Measuring Perceptions of Shares of Salient and Stereotypical Groups*

Douglas J. Ahler† Gaurav Sood‡

Misinformation—belief in incorrect information (Luskin, Sood and Blank 2013)—is distressingly common. Despite overwhelming, widely available evidence showing otherwise, some citizens believe that President Barack Obama was born outside the U.S. (ch. 2), and some that "light" cigarettes are neither addictive nor cancer causing (ch. 4).

Group stereotypes are a type of misinformation, perhaps the most commonly held kind of misinformation. Unlike the examples above, however, stereotypes are rooted in "kernels of truth" (Bordalo et al. Forthcoming). People develop mental images of groups to better understand the social world and their own place in it (Lippman 1922). But the social world is complex, and cognitive capacity, limited. Hence, people rely on mental shortcuts, or heuristics, when developing these mental images, ending with impressionistic accounts rather than photo-realistic portraits of the world outside.

These impressionistic accounts often, however, are systematically biased. Even where the genesis of stereotypes is in actual differences across groups, beliefs about the differences are often vastly exaggerated. And given exaggerated differences likely inflame intergroup animus (e.g., Ahler and Sood 2016), group stereotypes speak to some of the most fundamental questions in social science as they relate to inter-group conflict. What explains inter-group conflict? What are its consequences? And how do we temper (or inflame) it?

*We thank Rachel Bernhard, Andy Guess, Stephen Goggin, and Kabir Khanna for useful feedback.

†Doug is a post-doctoral fellow at the Center for the Study of Democratic Institutions, Vanderbilt University, and Assistant Professor of Political Science starting Fall 2017 at the Department of Political Science, Florida State University, doug.ahler@gmail.com.

‡Gaurav can be reached at gsood07@gmail.com.

To try to answer these questions, social scientists study stereotypes through the broader lens of how individuals think about group and population composition, searching for psychological mechanisms behind perceptual bias (Bordalo et al. Forthcoming; Gennaioli and Shleifer 2010) and manipulating such distortions experimentally to better understand the effects of stereotyping (Ahler and Sood 2016). In this chapter, we focus on measurement of perceptions of numerical strength of salient, stereotypical groups in the population and in other stereotype-relevant groups.

It is unlikely that more than a handful of people hold any beliefs about the share of most groups—for example, beekeepers, sedan drivers, or people who own more than two pairs of jeans.[1] But for stereotypical groups or groups that are salient to policy debates, many people likely hold crude beliefs about their numerical strength—e.g., immigrants, African-Americans in the Democratic Party, etc.

We contend that these crude beliefs implicitly map to specific numbers. And that we can elicit these numbers on surveys, though likely only with considerable noise. For instance, a person may hold the belief that "many" undocumented immigrants live in the United States. This belief may affect their attitude toward immigration without they ever cognizing the mapping of "many" to the number that the belief implicitly maps to. However, when asked about the share of undocumented immigrants in the population, the person may report that 15% of the people living in the country. (The actual number is 3.5%, as per Krogstad and Passel 2016.) We contend that the reported 15% is a function of the underlying belief and random error.

This conceptualization raises a variety of mechanistic and conceptual concerns. For instance, does it matter how much time we give people to report their beliefs? Do people honestly report what they believe? Or do they instead offer responses that reflect how they feel about the group? Does innumeracy, rather than genuine misperception, explain survey reports exaggerat-

[1] This isn't to say that most people will draw a blank if asked these questions. Most will simply use 'related knowledge' to infer these quantities. For instance, a person may reason that I don't eat honey often, and I don't think there are a lot of people who eat honey, so if I had to guess, I would say not many people keep bees.

ing certain groups' shares? Each of these concerns highlights a threat to the *validity* of measures of perceptions about composition. Each suggests that something intervenes between people's crude beliefs about the numerical strength of group g and the survey reports and alters those survey reports systematically.

However, even if concerns like these prove unfounded, it is not clear how to interpret elicited perceptions. If we find that people overestimate the share of group g in subpopulation s—e.g., the percentage of Democrats $[s]$ who are black $[g]$—what do we make of it? Could it be that people think that the share of g in the population is larger than it is, and that causes them to overestimate $p(g|s)$. Or is it that people accurately perceive the share of g in the population but overestimate its prevalence in s?

We shed light on these and other such questions around conceptualization, measurement, and interpretation of perceptions of numerical strength of groups in populations and subpopulations. We provide methods for assessing the validity of reported perceptions against alternative explanations and interpretations, demonstrating the use of some of the tools with two large survey experiments.

Causes and Consequences of Misperceptions of Numerical Strength of Groups

Misperceptions about the numerical strength of groups are thought to affect a host of socially important variables, including how people feel toward groups and the people who belong to them. But why should that be so? Sometimes attitudes toward groups are rooted in the zero-sum struggle between groups for resources and power (Campbell 1965). In such a scenario, an increase in the actual (or imagined) share of an out-group naturally increases one's anxiety that the out-group will seize power—especially in a democracy—and execute its agenda. Members of the threatened group respond to this heightened threat with greater in-group solidarity and out-group hostility (e.g., Nadeau, Niemi and Levine 1993; Sides and Citrin 2007*a*), greater opposition to policies that benefit the out-group, and greater likelihood of preemptive action, e.g., voting to reduce the power of the threatening group (Key 1949; Blalock 1967; Dancygier 2010).

Social identity theory provides another explanation for how people evaluate groups and why perceptions of group shares may matter. People seek positive distinctiveness. In-group favoritism arises because individuals' self-concepts are intricately tied to their membership in social groups (Tajfel and Turner 2005). So powerful is the need for positive (group) distinctiveness that membership in even arbitrarily assigned groups engenders in-group favoritism (Sherif 2015; Tajfel 1970). The importance of social identity means that people often evaluate novel groups based on their perceptions of how known social groups compose them. For instance, people conceptualize political parties in terms of longer-standing social groups, like those based on race and social class Green, Palmquist and Schickler (2002). And partisans' perceptions about the share of these groups in the parties affects their feelings toward opposing party supporters (Ahler and Sood 2016). More broadly, how large people think a group is affects not only how threatened they feel by the group, but also attitudes toward the group, entangled policy preferences, and behavior toward members of the group (Wong 2007; Wong et al. 2012; Sides and Citrin 2007*a*)—as both realistic conflict and social identity theories would predict.

Troublingly, perceptions of group shares are often biased in ways that heighten conflict (e.g., Ahler and Sood 2016; Nadeau, Niemi and Levine 1993; Sides and Citrin 2007*a*). But how is it that people come to hold these erroneous beliefs? Common pathways include reliance on accessible information, disinformation, and the use of representativeness heuristic.

We are most likely to receive (and thus accept) information that is most readily available. But what is accessible is not always apt for drawing correct inferences. For example, local television news covers violent crime far more than non-violent crime even though non-violent crime is far more common. And watching local news likely leads some people to (often wrongly) believe that violent crime in their local area is common (Romer, Jamieson and Aday 2003). Similarly, it is no surprise that Americans' beliefs about the percentage of the poor who are black hew closely to media depictions of the poor (Gilens 1996), and that perceptions of the percentage of Democrats and Republicans belonging to party-stereotypical groups are the worst among people who report having the greatest interest in political news (Ahler and Sood 2016).

Accessibility implies that people make erroneous inferences about group shares from readily available information; elite communications play a part in *indirectly* driving misinformation. By contrast, disinformation implies that people are directly misled. Cynical spreading of falsehoods with the aim of persuading the electorate to change their preferences and behavior is not new (Jackson and Jamieson 2007), but the 2016 presidential elections brought this concern to fore as never before. For example, several stories that grossly overstated the total number of immigrants (especially illegal immigrants) in the country circulated during the campaign. And some of these stories were shared (and read) widely on the social media, sometimes cynically disseminated by others—"14% [said] they shared a story they knew was fake at the time [of sharing]" (Barthel, Mitchell and Holcomb 2016). As such, there is reason to think that some people hold incorrect beliefs about the share of prominent social groups entangled in policy debates because they took as fact disinformation from a trusted source.

As accessibility bias and disinformation imply, external information may drive misperceptions about group shares. But internal processes may do so as well. Foremost among these is the representativeness heuristic (Bordalo et al. Forthcoming; Tversky and Kahneman 1974)—focusing exclusively on the similarity between targets and known categories when making judgments. In the case of perceptions about group composition, this is likely to result in people neglecting base rate information. For example, when people are asked to evaluate the percentage of poor Americans who are black, they are apt to focus on the categorical overlap in memory between blacks and the poor and not consider that just 13% of the U.S. population is black. Bordalo et al. (Forthcoming) formalize this logic, asserting that group g is representative of subpopulation s if it scores high on the ratio $\frac{p(g|s)}{p(g|\neg s)}$. They further empirically demonstrate that people overweight representative groups (g) when making judgments about other groups' (s) composition.

Perceptions about strength of groups, therefore, are of interest to social scientists not only because they affect intergroup attitudes and relations, but also because misperceptions may shed light on the nature of information flows about social groups and events. However, measuring these perceptions presents unique challenges, which we turn to now.

Conceptual, Measurement, and Interpretation Concerns

A half century of survey data suggests that the average American knows little about politics (Campbell et al. 1960; Delli Carpini and Keeter 1996). Roughly half of the survey respondents fail to identify their member of Congress, and similar numbers fail to correctly place the political parties on major issues (e.g., Freeder, Lenz and Turney 2016). Such disengagement means that it is unlikely that most people will encounter—much less remember—specific data on the numerical strength of even salient or stereotypical social groups in the population or stereotype-relevant groups. Instead, most people are likely to have crude beliefs—stereotypes founded in representativeness, or impressions based on inferences from accessible information. These crude beliefs, however, likely map implicitly to precise numbers, which people use to react to situations. And it is the numbers that people's beliefs map to that survey researchers want to elicit. But a variety of concerns and questions attach themselves to measurement of these numerical perceptions.

Eliciting Valid Mappings of Crude Beliefs

The quantity of interest is the numbers that people's crude beliefs about group composition implicitly map to. From a face validity perspective, the closer the responses are to being automatic, the better they are at capturing the implicit mapping. To obtain such responses, researchers may want to curtail the time the respondents have between accessing the crude belief and reporting its numerical mapping. Providing more time to respond may yield estimates that reflect additional considerations and reasoning beyond what people would normally consider when accessing and using these beliefs outside the survey environment. For instance, given additional time, a respondent may reason that their "gut answer" is too large or too small. Thus, these considered reports are liable to be different than beliefs that inform people's judgments in the real world.

Eliciting more considered beliefs may also change the underlying beliefs that people have. Presenting people with circumstances in which they can carefully consider the beliefs they report may cause people to change not only what they report, but also what they actually believe. Having considered the number their belief maps to, some people may find it too large, and change their original beliefs. This also means that eliciting more considered responses may be useful in elucidating the degree to which misperceptions can be corrected through slower, more effortful processing (e.g., Kahneman 2011; Petty and Cascioppo 1986).

Use of Denominators Larger than 100

When reporting beliefs about shares of groups, people often implicitly use denominators larger than 100 (Wong 2007). That is, when asked to report shares of an exhaustive set of mutually exclusive groups in a population or subpopulation, respondents' summed answers often exceed 100. This implies that innumeracy about percentages and genuine misperceptions about group shares may be observationally equivalent. Two strategies exist for addressing this concern. The first is recalibration. For instance, if shares of an exhaustive, mutually exclusive set of categories sum to 125, estimates for each of the categories can be divided by the more appropriate denominator (125). Such recalibration assumes that relative error is the same across categories; it may not be.

