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Ideology and Perceptions of Inequality

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Abstract

Rising social inequality across economic, gender, and racial lines is a pressing issue of our time. Despite widespread agreement that inequality *exists*, there are stark ideological disagreements about its extent, its victims, and about what – if anything – should be done to address it. Prior work demonstrates that the political left – who possess an ideological motivation to attenuate social inequality – perceive *more* inequality than the political right – who tend to be more tolerant of group-based disparities. Across three chapters, the present dissertation examines potential drivers of partisan differences in perceptions of social inequality. In Chapter 1, I introduce a theoretical model linking ideological motivations – specifically, the motivation to uphold vs. attenuate existing social inequalities – to perceptions of inequality through three key mechanisms: differential access to inequality, differential interpretations of inequality, and differential attention to inequality. In Chapter 2, across five studies, I provide an empirical test of the mechanism of differential attention to examine whether ideological motives shape people's proclivity to naturalistically notice – and accurately detect – inequality. Moreover, in Chapter 2 I consider whether the link between ideology and attention to inequality depends on whether traditionally disadvantaged or advantaged groups bear the brunt of inequality. In Chapter 3, across two studies, I empirically test two intervention strategies intended to nudge attention to – and recalibrate inaccurate perceptions of – instances of social inequality which affect societally disadvantaged groups. Efforts to address social inequality will remain stifled without an agreed-upon – and accurate – understanding of the extent of the problem. In clarifying how people's ideological motivations shape their perceptions of – and their proclivity to attend to – social inequality, the present dissertation aims to provide a starting point from which we can work to bring partisan perceptions into greater alignment

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Chapter 1

Despite glaring disparities across economic (Alvaredo, Chancel, Piketty, Saez, & Zucman, 2018; Solt, 2016), gender (United Nations, 2018), and racial (Inequality.org, 2021; Kovera, 2019) lines, there are stark ideological differences in people's perceptions of the extent of inequality in the world around them. For example, while the majority of those on the political left (72%) believe poor people in the United States 'have difficult lives,' the same percentage of those on the political right (74%) believe poor people 'have it easy' (Horowitz, Igielnik, & Kochhar, 2020). Perceptions of gender and racial inequality follow similar ideological patterns (Horowitz, Brown, & Cox, 2019; Horowitz, Parker, & Stepler, 2017). In general, the wider political left perceives *more* inequality than those on the political right (Chambers, Swan, & Heesacker, 2014; Chambers, Swan, & Heesacker, 2015; Davidai & Gilovich, 2015; Schmalor & Heine, 2022; Kteily, Sheehy-Skeffington, & Ho, 2017).

What drives partisan differences in perceptions of the extent of social inequality? Why does the political left perceive *more* inequality than the political right? The present work introduces a theoretical model linking ideological motivations to perceptions of inequality through three processes: access, interpretation, and attention. First, perceptions of inequality might differ across ideological lines because political partisans encounter different levels of *objective* inequality as they go about their everyday lives. Even when exposed to equivalent levels of objective inequality, however, political partisans might differ in their perceptions of its extent because they make differential *interpretations* about the meaning of inequality and because they differentially *attend* to evidence of inequality. In other words, ideology might moderate the relationship between access to inequality and subjective perceptions of inequality through two mechanisms: (1) interpretation and (2) attention. The current work joins recent work

in its conceptualization of inequality perceptions as an iterative, multifaceted process (Phillips, et al., 2020; Willis, Garcia-Sanchez, Sanchez-Rodriguez, Garcia-Castro, & Rodriguez-Bailon, 2022), but is unique in its specific focus on how ideological motivations – and in particular, people's beliefs about the desirability of social inequality – affect perceptions of inequality.

Ideological Motivations

People are motivated information processors, construing the world in ways that align with their personal goals or those goals of the collectives to which they belong (Balcetis & Dunning, 2006; Dunning & Balcetis, 2013). For example, people's desire to maintain a positive view of the self leads them to perceive self-descriptive attributes as broadly socially desirable while deeming traits that are *not* self-descriptive as generally undesirable (Dunning, Perie, & Story, 1991). Similarly, people's motivation to maintain a positive view of the groups to which they belong prompts them to make charitable attributions for their own group's aggressive behavior, crediting it to 'ingroup love', while attributing the same aggressive outgroup behavior to 'outgroup hate' (Waytz, Young, & Ginges, 2014).

Beyond general motivations to view the self and the groups to which one belongs positively, people are deeply motivated to defend and uphold their ideological – or worldview – beliefs. These ideological belief systems allow people to make sense of the world in which they live and at their core, reflect an orientation either *for* or *against* the existing social system (Elster, 1982; Jost, Nosek, & Gosling, 2008). Put differently, people have a general motivation to either maintain or dismantle the hierarchical social status quo. The current work focuses on two ideologies that capture the extent to which people are specifically motivated to maintain (vs. attenuate) societal inequalities: (1) social dominance orientation (Ho, et al., 2012; Ho, et al.,

2015; Sidanius & Pratto, 1999) and (2) political conservatism (Jost, Glaser, Kruglanski, & Sulloway, 2003; Jost & Amodio, 2012).

Social dominance orientation (SDO) measures an individual's preference for hierarchical arrangements between social groups with items like 'An ideal society requires some groups to be on top and others to be on the bottom' and 'We should not push for group equality' (Ho, et al., 2012; Ho, et al., 2015; Sidanius & Pratto, 1999). Those who score higher on measures of SDO (e.g., anti-egalitarians) have an ideological motivation to maintain present social inequalities and, in service of this broader goal, endorse a variety of beliefs and policies that uphold the social status quo, for example, just-world beliefs, nationalism, meritocracy, and an overall opposition to redistributive social policies (Sidanius & Pratto, 1999; Cotterill, Sidanius, Bhardwaj, & Kumar, 2014). In contrast, individuals who score lower on SDO (e.g., social egalitarians) are ideologically committed to the goal of attenuating group-based hierarchies and, in line with this broader motivation of lessening inequality, endorse beliefs and policies which push towards social change (e.g., affirmative action, redistribution, government safety nets).

The ideological commitment to maintaining existing hierarchical arrangements and resisting equality between groups is a core feature of political conservatism alongside a general preference for traditionalism (Jost, Glaser, Kruglanski, & Sulloway, 2003; Jost & Amodio, 2012). Relative to those on the political left (i.e., political liberals), the political right (i.e., political conservatives) are more tolerant of group-based inequalities and, like anti-egalitarians, are ideologically committed to justifying and maintaining inequality. In contrast, the political left, like social egalitarians, are more supportive of egalitarian social change.

While political conservatism and anti-egalitarianism are not identical, both ideologies capture people's general preference for social inequality. As such, in the present work, 'the wider

political right' refers to individuals with an ideological motivation to maintain existing group-based inequality (and therefore encompasses anti-egalitarians and political conservatives), whereas 'the wider political left' refers to individuals with an ideological motivation to attenuate existing inequalities (e.g., social egalitarians, political liberals). In sum, while the wider political left is motivated to minimize, problematize, and attenuate existing inequalities, the political right is motivated to ignore, justify, and rationalize existing inequalities. Prior work demonstrates that the wider political left perceives *more* inequality than the political right. In the sections that follow, I argue that these ideological differences in perceptions of inequality may stem from three mechanisms: differential access, differential interpretation, and differential attention.

Differential Access to Inequality

First, perceptions of inequality might differ along ideological lines because the political left (vs. right) is simply exposed to more objective inequality. In other words, the political left might perceive more inequality than the political right simply because they have greater access to inequality in their everyday lives.[1] In the United States, the political left is more likely to reside in large urban areas whereas the political right tends to live in rural and smaller metropolitan areas (Morrill, Knopp, & Brown, 2007; Pew Research Center, 2020). Large cities, in the US and around the world, are more unequal than small cities and rural areas (Social, Economic, and Housing Statistics Division, 2016; United Nations, 2020). To the extent that those on the political left are more likely to live in areas with starker *actual* inequalities, the political left (vs. right) may perceive more inequality simply because they are exposed to more evidence of it.

[1] I make no directional causal claim here. It may be that ideology *causes* people to select into environments with different levels of objective inequality or that objective inequality has *causal* effects on ideology (or both). I simply mean to emphasize that ideology might influence (and be influenced by) access to inequality, such that the amount of inequality in one's environment may meaningfully relate to one's ideological motivations.

Relative to the wider political right, the political left may also encounter more inequality and heterogeneity in their social environment (i.e., the people they encounter may be less economically and racially homogeneous). Both political liberals and conservatives tend to live in communities with overall ideologies similar to their own (McPherson, Smith-Lovin, & Cook, 2001; Motyl, Iyer, Oishi, Trawalter, & Nosek, 2014; Brown & Enos, 2021). While 40% of political liberals in the United States are racial minorities, only 20% of political conservatives are (Pew Research Center, 2020). Moreover, in the United States, political liberals (vs. conservatives) tend to live in more racially and ethnically diverse counties (Parker, et al., 2018; Pew Research Center, 2020), thus it's possible that the wider political left (vs. right) interacts with a more heterogeneous set of individuals. *Who* people interact with (i.e., people's social network composition) can meaningfully affect their perceptions of inequality. In general, people's inferences about – and perceptions of – the world around them are constrained by the cues they encounter in their environment (Dawtry, Sutton, & Sibley, 2015; Kraus, Rucker, & Richeson, 2017; Mo & Conn, 2018). For example, higher (vs. lower) income individuals perceive overall population wealth to be higher because they sample on and extrapolate general wealth from their own (relatively wealthy) social networks (Dawtry, Sutton, & Sibley, 2015).

In contrast, individuals with more racial diversity in their social networks have more accurate perceptions of the extent of inequality between groups (Kraus, Rucker, & Richeson, 2017). Moreover, quasi-experimental evidence suggests that economically advantaged Americans who participate in extended intergroup contact with low-income Americans in a Teach for America service context are more likely than non-participants to perceive the current economic system as unfair (Mo & Conn, 2018). Thus, to the extent that *who* we encounter constrains our perceptions of inequality, political liberals (who may have relatively more

heterogeneous social networks) will likely have different perceptions of inequality relative to political conservatives (whose social networks may be more homogeneous). Thus, in addition to differences in inequality perceptions stemming from the political left (vs. right) having more inequality-relevant cues in their physical environment, ideological differences in inequality perceptions may also stem from the political left (vs. right) having more inequality-relevant cues (e.g., racial and economic heterogeneity) in their social environment.

Finally, those on the political left (vs. right) might perceive more inequality because they encounter more information about inequality on a daily basis. As one example, the political left (vs. right) might consume media that more readily covers inequality-relevant issues. At present, most news outlets in the United States are consumed by *either* the political left *or* the political right, but not by both groups (Grieco, 2020). Recent work finds that consuming left-leaning news sources entrenches liberal political positions whereas consuming right-leaning news sources further cements conservative political positions (Earle & Hodson, 2022). To the extent that certain news outlets are more likely than others to report on issues of inequality, this could affect partisan access to inequality-relevant information.

Ideological motivations may also affect information environments by influencing not only *whether* people pursue higher education but also *what* they choose to study. In the United States, there are ideological differences in higher education; as of 2019, of all registered voters with a college degree, 53% identified as (or leaned towards) Democrats whereas 40% identified as (or leaned towards) Republicans (Pew Research Center, 2020). Beyond ideological differences in overall rates of university education, there are ideological differences *within* universities in what students choose to study. For example, the political left is more readily associated with 'hierarchy-attenuating' college majors (e.g., social work, public health, ethnic

studies, and sociology) and careers aimed at helping societally disadvantaged groups such as poor people, racial and ethnic minorities, and women (Sidanius, van Laar, Levin, & Sinclair, 2003). In contrast, the political right is more readily associated with 'hierarchy-enhancing' college majors (e.g., management, accounting) and, ultimately, hierarchy-enhancing careers (e.g., financial managers, law enforcement) after graduation (Sidanius, van Laar, Levin, & Sinclair, 2003). To the extent that higher education in general – and certain majors in particular – differentially expose people to information about inequality (Sinclair, Sidanius, & Levin, 1998), ideological differences in inequality perceptions may relate to these educational differences.[2]

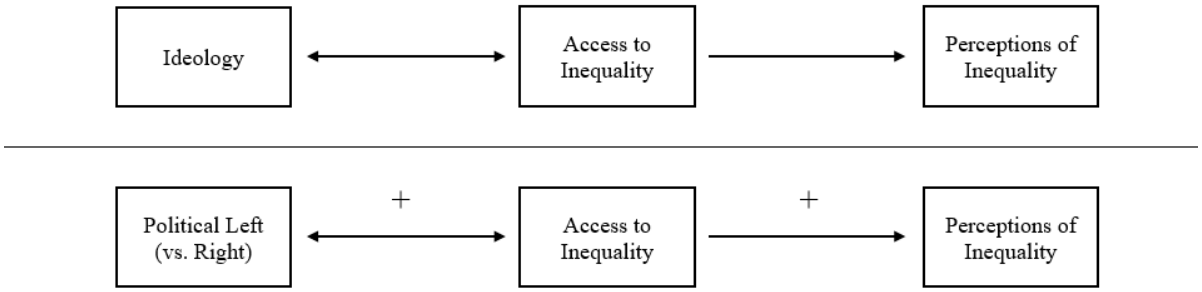
Taken together, the political left might perceive more inequality than the political right simply because they have greater *objective* access to inequality-relevant cues in their everyday lives (whether in their physical, social, or informational environments). There is correlational evidence to suggest that the more access to inequality one has, the more inequality one perceives (Johnston & Newman, 2016; Minkoff & Lyons, 2019; Newman, 2014; Xu & Garand, 2010). For example, people who reside in areas with greater income disparities (vs. those who reside in more financially homogeneous areas) perceive larger gaps between the rich and the poor (Minkoff & Lyons, 2019; Xu & Garand, 2010) and are more likely to support government redistribution (Johnston & Newman, 2016). Those with (vs. without) economically distressed friends in their social networks perceive greater class-based inequality (Newman, 2014). Experimental evidence suggests a causal link between access to inequality and heightened perceptions of it: relative to those in a control condition, participants who read information about

[2] Note that I again make no directional causal claim about the relationship between ideology and access to information about inequality. It could be that people's ideological motivations *cause* them to select into (or opt out of) higher education. It could also be that higher education *causes* changes to ideological motives.

economic inequality were subsequently more supportive of government redistribution – ostensibly because they perceived greater levels of societal inequality (McCall, Burk, Laperriere, & Richeson, 2017). Thus, to the extent that increases in *objective* inequality cause (or simply correspond with) increases in perceptions of the extent of inequality, those on the political left (vs. right) might perceive more inequality simply because they are exposed to more of it as they navigate their world.

The link between objective inequality and subjective perceptions of inequality, however, is tenuous. While there is evidence to suggest that the more inequality in one's environment, the more inequality one perceives, there is also evidence to suggest the opposite. For example, one recent paper found that increased access to inequality in one's physical environment predicted *decreased* perceptions of inequality six years later (Du & King, 2022). Other work suggests no relationship whatsoever between objective inequality and perceptions of inequality (Breznau & Hommerich, 2019; Gimpelson & Treisman, 2018; Kenworthy & McCall, 2008; Niehues, 2014). And while there is experimental evidence to suggest a positive relationship between access to and perceptions of inequality (McCall, Burk, Laperriere, & Richeson, 2017), other work which provides inequality-relevant information and measures subsequent perceptions finds null or even negative effects (Hoy & Mager, 2018). Despite these mixed effects, one explanation for ideological differences in perceptions of the extent of inequality is that political partisans are simply exposed to different realities; the political left perceives more inequality than the political right because they are exposed to more evidence of inequality in their physical, social, and information environments (Figure 1).

Figure 1 *Hypothesized Relationship Between Ideology, Access to Inequality, and Perceptions of Inequality*



Does the Link Between Access to and Perceptions of Inequality *Depend* on Ideology?

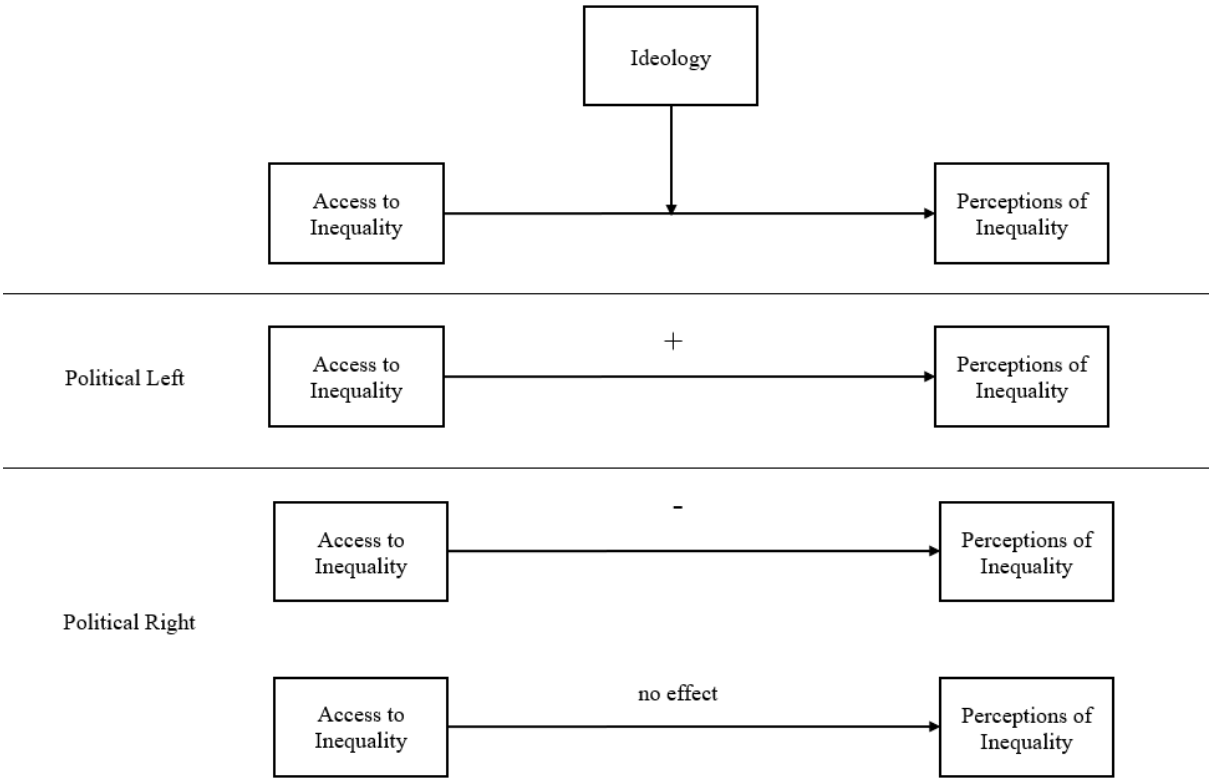
Alternatively, it may be that even when exposed to identical levels of objective inequality, political partisans continue to differ in their perceptions of it. In other words, ideological motives might moderate the link between access to inequality and perceptions of inequality. While work to date has not looked explicitly at whether ideological motives moderate this link, prior work does suggest that people's income level is a moderator of this relationship (Sands, 2017; Sands & de Kadt, 2020; Xu & Garand, 2010; Newman, Shah, & Lauterbach, 2018; Newman, Johnston, & Lown, 2015). For example, experimental work which manipulates the presence (vs. absence) of a luxury car in an impoverished area finds that individuals from low socioeconomic (SES) backgrounds – but not those from high SES backgrounds – are *more* likely to support government redistribution in the presence of inequality cues (Sands & de Kadt, 2020). Correlational evidence provides further support for the notion that income moderates the relationship between objective inequality and subjective perceptions of inequality; those with lower (vs. higher) incomes perceive *more* income inequality when they live in areas with greater income disparities (Newman, Shah, & Lauterbach, 2018; Xu & Garand, 2010).

Some work argues that this moderation effect is driven by the disparate influence that objective inequality has on low (vs. high) income individuals' beliefs about the fairness of the broader economic system (Newman, Johnston, & Lown, 2015). Specifically, for lower income individuals, greater exposure to inequality makes salient their disadvantaged social position and prompts them to reject the notion that society is fair and meritocratic (Newman, Johnston, & Lown, 2015). In contrast, increases in objective inequality have the opposite effect on higher income individuals, boosting their belief that the economic system is fair as a means of justifying and defending their relatively advantaged position in the current social hierarchy (Newman, Johnston, & Lown, 2015). In sum, prior work suggests that the effect of objective inequality on perceptions of inequality might *depend* on the extent to which people justify and rationalize existing social inequalities.

Because ideological motivations (like advantaged social positions) *also* affect whether people rationalize and justify existing inequalities, ideology may be an additional moderator of the link between access to inequality and perceptions of inequality. In the sections that follow, I argue that ideology moderates this link through two mechanisms: (1) interpretations of or attributions about inequality and (2) attention to – and the motivational relevance of – inequality. In particular, I'll suggest that increases in objective inequality will increase perceptions of the extent of inequality among those on the political left. Because the wider political left possesses an ideological commitment to attenuating social inequality, I'll suggest that greater access to inequality prompts greater attunement to its existence and leads them to interpret evidence of inequality in ways that perceptually highlight it. In contrast, I'll argue that heightened exposure to inequality will not necessarily increase perceptions of the extent of inequality among those on the political right as the ideological goal of justifying existing inequalities leads them to

perceptually process and interpret inequality in ways that minimize or downplay its existence (Figure 2).

Figure 2 *Ideology as a Moderator of the Relationship Between Access to and Perceptions of Inequality*



Differential Interpretations of Inequality

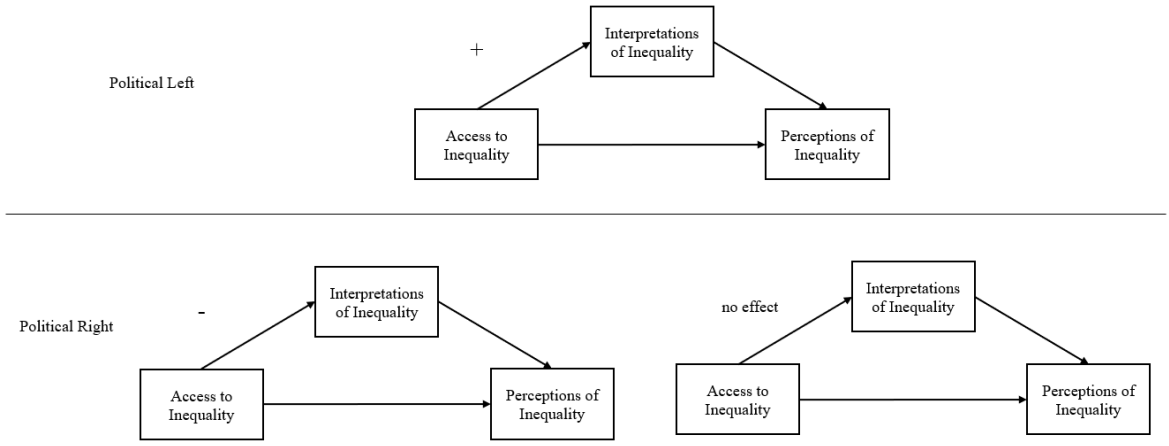
Differences in how people interpret – or make sense of – social inequality is one mechanism through which ideology might influence the link between access to inequality and perceptions of inequality. Thus, even when exposed to the same objective amount of inequality, the political left might perceive more inequality than the political right because they interpret evidence of inequality in ways that *problematize* or *highlight* it. In contrast, the political right might perceive less inequality than the political left because they interpret evidence of inequality in ways that *downplay* or *minimize* its existence.

Prior work suggests that ideological motivations are associated with meaningful differences in beliefs about the *causes* of inequality. While the political right tends to make individualistic attributions for poverty and believe that poor people themselves are to blame for their plight, the political left tends to attribute poverty to external, structural factors (Sniderman, Hagen, Tetlock, & Brady, 1986; Zucker & Weiner, 1993). The political right likewise tends to attribute wealth to internal, individualistic factors (e.g., hard work), whereas the political left interprets wealth as a product of external, societal (e.g., being born into a rich family) factors (Kluegel & Smith, 1986). Those who attribute poverty and wealth to internal (vs. external) factors are more likely to justify and maintain inequality and are less likely to support economic redistribution (Piff, et al., 2020). In other words, blaming poor people themselves for their disadvantaged social position (as the political right is inclined to do) minimizes the problem of inequality; if poor people are lazy and undeserving and these individualistic factors *cause* inequality, it's easier to rationalize the existence of inequality. In contrast, blaming inequality on a broader social system (as the political left tends to do) problematizes it; if I believe poverty is caused by external societal structures (e.g., unfair economic and social systems), I'm more likely to work against and dismantle those unfair structures that give rise to it.

In addition to perceptually downplaying inequality by attributing its causes to internal (vs. external) characteristics, prior work suggests that those on the political right (vs. left) may also more generally interpret evidence of inequality in ways that minimize (vs. highlight) the extent of it (Eibach & Keegan, 2006; Kahn, Ho, Sidanius, & Pratto, 2009; Kteily, Sheehy-Skeffington, & Ho, 2017). For example, relative to social egalitarians, anti-egalitarians (1) interpret status differences between groups as smaller (Kahn, Ho, Sidanius, & Pratto, 2009), (2) interpret gaps in equality between disadvantaged and advantaged groups as smaller (Kteily,

Sheehy-Skeffington, & Ho, 2017), and (3) interpret pushes towards group equality as more substantial (Eibach & Keegan, 2006). This perceptual minimization of inequality allows anti-egalitarians to maintain the belief that efforts to attenuate social inequality are not necessary; if groups in society are already relatively equal (and status differences between them are already small), there's no need for egalitarian social policies (Kteily, Sheehy-Skeffington, & Ho, 2017). In contrast, for social egalitarians, interpreting status differences between groups as larger and efforts towards group-based equality as smaller means that further action is needed to remedy inequality. Thus, people's overarching ideological goal to either uphold or attenuate existing social inequalities might prompt them to differentially interpret evidence of inequality in ways that minimize or highlight its extent (Figure 3).

Figure 3 *Differential Interpretations as a Mediator of the Relationship Between Access to and Perceptions of Inequality*



Differential Attention to Inequality

While the political left (vs. right) might perceive more inequality due to ideological differences in interpretations, differential perceptions of inequality may also stem from a

differential tendency to notice inequality in the first place.[3] It's possible that, in addition to prompting differential interpretations of inequality, ideological motives moderate the link between access to inequality and perceptions of inequality by influencing what is *motivationally relevant* and therefore noticed. Prior work demonstrates that attention is a critical precursor to perception (Lassiter & Irvine, 1986; Lassiter, Geers, Handley, Weiland, & Munhall, 2002; Ware, Lassiter, Patterson, & Ransom, 2008). People do not perceive seemingly obvious and repeated changes to scenes if their attention is not focused on the parts of the scene undergoing the change (Rensink, O'Regan, & Clark, 1997). Because people are unable to focus their attention on everything all at once, they selectively attend to the features of their environment that are *motivationally relevant* or those features that best facilitate goal achievement (Brosch & Van Bavel, 2012; Isaacowitz, 2006; Lang, Bradley, & Cuthbert, 1997). In other words, people can be *perceptually ready* to attend to certain features of their environment over others (Bargh & Pratto, 1986; Bruner, 1957; Riccio, Cole, & Balcetis, 2013). Because evidence of inequality is more goal-relevant to those with the ideological motivation to attenuate (vs. ignore or justify) social inequality, it's possible that the political left perceives more inequality than the political right simply because they are more likely to notice evidence of it.

Outside of social contexts, people's goals affect what they notice. For example, hungry individuals, relative to those low in hunger, show a greater attentional bias towards food-related (vs. control) stimuli (Mogg, Bradley, Hyare, & Lee, 1998). Relative to nonsmokers, smokers are

[3] In the proposed model, noticing inequality is not a necessary precondition to interpreting inequality. Noticing a specific instance of inequality in the moment may realistically be a precondition to interpreting – or making sense of – that specific instance of inequality, but interpretations of inequality can also occur in the abstract. Put differently, an individual may make attributions about poverty in the abstract without necessarily picking up on specific poverty-relevant cues in their immediate environment.

