelings toward the parties themselves (Hetherington and Weiler 2009, ch. 9; Mason and Davis 2015; Green, Palmquist and Schickler 2002; cf. Abramowitz and Saunders 2006).

With roughly a third of partisans today describing the other side as "a threat to the nation's well-being" (Pew 2014)—and nearly as many aghast at the idea of an out-party supporter marrying into their family (Iyengar, Sood and Lelkes, 2012)—intense partisanship strains social civility and trust. To curb these effects, we must carefully consider the compet-

ing explanations for partisan entrenchment and develop interventions based on those which hold water.

This paper evaluates the claim that people's beliefs about who Democrats and Republicans tend to be drive their feelings toward the parties.[1] A challenge facing this account is that the parties look quite alike at the mass-level: Democratic and Republican supporters overwhelmingly are white, middle-class, middle-aged, heterosexual, and mainline Protestant.[2] Given the social similarity between the mass-level parties, how can party composition explain the power of partisanship?

The answer, as we discover, lies not in the actual composition of the parties, but in how people perceive the composition of the parties. We find that people make large, systematic errors when thinking about party composition, considerably exaggerating the extent to which partisans belong to party-stereotypical groups. For instance, Americans appear to believe that 32% of Democrats are gay, lesbian, or bisexual (compared to 6.3% in reality), and that 38% of Republicans earn over $250,000 per year (just 2.2% in reality). After documenting this perceptual phenomenon and validating our measures, we show that these misperceptions influence partisans' beliefs about and feelings toward the parties. Across multiple experiments, partisans who receive accurate information about their out-party's composition rate that party's supporters as less extreme on policy and feel less affectively polarized. Importantly, we find similar associations in original observational data. Thus, citizens' beliefs about who Democrats and Republicans tend to be affect their feelings toward the parties, and the systematic inaccuracy in these beliefs contributes to intense partisanship. However, the experimental results also demonstrate that challenging people's partisan prototypes may remedy dislike and distrust across party lines.

Parties as Sociopolitical Brands

According to a dominant perspective on partisanship, people think about political parties primarily in terms of other long-standing groups. These group-party associations are relatively ubiquitous and temporally stable. Democrats, Republicans, and independents tend to hold similar beliefs about the types of people who identify with the two major parties, and these beliefs tend to endure in the aggregate. For example, perceptual associations between the working class and Democrats and the wealthy and Republicans have appeared in survey data for nearly a century (Green, Palmquist and Schickler, 2002).

We conjecture that associations like these reflect a tendency to think about parties in terms of prototypes—abstract composites of sociopolitical characteristics associated with the party. For instance, when people think about Republicans, they may conjure a wealthy businessman or a southern evangelical Christian. Prototypes like this are helpful. They serve as heuristics for comprehending groups and determining where one sits in relation to

[1] This paper is part of a joint project with Gaurav Sood.

[2] According to data from the 2012 American National Election Study (ANES). See Section SI 6.1 for further details.

them (Lippman, 1922; Mutz, 1998; Turner et al., 1987)—essentially functioning as "brands" to distinguish social groups. Green, Palmquist and Schickler (2002, p. 8) thus theorize that when Americans consider the two parties, they ask: "What kinds of social groups come to mind as I think about Democrats, Republicans, and Independents? Which assemblage of groups (if any) best describes me?"

To best assist with such social classification, prototypes tend to reflect characteristics that most distinguish groups (Rosch and Mervis, 1975; Tajfel, 1959). That is, characteristic c is prototypical of group g if c strongly predicts membership in g rather than membership in $\neg g$. Characteristics that highlight distinctions between groups, however, need not be particularly common within group g.[3] As a consequence, people's perceptions of group composition are likely to be skewed when they think in terms of prototypes (Taylor, 1981).

More generally, this relates to people's tendency to rely on representativeness heuristics. People tend to focus primarily on similarities between objects and characteristics when assessing conditional probabilities under uncertainty, often ignoring other relevant information like base rates (Kahneman and Frederick, 2002). Thus, if people do primarily think about the parties in terms of other, more primal social groups, they are liable to overestimate the percentage of partisans belonging to groups they perceive as core to the party brand, especially when those groups that compose relatively small percentages of the population writ large.

How do people form beliefs about the parties' sociopolitical brands? Like all groups, political parties cannot be experienced first-hand—we cannot literally meet the party. Thus, how we perceive the parties–that is, what we learn about them—is neccessarily mediated. As Mutz (1998, p. 12) notes, "while (personal-level knowledge) comes to us primarily through personal experience, (societal-level knowledge) usually reaches us by means of abstracted discussions conveyed through impersonal channels." The most common of these interpersonal channels for politics continues to be the mass media (Gentzkow and Shapiro, 2011; Olmstead et al., 2013). Mass-media's role in popularizing certain images of parties potentially explains the ubiquity of partisan prototypes, and generates further testable hypotheses about people's perceptions of party composition.

For one, we conjecture that people more interested in politics will see the parties as more prototypical in their composition. As Taylor (1981) notes, using prototypes and other classification heuristics requires familiarity with relevant social categories. The most voracious news consumers—those most likely to know "which groups go with which parties"—are thus most liable to hold skewed perceptions about party composition. Like Luskin, Sood and Blank (2013) and Pasek, Sood and Krosnick (2015), we assert that those who pay the most attention to political media are not just most likely to be able to recall political facts but, perversely, also have the greatest potential to be misinformed in systematic ways.

[3]For example, black Americans overwhelmingly identify as Democrats and vote for Democratic candidates. Blackness thus discriminates strongly between Democrats and Republicans, and Americans tend to cite blacks and other racial minorities as the types of people who tend to be Democrats (Green, Palmquist and Schickler, 2002), despite just a quarter of Democrats being black.

Second, we hypothesize that partisans' judgments about the composition of the opposing party will be especially distorted. Because of partisan homophily, partisans are less likely to have personal information about the out-party (Halberstam and Knight, 2014; Mutz, 2006), rendering impersonal information—e.g., media portrayals of the parties—more meaningful. However, people also tend to rely on impersonal information when making judgments about all collectives, even when they have personal information (Mutz, 1998; Sears and Funk, 1990). As such, people's beliefs about the composition of both parties should exhibit prototype-bias, but this tendency should be pronounced when they consider the out-party.

Party Brands Bias Judgments about Party Composition

Research Design

In March 2015, we surveyed 1000 Americans through YouGov. (See the Supporting Information, section SI 6.1, for additional details on sampling, and SI 6.1 for comparisons to established population-based benchmarks, which are quite favorable.) For each of the two parties, we asked respondents to estimate the percentage of supporters belonging to four party-stereotypical groups (or holding party-prototypical characteristics). We relied on existing research to identify these eight salient group-party associations.[4] To make measurement tractable, we substitute vaguely defined groups with similar precisely defined groups. (E.g., we substitute "earning more than $250,000 per year"—a contemporary signpost for a high level of wealth in the United States—for "rich.")

We asked respondents to estimate the percentage of Democratic party supporters who are black, atheist or agnostic, union members, and gay, lesbian, or bisexual, and the percentage of Republican party supporters who are evangelical Christian, 65 or older, Southern, and earn over $250,000 per year.[5] Respondents typed their estimate, required to be between 0 and 100 to pass validation, in a box next to each group. We randomized the order of the Democratic and Republican batteries as well as the order of the questions within batteries.

We compare respondents' reported perceptions to the true prevalence of these groups in the parties, estimated from Pew's 2012 Religion & Public Life Project (for the two religious party-group dyads) and the 2012 American National Election Study. (See SI 6.1 for details.)

[4]The list is neither comprehensive nor systematic, but it covers prominent groups associated with the parties. This is adequate for the purposes of our study—to describe the degree to which certain prominent prototypes bias assessments of partisan composition, and what we may gain by clearing up such misperceptions. See SI section SI 6.1 for a brief literature review.

[5]For evidence that people interpreted the question as asking about the mass parties, see SI 6.1.

People Overestimate the Share of Party-Stereotypical Groups in Parties

People's perceptions of the share of party-stereotypical groups in the parties contain large, systematic errors. People considerably overestimate the share of each of the eight party-stereotypical groups in the respective party (see Figure 5.1 and Table 5.1). On average, respondents overestimated the share of these groups by 71.2% (95% confidence interval (CI): [70.1%, 73.3%]). Not only were misperceptions large, they were also extremely widespread. For all party-group dyads, a majority of respondents overestimated the group's share in the party, and for six of these eight dyads, over 70% of respondents did so (see SI 6.1).[6]

Looking separately at individual groups, little distinguishes misperceptions on old and new social cleavages. Respondents thought that 39.3% of Democrats belonged to a labor union—only 10.5% do. Even more erroneously, they estimated that 38.2% of Republicans earned over $250,000 per year when just 2.2% of GOP supporters do. But misperceptions were equally common on more recent cleavages. For instance, respondents thought that the share of atheists and agnostics among Democrats was more than four times greater than it actually is (28.7% vs. 6.2%). Similarly, though by a considerably less dramatic margin, respondents overestimated the share of evangelical Republicans by over 20%.

While all tend to overestimate the share of party-stereotypical groups in the parties, the extent to which people overestimate varies by partisanship. Comparing columns 4-5 of Table 5.1, we see that Republicans' perceptions of Democratic composition exhibit significantly more bias than do Democrats'.[7] For example, while Democrats overestimate the percentage of co-partisans belonging to a union by 25.2 percentage points, Republicans overestimate by an additional 8.3 points. Similarly, Democrats' perceptions of Republicans tend to be more error-prone.[8]

To formally test for differences between in- and out-party perceptions, we compared partisan respondents' mean error.[9] Democrats err about the degree to which the Democratic Party is composed of party-stereotypical group members by 31.8%, while Republicans err by 40.9%, a 9.2-point difference (95% CI: [5.9, 12.4]). Similarly, Democrats err about the degree to which the Republican Party is composed of party-stereotypical group members by

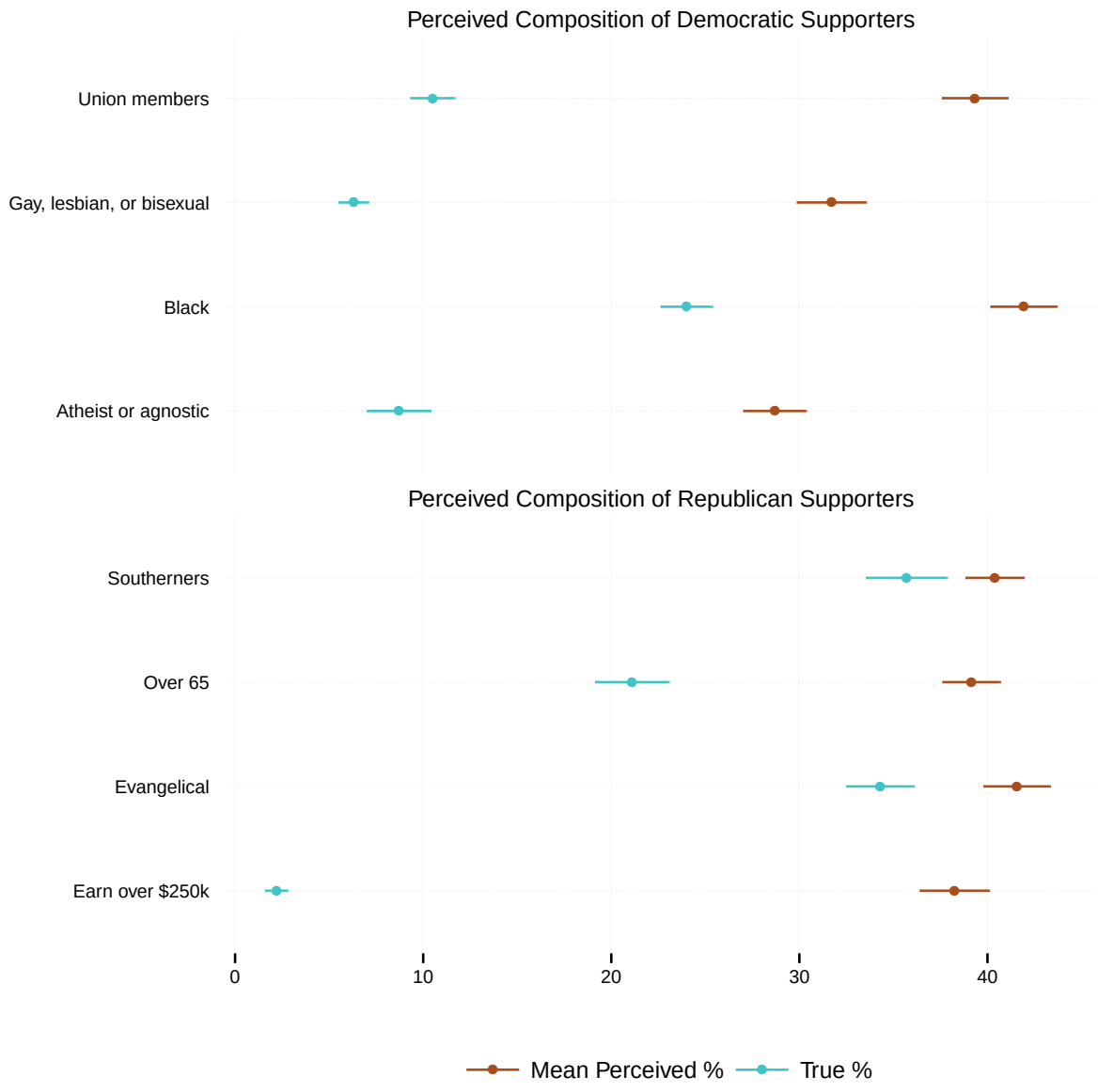
[6]We further demonstrate the robustness of our results to outliers in SI 6.1. Since medians are less sensitive to outliers than means, we compare median estimates to the truth (See SI 6.1). Median perceptions are generally lower than means, but only by a few percentage points. More pertinently, differences between median estimates and true proportions remain vast. We also show boxplots of the estimates, split by party, in SI 6.1.

[7]Independents tend to be about as accurate as in-party members, implying that out-party membership (rather than a mere lack of membership in the target party) explains this tendency. This comports with findings that independents are more likely to have bipartisan networks (e.g., Hui, 2013). See SI **??** for these results.

[8]To demonstrate that these results are robust to specification, SI 6.1 presents results excluding leaning independents' perceptions. Excluding leaners fails to move mean perceptions systematically or appreciably.

[9]We calculate: $\frac{1}{n_o}\Sigma_{g_o=1}^{n_o}\frac{\text{Estimated }\%_{g_p}-\text{True }\%_{g_o}}{\text{True }\%_{g_o}} - \frac{1}{n_i}\Sigma_{g_i=1}^{n_i}\frac{\text{Estimated }\%_{g_i}-\text{True }\%_{g_i}}{\text{True }\%_{g_i}}$, where g_o and g_i index out- and in-party-stereotypical groups, respectively.

Figure 5.1: People Overestimate the Share of Party-Stereotypical Groups Among Parties' Supporters



NOTE: 95% confidence intervals depicted.

Table 5.1: Perceptions of the Share of Party Identifiers Belonging to Party-Stereotypical Groups Across Different Studies

	True	YouGov			Amazon Mechanical Turk		
		Main Survey March 2015			Alt. Interps. November 2014	Extremity Exp. April 2014	Affect Exp. November 2014
		Full Sample ($n = 1000$)	Democrats ($n = 438$)	Republicans ($n = 336$)	($n = 382$)	($n = 1036$)	($n = 821$)
Democratic Party Groups							
Black	23.9	41.9	39.4	46.4	37.7	41.5	41.4
	[22.5, 25.5]	[40.2, 43.7]	[36.8, 41.9]	[46.4, 49.4]	[35.3, 40.1]	[38.9, 44.2]	[38.4, 44.4]
Union members	10.5	39.3	36.8	43.5	36.4	39.7	39.3
	[9.4, 11.6]	[37.6, 41.1]	[34.3, 39.4]	[43.5, 46.6]	[34.0, 38.8]	[37.1, 42.2]	[36.4, 42.3]
Gay, lesbian, & bisexual	6.3	31.7	29.0	38.2	29.4	29.7	30.5
	[5.4, 7.2]	[29.9, 33.6]	[26.5, 31.6]	[34.8, 41.7]	[26.6, 32.3]	[26.8, 32.6]	[27.1, 33.9]
Atheist/Agnostic	8.7	28.7	24.5	35.7	30.5	33.2	29.1
	[8.1, 9.2]	[27.0, 30.4]	[23.3, 26.6]	[32.5, 38.8]	[28.1, 32.9]	[30.4, 36.0]	[26.4, 31.8]
Republican Party Groups							
Earn over $250,000	2.2	38.2	44.1	33.3	34.3	37.3	37.3
	[1.5, 2.8]	[36.4, 40.1]	[41.0, 47.1]	[30.5, 36.1]	[31.4, 37.2]	[35.2, 39.4]	[35.0, 39.6]
Evangelicals	34.3	41.6	43.7	43.2	48.7	51.8	49.9
	[32.5, 36.1]	[39.8, 43.3]	[40.9, 46.5]	[40.6, 45.9]	[45.9, 51.5]	[49.9, 53.7]	[47.8, 52.0]
Southerners	35.7	40.4	44.4	39.7	47.7	50.8	48.8
	[33.5, 37.8]	[38.8, 41.9]	[41.9, 46.8]	[37.5, 41.9]	[45.4, 50.3]	[49.3, 52.3]	[47.1, 50.5]
Age 65+	21.3	39.1	44.2	38.3	47.3	See Footnote 14	50.6
	[19.5, 23.1]	[37.7, 40.6]	[41.8, 46.5]	[36.1, 40.5]	[45.1, 49.6]		[49.0, 52.3]

NOTE: 95% confidence intervals in brackets. For respondent perceptions, these reflect two standard errors plus/minus the mean estimate. For estimates of the true parameter, the confidence intervals reflect two standard errors plus/minus the population estimates. Details about YouGov sample recruitment and how the sample compares to established benchmarks can be found in SI 6.1 and SI 6.1. The Amazon Mechanical Turk (MTurk) samples were recruited to conduct experimental studies discussed later in the paper. For details about general characteristics of MTurk samples, see Berinsky, Huber and Lenz (2012). To see how our MTurk samples compare to the U.S. population, see SI 6.2 and SI 6.3. To calculate mean perceptions from the two MTurk experiments, we pooled perceptions across conditions as they were assessed pre-treatment. For the extremity and affect experiments, we only asked about perceptions of out-party group-party dyads; independents were randomly assigned to one of two parties.

43.5% — 4.9% worse than Republicans (95% CI: [2.2%, 7.6%]). In line with our hypothesis, the data suggest that out-party perceptions are more biased. But overall, consistent with the notion that people rely on commonly-shared, impersonal information to arrive at these judgments, people aren't especially accurate when thinking about their own party; they're just more inaccurate when thinking about the other.

Finally, the data speak to potential sources of these misperceptions. Perceptions of the percentage of party identifiers belonging to party-stereotypical groups tend to become not more, but less accurate with increased interest in political news (see Figure 5.2).[10] For seven of the eight party-group dyads, those who report following the news most closely also hold the most prototype-biased beliefs about party composition. Interestingly, the one dyad for which this is not true involves the social group for which a base rate has become popularized in political media—"the 1%" and an annual income of $250,000 per year have both become signposts for a high level of wealth in America. While further research is needed to conclusively identify media's influence, the associations presented here suggest more systematic study of how the media color people's beliefs about party composition.

Are These Perceptions Real?

The results thus far comport with the notion that people's beliefs about party composition are notably and systematically distorted by party prototypes. However, they are also consistent with three alternate explanations: expressive responding, innumeracy, and ignorance of group base rates.

We conducted an experiment on Amazon's Mechanical Turk to assess these alternative explanations. Reassuringly, MTurk respondents' perceptions of party composition are quite similar to those of YouGov respondents (see Table 5.1).

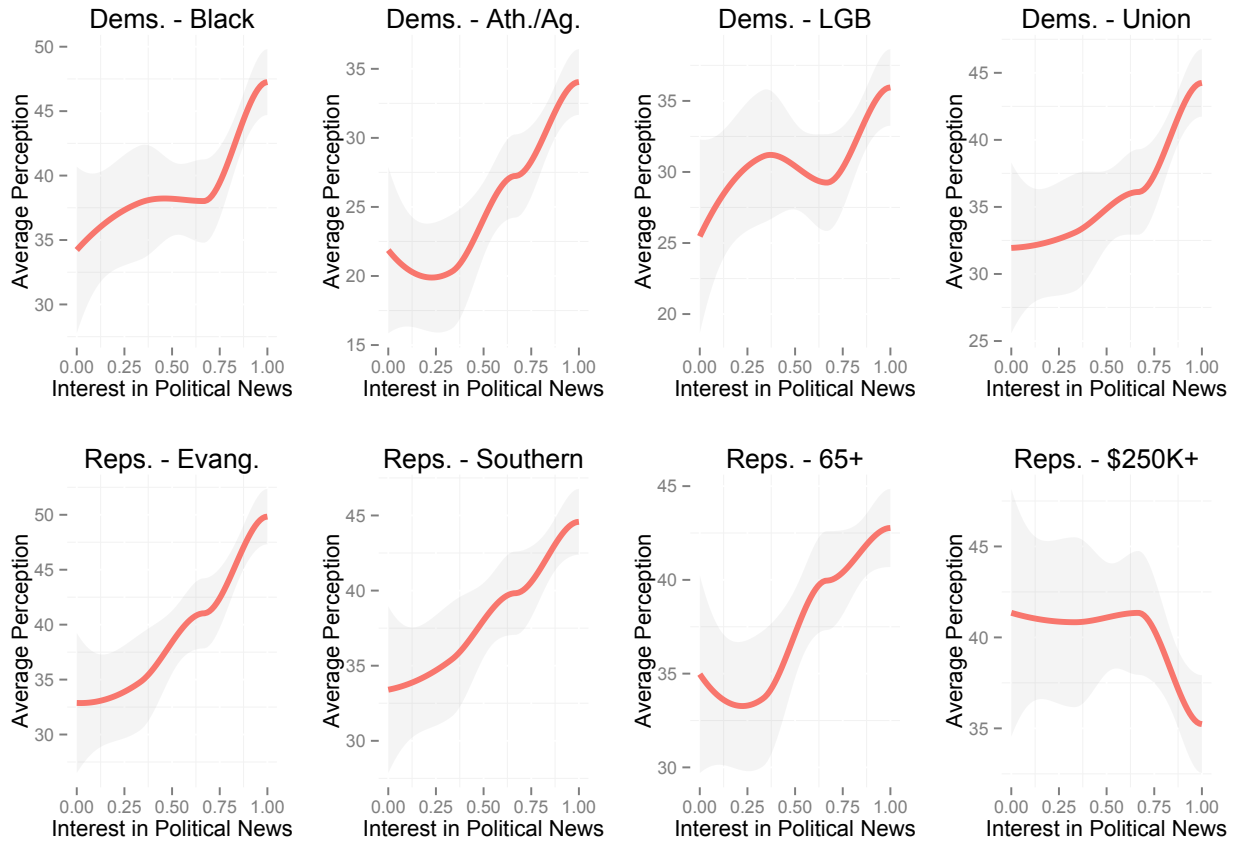
We randomly assigned respondents to one of four conditions (see SI 6.2 for screenshots of the conditions). The standard estimation condition was identical to the task in the main survey described above. Estimates from this condition thus serve as a baseline. We designed each of the other three conditions to preclude one specific alternative explanation. Thus, a significant reduction in mean perceptual error in any of these conditions would imply support for the corresponding alternative.

Figure 5.3 presents the results. As above, we plot respondents' estimates of the percentage of party p belonging to group g against the truth, but here we plot these estimates separately by experimental condition.[11]

[10] The exact question wording was: "Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs..." with response options: "Most of the time" (42.0%), "Some of the time" (31.5%), "Only now and then" (18.1%) and "Hardly at all" (8.5%).

[11] In Figure 5.3, we pool across partisans. However, it may be that the treatments affect only in- or out-party perceptions. To test that, we replicate the analysis with interactions between the conditions and partisanship (see SI 6.2). We fail to observe any systematic patterns.

Figure 5.2: Those Most Interested in Political News Hold the Most Skewed Perceptions



NOTE: Trends depicted via LOESS with 95% confidence intervals. All relationships are significant at $p < 0.01$, as measured via OLS.

Expressive Responding

People have strong feelings toward social groups and political parties. Thus, one concern is that people may intentionally misreport that groups they like (dislike) are prominent in parties they like (dislike) to express their partisan attitudes. To assess whether our measures capture expressive responding rather than genuine perceptions, we provided a random subset of respondents accuracy incentives for close-to-correct responses (see Bullock et al., 2013; Prior, Sood and Khanna, 2013). Participants received an additional five cents—an additional 20% of the compensation for finishing the survey (25 cents)—for each response that fell within five percentage points of the truth. We expect this opportunity to nearly triple earnings to motivate respondents to report their true beliefs.[12]

[12]According to Horton and Chilton (2010), the typical MTurker will work for an average wage of $1.40 per hour. The average completion time for this study was just under seven minutes. Thus, the standard compensation of 25 cents in our study implies an hourly wage of $2.14, while the potential hourly wage in the incentives condition was $5.57—nearly four times the hourly wage for which MTurk workers are willing

Expressive responding fails to explain our results. As Figure 5.3 shows, participants in the incentives condition are just as erroneous as those in the standard estimation condition. Not only do we fail to find significant differences in mean perceptions for any of the eight party-group dyads, but we fail to find any significant differences between the distributions of responses in these two conditions. (See SI 6.2 for Kolmogorov-Smirnoff tests, and see SI 6.2 for further observational evidence against expressive responding.)