Alternatively, one can address the problem during measurement, amending the survey instrument in a way that makes respondents more acutely aware of the appropriate denominator. For example, we may force respondents to sum the shares of comprehensive sets of mutually exclusive groups to 100 (e.g., Ahler and Sood 2016). This solution, however, likely has some side effects. Not only is it likely to be cognitively taxing for the survey respondents, but it may also cause them to think more effortfully about the quantities than researchers would like. Alternatively, and more simply, to address the concern, one may rephrase the question stem as "Out of 100, how many..." as opposed to "What percentage..." (Sides and Citrin 2007*b*). Existing research isn't clear, however, as to whether this strategy ameliorates the concern.

Motivated Responding

Respondents may intentionally misreport their beliefs about the numerical strength of groups to express their feelings about the groups referenced in the survey question (see Bullock et al. 2013; Khanna and Sood 2015; Prior et al. 2015). In particular, people may intentionally overstate the share of groups they (dis)like in groups they (dis)like. For example, a white racist may purposefully over-report the percentage of poor Americans who are black, as doing so casts an aspersion on a disliked racial out-group.

Evidence for motivated responding is hard to collect. A bulk of the evidence comes from experiments that pay people for providing correct answers and for confessing to ignorance. For instance, Prior et al. (2015) gives a random set of respondents accuracy incentives (monetary and text appeals) for correct answers. They find that the partisan gap in responses to affectively-charged items (e.g., changes in the unemployment rate under a Democratic president) falls by about half. Bullock et al. (2013) also provide accuracy incentives, either for marking "Don't Know" or marking the correct answer, and arrive at similar estimates.

Interpreting the results of experiments that provide incentives, however, can be tricky. For one, incentives may encourage cheating; like Bullock et al. (2013), researchers may want to use placebo questions to gauge the extent of the concern. Second, to earn the reward, respondents may revise their responses to comport with their perception of the researchers' beliefs, even if they do not believe those reports. Asking respondents to guess how the researcher(s) would answer may be one way to gauge that concern. Lastly, providing incentives likely yields more considered responses. And as we argue above, top-of-the-head answers may be closer to the beliefs respondents generally carry about salient and stereotypical groups.

Misunderstanding Question or Scale

Respondent ambiguity about what is being asked can hamper the validity of any survey item. These concerns extend to items tapping perceptions of group shares. For instance, the question, "What percentage of Democrats are black?" is somewhat ambiguous. Some respondents may reasonably wonder if Democrats just means people who identify themselves as Democrats, or also those who lean toward the party—or, instead, those who voted for the Democratic candidate in the last presidential election. All of this assumes that these distinctions appreciably alter the elicited number. Many a times they don't. For instance, including those who *lean* toward a party doesn't appreciably change the share of prominent party-stereotypical groups in the "party" (Ahler and Sood 2016). But even so, precision is preferred.

There is, however, generally a trade-off between precision, compactness, comprehensibility. And given only a small chunk of respondents are likely to be aware of these finer distinctions, one idea may be to keep the question stem as is and ask an additional open-ended question about the definition of the quantity being estimated. People's understanding of the quantity being asked can then be used to more clearly interpret the responses.

Others have raised concerns about comprehensibility of response scales for numerical perception items. Ansolabehere, Meredith and Snowberg (2013) write, "providing respondents with benchmark quantities…can reduce measurement error due to respondents not understanding the scale on which more complex quantities, such as the unemployment rate, are measured." We are skeptical that this improves measurement of group shares—and even numerical perceptions more broadly. It is odd to claim that people can know the unemployment rate and yet not know its scale. Simply, if a respondent knows that the unemployment rate is 4.4%, comprehension of the scale is moot—and again very likely obvious to people who know the unemployment rate. And interpreting the effect of offering a benchmark rate—lower error—as better comprehension of the scale seems unwarranted. Offering a benchmark rate is liable to reduce error not because respondents suddenly realize that the unemployment rate is based on a 101 (0–100) point scale, but because respondents can better calibrate their guesses. Another negative side-effect of offering benchmark quantities is that benchmarks are likely to act as low-information *anchors*, shrinking variance, and adding bias to the elicited answers (Tversky and Kahneman 1974).

Beliefs About Related Quantities

When interpreting perceptions of subpopulation composition, researchers face a unique challenge. Consider the case where people are asked to assess the percentage of subpopulation s belonging to group g, for instance, the percentage of Republicans who earn $250,000 per year or more.[2] Someone using Bayes rule would do the following calculation:

$$p(g|s) = \frac{p(s|g)p(g)}{p(s)} = \frac{p(s|g)p(g)}{p(s|g)p(g) + p(s|\neg g)p(\neg g)}$$

or using the example,

$$p(\$250\text{K}|\text{Republican}) = \frac{p(\text{Republican}|\$250\text{K})p(\$250\text{K})}{p(\text{Republican}|\$250\text{K})p(\$250K) + p(\text{Republican}| < \$250\text{K})p(< \$250\text{K})}$$

As the equation suggests, Bayesians can overestimate $p(\$250\text{K}|\text{Republican})$ for two different reasons. They may believe that a larger share of Republicans fit the Republican stereotype of being wealthy than in reality. Alternatively, they may believe that a larger share of Americans

[2] Bordalo et al. (Forthcoming) formalize the difference between what we term "population" and "subpopulation" estimates as follows: The degree to which a group composes the population is "the probability that a randomly drawn member from a universe Ω belongs to group G," or $p(g)$, the group's base rate. By contrast, the degree to which a group composes a distinct subpopulation is the probability we draw "type t once we know that we are facing group G." In our notation, this is $p(g|s)$. Examples include the percentage of Republicans who earn $250,000 per year or more and the percentage of Americans in poverty who are black. In these cases, $\{\$250\text{K earners, blacks}\} \in G$ and $\{\text{Republicans, poor Americans}\} \in S$.

earn over \$250,000 than in reality, i.e., overestimate $p(\$250\text{K})$. The latter would imply that they would also overestimate the percentage of Democrats and Independents who are very wealthy, and naturally, such a response would reflect little about party stereotypes.

Note, however, that our assumption of numerate Bayesians is likely heroic. Data suggest that people are not just ignorant of base rates, but that they are also liable to neglect them even when they know them. Failure to attend to the base rate $p(g)$ when estimating $p(g|s)$—the *base rate fallacy*—is a sign that people are using the representativeness heuristic, equating $p(g|s)$ with $p(s|g)$ (Kahneman and Frederick 2002). Thus, researchers interested in assessing potential mechanisms behind misperceptions about subpopulation composition may find value in providing and/or experimentally manipulating base rates.

Researchers interested in correcting misperceptions may also find value in priming $p(s|\neg g)$ before asking respondents about $p(g|s)$. This could be done in several ways. A survey might simply ask respondents about the percentage of Republicans who earn less than \$250,000 per year. More creatively, it might ask respondents to think about five Republican friends or associates—who, because only a small fraction of the populations earns that much, are unlikely to earn over \$250,000 per year—or even to write down something about their jobs or socioeconomic status (e.g., Thorson 2016).

Data and Research Design

The data primarily come from two surveys with multiple embedded experiments conducted on Amazon's Mechanical Turk (MTurk). MTurk is a market for trading small services. Compared to the population, survey respondents recruited on MTurk tend to be younger, better educated, and more likely to identify with the Democratic party (Berinsky, Huber and Lenz 2012) (For details about the composition of the samples, see Online Appendix [OA] OA 1.1 and OA 2.1.) Pleasingly for us, MTurk respondents' perceptions of party composition are similar to those of respondents from more representative samples (see Ahler and Sood 2016). This suggests that results for the party composition items in the experiments are likely generalizable to the broader population.

We conducted the first study (Study 1) in November, 2014 to assess concerns about the validity of the finding that Americans overestimate the share of party-stereotypical groups in the two main political parties (see Ahler and Sood 2016). To assess the concern, we randomly assigned respondents to one of four conditions: `standard estimation`, a control condition, and three other conditions designed to assess the validity of a specific alternative explanation or interpretation of our results. We describe the different conditions in greater detail as part of discussion about each specific concern.

In the second study (Study 2), we asked about perceptions of shares of a broader variety of groups: 1) the share of Democrats who are atheist/agnostic, black, gay/lesbian/bisexual, and union members, 2) the share of Republicans who are age 65+, Evangelical, Southern, and earn over $250,000 per year, 3) the share of Americans who drink diet soda (for some respondents), and the percentage of American men and women who do so (all respondents), 4) the percentage of people living in America who were born outside the US (e.g., Alba, Rumbaut and Marotz 2005), and 5) the percentage of the American poor who are black (e.g., Gilens 1996). As before, we describe the specific experiments in Study 2 as part of the discussion about each specific concern.

Top-of-the-Head vs. Considered Responses

Top-of-the-head answers are likely closest to the numbers that (fuzzy) stereotypes implicitly map to. If so, how biased (if at all) are more considered responses? To evaluate that, in Study 2, we assigned roughly 300 respondents to a timing experiment. Half of the respondents were randomly assigned to the `time pressure` condition in which they were only given 10 seconds to answer each item, and half were assigned to the `time requirement` condition in which they had to wait 15 seconds with just the question text on the screen before they could input their response. Respondents were alerted about the timed portion of the survey before answering the questions.

(See the online appendix to Ahler and Sood 2016 for depictions of all treatments in Study 1. See OA 2.2 for depictions of all treatments in Study 2.)

Use of Denominators Larger than 100

To gauge the impact of implicit use of denominators larger than 100, we conducted two experiments. In Study 1, respondents were assigned to a `sum-to-100` condition in which they not only reported their beliefs about the percentage of partisans belonging to a party-stereotypical group, but also beliefs about a comprehensive set of complementary, mutually exclusive categories. For instance, respondents not only estimated the percentage of Republicans who are evangelical Christian, but also the percentage of Republicans who are mainline Protestant, Catholic, and "other/no religion." We required that the estimates sum to 100. To help respondents make sure that their estimates summed to 100, an on-screen counter tracked the total. The difference between results in the `sum-to-100` condition and the `standard estimation` condition give us the extent to which implicit use of denominators larger (different) than 100 affects estimates of $p(\text{group}|\text{party})$.

In Study 2, another 300 respondents were assigned to a wording experiment designed for the same purpose. We randomly manipulated question stems to read either, "Out of every 100 P/S, how many do you think are G?" or, "What percentage of P/S do you think are G." Following Sides and Citrin (2007*b*), we expect "Out of every 100 ..." to make the correct denominator more salient. We manipulated the stems of the party composition items, foreign-born population item, and demographic composition of the poor item.

Motivated Responding

We assessed the extent to which motivated responding affects responses by offering `accuracy incentives` to a random subset of respondents in Study 1. Respondents in the `accuracy` `incentives` condition received an additional five cents, 20% of the compensation for finishing the survey (25 cents) for each response that fell within five percentage points of the truth. While the bonus may seem small, given that respondents answered items on eight groups' shares, they had the opportunity to nearly triple what they made for the survey. If Americans' apparent misperceptions about party composition reflect motivated responding, estimates of respondents assigned to the `incentives` condition should be substantially different from those elicited without incentives.