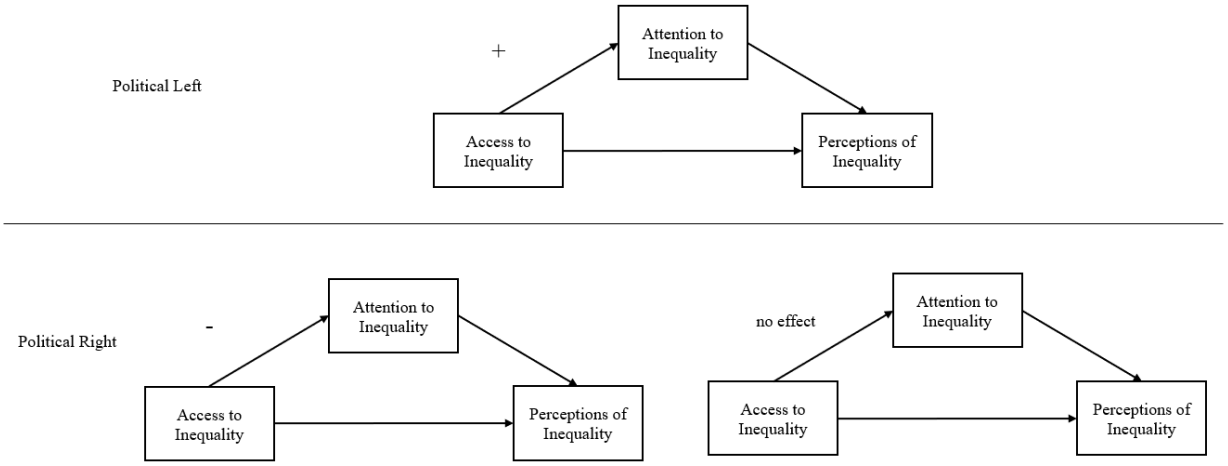
more likely to attend to smoking-related (vs. control) cues (Ehrman, et al., 2002). Relative to non-drinkers, individuals who drink alcohol – and particularly those with a high level of alcohol craving – show a greater attentional bias for alcohol-related cues (Field, Mogg, & Bradley, 2005). Thus, ostensibly in service of the goal of acquiring the desired objects (here, food, cigarettes, and alcohol), people exhibit selective attention towards goal-relevant cues.

Within the social domain, people's underlying motivations also shape their attention. Those who have (vs. have not) experienced prior social exclusion pay greater attention to cues of potential social acceptance (DeWall, Maner, & Rouby, 2009). More recently, eye-tracking studies find that lonely individuals (vs. those who are not lonely) show an automatic attentional preference for warm, friendly-looking faces, ostensibly in service of the goal of establishing social connection to minimize feelings of loneliness (Saito, Motoki, Nouchi, Kawashima, & Sugiura, 2020). Relative to those from high socioeconomic (SES) backgrounds, individuals from low SES backgrounds exhibit greater selective attention towards other people, consistent with the broader goal of interdependence (Dietze & Knowles, 2016). Whereas those from high SES backgrounds tend to understand themselves as independent from others, individuals from low SES backgrounds view themselves as interconnected with others (Stephens, Markus, & Townsend, 2007). The relative emphasis on interdependence (vs. independence) affects the extent to which other people are motivationally relevant and, in service of the broader goal of interdependence, individuals from low SES backgrounds exhibit greater attention to other people relative to those from high SES backgrounds (Dietze & Knowles, 2016).

Applied to the context of inequality, it's likely that people's ideological motivations – to either uphold or attenuate the existing system – shape the extent to which inequality is motivationally relevant. Specifically, it's possible that the wider political left, who seek to

remedy existing inequalities, will be more likely to notice inequality in the world around them relative to the political right. Or said differently, the political right, who have an ideological motivation to rationalize and justify societal inequality, may be more likely to ignore evidence of inequality in the world around them relative to the political left (Figure 4).

Figure 4 *Differential Attention as a Mediator of the Relationship Between Access to Inequality and Perceptions of Inequality*



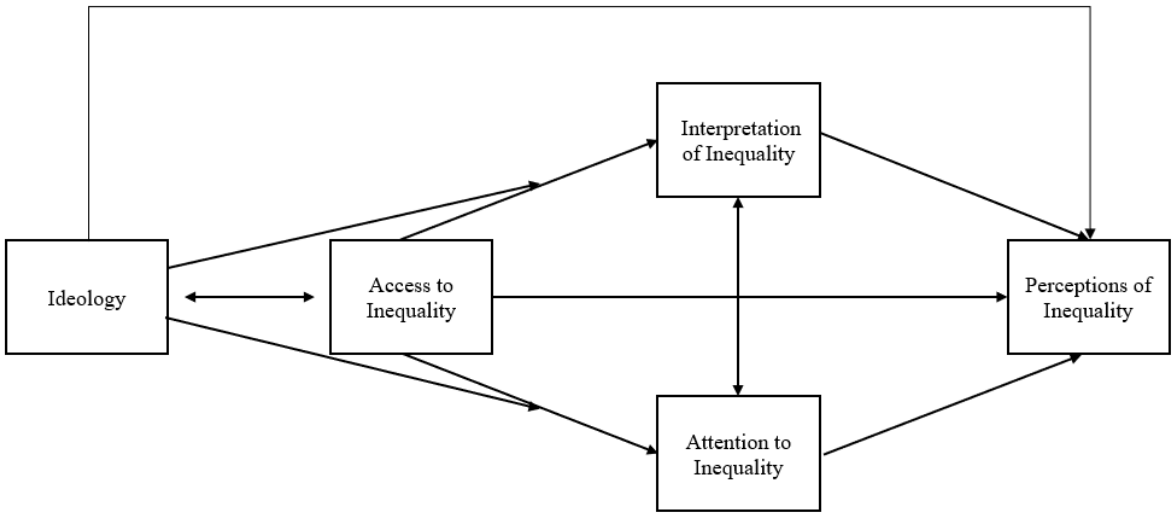
Prior work suggests that those who view the current economic system as unfair and illegitimate (vs. fair and legitimate) demonstrate heightened emotional and physiological reactions to evidence of inequality, which suggests that inequality might be more motivationally relevant to the political left (who also tend to view the present economic system as unfair) relative to the political right (Goudarzi, Pliskin, Jost, & Knowles, 2020). Moreover, prior work shows that individuals prompted to write an essay about failing to be egalitarian to Black men offset this failure by selectively attending to faces of Black men in a later task (Moskowitz, Li, Ignarri, & Stone, 2011). In other words, individuals who are temporarily primed with the goal of egalitarianism are subsequently more attentive to inequality-relevant cues (Moskowitz, 2002;

Moskowitz, Li, Ignarri, & Stone, 2011). To the extent that those on the political left (vs. right) have a chronic goal of egalitarianism, they may be more attentive to evidence of inequality. Thus, ideological motives might influence the link between objective inequality and perceptions of inequality by affecting whether inequality is motivationally relevant and therefore, noticed.

Model Walkthrough and Hypotheses

To clarify the full model (Figure 5) and predictions regarding the link between ideology and perceptions of inequality, imagine two individuals: Leslie and Ryan. Imagine that Leslie is affiliated with the political left and has an ideological commitment to attenuating social inequality. Imagine that Ryan, in contrast, is affiliated with the political right and has an ideological motivation to maintain the existing hierarchical social system. Consistent with prior work, imagine that Leslie perceives more inequality than Ryan does. Why?

Figure 5 *Full Model Linking Ideological Motives to Perceptions of Inequality*



Differential Access. It could be that Leslie and Ryan simply live in areas with different levels of objective inequality. Perhaps Leslie perceives more inequality than Ryan does because

she is exposed to more of it. Let's imagine, however, that Leslie and Ryan live in the exact same area and are therefore exposed to the same amount of objective inequality, yet Leslie continues to perceive more inequality than Ryan. Why might this be?

Differential Interpretations. It could also be that for Leslie, greater access to inequality prompts an increased likelihood that she will make structural attributions for inequality and interpret inequality in ways that perceptually highlight its existence, thus prompting her to perceive more inequality than Ryan does. In other words, for Leslie, access to objective inequality may be positively related to perceptions of inequality via the mechanism of interpretation. For Ryan, in contrast, perhaps increased access to inequality has no effect on his interpretations – regardless of the extent of inequality, he will attribute it to internal causes and will not interpret it as an issue worthy of intervention, and thus there will be no corresponding effect on his perceptions. It could also be that for Ryan – as a function of his ideological motivation to maintain the existing hierarchy – the presence of inequality actively decreases his likelihood of interpreting inequality as an issue worthy of intervention, which decreases his perceptions of inequality. Said differently, for Ryan greater access to inequality may have no effect on his perceptions of inequality *or* may be negatively related to perceptions of inequality via the mechanism of interpretation.

Differential Attention. Distinct from (but related to) Ryan and Leslie's interpretations of inequality, Ryan and Leslie may differentially notice inequality. In other words, even without differentially making sense of inequality, Ryan and Leslie may be differentially attentive to evidence of inequality in the first place. For Leslie, it could be that greater access to inequality prompts an increased likelihood that she will pay attention to evidence of it, thus prompting her to perceive more inequality than Ryan does. In other words, for Leslie, access to objective

inequality may be positively related to perceptions of inequality via the mechanism of attention. For Ryan, though, perhaps increased access to inequality has no effect on his proclivity to notice inequality – regardless of the extent of inequality, inequality is not motivationally relevant to Ryan and will not be noticed, thus there will be no corresponding effect on his perceptions of inequality. It could also be that for Ryan, increased access to inequality actively decreases the likelihood of noticing inequality, which decreases his perceptions of inequality. Said differently, for Ryan greater access to inequality may have no effect on his perceptions of inequality *or* may be negatively related to perceptions of inequality via the mechanism of attention.

Overview of the Present Dissertation

The present dissertation continues as follows. In Chapter 2, across five studies, I test the mechanism of differential attention to examine whether those on the political left (vs. right) are differentially likely to notice – and accurately detect – instances of social inequality. Because evidence of inequality is more motivationally relevant to those with the ideological commitment to lessening inequality, I suggest that the political left (vs. right) will be more likely to pay attention to inequality-relevant cues. Moreover, I consider whether the relationship between ideology and attention to inequality depends on whether societally disadvantaged or advantaged group members bear the brunt. In Chapter 3, I examine two intervention strategies to nudge attention to – and recalibrate inaccurate perceptions of – social inequality among samples of participants affiliated with the political right. In particular, I examine whether providing information about rising inequality affects subsequent perceptions of inequality. I then test whether reframing inequality in terms of moral values endorsed by the political right increases the motivational relevance of inequality, thereby increasing attention to and accurate detection of social inequality.

Chapter 2

Inequality between groups in society is evident, yet there are stark ideological disagreements about the nature of inequality. The political left (who value group-based equality) tends to perceive *greater* levels of inequality in the world around them relative to the political right (who are more tolerant of disparities between groups). How might those on the political left and right come to such different conclusions about the nature of inequality? Chapter 2 explores this question by suggesting that, beyond shaping how people evaluate and interpret evidence of inequality, people's ideological beliefs about the desirability of group-based inequality shape their proclivity to notice – and accurately detect – inequality in the first place.

As detailed in Chapter 1, ideological motives affect how people process inequality-relevant information. In particular, people's beliefs about the desirability of group-based inequality, as captured by social dominance orientation (Ho, et al., 2012; Ho, et al., 2015; Sidanius & Pratto, 1999) affect how they make sense of inequality in the world around them. While those lower in SDO (e.g., social egalitarians) believe groups in society should be equal, those higher in SDO (e.g., anti-egalitarians) are more tolerant of hierarchy between groups. This difference in preferences for group-based equality is a core feature of political conservatism (alongside preferences for traditionalism) with the political right demonstrating a heightened tolerance for inequality relative to the political left (Jost, Glaser, Kruglanski, & Sulloway, 2003).

Prior work demonstrates that while those with the ideological motivation to attenuate social inequality (i.e., egalitarians) tend to perceptually highlight evidence of it, those with the ideological goal of maintaining existing hierarchy (i.e., anti-egalitarians) tend to perceptually downplay its existence. For example, relative to social egalitarians, anti-egalitarians perceive smaller status differences between groups in society (Kahn, Ho, Sidanius, & Pratto, 2009),

perceive that there has been *more* progress towards intergroup equality (Eibach & Keegan, 2006), and perceive *less* inequality in the world around them (Chambers, Swan, & Heesacker, 2014). Even when exposed to identical stimuli depicting a hierarchical organization, egalitarians perceive the same organization as being steeper and more unequal relative to anti-egalitarians (Kteily, Sheehy-Skeffington, & Ho, 2017). When later asked to recall which hierarchical organization they originally saw, anti-egalitarians (vs. egalitarians) are more likely to *underestimate* the extent of inequality they previously encountered (Kteily, Sheehy-Skeffington, & Ho, 2017). Each of these motivated perceptions allow anti-egalitarians to maintain their opposition to efforts to attenuate inequality; if status differences between groups are small, if much progress has been made towards alleviating inequality, and if there is not much inequality in the world around us to begin with, there is no need for action to remedy inequality.

While prior work suggests that people differentially evaluate, interpret, and distort evidence of inequality in ways that align with their underlying ideological motivations, in the present work, we suggest that ideological motives may also shape naturalistic attention to inequality. In other words, people's underlying ideologies might prompt them to notice (or not notice) evidence of inequality in the world around them, leading partisans to experience different realities even when exposed to the same environments.

Ideology and Naturalistic Attention

Attention is a tool that people adapt to their current needs (Treisman, 2006). Because people cannot attend to all features of their environment, priority is given to those aspects that are *motivationally relevant*, or those features that best facilitate goal achievement (Brosch & Van Bavel, 2012; Summerfield & Egner, 2009). In terms of attention to inequality, we suggest that relative to individuals who are more tolerant of disparities between social groups, for social

egalitarians (who possess the ideological goal of *reducing* the gap between socially disadvantaged and advantaged groups) evidence of inequality is more motivationally relevant and thus, they are more naturally vigilant for it. In other words, those with the ideological motivation to increase social equality may be more likely to encode the world around them in inequality-relevant terms. Consider, for example, two individuals sitting in a classroom where the professor disproportionately calls on the men (vs. the women) in the room. Someone who is ideologically committed to reducing gaps between groups in society (for example, the gap between men and women) may, naturalistically, be more likely to encode the unbalanced share of men versus women being called on. In contrast, an individual who is more tolerant of social inequality might not think to encode who the professor calls on through this lens of gender. These two individuals, as a result of their ideological motivations, might arrive at different conclusions about the existence and extent of gender-based inequality in the classroom. Importantly, these different conclusions are not a result of differential interpretations or rationalizations, but rather stem from ideological differences in the perceptual readiness to notice inequality in the first place.

This theorizing draws from prior work demonstrating how motivational relevance affects selective attention outside of the ideological domain. For example, hungry individuals, relative to those low in hunger, show a greater attentional bias towards food-related stimuli (Mogg, Bradley, Hyare, & Lee, 1998) and smokers, relative to non-smokers, show a greater attentional bias towards objects of addiction (Hogarth, Mogg, Bradley, Duka, & Dickinson, 2003). Selective attention occurs in social contexts as well. Lonely individuals, or those who are concerned about being excluded socially, pay greater attention to social cues (Gardner, Pickett, Jefferis, & Knowles, 2005; Pickett, Gardner, & Knowles, 2004) and, in particular, to signs of acceptance

(DeWall, Maner, & Rouby, 2009), in service of the broader goal of establishing or re-establishing social connection. Individuals from low socioeconomic (SES) backgrounds, who prioritize interdependence with others (Stephens, Markus, & Townsend, 2007), are more likely than high SES individuals (who are more independent) to selectively attend to other people in their environment (Dietze & Knowles, 2016).

Particularly relevant to the present research is work that has explored how experimentally primed goals of egalitarianism affect selective attention (Moskowitz, 2002; Moskowitz & Li, 2011; Moskowitz, Li, Ignarri, & Stone, 2011). In this work, participants are typically asked to write about a time they failed to act in an egalitarian way towards Black men (Moskowitz & Li, 2011). Afterwards, participants complete a seemingly unrelated task where they are asked to find a man wearing a bow tie among arrays of Black and White male faces. Participants initially primed with the goal of egalitarianism demonstrate preferential attention towards Black faces even when these faces should be ignored for optimal task performance. In other words, participants who are experimentally, temporarily primed with the goal of egalitarianism selectively attend to motivationally- and inequality-relevant aspects of the subsequent task.

Based on this theorizing, in the present work, we suggest that people's ideological beliefs about the desirability of group-based equality affect their proclivity to notice – and accurately detect – evidence of inequality in the world around them. Because inequality is more motivationally relevant to them, we theorized that individuals strongly committed to the ideological goal of social equality (egalitarians) would be more attentive to and more accurate at detecting evidence of inequality than individuals more tolerant of inequality (anti-egalitarians).

Overview of Chapter 2

Across five studies, we investigated whether chronic differences in ideological beliefs about the desirability of group-based equality shape individuals' attention to – and accurate detection of – inequality. Study 1 examined naturalistic attention to cues of inequality in scenes of city-life. Study 2 employed a Go-No Go task analyzed using signal detection framework to assess whether spontaneous attention to inequality manifests in greater accuracy at arbitrating between unequal and equal resource distributions. Study 3 used a change blindness paradigm to naturalistically index individuals' visual attention to inequality-relevant aspects of social scenes.

While socially disadvantaged groups (e.g., poor people, women, racial minorities) always bear the brunt of inequality in Studies 1-3, in Studies 4 and 5, we explicitly manipulated the social standing of inequality's victims, allowing us to empirically test two competing predictions regarding the link between egalitarianism and attention to inequality. To the extent that inequality itself is motivationally relevant for social egalitarians, they should attend to evidence of inequality regardless of whether typically disadvantaged or advantaged groups are affected.

Recent work suggests, however, that it is not inequality *per se* that is motivationally relevant to egalitarians, but rather, that egalitarians are primarily motivated to close the gap between groups in society, thereby treating targets differently as a function of target social group status. For example, relative to the political right, those on the political left preferentially amplify the success of members of disadvantaged (but not advantaged) groups (Kteily, Rocklage, McClanahan, & Ho, 2018), perceive the same act as causing more harm when committed against a disadvantaged (vs. advantaged) target (Lucas & Kteily, 2018), and endorse hierarchy-enhancing beliefs in contexts where those beliefs can be used to support diversity-related organizational efforts and help disadvantaged groups (Ponce de Leon & Kay, 2021). From this

perspective, it is specifically inequality that harms societally disadvantaged (but not advantaged) groups that is motivationally relevant for social egalitarians, and thus, any link between egalitarianism and heightened attention to inequality might apply selectively to instances in which the inequality harms groups at the bottom of society.

Study 1

The primary aim of Study 1 was to examine how an individual's (anti-)egalitarianism (assessed by their social dominance orientation [SDO]) predicts their spontaneous tendency to notice inequality in everyday scenes. We examined this across five samples of participants who viewed a series of images depicting scenes of city-life, half of which contained inequality-relevant cues. We anticipated that individuals higher (vs. lower) in SDO would be less likely to notice inequality-relevant cues. Across four samples (Samples 1a-d), we employ nearly identical methods to test this hypothesis (any differences are noted in the procedure section below). Sample 1e extends our findings with an experimental manipulation of task instructions.

Samples 1a-d

Sample 1a. Because we were using a newly developed paradigm, we did not have an objective metric to determine an appropriate sample size. We conducted this study with a sample of 227 participants using Amazon's Mechanical Turk (MTurk; M_{age} = 34.9, *SD* = 10.7; 41.5% Female, 58.5% Male; 148 White, 18 Asian; 19 Black; 9 Latino; 2 Native American; 4 Biracial), of whom 200 provided data on all focal variables (88.1% of full sample). A sensitivity analysis suggested that with this sample size, we had 80% power to detect a correlation of r = .20.

Sample 1b. Sample 1b consisted of 527 participants from MTurk (M_{age} = 35.8, *SD* = 10.8; 54.4% Female, 45.4% Male; 384 White, 32 Asian; 48 Black; 27 Latino; 2 Middle Eastern; 2

Native American; 9 Biracial; 1 Other; 2 n/a) of whom 507 provided data on focal variables (96.2% of full sample). At this sample size, we had 80% power to detect a correlation of $r = .12$.

Sample 1c. Sample 1c was collected using Prolific Academic and included 522 participants from the UK (50.9% Female, 49.1% Male; 481 White, 16 Asian, 8 Black, 2 Latino, 2 Middle Eastern, 8 Biracial, 2 Other) of whom 519 provided data on all focal variables (99.4% of full sample). With this sample size, we had 80% power to detect a correlation of $r = .12$.

Sample 1d. Sample 1d consisted of 738 participants from MTurk ($M_{age} = 35.7$, $SD = 10.9$; 49.3% Female, 50.7% Male; 449 White, 45 Asian; 55 Black; 35 Latino; 2 Middle Eastern; 4 Native American; 19 Biracial; 1 Other) of whom 607 provided data on all focal variables (82.2% of full sample). With this sample size, we had 80% power to detect a correlation of $r = .11$.

Procedure

Across samples 1a-d, participants were directed to a survey which asked them to complete a Visual Attention Task. Participants were shown a series of images and for each, were given the following instructions: "What stands out to you in this image? Please list three things that stand out to you." The task instructions were altered slightly for samples 1b and 1c. Participants in these samples saw the following instructions: "From the image above, please list the first 3 concrete details (e.g., objects, characters, clothing) that you notice."

We used a variety of focal and distractor images, sampling across a broad range of stimuli (see Appendix A Figures A1-A4 for all images) The focal images each depicted inequality-relevant scenes. Specifically, these images juxtaposed, in the same scene, certain cues reflecting high status (e.g., wealthy women receiving pedicures, luxury vehicle) and others reflecting low status (e.g., employees at a nail salon, a homeless person's cart). The distractor images contained no obvious inequality-relevant content. In samples 1a-c, participants saw 6

images (3 inequality, 3 distractor). Participants in sample 1d saw 10 images (5 inequality, 5 distractor). After, participants filled out measures of (anti-)egalitarianism and demographics.

Measures

(Anti-)Egalitarianism. Samples 1a, 1c, and 1d assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha s > .85$). Sample 1b used the 8-item $SDO_{7(s)}$ scale (Ho et al, 2015, $\alpha = .92$).

Coding Scheme. First, we examined whether participants' open-ended responses directly and explicitly remarked on the inequality in the image (*Direct Inequality*). Participant responses were coded as '1' for direct inequality if the response overtly mentioned status differences in the image or remarked directly on the fact that the scene depicted inequality (if not, the response was coded as '0'). For samples 1a-c, participants saw three images and reported three details they noticed for each, thus, direct inequality scores for these samples ranged from 0 to 9 (with a score of '0' reflecting that no reported details for any image explicitly mentioned inequality and a score of '9' reflecting that all three cues reported for all three images involved direct mentions of inequality). Participants in sample 1d viewed five inequality images and provided three details for each image, thus, direct inequality scores for sample 1d could range from '0' to '15'.

Next, we focused on indirect indices of attention to inequality. Each inequality-relevant image contained cues associated with high status (e.g., women receiving pedicures, luxury car) as well as cues associated with low status (e.g., nail salon employees, a homeless person). Beyond directly commenting on inequality, individuals might indirectly reveal themselves to have noticed inequality by mentioning both high *and* low status cues in the image (*Indirect Inequality*). If both types of cues were mentioned, we gave the response a score of '1' for indirect inequality; if participants mentioned *only* high or low status cues (or neither), we gave

the response a '0'. For the indirect inequality code, participants received a score for each image (rather than each cue), thus, for samples 1a-c, indirect inequality scores ranged from '0' to '3' and for sample 1d, indirect inequality scores ranged from '0' to '5'. One rater coded all responses, and a second rater coded a subset of responses (all κs > .70).

Sample 1e

Sample 1e consisted of 571 MTurk participants (M_{age} = 38.9, *SD* = 12.9; 56.5% Female, 43.5% Male; 274 White, 25 Asian; 15 Black; 16 Latino; 2 Native American; 11 Biracial; 2 Other; 23 n/a) of whom 368 provided data on all variables (64.4%, see Appendix A for attrition analyses). With this sample, we had 80% power to detect a correlation of r = .15.

Procedure

Data for Sample 1e was collected in two waves. In wave 1, participants filled out measures of anti-egalitarianism and demographics. In wave 2, participants completed the same Visual Attention Task as samples 1a-d with one slight alteration. Here we experimentally manipulated the task instructions participants received, with half of participants receiving *General Impression* task instructions ("What is your impression of the image? Please write at least 3 sentences") and the other half receiving *Concrete Details* task instructions ("Please list three features of the image that stand out to you). Participants in sample 1e viewed eight images (4 inequality, 4 distractor).

Measures

(Anti-)Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015; α = .95).

Coding Scheme. Participant responses were coded using the scheme described above.

Results and Discussion

Our primary interest was the relationship between an individual's (anti-)egalitarianism and their reported attention to inequality. We anticipated that social egalitarians would be more likely than anti-egalitarians to notice inequality and thus, predicted that SDO would be negatively correlated with both direct and indirect inequality. Below we report the results of a meta-analysis across all five samples (see Appendix A Table A1 for results by sample and Appendix A Figures A5-A12 for forest plots).

Direct Inequality. A meta-analysis of all six correlation coefficients (separating out by instruction condition for sample 1e) reveals a significant negative correlation between SDO and direct inequality (fixed effects model: $z = -2.89$, $p = .004$, $r = -.06$, random effects model: $z = -2.19$, $p = .03$, $r = -.07$), suggesting that those higher (vs. lower) on SDO were *less* likely to explicitly remark that the scene depicted inequality.

Indirect Inequality. A meta-analysis of all six correlation coefficients (again separating out by instruction condition for sample 1e) reveals a significant negative correlation between SDO and indirect inequality (fixed effects model: $z = -3.44$, $p < .001$, $r = -.07$, random effects model: $z = -2.93$, $p = .003$, $r = -.08$), suggesting that those higher (vs. lower) on SDO were also *less* likely to report picking up on the high- and low-status details in the scene.

Study 1 suggests that, whether reporting it as a salient issue or picking up on relevant details in a scene, those low (vs. high) in anti-egalitarianism were more likely to notice inequality in images of contemporary city-life – scenes similar to those they might encounter on a day-to-day basis. While Study 1 does provide initial evidence that social egalitarians are more attentive to evidence of inequality, it does not allow us to rule out the possibility that individuals more tolerant of hierarchy are just as likely to notice inequality but are simply less likely to

report noticing it. If, on the other hand, ideological motivations shape whether people are chronically cognitively attuned to inequality, then this should also be reflected in greater accuracy at detecting inequality in a rapid response task.

Study 2

The primary goal of Study 2 was to examine how (anti-)egalitarianism shapes accurate detection of inequality. We used a basic cognitive task which asked participants to judge whether two arrays of a socially relevant resource (here, moneybags) were equal or unequal to one another. We expected that those lower (vs. higher) on SDO would be more accurate at detecting the presence vs. absence of inequality. Study 2 was preregistered (see Appendix A for a link).

Participants

To detect an association of $r = .10$ with 80% power, the required sample size was roughly 780 participants. Because we had two versions of the task and as preregistered, we aimed to collect data from 1,600 participants (800 participants per task version). In total, we collected data from 1,591 participants using MTurk ($M_{age} = 40.1$, $SD = 12.7$; 61.9% Female, 37.9% Male, .2% Other; 1,120 White, 74 Asian, 110 Black, 62 Latino, 7 Native American, 28 Biracial, 1 Middle Eastern, 3 Other, 1 n/a) of whom 1,406 were used in focal analyses (88.4% of full sample).

Procedure

Task Structure. Participants completed 120 trials of a Go-No Go task that asked them to judge whether two distributions of a socially relevant resource were equal or unequal to one another. On any given trial, participants saw the same picture of a group of men and a group of women (separated by a divider), each with a set of moneybags associated with them. On 'equal' trials, the distribution of moneybags associated with men and the distribution of money bags associated with women were equal to one another. On 'unequal' trials, the group of men had

more moneybags than the group of women (in this experiment, inequality therefore was always at the expense of the socially disadvantaged group). On 'Go' trials, participants were asked to hit the space bar. On 'No Go' trials, participants were asked to refrain from hitting any key on the keyboard. Trials advanced after 6 seconds or (if sooner) when participants hit the space bar. We counterbalanced whether participants were instructed to hit the space bar ('Go' trials) when the two distributions of moneybags were equal or unequal. After the task, participants filled out measures of anti-egalitarianism and demographics.