Innumeracy: Using a Denominator Larger than 100

Even if responses are genuine, they may reflect *innumeracy* rather than true misperception. For example, when asked to report their perceptions on how mutually exclusive groups compose the U.S. population, people's summed responses often exceed 100 (e.g., Wong, 2007). To assess this concern, we asked a random set of respondents to not only report the perceived share of partisans belonging to each party-stereotypical group, but also the share of that party belonging to an exhaustive set of mutually exclusive, complementary group(s). Respondents were asked about groups for either the in-party or the out-party (e.g., "What percentage of Democrats do you think are: Black? White? Latino? Other?") and responses were required to sum to 100 to pass validation (with a counter automatically summing all entries for respondents).

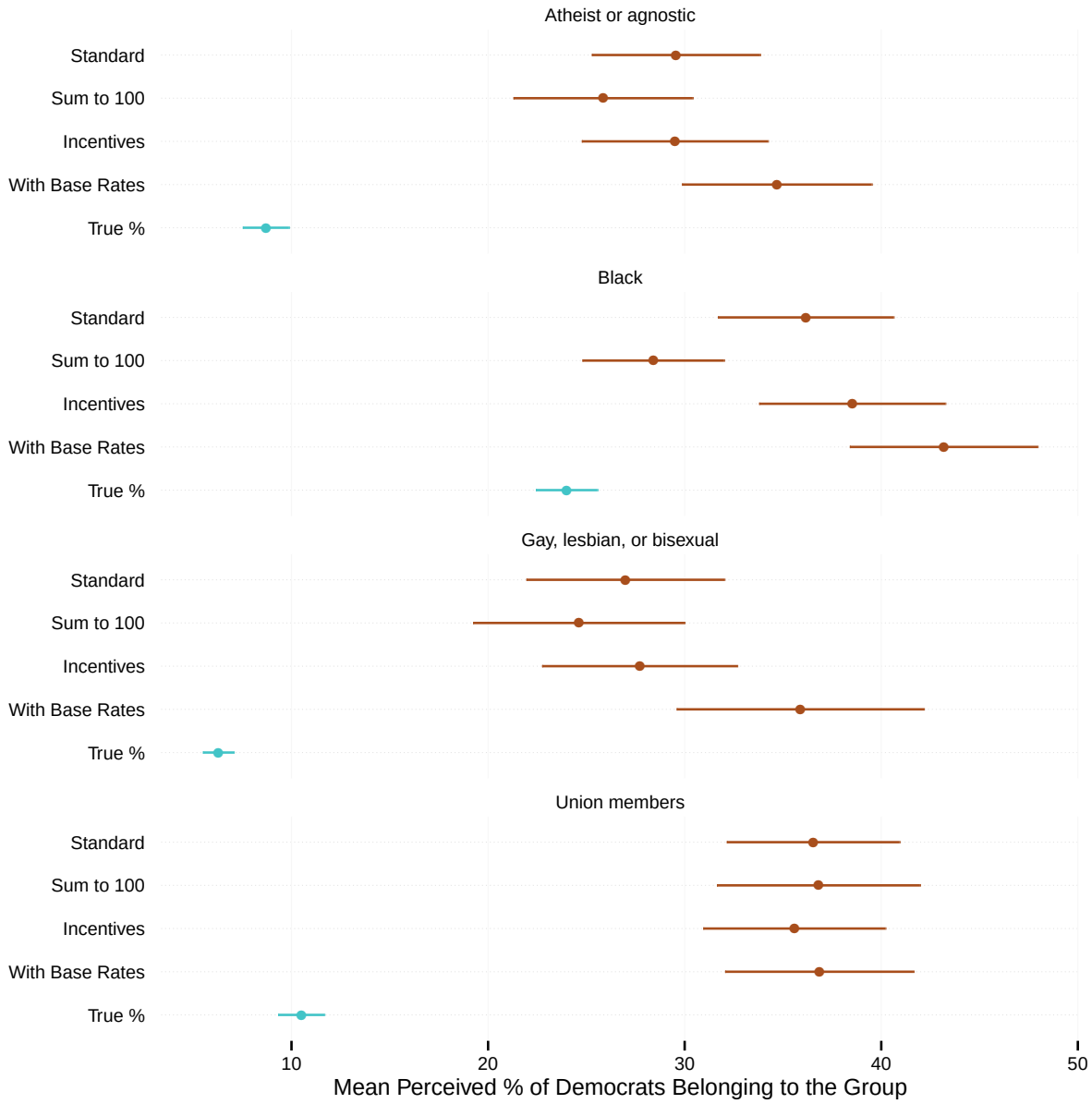
If innumeracy drives our results, estimates in the Sum to 100 condition ought to be considerably smaller. They are not. For seven of the eight groups, the difference between reports under the Standard condition and the Sum to 100 condition were statistically indistiguishable from zero or in the wrong direction. Only for the Democratic-black dyad were estimates in the Sum to 100 condition somewhat smaller—they declined from 36.2% in the Standard condition to 28.4%, still nearly five points greater than true percentage of blacks among Democrats. Pooling across groups, reports of perceptions in the Sum to 100 condition were 1.94 points less erroneous than reports in the standard condition. Given that the typical perception is off by 23.1 points in the standard condition, this reduction is neither substantively nor statistically significant. (See SI 6.2 for a regression with group-party dyad fixed effects.) Thus, innumeracy fails to account for the perceptual errors we observe.

The Sum to 100 task provides additional insight. We asked about multiple complementary groups for five of the party-group dyads. In each of these cases, one group was most clearly counter-stereotypical to the party. Respondents greatly underestimated partisans' tendency to belong to these groups. They thought 19% of Republicans earned under $50,000 per year (compared to 41% in reality), 16% were between 18 and 39 years old (33%), and 12% were non-Christian or did not identify with a religion (19%). Similarly, they thought just 42% of Democrats were white (60%), and 26% Protestant (45%). (Each of these misperceptions reaches conventional levels of statistical significance; see SI 6.2 for the tables). Even when given a task that demands significant cognitive effort and that leads them to think about how groups compose parties relative to each other, respondents frequently err

to work.

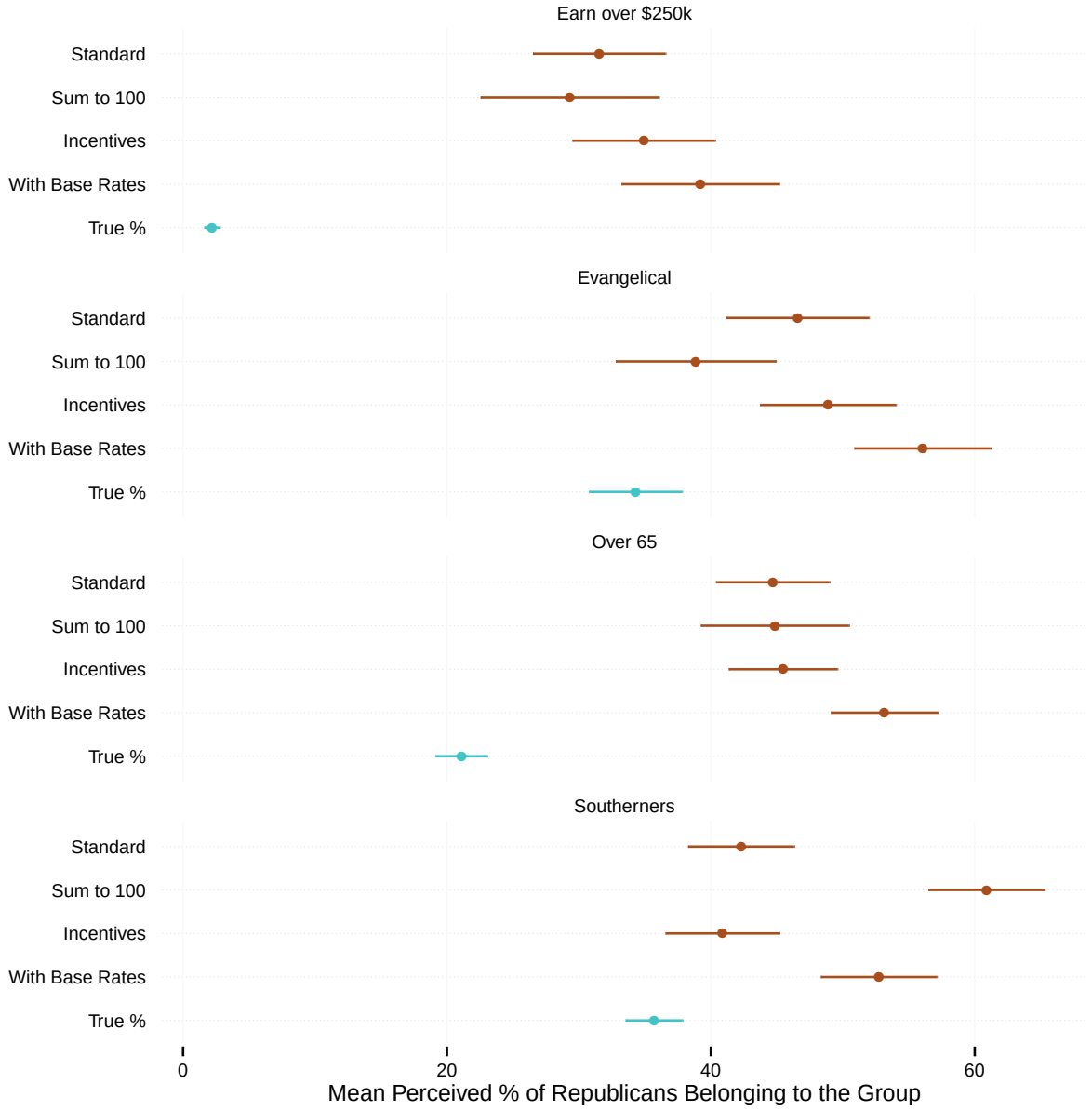
Figure 5.3: Expressive Responding, Innumeracy, and Poor Knowledge of Base Rates Fail to Explain Apparent Perceptual Errors

(a) Perceived Composition of Democratic Identifiers, by Experimental Condition



NOTE: 95% confidence intervals depicted.

(b) Perceived Composition of Republican Identifiers, by Experimental Condition



NOTE: 95% confidence intervals depicted.

by 15-20 percentage points. In sum, people systematically overestimate the share of particular groups and underestimate the share of others, in a manner consistent with the parties' sociopolitical brands.

Ignorance of Base Rates

A well-documented finding, and one we replicated in the standard estimation condition, is that people are largely ignorant of the share of various groups in the population at large.[13] Thus, the misperceptions we have documented may be genuine, but may reflect misperceptions about the composition of the population rather than anything specific to the parties. We put this question to a dispositive test by removing ignorance as a plausible alternative explanation. In the base rates condition, we anchored sliding scales at the base rate for each party-stereotypical group in the adult American population and informed respondents that we had done so. We then asked respondents to use these sliders to estimate the percentage of partisans belonging to party-stereotypical groups.

Shockingly, providing base rates makes participants *less* accurate. This apparent effect is unexpected and warrants further study. However, the fact that perceptions fail to improve even when we provide accurate (and often very low) base rates suggests support for representativeness as a psychological mechanism. As Kahneman and Frederick (2002) note, when making categorical predictions under uncertainty—such as, "What percentage of Democrats are black?"—people are apt to substitute "the heuristic attribute of representativeness for the target attribute of probability." That is, people focus primarily on the notion that "like goes with like" (Gilovich and Savitsky, 1996) to the detriment of base rates, even when base rates are known or provided. While not completely dispositive of other mechanisms, the data comport with this explanation and suggest a promising avenue for further research into why people overestimate differences between the parties.

Consequences of Misperceptions about Party Composition

The evidence thus far suggests that people tend to misperceive how prototypical party supporters are. However, we also find significant variation in partisans' perceptions, which allows us to test the hypothesis that people use these perceptions when reasoning about party politics.

The "sociopolitical brands" account aptly characterizes how unique partisanship is as an identity. On the one hand, partisanship is functional. Commonly-shared beliefs about

[13]In the standard estimation condition, we asked participants about the share of party-stereotypical groups in the population after quizzing them about their share in the parties. Respondents overestimated the share of all the groups in the population except for Southerners, consistent with past results that people are bad at base-rates. However, party-specific perceptions were significantly more inflated, implying that people's images of the parties matter. See SI 6.2 for complete results.

"which groups go with which parties" can help citizens evaluate the parties' positions and make political decisions (e.g., Campbell et al., 1960; Converse, 1964). Considered in tandem with people's tendency to associate particular social groups with particular policy preferences (Chambers, Schlenker and Collisson, 2013; Wilder, 1978), this implies that people's perceptions about party composition may inform their beliefs about where the parties stand. (That is, overestimating the proportion of Republican supporters who are wealthy is liable to cause people to infer that Republicans are more conservative than they actually are, especially on economic issues.)

On the other hand, partisanship is an identity in and of itself, and the sociopolitical brands account argues that people identify strongly with parties precisely because parties reflect salient social cleavages. As such, party identification tends to endure (Green, Palmquist and Schickler, 2002) and strength of identification tracks inter-party animus (Iyengar and Westwood, 2014). However, identity-fueled animus itself may be a consequence of how stereotypical people believe their out-group to be (e.g., Plant et al., 2009; Ramasubramanian, 2011). Thus, if people do primarily understand the parties as sociopolitical brands, we would expect beliefs about party composition to contribute to affective polarization between Democrats and Republicans.

In what follows, we use two distinct strategies to test these hypotheses. First, we use information provision experiments to demonstrate that inaccurate beliefs about party composition cause people to see partisans as more extreme on issues than they typically are, and also to feel greater animus toward the out-party. Importantly, we show that these apparent effects increase with prototype-bias in people's perceptions of party composition. Then, to demonstrate the external validity of these findings, we report results from a recent, high-quality survey of Californians showing that partisan attitudes track beliefs about party composition.

Experimental Design

We conducted two experiments on MTurk to assess the consequences of misperceptions about out-party composition. (See SI 6.3 for details of how the samples compare to established benchmarks.) The experimental designs were identical, differing only in dependent measures.

The experiments focus exclusively on out-party perceptions for two reasons. First, perceptions of out-party composition tend to be more erroneous and thus provide greater opportunity for correction. (In information provision experiments, correction itself is the treatment while the magnitude of correction can be thought of as "dosage.") Second, partisan entrenchment and polarization at the mass level motivates much of this research. Since existing evidence suggests that out-party hostility animates this polarization (e.g., Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2014), we assess and manipulate out-party perceptions.

To determine the causal effect of misperceptions about out-party composition, we provided information about the actual share of four party-stereotypical groups in the out-party

to a random set of participants. In particular, we assigned participants to one of three conditions: ask, tell, or control. In the ask condition, we asked participants about the share of out-party identifiers belonging to party-stereotypical groups (as we did in the studies discussed above) prior to administering the dependent measures.[14] Participants in the tell condition also answered these questions, but received correct information about the share of party-stereotypical groups in the out-party before responding to the dependent measures. Finally, participants assigned to the control condition responded to the dependent measures without first answering questions about out-party composition. However, they responded to these questions later in the survey, so we can examine the relationship between the dependent measures and respondents' degree of misperception within conditions. Across conditions, to deter demand effects, we couched the questions about party composition as part of a broader political knowledge survey.

As a manipulation check, participants in the tell condition answered the party composition questions again at the end of the survey. Their beliefs became significantly more accurate post-treatment. Mean absolute error decreased substantively and significantly for each of the eight party-group dyads; across groups, mean absolute perceptual error declined from 27.7 points to 6.1 points, a 21.6 point drop (95% CI: [-23.6, -19.5]). (SI 6.3 and SI 6.3 presents the results for each individual group-party dyad. SI 6.3 presents data that show that respondents also thought that the information was novel.)

We compare responses in the tell condition to responses from the two other conditions to identify the impact of correcting misperceptions about out-party composition. The control condition provided us with a baseline for the dependent variables, while the ask condition mimicked the tell condition most closely by priming (albeit without correcting) people's beliefs about the share of party-stereotypical groups. However, we conjecture that the ask condition, which makes people think about the out-party as a collective rather than a pure prototype, also reduces social distinctiveness in participants' "working mental images" of the out-party. (Recall that while people overestimate the share of party-stereotypical groups in their respective parties, the typical respondent still responds that these group members are a minority in the party—thus likely priming people to think about the bulk of partisans who break from the prototypes they typically use to represent the parties.)

We estimate that we successfully treated 74.2% of participants in the tell condition.[15] Assuming that the effect of our informational treatment is limited to those who learn it, the difference-in-means between conditions is a conservative *intention-to-treat* (ITT) estimate.

[14] In the first experiment, one party-group dyad was different: instead of asking Democrats for their perceptions of the percentage of Republicans who are aged 65 or more years, we asked about the *counter-stereotypical* group, the percentage of Republicans aged 35 or below. Average responses to this question were almost exactly accurate (26.6% versus 25.7% in reality). However, Democratic participants were as inaccurate on the other three groups as they were in the above studies; thus, treatment impact did not vary excessively across the two experiments.

[15] We operationalize failure-to-treat as the participant's failure to answer at least one of the four post-test out-party composition questions within five percentage points of the correct answer. This is a liberal definition of compliance, but choosing such a liberal definition yields a conservative (lower) estimate of $CACE$, since $CACE = \frac{ITT}{\%_{\text{complier}}}$

To assess the effect of actually learning something about out-party composition, we also estimate a *causal average complier effect*, $CACE = \frac{ITT}{\%_{\text{complier}}}$ (Bloom, 1984).

Dependent Measures

In our first experiment (conducted April 2014, $n = 1036$), we investigated whether misperceptions about the out-party affect beliefs about the extremity of that party's supporters. We asked participants to place the typical Democrat and Republican on four semantic policy scales: taxes, abortion, gay rights, and racial policy. Each point reflected a specific policy position and scale points covered a wide spectrum of the policy space. For example, "Decrease federal income taxes on just the highest earners, keeping the tax rate the same on all others" and "To address inequality, establish a national maximum income by taxing all income over a certain amount at 100%" bookended the taxes scale. (See SI 6.3 for exact question wording and response options.) We crafted the scale endpoints so that they fell outside the political mainstream and instead reflected the intense policy demands of party-stereotypical groups (e.g., Bawn et al., 2012)—e.g., in the taxes example, the highly wealthy and the economically marginalized. By contrast, party leaders' public positions and mainstream political debate reflect the positions at the middle scale points. Thus, our primary measure of *Extremity perception* is placing the typical out-party supporter at that party's ideologically-orthodox, extreme scale endpoint—e.g., saying that the typical Democrat supports a national maximum income—rather than at scale points that fall within the political mainstream. As we show in SI 6.3, results are robust to specification; however, the strategy we employ here most clearly comports with our hypothesis that overestimating out-party stereotypicality leads people to see the typical out-party supporter as an "intense policy demander."

We stacked the data so that the unit of analysis is the participant-policy domain. In all experimental analyses, we first test for main effects of the treatment with simple difference-in-means estimators. In this case, with each respondent reporting four policy perceptions, we cluster standard errors at the respondent-level. To assess whether beliefs about out-party extremity track prior beliefs about out-party composition—and whether this relationship changes meaningfully between conditions—we regress the dependent measure on *average prior perceptions* (rescaled 0-1) within-conditions. In the extremity perception study, with an indicator serving as the dependent measure, we use a logistic link function. We also include fixed effects for policy (to improve precision of estimates) and random effects for respondents (again, to address clustering of errors; see Arceneaux and Nickerson (2009) for a thorough discussion of different approaches).[16] Letting i index respondents and p policy domains, and letting δ denote fixed effects, ϵ random error, α random effects for each respondent, and X the respondent's average prior perception of $Pr(\text{group} \mid \text{party})$, our within-conditions model

[16]To demonstrate that our results are robust to model specification, we present nearly identical results obtained via OLS with clustered robust standard errors in SI 6.3.

takes the form:

$$Pr(y_{ip} = 1) \sim \text{logit}^{-1}(\beta * X_i + \delta_p + \alpha_i;\ \ \sigma_\epsilon^2)$$
$$\alpha_i \sim N(0, \sigma_\alpha^2)$$

We supplement this analysis with a placebo test. In addition to asking participants for out-party policy placements, we asked for in-party measures. Since we did not provide information about the composition of the in-party in any condition, treatment assignment should not affect these perceptions.

The first experiment also assessed the degree to which misperceptions about out-party composition drive hostility across party lines. We measured such partisan hostility with a 101-point feeling thermometer for the out-party.

In a second experiment, conducted in November 2014 ($n = 821$), we further investigated the effect of these misperceptions on affective polarization. In particular, we substituted a social distance battery for the more abstract feeling thermometer to assess whether social misperceptions merely move symbolic affect toward the out-party (as measured by the feeling thermometer), or whether they contribute to the kind of affective polarization that can influence partisans' daily lives and social contentment. Consistent with Almond and Verba (1963), Bogardus (1947), and Iyengar, Sood and Lelkes (2012), we measured negative affect toward the out-party via a series of four questions that elicited emotional responses to tangible situations: a family member marrying a Republican (Democrat), being assigned to work closely with a colleague who supports the Tea Party (Occupy Movement) and enjoys discussing politics at work, a neighbor putting a "Palin for President" ("Hillary Clinton 2016") sign in their yard, and George W. Bush (Bill Clinton) receiving an honorary degree from a nearby college. Again consistent with previous work, we averaged the items to create a *Partisan social distance* index ($\alpha = .71$).

Misperceptions About Composition Cause Perceptions of Out-Party Extremity

Partisans tend to overestimate the extremity of views held by supporters of the main opposing party (Ahler, 2014; Levendusky and Malhotra, Forthcoming). We suspect that this is at least partly because people misperceive the parties as highly composed of party-stereotypical groups associated with intense policy demands. Our experimental results provide support for this hypothesis. Partisans assigned to the tell condition were 6.6 points less likely to place the typical out-party supporter as an intense policy demander than those in the control condition (95% CI: [-0.11, -0.02]; see the left pane of Figure 5.4).

The corresponding difference between the ask and control conditions is about about half as large, providing a glimmer of evidence that simply asking people to provide numerical estimates about party composition breaks the usual tendency to think about parties as prototypes, which alone may reduce perceived out-party intensity. However, this estimated effect falls short of conventional statistical significance (diff. = -0.03, 95% CI: [-0.08, 0.01]).

This main effect only appears among partisans; we fail to observe such a treatment effect in the full sample. One reason for this is that independents are generally less likely to see others as extreme (Ahler, 2014), a tendency that appears in our data. (Just 22% of placements given by independents are at the endpoint, compared to 31% of those given by partisans.) Further, independents tend to be more accurate about Democratic (Republican) composition than Republicans (Democrats; see SI **??**), implying lower treatment impact on average.

Perhaps most importantly, we find that the degree of prototype-bias in people's perceptions of out-party composition moderates the main treatment effect—a trend we observe among all respondents in addition to the partisan subset. First, as Figure 5.4b shows, people who most overestimate the share of party-stereotypical groups are also most likely to see the typical party supporter as an intense policy demander. However, this trend is significantly less pronounced among those who received accurate information about party composition. In the Tell condition, participants with the most prototype-biased out-party perceptions were just 12.8 points more likely to see the typical out-party supporter as an intense policy demander than those who perceived the out-party accurately. (Accuracy-on-average, across groups, is denoted by the dashed vertical line in Figure 5.4b). In the Ask and Control conditions, the corresponding differences are 27.4 and 32.7 points, respectively. More generally, among partisan respondents, the interaction of misperception about out-party composition with the Tell treatment is associated with a -0.86 reduction in log-odds of placing the out-party as extreme (95% CI: [-2.39, 0.40]). In the full sample, where we have more precision, the interaction effect is similar (-1.07 reduction in log-odds, 95% CI: [-2.38, 0.21]), implying that independents with strongly prototype-biased perceptions of the parties also experience a treatment effect.

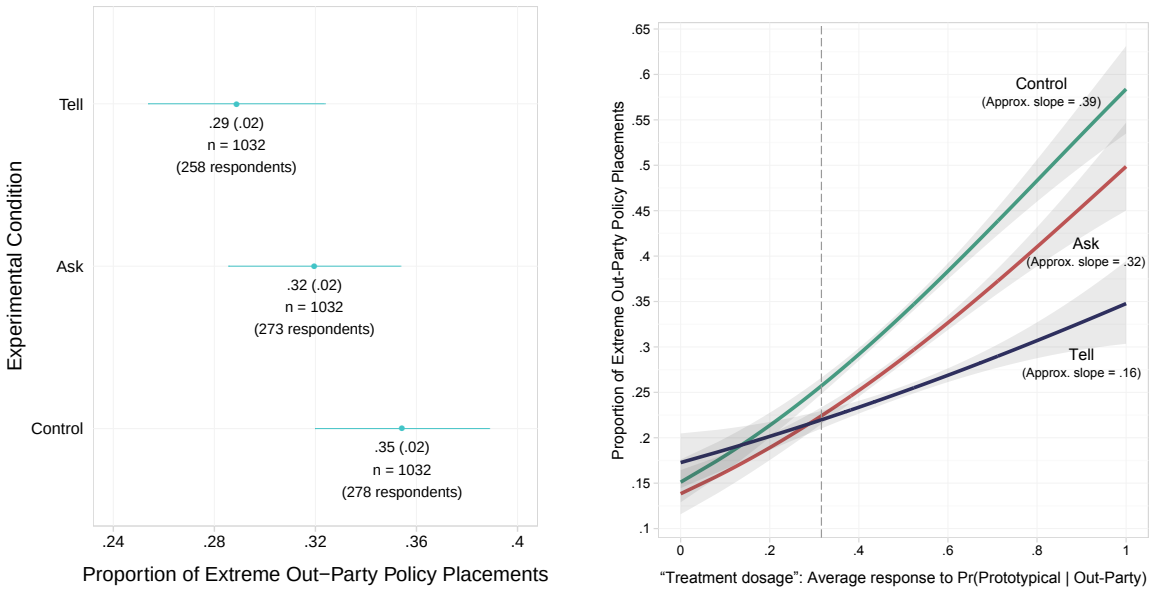
Until now we have discussed estimates of ITT effects and how they vary by perceptual error. But ITT underestimates the effect of learning about how stereotypical groups actually compose the out-party. Using assignment to tell as an instrument for learning the information provided in that condition, we estimate a complier average causal effect ($CACE$) of 8.7 points. That is, partisans assigned to the tell condition who actually learned the information were nearly 9 points less likely to place the typical out-party supporter as an intense policy demander (95% CI: [-0.12, -0.06]) than they would have been in the control condition.