Beliefs About Related Quantities and Interpretation of Responses

Do misperceptions about $p(\text{group}|\text{party})$ merely reflect misperceptions about $p(\text{group})$ and nothing particular to partisan stereotypes? We gauged the possibility in three ways. Firstly, and perhaps most dispositively, in Study 1, we removed ignorance about base rates as a plausible alternative explanation. We did so by anchoring sliders at the base rate for each party-stereotypical group, alerting respondents to this design feature, and then asking them to use the sliders to estimate $p(\text{group}|\text{party})$. Significantly lower estimates in the `base rates` condition would mean that inflated base rates *potentially* explain inflated beliefs about $p(\text{group}|\text{party})$.

Secondly, in the `standard estimation` condition, we asked respondents to estimate the groups' base rates in addition to their prevalence in a particular party. We can use these data to directly assess people's beliefs about base rates. Moreover, we can compare respondents' estimated group base rates to their estimates of group shares in the party to test whether misperceptions reflect anything beyond base rate ignorance. Finally, to better understand misperceptions about subpopulation composition, we can compute a difference-in-differences:

$$(p(\text{group}|\text{representative party})_P - p(\text{group}|\text{representative party})_A) - (p(\text{group})_P - p(\text{group})_A)$$

where P indexes perceived quantities and A indexes actual quantities. Note that the former estimate, $p(\text{group}|\text{representative party})_P - p(\text{group})_P$ informs us as to whether people's misperceptions significantly exceed what we would expect if they were only using their erroneous base rates to assess $p(\text{group}|\text{party})$. The difference-in-differences estimate, by contrast, informs us as to whether party stereotypes or erroneous beliefs about the group's base rate more strongly color judgments about party composition.

Lastly, in Study 2, we used the diet soda items to test a hypothesis regarding beliefs about related quantities. Although roughly equal percentages of men (23%) and women (24%) report drinking diet soda (Gallup 2013), advertising often targets women (Lin 1998; Yoder, Christopher and Holmes 2008). As such, we suspect that Americans likely overestimate the gap in diet soda consumption between men and women. Since the share of men and women in the population is roughly 50% and common knowledge, we can identify the source of error in people's perceptions of women as more likely to drink diet soda than men. We randomly assigned half of respondents to provide their belief about the base rate of diet soda consumption in America before answering the items specific to men and women. With between-conditions data, we can assess whether asking about base rates reduces error in reported perceptions of proportion of men and women who drink soda.

Results

Thinking (About Group Shares) Fast and Slow

For all ten items in the timing experiment—the eight party composition items, the percentage of foreign-born in the U.S. item, and the percentage of poor who are black item—responses are more accurate in the `time requirement` condition. Not all differences are statistically significant (although half are at $p < 0.05$ and 7 of 10 are at $p < 0.1$; see OA 2.3). To get a sense of the average difference across conditions across items, we regressed perceptual bias, the signed difference between a respondent's perception and the true estimate, on an indicator for assignment to the `time requirement` treatment, and fixed effects for items, clustering the standard errors by respondent.[3] On average, perceptual bias in the `time requirement` condition was 4.3 points lower than in the `time pressure` condition (see Figure 1). However, it's worth noting that this is only a 22% decline in perceptual bias. Across all items, bias in reported perceptions was 19.7 points in the `time pressure` condition, which fell to 15.5 points among those assigned to the `time requirement`.

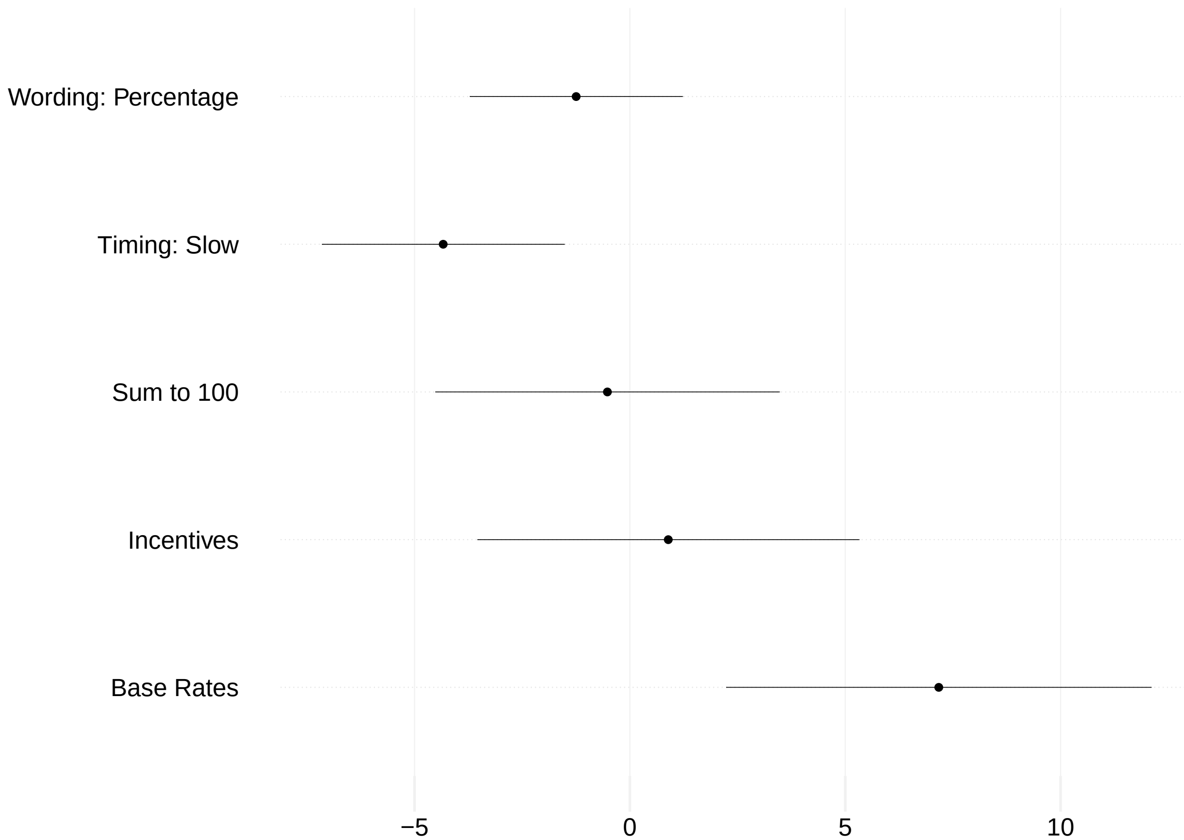
Lower bias in the `time requirement` condition, however, may be due to respondents using the additional time to consult outside sources. To assess the concern, we plotted the density curve of all responses to all the items, by treatment condition. If lower error in the `time requirement` condition was a consequence of cheating, we should observe spikes in the density plot at the correct answer. But we do not see these spikes (see OA 2.4). To formally test for cheating, we compared proportion correct (within one percentage point) across conditions; the data suggest no differences (see OA 2.5).

Use of Denominators Larger than 100

If people implicitly use denominators larger than 100 when reporting percentages, estimates in the `sum-to-100` condition should be significantly lower than in the `standard estimation` condition. However, when respondents are required to ensure that shares for a comprehensive set of mutually exclusive groups sum to 100, they are generally just as biased. For just one of the eight items—the black-Democratic group-party dyad—are estimates significantly less biased (see OA 1.1). As Figure 1 shows, average perceptual bias across all party-group dyads fell by just 0.5 points in the `sum-to-100` condition, from a baseline of 18.9 points in the `standard`

[3]By *perceptual bias*, we mean average directional error in the respondents' reports across items. In OA 2.8, we also investigate impact of the treatments on *absolute error*, average absolute error in respondents' reports across items, and *percentage bias*, average percentage by which respondents err (signed error) across items. Impact of the treatments on both *absolute error* and *percentage bias* is negligible.

Figure 1: Average Effects of Eliciting Perceptions of Various Groups in Different Ways



NOTE: 95% confidence intervals depicted. Results are from OLS regression of *perceptual bias* on treatment indicators (with baseline conditions noted here), with item fixed effects and standard errors clustered by respondent. The "standard estimation" condition serves as a baseline for the "Sum-to-100," "Incentives," and "Base rates" conditions. The "time pressure" condition serves as a baseline for the "time requirement" (or "slow") condition. The alternative stem wording ("Out of every 100...") serves as a baseline for the "Percentage" (standard stem wording) condition.

`estimation` condition. (Results are from OLS regression of error on indicators for Study 1 treatments, with item fixed effects and standard errors clustered by respondent.) Thus, inflated implicit denominators appear to minimally affect perceptions of group shares.

Finally, changing question stem has a small, variable effect on the responses. "Out of every 100..." wording reduced bias in just three of the ten items (see OA 2.6 for plots). Pooling across items, the "Out of every 100" wording is associated with a 1.2-point increase in perceptual bias. (Again, results are from an OLS regression of perceptual bias on an indicator for the "out of every 100" treatment, with item fixed effects and standard errors clustered by respondent.) Given that the usual "What percent" wording yields an average perceptual bias of 18.9 points, this difference is neither statistically nor substantively significant. In sum, neither priming the appropriate denominator nor constraining respondents to the right denominator significantly reduces bias in their estimates, suggesting that respondents' implicit use of denominators greater than 100 doesn't strongly bias reported perceptions.

Motivated Responding

In Study 1, we offered `incentives` to a random set of respondents to deter motivated responding. If reports of $p(\text{group}|\text{party})$ were distorted by motivated responding, estimates should be considerably different when incentives are given. They are not. As Figure 1 shows, pooling across items, average perceptual bias in the `incentives` condition is roughly the same as in the `standard` condition. Furthermore, responses are distributed similarly in the two conditions (see OA 1.3).

Additional observational evidence suggests that motivated responding did not significantly bias responses on these items. In a separate survey that we conducted on MTurk in April 2014, we asked respondents to tell us how they felt toward the eight party-stereotypical groups on a 101-point "feeling thermometer" scale (Ahler and Sood 2016). Later in the survey, we collected perceptions of $p(\text{group}|\text{party})$ for the four out-party dyads. If motivated responding explains the responses, group affect should predict beliefs about party composition. We do not. For all the party-group dyads, the linear relationship is extremely weak (see OA 1.4 for tables). And pooling across party-group dyads, a one-point increase in group feeling thermometer rating corresponds to just a .03-point decrease in perceptions of $p(\text{group}|\text{party})$—a relationship that is neither statistically nor substantively significant. In all, it appears that bias in reports of perceptions of shares of groups due to motivated responding is small.

Beliefs About Related Quantities

Even if measures of perceptions of $p(\text{group}|\text{subpopulation})$ capture genuine beliefs, they can still be hard to interpret. Notably, without additional data, it is unclear whether the perceptions reflect beliefs about composition of the subpopulation, or perceptions of the group's share in the population.