Stimuli Development. We developed a large set of stimuli (120 total trials) that varied on three dimensions: (1) the overall number of moneybags, (2) the proportion by which the moneybags differed from equality, and (3) whether the two arrays of moneybags shared a structural 'base' (see Appendix A Figures A13-A18 for stimuli examples). The number of moneybags ranged from 5 to 45 items per array. The 'equal' image pairs depicted an equal number of moneybags in each array. For 'unequal' image pairs, we systematically varied the proportion of difference. For example, one 'class' of unequal pairs differed by an amount less than 20% and a second class differed by an amount between 20 and 30%. For example, an image pair that had 20 moneybags in one array and 25 in another would differ by 25% (thereby falling into the second 'class'). Finally, we varied whether the two images in each pair came from the 'same' or 'different' structural base, reflecting how the moneybags were spatially arranged. Image pairs with the 'same' base had an identical arrangement: for equal trials, the two arrays were identical copies of one another; for unequal trials, we created a larger array and then deleted some of the moneybags to create the smaller array. Image pairs with a 'different' structural base had different spatial arrangements of moneybags. For example, even when the arrays had an equal number of moneybags, they were spatially organized differently.

Measures

(Anti-)Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha = .95$).

Signal Detection Parameters. Our key dependent measures were (1) participant ability to differentiate accurately between equal and unequal trials and (2) participant bias towards responding in a particular direction (i.e., a bias towards responding that the arrays were equal or unequal). We used signal detection framework to calculate our key dependent variables: accuracy or sensitivity (*d'*) and response bias (*c*). Sensitivity (*d'*) indexes an individual's ability to accurately differentiate between *signal* and *noise* trials, or in our case, differentiating between *equal* and *unequal* trials. Larger *d'* values indicate a greater ability to arbitrate between the two. To calculate *d'*, two measures are required: (1) hit rate and (2) false alarm rate. Hit rate reflects the proportion of times participants correctly hit the space bar (i.e., hitting the space bar for an equal trial in the equal task or for an unequal trial in the unequal task). Participant false alarm rate reflects the proportion of times participants incorrectly hit the space bar (i.e., hitting the space bar for an equal trial in the unequal task or an unequal trial in the equal task). From these two measures, we can also determine whether participant responses are biased in a particular direction (*c*). If responses are unbiased (i.e., if participants exhibit no bias toward stating the distributions are equal or unequal), *c* is zero. We coded *c*-values for both task version such that a negative c-value always indicates a bias towards responding that the images are unequal, and a positive c-values always indicates a bias towards responding that the images are equal. Practically, a negative c-value indicates that an individual sees inequality even in its absence and a positive c-value indicates that an individual sees equality even in its absence.

Results and Discussion

We preregistered one central exclusion criterion. Based on a 1000-round simulation of the 120-trial task, we determined that the likelihood of having 17 'Go' or 'No Go' trials in a row was highly statistically improbable (less than .001%). Thus, we planned to exclude participants who had over 17 'Go' or 'No Go' responses in a row, reasoning that this consecutive responding likely reflected inattention.

As predicted, we found that SDO was significantly negatively correlated with *d'*, $r = -.08$, $p = .002$, suggesting that individuals lower (vs. higher) in SDO were more accurate at differentiating between equal and unequal resource distributions. In contrast, the relationship between *c* and SDO was not significant, $r = -.01$, $p = .76$.

Thus, using a speeded task assessing basic social cognition, social egalitarians— individuals chronically motivated to reduce the gap between groups at the top and bottom of society— were more accurate than anti-egalitarians at arbitrating whether inequality was present or absent, consistent with the possibility that they are more attentionally vigilant for it. We did not find any evidence suggesting that social egalitarians have a bias towards claiming inequality. That is, egalitarians' response pattern was marked by more accurately arbitrating whether inequality was present or absent, rather than simply a lower threshold for claiming inequality (even in its absence). We now turn to Study 3, which combines the strengths of Study 1 in terms of its focus on naturalistic attention to inequality cues, and the strengths of Study 2 in terms of its focus on the processing of inequality-relevant information early in the cognitive stream.

Study 3

Across two samples, Study 3 examined how an individual's (anti-)egalitarianism predicts spontaneous attention to inequality using a change detection task. Specifically, we examined how

long it took individuals higher (vs. lower) on SDO to notice inequality-relevant changes occurring to inequality-relevant images. Our primary hypothesis was that individuals higher (vs. lower) in SDO would take longer to identify inequality-relevant changes occurring in inequality-relevant images, controlling for the time it took them to notice inequality-irrelevant changes occurring in inequality-irrelevant (neutral) images. We conducted two studies (3a and 3b below) to assess this question, both of which we preregistered (see Appendix A for links).

Study 3a

Participants

To detect an association of $\beta = .10$ using multiple linear regression with a two-tailed test and 90% power, the suggested sample size was 1,043. In total, we collected data from 1,027 participants using MTurk ($M_{age} = 40.8$, $SD = 12.6$; 55.3% Female, 44.7% Male; 828 White, 45 Asian, 74 Black, 48 Latino, 8 Native American, 18 Biracial, 3 Middle Eastern, 3 Other).

Procedure

Participants completed 10 trials of a flicker task (Rensink, O'Regan, & Clark, 1997), which included five inequality-relevant and five neutral (inequality-irrelevant) trials, presented in a random order. The task structure was such that participants viewed an original image for 1 second followed by a blank screen for 250 milliseconds. This blank screen was followed by an altered version of the original image, which stayed on the screen for 350 milliseconds and was followed by a second blank screen for 250 milliseconds. This sequence repeated on a loop until participants hit the space bar to indicate they noticed the change. In inequality trials, the change always involved an inequality-relevant cue (e.g., a homeless man's bag disappearing, see Appendix A Figures A19-A30 for all images). In neutral trials, the change was irrelevant to

social inequality (e.g., a message disappearing from a bus LED screen).[4] Once participants hit the space bar to indicate that they noticed the change, they were asked to describe in detail what changed in the image.

Measures

(Anti-)Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha = .95$).

Average Number of Inequality and Neutral Image Repeats. We were primarily interested in how long it took participants to (correctly) notice changes occurring in inequality-relevant images, which served as a proxy for their attention to different parts of the image. We theorized that those paying closer attention to parts of the scene in which the change occurs should be faster to notice the change. To index this, we measured the number of times participants viewed the flickering sequence before they reported noticing the change. The flickering sequence repeated at maximum 25 times before moving to the next trial, during which time participants were asked to hit the spacebar once they noticed the change. At that point (or after the 25 maximum repetitions were up), participants were asked to describe the change in detail.

If participants identified the change correctly (as rated by manual coders), we reported their score for that trial as the number of views at which they hit the spacebar (e.g., 11 if they hit the space bar after 11 repeats of the sequence). We preregistered that if participants reported the change incorrectly, we would automatically set their time for that trial to the maximum of 25 views. We were also interested in how long it took participants to (correctly) notice changes

[4] In an image pretest, a separate sample of participants rated the changes in the set of inequality images as significantly more inequality-relevant than the changes in the set of neutral images, $t\,(57) = 8.16$, $p < .001$.

occurring in neutral images, which served as our control. We averaged participants' number of views for each of the inequality-relevant and neutral sets of images.

Results and Discussion

Using our preregistered analysis plan for Study 3a, we found our expected positive correlation between SDO and the average number of repeats for inequality images, $r = .15$, $p < .001$. Using OLS regression to control for the average number of repeats for neutral images, we found that SDO remained a significant positive predictor of the average number of repeats for inequality images, $b = .08$, $t\,(1027) = 3.07$, $p = .002$.

In Study 3a, we found our expected positive correlation between SDO and the average number of repeats on inequality images, which held when controlling for the average number of repeats on neutral images. Despite this supportive evidence, we reasoned that our preregistered analysis plan of setting incorrect trials to the 25-repeat max may be confounding attention to a particular cue (as indexed by a lower number of repeats) with accuracy in identifying that cue (a failure of which is treated as equivalent to 25 repeats). If individuals higher in SDO generally tended to be less correct across all trials, then these individuals would also receive slower times, potentially confounding accuracy and attention. In addition, we found that whereas over 92% of responses were accurate for eight of the ten images, participants had more difficulty correctly identifying the change in two of the neutral images (Neutral Images 1 and 5; with accuracies of 78% and 83%, respectively).

With these potential limitations of Study 3a in mind, we replicated our results among a new sample of participants in Study 3b. We updated the two neutral images and pretested to ensure that the overwhelming majority of participants would be able to *correctly* identify the change. We also revised our preregistered exclusion criteria to make it less susceptible to

accuracy concerns. Specifically, we preregistered that we would only include times from trials on which participants correctly identified the change and restricted our analyses to participants who had a high overall rate of accuracy (at least 3 in 5 accurate responses for each of the neutral and inequality-relevant categories).

Study 3b

Participants

To detect an association of $\beta = .10$ using linear multiple regression with a two-tailed test and 95% power, the suggested sample size was 1,293. We collected data from 1,514 participants using MTurk (M_{age} = 37.6, *SD* = 12.1; 50% Female, 50% Male, 1,085 White, 109 Asian, 140 Black, 82 Latino, 17 Native American, 37 Biracial, 3 Middle Eastern, 1 Other).

Procedure

The task procedure for Study 3b was identical to that of Study 3a except we replaced two neutral images that had relatively higher rates of inaccurate responding and updated our preregistered analytic criteria. We pretested all images in Study 3b to ensure that over 90% of participants correctly noticed the change for each image.

Measures

Anti-Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha = .94$).

Average Number of Inequality and Neutral Image Repeats. As in Study 3a, we measured the number of times participants viewed the flickering sequence before they (correctly) reported noticing inequality-relevant changes to inequality-relevant images and neutral changes to neutral images. As preregistered (and in contrast to Study 3a), if participants reported the change incorrectly, we ignored their time for that image. As preregistered, we excluded participants who received more than 4 'incorrect' responses across all 10 trials or more than 2 'incorrect'

responses across either of the neutral or inequality-relevant trials (ensuring that for each participant there were, at minimum, times from 3 'correct' trials entering into both the inequality and neutral composites). With these exclusions applied, our sample was 1,474 participants (97.4% of the initial sample).

Results

Using our preregistered analysis plan for Study 3b, we again found our expected positive correlation between SDO and the average number of repeats for inequality images, $r = .10$, $p < .001$. Using OLS regression to control for the average number of repeats for neutral images, we found that the relationship between SDO and average number of repeats for inequality images was marginally significant, $b = .04$, $t\,(1474) = 1.89$, $p = .059$.

Study 3 Meta-Analysis Results and Discussion

We found that the relationship between SDO and average number of inequality image repeats was robust when meta-analyzing across Studies 3a and 3b (zero-order correlations: $r = .13$, $z = 6.35$, $p < .001$; controlling for neutral trials: $b = .06$, $z = 2.82$, $p = .005$), including when analyzing both studies using Study 3b's updated exclusion criteria (zero-order correlations: $r = .11$, $z = 5.52$, $p < .001$; controlling for neutral trials: $b = .05$, $z = 2.32$, $p = .02$).

Thus, across two sub-studies without any prompting regarding the theme of inequality, we obtained evidence suggesting that individuals more committed to social egalitarianism are chronically more visually attentive to cues of inequality in everyday scenes. One notable aspect of Study 3 was that all of the inequality-relevant changes involved low status targets (e.g., a homeless man) rather than changes to high status targets (e.g., a luxury car). This raises the possibility that egalitarians are particularly attuned to inequality *only* when it involves bias against groups that they ideologically favor (i.e., socially disadvantaged groups). This would be

consistent with the findings of Study 2, in which the disadvantaged group in the Go-No Go task, women, is *also* a societally disadvantaged group. In Studies 4 and 5, we examine whether the link between egalitarianism and attention to inequality depends on the target of that inequality (i.e., whether the target is from a socially disadvantaged or advantaged group).

Study 4

In Study 4, we used a financially incentivized task to examine whether the link between anti-egalitarianism and accurate detection of inequality depends on the social standing of the target of that inequality. Specifically, we examined whether the relationship between anti-egalitarianism and accurate detection of gender-based speaking time disparities differed depending on whether it was men (a socially advantaged group) or women (a socially disadvantaged group) who took up a disproportionate share of the airtime. Study 4 was preregistered (see Appendix A for link).

Participants

Based on a correlation of $r = .10$ between SDO and perceptions of inequality in any given condition, we needed 780 participants to achieve 80% power. Following our preregistration plan, we sought to collect data from 800 participants per condition, for a total of 1,600 participants. This sample size was intended to be based on the number of participants after our exclusion criteria was met (described below). In total we collected data from 2,130 participants using MTurk ($M_{age} = 38.9$, $SD = 12.4$; 56.6% Female, 42.9% Male, .5% Other; 1,084 White, 116 Asian, 119 Black, 91 Latino, 4 Native American, 39 Biracial, 5 Middle Eastern, 9 Other) of whom 1,467 participants passed all three exclusions (68.8% of full sample).

Procedure

All participants watched a 4.5-minute video which depicted a panel of two men and two women discussing technology design. Participants were randomly assigned to one of two conditions: (1) a condition in which the men spoke 1.5x longer than the women or (2) a condition in which the women spoke 1.5x longer than the men. Within each condition, we counterbalanced the version of the video participants watched. Participants watched one of two versions of the same panel, in which we varied which gender spoke first and which spoke last (to vary which gender might have been more salient due to primacy or recency effects).

Prior to watching the video, participants were informed that after the video they would be asked to answer a series of memory questions and that those who responded most accurately would receive a $50 prize. Importantly, participants were not told what aspects of the video we were interested in, and inequality was never mentioned. By providing a financial incentive for all participants to focus on the video, we reduce the possibility that any link between SDO and accuracy/attention to inequality is driven by those higher in SDO simply responding more carelessly to experiments in general (and/or experiments that appear to them to investigate inequality). After watching the video, participants were asked to "describe, to the best of your ability, what the video was about." Participants then answered our key dependent measures and filled out measures of anti-egalitarianism and demographics.

Stimuli Development. To develop the video stimuli, we began by finding a video of an inequality-irrelevant panel discussion that included equal numbers of men and women. We chose a 34-minute video of a panel discussing designing technology for users that consisted of two men and two women. From the 34-minute video, we created two sets of approximately 4 minute and 30 second videos (four videos total)– one set of videos in which the men spoke 1.5x longer than

the women and a second set of videos in which the women spoke 1.5x longer than the men. Recognizing the potential for primacy and recency effect, we counterbalanced which gender spoke first and last. In the set of videos where men spoke more than women, we created two video versions: in one, a woman spoke first, and a man spoke last and in the other, a man spoke first, and a woman spoke last. We similarly varied gender speaking order in the set of videos where women spoke more than men; one version began with a woman speaking and ended with a man speaking and the other began with a man speaking and ended with a woman speaking. Instead of splicing the precise same video clips together in a different order for each video (and having the videos be nonsensical), we used different 4 minute 30 second portions of the 34-minute video, in order to have more naturalistic stimuli.

Measures

(Anti-)Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha = .94$).

Accurate Detection of Inequality. Our key dependent measure asked participants to "Please select the chart that you think best represents the ratio of speaking time for men and women." Participants were presented with seven pie charts to choose from:

Option 1: 35% men: 65% women
Option 2: 40% men: 60% women
Option 3: 45% men: 55% women
Option 4: 50% men: 50% women
Option 5: 55% men: 45% women
Option 6: 60% men: 40% women
Option 7: 65% men: 35% women

In Condition 1 (where men spoke more than women), the correct answer was Option 6: 60% men: 40% women. In Condition 2 (where women spoke more than men), the correct answer was Option 2: 40% men: 60% women. We dichotomously examined whether or not participants

selected the correct answer. Participants received a score of '1' if they selected the correct pie chart for their condition and a score of '0' otherwise.

Underestimation. We also dichotomously coded whether participants made a selection indicating that they underestimated the extent of inequality actually faced by the disfavored target in their bias condition (a score of '0' indicated the absence of underestimation; a score of '1' indicated that participants' selection was an underestimate, see Appendix A Figure A31 for results using a measure of *degree* of underestimation). In Condition 1, where men spoke more, Options 1 through 5 constituted an underestimation of the amount of inequality actually faced by women. In Condition 2, where women spoke more, Options 3 through 7 constituted an underestimation of the amount of inequality actually faced by men.

Overestimation. We likewise dichotomously coded for whether participants made a selection indicating an overestimation of the extent of inequality actually faced by the disfavored target in their bias condition (a score of '0' indicated the absence of overestimation; a score of '1' indicated that participants made an overestimating selection). In both conditions, there was only one choice that reflected an overestimation. In Condition 1, where women were disadvantaged, Option 7 was an overestimating selection, whereas in Condition 2, where men were disadvantaged, Option 1 was an overestimating selection.

Exclusion Criteria

We preregistered three exclusion criteria. First, we planned to exclude participants we determined (based on ratings from blind coders to our question which asked participants to summarize what the video was about) as clearly not indicating knowledge of what was in the video. Of the 2,130 participants who completed the task, 1,955 passed the first exclusion and correctly described what was in the video (91.8% of full sample). In addition, we planned to

exclude participants who missed an attention check embedded in our survey. Of the 2,130 participants who completed the task, 2,090 passed the attention check (98.1%). Finally, we preregistered that we would exclude participants who did not complete the survey in the default full screen mode (our survey platform recorded whether participants closed the full screen). Of the original 2,130 participants, 1,569 kept the task in full screen mode (73.7%). In total, 1,467 participants passed all three exclusion criteria (68.8% of the initial sample).

Results and Discussion

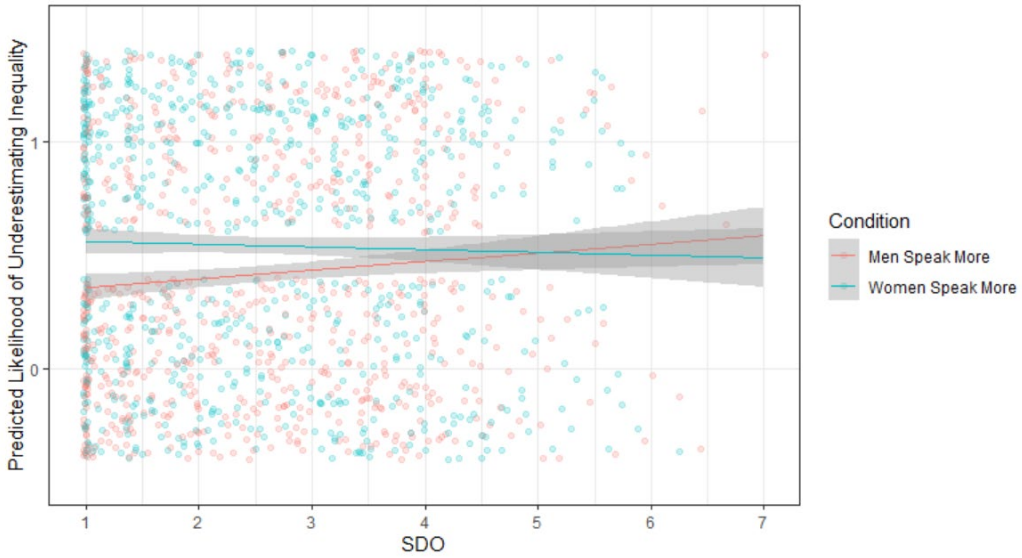
Accurate Detection of Inequality. Using binomial logistic regression, we observed a marginally significant interaction effect, $b = .19$, $p = .08$, 90% [.01, .36], between SDO and task condition in predicting whether participants accurately detected the extent of speaking time inequality (i.e., selected the accurate pie chart). In the condition where men spoke more than women, we observed a negative main effect of SDO on accuracy, $b = -.19$, $p = .01$, OR = .83, 95% [.70, .96], with egalitarians significantly *more* likely to select the accurate pie chart than those higher on anti-egalitarianism. In contrast, in the condition where women spoke more than men, we observe no significant differences between individuals lower and higher in SDO in terms of accuracy, $b = -.01$, $p = .92$, OR = .99, 95% [.86, 1.14].

At low levels of SDO (-1SD below the mean; $M_{SDO} = 2.58$, $SD = 1.29$), task condition was not a significant predictor of accuracy; individuals lower in SDO were equally likely to select the correct pie chart in Condition 1 (where men spoke more) as in Condition 2 (where women spoke more), $b = -.05$, $p = .80$, OR = .95, 95% [.66, 1.36]. At high levels of SDO (+1SD above the mean), however, individuals were significantly more likely to select the correct pie chart in Condition 2 (where women spoke more) relative to Condition 1 (where men spoke more), $b = 0.43$, $p = .03$, OR = 1.54, 95% [1.04, 2.29].

Underestimating Inequality. Turning to our measure of underestimation, a binomial logistic regression revealed a significant interaction effect, b = -.20, p = .01, 95% [-.37, -.04], between SDO and bias condition in predicting whether participants underestimated the extent of inequality. In the condition where men spoke more than women, individuals lower (vs. higher) in SDO were significantly *less* likely to underestimate the level of inequality, b = .16, p = .01, OR = 1.17, 95% [1.04, 1.31]. In contrast, when women spoke more than men, SDO did not significantly predict underestimation, b = -.05, p = .40, OR = .95, 95% [.85, 1.06].

Examining the interaction another way, those lower in SDO (-1 SD) were more likely to underestimate the extent of inequality when women spoke more than when men spoke more, b = .76, p < .001, OR = 2.14, 95% [1.60, 2.89]. Individuals higher in SDO (+1 SD), by contrast, were no more likely to underestimate inequality in one condition versus the other, b = 0.24, p = .11, OR = 1.27, 95% [.95, 1.70] (Figure 6)

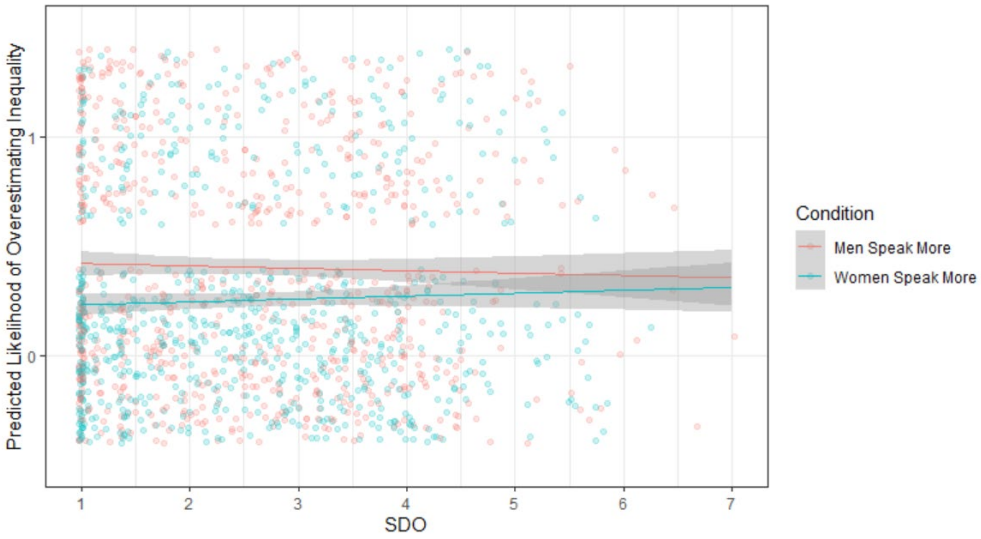
Figure 6 *Predicted Likelihood of Participants Underestimating Inequality*



Note: A score of '0' corresponds to an accurate or overestimating selection and '1' corresponds to an underestimating selection. Note that data points on this graph are "jittered" via R to aid in visualization (values of this variable are only '0' or '1').

Overestimating Inequality. Finally, using binomial logistic regression, we observed no significant interaction effect between SDO and bias condition in predicting participant likelihood of overestimating the extent of speaking time inequality, $b = .11$, $p = .19$, 95% [-.06, .29]. When men spoke more than women, we observed no significant association between SDO and the likelihood of overestimating inequality, $b = -.05$, $p = .44$, OR = .95, 95% [.85, 1.07]. The same was true when women spoke more than men, $b = .07$, $p = .29$, OR = 1.07, 95% [.94, 1.22]. For those both lower and higher in SDO (-/+ 1SD), there was a significant main effect of task condition, such that participants were less likely to overestimate inequality when women spoke more than men relative to when men spoke more than women (at -1SD SDO: $b = -.84$, $p < .001$, OR = .43, 95% [.31, .59]; at +1SD SDO: $b = -.55$, $p < .001$, OR = .58, 95% [.42, .79], Figure 7).

Figure 7 *Predicted Likelihood of Participants Overestimating Inequality*



Note: A score of '0' corresponds to an accurate or underestimating selection and '1' corresponds to an overestimating selection. Note that data points on this graph are "jittered" via R to aid in visualization (values of this variable are only '0' or '1').

Across the three measures, then, when women were disadvantaged, egalitarians (vs. those more tolerant of social hierarchy) were (1) significantly more accurate at detecting inequality, (2)

were significantly less likely to underestimate the extent of inequality, and (3) were no more likely to overestimate inequality. These accuracy advantages for egalitarians tended to dissipate (but not *reverse*) when men were disadvantaged (i.e., in the condition where women spoke more). In Study 5, we again examined how (anti-)egalitarianism differentially predicted attention to unequal treatment depending on the social standing of the target of that inequality, this time in the domain of racial biases in hiring.

Study 5

In Study 5, we examined how (anti-)egalitarianism predicted attention to racial bias in hiring across two experimental conditions: (1) a condition in which there was anti-minority bias in hiring and (2) a condition in which there was (equivalent) anti-White bias in hiring. In addition, we considered a potential downstream consequence of attention to inequality, namely whether participants endorsed wanting to investigate the organization's hiring process. Study 5 was preregistered (see Appendix A for a link).

Participants

Based on a correlation of $r = .10$ between SDO and noticing inequality in any given condition, we needed 780 participants per condition to achieve 80% power. Because we had two task conditions, we collected data from 1,603 participants using MTurk ($M_{age} = 40.6$, $SD = 13.1$; 56.0% Female, 43.7% Male, .3% Other; 944 White, 63 Asian, 94 Black, 60 Latino, 8 Native American, 25 Biracial, 5 Middle Eastern, 2 Other). We aimed to collect data from individuals across the political spectrum and in order to collect data from roughly equal number of Republicans and Democrats, all participants were asked "which political party do you most identify with?" and selected either Republican, Democrat, or Independent. Participants who selected Independent *(*$n = 380$) were then asked to indicate, "as of today, do you lean more

Republican or Democrat?" Due to a survey error, participants who selected Independent did not fill out the SDO scale and therefore could not be included in our primary analyses. Of note, however, when we conducted our (pre-registered) secondary analyses using conservatism in place of SDO, we obtained the same patterns as with SDO. Moreover, our distribution of SDO scores in the present study was comparable to our other studies. In total, 1,201 participants provided data on all focal variables (74.9% of full sample).

Procedure

Participants read about an organization called Connection Consulting (CC) that had just completed their hiring process and were shown the resumes of 56 applicants who varied across 5 dimensions: race, GPA, major, hometown, and hobby. Half of the applicants were White, and half were racial minorities (Latino, Asian, Black). After viewing each candidate's resume, participants learned whether that applicant was hired or not. Participants were randomly assigned to one of two conditions, which differed only in terms of the correlation between race and likelihood of being hired. In Condition 1, being a minority (vs. White) applicant was negatively correlated (at r = -.29) with the likelihood of being hired, whereas in Condition 2, being a minority (vs. White) applicant was positively correlated (at r = +.29) with the likelihood of being hired. In both conditions, the task was structured such that GPA was the best predictor of hiring decisions (and was correlated at r = +.57 with the likelihood of being hired) and the correlation between all other factors (major, hometown, hobby) and being hired was 0. We assessed the extent to which participants noticed inequality across the two conditions by asking participants, after they completed the resume task, to "Please note anything that stood out to you about the hiring process." Participants then filled out our other dependent measures (described below), a measure of anti-egalitarianism, and demographic information.

Task Structure. In the task, the 56 applicants were presented in proportions consistent with their representation in the United States Census. For example, Black individuals make up roughly 34% of the minority population in the United States. Our task was structured such that out of the 28 minority applicants, 10 (~35%) were Black. Applicants' GPAs ranged from 3.4 to 4.0 in 0.1 increments. Applicants were assigned to one of seven majors which were balanced across all other factors. In addition, applicants were assigned to one of 28 hometowns and one of 28 hobbies which were balanced across each of the other factors. In both conditions, Asian, Black, and Latino applicants did not have different probabilities of being hired from one another.

Measures

(Anti-)Egalitarianism. We assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha = .95$).