Lastly, we note that our placebo test yields expected results. The difference between the ask and control conditions on perceptions of *in-party* extremity is 0.01 (95% CI: [-0.02, 0.04]), as is the difference between the tell and control conditions (95% CI: [-0.02, 0.04]). Thus, we may conclude that party-specific information affects party-specific beliefs.

Misperceptions About Composition Cause Partisan Animus

In the first experiment, participants randomly assigned to the tell condition rated their out-party 6.4 points warmer on the feeling thermometer than did those in the control group [95% CI: [2.8, 10.0]). Their average out-party rating was also 1.5 points more positive than the ask group's (95% CI: [-2.1, 5.2]), who as Figure 5.5a shows, also reported significantly

Figure 5.4: Misperceptions Cause People to Attribute Extreme Policy Preferences to the Typical Out-Party Supporter



NOTE: Perceptions of out-party composition were measured after the policy perceptions battery in the control condition. Figure 5.4a shows the proportion of respondents in each condition placing the out-party at the extreme endpoint on the policy scales, with 95% confidence intervals. Figure 5.4b plots the predicted proportion of respondents in each condition placing the out-party as extreme, as a function of prior beliefs about party composition, with 95% confidence intervals. The slopes in Figure 5.4b refer to linear approximations estimated via ordinary least squares (OLS), which approximates the functional form well. See SI 6.3 for tables showing results estimated with OLS.

warmer feelings toward the other side vis-'a-vis the control group. Taken together, these comparisons suggest that inaccurate beliefs about out-party composition fuel dislike of that party, but that correcting those beliefs—or even merely asking partisans to consider the other side as a collective rather than a prototype—can reduce such animus. And, consistent with the extremity-perception results, Figure 5.5b suggests that tell participants with the most prototype-biased pre-treatment perceptions of out-party composition experienced the greatest reduction in out-party dislike.

The second experiment clarifies this result by investigating these misperceptions' effect on partisan social distance—a more tangible, meaningful expression of affect than the abstract feeling thermometer. Using a five-point scale, participants expressed their prospective pleasure or displeasure at four hypothetical social interactions with the out-party. Consistent with recent work on affective polarization, the range of scores on this measure is *de facto* truncated. Partisans rarely respond that they would be "somewhat happy" or "very happy"

about these interactions; as such, 93.1% of partisans' scores on the social distance index fall between 0.5 and 1, across all conditions. Despite partisans' near-universal tendency to dislike the out-party, the tell treatment reduced out-party dislike by 2.5 points on average (95% CI: [-0.05, 0.00]), with an estimated $CACE$ of 3.3 percentage points (95% CI: [-0.07, 0.00]).[17]

Once again, this treatment effect is concentrated among those with the most prototype-biased prior perceptions of the out-party. Figure 5.6b plots estimated linear relationships between beliefs about the share of party-stereotypical groups in the out-party and partisan animus, within conditions. As expected, the slope is positive in both the ask and the control conditions, implying that people with the most prototype-inflated perceptions feel the greatest partisan distance. However, such a relationship is virtually nonexistent in the tell condition—the slope is flat. With OLS, we estimate that the main effect of the tell treatment falls to zero when interacted with prior perceptions; all of its apparent effect is captured by the interaction. (This is also true when feeling thermometers are the outcome; see SI 6.3 for regression tables.) The results of these two independent experiments thus imply that people's beliefs about party composition affect how they feel about their out-party—and that correcting misinformation most strongly affects the partisan sentiments of the most misinformed.

The experimental results presented in Figures 5.4, 5.5, and 5.6 comport. In all three, the difference-in-means between the tell and the control group is the largest, statistically significant, and in a direction consistent with the hypotheses. Likewise, in both cases, the ask group falls between the other two groups in terms of the average score on the dependent measure. If, in fact, the treatments caused no systematic differences in how partisans think about the out-party, we would expect to see this meta-outcome in well under 1% of replications of these experiments (1 in 216 potential outcome spaces). Thus, both what people think of the out-party as a social entity (as manipulated by the tell condition) and how they think about it (as manipulated by the ask condition) appear to affect peoples' attitudes toward supporters of the out-party.

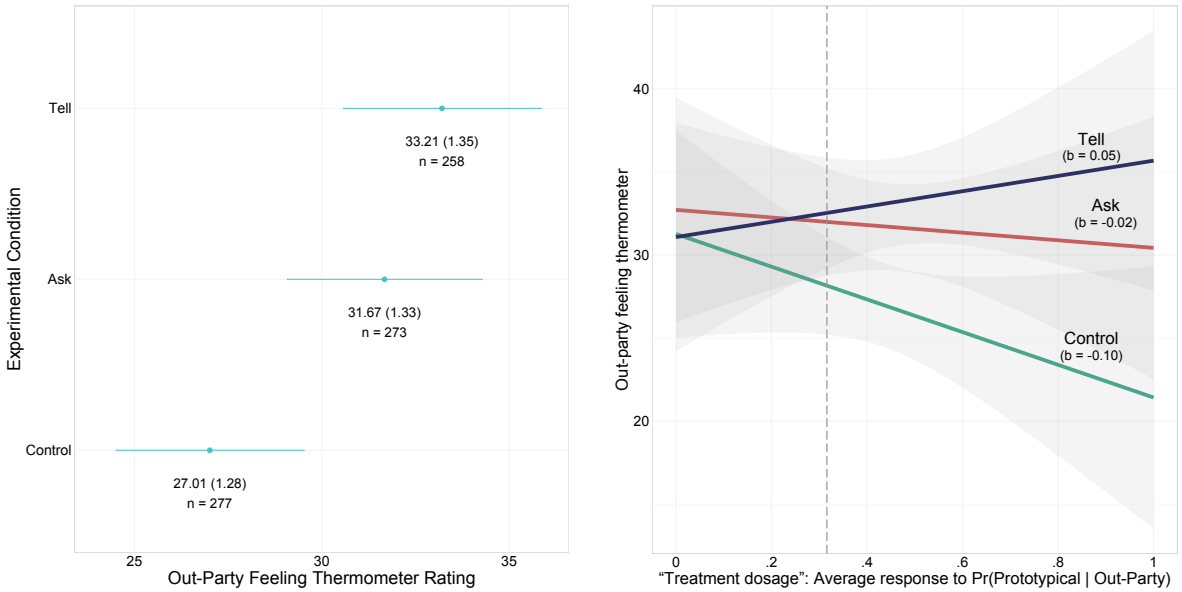
Observational Evidence

The experiments discussed above allow us to draw internally valid conclusions. Randomly selecting respondents to receive information about party composition renders potential outcomes independent of prior beliefs. As a consequence, omitted variables correlated with prior beliefs about party composition—e.g., interest in political news—are highly unlikely to explain the experimental results.

However, external validity concerns may linger. The most threatening of these would be a "reactive effect of the experimental arrangement" (Campbell and Stanley, 1963), meaning that the effects we observe are limited to the experimental setting. We therefore conducted

[17]Unsurprisingly, we fail to find clear evidence of treatment effects among non-leaning independent participants, since independents tend not to feel social distance toward partisans the way opposing partisans do. See SI 6.3 for analyses.

Figure 5.5: Misperceptions Cause People to Dislike the Out-Party



(a) (b)

NOTE: Perceptions of out-party composition were measured after the policy perceptions battery in the control condition. Figure 5.5a plots mean out-party feeling thermometer rating by condition with 95% confidence intervals. Figure 5.5b plots ratings by condition, as a function of prior beliefs about party composition, with 95% confidence intervals.

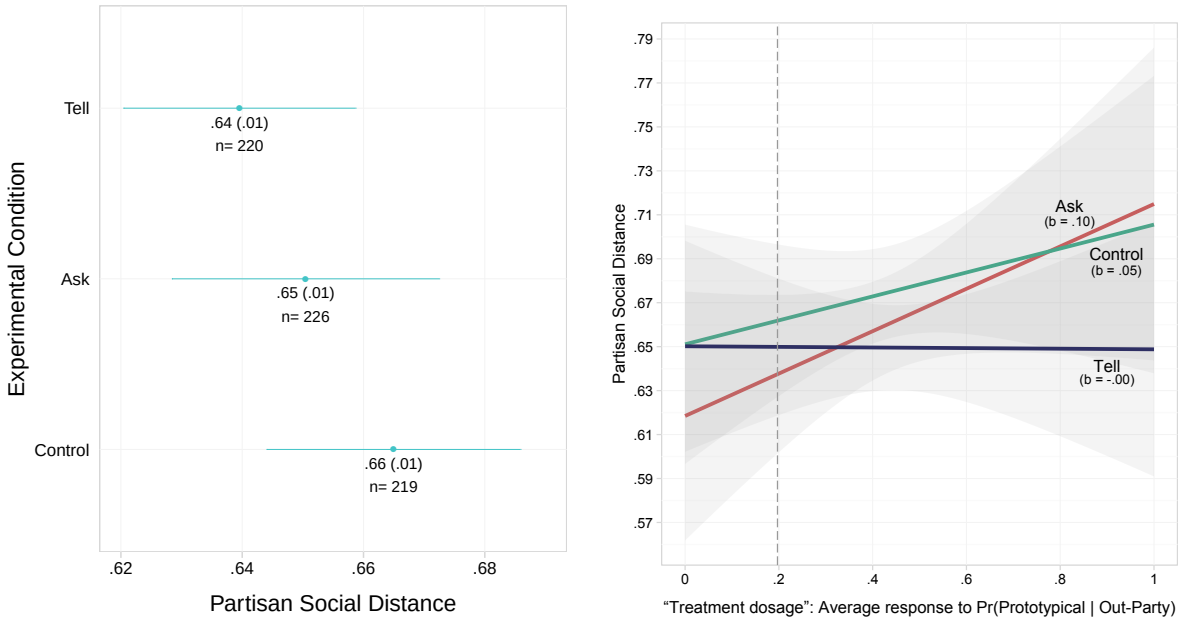
an observational study to assess whether the relationships we observed experimentally exist more generally.

We did so as part of the 2015 IGS-California Poll, which sampled 4257 California residents through Survey Sampling International (SSI). Like most online samples, this was a non-probability sample but it matches the population quite well on observed covariates (see SI 6.4 for demographics); survey weights improve this correspondence, although results are consistent in weighted and unweighted analyses. From this sample, we randomly selected 1815 partisans to answer party composition questions like those above. We also administered three measures to assess intense partisanship in voting behavior and four to assess respondents' sense of affective polarization from one of the two parties (randomly assigned), creating indices with these measures. Finally, we assessed respondents' feelings toward the party-stereotypical groups with 101-point feeling thermometers.[18] We rescale all variables 0-1 and interact reverse-coded feeling thermometers for group g (F_g) with perceptions of the percentage of the party belonging to group g (P_g).[19] We average across these interactions

[18]See SI 6.4 for details on all measures.

[19]Interacting affect toward g with perceptions of $\Pr(p|g)$ yields a more theoretically appropriate predictor.

Figure 5.6: Misperceptions Cause People to Feel Socially Distant from Out-Party Supporters



(a) (b)

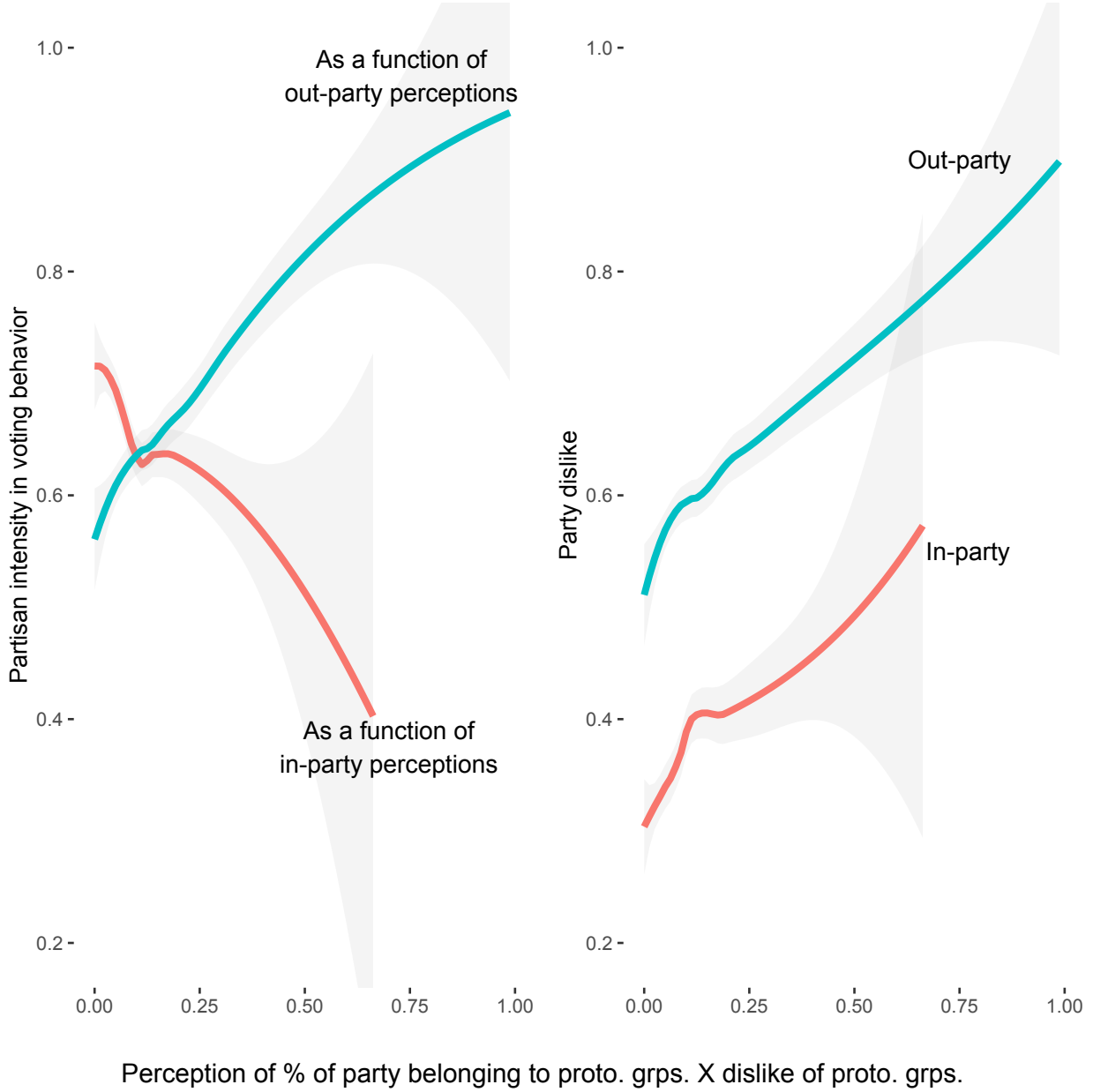
NOTE: Perceptions of out-party composition were measured after the policy perceptions battery in the control condition. Figure 5.6a plots mean partisan animus by condition with 95% confidence intervals. Figure 5.6b plots partisan animus by condition. as a function of prior beliefs about party composition, with 95% confidence intervals.

to create a measure of how much a respondent dislikes each party's prototypical groups and how much she believes they compose that party: $\frac{1}{n}\Sigma_{g=1}^{g=n} F_g P_g$.

Figure 5.7 confirms that people who perceive large differences between the parties on affectively-charged social dimensions are the most intense partisans. In the left panel, those who dislike their out-party's prototypical groups and see the out-party as heavily composed of those groups are most likely to vote with their own party and feel anger at the prospect of out-party victories. Similarly, as the right panel shows, such partisans are most likely to feel extreme social distance from the out-party. In-party perceptions matter as well: partisans who dislike their party's prototypical groups and see their party as heavily composed of them are less intense in their partisan leanings and feel a greater sense of distance from their co-partisan peers. Surprisingly, controlling for strength of party identification fails to meaningfully change these patterns (see SI 6.4 for regression tables). In sum, the observational results provide yet more evidence that the ersatz parties in our heads fuel our co-partisan affinity and our dislike of those across the proverbial aisle.

We could not do this in the experimental studies due to concerns about statistical power, suspicion, and priming respondents to consider inter-group affect pre-treatment.

Figure 5.7: Observational Evidence: Interaction of Misperception and Group Dislike Predicts Intense Partisanship



NOTE: LOESS curves with 95% confidence intervals. Data from the 2015 IGS-California Poll. All variables scaled 0-1.

Discussion

Across five studies, we find that people hold erroneous beliefs about the composition of the two major parties. These errors are systematic: people overestimate the degree to which partisans belong to party-stereotypical groups, often vastly so. Even in cases where party-stereotypical groups comprise just a sliver of the population—e.g., the rich (a stereotypically Republican group) or the LGBT (a stereotypically Democratic group)—people think that members of these groups constitute as much as a third of the parties. And even when people are given information about the share of these stereotypical groups in the population, estimates still tend to diverge wildly from reality, suggesting that people rely on representativeness when making judgments about how various groups compose the parties.

The data also suggest that media are a potentially important source of misperceptions. Strikingly, people with the most interest in consuming political news hold the most skewed perceptions about party composition. Perhaps yet more powerful evidence supporting the role of mediated, impersonal information in shaping people's perceptions of parties is the fact that these misperceptions are widely shared. Republicans, Democrats and Independents all overestimate the share of party-stereotypical groups, with partisan differences, although statistically significant, relatively small in comparison to the overall magnitude of the misperceptions.

We further find that these misperceptions hold important consequences. Erroneous beliefs about out-party composition affect people's beliefs about where the out-party's supporters stand politically. In an experiment, participants shown correct information about the share of these groups came to perceive the typical out-party supporter as less likely to hold extreme views. Our findings thus provide a potential explanation for why partisans tend to overestimate the extremity of opposing partisans. In future extensions, we plan to investigate whether misperceptions of one's own party's composition explain the striking finding that people also overestimate the extremity of their co-partisans (Ahler, 2014; Levendusky and Malhotra, Forthcoming).

We suspect that people's perceptions about mass-level party composition contribute to many more pathologies of partisanship than we have documented thus far. Beyond beliefs about extremity, for example, we suspect that perceptions about party composition affect people's beliefs about party priorities. For instance, believing that a third of Democrats are atheist or agnostic, or that half of Republicans are evangelical, is likely to lead one to believe that cultural issues like school prayer are far more important to the parties than they actually are. More generally, we suspect that people associate a narrow set of policy demands with each party-stereotypical group and think that these groups aggressively lobby the parties on those policies, which ultimately shapes perceptions of partisan priorities. If true, the democratic accountability ramifications would potentially encompass not just voters' attitudes toward the parties, but also party elites' ability to act on intense demands under guise of collective partisan representation. As such, we find this a valuable extension of research on party-group associations.

Outside of the realm of policy, misperceptions about the share of party-stereotypical

groups in the out-party drive partisans to feel more socially distant from the opposing party. Consistent with evidence presented by Hetherington and Weiler (2009) and Mason and Davis (2015), our data show that the roots of partisan animus at least partially lie in misperceptions about the share of party-stereotypical groups in the out-party. It is possible that as parties become relatively more socially distinct (Mason and Davis, 2015), these misperceptions become yet greater, which potentially explains the spike in partisan animus in recent years.

Along similar lines, our study also sheds light on the fundamental nature of partisanship. As we note, the data suggest that partisans overestimate the share of party-stereotypical groups in their own party. This is striking in that it suggests that, on average, partisans think that their own party is more heavily composed of groups to which they do not belong. For instance, many lower- and middle-class Republicans think that their party contains far more rich people than it actually does. This finding contributes to the literature in two ways. First, it suggests that many partisans like their own parties to the extent they do—a great deal, with average ratings exceeding 80 on the thermometer scale (Iyengar, Sood and Lelkes, 2012)—despite believing that the party doesn't look like them. Second, Green, Palmquist and Schickler (2002, p. 8) suggest that partisans choose parties based on "which assemblage of groups" looks like them. While this may still be true, our results suggest that a more apt description of people's use of party prototypes may be that people identify with parties—often intensely so—based on which groups they like.

Finally, and most broadly, this research furthers our understanding of people's perceptions of mass collectives and the role that these perceptions play in shaping individuals' own political attitudes. Mutz (1998) describes impersonal influence as the effect of people's perceptions of what others are experiencing, or what others believe, on their own attitudes and behaviors. We would take this one step further and assert that people's perceptions merely of who "generalized others" (Mead, 1934; Mutz, 1998) *are* can be a source of impersonal influence—and in this case, a potential catalyst for the mass-level pathologies of partisanship in contemporary American politics.

Chapter 6

Supporting Information for "The Parties in Our Heads"

6.1 Perceptions of Party Composition

Sample Information

YouGov interviewed 1294 respondents who were then matched down to a sample of 1000 to produce the final dataset. The respondents were matched to a sampling frame on gender, age, race, education, party identification, ideology, and political interest. The frame was constructed by stratified sampling from the full 2010 American Community Survey (ACS) sample with selection within strata by weighted sampling with replacements (using the person weights on the public use file). Data on voter registration status and turnout were matched to this frame using the November 2010 Current Population Survey. Data on interest in politics and party identification were then matched to this frame from the 2007 Pew Religious Life Survey. The matched cases were weighted to the sampling frame using propensity scores. The matched cases and the frame were combined and a logistic regression was estimated for inclusion in the frame. The propensity score function included age, gender, race/ethnicity, years of education, and ideology. The propensity scores were grouped into deciles of the estimated propensity score in the frame and post-stratified according to these deciles.

Sample Demographics

Table 6.1: Sample Demographics, Compared to Benchmarks

	YouGov Sample	YouGov Sample, Weighted	2012 ANES	2010 Census
Age				
18-29	18.3%	21.6%		19.2%
30-49	31.2%	30.0%		31.7%
50+	50.5%	48.5%		49.2%
Gender				
Male	47.6%	48.0%		49.1%
Female	52.4%%	52.0%		50.9%
Race/Ethnicity				
Non-Hispanic White/Caucasian	68.7%	66.5%		63.7%
Black/African-American	11.6%	11/5%		12.2%
Asian/PI	2.4%	3.7%		4.8%
Hispanic/Latino	13.1%	13.8%		16.4%
Native American	0.8%	0.7%		1.1%
Other/more than one	3.4%	3.8%		6.2%
Education				
Less than HS degree	5.4%	11.9%		8.9%
High school/GED	34.5%	30.8%		31.0%
Some college/2-year degree	32.9%	31.3%		28.0%
4-year college degree	19.0%	16.8%		18.0%
Graduate/professional degree	8.2%	9.2%		9.3%
Party Identification				
Democratic (inc. leaners)	45.9%	41.7%	49.0%	
Republican (inc. leaners)	35.0%	35.2%	39.0%	
No party preference/Other	19.1%	23.1%	11.9%	
Census Region				
Midwest	22.5%	20.3%		21.7%
Northeast	17.9%	17.8%		23.3%
South	35.9%	37.7%		37.1%
West	23.7%	24.2%		17.9%

Party-Group Associations

The most enduring images of the parties are from the New Deal—the association of the rich with Republicans and the working class with Democrats (Green, Palmquist and Schickler, 2002). And, for a long time, Republicans have been seen as the party of older Americans (Lewis-Beck et al., 2008, p. 148). Over time, however, additional social cleavages have aligned with partisanship. Most notably, as a consequence of partisan racial sorting in the 1960s, African Americans have come to be seen as prototypically Democratic. Events of half a century ago also precipitated the end of the long-standing association between the South and the Democrats, replacing it with a new linkage between the region and the GOP (Green, Palmquist and Schickler, 2002). Separately, the rise of the evangelical movement in the 1980s has led to evangelical Christians becoming more closely linked to Republicans, and the secular becoming more tightly linked to Democrats (Claassen, 2011).

In all, the literature suggests that Southerners, evangelical Christians, older Americans, and the wealthy are frequently associated with Republicans, while the working class, the religiously unaffiliated, and African-Americans are commonly linked to Democrats. Given recent highly salient politicization of gay rights, and the longer-standing linkage between civil rights groups and Democrats, we add gays, lesbians, and bisexuals to the list of groups associated with Democrats.

Sources of Data for Population-Based Estimates of Partisan Composition

Democrat-Stereotypical Groups

The estimated percentage of Democrats who are black (24.0%) comes from population-weighted 2012 ANES data, as do the estimated percentages of Democrats who are union members (10.5%) and gay, lesbian, or bisexual (6.3%). All estimates include Democratic-leaning independents. The percentage of Democrats who are atheist or agnostic (8.7%) is estimated based on Pew Research's 2012 Religion & Public Life Project.[1] While the public report does not report this particular statistic, we estimated with Bayes's Rule using publicly available statistics from the report.