In Study 1, we had measured beliefs about the share of party-stereotypical groups in the population. Respondents generally overestimate groups' base rates, especially for groups that compose a small share of the population. (For example, gays, lesbians, and bisexuals are just 3.8% of the population but receive a mean estimate of 14.9%. Similarly, respondents on average estimate that those who earn over $250,000 per year are 11.4% of the population, versus 2% in reality. See OA 2.7). People, however, generally overestimate the share of these groups in the parties they "represent" more than their share in the population writ large ($p(\text{group}|\text{representative party})_P - p(\text{group})_P$). These differences are significant for all eight party-group dyads except for the black-Democratic dyad; like Wong 2007, we find that respondents greatly overestimate the share of African-Americans in the US population.

The difference between the extent to which people overestimate the share of party-stereotypical groups in "their" party and in the population can shed light on the extent to which party stereotypes are more influential than base rates in people's assessments of $p(\text{group}|\text{party})$. These difference-in-differences estimates are significantly positive, implying a larger role for party stereotypes, for five of the eight dyads; estimates are not significantly different from zero for the others.

In addition to these descriptive and observational analyses, we conducted an experiment in Study 1 to more cleanly estimate the extent to which beliefs about base rates explain beliefs about party composition. As we discuss above, we randomly assigned one group of respondents to answer the party composition items with sliders anchored at the groups' base rates, informing respondents we had done so. As Figure 1 shows, reported perceptions became more biased, not less. These results suggest that perceptions like these are driven by *representativeness*; people's focus on the idea that "like goes with like" leads them to ignore other pertinent information like base rates (Gilovich and Savitsky 1996).

Lastly, asking people about their beliefs about base rates before asking about share of subgroups does not reduce bias. Recall that in Study 2 we had randomly assigned half of respondents to report their beliefs about the percentage of Americans who drink diet soda (24% in reality) before eliciting their beliefs about the percentage of men (23%) and women (24%) who do so. Respondents in the "no base rate" condition significantly overestimated the gender gap in diet soda consumption, reporting that 46.2% of women drink diet soda compared to 30.0% of men. If anything, those who were first asked about the base rate (estimated on average as 41.0%) became somewhat worse: rather than altering their estimates about women (46.7%), they may have changed their beliefs about men's diet soda consumption (27.4%). This difference in the estimated gender gap fails to reach statistical significance at conventional levels (95% CI: [-1.0, 7.2]) but comports with the evidence from Study 1. Overall, priming or providing base rates has little effect on people's reported beliefs about subpopulation composition, and may even make those reports more biased.

Discussion

A variety of politically salient concerns are linked to misperceptions about the share of groups in the population or in various subpopulations. For instance, nativist sentiment is associated with inaccurate beliefs about the share of immigrants in the population (e.g., Alba, Rumbaut and Marotz 2005), anti-welfare attitudes are linked to misperceptions about the share of poor who are black (Gilens 1996; 1999), and partisan antipathy to erroneous beliefs about the share of out-party supporters who belong to party-stereotypical groups (Ahler and Sood 2016). These concerns take on additional heft given how often they are exploited in political campaigns. For instance, both the "Brexit" referendum and Donald Trump's 2016 presidential campaign prominently highlighted claims about immigration levels.

These concerns, as well as the broader social scientific study of stereotyping, (e.g., Bordalo et al. Forthcoming), thus increasingly face challenges regarding valid measurement of perceptions of share of groups. This is vital not just for accurate description of people's perceptions, but also for producing accurate estimates of causes and consequences of misperceptions. In this monograph, our aim was to describe and probe several of these unique concerns, suggest methods for gauging these concerns, and finally demonstrate the use of some of the tools.

Results from these experiments suggest that people carry cognitions about the share of some politically salient and stereotypical groups in the population and some subpopulations, and that these beliefs can be reliably elicited by asking respondents to give numerical estimates of the share of these groups. In particular, the data suggest that commonly noted concerns like motivated responding, use of denominators larger than 100, and cheating—and less commonly noted concerns like considered responding—are unlikely to induce significant bias. And the impact of addressing some of the concerns may even be in the opposite direction than hypothesized. For instance, providing groups' base rates made reported beliefs about those groups' shares in certain subpopulations slightly worse.

The conclusions, however, may not hold for items other than those discussed here. We focused on items tapping prevalent group stereotypes. And we contend that for such groups, people are liable to have crude but firm beliefs about their numerical strength. Separately, our claim is not that the concerns raised about these measures never apply—we cannot even confidently rule out all the concerns for all the measures presented here without collecting significant additional data. Instead, our purpose was to highlight and illustrate some of the inferential strategies that researchers can use to assess the severity of the most pressing of these concerns in the data that they collect.

Much of science reduces to measurement—of a phenomenon, its causes, and its consequences. And much of scientific progress has been built on improvements in measurement. In this chapter, we highlighted some of the challenges in measuring perceptions of group shares and some strategies to assess the issues. Our hope is that the chapter will spur additional conversation and research on how best to measure these important quantities. And that a better measurement machinery for assessing numerical perceptions of group strength will lead to progress in our understanding of an important driver of inter-group conflict.

References

Ahler, Douglas J and Gaurav Sood. 2016. "The Parties in our Heads: Misperceptions About Party Composition and Their Consequences.".

Alba, Richard, Ruben G Rumbaut and Karen Marotz. 2005. "A distorted nation: Perceptions of racial/ethnic group sizes and attitudes toward immigrants and other minorities." *Social Forces* 84(2):901–919.

Ansolabehere, Stephen, Marc Meredith and Erik Snowberg. 2013. "Asking about numbers: Why and how." *Political Analysis* 21(1):48–69.

Barthel, Michael, Amy Mitchell and Jesse Holcomb. 2016. "Many Americans Believe Fake News Is Sowing Confusion." *Pew Research Center* . http://www.journalism.org/2016/12/15/many-americans-believe-fake-news-is-sowing-confusion/.

Benjamini, Yoav and Yosef Hochberg. 1995. "Controlling the false discovery rate: a practical and powerful approach to multiple testing." *Journal of the Royal Statistical Society. Series B (Methodological)* pp. 289–300.

Berinsky, Adam J., Gergory A. Huber and Gabriel S. Lenz. 2012. "Evaluating Online Labor Markets for Experimental Research: Amazon.com's Mechanical Turk." *Political Analysis* 20(2):351–368.

Blalock, Hubert M. 1967. "Toward a theory of minority-group relations.".

Bordalo, Pedro, Katherine Coffman, Nicola Gennaioli and Andrei Shleifer. Forthcoming. "Stereotypes." *Quarterly Journal of Economics* .

Bullock, John G, Alan S Gerber, Seth J Hill and Gregory A Huber. 2013. Partisan bias in factual beliefs about politics. Technical report National Bureau of Economic Research.

Campbell, Angus, Philip E. Converse, Warren E. Miller and Donald E. Stokes. 1960. *The American Voter.* Chicago: University of Chicago Press.

Campbell, Donald T. 1965. Ethnocentric and other altruistic motives. In *Nebraska symposium on motivation.* Vol. 13 pp. 283–311.

Dancygier, Rafaela M. 2010. *Immigration and conflict in Europe.* Cambridge University Press.

Delli Carpini, Michael X. and Scott Keeter. 1996. *What Americans Know About Politics and Why It Matters.* New York: Yale University Press.

Freeder, Sean, Gabriel S. Lenz and Shad Turney. 2016. "What Goes with What: Reinterpreting the Evidence on Attitude Stability, Policy Voting, and Multi-Item Issue Scales." Working paper: University of California, Berkeley.

Gallup. 2013. ""Regular Soda Popular With Young, Nonwhite, Low-Income"". http://www.gallup.com/poll/163997/regular-soda-popular-young-nonwhite-low-income.aspx.

Gennaioli, Nicola and Andrei Shleifer. 2010. "What Comes to Mind." *Quarterly Journal of Economics* 125(4):1399–1433.

Gilens, Martin. 1996. "Race and Poverty in America: Public Misperceptions and the American News Media." *Public Opinion Quarterly* 60(4):515–541.

Gilens, Martin. 1999. *Why Americans Hate Welfare: Race, Media and the Politics of Antipoverty Policy.* Chicago: University of Chicago Press.

Gilovich, Thomas and Kenneth Savitsky. 1996. Like Goes with Like: The Role of Representativeness in Erroneous and Pseudoscientific Beliefs. In *The Skeptical Inquirer.* Vol. 20 pp. 34–40.

Green, Donald, Bradley Palmquist and Eric Schickler. 2002. *Partisan Hearts & Minds: Political Parties and the Social Identities of Voters.* New Haven, CT: Yale University Press.

Jackson, Brooks and Kathleen Hall Jamieson. 2007. *unSpun: Finding facts in a world of disinformation.* Random House.

Kahneman, Daniel. 2011. *Thinking, Fast and Slow.* New York: FSG.

Kahneman, Daniel and Shane Frederick. 2002. Representativeness Revisited: Attribute Substitution in Intuitive Judgment. In *Heuristics of Intuitive Judgment: Extensions and Applications*, ed. Thomas Gilovich, Dale Griffin and Daniel Kahneman. New York: Cambridge University Press.

Key, Valdimer. 1949. "Southern politics in state and nation.".

Khanna, Kabir and Gaurav Sood. 2015. Motivated Learning or Motivated Responding? Using Incentives to Distinguish Between the Two Processes. In *Annual Meeting of the Midwest Political Science Association.*

Krogstad, Jens Manuel and Jeffrey S. Passel. 2016. "5 Facts About Illegal Immigration in the U.S." *Pew Research Center* 19 November. http://www.pewresearch.org/fact-tank/2015/11/19/5-facts-about-illegal-immigration-in-the-u-s/.

Lin, Carolyn A. 1998. "Uses of Sex Appeals in Prime-Time Television Commercials." *Sex Roles* 38(5/6):461–475.

Lippman, Walter. 1922. *Public Opinion.* Transaction Publishers.

Luskin, Robert C, Gaurav Sood and Joshua Blank. 2013. The Waters of Casablanca: Political Misinformation (and Knowledge and Ignorance). In *Annual Meeting of the Midwest Political Science Association.*

Nadeau, Richard, Richard G Niemi and Jeffrey Levine. 1993. "Innumeracy about minority populations." *Public Opinion Quarterly* 57(3):332–347.

Petty, Richard and John Cascioppo. 1986. "The Elaboration Likelihood Model of Persuasion." *Advances in Experimental Social Psychology* 19:123–181.

Prior, Markus, Gaurav Sood, Kabir Khanna et al. 2015. "You cannot be serious: The impact of accuracy incentives on partisan bias in reports of economic perceptions." *Quarterly Journal of Political Science* 10(4):489–518.

Romer, Daniel, Kathleen Hall Jamieson and Sean Aday. 2003. "Television News and the Cultivation of Fear of Crime." *Journal of Communication* 53(1):88–104.

Sherif, Muzafer. 2015. *Group conflict and co-operation: Their social psychology.* Vol. 29 Psychology Press.

Sides, John and Jack Citrin. 2007*a*. "European opinion about immigration: The role of identities, interests and information." *British journal of political science* 37(03):477–504.

Sides, John and Jack Citrin. 2007*b*. How large the huddled masses? The causes and consequences of public misperceptions about immigrant populations. In *annual meeting of the Midwest Political Science Association, Chicago, IL.*

Tajfel, Henri. 1970. "Experiments in intergroup discrimination." *Scientific American* 223(5):96–102.

Tajfel, Henri and John C. Turner. 2005. *Political Psychology.* New York: Psychology Press chapter The Social Identity Theory of Intergroup Behavior.