Naturalistic Noticing of Bias. Participants were asked to "Please note anything that stood out to you about the hiring process." We coded for whether participants naturalistically mentioned inequality in the hiring process. By naturalistic, we mean that we did not explicitly ask participants about whether they noticed inequality, and thus, this serves as a measure for whether they spontaneously brought it up. For this metric, we dichotomously coded whether or not participants – correctly – mentioned unequal treatment against the group actually disadvantaged within their experimental condition. Participants in Condition 1 received a score of '1' if they mentioned unequal treatment against minority applicants and a score of '0' otherwise. Participants in Condition 2 received a score of '1' if they mentioned unequal treatment against White applicants and a '0' otherwise.

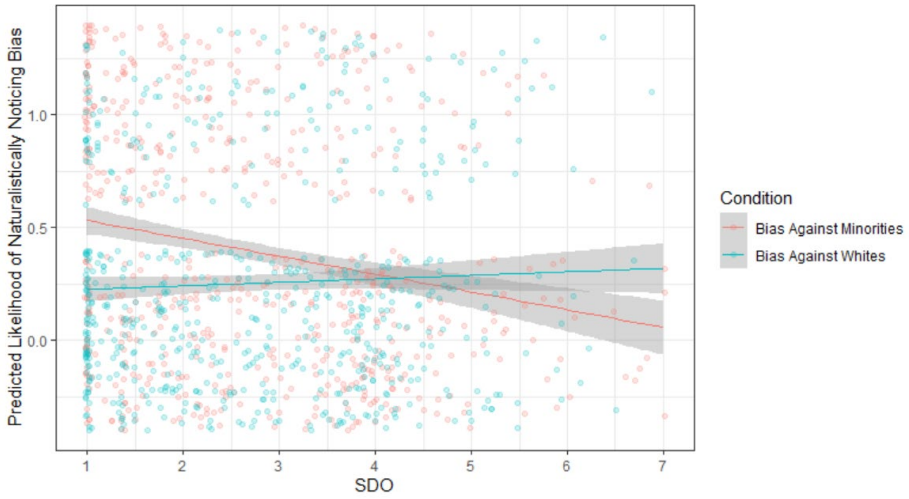
Desire to Investigate. We assessed a potential downstream consequence of noticing inequality, namely, the extent to which participants supported investigating Connection

Consulting (CC) for its hiring practices. Participants endorsed their level of agreement with each of the following statements on a 1 (*Strongly disagree*) to 7 (*Strongly agree*) scale (α = .94): (1) 'A third party should investigate CC's hiring practices', (2) 'CC should reconsider its hiring practices', (3) 'CC's hiring practices should be reviewed', (4) 'CC's hiring practices seem fair (reverse coded)', and (5) 'CC's hiring practices appear to be legitimate (reverse coded)'.

Results and Discussion

Naturalistic Notice Bias. Using binomial logistic regression, we observed a significant interaction effect, $b = 0.44$, $p < .001$, 95% [0.26, 0.62], between SDO and condition in predicting whether participants naturalistically (and correctly) noticed bias. In the anti-minority bias condition, we observed our predicted main effect of SDO, $b = -.36$, $p < .001$, OR = .70, 95% [.61, .79]: in line with the conclusions of Studies 1-4, individuals lower (vs. higher) in SDO were significantly more likely to notice bias against minority applicants when it was present. In contrast, in the anti-White bias condition, we observed a positive but non-significant trend between SDO and attention to bias, $b = 0.08$, $p = .22$, OR = 1.08, 95% [.95, 1.23] (Figure 8).

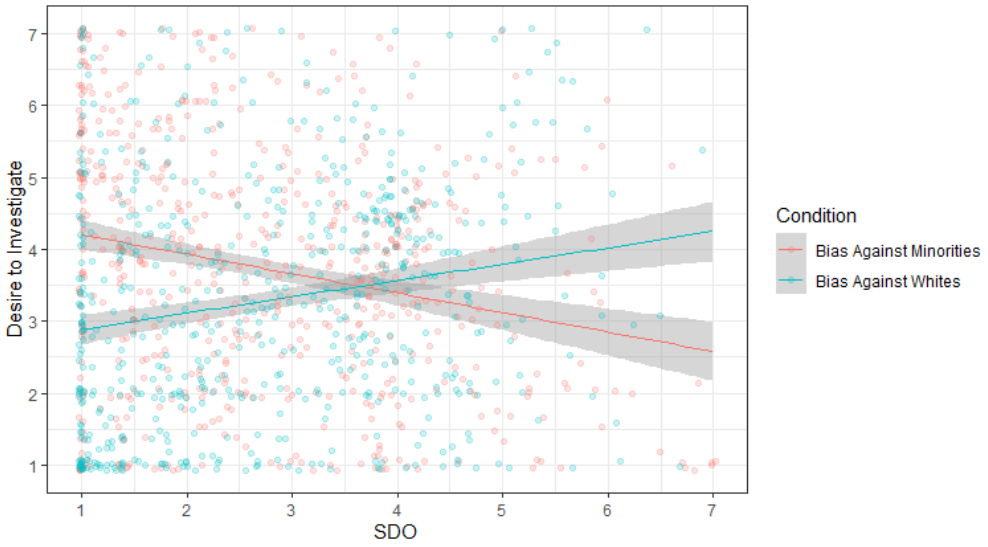
Figure 8 *Link Between SDO and Naturalistically Noticing Bias*



At low levels of SDO (-1SD below the mean—M_{SDO} = 2.77, SD = 1.43), bias condition was a significant predictor of the proclivity to naturalistically notice bias; individuals lower in SDO were significantly more likely to report noticing bias in Condition 1 (anti-minority bias condition) versus Condition 2 (anti-White bias condition), b = -1.25, p < .001, OR = .29, 95% [.20, .41]. At high levels of SDO (+1SD above the mean), there was no significant difference between the likelihood of naturalistically noticing bias across the two conditions, b = 0.001, p = 1.00, OR = 1.00, 95% [.69, 1.44]. Individuals even higher in SDO (+2SD above the mean) were significantly more likely to naturalistically report noticing unequal treatment in the anti-White bias versus anti-minority bias condition, b = 0.65, p = .03, OR = 1.92, 95% [1.06, 3.46]. Of note, it was low SDOs in the condition where there was bias against minority applicants who exhibited the highest overall likelihood of (correctly) noticing bias (about 50.6%).

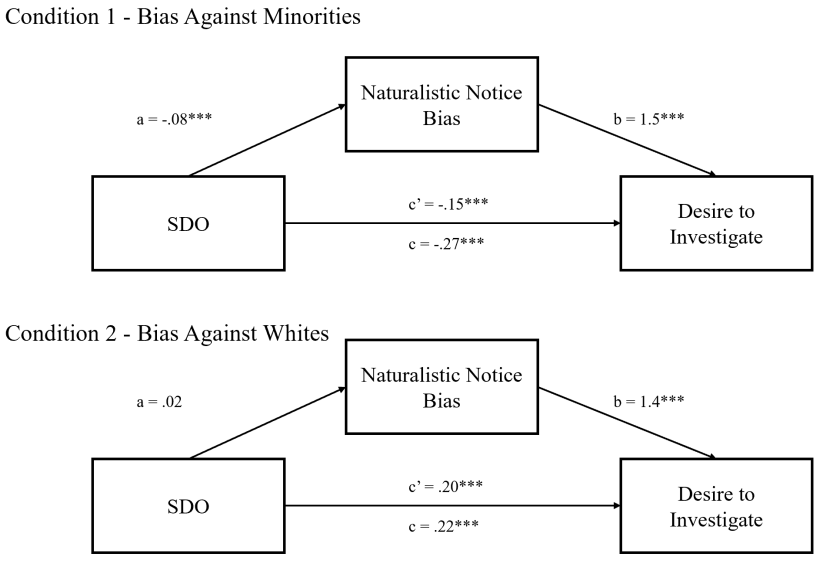
Desire to Investigate. We observed a significant interaction between SDO and task condition in predicting the desire to investigate Connection Consulting's hiring practices, b = 0.50, p < .001, 95% [0.37, 0.63]. In the anti-minority bias condition, individuals higher (vs. lower) in SDO were significantly *less* likely to endorse wanting to investigate the hiring practices, b = -0.27, p < .001, 95% [-0.36, -0.18], whereas when there was anti-White bias, we found that individuals higher (vs. lower) in SDO reported a significantly greater desire to investigate, b = 0.23, p < .001, 95% [0.13, 0.32]. Examining the interaction a different way, we found that individuals lower in SDO (-1SD below mean) reported a significantly greater desire to investigate the hiring practices in the anti-minority versus anti-white bias condition, b = -1.15, p < .001, 95% [-1.41, -0.89], whereas individuals higher in SDO (+1SD above mean) reported a marginally greater desire to investigate in the anti-White versus anti-minority bias condition, b = 0.25, p = .055, 95% [-0.01, 0.51] (Figure 9).

Figure 9 *Link Between SDO and Desire to Investigate*



We next examined evidence for moderated mediation, entering SDO as the predictor, naturalistic notice bias as the mediator, and desire to investigate as the outcome, with condition as a moderator of each of the *a*, *b*, and *c* paths. In the anti-minority bias condition, we find a significant negative indirect effect of SDO on the desire to investigate via naturalistically (and correctly) noticing bias, b = -.12, SE = .02, 95% [-.16, -.08]. In contrast, in the anti-White bias condition, there was no significant indirect effect of SDO on desire to investigate via naturalistically noticing bias, b = .02, SE = .02, 95% [-.02, .06]. For low SDOs (-1SD), the indirect effect of task condition on the desire to investigate via naturalistically noticing bias was significantly negative, b = -.44, SE = .07, 95% [-.58, -.30]. For high SDOs (+1SD), in contrast, the indirect effect of task condition on the desire to investigate via naturalistically noticing bias was not significant, b = -.002, SE = .05, 95% [-.09, .09] (Figure 10).

Figure 10 *Mediation Model Linking SDO to the Desire to Investigate Via Naturalistically Noticing Bias*



Note: ***p <.001

In line with our predictions, when there was bias against racial minority students (Condition 1), individuals lower (vs. higher) in (anti-)egalitarianism were more likely to spontaneously report noticing the inequality, which led them to express a greater desire to investigate Connection Consulting's hiring process. Thus, consistent with the results of Studies 1-4, when unequal treatment affected individuals from traditionally socially disadvantaged groups, social egalitarians were more likely to notice the presence of inequality.

Previously, we outlined two potential predictions for the relationship between (anti-)egalitarianism and attention to inequality. If egalitarians are better overall at detecting inequality when it is present, we should see a similar pattern in Condition 2, such that social egalitarians remain more likely to notice inequality relative to anti-egalitarians. Our results suggest, however, that when there was bias against White students (Condition 2), social egalitarians were not more likely than anti-egalitarians to notice it.

General Discussion

Social inequality is a pressing issue of the 21st century, yet efforts to address it will remain stifled without a common, accurate understanding of the magnitude of the issue. While most people recognize the existence of inequality, there are stark disagreements across ideological lines about its extent, its victims, and about what – if anything – should be done to alleviate it. Prior work suggests that when asked to explicitly evaluate inequality-relevant information, individuals adopt interpretative frames that allow them to preserve their preexisting ideological – or worldview – motivations. Whereas anti-egalitarians (who are motivated to maintain existing hierarchical social arrangements) perceptually downplay the existence of inequality between groups, social egalitarians (who are ideologically motivated to reduce inequality) process evidence of inequality in ways that perceptually highlight its existence.

While being asked to explicitly evaluate or interpret inequality is unlikely to be a common occurrence, people *are* likely to frequently encounter evidence of inequality in their everyday lives. Commuting to work through different neighborhoods might illuminate discrepancies between rich and poor. Organizational hiring and promotion efforts might reveal unequal treatment based on gender and race. Building upon previous work which examines the ways in which ideological motives affect how inequality-relevant information is evaluated, interpreted, and perceived, the present work examines whether and how ideology affects people's proclivity to naturalistically notice inequality in the first place.

We theorized that as a function of the ideological motivations to attenuate inequality and increase equality between groups in society, social egalitarians would be more perceptually vigilant for and thus more likely to encode evidence of inequality relative to those more tolerant of group-based inequality. In line with this prediction, Study 1 found that social egalitarians were

more likely than anti-egalitarians to report noticing inequality when asked to note anything that stood out to them about images of everyday city-life, some of which contained cues relevant to inequality. In Study 2, employing a basic cognitive task, we found that social egalitarians were more accurate than anti-egalitarians at arbitrating unequal distributions of resources (which favored men over women) from equal distributions. In Study 3, we measured naturalistic attention to inequality with a change-detection task and found that social egalitarians (vs. anti-egalitarians) were faster to detect inequality-relevant changes to visual scenes, suggesting a greater attentional focus to evidence of inequality.

Importantly, inequality in Studies 1-3 always came at the expense of traditionally disadvantaged (vs. advantaged) groups (e.g., women, poor people). Studies 1-3 thus raise an important question: are social egalitarians (vs. anti-egalitarians) more attentive to *all* forms of social inequality or do they selectively attend to particular types? In Studies 4 and 5, we experimentally manipulated whether participants viewed panels in which men versus women dominated speaking time (Study 4) or hiring processes in which White versus minority candidates were disproportionately less likely to be hired (Study 5). We replicated the findings of Studies 1-3, again observing that egalitarians (vs. anti-egalitarians) were more likely to naturalistically (and correctly) notice bias when traditionally disadvantaged groups (women, racial minorities) were affected. Importantly, however, we found that egalitarians were not more likely (and by some metrics – see Appendix A Figures A34-35– were *less* likely) than anti-egalitarians to notice inequality when advantaged groups (e.g., men, Whites) were affected.

Our findings provide evidence for an attentional mechanism by which political partisans might come to disagree about the extent of inequality in the world around them. As a function of their ideological motivation to attenuate inequality and their propensity to encode inequality-

relevant cues, egalitarians (and the wider political left) might be frustrated when anti-egalitarians (and the wider political right) fail to notice the mistreatment of disadvantaged groups in society. As a function of their own perceptual tendencies, however, individuals who tolerate hierarchy between groups, however, might not notice inequality (particularly when it harms disadvantaged groups) and might therefore believe that social egalitarians are either seeing inequality where none exists or selectively attending to certain types of inequality.

Theoretically, the present work contributes to a growing body of research suggesting that the social status of targets (and specifically, whether targets belong to a societally advantaged or disadvantaged group) deeply shapes (anti-)egalitarians' perceptions and behavior. For example, while prior work suggested that egalitarians (and the wider political left) are more dispositionally empathic than anti-egalitarians, recent work refutes this theory by demonstrating that egalitarians (vs. anti-egalitarians) perceive an identical act as causing *less* objective harm when it is committed against an advantaged, high-power target (e.g., a CEO) versus when it affects a disadvantaged, low-power target (Lucas & Kteily, 2018). Similarly, in contrast to the political right, those on the political left – in service of their desire to increase equality between groups – preferentially amplify the success of members of disadvantaged (but not advantaged) groups, in this case, by tweeting (Kteily, Rocklage, McClanahan, & Ho, 2018). This work suggests that social egalitarians are primarily concerned with reducing disparities between groups in society, which might require them to selectively focus on improving outcomes for groups at the bottom. Our finding here that social egalitarians selectively attend to evidence of inequality which harms disadvantaged (but not advantaged) groups is consistent with this possibility.

It is important to note that our results do not support the notion that egalitarians saw inequality when it did not exist. In Study 2, we found a significant link between egalitarianism

and our measure of accuracy (*d'*), suggesting that egalitarians (vs. anti-egalitarians) were more accurate at distinguishing between equal and unequal resource distributions. We did not find, however, any significant link between egalitarianism and our measure of response bias (*c*), suggesting that egalitarians (vs. anti-egalitarians) did not simply have a lower threshold for claiming that distributions were unequal. In other words, our results suggest that egalitarians are not seeing inequality 'everywhere' (even where it does not exist), but rather seem to be accurately detecting inequality when it is present. Moreover, in Study 4 using our measures of accuracy, we found that social egalitarians (vs. anti-egalitarians) were more likely to accurately detect, were less likely to underestimate, and were no more likely to overestimate the extent of unequal airtime when men (vs. women) dominated speaking time. When women spoke more than men, egalitarians were no less accurate than anti-egalitarians. In sum, relative to anti-egalitarians, egalitarians seem to be especially likely to notice – and accurately detect –inequality affecting groups at the bottom of the hierarchy *when* such inequality exists, rather than seeing inequality where none exists or selectively *overlooking* inequality affecting those at the top.

Despite the contributions of the present work, there are key limitations worth mentioning. First, the effect sizes we observed, while robust, were typically very small, although consistent with similar research (Kraus, Torrez, Park, & Ghayebi, 2019). The size of our effects makes it difficult to conclude from our findings that there are overwhelming differences in how social egalitarians and anti-egalitarians differentially attend to their surrounding social contexts. But because we were investigating naturalistic attention to forms of inequality that people are likely to frequently encounter in their everyday lives, even small effects can add up.

In addition, although we tested our theorizing here across a variety of experimental paradigms and across different inequality-relevant domains (e.g., economic, gender, racial),

future work would benefit from paradigms which move beyond the lab to explore people's naturalistic attention to inequality 'in the wild'. Experience sampling or daily diary methodologies could, at random intervals, prompt participants to indicate what stands out to them as they navigate the world. Participant responses could then be coded to examine whether individuals are differentially likely to mention inequality-relevant topics as a function of their ideological leanings. In addition, eye tracking methodology could be used to examine what individuals visually attend to in their daily lives. It would be especially interesting to examine whether individuals with the same objective access to inequality on their daily commute (i.e., who take the same train to work) are differentially likely to notice evidence of inequality as a function of their ideological motivations.

Moreover, future work should examine how attention to inequality shapes support (or lack thereof) for real-world policy preferences (e.g., support for redistribution, affirmative action), as our Study 5 analysis of the desire to investigate 'Connection Consulting' preliminarily suggests. Future work should further examine whether these patterns hold in different social and cultural contexts (for example, with non-WEIRD participants or in contexts outside of the United States where inequality is less of a partisan issue). Finally, future work should consider how we might nudge attention to – and recalibrate inaccurate perceptions of – social inequality. Given the findings in the current work that anti-egalitarians (and the wider political right) are *less* likely than social egalitarians (and the wider political left) to notice unequal treatment affecting members of societally disadvantaged groups, in Chapter 3 we specifically explore how we might nudge the wider political right to notice – and accurately perceive the extent of – these instances of social inequality.

Chapter 3

Inequality[5] is a pervasive feature of social life with particular groups (e.g., poor people, women, racial minorities) bearing a disproportionate amount of its burden (Chancel, Piketty, Saez, & Zucman, 2022; World Economic Forum, 2019; Inequality.org, 2021). While social inequality is itself ubiquitous, attention to – and accurate perceptions of – its extent is not. Prior work demonstrates that people's proclivity to notice and accurately detect the extent of inequality affecting disadvantaged groups diverges in meaningful ways, often along ideological lines. Relative to the political left – who are ideologically committed to attenuating social inequality – those on the political right, who are more tolerant of group-based hierarchies, are less attentive to, less accurate at detecting, and more likely to underestimate the extent of social inequality when societally disadvantaged groups bear its brunt (Waldfogel, Sheehy-Skeffington, Hauser, Ho, & Kteily, 2021).

With these underlying ideological differences in mind, the present chapter asks, how might we encourage the wider political right to notice and be accurately calibrated to the extent of inequality affecting groups at the bottom of the social hierarchy? Chapter 3 tests two interventions aimed specifically at increasing (1) attention to and (2) accurate detection of the extent of inequality affecting racial minorities among those affiliated with the political right.

Why *doesn't* the political right notice unequal treatment which affects disadvantaged groups? Perhaps failures to notice inequality stem from people being unaware or uninformed of

[5] While inequality can be defined in a variety of ways and exist across different domains (i.e., racial, gender, economic, racial economic, etc.), here we define inequality as an asymmetric distribution of valuable resources, which may impact consequential outcomes (Willis et al, 2022). This definition of inequality is intentionally broad, encompassing both unequal outcomes and the processes that give rise to those outcomes. While instances of unequal treatment can theoretically affect members of societally high- or low-power groups, in the present work we focus solely on instances of inequality which affect traditionally disadvantaged (vs. advantaged) groups.

its extent. If so, prompting attention to and accurate detection of the extent of inequality might require simply providing factual information about inequality. In Study 1, across two samples of participants affiliated with the political right, we test how information about inequality affects subsequent attention to – and accurate detection of – racial inequality.

It's possible, however, that the political right fails to notice inequality not because they are unaware of relevant information, but because evidence of inequality – particularly inequality which affects societally disadvantaged groups – (1) lacks motivational relevance and is therefore ignored or that, even when noticed, evidence of inequality (2) prompts active rationalization processes which perceptually downplay its extent.[6] In other words, it may be indifference or active resistance, rather than a lack of knowledge, which explains failures to attend to inequality. If this is the case, then reframing inequality in ways that more clearly align with conservative values might make inequality more motivationally relevant and less likely to elicit resistance, thereby increasing the tendency to notice and accurately detect its extent. In Study 2, across two samples of individuals affiliated with the political right, we test whether reframing inequality in terms of conservative moral values increases subsequent attention to and accurate detection of social inequality in instances where disadvantaged group members bear the brunt. We theorize that while it's possible that information about inequality may be ignored or actively downplayed rendering it ineffective, reframed and value-consistent inequality information will be more easily digestible by the political right, and therefore will capture attention and bypass rationalization processes.

[6] The distinction here is between inequality being ignored/*not* processed vs. processed in a way that minimizes it.

Ideology and Naturalistic Attention to Inequality: A Refresher

People's ideological motivations shape whether they notice and accurately detect the extent of unequal treatment affecting societally disadvantaged groups. In general, people perceive the world around them in ways that align with and further their preexisting goals and beliefs, whether individual or collective (Balcetis & Dunning, 2006; Dunning, Perie, & Story, 1991; Waytz, Young, & Ginges, 2014). In addition to the overall motivations to view themselves and their groups positively, people are further motivated to uphold and protect their ideological worldviews. In the current work, we concentrate on the ideological motivation to maintain and justify existing social inequalities and focus specifically on two ideologies shown to be especially relevant to how – and whether – inequality-relevant information is processed: social dominance orientation (SDO) and political conservatism. While SDO specifically focuses on whether people are motivated to maintain (vs. dismantle) forms of inequality, political conservatism is comprised both of attitudes towards inequality and attitudes towards traditionalism (Jost, Glaser, Kruglanski, & Sulloway, 2003; Sidanius & Pratto, 1999).

SDO (Ho, et al., 2012; Ho, et al., 2015; Sidanius & Pratto, 1999) measures an individual's preference for inequality between groups in society. Those lower in SDO (egalitarians) believe that groups in society should be equal, whereas those higher in SDO (anti-egalitarians) are more tolerant of intergroup hierarchy. In service of the ideological motivation to maintain inequality, anti-egalitarians endorse a range of hierarchy-enhancing beliefs, including an overall tendency to justify the existing (hierarchical) social system (Ho, et al., 2015; Kay & Jost, 2003). In contrast, and in service of their ideological commitment to attenuating inequality between groups, egalitarians favor beliefs which problematize inequality and policies which lessen it (e.g., wealth redistribution, affirmative action, Ho et al, 2015). The difference in beliefs

about the desirability of group-based equality represents a core feature of political conservatism (alongside differences in traditionalism; Jost et al, 2003), with the wider political left subscribing more strongly to egalitarian beliefs than the political right (Jost, Glaser, Kruglanski, & Sulloway, 2003; Jost & Amodio, 2012).[7]

Prior work reveals that ideological motivations relate to differences in (1) attention to and (2) accurate detection of the extent of inequality affecting societally disadvantaged groups (e.g., racial minorities, poor people, women). For example, Chapter 2 (Waldfogel, Sheehy-Skeffington, Hauser, Ho, & Kteily, 2021) demonstrates that anti-egalitarians are less likely than egalitarians to spontaneously report noticing inequality-relevant cues in images of everyday life (Study 1) and are slower to detect inequality-relevant changes to those images (Study 3), reflecting overall less visual attention to inequality. Moreover, anti-egalitarians are less likely than egalitarians to naturalistically report noticing unequal treatment in hiring affecting racial minority applicants and are less willing to investigate the company involved (Study 5). Thus, prior work suggests that relative to the political left, those ideologically committed to maintaining inequality are *less* attentive to inequality affecting low-power groups.

Chapter 2 further suggests that the political right is not only less attentive to inequality but also less accurate at calibrating its extent, in particular because they *underestimate* the level of inequality affecting disadvantaged groups. When exposed to a discussion panel where men spoke 1.5x longer than women, anti-egalitarians were less accurately calibrated to the extent of inequality affecting disadvantaged groups (here, women) relative to egalitarians (Study 4).

[7] Because the current chapter is primarily concerned with interventions aimed at the political right, the terms 'anti-egalitarian', 'high SDO', 'politically conservative', 'Republican', and 'the wider political right' may be used interchangeably to mean individuals who are more tolerant of societal inequality.

Specifically, when asked to select from a series of pie charts depicting speaking time distributions, anti-egalitarians (vs. egalitarians) were less likely to select the accurate distribution (60% men, 40% women) and were more likely to select a distribution which underestimated the extent of inequality in speaking time (i.e., perceiving *less* inequality affecting women than was truly present).

These results suggest that relative to the political left, the political right is less likely to notice and is less accurately calibrated to the extent of inequality affecting traditionally disadvantaged groups. Moreover, this work suggests that inaccurate calibrations are driven in large part by those on the political right *underestimating* the extent of inequality affecting these groups. Keeping in mind these underlying ideological differences, the present work tests the effectiveness of two interventions intended to increase the likelihood that those on the political right (1) notice and (2) are accurately calibrated to the extent of social inequality.

Goals of an Intervention

What does it mean for an intervention to be effective? In the present work, we intentionally distinguish between attention – whether people *notice* inequality – and accuracy – whether people are correctly calibrated to its extent. While related (i.e., it's reasonable to assume that greater attention should prompt greater accuracy), these measures are distinct. To fully differentiate the two, consider the difference between not *noticing* inequality and not being accurately *calibrated* to the extent of inequality.

To not notice inequality is to ignore or overlook it. Put differently, it means that instances of inequality do not rise to the level of attention in the first place. Unlike inattention to inequality – which can be thought of as a binary measure where inequality is either subjectively noticed or not – there are multiple ways to be inaccurately calibrated to the extent of inequality. For

example, individuals can notice inequality while overestimating or underestimating its extent. Both over- and under-estimation reflect inaccurate calibrations, yet the *patterns* of inaccuracy differ; one pattern of inaccuracy reflects that inequality is highlighted and overperceived, while the other reflects that inequality is downplayed and underperceived.

The distinction between attention and accuracy is further evident when considering the potential effects of an intervention on each measure. In terms of attention to inequality, an intervention can either (1) increase the likelihood that people report noticing inequality, (2) decrease that likelihood, or (3) have no effect on people's proclivity to report noticing inequality. In contrast, because accurate detection of inequality encompasses not only accuracy but also over- and under-estimations of the extent of inequality, there are a variety of possibilities for how an intervention might affect patterns of inaccuracy.

For example, consider how an intervention might affect patterns of inaccuracy in the context of gender-based speaking time disparities where men (vs. women) take up a disproportionate amount of the airtime (e.g., the condition in Chapter 2, Study 4 where men speak for 60% of the time and women speak for 40% of the time). An intervention might increase the likelihood that people report noticing that men spoke more than women without actually affecting their proclivity to accurately detect the extent of inequality. For example, an intervention might increase people's subjective attention to inequality without changing their estimates of the speaking-time distribution (i.e., which pie chart they select). Or an intervention might increase people's subjective attention to unequal airtime while simultaneously making them more likely to overestimate the extent of inequality by the same amount they would have underestimated it, for example by pushing participants who would have selected an underestimating pie chart option (e.g., 55% men:45% women) to select an overestimating option

(e.g., 65% men:35% women). Or an intervention might increase attention while pushing those who would have *vastly* underestimated the extent of inequality (e.g., selecting the 35% men:65% women pie chart) closer to an accurate selection while still prompting them to select a distribution which underestimates the extent of inequality (e.g., 55% men:45% women). Because these distinctions are meaningful, it's important to look not only at whether an intervention affects people's likelihood of subjectively reporting "I noticed inequality" but also at whether an intervention affects objective patterns of (in)accuracy. In sum, attention to inequality and accurate calibrations of its extent capture correlated but distinct outcome measures.