Republican-Stereotypical Groups

The estimated percentage of Republicans who earn over $250,000 per year (2.2%) comes from population-weighted 2012 ANES data, as do the estimated percentages of Republicans who are from the South (35.7%, based on residence in a state identified by a majority of respondents as "Southern" in a recent 538 poll[2]) and opver 65 years old (21.3%). The percentage of Republicans who are evangelical (34.0%) is provided in Pew Research's 2012 Religion & Public Life Project.

[1] http://www.pewforum.org/2012/10/09/nones-on-the-rise

[2] The poll was 50% nationally representative and 50% from a Southern-region representative sample. See more at http://fivethirtyeight.com/datalab/which-states-are-in-the-south/.

Who Do Respondents Have in Mind?

One potential concern is that respondents may have misconstrued the question, "What percentage of Democratic (Republican) Party supporters do you think (have a party-prototypical characteristic)?" In particular, they may have had group of people in mind other than the general mass of citizens who identify with the Democratic (Republican) Party—e.g., strong identifiers, activists, or even elites. However, we have two distinct pieces of evidence that assuage this concern.

First, as part of the 2015 IGS-California Poll, we asked respondents two parallel questions to assess this concern directly. First, we asked, "What percentage of adults living in California do you think are..." with text-entry fields for "Democrats" and "Republicans." Respondents, on average, reported that 59.2% of Californians are Democrats (95% CI: [58.6, 59.8]) and that 39.1% are Republicans (95% CI: [38.5, 39.8]). These are overestimates—respondents appear, on average, to neglect the fact that many Californians do not identify with one of the parties—but they imply that people have large collectives in mind when asked about the parties.

We then asked the party composition perception questions with nearly-identical language: "What percentage of Democrats (Republicans) living in California do you think (have a party-prototypical characteristic)?" These results are presented in 6.4, and they are consistent with the perceptions reported in the YouGov and MTurk studies. Thus, when respondents answer the party composition questions, they appear to have the correct population in mind and appear to reason about the parties in that population in a prototype-biased fashion.

But even if people do have other partisan subsets in mind, it's not clear that their composition perceptions are any more accurate. The table below shows this. While some "activist subsets" are indeed more likely to have party-prototypical traits—e.g., blacks are disproportionately represented among strongly-identifying Democrats—many of these cases simply reflect the tendency of activists to have certain traits. For example, donors are more likely to be be old regardless of their partisanship, and delegates and politicians are more likely to be wealthy, regardless of partisanship. Furthermore, in many cases, the party-stereotypical groups are *less* represented in the activist or elite classes of the parties. For example, few Democratic politicians are openly non-religious, non-heterosexual, or have union membership. Thus, even if some respondents do have partisan subsets in mind other than the general population of Democrats and Republicans, it's not clear that their perceptions are any more accurate.

Table 6.2: Actual Mass and Elite Party Composition, Compared to Perceptions

	Mean perception (YouGov)	Actual, all partisans (2012 ANES and 2014 Pew R&PLP)	Just strong partisans (1s and 7s, 2012 ANES)	Just campaign donors (2012 ANES)	Convention delegates (2008 CBS Poll)	MCs (114th Congress)
Dem. groups						
Atheist/agnostic	28.7	8.7	10.5	16.4	¡ 22	0.4
	[27.0, 30.4]	[8.1, 9.2]	[8.9, 12.0]	[12.8, 20.0]		
Black	41.9	23.9	34.7	28.0	23	19.4
	[40.2, 43.7]	[22.5, 25.5]	[32.3, 37.2]	[23.6, 32.3]		
Gay, lesbian, or bisexual	31.7	6.3	6.9	6.6		3.1
	[29.9, 33.6]	[5.4, 7.2]	[5.6, 8.2]	[4.2, 9.1]		
Union members	39.3	10.5	11.9	15.3	24	
	[37.6, 41.1]	[9.4, 11.6]	[10.2, 13.5]	[11.9, 18.8]		
Rep. groups						
Age 65+	39.1	21.3	26.6	38.5		19.3
	[37.7, 40.6]	[19.5, 23.1]	[23.5, 29.7]	[32.2, 44.8]		
Earn over $250k/yr	38.2	2.2	1.9	5.1	¿ 34	
	[36.4, 40.1]	[1.5, 2.8]	[0.9, 2.9]	[2.2, 7.9]		
Evangelical	41.6	34.3	47.9	26.6	31	
	[39.8, 43.3]	[32.5, 36.1]	[41.0, 54.8]	[23.5, 29.7]		
Southern	40.4	35.7	37.6	32.1		42.3
	[38.8, 41.9]	[33.5, 37.8]	[34.2, 41.1]	[26.0, 38.1]		

Robustness

Figure 5.1 in the main text plots the mean estimate of various groups by partisan affiliation. It is possible that the distribution is skewed, making the mean not a particularly good summary of the views of an average respondent. In Figure 5.1 in the main text, we also took the decision of including responses of partisan leaners and counting them as partisans. In Table we put means plotted in Figure 5.1 side-by-side with medians, and mean estimates by only non-leaning partisans. The estimates are quite similar – differening generally by only a few percentage points.

Table 6.3: Median, No Leaners

		Out-Party			In-Party		
Groups	True	Means	Medians	No Leaners	Means	Medians	No Leaners
Black	24.0%	47.40%	41.50%	45.50%	38.26%	30.00%	39.21%
Atheist or Agnostic	8.7%	35.54%	25.00%	34.13%	23.87%	20.00%	23.42%
LGBT	6.3%	36.83%	25.00%	35.45%	29.36%	20.00%	29.13%
Union Members	10.5%	44.02%	40.00%	42.81%	35.69%	30.00%	35.90%
Rich	2.2%	43.91%	40.00%	45.45%	30.74%	25.00%	32.18%
Evangelical	34.3%	43.94%	40.00%	41.41%	45.25%	45.00%	44.56%
Southern	35.7%	43.34%	40.00%	41.70%	39.99%	40.00%	38.86%
Over 65	21.1%	42.84%	40.00%	42.30%	38.57%	35.00%	37.28%

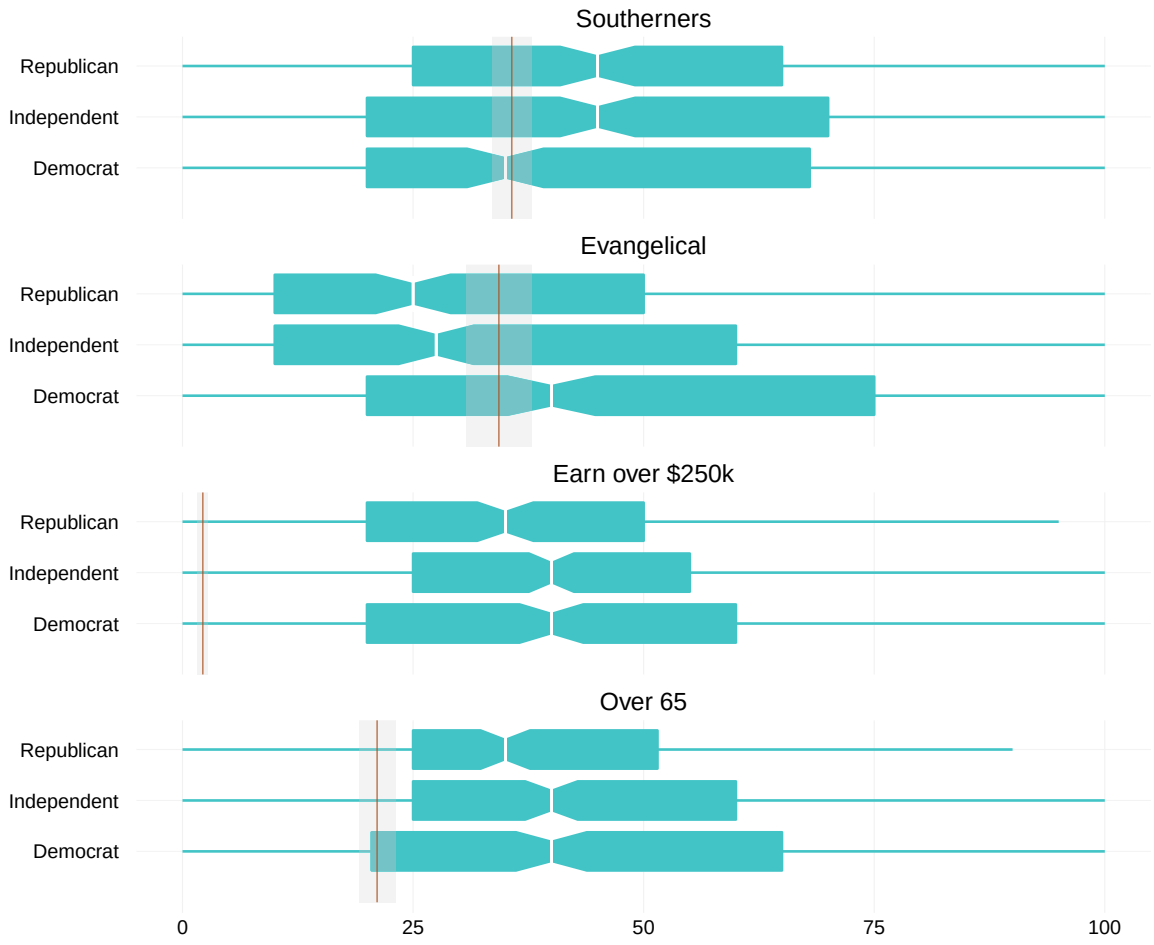
Majorities of Respondents Overestimate $Pr(\textbf{group}|\textbf{party})$

Table 6.4: Majorities of People Overestimate Party Stereotypicality

Party-Group Dyad	% Overestimating	95% Confidence Interval
Dems. – Atheist/agnostic	76.4%	[73.7%, 79.0%]
Dems. – Black	70.8%	[68.0%. 73.3%]
Dems. – Gay, lesbian, or bisexual	80.1%	[77.6%, 82.6%]
Dems. – Union members	79.4%	[76.9%, 81.9%]
Reps. – Earn over $250K	90.9%	[89.1%, 92.7%]
Reps. – Evangelical	52.4%	[49.3%, 55.5%]
Reps. – Over 65	72.4%	[69.6%, 75.2%]
Reps. – Southerners	50.9%	[47.8%, 54.0%]

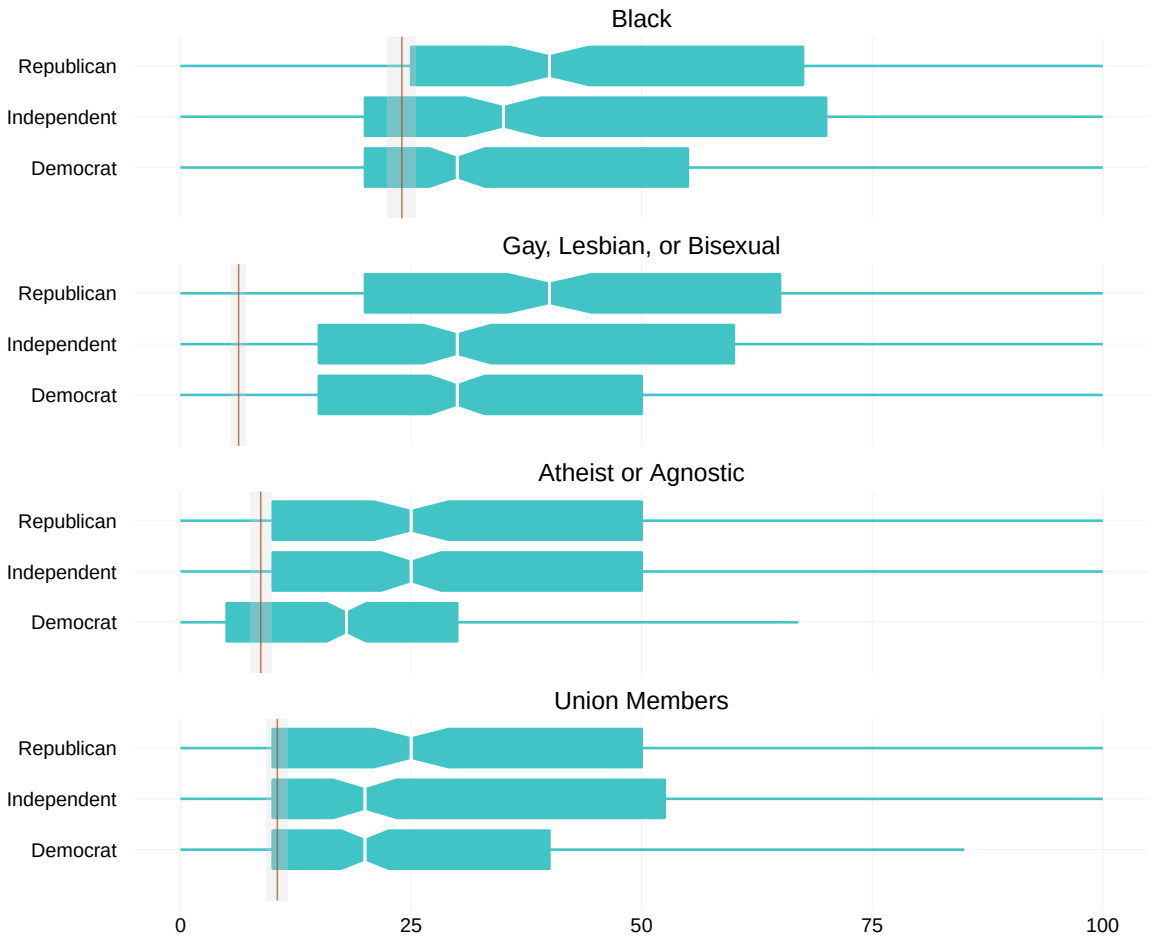
Distribution of Estimates of $Pr(\textbf{group}|\textbf{party})$

(a) Perceived Composition of Democratic Identifiers, by Respondent Partisanship



NOTE: The plots display the full range of perceptions reported (the thin teal lines), the interquartile range of perceptions (the thick teal section), and the median with a 95% confidence interval (the white band and notch in the middle of the IQR). They also display the population estimate of PR(group|party), depicted as verticla red lines with gray 95% confidence intervals based on sampling error.

(b) Perceived Composition of Republican Identifiers, by Respondent Partisanship



NOTE: The plots display the full range of perceptions reported (the thin teal lines), the interquartile range of perceptions (the thick teal section), and the median with a 95% confidence interval (the white band and notch in the middle of the IQR). They also display the population estimate of PR(group|party), depicted as verticla red lines with gray 95% confidence intervals based on sampling error.

6.2 Assessing Alternative Explanations

Alternative Explanations Study: MTurk Sample Demographics

Table 6.5: Characteristics of the MTurk Sample

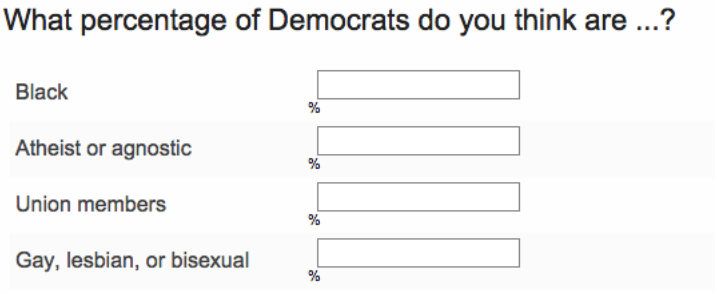
	Sample	Population
Partisanship		
Democratic (inc. leaners)	58.9%	49.0%
Republican (inc. leaners)	22.3%	39.0%
Non-leaning Independent	18.9%	11.9%
Gender		
Female	50.5%	50.9%
Male	49.5%	49.1%
Race/Ethnicity		
White/Caucasian	76.2%	63.7%
Black/African-American	8.1%	12.2%
Asian-American/Pacific Islander	10.1%	4.8%
Native American/Native Alaskan	1.6%	1.1%
Latino/Hispanic	9.7%	16.4%
Education		
Less than high school	0.5%	8.9%
High school diploma (or equiv.)	9.7%	31.0%
Some college	46.1%	28.0%
4-year degree	34.8%	18.0%
Advanced degree	8.9%	9.3%
Age		
18-39	79.1%	39.1%
40-64	19.1%	43.7%
65+	1.8%	17.2%

NOTE: Population estimates come from the 2010 US Census, except for partisanship, which comes from the 2012 ANES.

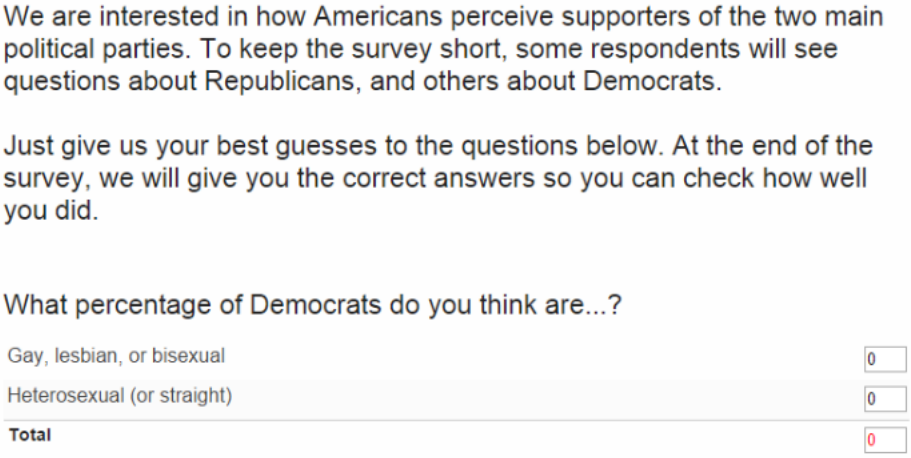
Depiction of Treatments

Figure 6.2: Standard Estimation, Sum-to-100, Base-Rates, and Incentive Conditions

(a) Standard Estimation Condition

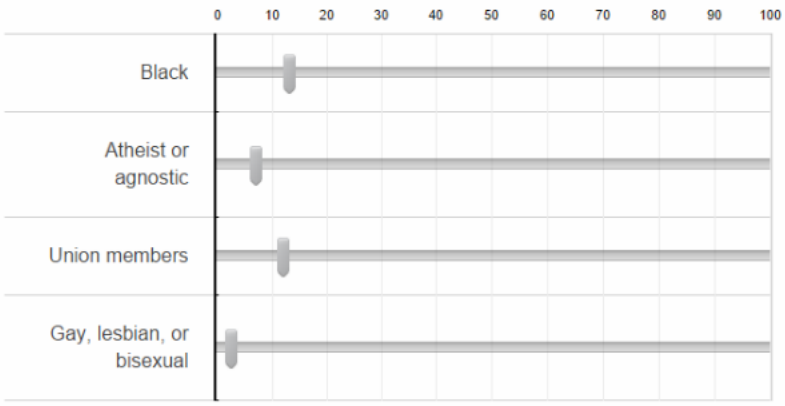


(b) Sum-to-100 Condition



(c) Base Rates Condition

What percentage of Democrats do you think are...?
(Note: each slider starts at the corresponding social group's estimated prevalence in the adult American population as a whole.)



(d) Incentives Condition

We are interested in how Americans perceive supporters of the two main political parties.

Give your best guesses to the questions below. At the end of the survey, we will give you the correct answers so that you can check how well you did.

We will give respondents a bonus of 5 cents for each response that comes within 5 percentage points of the correct answer. (Please do not look answers up though; we are interested in *your* perceptions! Each page has a time limit before it auto-advances. You will receive the bonus within a week of completing the survey.)

Out of every 100 Democrats, how many do you think are ...?

Black		%
Atheist or agnostic		%
Union members		%
Gay, lesbian, or bisexual		%

K-S Tests Comparing All Conditions to the Standard Estimation Condition

In the main text, we fail to find evidence that alternate ways of asking the question yield more accurate (or less biased) perceptions *on average*. However, one possibility is that the changes in how we ask questions leads some individual respondents to provide more plausible responses—i.e., to be less likely to provide overly high or overly low perceptions—without affecting the mean response. To examine this possibility, we compare the distributions of reported perceptions of the percentage of partisans belonging to each of the party-stereotypical groups studied, as provided by respondents assigned to the standard estimation condition and other condition. As the results in the table illustrate, the differences in distribution are minor, with only 2 of 24 comparisons statistically significant. A closer inspection suggests that the Base Rates condition produces distributions that are the most dissimilar to the standard condition. But as we note elsewhere, people in the Base Rates condition are more inaccurate than in the standard condition.

	Incentives		Sum-to-100		Base Rates	
Party-Group	D	Pr(Same dist.)	D	Pr(Same dist.)	D	Pr(Same dist.)
Dem.-Ath./ag.	0.14	0.25	0.11	0.78	0.17	0.12
Dem.-Black	0.08	0.85	0.20	0.12	0.23	0.01*
Dem.-LGB	0.10	0.64	0.19	0.64	0.19	0.06
Dem.-Union	0.09	0.80	0.11	0.8	0.09	0.74
Rep.-$250k+	0.11	0.57	0.09	0.9	0.18	0.07
Rep.-Evangelical	0.12	0.48	0.21	0.08	0.20	0.03
Rep.-Age 65+	0.07	0.96	0.49	0.00***	0.19	0.06
Rep.-Southern	0.07	0.93	0.07	0.93	0.19	0.05

NOTE: Statistics are based on K-S tests comparing the distribution of responses under the condition named at the top of each column to the distribution under the "standard estimation" condition. Asterisks denote statistical significance under the Benjamini and Hochberg (1995) method for controlling the false discovery rate. (Family defined as each set of eight K-S tests comparing responses under two distinct conditions.) * = $p < \alpha^*$ when $\alpha = 0.1$, ** = $p < \alpha^*$ when $\alpha = 0.05$, *** = $p < \alpha^*$ when $\alpha = 0.01$.

Does Partisanship Condition Treatment Effects?

To assess the validity of alternative explanations for the patterns we found in our main study, we compared perceptions across experimental conditions designed to limit these explanations' power in the follow-up study on MTurk. In doing so, we pooled responses by partisanship within condition to maximize power. However, a potential concern is that the experimental conditions may differentially affect respondents based on partisanship. That is, Democrats may respond to some treatments differently than Republicans, or some treatments may more heavily affect perceptions of the in-party or out-party. Essentially, the concern is that there may be an interaction between these experimental treatments and partisanship. If so, pooling across partisanship in the presentation of results masks important information.

We test for these potential interaction effects by regressing perceptions of the percentage of Democrats (Republicans) belonging to each of their parties' four party-stereotypical groups on experimental condition, respondent partisanship, and the interaction of condition and partisanship. (The standard estimation task serves as the baseline condition, while non-leaning independents serve as the baseline for partisanship.) We find few statistically significant average apparent interaction effects. 6% of these interaction coefficients are statistically significant at the $p < .05$ level—nearly exactly the percentage we would expect to be significant by chance. Additionally, as Table 6.6 shows, these apparent effects lack consistency: the individual treatments fail to always push Democrats or Republicans in the same direction. In all, results suggest that treatment effects do not vary appreciably by partisanship.

Table 6.6: Estimating Conditioning of Treatment Effects on Reports of Prevalence of Party-Stereotypical Group Members by Partisanship

	Blacks	Atheists/Agnostics	LGB	Union	Rich	Evangelical	South	Over 65
(Intercept)	35.74***	29.61***	27.96***	32.57***	29.48***	48.22***	48.00***	48.52***
	(4.53)	(4.57)	(5.48)	(4.63)	(5.60)	(5.30)	(4.22)	(4.30)
Incentives	−2.33	−3.75	−5.55	7.57	9.57	−12.31	−10.82	−7.52
	(6.48)	(6.54)	(7.83)	(6.62)	(8.01)	(7.58)	(6.03)	(6.15)
Sum-to-100	−6.99	−6.86	−5.54	7.02	9.63	−6.55	17.78*	2.03
	(7.74)	(7.81)	(9.35)	(7.90)	(10.56)	(10.00)	(7.95)	(8.10)
Base rates provided	−.42	−.35	2.62	−4.41	7.21	3.20	−1.53	4.27
	(6.74)	(6.80)	(8.14)	(6.88)	(8.32)	(7.88)	(6.27)	(6.39)
Democratic PID	−.79	1.17	−2.09	4.50	4.39	−1.88	−6.12	−2.37
	(5.34)	(5.39)	(6.46)	(5.45)	(6.60)	(6.25)	(4.97)	(5.07)
Republican PID	5.51	−4.61	1.79	7.75	−3.54	−3.03	−12.37	−14.71*
	(7.08)	(7.14)	(8.55)	(7.22)	(8.74)	(8.28)	(6.59)	(6.71)
Incentives X Democratic PID	4.65	4.29	11.24	−13.02	−7.34	19.24*	13.05	9.00
	(7.69)	(7.76)	(9.29)	(7.85)	(9.50)	(8.99)	(7.15)	(7.29)
Sum-to-100 X Democratic PID	−.46	.61	3.89	−8.01	−16.43	−2.44	.87	−6.00
	(9.21)	(9.29)	(11.13)	(9.40)	(12.09)	(11.45)	(9.11)	(9.28)
Base rates provided X Democratic PID	7.09	1.64	6.98	4.73	1.24	6.44	16.46*	2.60
	(7.89)	(7.96)	(9.53)	(8.05)	(9.74)	(9.22)	(7.34)	(7.48)
Incentives X Republican PID	10.08	7.39	−1.38	−6.06	−9.50	19.41	10.96	18.42
	(9.97)	(10.06)	(12.04)	(10.17)	(12.31)	(11.66)	(9.28)	(9.45)
Sum-to-100 X Republican PID	−2.83	18.29	5.79	−12.33	−7.87	2.56	4.30	12.45
	(12.53)	(12.64)	(15.14)	(12.79)	(15.12)	(14.32)	(11.39)	(11.60)
Base rates provided X Republican PID	12.99	21.16*	9.13	7.14	.63	11.79	14.04	15.69
	(9.82)	(9.91)	(11.86)	(10.02)	(12.13)	(11.49)	(9.14)	(9.31)
R^2	.09	.05	.04	.03	.04	.07	.15	.06
Adj. R^2	.05	.02	.01	-.00	.01	.04	.12	.02
Num. obs.	331	331	331	331	335	335	335	335

Note: Perceptions of pure independents and the standard estimation condition serve as baselines for partisanship and condition. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Affect Toward Groups is a Weak Predictor of Perceptions of Prevalence of Groups in Parties

Expressive benefits (rather than misperception) are a potential alternative explanation for the apparent misperceptions we find. Under this explanation, respondents dislike particular social groups and also dislike the out-party and, thus, declare the out-party to be excessively composed of groups they dislike. If expressive responding explained our findings, we would expect perceptual errors to be associated with how much a respondent likes a group. At the start of the extremity perceptions experiment we asked respondents to rate the eight groups related to party prototypes on a 101-point feeling thermometer. (We separated these feeling thermometers and the experimental content with a lengthy demographics and political information battery.) If dislike of the groups and the out-party explains our primary descriptive finding, we should find a significant correlation between respondents' feeling thermometer ratings for group g and the reported perceptions of how prevalent g is in party p. However, as the table below shows, we fail to find relationships that are distinguishable from zero for any of the eight group-party dyads.