Thorson, Emily A. 2016. "Some of My Best Friends are Poor? Income Misperceptions and Policy Attitudes." Working paper.

Tversky, Amos and Daniel Kahneman. 1974. "Judgment Under Uncertainty: Heuristics and Biases." *Science* 185:1124–1131.

Wong, Cara J. 2007. ““Little” and “Big” Pictures in Our Heads Race, Local Context, and Innumeracy About Racial Groups in the United States.” *Public Opinion Quarterly* 71(3):392–412.

Wong, Cara, Jake Bowers, Tarah Williams and Katherine Drake Simmons. 2012. “Bringing the person back in: Boundaries, perceptions, and the measurement of racial context.” *The Journal of Politics* 74(04):1153–1170.

Yoder, Janice D., Jessica Christopher and Jeffrey D. Holmes. 2008. “Are Television Commercials Still Achievement Scripts for Women.” *Psychology of Women Quarterly* 32(3):303–311.

Online Appendix

OA 1 Study 1

Study 1 utilizes data from Ahler and Sood (2016). The results presented in SI I are also part of the appendix in Ahler and Sood (2016), and provided here only for convenience.

OA 1.1 Study 1: MTurk Sample Demographics

Table OA 1.1: Characteristics of the MTurk Sample

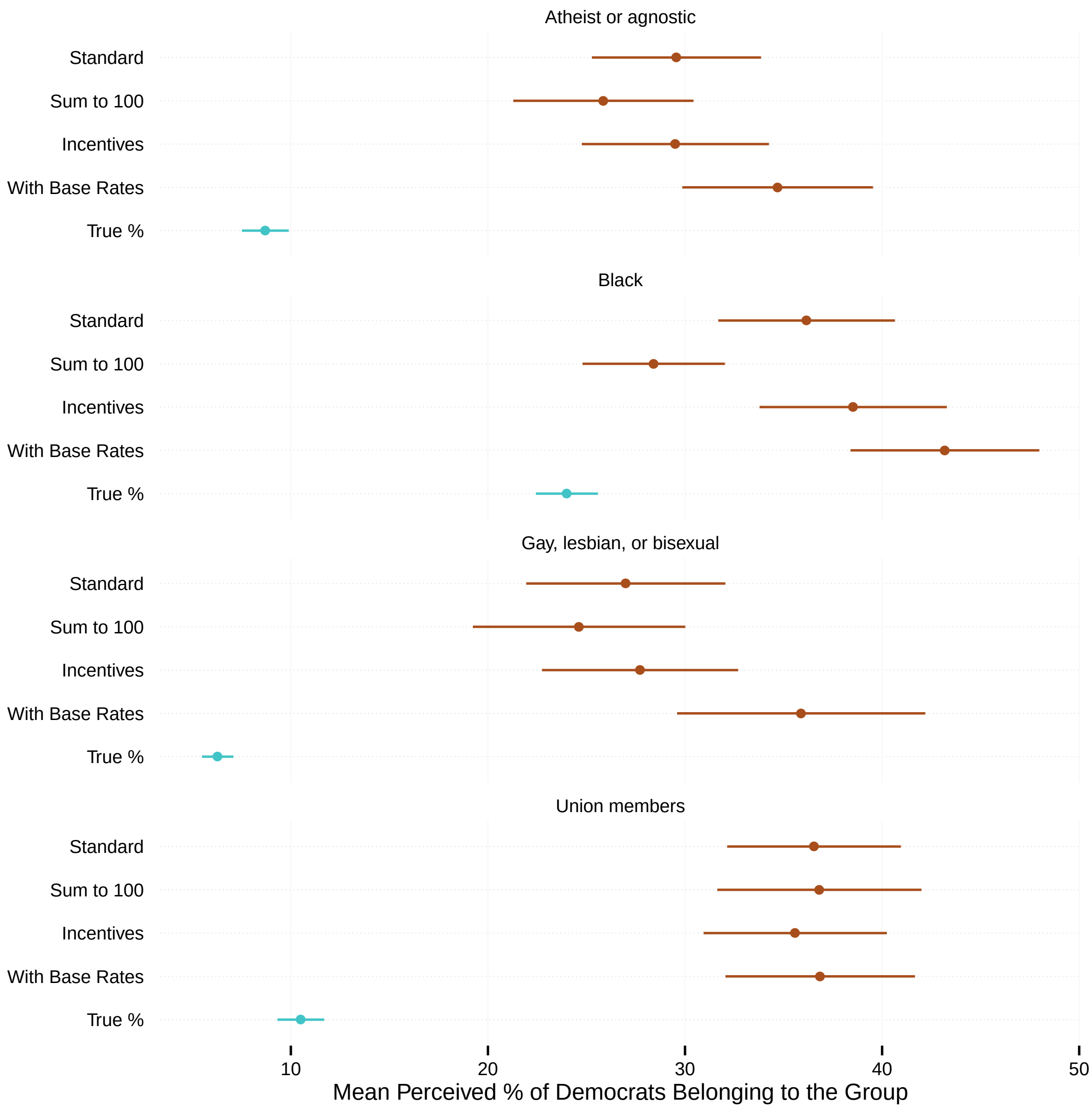
	Sample	Population
Partisanship		
Democratic (inc. leaners)	58.9%	49.0%
Republican (inc. leaners)	22.3%	39.0%
Non-leaning Independent	18.9%	11.9%
Gender		
Female	50.5%	50.9%
Male	49.5%	49.1%
Race/Ethnicity		
White/Caucasian	76.2%	63.7%
Black/African-American	8.1%	12.2%
Asian-American/Pacific Islander	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.

OA 1.2 Study 1 Results by Item

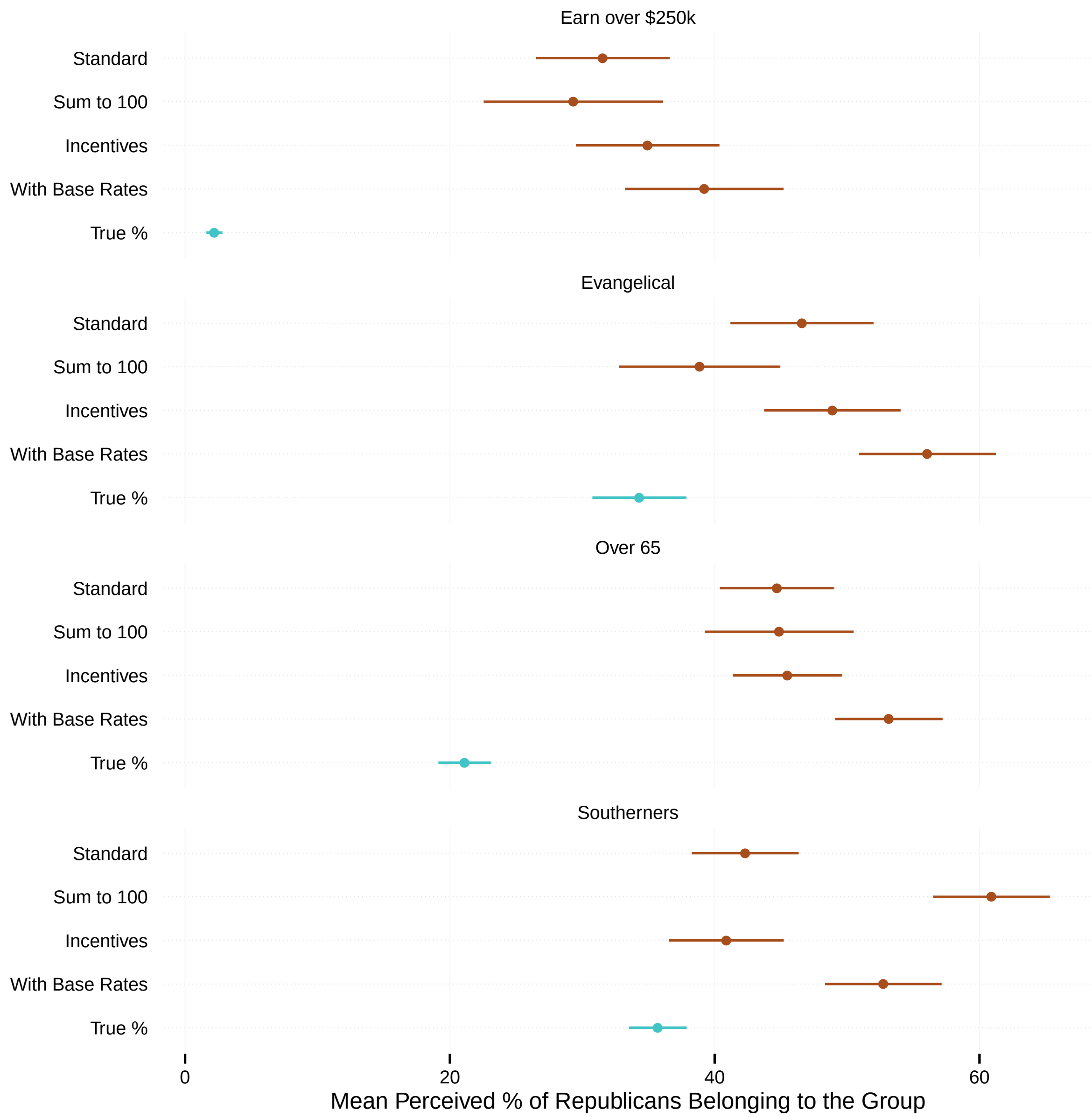
Figure OA 1.1: Expressive Responding, Innumeracy, and Poor Knowledge of Base Rates Fail to Explain Apparent Perceptual Errors

(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.

OA 1.3 Kolmogorov-Smirnov Tests

Table OA 1.2: Kolmogorov-Smirnov tests of Distributions

	Incentives		Sum-to-100		Base Rates	
Party-Group	D	p(Same dist.)	D	p(Same dist.)	D	p(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 $\alpha = 0.01$.

OA 1.4 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. (Note that in this study we swapped one party-stereotypical group, "people over 65" for Republicans, for a counter-stereotypical group, "people under 35.")

Table OA 1.3: 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.