Of note, an intervention may not make individuals more or less accurate, even as it nevertheless influences the *ways* in which they are inaccurate. For example, an intervention could make someone who underestimated inequality by a factor of 10% come to *overestimate* it by 10%; in principle, it could also make someone who overestimated inequality by 10% now come to *underestimate* it by 10%. In both cases, the individuals would be off the true answer by 10%. Still, from a societal perspective, it's possible that one could deem certain patterns of inaccuracy as more desirable than others; for example, given the hierarchical status quo which disadvantages groups at the bottom, some might consider it preferable to have individuals overestimate inequality by a factor of 10% than underestimate it by the same amount.

In the present work, we deem an intervention effective if it satisfies both of the following:

1. Increases the likelihood that individuals on the political right notice inequality (attention).
2. Increases the likelihood that individuals on the political right are accurately calibrated to the extent of inequality. That is, ideally an intervention would bring those on the political right closer to an objectively correct perception of the extent of inequality,

reducing any tendency to underestimate without simultaneously increasing any tendency to overestimate or vice-versa (accuracy).

Information-Based Inequality Interventions

If the wider political right fails to notice and accurately detect the extent of inequality in the world around them because they are unaware of it, one strategy may be the provision of factual information about forms of social inequality affecting societally disadvantaged groups. Prior work (not focused on ideology) suggests that such information might effectively correct misperceptions of and prompt people to take action against inequality (Callaghan, Harouni, Dupree, Kraus, & Richeson, 2021; McCall, Burk, Laperriere, & Richeson, 2017).

One study examined how information about rising economic inequality (e.g., "from 1979 to 2007, average income grew by 278% for the 1% of the population with the highest total household income… for the poorest 20% of the population, average income rose 18%") affected subsequent beliefs about the importance of structural (e.g., family wealth) and individual (e.g., hard work) factors in "getting ahead" in life (McCall, Burk, Laperriere, & Richeson, 2017). Relative to a control, participants who read about inequality placed greater emphasis on the importance of structural (vs. individual) factors in succeeding in life and were more likely to endorse the view that institutions (e.g., governments, corporations), rather than individuals, have a responsibility to reduce inequality. These authors theorize that information about inequality interfered with participants' beliefs in economic opportunity, which prompted greater support for redistribution (McCall, Burk, Laperriere, & Richeson, 2017). Thus, to the extent that the political right is merely uninformed about inequality, information about its existence may prompt greater attunement to cues of inequality and thus, encourage heightened attention to and correct inaccurate calibrations of its extent.

Information-based interventions aimed at correcting misperceptions of racial economic inequality (i.e., the magnitude of the wealth gap between White and Black Americans) find similar effects. On average, Americans underestimate the extent of racial economic inequality, a phenomenon attributed, in part, to widespread and deeply held beliefs about racial progress (e.g., that the US has moved towards racial equality over time) and meritocracy (e.g., that outcomes are proportional to effort; Callaghan et al, 2021; Kraus et al, 2017, 2019). Recent work attempting to correct racial wealth gap misperceptions finds that a data intervention (which provides concrete statistics about racial inequalities) prompts more accurate perceptions relative to a narrative intervention (which emphasizes one Black family's struggles with unequal treatment, Callaghan et al, 2021). Whereas a narrative intervention leads people to focus on individual (vs. structural) efforts to alleviate inequality, a data intervention concentrates attention on the broader societal structures that uphold and maintain inequality, thus promoting a more accurate understanding of it (Callaghan, Harouni, Dupree, Kraus, & Richeson, 2021). Again, this work suggests that to the extent that the political right is simply uninformed about inequality, providing information about it – and, in particular, information that directs attention to structural (vs. individual) determinants of inequality – might effectively prompt greater attunement to it.

Backfire Effects. While information about inequality may effectively increase attention to it, it may also trigger reactance (Hetey & Eberhardt, 2014; 2018) and prompt people to engage in motivated processing to avoid uncomfortable realities that violate their worldview beliefs (Onyeador, et al., 2020). In one study, participants provided with statistics about the realities of Black-White racial disparities in incarceration rates subsequently expressed *decreased* support for policies that would alleviate these disparities (i.e., they were less willing to end a prejudicial stop-and-frisk policy; Hetey & Eberhardt, 2014). In this case, rather than information about

inequality recalibrating inaccurate perceptions, exposure to accurate information about racial disparities in criminal justice increased fear and triggered stereotypes, reinforcing an implicit association between Black Americans and crime (Hetey & Eberhardt, 2014; 2018).

Truthful information about inequality may also backfire to the extent that it prompts motivated processing. People underestimate the extent of racial economic inequality, in part, because they cling to narratives of racial progress, believing that the US has moved linearly towards racial equality over time (Kraus, Rucker, & Richeson, 2017; Kraus, Onyeador, Daumeyer, Rucker, & Richeson, 2019; Richeson, 2020). In addition to specific narratives of progress, people tend to believe the world in which they live is fair, just, and meritocratic and that people get what they deserve (Benabou & Tirole, 2006; Jost, Banaji, & Nosek, 2004). Information about the realities of inequality clashes with the motivation to view society as fair and in order to minimize the discomfort that arises from the dissonance, people may perceptually downplay the extent of inequality.

In one study documenting these motivated processing effects, participants read information about current racial inequality (e.g., "racial disparities in health, wealth, and overall wellbeing") and information about how racial inequality has persisted over time (e.g., "compared to the 1960s, neighborhoods and… schools have remained largely racially segregated," Onyeador et al, 2020). After, participants were asked to estimate past (1963) and present-day (2016) racial economic disparities. Relative to a control condition, participants who read about the persistence of racial inequality *did* more accurately assess the lack of racial progress over time, but this increased accuracy came not from participants updating their estimates of present-day disparities, but instead from participants assessing past disparities as *more* equitable. Said differently, rather than information about the persistence of racism prompting participants to

accurately calibrate to the extent of present and past disparities, participants downplayed the realities of past disparities, a result the authors suggest stems from a desire to reduce the psychological discomfort triggered by discrepancies between deeply-held progress narratives and information which violates those narratives (Onyeador, et al., 2020).

Thus, while there is evidence to suggest that information about inequality may prompt greater attention to and calibrate inaccurate perceptions of inequality, such information may be liable to motivated processing. In the section that follows, I argue that people's ideological motivations – and their desires to maintain their worldview – may affect how they react to evidence of inequality affecting traditionally disadvantaged groups. In particular, it's possible that the ideological motivation to maintain existing inequality will render information-based inequality interventions less effective because such information is either (1) ignored (not processed at all) or (2) minimized (processed in a way that perceptually downplays it).

Ideologically Motivated Responses to Inequality-Relevant Information

Providing information about inequality affecting societally disadvantaged groups – and in particular information which implies that inequality is an issue that should be addressed – may be ineffective for those on the political right for two reasons. First, for those motivated to maintain existing inequalities, evidence of inequality implying it should be reduced is inconsistent with the underlying motivation to maintain existing inequalities and may therefore be ignored (i.e., not noticed). Second, even when noticed, evidence of inequality which implies the need for its attenuation might be psychologically threatening for individuals motivated to uphold existing hierarchies, thus prompting them to engage in perceptual processing which rationalizes and minimizes the existence or extent of inequality.

Ignoring Inequality. Whereas the political left is ideologically committed to the goal of attenuating inequality and is therefore motivated to work against the existing system to bring about egalitarian social change, the political right is ideologically motivated to maintain, justify, and even enhance existing social hierarchies (Ho, et al., 2015; Jost, Glaser, Kruglanski, & Sulloway, 2003; Pratto, Sidanius, & Levin, 2006). These underlying motivations affect how – and whether – people process information about inequality. In general, people attend to the features of their environment that are motivationally relevant (Brosch & Van Bavel, 2012; Lang, Bradley, & Cuthbert, 1997). For the political left, noticing inequality is a critical precursor to bringing about social change, thus information about inequality affecting societally disadvantaged groups – particularly information which implies that inequality should be reduced – is in line with these broader motivations. Consistent with this theorizing, prior work suggests that egalitarian goals – whether chronically held or temporarily experimentally activated – predict greater attention to evidence of inequality affecting societally disadvantaged groups (Moskowitz, 2002; Moskowitz & Li, 2011; Waldfogel, Sheehy-Skeffington, Hauser, Ho, & Kteily, 2021). In contrast, for the political right, information about inequality affecting disadvantaged groups – especially that which implies the need for inequality to be reduced – is inconsistent with the motivation to maintain inequality and may instead be overlooked. To the extent that information which aligns with preexisting motivations is less likely to be ignored (Taber & Lodge, 2006), it's possible that evidence of inequality will be disregarded by the political right, rendering information-based inequality interventions less effective.

Downplaying Inequality. Beyond the possibility that evidence of inequality is ignored by the political right, it may also prompt processing which actively downplays the extent of inequality. Specifically, because the political right is motivated to maintain existing hierarchies

in society, information about the attenuation or minimization of inequality – or information implying that changes to the social system are occurring or should occur – can threaten deeply-held beliefs and prompt rationalization processes.

Outside of the domain of ideology, people's general motivations to maintain their advantaged social position prompt them to process information in ways that downplay the extent of inequality. For example, White Americans provided with evidence that they benefit from racial privilege manage the threat associated with this information by increasing claims of personal hardship (Phillips & Lowery, 2015). This motivated response allows advantaged group members to deny that they themselves have benefitted from racial privilege, thus downplaying the extent of racial inequality in society (Phillips & Lowery, 2015). When exposed to evidence of their privilege, beneficiaries of class privilege similarly respond by highlighting their personal struggles, which allows them to downplay the existence of societal group-level class-based advantages (Phillips & Lowery, 2020). Moreover, individuals belonging to societally advantaged groups misconstrue policies that increase equality (i.e., policies that decrease disparities between groups) as explicitly harmful to their own group (Brown, Jacoby-Senghor, & Raymundo, 2022) even when such policies actually confer benefits to their group (Brown & Jacoby-Senghor, 2021). Taken together this work suggests that information about inequality – especially that which implies inequality should be reduced – may be particularly worldview-threatening to those who belong to societally advantaged groups and may therefore prompt processing which perceptually minimizes the extent of inequality.

Prior work further suggests that ideological motivations to maintain inequality prompt people to downplay evidence of inequality affectively, physiologically, and perceptually. Relative to those who view the current economic system as illegitimate and unfair, people

motivated to justify the economic system exhibit muted affective and physiological responses to inequality (Goudarzi, Pliskin, Jost, & Knowles, 2020). Essentially, this work suggests that the ideological motivation to justify the existing social system might buffer individuals from negative emotions around inequality, allowing them to downplay its existence (Goudarzi, Pliskin, Jost, & Knowles, 2020). Beyond prompting affective and physiological downplaying, ideological motives also shape whether inequality is *perceptually* downplayed. For example, when presented with identical stimuli depicting hierarchical organizations, anti-egalitarians perceptually downplay the extent of inequality, perceiving the same stimuli as being less steep (i.e., less unequal) than do social egalitarians (Kteily, Sheehy-Skeffington, & Ho, 2017).

Prior work further suggests that inequality-relevant information is particularly worldview-threatening to the political right. Anti-egalitarians – those concerned with maintaining existing societal inequalities – perceive the same change in equality between two groups as more substantial than egalitarians do (Eibach & Keegan, 2006). This difference in perception occurs in part because anti-egalitarians (like individuals belonging to advantaged groups, see Brown et al, 2022) view hierarchy-attenuating changes (i.e., increases in opportunity for racial minority Americans) as *losses* for their own group and as threats to their own group's status, privilege, and dominance (Eibach & Keegan, 2006).

In light of this prior work, we theorize that evidence of inequality may be ineffective in prompting those on the political right to attend to and accurately calibrate the extent of inequality affecting societally disadvantaged groups for two reasons. First, because information about inequality – especially that which implies inequality should be reduced – is inconsistent with the ideological goal of maintaining existing inequalities, such information may simply be ignored or overlooked. Beyond being ignored, information about inequality may be particularly worldview-

threatening to the political right, thus prompting them to process evidence of inequality in ways that rationalize its existence and downplay its extent. Despite this theorizing, we nevertheless test how providing information about inequality affects subsequent attention to and accurate detection of unequal treatment affecting disadvantaged groups. We also supplement this test with an examination of another intervention that we expect to be less worldview-threatening and therefore more robust to motivated information processing – namely, an intervention that reframes the issue of inequality in terms of conservative values.

Overview of Chapter 3

Across two studies (total N = 2784), we examine the effects of two interventions on the likelihood that those on the political right notice and accurately detect the extent of social inequality affecting disadvantaged groups (e.g., racial minorities). In both studies, participants complete a mock hiring task where minority (vs. White) applicants are disproportionately less likely to be hired given equivalent credentials. In Study 1, across two samples, we test how factual information about racial inequality affects subsequent attention to and accurate detection of unequal treatment affecting racial minorities. In Study 2, we examine whether reframing information about inequality in terms of resonant moral values affects our outcomes of interest.

Study 1

In Study 1, we sampled from individuals affiliated with the political right (i.e., Republicans or Republican leaners) to examine attention and accurate detection of the extent of racial bias in hiring across two conditions: an information intervention and a control. We conducted this study with two samples of participants (Sample 1a and 1b below). Both samples

included additional conditions and participants not reported here.[8] Our study design, key dependent measures, exclusion criteria, and central analyses were preregistered for both samples (see Appendix B for links).

Participants

Sample 1a. We collected data from 830 participants for our focal conditions of interest using Amazon's Mechanical Turk (MTurk). As preregistered, we excluded participants who failed either of two attention checks, leaving us with a final sample of 735 participants (88.6% of initial sample; M_{age} = 44.4, *SD* = 13.2, 55.9% Female, 44.1% Male; 646 White, 26 Asian, 20 Black, 23 Latino, 3 Native American, 2 Middle Eastern, 12 Biracial, 3 Other).

Sample 1b. We collected data from 765 participants for our focal conditions of interest using MTurk and Prolific Academic.[9] As preregistered, we excluded participants who failed either of two attention checks, leaving us with a final sample of 698 participants (91.2% of initial sample; M_{age} = 40.5, *SD* = 13.6, 53.2% Female, 46.8% Male; 605 White, 22 Asian, 24 Black, 32 Latino, 3 Native American, 11 Biracial, 1 Other).

Procedure.

The procedure for both focal conditions was identical across the two samples. At the start of the survey, participants filled out demographic questionnaires, including measures of their political orientation and anti-egalitarianism. They were then randomly assigned to read

[8] Both samples in Study 1 also included participants affiliated with the political left. Sample 1a included a second intervention condition, specifically, an attention nudge condition, which gave participants the explicit goal of paying attention to any bias in hiring. Sample 1b included a second bias condition, specifically, a condition in which White (vs. minority) applicants were disproportionately less likely to be hired. See Appendix B for information regarding additional conditions and participants.

[9] To ensure that all participants completing this task were unique, we excluded MTurk participants who completed previous versions and sampled only from Prolific Academic participants who did not use other survey platforms.

information about racial economic inequality or to a control condition with no information and subsequently completed the mock hiring task from Chapter 2, Study 5. All participants read:

> Connection Consulting received 56 applications for 28 positions from students at universities around the country. In the following task, we will ask you to look through the resumes of the 56 applicants. You'll see information about each student's background and whether they were hired by Connection Consulting. At the end of the task, we will ask you to answer a series of memory questions. The people who respond most accurately to the memory questions will be entered to win a $50 prize. Please read the resume information carefully.[10]

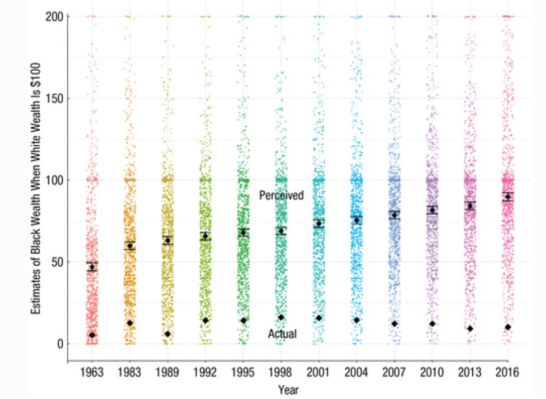
All participants were assigned to complete the version of the mock hiring task in which minority applicants were disproportionately less likely to be hired relative to White applicants with equivalent credentials. Participants finished by filling out our dependent measures.

Intervention.

Information Treatment. People have a general proclivity to underestimate racial economic inequality, specifically because they tend to believe that there has been *more* progress towards racial equality than actually exists (Kraus, Rucker, & Richeson, 2017; 2019). Motivated by this and by related work demonstrating the effectiveness of data interventions in correcting inaccurate perceptions (Callaghan, Harouni, Dupree, Kraus, & Richeson, 2021), participants in the information treatment read about racial wealth inequality. Specifically, participants were asked to "Assume the average White family in the United States has $100. How much money do you think a comparable Black family has?" After making an estimation, participants were informed of the correct answer, which is less than $10. Participants then read information about how misperceptions of racial inequality have persisted over time and viewed a figure which depicted the persistence of these misperceptions (Figure 11).

[10] Unlike Chapter 2, Study 5, here we explicitly incentivized accuracy prior to the task, to make the task (financially) incentive compatible.

Figure 11 Information Treatment Condition



Note. Figure sourced from Kraus et al (2019).

Measures.

Political Orientation. Because we were interested in the effect of the intervention on individuals who identified with the political right (vs. left), we asked participants, "Which political party do you most identify with?" and provided the options: 'Democrat', 'Republican', and 'Independent'. If 'Independent' was selected, we followed up with a question asking, "As of TODAY, do you lean more towards Democrats or Republicans?" We also assessed (anti-)egalitarianism using the 16-item SDO_7 scale (Ho et al, 2015, $\alpha s > .95$) and assessed general political conservatism with two measures asking participants to indicate their political attitudes on a scale of 1 (*very liberal*) to 7 (*very conservative*) for both economic and social issues. Our final analyses include only participants who identified as Republican or as leaning Republican.

Attention to Inequality. After clicking through the 56 resumes and hiring decisions, participants were asked to "Please note anything that stood out to you about the hiring process" and were provided with a free response box. To decrease potential demand effects, here we did not explicitly ask participants about inequality, and thus, this serves as a measure of whether they spontaneously brought it up. We coded only for the presence or absence of mentions of bias

against minority applicants. Participants received a score of '1' if they indicated noticing unequal treatment against minority applicants and a score of '0' otherwise. This measure corresponds to our first intervention goal of increasing attention to inequality.

(In)Accuracy at Detecting Inequality. While attention to inequality allows us to examine whether people subjectively report noticing unequal treatment, it doesn't allow us to examine whether participants are accurately calibrated to the extent of unequal treatment. Our measure of (in)accuracy allows us to gauge not only whether people are accurately detecting the extent of inequality, but also the patterns of inaccuracies (i.e., over- vs. under-estimation).

After noting anything that stood out about the hiring process, participants read:

> Earlier, you saw 56 resumes. From those resumes, 28 people were hired, and 28 people were not hired. Please select the chart that best represents the distribution of hired white students to hired minority students.

Participants were presented with 13 pie charts, depicting the following hiring ratios:

Option 1: 26 minority students: 2 White students
Option 2: 24 minority students: 4 White students
Option 3: 22 minority students: 6 White students
Option 4: 20 minority students: 8 White students
Option 5: 18 minority students: 10 White students
Option 6: 16 minority students: 12 White students
Option 7: 14 minority students: 14 White students
Option 8: 12 minority students: 16 White students
Option 9: 10 minority students: 18 White students
Option 10: 8 minority students: 20 White students
Option 11: 6 minority students: 22 White students
Option 12: 4 minority students: 24 White students
Option 13: 2 minority students: 26 White students

From the distribution selected, we generated a series of (in)accuracy measurements. First, we examined whether participants selected the accurate pie chart (*Accurate Detection*). The correct distribution for all participants is Option 9: 10 minority students: 18 White students, thus, participants received a '1' for accurate if they selected Option 9 and a '0' otherwise. We then

examined patterns of inaccuracy and specifically explored whether participants under- or overestimated the extent of racial inequality in hiring. Participants received a '1' on *Underestimation* if they selected a pie chart that involved *less* inequality disfavoring minority students than was actually present (i.e., if they selected Option 1 through Option 8) and a '0' otherwise. We coded *Overestimation* in the same way conceptually, giving participants an overestimation score of '1' if they selected an answer that involved *more* inequality disfavoring minority students than was actually present (i.e., if they selected Option 10 through Option 13) and a score of '0' otherwise. Together, these measures correspond to the second intervention goal of increasing accurate calibrations of the extent of inequality.

Desire to Investigate. As in Chapter 2 Study 5, we assessed a potential downstream consequence of noticing and accurately detecting inequality, namely, whether participants wanted to investigate Connection Consulting's (CC's) hiring practices. Participants endorsed their agreement with the following items on a 1 (*Strongly disagree*) to 7 (*Strongly agree*) scale (αs > .94): (1) A third party should investigate CC's hiring practices, (2) CC should reconsider its hiring practices, (3) CC's hiring practices should be reviewed, (4) CC hiring practices seem fair (reverse coded), and (5) CC's hiring practices appear legitimate (reverse coded).

Analytical Strategy

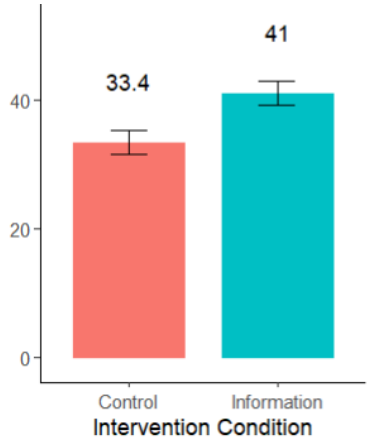
Our central goals were to increase the likelihood that those on the political right (1) reported noticing and (2) were accurately calibrated to the extent of unequal treatment faced by racial minority students. In our focal analyses we include only participants who identified as Republican or as leaning Republican (SDO: $M = 3.28$; economic conservatism: $M = 5.55$; social conservatism: $M = 4.98$). As preregistered, we excluded participants who missed either of two attention checks embedded in the survey. Analyses for dichotomous variables are conducted

using binomial logistic regression. Analyses for continuous variables are conducted using ordinary least squares regression. Because the procedure was identical across both samples, we mega-analyze (Eisenhauer, 2021) the pooled raw data for each focal DV (attention, accurate detection, underestimation, overestimation). After examining our key DVs, we examine whether the intervention directly – or indirectly, via attention to and/or accurate calibrations of the extent of inequality – affected participants' willingness to investigate the hiring practices.

Results

Attention to Inequality. A mega-analysis of Samples 1a and 1b reveals that, relative to the control condition, participants were, on average, significantly more likely to report noticing unequal treatment against minority applicants after reading information about racial inequality, OR = 1.384, 95% [1.109, 1.728], p = .004 (Figure 12).

Figure 12 % of Participants Reporting Noticing Inequality as a Function of Experimental Condition



Note. % of participants noticing bias as a function of experimental condition. Differences in noticing are significant at $p < .01$ level. Graph error bars reflect standard error of the mean.

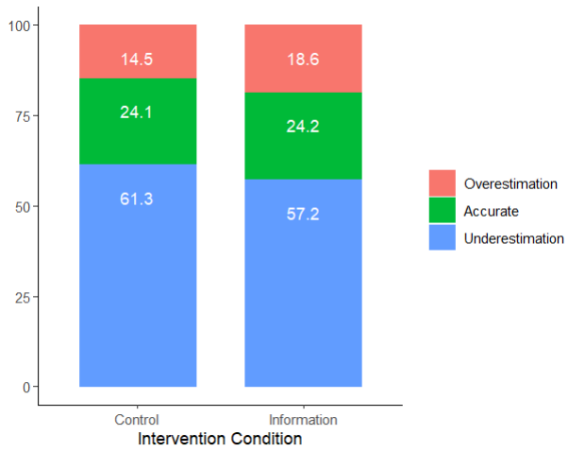
Accurate Detection. Turning now to examine whether the information treatment affected (in)accurate calibrations of the extent of unequal treatment, a mega-analysis of both samples

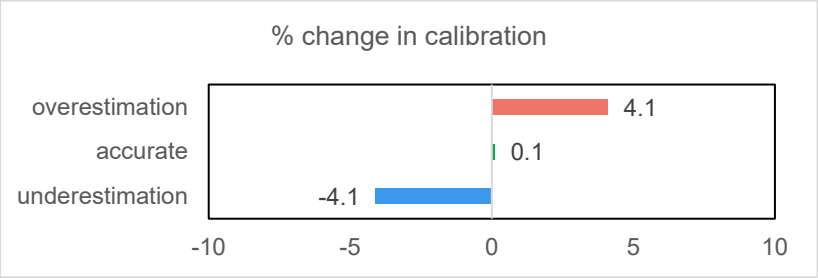
reveals no effect of the information treatment on the likelihood that participants selected the correct pie chart, OR = 1.004, 95% [.782, 1.289], p = .97 (Figure 13).

Underestimation. When examining the likelihood that participants underestimated the extent of unequal treatment faced by minority applicants, a mega-analysis revealed a non-significant negative trend of the information treatment on subsequent proclivity to underestimate inequality, OR = .842, 95% [.677, 1.046], p = .12 (Figure 13). This suggests that relative to those in a control condition, participants who read about racial economic inequality were less likely – albeit not significantly so – to underestimate the extent of unequal treatment.

Overestimation. Finally, a mega-analysis of Samples 1a-b reveals a significant positive effect of the information intervention on the likelihood that participants overestimated the extent of unequal treatment faced by minority applicants, OR = 1.344, 95% [1.008, 1.797], p = .04. This suggests that relative those in a control condition, participants who read information about racial economic inequality were subsequently more likely to select a pie chart whose distribution overestimated the extent of unequal treatment faced by minority students (Figure 13).

Figure 13 % of Participants Underestimating, Accurately Detecting, and Overestimating Bias as a Function of Experimental Condition





Note. Top panel: % of participants underestimating, accurately detecting, and overestimating the extent of inequality by experimental condition. Difference in overestimation is significant at the p = .04 level. Differences in accurate estimation and underestimation are not significant. Bottom panel: % change between the intervention and control.

Desire to Investigate. We next examined whether the information treatment (vs. control) directly affected participants' willingness to investigate the company hiring practices. Analyses pooling the two samples reveal no direct effect of the information treatment on the desire to investigate, b = -.053, 95% [-.232, .126], p = .56. Thus, the intervention does not directly affect the likelihood that participants endorsed taking action against the unequal treatment.

We next explored whether the intervention affected participant willingness to investigate Connection Consulting *indirectly* via the increase in either attention to inequality or overestimation. We conducted two separate mediation analyses with experimental condition as the predictor, attention and overestimation as mediators[11], and desire to investigate as the outcome measure. While we observe no *direct* effect of the information treatment on the desire to investigate, we do observe a significant positive indirect effect of the information treatment on the desire to investigate via increases in the proclivity to report noticing unequal treatment, b = .117, 95% [.036, .199]. We find no significant indirect effect of the intervention on the desire to investigate via the increased tendency to overestimate, b = .041, 95% [-.0004, .083].

[11] 'Attention' and 'overestimation' were used as mediators as they were the only variables significantly affected by the intervention.

Discussion

Based on prior work demonstrating that information about inequality might effectively focus attention on and correct misperceptions of social inequality, in the present study, we examined whether reading about racial economic inequality (relative to a control condition) affected subsequent attention to and accurate calibrations of the extent of unequal treatment faced by racial minority job applicants. Specifically, we examined whether (and if so, how) an information intervention differentially affected these key outcomes for two samples of participants affiliated with the political right.

We find clear evidence that the information intervention increased the tendency to subjectively report noticing unequal treatment against minority students when such bias was present (corresponding to intervention goal #1). Moreover, we find that this increased attention to unequal treatment mediated the relationship between the information intervention and the desire to investigate Connection Consulting. In other words, reading information about racial economic inequality seems to have increased the likelihood that those on the political right reported noticing inequality and this increased attention to unequal treatment increased subsequent willingness to investigate the potentially biased organizational hiring practices.