Table 6.7: Feeling Thermometer Ratings for Groups Fail to Predict Perceptions of Group Prevalence

	Reg. coefficient	Std. error	95% conf. interval	n
Democratic Party Groups				
Blacks	-0.01	0.05	[-0.11., 0.10]	297
Union members	-0.04	0.05	[-0.15, 0.06]	297
Gay, lesbian, & bisexual	-0.07	0.05	[-0.17, 0.03]	297
Atheist/Agnostic	-0.03	0.05	[-0.13., 0.06]	297
Democratic Party Groups				
The rich/earn over $250,000	0.01	0.04	[-0.07., 0.10]	659
Evangelicals	-0.02	0.04	[-0.09, 0.05]	659
Southerners	-0.03	0.03	[-0.09, 0.04]	659
The young/people under 35	0.02	0.02	[-0.03, 0.07]	659

Note: The coefficient is from the regression of response to the question, "What percentage of supporters of party p do you think are members of group g?" on feeling thermometer rating of g.

Results from the Sum to 100 Condition

Full Descriptive Results

Table 6.8: Average Perception of the Percentage of Democrats Belonging to Party-Stereotypical and Complementary Groups

	Mean Perception	True Estimate
Race		
Black	28.4%	24.0%
Non-Hispanic White	41.5%	59.4%
Latino/Hispanic	20.6%	1.0%
Other	9.5%	5%
Religion		
Atheist/Agnostic	25.9%	9%
Protestant	26.4%	45%
Catholic	28.8%	21%
Other	18.9%	24%
Sexual Orientation		
LGBTQ	24.6%	6.3%
Heterosexual	75.4%	93.7%
Union Affiliation		
Union member	36.8%	10.5%
Not a union member	63.2%	89.5%

NOTE: The estimates of the actual percentages of Democrats belonging to racial groups sum to slightly more than 100 because of the way the ANES asks about Hispanic/Latino background separately from race.

Table 6.9: Average Perception of the Percentage of Republicans Belonging to Party-Stereotypical and Complementary Groups

	Mean Perception	True Estimate
Income		
Over $250,000	29.3%	2.0%
$100,000-$250,000	27.6%	20.2%
$50,000-$100,000	23.7%	36.8%
Under $50,000	19.4%	40.8%
Religion		
Evangelical Christian	38.9%	34%
Mainline Protestant	21.4%	20%
Catholic	28.2%	22%
Other	11.6%	19%
Age		
Over 65	44.9%	21.3%
40-64	39.5	45.6%
18-39	15.7%	33.1%
Region		
From the South	39.1%	35.7%
Not from the South	60.9%	64.3%

Respondents Underestimate the Prevalence of Counter-Stereotypical Group Identifiers in Parties

Table 6.10 below presents the results of one-sample t-tests comparing the average perception of the percentage of a party belonging to a counter-stereotypical group to the actual prevalence of that group in the party. It does so for the five group-party dyads in which we provided respondents with more than two groups in the complex task (thus not providing a *de facto* counter-stereotypical group).

Table 6.10: Respondents Underestimate the Percentage of Partisans Who Belong to Counter-Stereotypical Groups

Party-Group Dyad	n	Mean Estimated %	Std. Error	Actual Estimated %	$P(\|T\| > \|t\|)$
Dems.-White	47	41.5	2.1	60	< 0.001
Dems.-Protestant	47	26.4	2.2	45	< 0.001
Reps.-Other relig.	51	11.6	1.8	19	< 0.001
Reps.-Under 40	51	15.7	1.4	33.1	< 0.001
Reps.-Under $50K	51	19.4	2.6	40.8	< 0.001

Perceptions of Base Rates of Party-Stereotypical Groups in the Population at Large

In the alternative explanations study, after measuring party-specific perceptions, we asked respondents assigned to the standard estimation condition to estimate the percentage of the US adult population that belongs to a randomly-assigned subset of the eight party-stereotypical groups. As the table below shows, consistent with previous work (e.g., Wong, 2007), respondents tend to overestimate the prevalence of these groups. However, misperceptions do appear to be party-specific: perceptions of the prevalence of these groups in their associated parties are significantly higher than those for the population writ large. And, importantly, the substantive difference between these party-specific and base rate perceptions tend to be quite large.

Table 6.11: Comparison of Party-Specific Perceptions to Perceptions of Population Base Rates of Party-Stereotypical Groups

Group	Mean Perceived Base Rate	Mean Perceived Party Rate	Difference
Southerners	32.74%	41.94%	-9.20**
Over 65	30.36%	46.54%	-16.18***
Evangelical	35.5%	49.98%	-14.48***
Earning Over $250K	11.4%	28.6%	-17.19***
Black	31.38%	35.96%	-4.58
Atheists or Agnostics	22.93%	28.04%	-5.11+
Union Members	25.74%	33.52%	-7.78**
LGBT	14.86%	27.33%	-12.47**

$+ p < .1$, $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$

Failure to Reduce Mean Absolute Perceptual Error in the Alternative Explanations Conditions

In the main paper, we primarily focus on the similarity of the point estimates of the mean perception of the percentage of party p belonging to group g between conditions. Another way to analyze the data is to stack it so that the unit of analysis is not the respondent, but the respondent-perception. (Each respondent appears 4-8 times in the data set, depending on the condition to which she was assigned.) Stacking the data this way, we can determine whether any of the conditions—Sum to 100, Incentives, or Base Rates Provided—reduce mean absolute perceptual error.

We first do this by regressing mean absolute error for each perception of party p belonging to group g on indicator variables for the three treatments (leaving the Standard estimation condition as the baseline). We include fixed effects for each $g - p$ dyad and cluster standard errors at the respondent level. As the table below shows, neither providing incentives for accuracy nor requiring participants to consider complementary groups and sum the percentage of each group to 100 has any kind of substantively or statistically significant effect. And, consistent with results in the paper, providing base rates for group g actually appears to increase error by 21.8%, on average.

Table 6.12: Impact of Different Ways of Asking about Party Perceptions on Mean Absolute Error in Perceptions

	Mean Directional Error	Mean Absolute Error
Incentives	0.89	0.66
	(2.25)	(1.91)
Sum-to-100	-0.52	-1.94
	(2.03)	(1.74)
Base rates	7.17***	5.05**
	(2.51)	(2.22)
Constant	18.95***	23.13***
	(1.58)	(1.32)
Party-group dyad fixed effects	X	X
R^2	0.09	0.05
Reported perceptions	2664	2664
Respondents	382	382

NOTE: Mean error is the average deviation of a respondent's reported perceptions of the % of party p belonging to group g from the true %. Model estimated via OLS. The "standard" condition serves as the baseline. Standard errors, clustered by respondent, are reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

6.3 Survey Experiments on the Effects of Social Misperceptions of the Parties

MTurk Sample Demographics for The Extremity Perceptions and The Affect Study

Table 6.13: Characteristics of the MTurk Sample

	Extremity Perceptions Study	Partisan Affect Study	Population Estimate
Partisanship			
Democratic (inc. leaners)	59.9%	57.3%	49.0%
Republican (inc. leaners)	20.8%	19.0%	39.0%
Non-leaning Independent	19.3%	23.8%	11.9%
Gender			
Female	45.9%	50.4	50.9%
Male	54.1%	49.6	49.1%
Race			
White/Caucasian	81.4%	79.3%	63.7%
Black/African-American	7.9%	9.4%	12.2%
Asian-American/Pacific Islander	11.1%	8.4%	4.8%
Native American/Native Alaskan	1.7%	2.6%	1.1%
Hispanic/Latino		6.1%	16.4%
Ethnicity			
Latino/Hispanic	8.9%		
Not Latino/Hispanic	91.1%		
Education			
Less than high school	1.3%	0.7%	8.9%
High school diploma (or equiv.)	10.9%	9.5%	31.0%
Some college	40.1%	43.2%	28.0%
4-year degree	35.8%	36.8%	18.0%
Advanced degree	12.0%	9.7%	9.3%
Age			
18-29	48.2%		22.1%
30-49	38.2%		35.7%
50+	13.6%		42.2%
18-39		75.7%	39.1%
40-64		22.7%	43.7%
65+		1.6%	17.2%

NOTE: Population estimates come from the 2010 US Census, except for partisanship, which comes from the 2012 ANES.

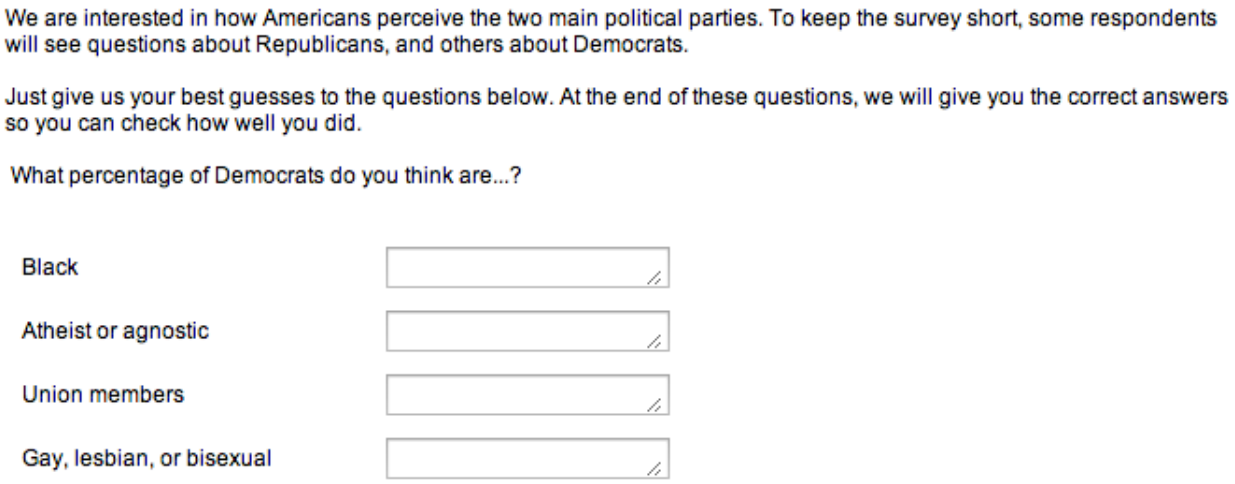
Manipulation in the Survey Experiment

The exact question given to participants in the "tell" condition is shown in Figure 6.3. It is similar to the questions given to participants in the other conditions, except that we told those participants they would learn the correct information and thus be able to check their answers at the end of the survey.

On the screen immediately after these questions, we tailored the correct information based on whether respondents overestimated or underestimated the percentage of the out-party belong to group g. If participants overestimated, they saw this message: "The percentage of Democrats (Republicans) who are g is smaller than you think. Only x% are g. (You overestimated by $[\frac{\text{participant's guess}-x}{x}]$%).

If participants underestimated the percentage of the out-party belonging to g, we showed them this message: "The percentage of Democrats (Republicans) who are g is larger than you think. x% are g. (You underestimated by $[\frac{x-\text{participant's guess}}{x}]$%).

Figure 6.3: Questions in the "Tell" Condition



Manipulation Checks

Reduction of Mean Absolute Error in the "Tell" Group in the Partisan Affect Study

As we note in the paper, in the partisan affect experiment, we not only asked participants assigned to the "tell" condition to estimate the percentage of out-party supporters belonging to four party-stereotypical groups prior to administering the dependent measures, but also in a quiz at the very end of the survey. Participants tended to be more accurate in the latter battery. The mean absolute error (from the true survey-based estimates) decreased drastically for participants' estimates regarding each of the eight group-party dyads, as Table 6.14 shows.

Table 6.14: Manipulation Check: Mean Absolute Error

	Mean Absolute Error	
Group-Party Dyad	Initial	Recall
Reps.-Southerners	19.16	8.61
Reps.-Over 65	29.87	6.79
Reps.-Evangelical	24.96	9.47
Reps.-Earning Over $250K	37.35	3.67
Dems.-Black	24.5	5.9
Dems.-Atheists/Agnostics	24.02	2.87
Dems.-Union Members	32.67	4.48
Dems.-LGBT	28.44	3.09

Individual-Level Manipulation Analysis for the Partisan Affect Study

Since we asked "tell" participants to estimate the percentage of out-party supporters belonging to the four party-stereotypical groups both before and after the manipulation check, we can investigate whether individual participants' perceptions tended to become more accurate. We use a Wilcoxon signed-rank test to do so. Since our data are matched pairs—a pre-test and a post-test for each respondent—we test the null hypothesis that our manipulation failed to affect participants' perceptions. If this hypothesis is correct, then we would expect equal numbers of participants to have more accurate perceptions on the pre-test and on the post-test. The signed-rank test evaluates this sharp null hypothesis for each group-party dyad by determining the sign and absolute difference between each respondent's pre- and post-test reported perceptions, ranking these absolute differences, and then computing the absolute value of the sum of the signed ranks. If the manipulation failed to affect perceptions, we would expect the test statistic to be 0. But as Table 6.15 shows, the test statistic is positive and statistically significant for each of the eight group-party

dyads, implying that the manipulation moved participants' perceptions about out-party composition, and that such change stuck throughout the survey.

Table 6.15: Participants Assigned to the "Tell" Condition Tend to Become More Perceptually Accurate

Party-Group Dyad	n More Accurate	n Less Accurate	Sum Ranks	z	$P > \|z\|$
Dems.-Black	72	14	3138	6.5	< 0.001
Dems.-Atheist/Agnostic	75	6	3572	7.6	< 0.001
Dems.-LGBT	75	11	3508	7.3	< 0.001
Dems.-Union	79	5	3463	7.5	< 0.001
Reps.-Evangelical	132	25	10,553	8.7	< 0.001
Reps.-Over 65	151	14	13,150	10.3	< 0.001
Reps.-Over $250K	163	4	14,425	11.0	< 0.001
Reps.-Southern	123	39	9068	7.2	< 0.001

The Manipulation Appears to Present Novel Information

An alternative notion of the manipulation is not only that it causes participants to learn, but also that the information is novel. We check for manipulation this way by asking participants how much the information presented to them on the "tell" screen surprises them. This task serves a second purpose as well: it provides respondents with a task on the manipulation screen, which both requires participants to spend time reading the information (rather than click through the screen) and deters demand effects by masking the manipulation as a question. We asked respondents how much they were surprised by the "tell" information in both the extremity perceptions and the affect studies. We present the results of this question in the table below.

Table 6.16: "Tell" Participants Appear Surprised by the Information We Present

"Overall, how surprised are you by these statistics?"	E.P. Study	P.A. Study
Very surprised	29.9%	33.3%
Somewhat surprised	38.8%	36.6%
A little surprised	20.9%	18.3%
Not surprised at all	10.5%	11.7%

Wording and Scales for Political Perception Questions

We randomly assigned participants to respond to questions about Democrats first or Republicans first. We asked the following four questions about where participants thought partisans tend to stand politically:

Taxes "Which of the following statements do you think comes closest to what the average Republican Party supporter believes about taxes?" (Subquestion: "How about the average Democratic Party supporter?")

- Decrease federal income taxes on just the highest earners, keeping the tax rate the same on all others
- Decrease federal tax rates for all income groups
- Maintain current levels of federal income taxes on all
- Increase federal income taxes on the highest earners, keeping the tax rate the same on all others
- To address inequality, establish a national maximum income by taxing all income over a certain amount at 100

Abortion "Which of the following statements do you think comes closest to what the average Republican Party supporter believes about abortion?" (Subquestion: "How about the average Democratic Party supporter?")

- By law, abortion should never be permitted
- The law should permit abortion only if the woman's life is in danger
- The law should permit abortion only if the woman's life is in danger or in cases of rape or incest
- The law should permit abortion if the woman's life is in danger, in cases of rape or incest, if there is a serious chance of a birth defect, and at the physician's discretion in the first two trimesters
- By law, a woman should be able to obtain an abortion as a matter of personal choice in the first two trimesters
- By law, a woman should always be able to obtain an abortion as a matter of personal choice

Gay rights "Which of the following statements do you think comes closest to what the average Republican Party supporter believes about gay rights?" (Subquestion: "How about the average Democratic Party supporter?")

- Gay sex should be illegal and punishable by imprisonment, similar to the penalties for committing incest and bestiality

- Gay sex should be legal. However, civil unions or any other formal recognition of gay partnerships should not be allowed
- Same-sex civil unions (but not marriage) should be allowed. However, gay couples should not be allowed to adopt children
- Same-sex civil unions (but not marriage) should be allowed, and gay couples in civil unions should be allowed to adopt children
- Same-sex couples should be allowed to marry each other and adopt children
- Same-sex couples should be allowed to marry each other and adopt children, and the government should require that all schools teach children about homosexual relationships

Race "Which of the following statements do you think comes closest to what the average Republican Party supporter believes about racial and civil rights policy?" (Subquestion: "How about the average Democratic Party supporter?")

- Any laws protecting racial groups should be repealed, including all voting rights and civil rights legislation
- Non-discrimination laws in universities and workplaces should be repealed
- The government should investigate and punish racial discrimination by universities and employers, but hiring or admissions based on race should be illegal
- Universities and employers should be encouraged to consider applicants' backgrounds to improve diversity, but no quotas should be set
- The government should mandate an aggressive affirmative action program in education and the workplace to ensure that certain numbers of underrepresented minorities are hired/admitted
- In addition to affirmative action, the government should provide cash payments to minority groups as reparations for slavery and other past injustices

Table of Results from the Extremity Perceptions Experiment

Table 6.17: Misperceptions Cause People to Attribute Extreme Policy Preferences to the Typical Out-Party Supporter

	DV: Extremity Perception			
	Full Sample		Just Partisans	
	(1)	(2)	(3)	(4)
Perceptual error		0.41***		0.39***
		(0.10)		(0.11)
Assignment to tell	-0.02	0.05	-0.07***	0.04
	(0.02)	(0.06)	(0.02)	(0.07)
Perceptual error X tell		-0.24*		-0.23
		(0.13)		(0.14)
Assignment to ask	-0.01	-0.03	-0.03	-0.01
	(0.02)	(0.06)	(0.02)	(0.07)
Perceptual error X ask		-0.03		-0.06
		(0.13)		(0.15)
Constant	0.30***	0.15***	0.35***	0.17***
	(0.02)	(0.05)	(0.02)	(0.05)
Policy fixed effects	X	X	X	X
R^2	0.04	0.06	0.05	0.06
SER	0.44	0.45	0.46	0.45
Policy perceptions	4144	4144	3236	3236
Participants	1036	1036	809	809

NOTE: In the full sample, non-leaning independents were randomly assigned to the Democratic or Republican treatment arm. The control condition serves as the baseline. Perceptual error is the mean error in respondents' perceptions of the percentage of the out-party belonging to party-stereotypical groups. All variables are scaled 0 to 1. Standard errors, clustered by respondent, are reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

Alternative Specifications of Perceived Extremity, with Results from Extremity Perceptions Experiment

We primarily operationalize perceived extremity as the placement of the typical out-party supporter at the ideologically-orthodox endpoint of the policy scale. That is, we say that a respondent perceives extremity in the out-party if he locates the typical Democrat at the most liberal scale point, or if he locates the typical Republican at the most conservative scale point. This comports most closely with our hypothesis that social perceptions of the out-party as highly composed of prototypical identifiers cause people to see typical out-party supporters as intense policy demanders. It further allows clean interpretation: the results are easily read as comparisons of proportions of participants who place Democrats (Republicans) at the scale end-point.

However, there are other ways to operationalize perceived extremity. We focus on three here, which, unlike our primary operationalization, are non-binary.

First, one may operationalize perceived extremity as the degree to which participants place Democrats toward the liberal scale endpoint and Republicans toward the conservative scale endpoint. That is, one may rescale the partisan placements such that placing a Democrat (Republican) at the extreme conservative (liberal) endpoint is 0, and placing a Democrat (Republican) at the extreme liberal endpoint is 0, with equally separated values at each point in between. The results using this "Raw, directional placement" measure of extremity perception are presented in columns 1-4 of Table 6.18.

This measure is problematic, though. From a face validity standpoint, it's difficult to claim that placing the typical Democrat at the conservative endpoint (e.g., "Abortion should always be illegal") is the opposite of perceived extremity. And since most respondents "get it right"—that is to say, most respondents place Democrats and Republicans on the "correct" ideological side of the spectrum—the bulk of the density is above 0.5, so the effective range of the measure is [0.5, 1]. But the handful of perceptions that are on the "wrong" side add significant noise to the estimates of treatment effects, and importantly, since these perceptions are far away from the bulk of the data and likely reflect significant measurement error, they produce significant attenuation bias.

One way to deal with this problem is through a process similar to winsorising the data (Wilcox and Keselman, 2003). We recode any values on the "wrong" side of the distribution—that is values less than 0.5—to 0.5. The results using this winsorised measure are presented in columns 5-8.

Finally, one could operationalize extremity as the absolute distance of one's perceived perception of the typical out-partisan's position from the scale midpoint. Since this measure is agnostic about directionality, this operationalization deviates most significantly from our hypothesis regarding perceptions of partisans as intense policy demanders, in a manner consistent with the perceived interests of the parties' core groups. However, it also has stronger face validity as a pure measure of perceived extremity. The results using this measure are presented in columns 9-12.

As the table shows, results are similar across all operationalizations, and all these results

are similar to those presented in the paper: the "Tell" condition appears to significantly reduce perception of extremity in the out-party (or randomly-assigned party, in the case of non-leaning independents), and we consistently find evidence of an interaction between the "Tell" treatment and the degree to which participants saw the out-party as composed of stereotypical identifiers prior to receiving treatment. (As in the paper, the statistical significance of this interaction term hovers around the $p = 0.10$ mark here.)