OA 2 Study 2

OA 2.1 MTurk Sample Demographics

Table OA 2.4: Characteristics of the MTurk Sample

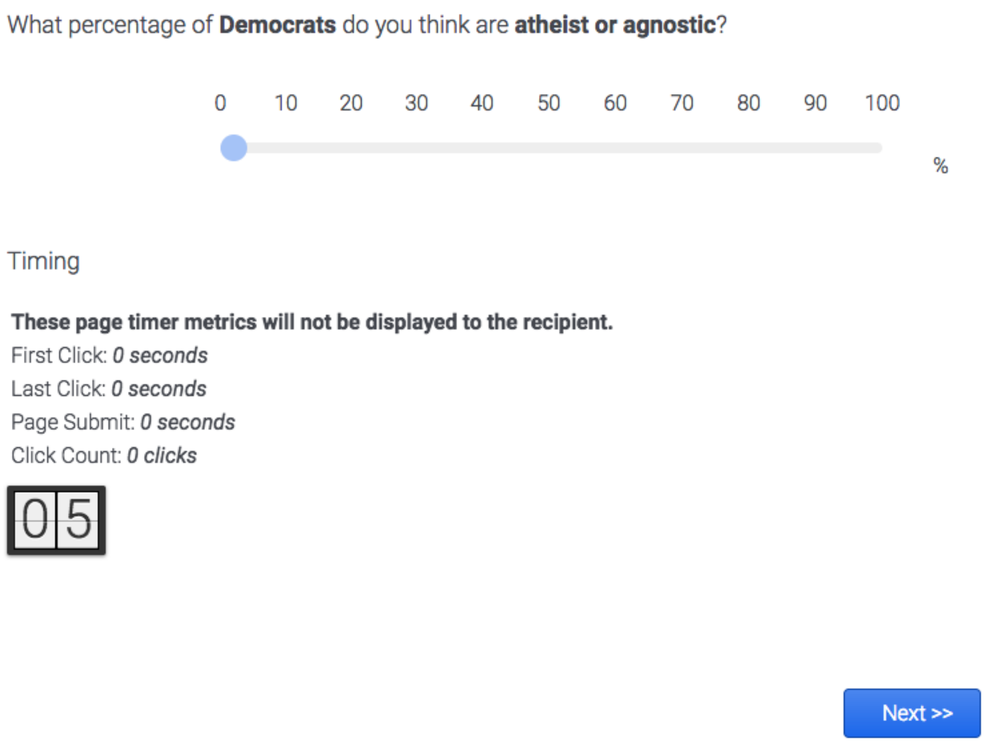
	Sample	U.S. Population
Partisanship		
Democratic (inc. leaners)	56.0%	49.0%
Republican (inc. leaners)	28.0%	39.0%
Non-leaning Independent	16.0%	11.9%
Gender		
Female	50.5%	50.9%
Male	49.5%	49.1%
Race/Ethnicity		
White/Caucasian	78.0%	63.7%
Black/African-American	6.7%	12.2%
Asian-American/Pacific Islander	7.8%	4.8%
Latino/Hispanic	19.4%	16.4%
Age		
18-39	66.8%	39.1%
40-64	30.9%	43.7%
65+	2.4%	17.2%

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

OA 2.2 Depiction of Study 2 Treatments

Figure OA 2.1: Timing Experiment

(a) "Time Pressure" Condition



(b) "Time Requirement" Condition

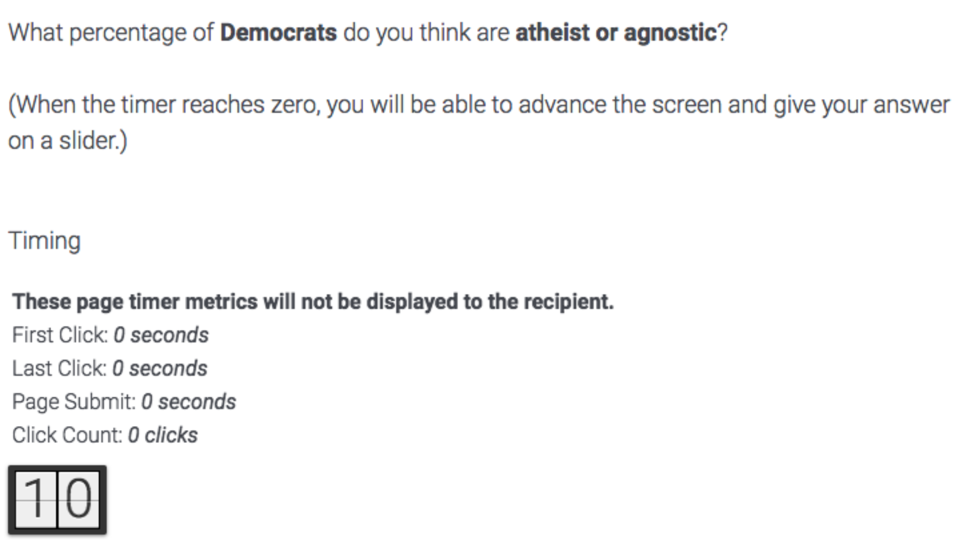
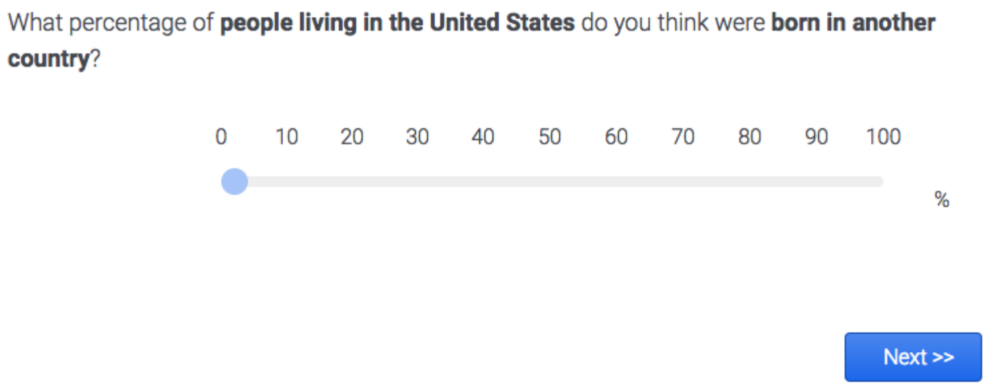


Figure OA 2.2: Wording Experiment

(a) "Percentage" Condition



(b) "Out of 100" Condition

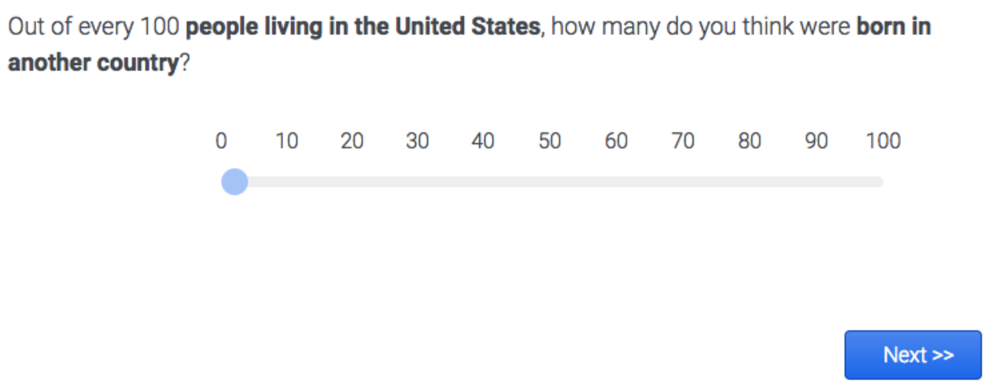
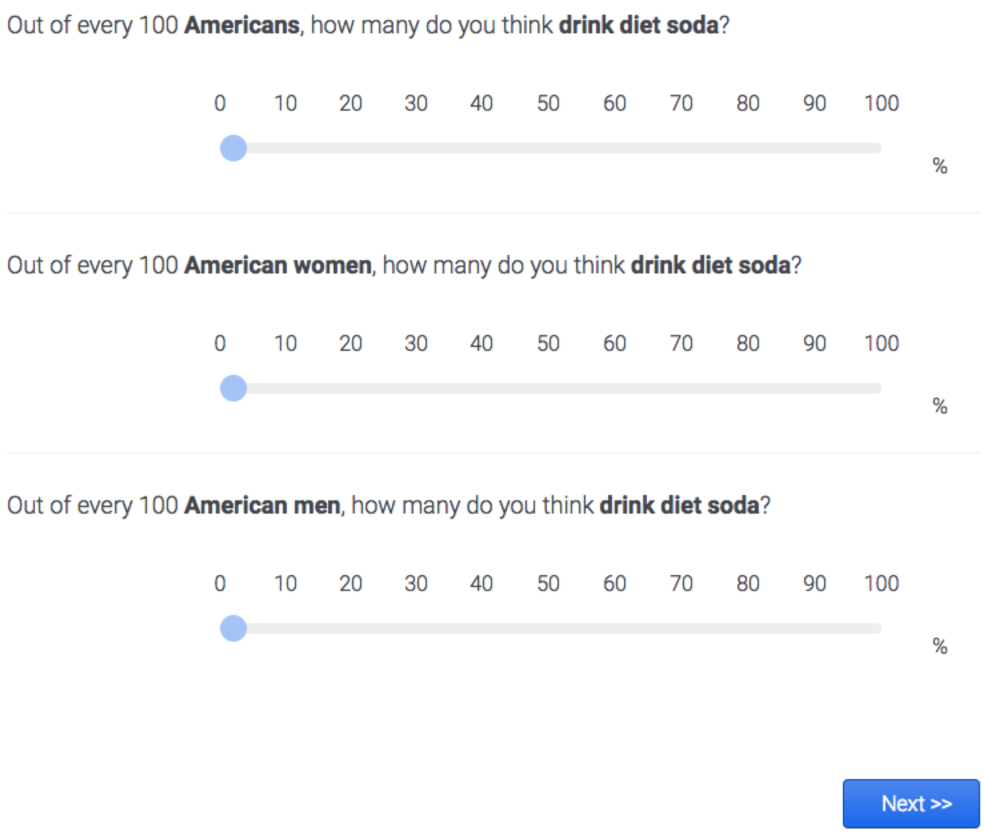


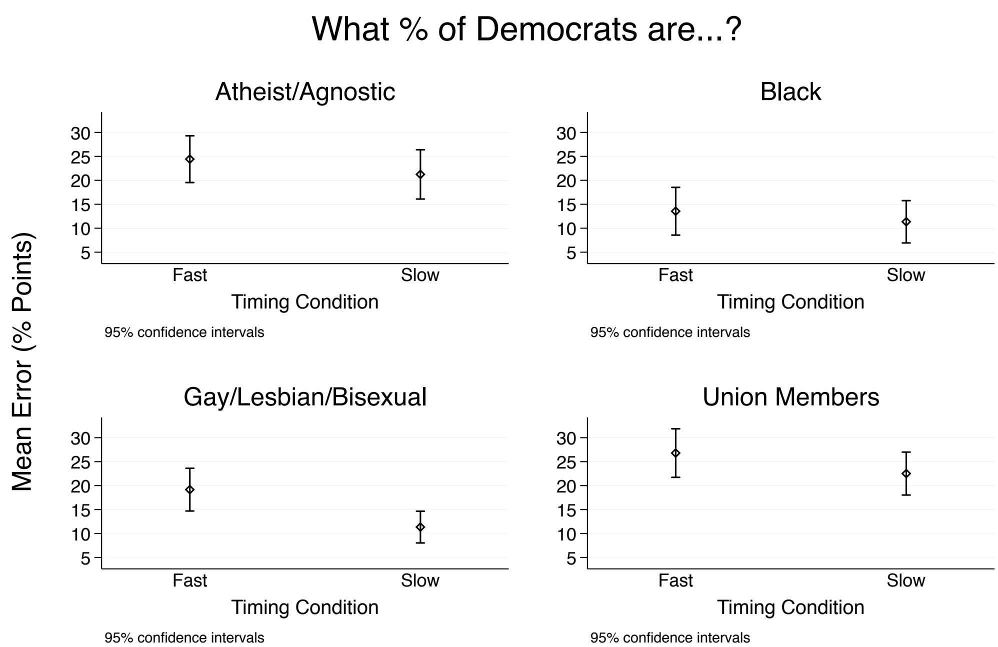
Figure OA 2.3: "Base rates and diet soda" experiment—"Control" condition omits top item