The results are mixed when examining the effects of the information treatment on (in)accurate calibrations of the extent of inequality (corresponding to intervention goal #2). First, we find that the information treatment had no effect on the likelihood that those on the political right selected a pie chart which accurately reflected the extent of the racial bias in hiring. Despite not increasing overall accuracy, one might consider—from some perspectives—the intervention to be "effective" if it shifts patterns of inaccuracy in a direction more consistent with viewing inequality as an issue worthy of intervention (i.e., if it increases overestimation and decreases

underestimation). When examining the effects of the intervention on patterns of inaccuracy, we find that, relative to a control condition, the information treatment significantly increased the likelihood of overestimation and decreased (although not significantly so) the likelihood that those on the political right underestimated the extent of unequal treatment. Thus, while not increasing overall accuracy, the pattern of errors exhibited by participants exposed to the information treatment indicates a *lower* threshold for claiming the presence of inequality. Still, although these results might be considered promising, our goal for an effective intervention was to increase individuals' tendency to *accurately* detect the extent of unequal treatment rather than just changing the direction of their errors towards overestimation.

Taken together, these results suggest important differences between attention to unequal treatment and accurate calibrations of its extent. While the information intervention was 'effective' in increasing the likelihood that those on the political right mentioned noticing bias against minority students when such bias existed, the information treatment did not increase the likelihood that perceptions of bias against minority applicants were accurate. Had we only looked at the results of the intervention on our measure of attention to inequality, we would likely have concluded that the information treatment was effective as the intervention (vs. control) increased subjectively reported attention to bias against disadvantaged groups. Examining the effect of the intervention on (in)accurate perceptions, however, reveals more nuanced results, highlighting the importance of capturing not only whether interventions increase reported attention to inequality, but also whether they accurately calibrate people to its extent.

These findings hint at potentially fruitful next steps. Specifically, these results suggest that information about inequality may be effective at increasing spontaneous mentions of bias but may *not*, on its own, be sufficient to prompt greater accuracy at detecting inequality among

the political right. In revisiting the question of *why* those on the political right fail to accurately detect the extent of unequal treatment faced by members of societally disadvantaged groups, rather than simply being uninformed, it's possible that these inaccuracies stem from the wider political right simply not caring enough about inequality—particularly that which affects societally low-power groups— to deeply process information in ways that align with greater accuracy. In Study 2, we examine whether reframing social inequality in terms of moral values commonly endorsed by the political right increases attention to and accurate calibration to the extent of unequal treatment experienced by disadvantaged groups members.

Reframing Information About Inequality

Moral Foundations Theory (Haidt & Graham, 2007) suggests that people's intuitions about right and wrong stem from the relative weight assigned to five 'foundations' of morality: (1) harm/care (i.e., concern for the suffering of others), (2) fairness/reciprocity (i.e., concern for justice and equality) (3) ingroup/loyalty, (4) authority/respect, and (5) purity/sanctity (i.e., concerns about avoiding 'disgusting' or norm-violating behaviors).

Importantly, people's ideological worldviews affect which of these foundations are prioritized. While the political left is focused primarily on harm/care and fairness/reciprocity (Graham, Haidt, & Nosek, 2009), the political right more equally weights all five foundations. In other words, while political liberals are concerned largely with whether a particular act is harmful and/or unfair, political conservatives, in addition to harm and fairness concerns, *also* care about whether a particular act betrays the group, defies authority, and/or violates norms.

Ideological differences in the moral weight assigned to each foundation are especially important when considering how the political right may respond to evidence of inequality. It's possible that information about inequality is primarily grounded in harm and fairness concerns.

For example, consider one of the information-based inequality intervention described earlier, which emphasizes racial disparities in domains ranging from housing to education to healthcare (Callaghan, Harouni, Dupree, Kraus, & Richeson, 2021). This intervention primarily underscores that racial disparities are unfair (e.g., "schools that serve communities of color received $23 billion less in funding than schools primarily serving White communities even though the school populations are roughly identical") and cause harm (e.g., "funding gaps can manifest in some non-White districts not being able to afford winter heating costs").

For the political right (vs. left), evidence of inequality which primarily includes concerns about harm and fairness is likely to be less resonant; even though political conservatives *do* care about harm and fairness to some extent, they also care about loyalty, authority, and sanctity. Thus, information about inequality grounded in harm and fairness concerns will, at most, tap a limited set of the moral foundations they consider relevant. While many of the arguments around inequality do emphasize harm and fairness, it's important to note that there *are* arguments against inequality which emphasize politically conservative moral values. For example, in *The Price of Inequality* (2012), Stiglitz argues that inequality in the US undermines core values, violates the sanctity of the American Dream, and disrupts the perception of America as the land of opportunity (Stiglitz, 2012). Moreover, this lack of opportunity affects America's global standing, leading to lower market efficiency, less innovation, less productivity, and impedes "the engines of growth" (Stiglitz, 2012). Prior work suggests that moral reframing (Feinberg & Willer, 2019) – whereby a political issue an individual would not normally support is reframed in a way that resonates with their moral values – may be an effective tool in motivating the political right to both notice *and* be accurately calibrated to the extent of inequality.

Morally reframed arguments have been shown to affect consequential outcomes, including support for politicians (Voelkel & Feinberg, 2018), support for policies (Campbell & Kay, 2014), engagement in pro-environmental behaviors (Bayes, Druckman, Goods, & Molden, 2020; Feinberg & Willer, 2013; Kidwell, Farmer, & Hardesty, 2013), and more general attitudes towards divisive political issues (Day, Fiske, Downing, & Trail, 2014; Feinberg & Willer, 2015). For example, one study conducted prior to the 2016 presidential election framed arguments against both candidates (Donald Trump and Hillary Clinton) in terms which emphasized fairness or ingroup loyalty concerns (Voelkel & Feinberg, 2018). Arguments opposing Donald Trump which were grounded in loyalty concerns (e.g., Trump "has repeatedly behaved disloyally towards our country to serve his own interests") were more effective at reducing conservatives' endorsement of Trump than arguments grounded in fairness concerns (e.g., Trump "openly discriminates against Muslims threatening their rights to be treated with fairness and equality").

Applied to the context of inequality, we theorize that arguments against inequality which highlight conservative moral values (e.g., ingroup loyalty, authority, and/or sanctity) will resonate more readily with the wider political right. In turn, we expect information about inequality grounded in these resonant values to increase attention to and prompt accurate calibrations of the extent of inequality affecting disadvantaged groups.

Study 2

Overview.

In Study 2, we again sought to increase the likelihood that the wider political right noticed and accurately detected the extent of unequal treatment experienced by disadvantaged group members. Across two samples of participants, we tested the effects of a reframing intervention on attention to and accurate detection of inequality faced by racial minority (vs.

White) applicants in a mock hiring task. Our study design, hypotheses, central dependent measures, exclusion criteria, and key analyses were preregistered (see Appendix B for links).

Participants.

Sample 2a. We collected data from 1370 participants using MTurk (M_{age} = 41.6, *SD* = 12.5; 51.4% Female, 48.6% Male; 1052 White, 37 Asian, 122 Black, 68 Latino, 5 Middle Eastern, 43 Native American, 14 Biracial, 5 Other, 24 N/A). As preregistered, we included only participants who identified as Republican/lean Republican leaving a sample of 1064 participants prior to attention check exclusions (77.7% of initial sample). In addition, and as preregistered, we included only those participants who passed both attention checks embedded in the survey, leaving a final sample of 894 participants.

Sample 2b. We collected data from 545 participants using Prolific Academic (M_{age} = 35.3, *SD* = 14.3; 66.8% Female, 32.5% Male, .7% Other; 448 White, 31 Asian, 13 Black, 22 Latino, 1 Middle Eastern, 5 Native American, 16 Biracial, 2 Other, 7 N/A). As preregistered, we included only those participants who identified as Republican/lean Republican, which left us with a sample of 508 participants (93.2% of initial sample). In addition, we included only those participants who passed both attention checks embedded in the survey, leaving a final sample of 457 participants for analyses.

Procedure.

The procedure for both samples was identical. At the start of the survey, participants filled out demographic questionnaires. Participants were then randomly assigned to either a reframing intervention condition or a control condition with no inequality information. Participants completed the same mock hiring task used in Study 1 and, as in Study 1, all

participants completed the version of the task in which minority (vs. White) students were disproportionately less likely to be hired despite equivalent academic credentials.

Intervention.

Reframing Intervention. Much of the language around rising inequality and why people should remedy it is grounded in moral concerns about harm (e.g., inequality causes direct suffering) and fairness (e.g., inequality is unfair). Prior work demonstrates that the political right differs from the political left in terms of the relative weight assigned to these moral values and, specifically, that the political right places more weight on ingroup loyalty, authority, and sanctity (Graham, Haidt, & Nosek, 2009). Motivated by prior work demonstrating that the political right can be persuaded by morally reframed arguments (Feinberg & Willer, 2019), here we aimed to reframe inequality in terms of resonant moral values. Specifically, our reframing intervention included moral arguments grounded in concerns about the American ingroup (e.g., inequality negatively affects the country as a whole by reducing growth, productivity, and innovation) and about sanctity (e.g., inequality challenges fundamental values and puts the American dream at stake). Participants in the reframing condition read this information prior to completing the mock hiring task. The full text of the reframing condition was as follows:

> "Inequity poses a major threat to the United States and challenges our most fundamental values. The essence of the American dream is that life should be better and richer and fuller for everyone, with opportunity for each according to their ability or achievement regardless of social class or circumstances at birth. That anyone, irrespective of zip code, income, race, ethnicity, occupation, etc. can pull themselves up by their bootstraps by working hard.
>
> Instead of America being the land of opportunity, there is a lack of opportunity today. It is becoming harder and harder for people to pull themselves up through hard work. Meritocracy, or the idea that people should be rewarded based on the amount of work they put in, is no longer working for all Americans.
>
> Some Americans are experiencing even greater levels of this inequity than others. Relative to White Americans, racial minority Americans are not equally rewarded for their efforts; Black Americans with the same qualifications (same level of education and experience) who do the same job as White Americans make only 87% of what White Americans make.

> This inequity hurts society as a whole; it leads to lower growth, lower productivity, less innovation, and less market efficiency. The United States' most valuable asset – its people and their ability to innovate – is being wasted. Many are not being allowed to realize their full potential, which impedes the engines of America's growth.
>
> Most importantly, America's inequity is undermining its values and identity. Inherent in the national ethos of the United States is a set of ideals: democracy, liberty, opportunity, and freedom. Freedom includes the opportunity for success, achieved through hard work. With inequity reaching such extremes, the sanctity of the American dream is at stake; America is no longer a country with 'justice for all'. To preserve America's identity as the land of opportunity, it's important for all of us to call out instances of unequal treatment."

Measures.

All measures were identical to Study 1.

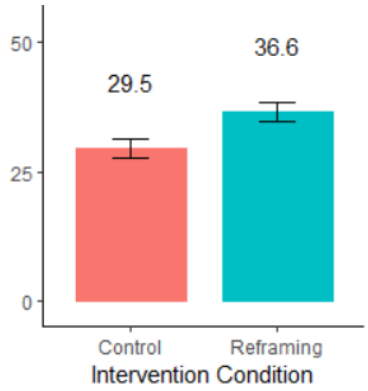
Analytical Strategy

As in Study 1, our central goal in Study 2 was to increase the likelihood that those on the political right attended to and were accurately calibrated to the extent of unequal treatment faced by racial minority students. Thus, as preregistered, in our focal analyses we include only those participants who identified as Republican/lean Republican (SDO: $M = 3.41$; economic conservatism: $M = 5.61$; social conservatism: $M = 5.18$). In addition, we excluded participants who missed either of two attention checks embedded in the survey. Analyses for dichotomous measures are conducted using binomial logistic regression. Analyses for continuous measures are conducted using ordinary least squares regression. Because the procedure was identical for both samples, we again mega-analyze the pooled raw data to examine each of our focal DVs (attention to inequality, accurate detection, underestimation, overestimation). After examining our central DVs, we again examine whether the intervention directly (or indirectly, via attention to and/or accurate calibrations of the extent of inequality) affected participants' willingness to take action against inequality by investigating the hiring process.

Results

Attention to Inequality. Analyzing the pooled raw data from Samples 2a and 2b, we observe a significant positive effect of the reframing intervention on the likelihood that participants reported noticing unequal treatment against minority students, OR = 1.381, 95% [1.096, 1.741], p = .006, suggesting that relative to those in a control condition, participants who read reframed information about social inequality were more likely to spontaneously call out unequal treatment affecting disadvantaged group members (Figure 14).

Figure 14 % of Participants Reporting Noticing Inequality as a Function of Experimental Condition



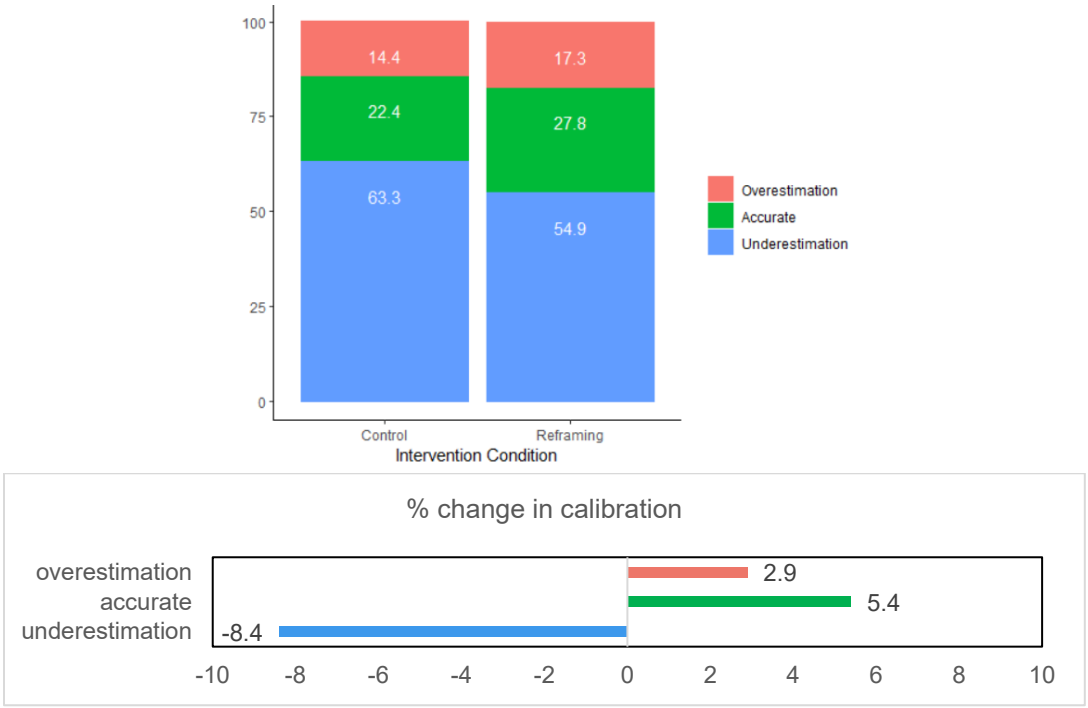
Note. % of participants noticing bias as a function of experimental condition. Differences in noticing are significant at $p < .01$ level. Graph error bars reflect standard error of the mean.

Accurate Detection. A mega-analysis of both samples reveals a significant positive effect of the reframing intervention on the likelihood that participants accurately detected the extent of unequal treatment faced by minority applicants, OR = 1.332, 95% [1.036, 1.712], p = .03. Thus, in addition to increasing the likelihood that those on the political right reported noticing inequality, the intervention also seems to have increased the proclivity to *accurately* detect the extent of inequality, by a factor of over 5 percentage points (Figure 15).

Underestimation. When examining participant likelihood of underestimating the extent of inequality experienced by racial minority applicants, a mega-analysis reveals a significant

negative effect of the reframing intervention (vs. control) on the likelihood that participants underestimated the extent of inequality, OR = .709, 95% [.569, .885], p = .002. Thus, in addition to increasing the overall likelihood that those on the political right accurately detected the extent of inequality, the intervention (vs. control) seems also to have decreased the likelihood that participants selected a pie chart which underestimated the extent of inequality (Figure 15).

Figure 15 % of Participants Underestimating, Accurately Detecting, and Overestimating Bias as a Function of Experimental Condition



Note. Top panel: % of participants underestimating, accurately detecting, and overestimating the extent of inequality by experimental condition. Differences between accurate estimation and underestimation are significant at the $p <$ .05 level. Bottom panel: % change between the intervention and control.

Overestimation. Finally, pooling the raw data from Samples 2a and 2b to predict participant likelihood of overestimating inequality reveals no significant effect of the intervention on overestimation, OR = 1.246, 95% [.925, 1.678], p = .15 (Figure 15).

Taken together, the reframing intervention (vs. control) seems to have increased the likelihood that those on the political right called out instances of unequal treatment faced by

societally disadvantaged groups and increased their proclivity to accurately detect the extent of inequality, specifically by decreasing their likelihood of underestimating the extent of inequality while having no simultaneous effect on their proclivity to overestimate.

Desire to Investigate. Using ordinary least squares regression, we examined whether the reframing intervention *directly* affected participants' desire to investigate the company hiring practices. Analyses pooling the data from Samples 2a and 2b reveal a positive but nonsignificant effect of the reframing intervention on the desire to investigate, $b = .068$, 95% CI [-.104, .241], $p = .44$, which suggests that the intervention did not directly affect participants' desire to investigate Connection Consulting.

We next examined whether the reframing intervention affected the desire to investigate indirectly via our key dependent measures (attention to inequality, accurate detection, underestimation, and overestimation). We conducted separate mediation models with task condition as a predictor, each key dependent measure as a mediator, and desire to investigate as the outcome of interest. We observe significant indirect effects of the reframing intervention on the desire to investigate via our measure of noticing unequal treatment, $b = .093$, SE = .034, 95% [.026, .160] and via our measure of underestimating the extent of unequal treatment, $b = .069$, SE = .024, 95% [.023, .115]. We observe a marginally significant indirect effect of the reframing intervention on the desire to investigate via accurate assessments of the extent of inequality, $b = .018$, SE = .010, 95% [-.001, .038] and no indirect effect of participant proclivity to overestimate, $b = .030$, SE = .021, 95% [-.011, .070].

A parallel mediation model which includes both attention to inequality and underestimation as separate mediators reveals unique indirect effects for both measures (attention: $b = .082$, 95% [.022, .141]; underestimation: $b = .041$, 95% [.011, .070]). Thus, while

the reframing intervention did not directly affect the desire to investigate, it does seem to have indirectly affected participants' willingness to investigate the biased hiring practices by simultaneously increasing the likelihood that participants noticed the unequal treatment and by decreasing their likelihood of underestimating the extent of unequal treatment.

Discussion

Motivated by prior research demonstrating the power of morally reframed arguments (see Feinberg & Willer, 2019 for a review), in Study 2 we examined how reframing inequality in terms of conservative moral values (e.g., ingroup/loyalty, purity/sanctity) affected the likelihood that individuals affiliated with the political right attended to, accurately calibrated the extent of, and were willing to take action against unequal treatment affecting racial minority applicants in a mock hiring task.

Mega-analyzing data from two distinct samples of political conservatives revealed that, relative to a control, individuals who read morally reframed arguments about inequality were both more attentive to *and* more accurate at detecting inequality in the subsequent hiring task. Specifically, we found that study participants exposed to the reframing intervention (vs. a control) were significantly more likely to report noticing unequal treatment against minority applicants when asked to indicate anything that stood out to them about the task (corresponding to intervention goal #1). Thus, as in Study 1 where we found that participants were more likely to report noticing unequal treatment after reading information about racial economic inequality, here we find consistent effects as morally reframed information about inequality also seems to increase subjectively reported attention to it.

In Study 1 we further observed that the information intervention had no effect on the likelihood that those on the political right accurately detected the extent of unequal treatment.

Moreover, when examining the direction of inaccurate perceptions (and specifically the extent of overestimation and underestimation), in Study 1, we found that the information intervention increased the likelihood that participants overestimated the extent of unequal treatment but came with no simultaneous decrease in their proclivity to underestimate. In contrast, in Study 2, we find that participants who read reframed information about inequality, relative to those in a control condition, were significantly more accurate at detecting the extent of inequality affecting racial minority applicants. Moreover, when examining patterns of inaccuracy, we find that the reframing intervention seems to have decreased participant likelihood of underestimating the extent of inequality without having a corresponding effect on their proclivity to overestimate. In sum, then, the reframing intervention seems to have (1) increased accurate detection of unequal treatment by specifically (2) decreasing the likelihood of underestimating unequal treatment, while (3) having no simultaneous effect on the likelihood of overestimating the extent of unequal treatment (corresponding to intervention goal #2). This pattern of errors is more consistent with being accurately calibrated to existing levels of inequality, rather than ignoring the extent of inequality or seeing more inequality than is actually present.

Finally, while we observe no direct effect of the reframing intervention (vs. a control) on participant's willingness to investigate Connection Consulting's hiring practices, we do find evidence that the reframing intervention indirectly affects the desire to investigate. Specifically, we find that the reframing intervention increased the likelihood that participants reported noticing unequal treatment against minority students and this increase in attention affected their subsequent willingness to investigate the potentially biased organizational hiring practices. Similarly, we find that the reframing intervention (vs. a control) decreased the likelihood that

participants underestimated the extent of inequality and this decrease in underestimation uniquely affected subsequent willingness to investigate Connection Consulting.

General Discussion

Inequality between groups is a persistent feature of social life and addressing inequality is a major goal of governments and organizations around the world (United Nations, 2022). Particular groups, such as poor people, racial minorities, and women, are disproportionately affected by inequality relative to their rich, White, and male counterparts (Chancel, Piketty, Saez, & Zucman, 2022; World Economic Forum, 2019; Inequality.org, 2021). Despite the existence of inequality, there are stark disagreements, often along ideological lines, about its extent, about who is most affected by it, and about what, if anything, should be done. Efforts to remedy inequality require an agreed upon – and ideally, an accurate – understanding of its extent.

Prior work demonstrates that people's ideological motivations, and specifically, their beliefs about the desirability of inequality, affect whether they notice and are accurately calibrated to the extent of inequality affecting societally disadvantaged groups in the world around them. Relative to social egalitarians (and the wider political left), who have an ideological motivation to attenuate social inequality, anti-egalitarians (and the wider political right), who are generally more tolerant of group-based inequality, are less likely to notice and accurately detect the extent of inequality when disadvantaged groups bear the brunt (Waldfogel, Sheehy-Skeffington, Hauser, Ho, & Kteily, 2021). With these underlying ideological differences in mind, the present work examined two strategies aimed at nudging attention to and increasing accurate detection of social inequalities affecting societally disadvantaged groups among samples of participants affiliated with the political right.

Across two studies, the present chapter examined how (1) factual information about racial inequality and (2) morally reframed information about inequality affected subsequent attention to and accurate calibrations of the extent of unequal treatment affecting racial minorities. We reasoned that if the political right fails to notice and accurately assess the extent of inequality in the world around them because they are simply unaware, providing factual information about existing inequalities might effectively prompt attention to and re-calibrate inaccurate perceptions of social inequality. If, however, the political right fails to notice inequality because evidence of inequality is ignored or perceptually downplayed, simply providing information about inequality may be ineffective in prompting attention to and accurate detection of its extent. If this is the case, we reasoned that reframing inequality in terms of moral values endorsed by the political right might render inequality more motivationally relevant, thus prompting greater attunement to cues of inequality affecting disadvantaged groups.

In Study 1, across two samples of Republicans/lean Republicans, we tested whether factual information about racial economic inequality prompted greater attention to and accurate perceptions of inequality in a mock hiring task where racial minority (vs. White) applicants were disproportionately disadvantaged. We found that while information about racial economic inequality did increase the likelihood that participants reported noticing unequal treatment facing minority applicants, it did not increase the likelihood that they were accurately calibrated to the extent of bias in hiring. Based on the 'baseline' proclivity of the political right to underestimate the extent of inequality affecting disadvantaged groups, we might deem an intervention 'effective' – even if it does not increase accuracy – should it reduce the likelihood that those on the political right (who tend to downplay the extent of inequality) underestimate the extent of

inequality. We found, however, that the information intervention did not affect the proclivity to underestimate, but instead made participants *more* likely to overestimate the extent of inequality.

In Study 2, again across two samples of individuals affiliated with the political right, we tested how reframing inequality in terms of conservative moral values (e.g., ingroup loyalty, sanctity) affected subsequent attention to and accurate calibrations of the extent of unequal treatment affecting disadvantaged groups. We found that, relative to those in a control condition, participants who read reframed information about racial inequality were subsequently (1) more likely to report noticing biased hiring practices which affected racial minority applicants and were (2) more accurate at detecting the extent of unequal treatment, specifically because they became (3) less likely to underestimate its extent. Moreover, we found that this increase in accuracy and decrease in underestimation came with no simultaneous increase in overestimation.

The present work contributes to the growing literature on information-based strategies aimed at correcting (mis)perceptions of inequality (Callaghan, Harouni, Dupree, Kraus, & Richeson, 2021; McCall, Burk, Laperriere, & Richeson, 2017; Onyeador, et al., 2020) by testing the effects of two distinct interventions on attention to and accurate detection of inequality. A key limitation of this prior work is that it tests intervention strategies among samples of participants that skew liberal (with the exception of McCall et al, 2017 Study 3 which intentionally recruits a nationally – and politically – representative sample), which makes it difficult to generalize past findings to the full ideological spectrum. Moving beyond this prior work, in both studies in the present paper, we intentionally recruit politically conservative participant samples to examine how those on the political right respond to these interventions.

In addition, the present work contributes to the literature on moral reframing by extending this technique to the domain of social inequality – a highly divisive political topic.

While prior tests of moral reframing have demonstrated its efficacy in domains ranging from environmental attitudes and behavior (Feinberg & Willer, 2013) to support for political candidates (Voelkel & Feinberg, 2018), tests of moral reframing in the domain of societal inequality are scant. The present work provides promising evidence that reframed information about inequality might effectively prompt those on the political right to notice it and, even more exciting, to become more accurate at detecting – and less likely to underestimate – its extent.

Theoretically, the present research illuminates a clear distinction between attention to inequality and accurate detection of the extent of inequality. In other words, noticing the existence of inequality is *not* equivalent to being accurately calibrated to its extent. In Study 1, had we only looked at whether the information intervention prompted greater attention to inequality (in terms of the overall likelihood that participants reported noticing unequal treatment), we may have deemed the intervention effective. An examination of (in)accurate perceptions of inequality, however, reveals that the information treatment did not affect accurate detection of its extent. Parsing out the two sets of dependent measures reveals important nuance.

Limitations and Future Directions

The present work is not without its limitations. Here, we only examine (or report)[12] the results of the interventions in contexts where disadvantaged group members face unequal treatment. Thus, we cannot know for certain how these interventions would affect attention to and accurate detection of unequal treatment in instances where advantaged group members (e.g., the rich, males, Whites) are affected. It's possible, for example, that an intervention which

[12] Of note, in one of our samples (Study 1 Sample 1b) we did include an additional condition which examined the effects of the information treatment (vs. control) on attention to and accurate perceptions of unequal treatment when the advantaged group (e.g., White applicants) were disproportionately less likely to be hired. Results of this condition are reported in Appendix B. To ease interpretation (and because we did not include this condition in Sample 1a or in Study 2), we do not focus on those results here.

provides information about inequality affecting racial minorities makes people *less* attuned to unequal treatment in instances where advantaged groups (e.g., White students) are affected. Before widely deploying any intervention, future research should examine more generally how evidence of inequality affecting one group (e.g., Black Americans) influences attention to and accurate calibrations of the extent of inequality affecting other groups (e.g., Native Americans, White Americans).

In Study 2, because we were primarily interested in increasing the likelihood that individuals more supportive of social inequality noticed and accurately detected the extent of unequal treatment faced by disadvantaged groups, we examined the effects of a reframing intervention *only* for those on the political right. Future work should examine how information about inequality grounded in concerns held by the political right affects subsequent attention to – and accurate calibrations of – unequal treatment among those on the political left. It's possible that the political left would be unaffected by this intervention; perhaps they find arguments – which align with a conservative mindset – unconvincing and/or perhaps they already care enough about inequality that the arguments for attenuating inequality are superfluous. Perhaps conservative arguments which problematize inequality elicit reactance among those on the political left, making them *less* likely to notice inequality because they reject the conservative premises. Prior work has generally found null effects on the untargeted group (i.e., the group that already supports the policy) in moral reframing work (see Feinberg & Willer, 2019), but it's possible that reframed information about inequality could backfire for political liberals and thus, future work should test this possibility.