Table 6.18: Results of the Extremity Perceptions Experiment Under Different Specifications of the Dependent Measure

	DV: Raw Placement, Directional				DV: Winsorised Placement, Directional				DV: Absolute Distance of Placement from Scale Midpoint			
	Full Sample		Just Partisans		Full Sample		Just Partisans		Full Sample		Just Partisans	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Perceptual error		0.15**		0.14**		0.22***		0.20**		0.12*		0.11*
		(0.06)		(0.06)		(0.08)		(0.09)		(0.06)		(0.07)
Assignment to Tell	-0.03**	-0.02	-0.02**	0.01	-0.05**	0.03	-0.05**	0.02	-0.04***	0.04	-0.05***	0.03
	(0.01)	(0.04)	(0.01)	(0.05)	(0.02)	(0.06)	(0.02)	(0.06)	(0.02)	(0.04)	(0.02)	(0.05)
Perceptual error X Tell		-0.04		-0.07		-0.16		-0.15		-0.18**		-0.17*
		(0.08)		(0.09)		(0.11)		(0.12)		(0.09)		(0.10)
Assignment to Ask	-0.01	-0.02	-0.00	0.01	-0.03	-0.02	-0.02	-0.01	-0.04**	-0.04	-0.03*	-0.03
	(0.01)	(0.04)	(0.01)	(0.05)	(0.02)	(0.05)	(0.02)	(0.06)	(0.02)	(0.04)	(0.02)	(0.05)
Perceptual error X Ask		0.02		-0.03		-0.02		-0.04		0.01		-0.01
		(0.08)		(0.09)		(0.11)		(0.12)		(0.09)		(0.10)
Constant	0.75***	0.68***	0.76***	0.69***	0.57***	0.47***	0.58***	0.49***	0.63***	0.57***	0.64***	0.59***
	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.04)	(0.01)	(0.04)	(0.01)	(0.03)	(0.01)	(0.03)
Policy fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
R2	0.04	0.05	0.04	0.04	0.05	0.05	0.05	0.05	0.05	0.05	0.05	0.05
SER	0.24	0.24	0.23	0.23	0.36	0.35	0.35	0.35	0.31	0.31	0.31	0.31
Policy perceptions	4008	4008	3236	3236	4008	4008	3236	3236	4008	4008	3236	3236
Participants	1002	1002	809	809	1002	1002	809	809	1002	1002	809	809

Experimental Results: Independent Participants

Extremity Perceptions Experiment

Table 6.19: Effect of the Experimental Treatments (Baseline = Control) on Independents' Perceptions of Partisans' Extremity

	Coefficient	Std. Error	$p > \|t\|$
Ask	-0.07	.04	0.11
Tell	-0.01	0.04	0.74
Constant	0.52	0.03	0.00
R^2	0.02		
n	193		

Social Distance Experiment

Table 6.20: Effect of the Experimental Treatments (Baseline = Control) on Independents' Affect Toward a Party (Randomly Assigned)

	Coefficient	Std. Error	$p > \|t\|$
Ask	-0.02	.02	0.41
Tell	0.01	0.02	0.62
Constant	0.52	0.03	0.00
R^2	0.01		
n	154		

Regression Tables for Affect Experiments

Table 6.21: Misperceptions Cause People to Feel Partisan Animus

	DV: Out-party FT		DV: Social distance	
	(1)	(2)	(3)	(5)
Perceptual error		-0.10		0.04
		(0.07)		(0.08)
Assignment to tell	0.06***	-0.00	-0.03*	-0.00
	(0.02)	(0.05)	(0.02)	(0.04)
Perceptual error X tell		0.14		-0.06
		(0.10)		(0.07)
Assignment to ask	0.01	-0.03	-0.01	-0.03
	(0.05)	(0.06)	(0.01)	(0.04)
Perceptual error X ask		0.08		0.04
		(0.10)		(0.08)
Constant	0.27	0.31	0.66	0.64
	(0.01)	(0.03)	(0.01)	(0.03)
R^2	0.01	0.05	0.00	0.01
SER	0.22	0.22	0.16	0.16
Participants	808	808	664	664

NOTE: Partisan respondents only. The control condition serves as the baseline. Perceptual error is the mean error in respondents' perceptions of the percentage of the out-party belonging to party-stereotypical groups. All variables are scaled 0 to 1. Feeling thermometer coded such that 0 = low affect and 1 = high affect; social distance coded such that 0 = low social distance, 1 = high social distance. Standard errors are reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

6.4 Observational Evidence from the 2015 IGS-California Poll

Sample Demographics and Characteristics

The table below compares the distribution of demographic covariates in the SSI sample used throughout the paper to other established benchmarks.

	IGS Poll (August 2015)	Weighted IGS Poll (August 2015)	CCES (September 2012)	Weighted CCES (September 2012)	CA Census (2010)	CA Sec. of State (2015)
Sampling frame	population quota (SSI)		population quota (YouGov)		population census	registered voters census
Age						
18-29	26%	27%	18%	24%		
30-39	17%	20%	9%	16%		
40-49	12%	10%	13%	17%		
50-64	28%	27%	36%	26%		
65 & over	17%	17%	23%	17%		
Gender						
Female	59%	51%	50%	51%	50%	
Male	41%	49%	50%	49%	50%	
Race						
Asian	14.5%	17.8%	5%	8%	17%	
Black/African-American	4%	7%	9%	7%	8%	
Hispanic/Latino	19%	28%	20%	21%		
Native American	2%	1%	1%	1%		
White/Caucasian	61%	47%	59%	56%	75%	
Ethnicity						
Hispanic or Latino					38%	
Not Hispanic or Latino					62%	
Education						
Some high school	1%	12%	3%	11%	12%	
High school/GED	13%	24%	17%	23%	24%	
Some college/2-year degree	36%	35%	28%	31%	34%	
4-year college degree	33%	18%	25%	19%	18%	
Graduate/professional degree	17%	11%	13%	10%	11%	
Party registration/ID						
Democratic	46%	44%	44%	40%		43%
Republican	24%	34%	24%	22%		28%
No party preference	27%	30%	22%	28%		24%
Other	3%	3%	10%	10%		5%

Average Perceptions of Pr(group — party)

Table 6.22: "What percentage of Democrats/Republicans living in California do you think are...?"

	True %	Mean Perception	n
Democratic Party Groups			
Atheist/agnostic	19.7	27.6 [26.6, 28.5]	2103
Black	10.8	33.5 [32.2, 34.8]	1071
Latino	25.4	43.3 [42.0, 44.6]	1034
LGBT	6.3	29.2 [28.2, 30.3]	2106
Union members	10.9	36.2 [35.2, 37.1]	2104
Republican Party Groups			
Age 65+	25.2	37.6 [36.7, 38.5]	2103
Earn over $250k	2.2	35.8 [34.7, 36.9]	2104
Evangelical	42.1	35.5 [34.1, 36.9]	1059
Mormon	3.0	22.0 [20.6, 23.5]	1044

Dependent Measures

In the paper, we show that perceptions about party composition, interacted with affect toward party-stereotypical groups, predict partisan voting behavior and affective polarization. These two dependent measures are indices constructed from individual items.

Partisan Voting

The "partisan voting behavior" index is constructed from three items:

1. Because California uses a "top-two" format for primary elections, voters from any party can vote for candidates for any party. Thinking about your own district, how likely are you to vote for a (out-party candidate) in the 2016 congressional primary? (5-point response scale ranging from "Definitely will vote for a (out-party candidate)" to "Definitely will not vote for a (out-party candidate)")

2. How upset would you be if (an out-party candidate) won the presidential election in 2016? (5-point response scale ranging from "Extremely upset" to "Not upset at all")

3. Would you consider switching your political party registration in the future? (5-point response scale ranging from "Never" to "It's very possible")

Cronbach's alpha for this index is 0.67. While this only implies moderate inter-item reliability, a principal component analysis offers evidence of validity:

Table 6.23: Principal Components/Correlation

Component	Eigenvalue	Difference	Proportion	Cumulative
1	1.84	1.16	.61	.61
2	.68	.20	.23	.84
3	.48	–	.16	1

Table 6.24: Principal Components (Eigenvectors)

Item	Comp. 1	Comp. 2	Comp. 3
House voting	.62	-.14	.77
Presidential voting	.57	-.59	.57
Party switch	.54	.79	.28

Affective Polarization

The items in the social distance index are identical to the one used in the MTurk social distance experiment. Cronbach's alpha is 0.71 for respondents asked about their out-party (identical to the alpha calculated in the MTurk experiment), while it is 0.75 for respondents asked about their in-party. Once again, PCA offers evidence of validity:

Table 6.25: Principal Components/Correlation

Component	Eigenvalue	Difference	Proportion	Cumulative
1	2.46	1.85	.61	.61
2	.61	.09	.15	.77
3	.52	.10	.13	.90
4	.42	–	.10	1

Table 6.26: Principal Components (Eigenvectors)

Item	Comp. 1	Comp. 2	Comp. 3	Comp. 4
Marriage	.50	-.71	.77	.38
Neighbor	.53	-.08	.57	-.79
Work	.46	.88	.09	-.04
Honorary degree	.51	.18	-.69	.48

Results: Regression Tables

The tables below formalizes the results presented in Figure 5.7. They also show that the relationships depicted in the bivariate context in the figure do not change when controlling for strength-of-partisanship in a multivariate regression. This is notable because we might expect strong partisans to also dislike out-party-stereotypical groups and like in-party-stereotypical groups more than weak/leaning partisans, and we further might expect strong partisans (who tend to be more politically interested) to more significantly overestimate Pr(prototypical — party)—but these likely relationships do not explain the apparent trend in the observational data.

As the first table shows, respondents who dislike their out-party's stereotypical groups and who see those groups as composing large portions of the out-party also report more strongly partisan voting attitudes. They further feel greater social distance from the out-party, consistent with the experimental results.

Table 6.27: IGS-California Poll: Summary of Results, Out-Party Items

	DV: Partisan voting		DV: Social distance	
	(1)	(2)	(3)	(5)
Perception X neg. affect	0.45***	0.41***	0.29***	0.28***
	(0.04)	(0.04)	(0.03)	(0.03)
Strong partisan		0.21***		0.06***
		(0.01)		(0.01)
Constant	0.56	0.48	0.54	0.52
	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.06	0.25	0.07	0.11
SER	0.23	0.21	0.14	0.14
Respondents	1815	1815	925	925

NOTE: The analyses use data from partisan respondents only. All variables are scaled 0 to 1. Standard errors are reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

Interestingly, we find analogous relationships for in-party perceptions. Respondents who dislike their in-party's stereotypical groups and see their own party as highly composed of those groups are less partisan in their voting attitudes. And as one might expect, they feel more socially distant from the out-party.

Table 6.28: IGS-California Poll: Summary of Results, In-Party Items

	DV: Partisan voting		DV: Social distance	
	(1)	(2)	(3)	(5)
Perception X neg. affect	-0.49***	-0.32***	0.45***	0.30***
	(0.07)	(0.06)	(0.07)	(0.06)
Strong partisan		0.21***		-0.14***
		(0.01)		(0.01)
Constant	0.69	0.59	0.33	0.41
	(0.01)	(0.01)	(0.01)	(0.01)
R^2	0.03	0.21	0.05	0.21
SER	0.23	0.21	0.17	0.15
Respondents	1814	1814	889	889

NOTE: The analyses use data from partisan respondents only. All variables are scaled 0 to 1. Standard errors are reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

Chapter 7

Mass Polarization and Elite Polarization

In the study of polarization and party conflict, scholars typically distinguish between mass polarization and elite polarization (e.g., Fiorina and Abrams, 2008). One reason for this is that the primary dimension on which Democratic and Republican citizens have polarized is different from the primary dimension on which elites have polarized.

The nature of elite polarization is the increasing consistency with which Democratic politicians to take liberal positions and Republicans take conservative ones (e.g., McCarty, Poole and Rosenthal, 2006). While similar ideological polarization has occurred among the most politically attuned rank-and-file party supporters (e.g., Abramowitz, 2010), most citizens lack the ideological consistency that characterizes contemporary politicians (Ahler and Broockman, 2016; Baldassarri and Gelman, 2008).

Instead, citizens are primarily polarized on an affective dimension. Over the same time that elites' policy positions have become more distinct, citizens' evaluations of the two parties have polarized—with attitudes toward out-party supporters becoming especially negative (Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2014). And since moderate partisans appear to dislike the out-party just about as much as extreme partisans, ideological polarization fails as an explanation for this heightened animus.

Whether Democratic and Republican elites have become more affectively polarized is unclear. Certainly, public debate between the two parties-in-government has become more uncivil in recent decades (Jamieson and Falk, 2000; Uslaner, 1993). This may reflect genuine dislike, but it may also be strategic. Competition for control of policymaking institutions has tightened in recent decades. Lee (2016) argues that this has affected inter-party relations in Congress, with the parties working harder on "collective enterprises to promote their own party's image and undercut that of the opposition" (p. 4). The way they do this, Lee asserts, is by attempting to magnify the public's perception of partisan differences. Importantly, the distinctions the parties attempt to draw are often non-ideological—*any* source of disagreement may be magnified for messaging purposes. This "strategic disagreement" (as per Gilmour, 1995), rather than genuine animus, may explain the apparent increase in elite

partisan incivility.

Uncivil discourse, in and of itself, is a relatively minor consequence of elite polarization. More concerning are those consequences with tangible effects on the American economy or government performance, like gridlock on must-pass legislation and the seemingly constant threat of a government shutdown. Political scientists generally assert that the parties' strong incentives to disagree with each other give rise to these "process consequencess" of polarization. That is, despite being internally cohesive, today's parties are far from "responsible" in the way they pursue party government. Instead, the separated institutional system, with its supermajoritarian rules, multiple veto points, and frequent elections, gives rise to "irresponsible partisanship" when parties are strong (e.g., Lee, 2016; Mann and Ornstein, 2012; Ranney, 1954).

Holding elected officials accountable for irresponsible partisanship—and the governmental failures it has yielded, some which are detailed in the following paper—is one of the democratic citizen's most important duties. But to hold the government accountable for these failures, citizens must understand why they occur. Since citizens are primarily polarized affectively, one concern is that they may believe that party elites are genuinely motivated by ill-will toward the other side (or love of their own party) when they observe elite incivility and irresponsible partisanship more broadly. Thus, the following paper asks, "When citizens observe party conflict, how do they explain it?"

This is ultimately a question of *attribution*. In social perception, people seek to understand others' intentions when they observe an action, especially if that action is novel or unexpected (e.g., Churchland, 1991). Despite theories of attribution emphasizing ways in which humans may arrive at likely conclusions, empirical research finds biases in how people explain others' actions (e.g., Ross, 1977). The following paper considers one apparent, recently noted attribution bias: people tend to explain their own groups' role in conflict as motivated by love of fellow group members, while they explain out-groups' role in conflict as motivated by hatred for the groups they are in conflict with. Waytz, Young and Ginges (2014) document this attribution bias among Democrats and Republicans and note that it has the potential to fuel "intractable conflict" of the kind that we observe among the political class today. This dour conclusion implies that citizens do, in fact, interpret party conflict through the affect they ascribe to the parties-in-government. However, the study fails to consider whether citizens see the parties as motivated by strategy—as we might hope that competent citizens with a grasp of contemporary American politics would.

The following paper gives citizens such an opportunity. In doing so, it attempts to illuminate the source of an outstanding democratic accountability failure: the continued use of irresponsible partisanship as a messaging strategy despite citizens' clear distaste for its consequences.

Chapter 8

Irresponsible Partisanship and Democratic Accountability: How Citizens Understand Party Conflict

Seemingly intractable partisan conflict is the new norm in American politics. As the parties have polarized, Democratic and Republican politicians have become increasingly and openly uncivil toward each other (Jamieson and Falk, 2000; Uslaner, 1993) and government dysfunction has waxed tremendously. Gridlock was the outcome on a striking 71% of major agenda items in the 112th Congress (Binder, 2015). Even more concerning are the acute symptoms of party conflict. Between 2011 and 2016, partisan skirmishes in Washington caused the U.S. to suffer its first-ever credit downgrade (2011), forced a 16-day government shutdown estimated to have cost the economy $24 billion (Hicks, 2013), and threatened another shutdown, resolved only by the resignation of Speaker of the House John Boehner (2015). Despite being internally cohesive and polarized from each other, today's parties-in-government are far from the "responsible parties" past political scientists hoped they might be (APSA 1950).

Unsurprisingly, citizens disapprove of this irresponsible partisanship. Congressional approval hit an all-time low of 7% in 2014 (Riffkin, 2014) and, more generally, many have noted correspondence between partisan polarization and citizens' dissatisfaction with government (e.g., Durr, Martin and Wolbrecht, 1997; Fiorina and Abrams, 2009; Ramirez, 2009). While political science has primarily focused on polarization's implications for *policy* representation—namely, the purported "disconnect" between ideological politicians and ideologically-innocent voters (e.g., Bafumi and Herron, 2010)—citizens may also rue elite polarization for its comorbid effects on the political *process*: the dysfunction, incivility, and brinkmanship described above (Harbridge, Malhotra and Harrison, 2014; Flynn and Harbridge, Forthcoming; Hetherington, 2008; Hibbing and Theiss-Morse, 2001; Lee, 2009). In what follows, I present original survey evidence demonstrating that citizens disapprove of polarization primarily because of these process consequences.

Party conflict's process consequences thus present a democratic accountability puzzle,

particularly because they often emerge from elites' strategic choices and in public view—despite citizens' distaste for the outcome. The 2013 shutdown is a prime example. Planned months in advance by the Republican elite, the shutdown itself was preceded by a massive public relations blitz despite strong opposition to the tactic, even among conservative citizens (Stolberg and McIntyre, 2013). By many accounts, the shutdown was part of a broader messaging strategy: many in the GOP believed the tactic would make President Obama and congressional Democrats appear intransigent, which would bolster support for repealing the Affordable Care Act (Joseph, 2013; Stolberg and McIntyre, 2013). In addition to journalistic accounts, shutdown participants provided direct evidence: Senators McConnell and Paul of Kentucky were caught by a hot microphone discussing how to best use the shutdown for messaging purposes (Everett, 2013), and Representative Bachmann of Minnesota was even more candid, noting, "This is about the happiest I've seen members in a long time, because we see we are starting to win this dialogue on a national level" (qtd. in Scheiber, 2013).

More generally, existing scholarship concludes that the parties-in-government often strategically manufacture or intensify political conflict. Ranney (1954) first noted the potential for polarized parties to behave irresponsibly under the U.S. system of institutions and elections; his wisdom has been borne out. Today's polarized parties are said to engage in "partisan misbehavior" (Mann and Ornstein, 2012), "hostage taking" (Mann and Ornstein, 2012), "partisan bickering" (Lee, 2009), "strategic disagreement" (Gilmour, 1995), "blame-game politics" (Groseclose and McCarty, 2001; Smith, 1988), and even "guerrilla-style tactics" (Schickler, 2001) to articulate their agendas and advance their short-term interests. These tactics are characterized by their effects on the process—indeed, in cases of "strategic disagreement" and "blame-game politics," genuine disagreement between the parties is either minimal or of little importance as a motive. Thus, irresponsible partisanship is often deployed as an electoral and messaging strategy—especially when control of policymaking institutions is closely contested (Lee, 2016).

Given that citizens disapprove of irresponsible partisanship, why do elites use it so brazenly as a messaging strategy? And ultimately, why doesn't the public hold its democratically-elected government accountable for irresponsible partisanship?

One possible explanation is that citizens are just as strategic as the elites who represent them. That is, Democrats (Republicans) may dislike irresponsible partisanship but accept its use to advance the Democratic (Republican) agenda and improve Democrats' (Republicans') electoral fortunes. Harbridge and Malhotra (2011) find that the strongest partisans do support partisan action by individual legislators while punishing Congress as a whole for conflict. However, weak and leaning partisans and independents—the bulk of citizens—generally prefer legislators who project bipartisanship. Moreover, Harbridge and Malhotra (2011) did not investigate attitudes toward irresponsible partisanship or process consequences, but simply toward bipartisanship (or lack thereof) in legislators' behavior. By contrast, Flynn and Harbridge (Forthcoming) investigate citizens' reactions to gridlock and find, surprisingly, that citizens often prefer *out-party* policy victories to gridlock. (Even on the hotly-contested issue of gun regulation, Flynn and Harbridge [Forthcoming] find that partisans, on average, are indifferent between status-quo preserving gridlock and an out-party victory.) Thus, citizens'

own strategic considerations are unlikely to sustain irresponsible partisanship.

A second explanation for irresponsible partisanship in the face of popular disapproval is that the public fails to observe the strategic nature of party conflict. Under this explanation, citizens interpret party conflict not through elite strategy, but instead through the affective motives they attribute to the parties in conflict. Building on the attribution literature in psychology (e.g., Hewstone, 1989; Pettigrew, 1979; Ross, 1977), Waytz, Young and Ginges (2014) find that partisans tend to attribute their party's role in conflict to in-group love while attributing the out-party's role to out-group hatred. As such, partisans may see their out-party as purely vengeful when they observe irresponsible partisanship, and their own party as justified in responding accordingly. For this reason, Waytz, Young and Ginges (2014) argue that the bias they document has the potential to fuel intractable political conflict.

This provokes a pessimistic conclusion about democratic competence. In particular, it suggests that citizens may support their own party's role in irresponsible partisanship—despite their opposition to its consequences—out of blind party-following and motivated emotional reasoning. Moreover, it suggests that citizens fail to connect suboptimal democratic outcomes to elite strategy, instead interpreting them simply as the result of ill-will on the part of the out-party. If true, then latent public demand for institutional reforms is likely weak.

Alternatively, irresponsible partisanship may simply be an institutional moral hazard problem. That is, the public and politicians alike recognize that conflict is often strategic and that citizens dislike the resultant process consequences, but single-member districts shield individual politicians from bearing blame for collective irresponsibility (similar to Fenno's [1978] paradox). And if this is the case, then the two-party system further gives rise to a textbook prisoner's dilemma in which irresponsibility strictly dominates responsible party government.

Two variations on this explanation exist. First, citizens may be highly attuned to the reality of party conflict: they may fully understand that intense party conflict is often strategic in nature, and moreover, can trace institutional arrangements to irresponsible partisanship. If so, then the onus for continued irresponsibility falls entirely on party elites for precluding reform.

On the other hand, citizens may fall somewhere between uninformed and fully informed about the nature of party conflict. Under this explanation, citizens understand that intense party conflict is strategic but fail to connect institutional arrangements to partisan misbehavior and suboptimal outcomes. In sum, citizens' and elites' asymmetric information about how institutions structure elites' incentives to be responsible, rather than partisans' asymmetric beliefs about the parties' motives, may sustain irresponsible partisanship and its consequences.

This paper is a first step toward adjudicating between these explanations. With original survey data, I find that citizens recognize that party politicians' strategic motives, not just political passion, drive conflict. And while partisan citizens are more likely to attribute positive affective motives to their own party and negative ones to the out-party (consistent with Waytz, Young and Ginges [2014]), they more strongly attribute conflict to strategy, and

do so consistently across party lines. However, I also show that while citizens favor relatively mild electoral and political reforms that have little chance of curbing party conflict's process consequences, they reject more substantial, potentially impactful, reforms. Thus, I conclude that citizens generally recognize the strategic nature of party conflict—but that they may be blind to how policymaking institutions structure the parties' strategic incentives to behave irresponsibly.

Citizens Disapprove of Irresponsible Partisanship and its Process Consequences

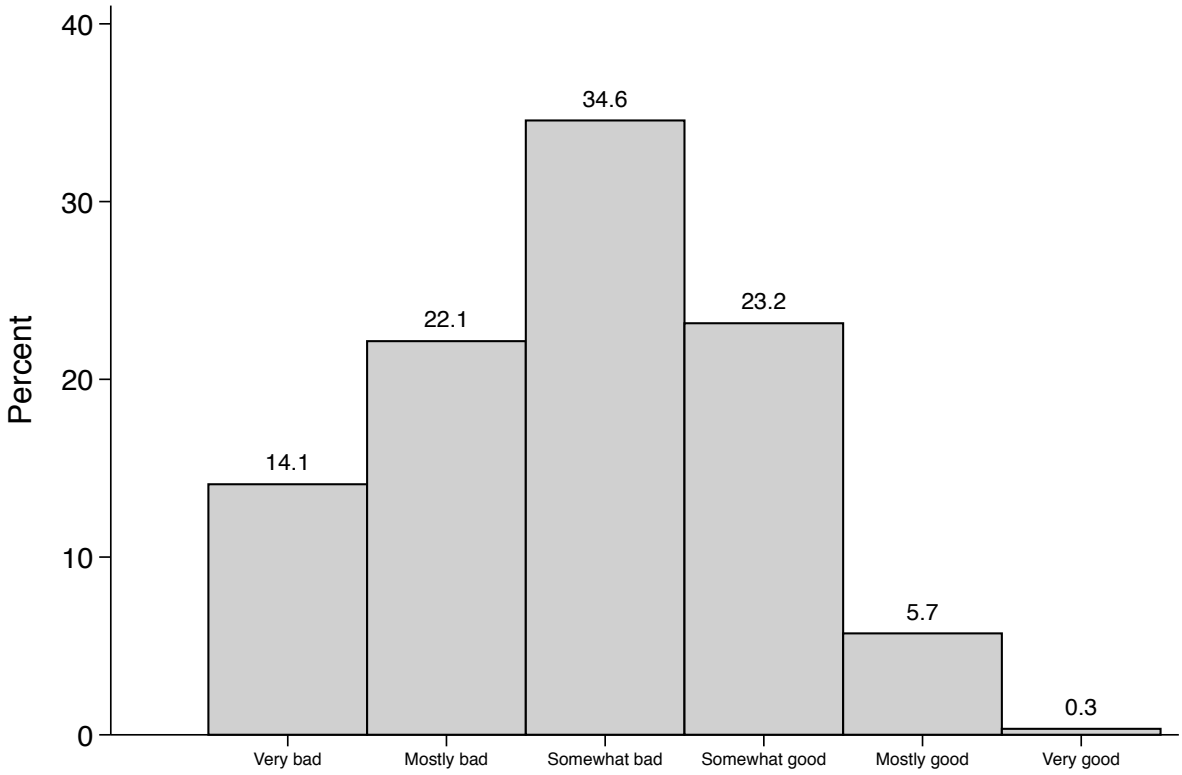
Irresponsible partisanship is puzzling as a political strategy because citizens (the messaging audience) appear disgusted by such gamesmanship and its effects. I provide new evidence that this is the case. The 2015 IGS-California Poll asked a high-quality sample of Californians, recruited via Survey Sampling International (SSI) whether they believe elite party polarization to be good or bad. (The full question text is shown alongside the distribution of responses in Figure 8.1. See see the Supporting Information [SI], section 9.1, for demographics and additional details on the sample. While the sample was a non-probability sample, as most online samples are, sample demographics correspond well to the target population, and all analyses use survey weights to account for purposeful oversampling of Latinos and Asians for other projects included in the poll.)

70.1% of respondents reported that polarization was at least "somewhat bad," as Figure 8.1 shows. By contrast, just 29.9% reported that polarization has been a net positive, with just 0.3% saying it's been "very good."