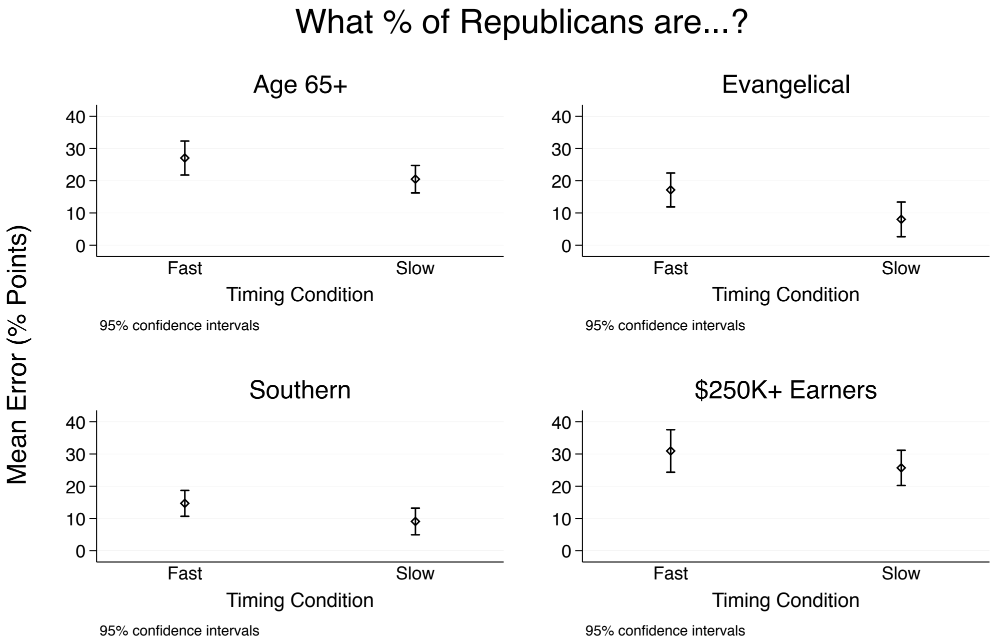
OA 2.3 Timing Experiment: Average Estimates by Item and Condition

Figure OA 2.4: Average Responses (and Perceptual Error) Tend to Be Lower in the "Slow" Condition

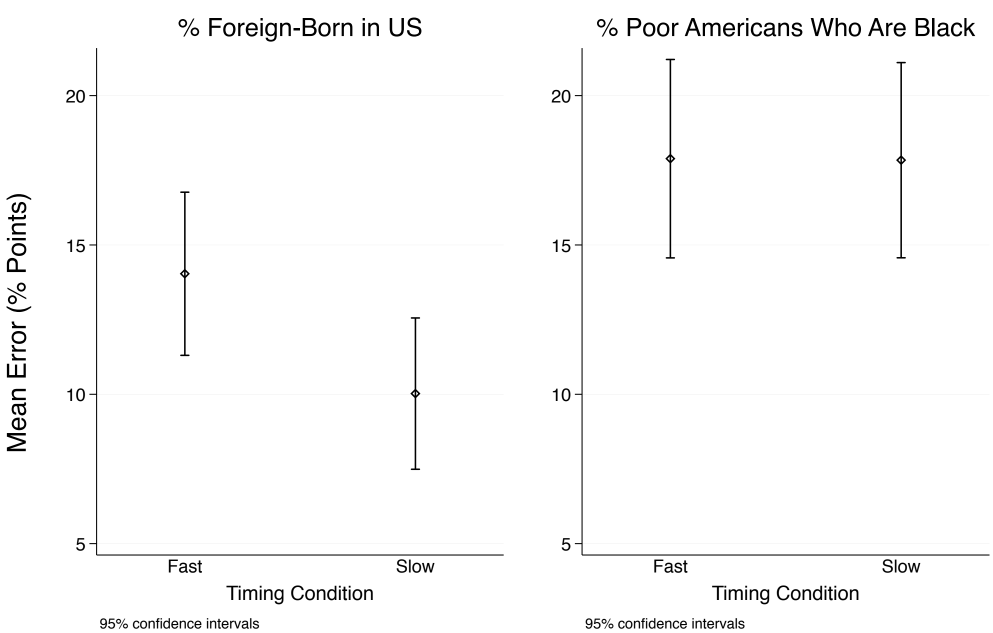
(a)



(b)



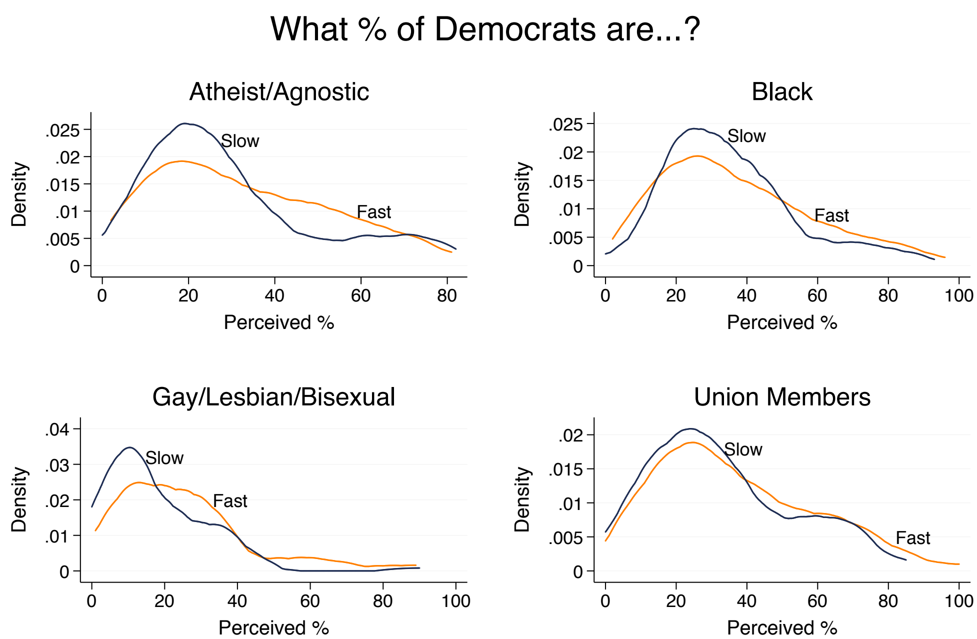
(c) Estimates for the Immigration and Race/Poverty Items



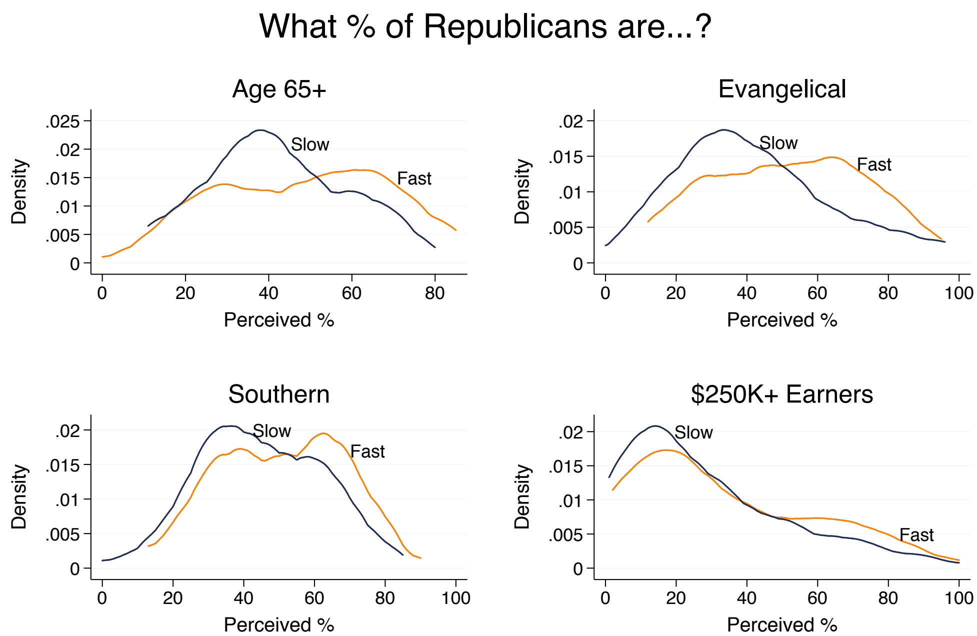
OA 2.4 Timing Experiment: Density Plots by Item and Condition

Figure OA 2.5: Average Responses (and Perceptual Error) Tend to Be Lower in the "Slow" Condition

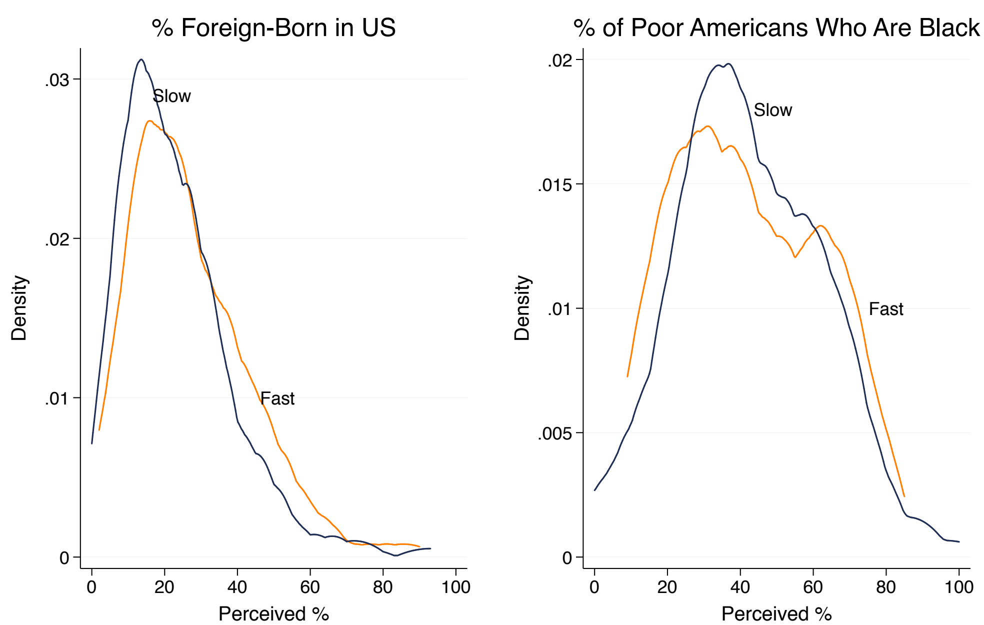
(a)



(b)



(c) Estimates for the Immigration and Race/Poverty Items



OA 2.5 Timing Experment: A Statistical Test Showing No Evidence of Cheating

One concern in the timing experiment is that respondents, given more time to consider their responses, may use that time to consult outside references. This is undesirable because we want to gauge actual perceptions that people generally hold. To assess this, we calculate the percentage of respondents in each condition (slow, or "time requirement," versus fast, or "time pressure") responding with either integer adjacent to the correct response. (E.g., if someone said that 8 or 9 percent of Democrats are atheist or agnostic, they would be "correct" since the correct response is 8.7.) As the table below shows, few significant differences emerge on individual items. Only in the case of responses to the percentage of Republicans who are from the U.S. South are responses significantly more accurate ($p < .1$) in the "slow" condition. Aggregating across items (including item fixed effects and clustering standard errors by respondent) lends more power, and as the bottom line of the table indicates, we do detect a significant difference across conditions. 1.3% more responses are "on the nose" in the "slow" condition. However, interpreting this result ameliorates concerns. First, the difference is substantively very small. Even in the "slow" condition, very few responses are close-to-correct—just 3.5% (compared to 2.2% in the "fast" condition). This suggests that even if there is cheating, only 1% of respondents are doing so. Further, the increase is not consistent across all items—three items show a decrease in the "slow" condition, which provides evidence against systematic cheating. More likely, respondents simply became less biased in the "slow" condition as a result of having time to consider the numbers they called to mind.