The present work is constrained in its ability to generalize to other forms of inequality; here, we tested these interventions only in the domain of racial bias in an artificial mock hiring

task. While there is good reason to believe that these results would extend to other instances of inequality in which traditionally societally disadvantaged groups (e.g., women, poor people, etc.) are affected, future work should consider and explicitly test these intervention strategies in additional inequality-relevant domains. Moreover, the present work is limited in its external validity; the mock hiring task paradigm allows us to precisely control the amount of unequal treatment and to therefore test for whether people are accurately calibrated to the extent of inequality, however, inequality in the world around us is likely to be more complicated and ambiguous. An important task for future research is to examine the effect of the interventions tested here on attention to – and accurate detection of – inequality in people's everyday lives.

In the current research, we were primarily interested in examining *if* these intervention strategies would be effective in prompting those on the political right to notice and accurately detect the extent of unequal treatment against racial minorities. And while the interventions do seem to have increased attention (Studies 1 and 2) and accuracy (Study 2), we do not have a clear answer as to *why* these strategies were effective. Future work should more directly test the potential mechanisms at play. For example, it's possible that value-consistent reframed information about inequality prompted participants to temporarily adopt a goal of social egalitarianism, which increased their attunement to evidence of inequality (Moskowitz, 2002).

In our studies, the desire to investigate Connection Consulting's hiring practices served as a proxy for downstream outcomes. While neither intervention directly affected the proclivity for those on the political right to want to investigate the biased hiring practices, we do find evidence that both the information-based (Study 1) and the reframing (Study 2) interventions indirectly increased participant willingness to investigate the hiring practices by increasing their proclivity to subjectively report noticing the unequal treatment. The reframing intervention

(Study 2) also seems to have indirectly increased the desire to investigate via decreases in the overall likelihood of underestimating the extent of inequality. An important task for future work is to more thoroughly consider how these interventions may – whether directly or indirectly – affect downstream policy preferences (e.g., government redistribution, affirmative action) and behaviors (e.g., voting).

Social inequality is a defining issue of the current age, yet efforts to address it will remain stifled without a shared – and accurate – understanding of the extent of inequality in the world around us. Ultimately, in testing strategies to increase attention to and recalibrate inaccurate perceptions of the extent of inequality and in considering the role of ideological motives in shaping the efficacy of such strategies, the hope is to bring political partisans in greater accord so that we can work to address key societal issues.

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Appendix A

Chapter 2 Supplemental Information Appendix

Table of Contents

Preregistration Information

Links

Study 2 preregistration - https://aspredicted.org/blind.php?x=4kj9pa
Study 3a preregistration – https://aspredicted.org/blind.php?x=zu8xv8
Study 3b preregistration - https://aspredicted.org/blind.php?x=ze84z6
Study 4 preregistration – https://aspredicted.org/blind.php?x=dp74ry
Study 5 preregistration - http://aspredicted.org/blind.php?x=m8gc9w

Deviation

There were no deviations from the preregistration for any of the studies, with the exception of one deviation in Study 4. In the Study 4 preregistration, we said we that we would include both dichotomous and continuous measures of (in)accuracy, but that we would prioritize our measures of 'degree' of inaccuracy over the dichotomous measures. Upon reflection, however, we later realized that, because of the structure of our task, it would be difficult to make clear inferences from one of the continuous measures of accuracy (we include details as to why in SI Appendix Section 5.5 below). We therefore decided to prioritize the dichotomous measures in the main text and included the relevant continuous measures—clarifying what inferences are and are not appropriate from these measures—in SI Appendix Section 5.6. We note that the conclusions drawn from the relevant continuous measures were consistent with the conclusions drawn from the dichotomous measures (namely, egalitarianism vs. anti-egalitarianism was associated with greater accuracy in the condition where women spoke less than men, but not in the condition where men spoke less than women).

Study 1

Images

Study 1a Images

Figure A1: The first 3 images are inequality-relevant images, and the second 3 are distractors.

Study 1b and 1c Images

Figure A2: The first 3 images are inequality-relevant, the second 3 are distractor images (here, referencing a separate social issue, environmental damage).

Study 1d Images

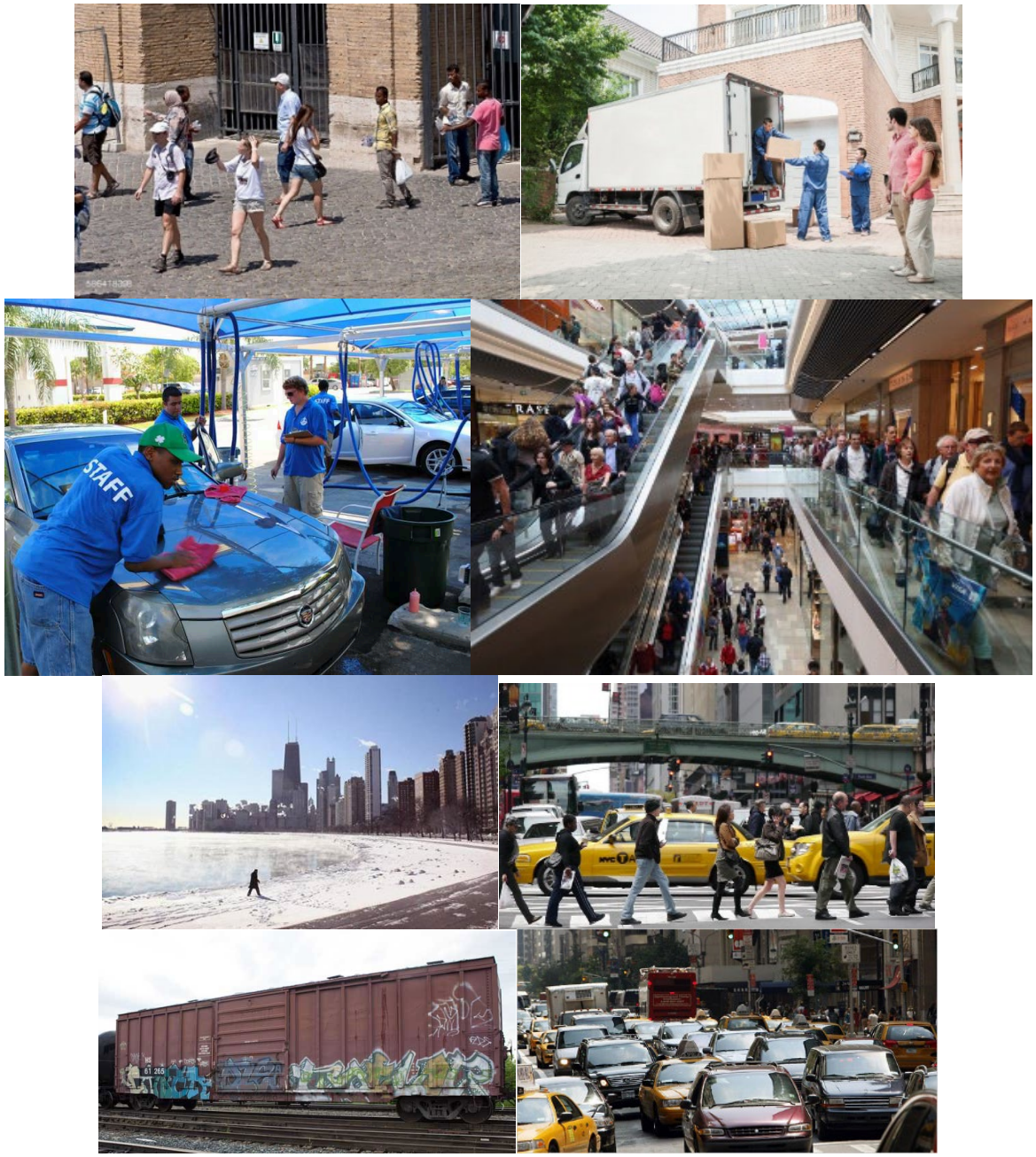


Figure A3: The first 5 images are inequality-relevant; the second five are distractors.

Study 1e Images

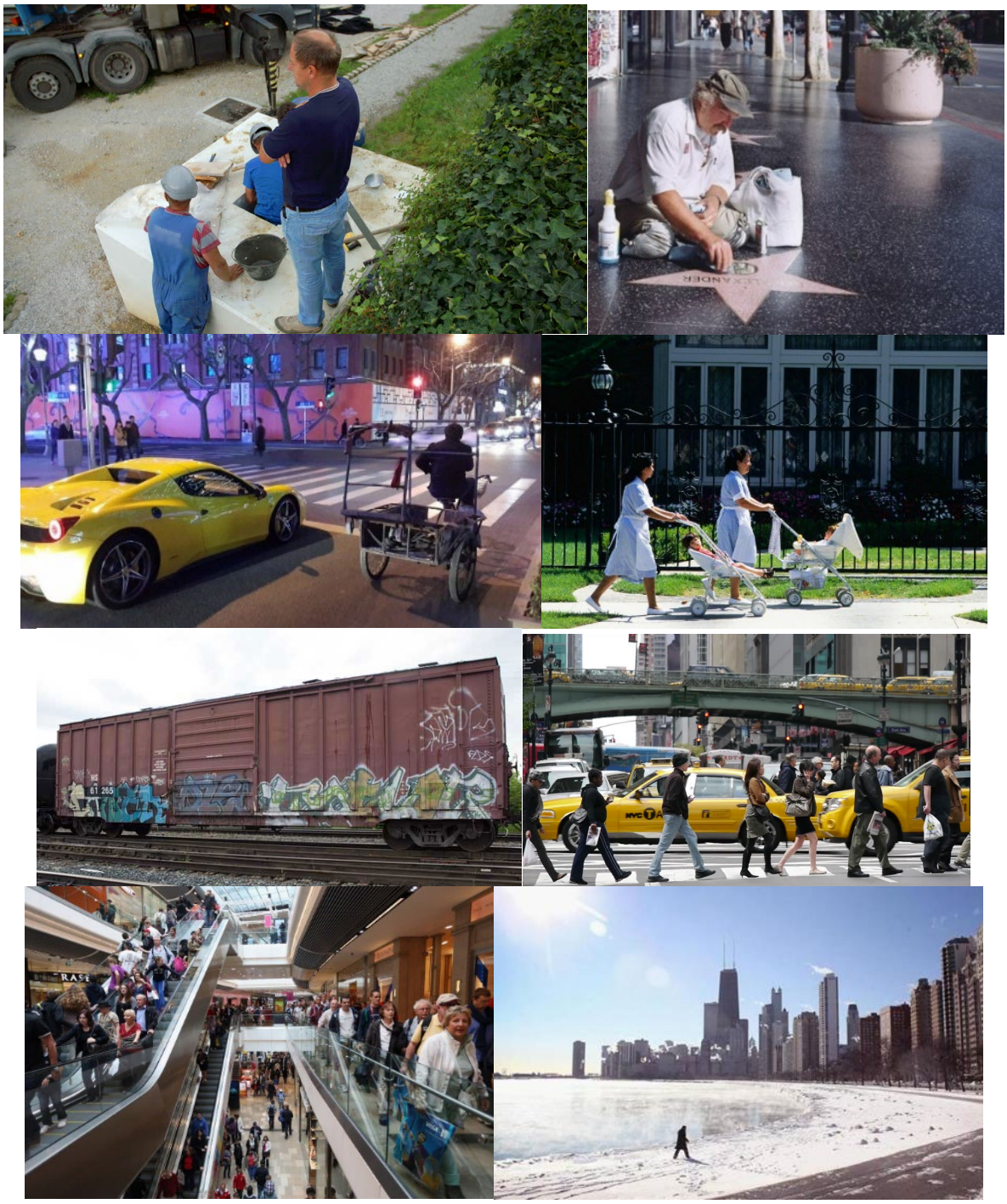


Figure A4: The first 4 images are inequality-relevant; next 4 are distractors.

Attrition Analyses (Sample 1e)

We conducted attrition analyses to compare those who completed only wave 1 with those who completed both waves. The two sets of participants did not significantly differ in SDO scores, F (1, 569) = 2.65, $p = .10$ or gender, χ^2 (2, $N = 552$) = 2.25, $p = .13$. The only exception was age, where we observed that participants who completed both waves were slightly older than those who completed only wave 1, F (1, 545) = 4.69, $p = .03$ (controlling for age did not affect our main findings). Thus, those completing both waves did not differ markedly from those who completed only wave 1.

Supplemental Results

Table A1: Correlation between SDO and Inequality Cues by Sample

	Direct Inequality	Indirect Inequality	High Status Cues	Low Status Cues
Sample 1a	-.10	-.07	-.05	-.13†
Sample 1b	-.13**	-.10*	-.01	-.11*
Sample 1c	.03	.01	.01	-.02
Sample 1d	-.03	-.09*	-.06	-.11**
Sample 1e – Concrete Details	-.07	-.19**	-.14†	-.20**
Sample 1e – General Impression	-.19*	-.06	-.07	-.08

Note: † p < .1. * p <.05. ** p < .01.

We reasoned in Study 1e that the link between SDO and Direct Inequality might be somewhat stronger when the task instructions asked for participants' general impressions of the images (vs. requesting three concrete details that stood out) and that the reverse might be true for the relationship between SDO and Indirect Inequality. The patterns we observed were directionally consistent with this, but the interactions between SDO and task instruction condition (i.e., concrete details vs. general impressions) were not statistically significant (Direct Inequality: $b = -.09$, $p = .24$; Indirect Inequality: $b = .10$, $p = .20$).

Forest Plots

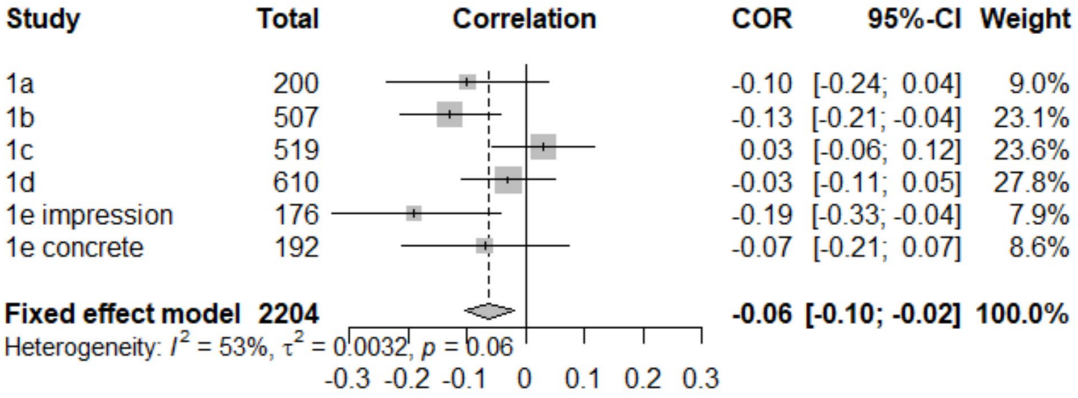


Figure A5. Forest plot of *direct inequality* fixed effects meta-analysis

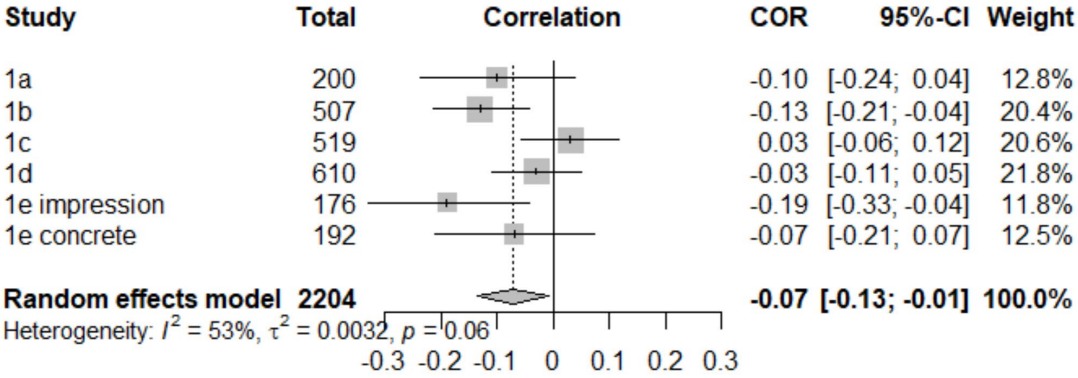


Figure A6. Forest plot of *direct inequality* random effects meta-analysis

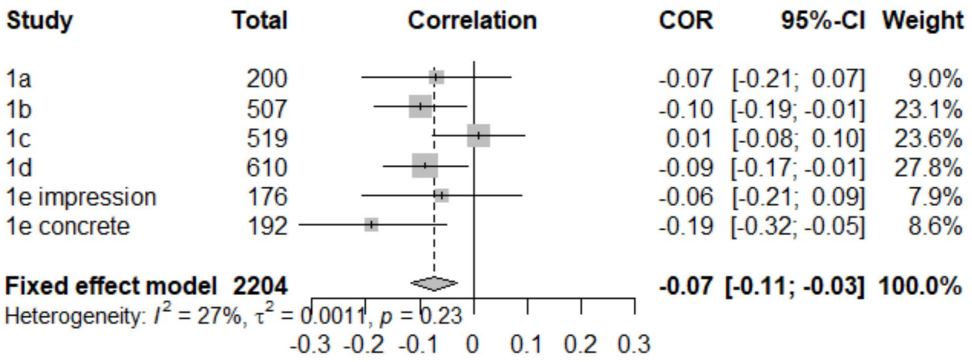


Figure A7. Forest plot of *indirect inequality* fixed effects meta-analysis

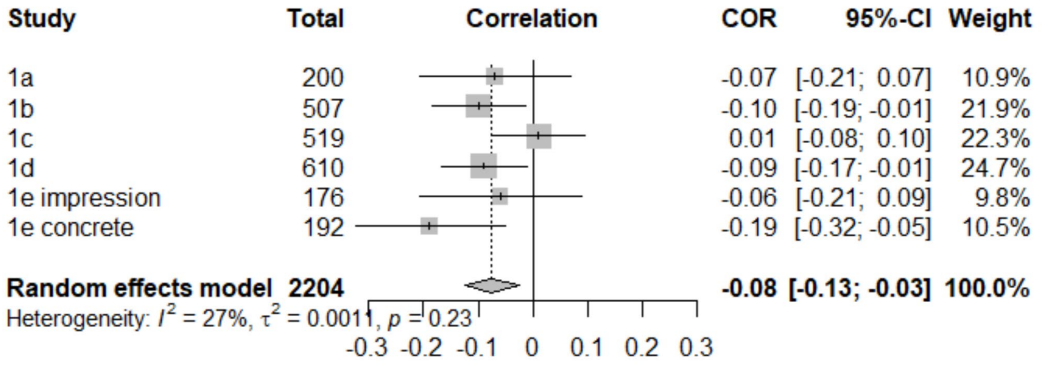


Figure A8. Forest plot of *indirect inequality* random effects meta-analysis

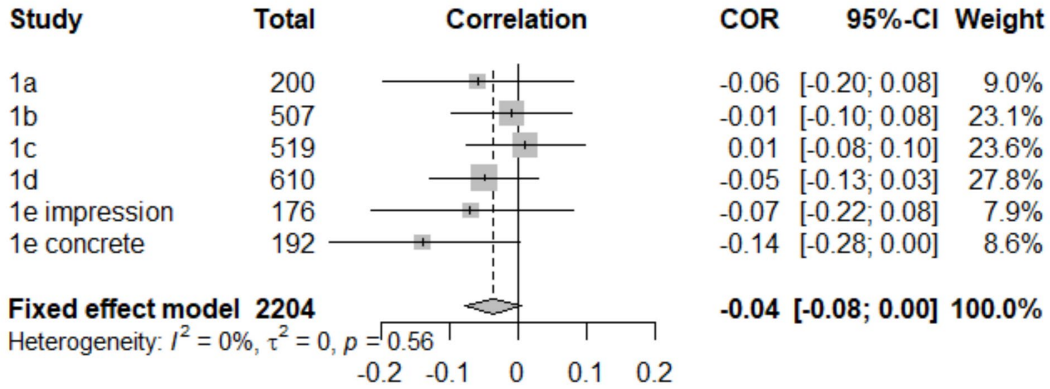


Figure A9. Forest plot of *high-status cues* fixed effects meta-analysis

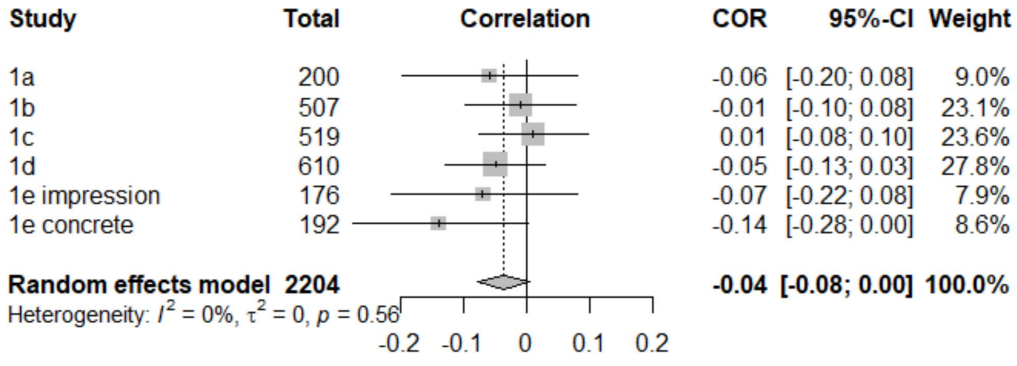


Figure A10. Forest plot of *high-status cues* random effects meta-analysis

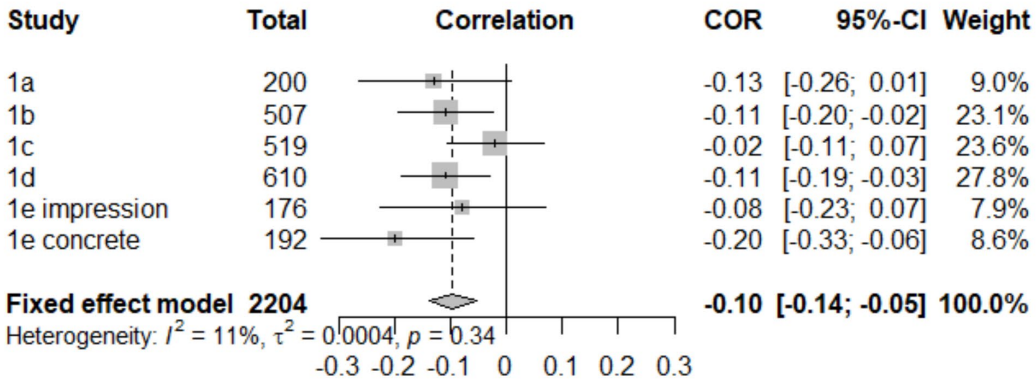


Figure A11. Forest plot of *low-status cues* fixed effects meta-analysis

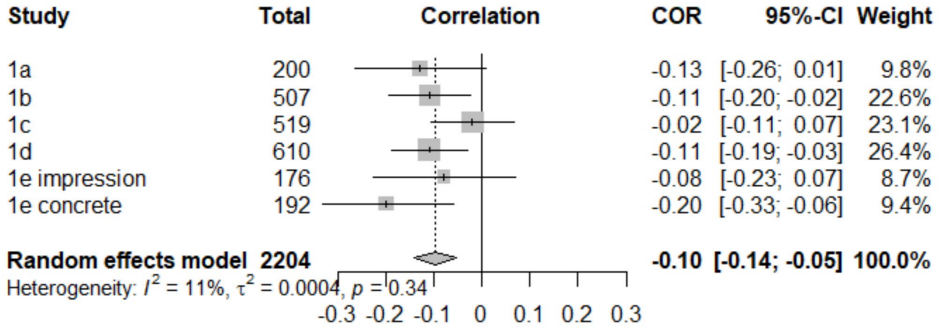


Figure A12. Forest plot of *low-status cues* random effects meta-analysis

Study 2

Stimuli Examples

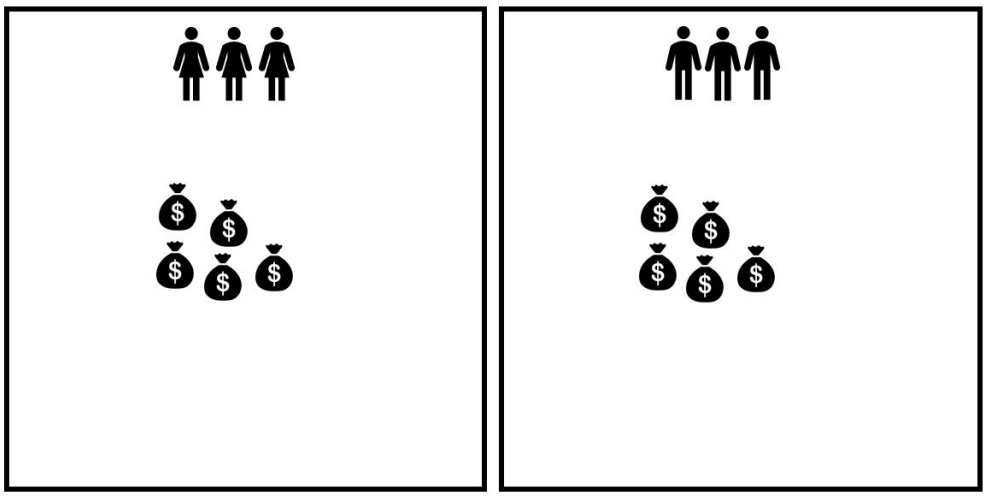


Figure A13. Example of an equal trial with ‘same’ structural base.