Respondents were then asked to provide reasons for their assessment of polarization. They were given a number of often-cited pros or cons of polarization (depending on their response to the previous question) and were instructed to pick and rank up to three. (They could also provide open-ended responses, although few did.) In Table 8.1, I present the results for participants who saw polarization as at least somewhat negative. (See 9.1 for responses from the 30% who believed polarization to be positive). As the second column demonstrates, citizens primarily lament polarization for its process consequences. And as the third column demonstrates, over 60% of respondents cited a process consequence as their primary reason for disliking polarization, compared to just 37.2% citing policy considerations generally. (Interestingly, just 11.4% cited partisan extremity, the most oft-lamented effect of polarization, in any form.)

If polarization is indeed responsible for citizens' increasing dissatisfaction with Washington, as many have argued, then its process consequences appear to be the reason why. But this is puzzling in that a large body of existing scholarship concludes that gridlock, incivility, and grandstanding often emerge from conflict strategically manufactured for public consumption. One potentially troubling possibility is that citizens overlook the strategic nature of elite partisan conflict, believing its intensity to be genuine rather than manufac-

Figure 8.1: "There is a lot of talk today about elite political polarization, that is, Democratic and Republican leaders increasingly having difficultly agreeing about government policies. In your opinion, is this polarization a good thing or a bad thing?"



NOTE: Data from the 2015 IGS California Poll. $n = 294$. Survey weights used to account for oversampling of certain demographic groups.

tured. If so, they may be easily mobilized to support their party even in the face of process consequences. Moreover, if citizens believe that interpersonal affect, rather than strategic incentives, fuels intense conflict, they may be less likely to believe that institutional reforms can change Washington.

Testing Whether Citizens see Irresponsible Partisanship as Strategic or Affect-Driven

A recent, prominent study takes this alternative as a given. Waytz, Young and Ginges (2014) investigate the mass-level, psychological roots of intractable political conflict. In doing so, they document an apparent cognitive bias known as *motive attribution asymmetry*

Table 8.1: Why Citizens View Polarization Negatively

	% citing	% ranking as worst consequence
"Neither party represents my personal mix of issue positions very well."	35.6	25.8
	[29.1, 42.4]	
"Polarization has made politics nasty and uncivil."	48.5	21.8
	[41.6,55.4]	
"Gridlock in Congress has prevented important legislation."	51.5	21.7
	[44.6, 58.4]	
"The parties focus too much on grandstanding and not on solving problems."	63.8	18.2
	[57.2, 70.4]	
"The parties want policies that are more extreme than what I want."	27.8	6.9
	[21.6, 33.4]	
"Republicans are too conservative."	14.9	2.6
	[10.0, 19.8]	
"Democrats are too liberal."	9.5	1.9
	[5.5, 13.6]	
"Media has become too biased."	30.6	1.2
	[24.2, 37.0]	
Other	0.6	
	[0, 1.8]	

NOTE: 95% confidence intervals presented in brackets for "% citing." I do not present confidence intervals for "% ranking as worst consequence" because of strict dependence between the selection of individual items.

and argue that it sustains intense political conflict. Under this explanation, Democratic and Republican supporters explicitly believe that their own party engages in conflict out of empathy for co-partisans, but that the other party does so out of out-group animus. As a result, Waytz, Young and Ginges (2014) suggest, citizens are less likely to support negotiation and compromise solutions.[1]

If people genuinely perceive party elites as affectively-motivated, and asymmetrically so by love and hate across party lines, then they likely view their own party's role in conflict as justified and the other party's as illegitimate. Thus, affective reasoning about politics may explain the regularity with which we observe irresponsible partisanship.

However, alternative explanations exist. These alternatives are especially troubling because, remarkably, Waytz, Young and Ginges (2014) fail to consider the strategic aspect of political conflict. The study's attribution task asks respondents to rate the plausibility of six potential motives for Democrats' or Republicans' "engaging in conflict with the opposing party." However, all six of these potential explanations are affective, with three capturing "in-group love" and the other three "out-group hate." Thus, the design provides no leverage for assessing the degree to which citizens reason about American party conflict through their explicit beliefs about the parties' affective motives, because it fails to provide alternative types of motives that citizens may see as more plausible. For similar reasons, one may worry that participants rated affective motives as more plausible than they actually believed

[1]Waytz, Young and Ginges's (2014) direct evidence on this point comes from Study 4, which uses the Israel-Palestine conflict as a context, rather than American party conflict.

them to be. Since the attribution battery only provided positively- and negatively-valenced affective explanations for conflict, and because partisans face consistency pressures when responding to questions about the parties (e.g., Lodge and Taber, 2013), they may have ascribed positive motives to their own party and negative ones to the out-party expressively rather than genuinely.

Other work in political psychology offers a reason to believe that citizens recognize conflict as strategic. Affective intelligence theory suggests that while citizens typically process political information automatically, unusual or threatening circumstances lead them to break from habit, pay closer attention to politics, and search for information (Marcus, Neuman and MacKuen, 2000). Irresponsible partisanship may render citizens *more* likely to observe elites' strategic motives for conflict, as they often involve intense rhetoric and tangible consequences—e.g., credit defaults and government shutdowns—the very circumstances that should lead citizens to seek out novel information. Moreover, information about the parties and the process is unusually available in times of intense conflict. Rather than being sustained by blindness to elites' strategic motives, irresponsible partisanship may actually lead citizens to recognize those motives.

In sum, one view holds that citizens' reliance on affective explanations for political conflict sustains such conflict, and that "curing" motive attribution biases might help citizens to hold politicians accountable. However, an alternate view suggests that citizens recognize the strategic nature of intense party conflict, but that an institutional moral hazard problem sustains partisan irresponsibility.

Hypotheses

In what follows, I first compare the degree to which people attribute strategic motives to the parties, vis-à-vis affective ones, when they think about political conflict. Building on Waytz, Young and Ginges (2014), the null hypothesis is that people see the parties as motivated primarily by group affect. Alternatively, if citizens appear to attribute elite conflict to party strategy as much or more than group affect, we must reconsider the view of citizens as political naïfs.

Second, I assess the degree to which question design influences the motives respondents attribute to the parties when asked about party conflict. In particular, I test the null hypothesis that respondents are no more likely to attribute conflict to group affect when provided with an attribution battery that includes non-affective motives in addition to affective ones.

Finally, I assess whether the way political conflict is described in these types of survey questions matters. I test the null hypothesis that respondents are equally likely to ascribe emotional and strategic motives to the parties, regardless of whether conflict is presented generally (as it is in Waytz, Young and Ginges [2014]) or in more policy-specific terms.

Research Design

To test these hypotheses, I extended Waytz, Young and Ginges's (2014) design, which asks respondents to attribute either their party's or the out-party's role in conflict to various affective motives, using a seven-point scale to indicate how much they ascribe each of those motives to the party.[2] I did so on the 2015 IGS-California Poll, discussed above. This study relied on a random subset of 1,383 partisan respondents (including leaners).

The original preamble to the battery read: "When (your party/the opposing party) engages in conflict with (the opposing party/your party), how much is (it) motivated by each of the following?" As discussed above, Waytz, Young and Ginges (2014) ask respondents to evaluate six potential motives. I first modified this design by asking people about "The Democratic (Republican) Party" instead of "your party (the opposing party)" and creating a dummy variable indicating whether the respondent's target was their in-party or out-party after all data had been collected.

Most important for testing the first two sets of hypotheses above, I randomly assigned (roughly) half of respondents ($n = 825$) to a battery that asked them only to rate the plausibility of two affective motives: "Empathy for people in the Democratic (Republican) Party" and "Dislike of people in the Republican (Democratic) Party." The other $n = 776$ respondents, by contrast, rated the plausibility of those two motives, plus three non-affective motives drawn randomly from this list: "Improving the Democratic (Republican) Party's chances in future elections," "Swaying public opinion," "Achieving public policies consistent with the Democratic (Republican) Party's agenda," "Satisfying groups that support the Democratic (Republican) Party's agenda," "Supporting (Opposing) President Obama," "Strategically trying to make the Republican (Democratic) Party look bad," "Achieving good public policies for the American people," and "Ignorance."

The items in the comprehensive attribution battery vary in two key ways. First, as noted, they differ in nature. Two are primarily affective, six are primarily strategic, and the final two are neither. Second, they differ in valence. Some of the items in the battery are positively-valenced, others are negatively-valenced, and still others carry minimal valence. I use the pattern of responses to these items to assess the uniqueness of people's beliefs about affective motives underlying conflict. In particular, if people asymmetrically ascribe non-affective but valenced motives to the parties the way they do affective ones, then it's likely that this tendency reflects partisan affect and a need for cognitive-affective consistency, not a deeply-rooted cause of intense partisanship.

I use the within-condition results in the comprehensive attribution battery condition to assess the degree to which people see the parties as strategically- and affectively-motivated. I use results between these two conditions to determine whether the attribution battery itself affects people's tendency to attribute conflict to partisan affect.

I also randomly assign how party conflict is presented to respondents to assess such framing's effects. 793 respondents saw a battery preamble asking them to evaluate the Democratic or Republican Party's motives when it "engages in conflict," the broad phrasing

[2] I rescale all variables 0-1.

Waytz, Young and Ginges (2014) use. The other 808 respondents were asked about the party's motives more concretely—in terms of policies that have (ostensibly) been at the heart of intense party conflict in recent years. These respondents assessed the Republican Party's motives when it "demands that the Democratic Party accept proposed tax cuts," and the Democratic Party's motives when it "demands that the Republican Party accept proposed spending increases."

I estimate the independent effects of the three randomly-assigned treatments—out-party target, comprehensive attribution battery, and policy-specific conflict. I first estimate these as *average marginal component effects* (AMCE) on people's tendency to attribute party conflict to in-party empathy and out-party dislike by regressing treatment indicators on the dependent measures (via OLS). I then examine how battery items and the description of conflict may work in tandem to influence the inferences people make.

Results

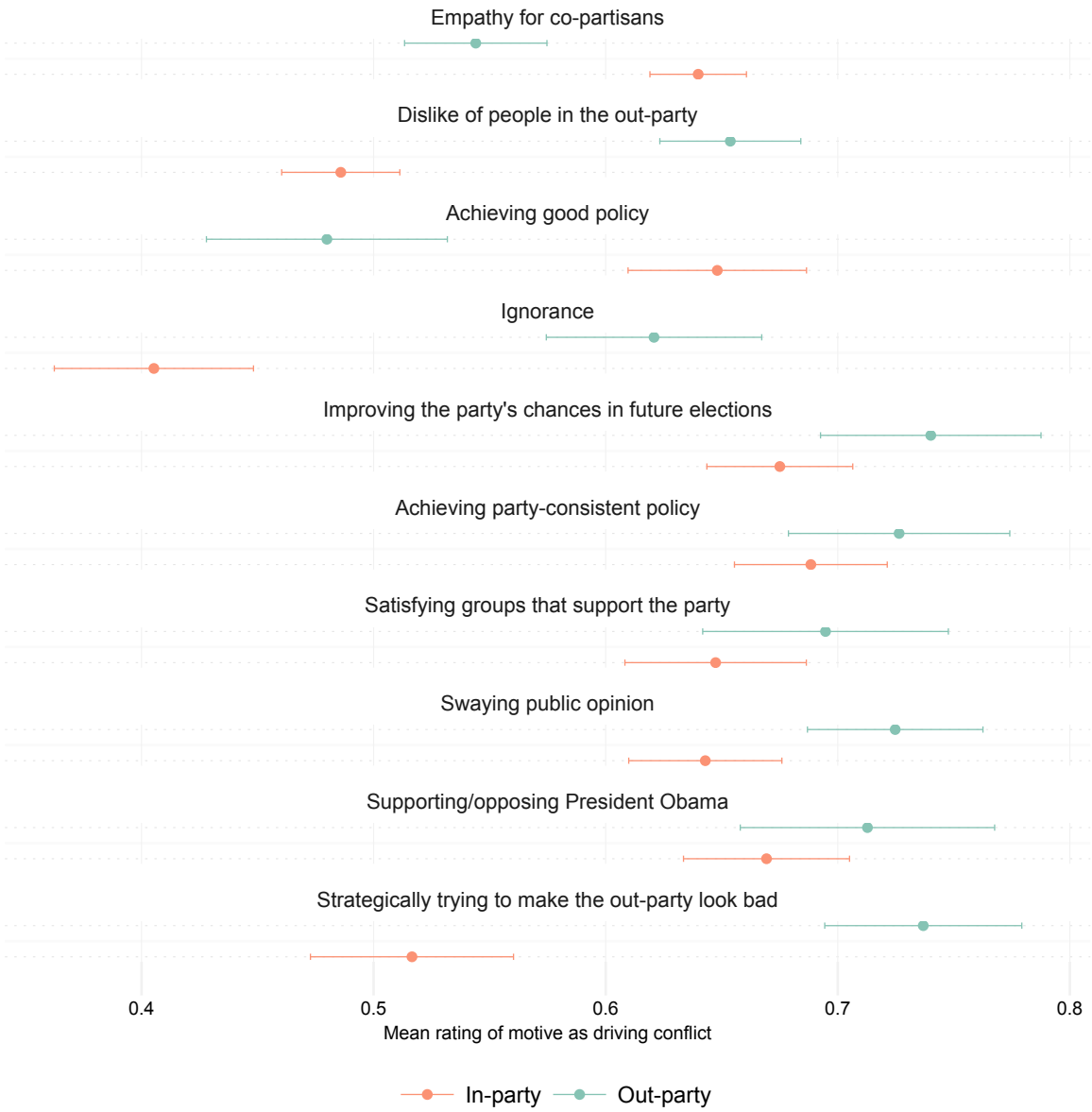
People Perceive Party Conflict as Strategic

To probe the relative weight that citizens place on affective explanations for party conflict vis-à-vis strategic ones, I first examine responses under the comprehensive attribution battery. Figure 8.2 plots the average plausibility respondents assigned to each of the potential motives in the battery, separately for respondents asked about their own party and about the out-party.

The top two panels of Figure 8.2 demonstrate asymmetric motive attribution for political conflict: people are more likely to report that their own party fights out of in-group love and the other party out of out-group animus. Waytz, Young and Ginges (2014) suggest that this fuels partisan intransigence. However, Figure 8.2 shows asymmetric attribution of conflict to other factors too. For example, while respondents asked about their own party rated in-party empathy as 0.10 points more likely to motivate conflict than did respondents asked about the out-party (95% CI: [0.06, 0.13]), the asymmetric ascription of sociotropic concerns ("achieving good policy [for the nation]") was significantly larger (diff. = 0.17, 95% CI: [0.10, 0.23]). Similarly, while respondents rated out-party dislike as significantly less credible when assessing their own party's motives (diff. = -0.17, 95% CI: [-0.22, -0.13]), I observe a larger asymmetry on the decidedly non-affective "ignorance" (diff. = -0.22, 95% CI: [-0.16, -0.28]).

Tellingly, respondents demonstrated substantively and statistically significant asymmetry when ascribing all valenced motives to the parties. This was not the case with motives that carried less obvious valence. (Figure 8.2 shows this visually between panels; see 9.2 for a table.) These differences are all in the expected direction: whether the possible cause of conflict is strategic, affective, or neither, if it carries a clear positive valence, respondents tend to ascribe it to their own party and not to the out-party, and vice-versa for negatively-valenced reasons.

Figure 8.2: Respondents Favor Strategic Motives as Explanations for Partisan Conflict



However, the broader finding in Figure 8.2 is the relative appeal of strategic motives as explanations for partisan conflict. Respondents rated "improving the party's chances in future elections," "swaying public opinion," "achieving public policies consistent with the (party) agenda," "satisfying groups that support the (party)," and "(supporting/opposing) President Obama" as equally or significantly more plausible than co-partisan empathy when evaluating their own parties' role in conflict. Even more notably, respondents rated all strategic motives as significantly better explanations for their out-parties' polarizing behavior than sheer dislike (or ignorance, for that matter). Thus, while partisans clearly disfavor inter-party hostility and in-group affinity as explanations for conflict initiated by their own party and the out-party, respectively, they also appear to see political conflict as animated by strategy as much as group affect, if not more.

Explaining Respondents' Explanations

To this point, I have examined results within the comprehensive attribution battery condition. I now compare results between conditions to better understand how questionnaire design may affect the explanations respondents provide for political conflict. I start by discussing the effect of these treatments on the ascription of inter-party dislike, and then turn to their effect on the attribution of conflict to co-partisan empathy.

Consistent with results from just the comprehensive condition, I find asymmetric attribution of dislike by partisanship in the full sample and controlling for the assignment of other treatments. As Table 8.2 shows, respondents were 16 points more likely to ascribe out-group dislike to their out-party than to their in-party. More surprising, however, is the apparent effect of how conflict is described. Respondents who were asked about conflict over specific policies rated out-party dislike as nine points less plausible as an explanation for conflict. Since political conflict is rarely presented as divorced from policy in the media, reports in the "specific conflict" condition are likely more representative of people's reasoning about actual irresponsible partisanship than those in the "vague conflict" condition.

In the full sample, the comprehensive attribution battery failed to meaningfully affect respondents' ascription of dislike to the parties. Its AMCE is quite close to zero (a two-point decrease) and precisely-estimated. However, when conflict is presented specifically, the comprehensive attribution battery yields a four-point decrease in the attribution of conflict to out-party dislike (95% CI: [-.08, .00]). I show this visually in Figure 8.3, which more generally shows that as conflict is described in a more externally valid way and as people are given a broad list of potential causes of conflict, they rely less on inter-group animus as an explanation.

Turning to respondents' use of empathy to explain conflict, I again find the expected asymmetry by partisanship. Table 8.2, Column 2 shows that people are ten points more likely to explain conflict through empathy when asked about their own party's role in conflict. On the other hand, I fail to observe substantively or statistically significant AMCEs of the other randomly-assigned treatments.

Table 8.2: Average Marginal Component Effects of Treatments

	DV: Attribution to dislike (1)	DV: Attribution to empathy (2)
Asked about out-party	-.16***	-.10***
	(.01)	(.01)
Comprehensive attribution battery	-.02	.01
	(.01)	(.01)
Specifically-defined conflict	-.09***	-.01
	(.01)	(.01)
Constant	.55	.63
	(.01)	(.01)
R^2	.13	.04
SER	.25	.25
n	1358	1359

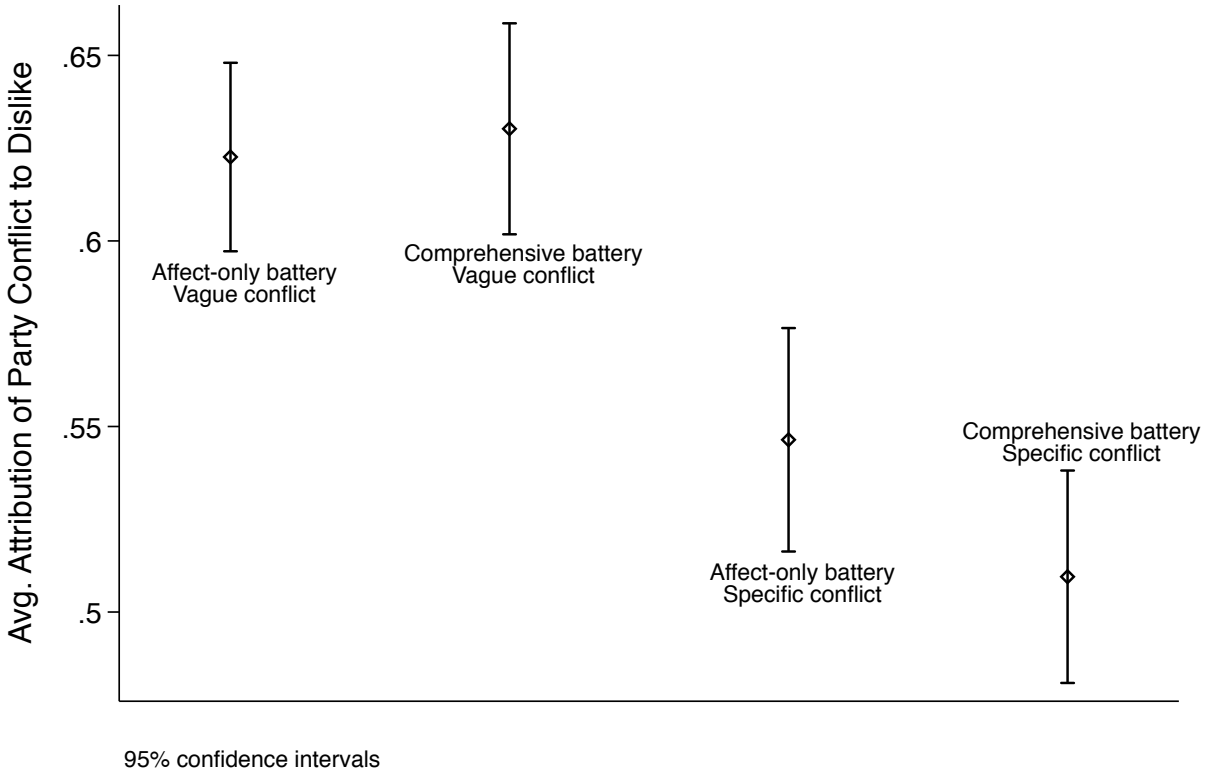
NOTE: All variables are scaled 0 to 1. Standard errors reported in parentheses. * = $p < .10$, ** = $p < .05$, *** = $p < .01$, two-tailed.

However, an interesting pattern belies the coefficients. Recall that respondents were randomly shown just three of the eight possible motives from the comprehensive attribution battery (in addition to empathy and dislike). I can thus leverage random assignment to rate the out-party's motivation to "achiev(e) public policies consistent with the (party) agenda" as an exogenous treatment. I do so to determine whether priming people to think about this potential motive also leads them to attribute greater empathy to the out-party. And, indeed, respondents randomly primed to consider party policies as a motive were 8 points more likely to attribute out-party-driven conflict to empathy (95% CI: [.02, 0.15]).[3] Thus, when citizens perceive their out-party as engaging in conflict out of a desire to achieve party-consistent policies, they also appear to perceive that party as more motivated by empathy. That is, giving respondents the chance to explain political conflict in terms of strategic motives, and not just affective ones, reduced the asymmetry with which respondents ascribed empathy to their in-party vis-à-vis the out-party.

This pattern is even more noteworthy because it depends entirely on how conflict is presented. Participants asked about their out-party and assigned to the vaguely-described conflict treatment were 14 points more likely to ascribe empathy when their attribution

[3]This is not true of people's ascription of empathy to their own parties, which see a 2-point decline, statistically indistinguishable from zero.

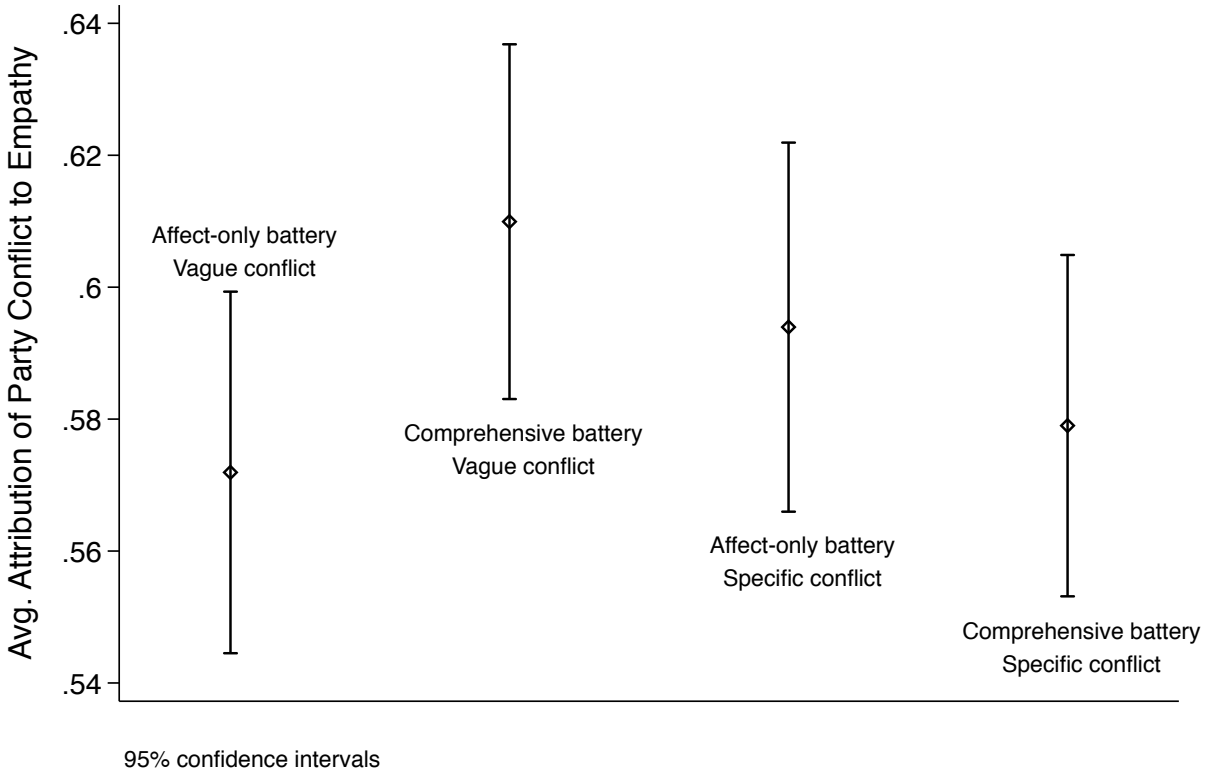
Figure 8.3: Attribution of Party Conflict to Dislike, by Attribution Battery and Description of Conflict



battery included "achieving policies consistent with the (party) agenda" (95% CI: [.05, .22]). The corresponding apparent effect among those assigned to the specifically-defined conflict treatment was a meager two points (95% CI: [-.08,.11]). Thus, priming people to think about the parties' policy motives appears to only affect perceptions of the the parties' empathetic motives when conflict is presented as divorced from specific policy battles—something that rarely occurs in media depictions of political conflict. This helps to explain the odd pattern of results in Figure 8.4.