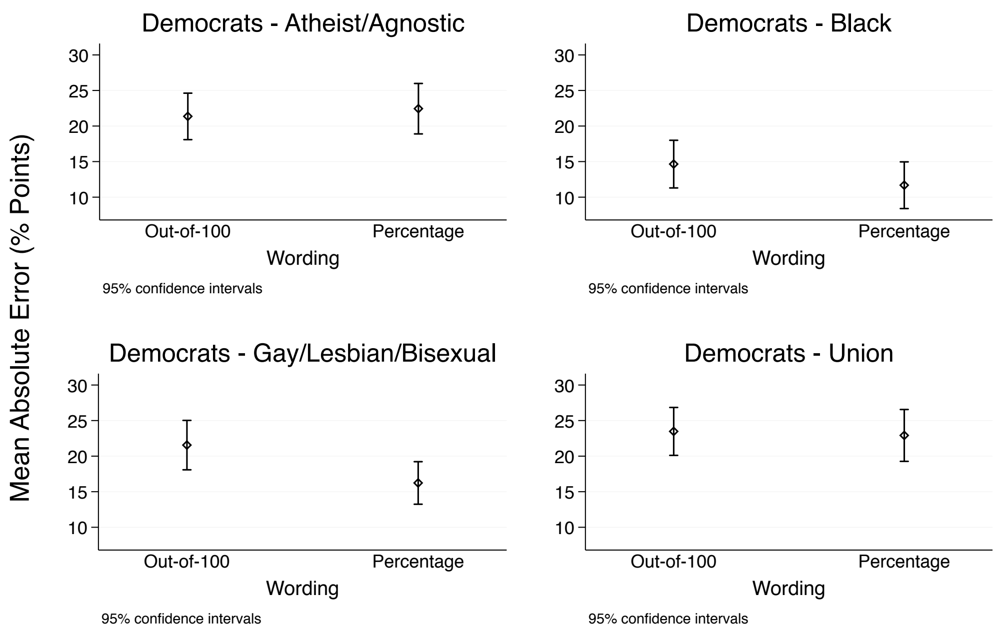
Table OA 2.5: Few Respondents Answer Perceptual Items Exactly Correct, and Differences are Small Across Timing Conditions

	Fast	Slow	Difference	Std. Error of Dif.	n	$P > \|t\|$
% Dems. − Ath./Ag.	.02	.01	-.01	.01	284	.32
% Dems. − Black	.04	.06	.03	.03	284	.27
% Dems. − LGB	.02	.06	.04	.02	284	.13
% Dems. − Union	.02	.04	.03	.02	284	.48
% Reps. − Age 65+	.01	0	-.01	.01	284	.32
% Reps. − Evang.	.02	.03	.01	.02	284	.70
% Reps. − Southern	.01	.05	.04	.02	284	.09
% Reps. − $250K+	.02	.01	-.01	.01	284	.32
% U.S. foreign-born	.02	.05	.03	.02	284	.20
% U.S. poor − Black	.04	.06	.02	.02	284	.40
Across all items	.02	.04	.01	.01	2840	.05

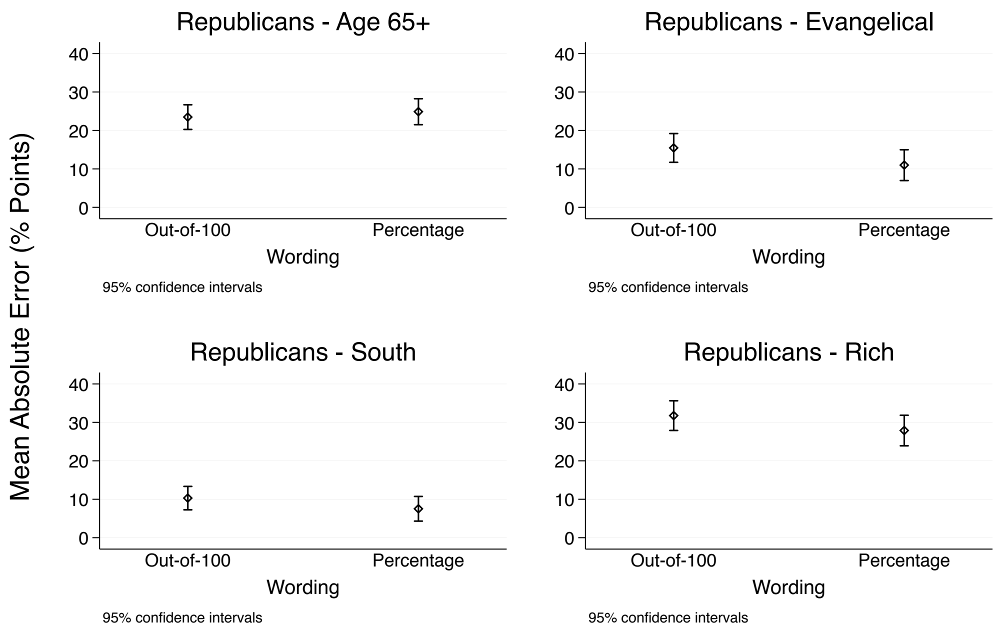
OA 2.6 Wording Experiment: Average Estimates by Item and Condition

Figure OA 2.6: Little Apparent Relationship Between Perceptual Error and “Out of 100” or “What Percentage” Wording

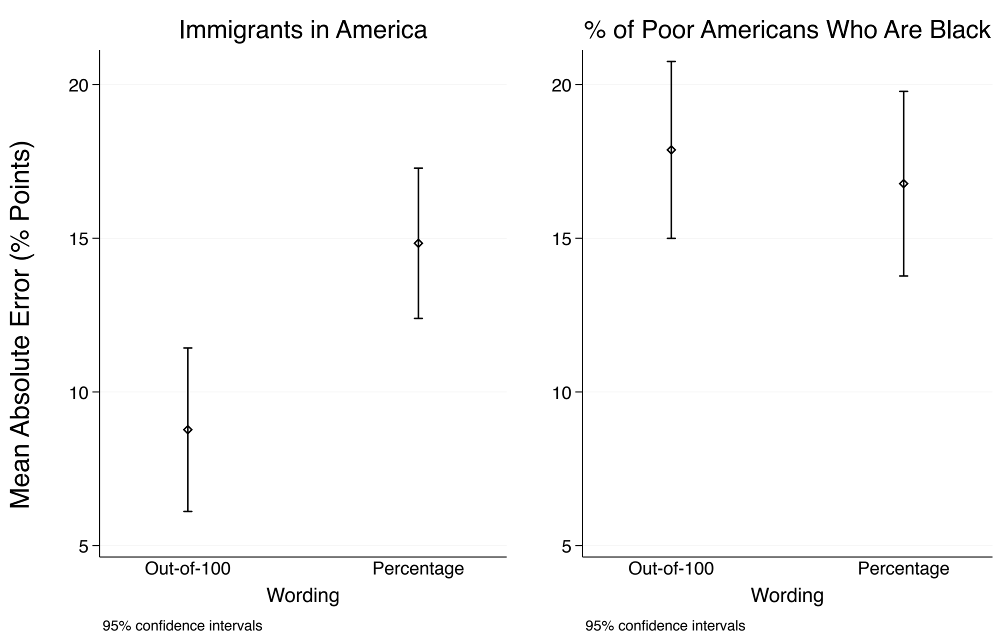
(a)



(b)



(c) Estimates for the Immigration and Race/Poverty Items



OA 2.7 Perceptions of Base Rates of Party-Stereotypical Groups in the Population at Large

In the Study 1's `standard estimation` condition, after measuring party-specific perceptions, we asked respondents 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 OA 2.6: 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**

+ $p < .1$, *$p < .05$, **$p < .01$, ***$p < .001$

Difference-in-Differences Estimator

To better understand misperceptions about subpopulation composition, we can compute a difference-in-differences:

$$(p(\text{group}|\text{representative party})_P - p(\text{group}|\text{representative party})_A) - \\ (p(\text{group})_P - p(\text{group})_A)$$

where P indexes perceived quantities and A indexes actual quantities. While the results in the above table confirm that base rates alone do not drive these perceptions, and that they reflect something about party stereotypes, the D/D estimator can tell us whether one of those factors—misperceived base rates or party stereotyping—more heavily influences the apparent beliefs we observe. If the D/D estimate is significantly negative, then the misperception is more a function of base rate error, in the aggregate. By contrast, if the D/D estimate is significantly positive, then the base rate error contributes relatively little to the misperception in the aggregate. The table below presents the results, showing that base rate error contributes less than apparent party stereotyping for five of the eight party-group dyads, and contributes more for none of them.
Note that the quantity being estimated (and presented in the table) is the raw error of the party-specific perception minus the raw error of the base rate perception.

Table OA 2.7: Difference-in-Differences Estimates: Do Misperceptions of Group Base Rates Contribute Significantly More/Less Than Beliefs About Parties to the Misperceptions We Observe?

Party-Group Dyad	D/D Estimate
Republicans—Southerners	7.04 **
Over 65	7.88 **
Republicans—Evangelical	-0.82
Republicans—Earning Over $250K 16.99 ***	
Democrats—Black	4.56
Democrats—Atheists or Agnostics	2.10
Democrats—Union Members	8.58 ***
Democrats—LGBT	9.97 **

+ $p < .1$, *$p < .05$, **$p < .01$, ***$p < .001$

OA 2.8 Alternative Specifications for Respondent Error

In the paper, the main dependent measure is *perceptual bias*,by which we mean average signed (or directional) error in the respondents' reports across items. We can also assess the effect of the treatments on absolute error, average absolute error in respondentsâĂŹ reports across items, and percentage bias, average percentage by which respondents err (signed error) across items. As Table OA 2.8 shows, results are consistent across these different specifications of respondent perceptual error.

Table OA 2.8: Results are consistent across different specifications of the dependent measure

	Timing Experiment			Wording Experiment		
	Perceptual bias	Percentage bias	Absolute error	Perceptual bias	Percentage bias	Absolute error
	(1)	(2)	(3)	(4)	(5)	(6)
Timing: slow	-4.34***	-48.1**	-3.58***			
	(1.43)	(20.2)	(1.24)			
Wording: percentage				-1.25	-23.7	-1.10
				(1.26)	(18.0)	(1.05)
Constant	19.7	235.0	22.0	18.9	264.6	22.2
	(1.12)	(15.5)	(1.24)	(1.03)	(13.8)	(0.86)
R^2	0.08	0.47	0.07	0.08	0.49	0.05
n perceptions	1696	1696	1696	3119	3119	3119
n respondents	284	284	284	312	312	312