Finally, data from the comprehensive attribution battery condition suggest one more way that a limited battery may drive partisans' tendency to rely on affect to explain party conflict: expressive responding. As noted above, some items in the comprehensive attribution battery carried negative valence—in particular, "ignorance" and "making the (other party) look bad." Partisans are unlikely to want to ascribe these motives to their party. If people lack "palatable strategic options" in the response set, one might expect respondents to attribute in-party polarizing behavior to empathy at higher rates. This is exactly what the data

Figure 8.4: Attribution of Party Conflict to Dislike, by Attribution Battery and Description of Conflict



show. Respondents asked about their own party's motives, and whose batteries randomly included both "ignorance" and "making the (other party) look bad" were 6.4 points more likely to explain the conflict through their own party leaders' co-partisan empathy than were respondents who had access to more palatable motives (95% CI: [-0.13, 0.00], $n = 221$). As a placebo test, I evaluate whether this random assignment affected respondents' tendency to explain their party's role in conflict as a function of out-group dislike and find no apparent effect (diff. = 0.03, 95% CI: [-0.07, 0.12]). Thus, partisans' reliance on "in-group love" to explain their own parties' role in political conflict appears to partially reflect the set of motives researchers allow them to choose from.

In sum, people's explanations for political conflict depend on both the way conflict is presented and the set of plausible reasons given to them. When political conflict is described specifically and respondents are given the opportunity to think about a broader set of explanations for conflict, they are less likely to cite out-party dislike. People's reliance on in-group empathy to explain political conflict appears, in part, to reflect the strategic

motives available in memory. This implies that people's reasoning about political conflict is more complex than the purely affective account Waytz, Young and Ginges (2014) offer. And, when provided with a comprehensive set of motives to choose from, people's explanations for party conflict look more like political scientists' and less like naïve rubes'.

In Search of the Accountability Failure

The analyses thus far suggest that citizens primarily rue polarization for its consequences on the political process—the bickering, gridlock, and dysfunction that stem from intense and strategic party teamsmanship. But the data also suggest that citizens, like political scientists, tend to recognize the strategic nature of party conflict. Thus, irresponsible partisanship appears not to be sustained by simple naiveté about politicians' motives.

While motives drive behavior, politicians' motives aren't determined exogenously. Electoral and policymaking institutions shape elites' incentives. The overarching view from political science is that the American system—with frequent elections, separated policymaking institutions, and multiple veto points—is unusually prone to "irresponsible partisanship" (e.g., Mann and Ornstein, 2012).

Do citizens favor reforms that would likely curb polarization's process consequences? If so, then irresponsible partisanship is truly a phenomenon forced on the American public. However, Table 8.3 suggests that this is not the case. As part of the 2015 IGS-California Poll, I asked respondents to indicate whether they would favor or oppose various electoral and political reforms. These reforms fall into three distinct categories, drawn from Mann and Ornstein's (2012) influential analysis of party polarization and conflict: "bromides to avoid" (reforms that are unlikely to cure polarization's ills, in this case, the first two items in Table 8.3), "reforms to the party system" (items 3-6), and "institutional reforms" (items 7-9). Respondents appeared to favor the "bromides to avoid"—term limits and full public financing of elections—more than all other reforms. By contrast, the least popular were the institutional reforms, and especially the one most likely to eliminate process consequences: "Amending the Constitution to create a parliamentary system, in which the the party that wins a majority of seats in Congress governs, with its leader serving as president."[4] Although this reform would remove the veto points and institutional separation that fuel partisan irresponsibility, it is even less popular than compulsory voting. Moreover, the modal respondent claims a zero-valence opinion on all reforms but term limits, suggesting that most citizens simply don't think much about institutions and reforms.

So while citizens aren't easy "marks" mobilized by party conflict without understanding its strategic nature, they also aren't clamoring for reforms to curb the process consequences they dislike so much. And the proliferation of zero-valence attitudes toward these reforms implies that many aren't even considering ways to do so. This suggests that citizens' lack

[4]One possible concern is that the question wording may have spurred Republicans to support the reform and Democrats to oppose it because of contemporary chamber control. However, Republicans were 1.3 points less favorable toward the reform, on average (95% CI: [-0.09, 0.06]).

Table 8.3: Citizens Favor "Bromides to Avoid," are More Skeptical of Reforms that Could Curb Process Consequences

	Strongly oppose	Oppose	Somewhat oppose	Neither favor nor oppose	Somewhat favor	Favor	Strongly favor	Mean rating (Std. error)	n
Congressional term limits	1.1	1.4	4.6	21.4	16.0	20.6	34.9	0.75 (0.02)	206
Full public financing of of elections (and ban on private contributions)	4.2	3.8	8.8	27.2	15.3	21.5	19.2	0.64 (0.02)	192
Ranked-choice voting	1.9	3.8	6.1	42.6	17.1	13.7	14.8	0.62 (0.02)	194
Independent redistricting commissions	0.9	4.0	5.8	49.1	18.6	9.7	12.0	0.60 (0.02)	186
Top-two primaries (to replace partisan primary elections)	6.9	6.9	14.2	38.9	18.6	11.3	3.2	0.50 (0.02)	208
Compulsory voting	18.6	10.9	10.6	33.5	13.8	7.6	5.1	0.43 (0.02)	207
Abolishing the filibuster	3.4	7.1	7.5	37.4	24.2	11.9	8.5	0.57 (0.02)	216
4-year terms and concurrent elections for pres. and all MCs	4.0	8.1	10.1	23.8	21.4	17.3	15.3	0.61 (0.02)	213
"Full parliamentary reform"	17.8	15.1	8.7	44.6	5.1	4.8	3.9	0.39 (0.02)	229

of knowledge about how institutions structure elite behavior, rather than misunderstanding the motives such underlie conflict, is a more likely explanation for the long-term existence of irresponsible partisanship. That is, citizens appear to understand that the parties' true motives often deviate from those publicly proclaimed, but their lack of institutional awareness prevents them from adequately diagnosing the causes of democratic dysfunction.

Discussion

Irresponsible partisanship is not a symbolic phenomenon. It has affected the national economy, the ability of government to conduct necessary business, and ultimately, citizens' trust in government. It is these process consequences that citizens most lament about contemporary party polarization. This is puzzling because political science generally concludes that process consequences—like incivility, gridlock, and government dysfunction—emerge strategically, often precisely when politicians believe the public is watching. Why doesn't the audience hold the parties accountable?

The evidence presented in this paper undercuts two compelling explanations. Under the first, citizens are naïfs who fail to perceive elite conflict accurately: they attribute party conflict to affect rather than strategy, seeing their own party as benevolent and the out-party as ill-intentioned. However, evidence presented here suggests citizens recognize that strategy motivates both parties in times of conflict, more so than partisan affect. Under the second explanation, by contrast, citizens are highly knowledgeable about the causes of conflict and desire reforms to abate its process consequences. If this is true, then political elites' unresponsiveness to public opinion is primarily to blame for continued irresponsible partisanship. But citizens overwhelmingly oppose or lack meaningful opinions on institutional and electoral reforms that could potentially ameliorate polarization's ills—and, indeed, support reforms that have been termed "bromides to avoid" (Mann and Ornstein, 2012). This suggests that irresponsible partisanship endures not because of the motives people attribute to party leaders, but instead out of their failure to link institutional incentives to strategic behavior.[5]

Of course, alternative explanations exist. This paper has assumed that citizens' attribution of party conflict to strategy means that they see irresponsible partisanship as systemic—as political scientists generally do—making electoral and institutional reform the appropriate solution. However, even if citizens believe that such conflict is strategic, they may also believe that individual politicians are to blame for irresponsible partisanship rather than the party system writ large. While political scientists are likely to disagree with this assessment, we must acknowledge that it nevertheless provides a pathway to accountability. As Figure 8.5 shows, though, it is a pathway with many possible points of failure. For citizens to

[5]An alternative explanation is that people are ambivalent about America's institutional system. For example, they may like its protection against policy volatility or its emphasis on local representation, even if they dislike its process consequences. This seems unlikely since the typical respondent appears not to have thought extensively about institutional or electoral reforms.

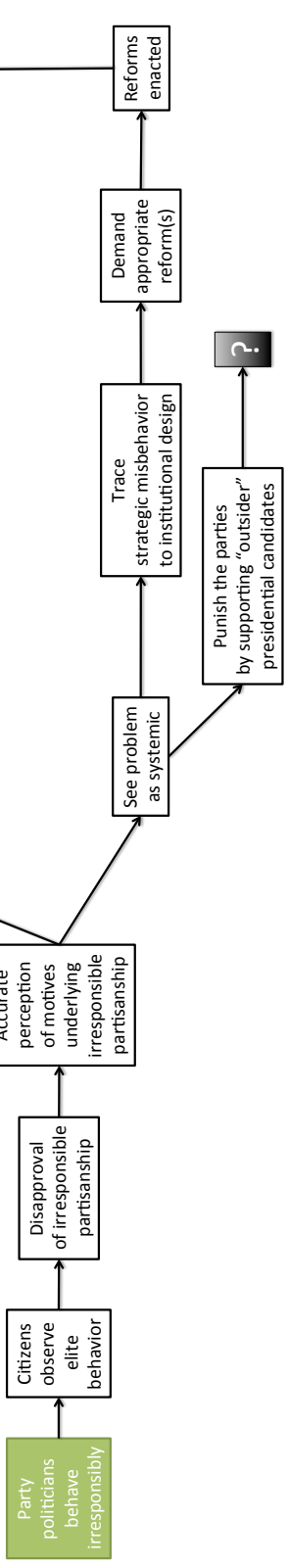


Figure 8.5: Pathways to Accountability (and Potential Points of Failure)

hold individual politicians accountable for their contributions to irresponsible partisanship, constituents must first recognize their own representatives' contributions. Then, viable candidates must challenge those incumbent politicians, and citizens must be willing to vote for those candidates. From a practical standpoint, this means that citizens must be willing to vote for out-party candidates in general elections or that viable co-partisan challengers must run in primaries. Future work might consider these potential alternatives, although as Figure 8.5 implies, their viability ultimately hinges on whether people see strategic irresponsible partisanship as an individual or systemic problem.

Furthermore, even if people do see irresponsible partisanship as a systemic problem, they may try to discipline the major parties rather than reform the system in which they operate. For example, the surprising support for Donald Trump and Bernie Sanders in 2016 might reflect dissatisfaction with politics-as-usual in Washington. If elected, those candidates would be unlikely to curb process consequences, which contribute to that dissatisfaction; indeed, such a president's extremity would likely lead their out-party to become even more intransigent. However, we must consider the possibility that citizens *think* such "outsider" candidates might reform Washington.

Thus, further work on citizens' attributions for political conflict is needed. While this paper has addressed the specific *reasons* that people give for why the parties engage in conflict, future work must address whether they perceive any *enabling factors* for irresponsible partisanship (Malle, 2011), such as party leadership or particular rules and institutions. By probing people's beliefs about enabling factors for irresponsible partisanship, we may learn which of the branches of Figure 8.5 citizens tend to find themselves on and, thus, where democratic accountability generally breaks down.

Although this paper does not solve the mystery of irresponsible partisanship, it represents a step forward in two key ways. First, as mentioned above, it rules out two potential explanations. Second, it illuminates potential pitfalls in measuring citizens' attributions for political conflict. Because Americans have such strong feelings toward the parties (Iyengar, Sood and Lelkes, 2012; Iyengar and Westwood, 2014), they may respond to survey questions about party conflict in an affectively-consistent manner (Lodge and Taber, 2013). In this case, partisan respondents may make invalid attributions when given limited attribution batteries with affectively-charged items. The more general point is that when asking people to attribute causes to effects, the universe of potential causes given—that is, the items in the battery—can shape their responses.

The way an effect is described can also influence how respondents explain it. In this case, I found that people are significantly less likely to attribute political conflict to inter-party animus when the conflict is described in specific policy terms instead of vaguely. Similarly, respondents shown the comprehensive attribution battery were less likely to ascribe empathetic motives to their out-party when conflict was described concretely. When studying citizens' beliefs about elite conflict, failing to attend to the fact that politics *is* strategic can lead researchers to overestimate the degree to which citizens reason about party conflict through an affective lens, especially if conflict is presented in the abstract. More broadly, scholars studying attribution processes through closed-ended questionnaires must give re-

spondents a diverse range of explanations, lest they force respondents into committing a fundamental attribution error (Ross, 1977).

Determining what the public knows about politics, and perhaps more importantly, what citizens need to know to hold their democratically-elected government accountable is an important function of political science (e.g., Delli Carpini and Keeter, 1996). As such, political psychology has much to contribute to resolving the puzzle of irresponsible partisanship. But we must also recognize that the problem is institutional in nature, and more systematically assess citizens' knowledge about how electoral and policymaking institutions fuel party conflict's ill effects.

Chapter 9

Supporting Information for "Irresponsible Partisanship and Democratic Accountability"

9.1 Citizens Disapprove of Irresponsible Partisanship and its Process Consequences

The 2015 IGS-California Poll: Sample Details

The table below compares the distribution of demographic covariates in the SSI sample used throughout the paper to other established benchmarks.

	IGS Poll (August 2015)	Weighted IGS Poll (August 2015)	CCES (September 2012)	Weighted CCES (September 2012)	CA Census (2010)	CA Sec. of State (2015)
Sampling frame	population quota (SSI)		population quota (YouGov)		population census	registered voters census
Age						
18-29	26%	27%	18%	24%		
30-39	17%	20%	9%	16%		
40-49	12%	10%	13%	17%		
50-64	28%	27%	36%	26%		
65 & over	17%	17%	23%	17%		
Gender						
Female	59%	51%	50%	51%	50%	
Male	41%	49%	50%	49%	50%	
Race						
Asian	14.5%	17.8%	5%	8%	17%	
Black/African-American	4%	7%	9%	7%	8%	
Hispanic/Latino	19%	28%	20%	21%		
Native American	2%	1%	1%	1%		
White/Caucasian	61%	47%	59%	56%	75%	
Ethnicity						
Hispanic or Latino					38%	
Not Hispanic or Latino					62%	
Education						
Some high school	1%	12%	3%	11%	12%	
High school/GED	13%	24%	17%	23%	24%	
Some college/2-year degree	36%	35%	28%	31%	34%	
4-year college degree	33%	18%	25%	19%	18%	
Graduate/professional degree	17%	11%	13%	10%	11%	
Party registration/ID						
Democratic	46%	44%	44%	40%		43%
Republican	24%	34%	24%	22%		28%
No party preference	27%	30%	22%	28%		24%
Other	3%	3%	10%	10%		5%

Beliefs about Polarization: Why 30% of Californians Say it's at Least "Somewhat Good"

29.9% of respondents reported that polarization has been at least "somewhat good," on the whole. (Compared to the vast majority, 70.1%, who reported that it's been at least "somewhat bad.") In the paper, I show why people tend to think polarization is negative. Here, I present a similar table, but showing people's reasons for saying polarization has been a net positive.

Table 9.1: Why Some View Polarization Positively

	% citing	% ranking as best consequence
"Clearer differences between Dems. and Reps."	64.1 [53.9, 74.2]	35.6
"Selecting candidates is easier."	47.0 [36.4, 57.6]	20.6
"My views are better represented by a party."	43.8 [33.3, 54.3]	17.9
"Politicians talk more about issues than in the past."	33.6 [23.6, 43.6]	9.9
"News outlets that share my views."	29.0 [19.3, 38.6]	5.9
"Politics is more interesting."	25.6 [16.3, 34.8]	5.4
Other	0.7 [0, 2.4]	0

9.2 Testing Whether Citizens see Irresponsible Partisanship as Strategic or Affect-Driven

Item Valence Appears to Drive Responses to Attribution Battery

The table below provides coefficients for the regression of attribution i on the out-party target indicator. That is, the table shows how well the party respondents were asked about predicts their responses to the attribution battery. I observe asymmetric ascription of all items carrying a clear valence—not just affective items.

	b	95% CI	n
Valenced items			
In-party empathy	-.10	[-.13, -.06]	657
Out-party dislike	.17	[.13, .21]	657
Good national policy	-.17	[-.23, -.10]	242
Ignorance	.22	[.15, .28]	237
Making the other party look bad	.22	[.16, .28]	294
Non-valenced items			
Achieving party policies	.04	[-.02, .09]	239
Satisfying party-affiliated groups	.05	[-.02, .11]	251
Supporting/opposing the pres.	.04	[-.02, .11]	253
Swaying public opinion	.08	[.03, .13]	256
Winning elections	.07	[.01, .12]	249

Chapter 10

Conclusion

On November 3, 1969, President Richard M. Nixon implored Americans to support continued U.S. ground operations in Vietnam. In doing so, he drew a contrast between those who opposed his agenda and the "silent majority" of Americans who did not. Two days later, Nixon clarified that the silent majority was the "large and normally undemonstrative cross section of the country that until (November 4) refrained from articulating its opinions on the war" (*New York Times* 1969). However, Bill Safire, at the time a speechwriter for Nixon who incorporated the phrase into Vice President Agnew's speeches months before the famous November address, notes that the "silent majority" is more than a political concept. Instead, it is "a *social* concept, referring to those who uphold traditional morality, and who resent the attention given by the media to the demonstrators and noisemakers" (Safire, 2008, p. 428, emphasis original). And Agnew's speeches left little doubt as to who comprised the silent majority's out-group: blacks, protestors, social liberals, and intellectuals, among other groups that have increasingly become associated with the Democratic Party since the late 1960s (Perlstein, 2008).

The key moment of Nixon's speech—the "silent majority" passage—was directed at Americans who were quiet on the issue of Vietnam. That is, the goal was not to persuade the anti-war crowd, but rather to amplify opposition to protest into vocal support for Nixon's policies. The means by which Nixon did so was to cast anti-war sentiment as a minority view. And considering Nixon's use of the term in tandem with Agnew's, those outside the "silent majority" were not only a minority, but a fundamentally countercultural minority.

The first two papers in this dissertation suggest why the idea of a "silent majority" is a powerful rhetorical device, and one that many politicians since Nixon have used—most recently and notably, Donald Trump. People's political opinions and feelings toward political actors do not form in a vacuum. Instead, as "Self-Fulfilling Mispercpetions" finds, people appear to consider their perceptions of where others stand when forming their own opinions. By describing one's own policy as supported by a "silent majority," one's goal is to remove pluralistic ignorance that the other side's "vocal minority" is actually a majority—and thus, to sway opinion to one's own side. However, the social aspect of the "silent major-

ity" gives the idea long-term power. By reducing political complexity to social stereotypy, the "silent majority-vocal minority" dichotomy creates clear distinctions between political factions where only "fuzzy differences" (Tajfel, 1969) may actually exist. This, as "The Parties in Our Heads" suggests, may turn lukewarm supporters into intense partisans. Rick Perlstein, one of America's pre-eminent historians of the 20th century right, thus concludes: "Nixon rose by stoking and exploiting anger and resentment...For what was his injunction to join his silent majority if not also an invitation to see one's neighbors as aliens, and to believe that what was alien would destroy us?" (p. 748).

Perlstein's thesis resonates with the notion that people's beliefs about the parties—how they are composed and what their supporters stand for—fuel their political reasoning. Moreover, it comports with the insight that people's beliefs about sociopolitical collectives primarily come from impersonally transmitted elite communications (Mutz, 1998). On the other hand, Perlstein's excruciatingly detailed account of Nixon's strategic attempts to manipulate these beliefs raises questions about which elites are the prime movers of people's political perceptions.

The first two papers in the dissertation suggest that the mass media primarily provides citizens with their beliefs about how political collectives are composed and what they stand for. "Self-Fulfilling Misperceptions" asserts that the media's use of a "polarization narrative" fuels misperceptions of mass ideological polarization—a proposition that Levendusky and Malhotra's (2016) content analysis supports. And "The Parties in Our Heads" finds that citizens with the most interest in politics and news are also the most likely to overestimate social differences between the parties. However, all of this evidence is descriptive. The media may not actually be the prime mover of people's political perceptions, but instead may be a mere conduit for elite communications (e.g., Zaller, 1992). Indeed, this seems more consistent with the historical evidence that Perlstein presents. On the other hand, even if politicians do provide fodder for the polarization narrative, the media may still selectively cover those stories as a form of negativity bias (e.g., Soroka, 2012). Assessing these comepting accounts—and determining the source of people's inaccurate information—is thus a worthy direction for future research on people's perceptions of mass polarization.

Pinning down the cause(s) of people's misperceptions about political collectives is an important next step, but so too is more thoroughly documenting their real-world effects. The first two papers show that these perceptions affect people's opinions, attitudes, beliefs, and partisan affect, but they rely exclusively on survey and survey-experimental evidence. To identify the effects of these misperceptions on behavior, future scholarship should extend this research into the field. A signature collection experiment seems especially promising: one could manipulate the measure for which signatures are being collected (a Democratic policy, a Republican policy, or a consensual policy) and the characteristics of the signature-collector (stereotypically Democratic, stereotypically Republican, or neither) independently of each other to determine the degree to which partisan social cues affect behavior. Additionally, one might consider including the information from the "tell" treatment in the first two experiments in a field experimental treatment attempting to persuade leaning independents to register as partisans. Such a study could illuminate the degree to which overstimation of

mass division leads individuals to separate themselves from partisan politics (as one might expect from Klar and Krupnikov, 2016).

By contrast, future work on citizens' perceptions of elite polarization should focus not on beliefs about people, but rather perceptions of the institutions that structure elite conflict. The third paper in this dissertation suggests that the most disliked aspects of polarization and party conflict are those which are sustained by the separated system of government—gridlock, strategic disagreement, and partisan gamesmanship. But people appear resistant to reforming our institutions in a meaningful way. This potentially suggests failures in people's reasoning about the causes of these democratic ills, which demand a more thorough accounting of the attributions people make about elite conflict.

The evidence in "Irresponsible Partisanship and Democratic Accountability" is merely speculative. It advances our understanding of the attributions citizens make about party conflict beyond the recent work of Waytz, Young and Ginges (2014), showing that people generally understand that elite party conflict is often strategic. Even in doing so, the paper only adjudicates between strategic and affective "reason" explanations that people may provide for intentional action (like engaging in conflict; Malle 2011). But by political science accounts, the best explanation for conflict is one of "enabling factors" (Malle, 2011): party elites have a strategic desire to maximize their party's electoral gains, and our institutions allow them to do so via irresponsible partisanship (e.g., Mann and Ornstein, 2012; Ranney, 1954). To assess citizens' perceptions of the underlying causes of polarization's process consequences, then, future work should determine whether citizens volunteer "enabling factor explanations" when explaining elite conflict, and the degree to which they see these explanations as plausible when listed with strategic and affective "reason" explanations.

Ultimately, in studying people's perceptions of polarization and party conflict, political psychology should better grapple with how citizens perceive institutional arrangements, as they directly affect the elite phenomena that citizens observe. Psychological work on biases in human perception, cognition, and judgment has taught us a great deal about mass political behavior; but it faces severe limits in its ability to explain failures of the electoral connection without also theorizing the role of institutions. Moving forward, this agenda might address how citizens separate political institutions and process from the "informational background," assessing aspects of elite politics that citizens are especially attuned to and others where blind spots exist.

Related to this dissertation's third paper, such a research agenda might also better unpack the zero-valence attitudes that citizens often report on political reforms. This could be done quite easily by administering the battery from the end of that paper but with a "don't know" response option, which would separate ignorant zero-valence attitudes from ambivalent ones. However, more complex designs might provide further information about the reforms to survey respondents, or even put participants through an interactive game mimicking the democratic process. Such experiments could potentially assess the effect of calling attention to the "enabling factors" of irresponsible partisanship on support for reforms.

This dissertation's starting point was the idea that simplified mental representations of the political world are necessary for citizens. They help citizens to see important distinctions

between political factions, to distill the essence of contemporary conflict from the incredible amount of information they encounter, and ultimately, to make political decisions. However, the "political pictures" we carry in our heads also lead us astray at times. In the contemporary polarized climate, citizens tend to overestimate political and social differences between parties, which in turn amplifies mass-level polarization through self-fulfilling processes. Most worrying is the prospect raised by the "silent majority" anecdote, that politicians strategically manipulate these perceptions to shore up their electoral coalitions and partisan loyalties.

Although there is more research to be done on this topic, the conclusion thus far need not be dour. While the first two studies demonstrated that people do hold systematically skewed beliefs about polarization, and that these beliefs fuel ideological polarization and partisan animus at the mass level, they also demonstrated that these pernicious effects can be abated. Simply providing citizens with information about how polarized the public actually is on key political values leads them to report more moderate opinions of their own. Perhaps more surprisingly, correcting misperceptions about the social composition of the parties partially cures the intense partisan animus that strains social and political trust (Iyengar, Sood and Lelkes, 2012). Moreover, the third paper demonstrates that citizens have a better understanding of elite polarization and its effects than previous research hypothesized. In sum, people have simplified mental representations of politics—and polarization—but they are not complete naïfs and, more importantly, they appear responsive to correction when the pictures in their heads lead them astray. Perhaps the most important extension of this work, then, is to develop and test real-world analogs to the experimental treatments so that citizens may more competently recognize and assess appeals made by politicians on collective grounds.

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