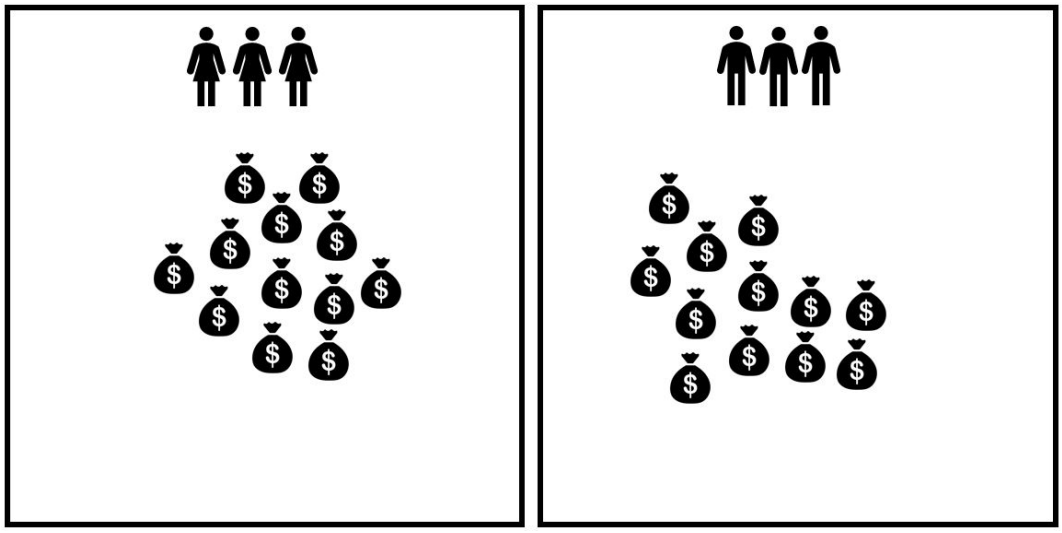


Figure A14. Example of an equal trial with ‘different’ structural base

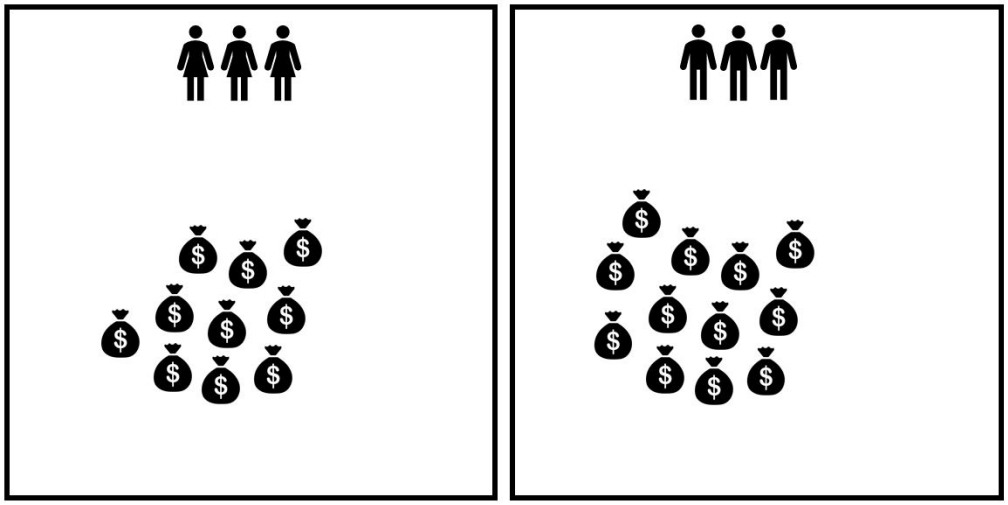


Figure A15. Example of an unequal trial with ‘same’ structural base with difference under 20%

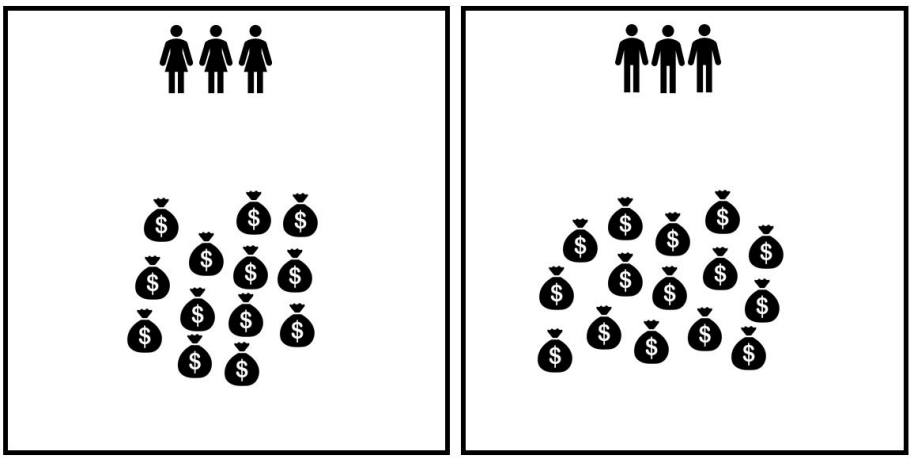


Figure A16. Example of an unequal trial with ‘different’ structural base with difference under 20%

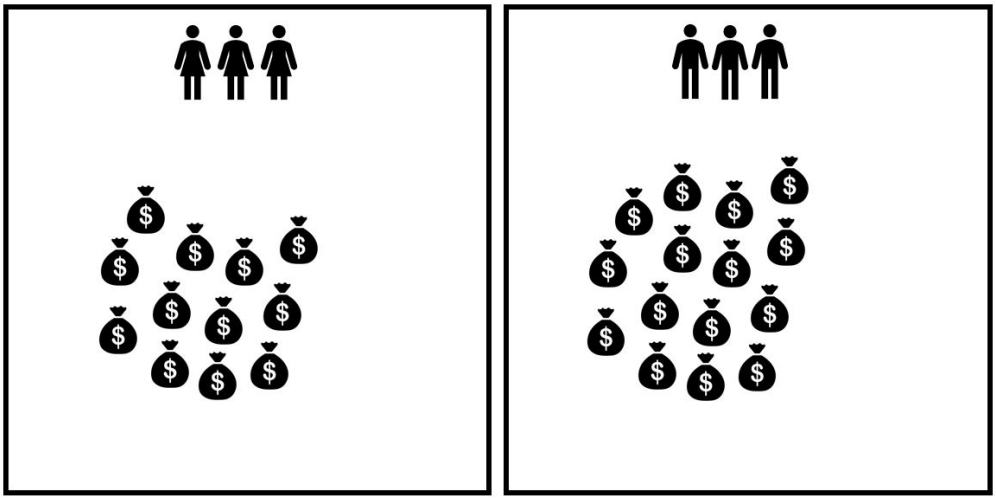


Figure A17. Example of an unequal trial with ‘same’ structural base with difference between 20-30%

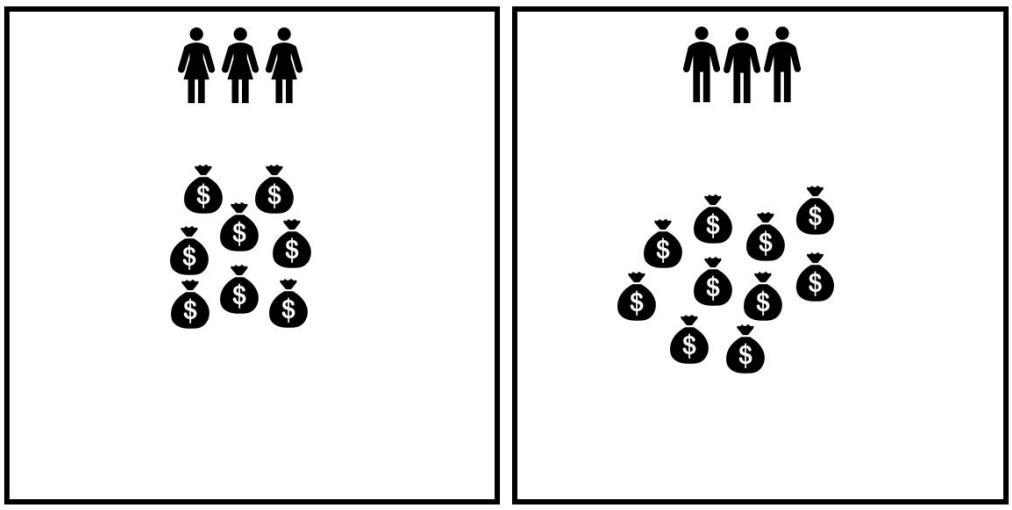


Figure A18. Example of an unequal trial with 'different' structural base with difference between 20-30%

Supplemental Results

In the version of the task where participants were asked to hit the space bar when the two arrays were equal and refrain from hitting any keys otherwise, we found that SDO was negatively correlated with *d'*, however, this relationship was not significant, r = -.06, p = .14. The relationship between SDO and *c* was not significant, r = - .02, p = .54.

In the version of the task where participants were asked to hit the space bar when the two arrays were *not* equal to one another and refrain from hitting any keys otherwise, we found a significant negative relationship between SDO and *d'*, r = -.11, p = .006. The relationship between SDO and *c* was not significant, r = .01, p = .90.

Study 2 Results for Full Sample (no exclusions)

We obtained consistent results among the full sample of participants (i.e., including those with over 17 consecutive 'Go' responses or 'No Go' responses): The correlation between SDO and *d*' was significant, r = -.10, p < .001, and the correlation between SDO and *c* was not significant, r = .01, p = .65.

Study 3

Pilot Results

We conducted a pilot test with 60 participants to ensure that the vast majority of participants were able to correctly identify the changes in each image. We also measured the average number of times participants viewed the flickering sequence before identifying the change (avg number of repeats).

	% Correctly identifying change	Avg number of repeats (sd)
Inequality image 1	90.0%	6.8 (7.2)
Inequality image 2	96.7%	6.1 (5.5)
Inequality image 3	90.0%	7.4 (6.9)
Inequality image 4	91.7%	5.9 (6.2)
Inequality image 5	95.0%	4.8 (6.3)
Neutral image 1	86.7%	9.1 (8.3)
Neutral image 2	96.7%	4.4 (4.4)
Neutral image 3	96.7%	4.7 (4.6)
Neutral image 4	96.7%	5.1 (4.6)
Neutral image 5	88.3%	9.9 (6.9)

Finally, we asked a separate sample of 58 participants to indicate their agreement with the following, "This image is relevant to inequality" on a scale from 1 (*Strongly Disagree*) to 7 (*Strongly Agree*).

	Avg Relevance to Inequality
Inequality image set	4.4
Neutral image set	2.7

Paired samples t-test, t (57) = 8.16, p <.001

Images (3a)

Figure A19. Inequality image 1

Figure A20. Inequality image 2

Figure A21: Inequality image 3

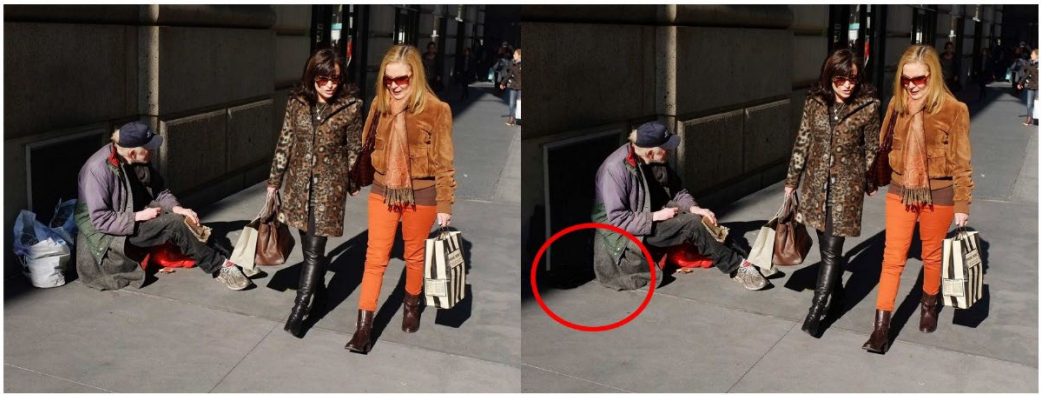


Figure A22: Inequality image 4

Figure A23: Inequality image 5

Figure A24: Neutral image 1

Figure A25: Neutral image 2

Figure A26: Neutral image 3

Figure A27: Neutral image 4

Figure A28: Neutral image 5

Results by Image (3a)

	% Correctly identifying change	Avg number of repeats (sd)
Inequality image 1	95.0%	6.0 (5.8)
Inequality image 2	97.3%	5.4 (4.6)
Inequality image 3	92.5%	6.8 (6.5)
Inequality image 4	98.0%	5.1 (4.2)
Inequality image 5	95.4%	4.6 (5.6)
Neutral image 1	78.1%	10.6 (8.9)
Neutral image 2	97.8%	4.6 (4.4)
Neutral image 3	98.0%	4.3 (4.4)
Neutral image 4	98.5%	5.1 (3.9)
Neutral image 5	83.3%	4 (7.9)

Updated Images (3b)

Figure A29. Updated neutral image 1

Figure A30. Updated neutral image 5

Results by Image (3b)

Image	% Correctly identifying the change	Avg number of repeats (SD)
Inequality 1	94.6%	5.5 (4.5)
Inequality 2	97.3%	4.7 (3.9)
Inequality 3	91.6%	6.0 (5.7)
Inequality 4	97.2%	4.6 (3.7)
Inequality 5	94.6%	4.3 (4.9)
Neutral image 1	97.7%	4.8 (3.9)
Neutral image 2	97.7%	4.2 (3.7)
Neutral image 3	96.8%	3.9 (3.5)
Neutral image 4	98.2%	4.7 (3.4)
Neutral image 5	96.6%	5.3 (4.9)

Study 4

Supplemental Results

Degree Measure of Underestimating Inequality. Next, we examined the degree to which participants underestimated inequality. We observed a marginally significant interaction effect, $b = .11$, $p = .06$, 90% [.01, .22], between SDO and task condition in predicting the degree to which participants underestimated inequality. In Condition 1 (where men spoke more than women), we observed that individuals lower (vs. higher) in SDO were significantly less likely to underestimate the degree of inequality, $b = -.10$, $p = .02$, 95% [-.19, -.02]. In Condition 2 (where women spoke more than men), there were no significant differences between individuals lower vs. higher in SDO in the degree to which they underestimated inequality, $b = .01$, $p = .78$, 95% [-.07, .10]. Individuals both lower (-1SD) and higher (+1SD) in SDO were significantly less likely to underestimate the degree of inequality in Condition 1 (where men spoke more than women)

than in Condition 2 (where women spoke more than men) (lower SDO, b = -.71, p < .001, 95% [-.93, -.49]; higher SDO, b = -.41, p < .001, 95% [-.63, -.19]).

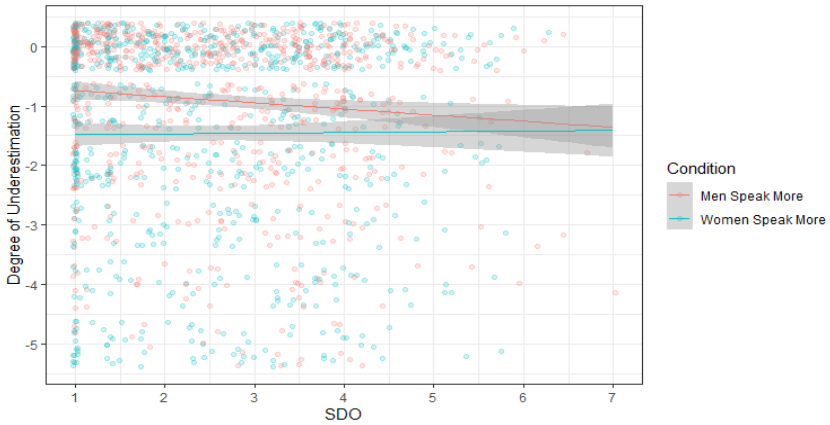


Figure A31. Relationship between SDO and degree of underestimation in identifying speaking-time distribution by gender in condition 1 ('men speak more') versus condition 2 ('women speak more'). Accurate and overestimating responses are scored as 0. Note that data points on this figure are "jittered" via R to aid in visualization (values of this variable are only '0', '-1', '-2', '-3', '-4', or '-5').

Study 5

Stimuli Example

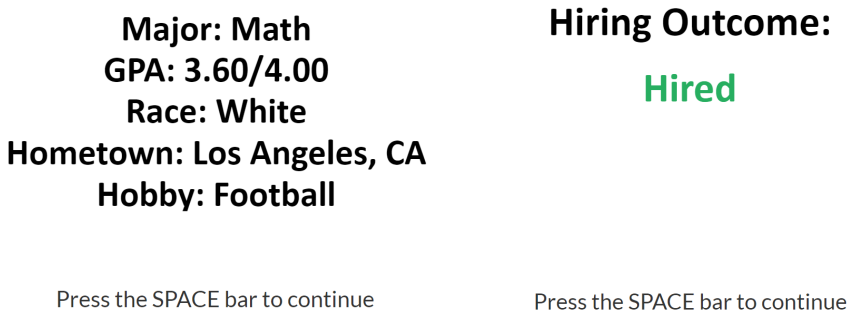


Figure A32. Resume task stimuli example.

Supplemental Measures

Relative Naturalistic Notice Bias. From participants' response to "Please note anything that stood out to you about the hiring process," we also accounted for any (incorrect) mentions of bias against the group that was favored in the participants' condition. On this metric, participants could receive a score of '-1', '0', or '+1'. Participants received a score of 0 if they did not mention bias, a score of +1 if they (correctly) mentioned bias against the category disfavored in

their experimental condition and a score of -1 if they (incorrectly) mentioned bias against the category that was in fact *favored* in their experimental condition.

Absolute Bias Judgments. After giving their open-ended response about their general impression of the task, we directly asked participants to indicate their level of agreement with each of the following statements on a 1 ('Strongly disagree') to 7 ('Strongly agree') scale: (a) 'Connection Consulting was biased against non-White students', and (b) 'Connection Consulting was biased against White students'. For this metric, which we termed 'absolute bias judgments', we examined results looking simply at perceptions of bias against the target who in fact encountered bias in the relevant condition. Thus, for those in Condition 1, our measure of self-reported bias was their rating of statement (a), whereas for those in Condition 2, it was their rating of statement (b).

Relative Bias Judgments. We examined results looking at perceptions of unequal treatment against the target who did encounter inequality in the relevant condition while also incorporating assessments of bias against the target who did not face bias in the relevant condition. Thus, for Condition 1, we calculated *relative bias judgments* by subtracting self-reported ratings of bias against Whites from ratings of bias against non-Whites. For Condition 2, we calculated *relative bias judgements* by subtracting ratings of bias against non-Whites from ratings of bias against Whites.

Supplemental Results

Relative Naturalistic Notice Bias. We observed a significant interaction effect between SDO and task condition in predicting *relative naturalistic notice bias*, $b = 0.09$, $p < .001$, 95% [0.06, 0.13]. In the anti-minority bias condition, we observed the predicted main effect of SDO on *relative naturalistic notice bias*, $b = -0.08$, $p < .001$, 95% [-0.10, -0.05], such that individuals lower (vs. higher) in SDO were significantly less likely to mention anti-minority bias. In the anti-White bias condition, we found a non-significant positive association between SDO and mentioning anti-White bias, $b = 0.02$, $p = .24$, 95% [-0.01, 0.04]. At low levels of SDO (-1SD below the mean), task condition significantly predicted *relative naturalistic notice bias*; individuals lower (vs. higher) in SDO were significantly more likely to mention bias in Condition 1 (anti-minority bias condition) versus Condition 2 (anti-white bias condition), $b = -0.30$, $p < .001$, 95% [-0.38, -0.22]. At high levels of SDO (+1SD above the mean), there was no significant difference between *relative naturalistic notice bias* across the two conditions, $b = -0.03$, $p = .41$, 95% [-0.11, .04].

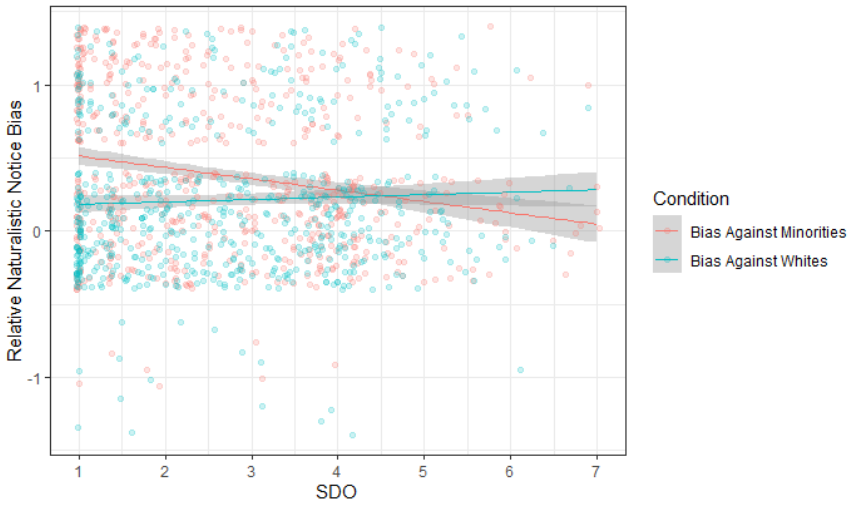


Figure A33. The link between social dominance orientation and relative naturalistic notice bias by condition. Note that data points on this figure are "jittered" via R to aid in visualization (values of this variable are only '-1', '0', or '1').

Absolute Bias Judgments. We next considered responses to our self-report questions about the extent of racial bias in Connection Consulting. For our measure of *absolute bias judgments*, we observed a significant interaction effect between SDO and task condition, b = 0.58, $p < .001$, 95% [0.43, 0.74]. In the anti-minority condition, we observed the predicted main effect of SDO on *absolute bias judgments*, b = -0.25, $p < .001$, 95% [-0.36, -0.14], such that individuals lower (vs. higher) in SDO reported significantly more (anti-minority) bias. In contrast, in the anti-White bias condition (Condition 2), individuals higher (vs. lower) in SDO were significantly more likely to report (here, anti-White) bias, b = 0.33, $p < .001$, 95% [0.22, 0.44]. Examining the interaction another way, individuals lower in SDO (-1SD below mean) reported significantly more bias in Condition 1 (anti-minority bias), b = -1.16, $p < .001$, 95% [-1.47, -0.85] relative to Condition 2, whereas individuals higher in SDO (+1SD above mean) reported significantly more bias in Condition 2 (anti-White bias) relative to Condition 1, b = 0.48, p = .002, 95% [0.17, 0.79].

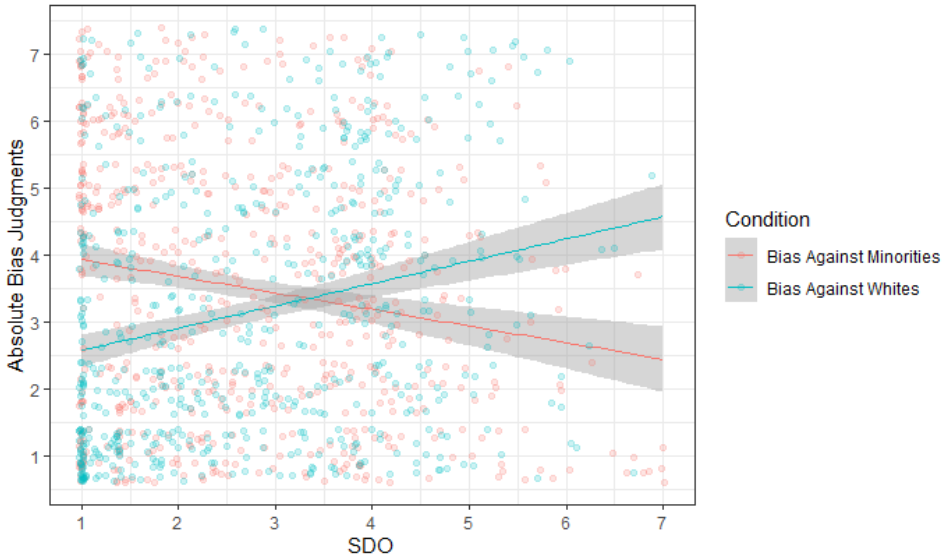


Figure A34. The link between social dominance orientation and absolute bias judgments by condition. Note that data points on this figure are "jittered" via R to aid in visualization.

Relative Bias Judgments. We observed a significant interaction effect between SDO and task condition in predicting *relative bias judgements*, $b = 0.63$, $p < .001$, 95% [0.47, 0.78]. In the condition with bias against minorities (Condition 1), we found the predicted main effect of SDO on relative bias judgements, $b = -0.49$, $p < .001$, 95% [-0.61, -0.38], such that individuals higher (vs. lower) in SDO report significantly less bias. In the condition with bias against Whites (Condition 2), however, we found that individuals higher (vs. lower) in SDO are significantly more likely to report bias, $b = 0.13$, $p = .02$, 95% [0.02, 0.24]. Individuals lower (vs. higher) in SDO (-1SD below mean) reported significantly more bias in Condition 1 (anti-minority bias), $b = -1.29$, $p < .001$, 95% [-1.61, -0.98] relative to Condition 2, whereas individuals higher (vs. lower) in SDO (+1SD above mean) reported significantly more bias in Condition 2 (anti-White bias) relative to Condition 1, $b = 0.47$, $p = .004$, 95% [0.15, 0.79].

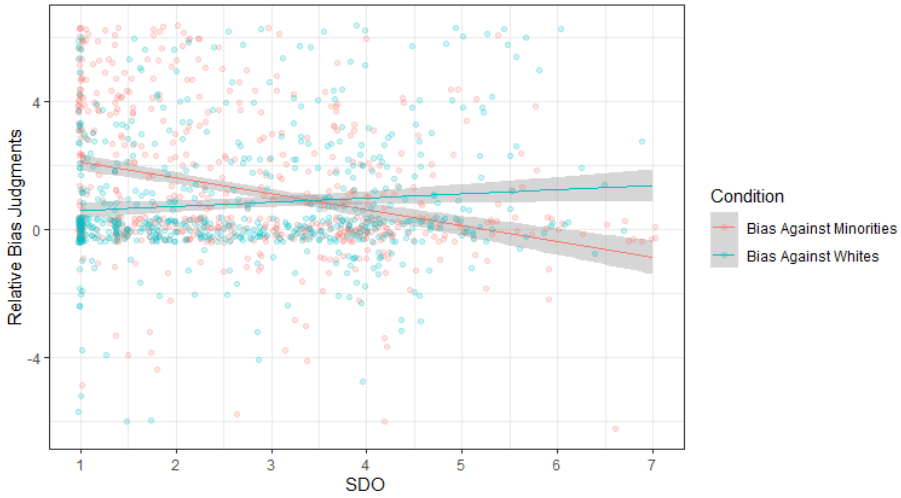


Figure A35. Link between social dominance orientation and relative bias judgements across condition. Note that data points on this figure are "jittered" via R to aid in visualization.

Relative Naturalistic Notice Bias. We entered SDO as the predictor, *relative naturalistic notice bias* as the mediator, and desire to investigate as the outcome measure, with bias condition as a moderator of each of the a, b, and c paths. In the anti-minority bias condition, there was a significant negative indirect effect of SDO on desire to investigate via *relative naturalistic notice bias*, b = -.10, SE = .02, 95% [-.14, -.06]. In contrast, in the anti-White bias condition, there was no significant indirect effect of SDO on desire to investigate via *relative naturalistic notice bias*, b = .02, SE = .01, 95% [-.01, .04]. For individuals lower in SDO (-1 SD below mean), the indirect effect of task condition on desire to investigate via *relative naturalistic notice bias* was significantly negative, b = -.37, SE = .06, 95% [-.50, -.25]. For individuals higher in SDO (+1 SD above mean), the indirect effect of task condition on desire to investigate via *relative naturalistic notice bias* was not significant, b = -.03, SE = .04, 95% [-.11, .05].

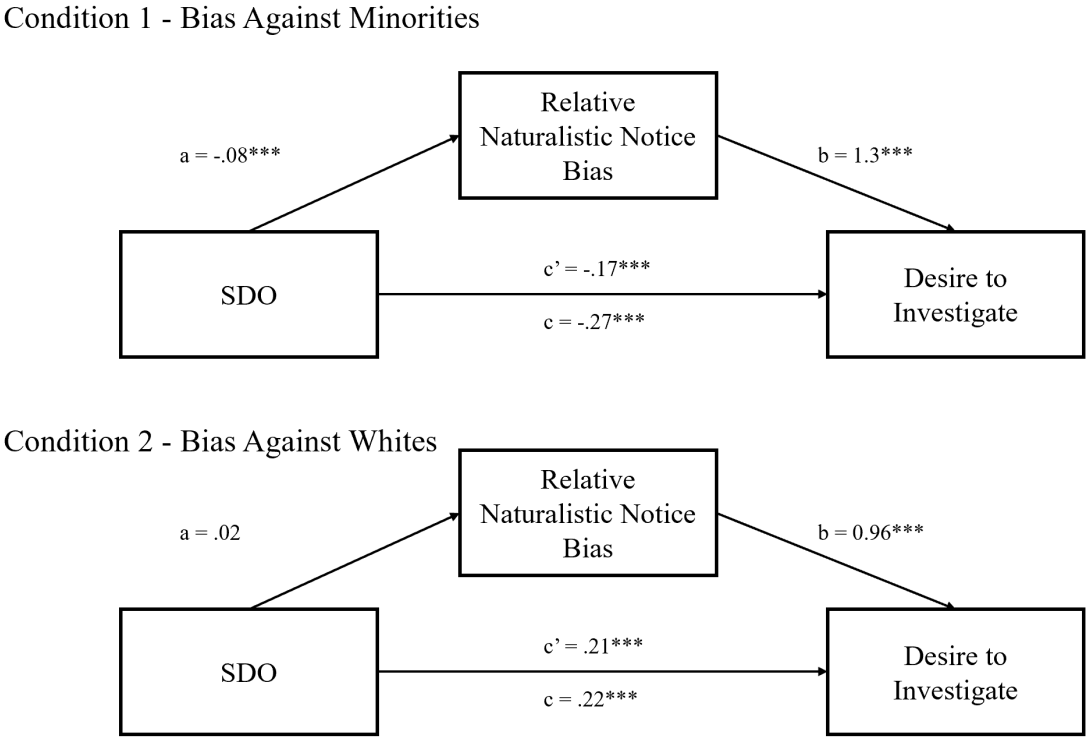


Figure A36. Mediation model linking SDO to the desire to investigate 'Connection Consulting' via *relative naturalistic notice bias* across task condition. ***p <.001

Absolute Bias Judgments. We entered SDO as the predictor, *absolute bias judgments* as the mediator, and desire to investigate as the outcome measure, with bias condition as a moderator of each of the a, b, and c paths. In the anti-minority bias condition, there was a significant negative indirect effect of SDO on desire to investigate via *absolute bias judgments*, *b* = -.15, *SE* = .03, 95% [-.21, -.08]. In contrast, in the anti-White bias condition, there was a significant positive indirect effect of SDO on desire to investigate via *absolute bias judgments*, *b* = .19, SE = .03, 95% [.13, .26]. For individuals lower in SDO (-1 SD below mean), the indirect effect of task condition on desire to investigate via *absolute bias judgments* was significantly negative, *b* = -.70, *SE* = .10, 95% [-.90, -.51]. For individuals higher in SDO (+1 SD above mean), the indirect effect of task condition on desire to investigate via *absolute bias judgments* was significantly positive, *b* = .28, *SE* = .09, 95% [.11, .46].

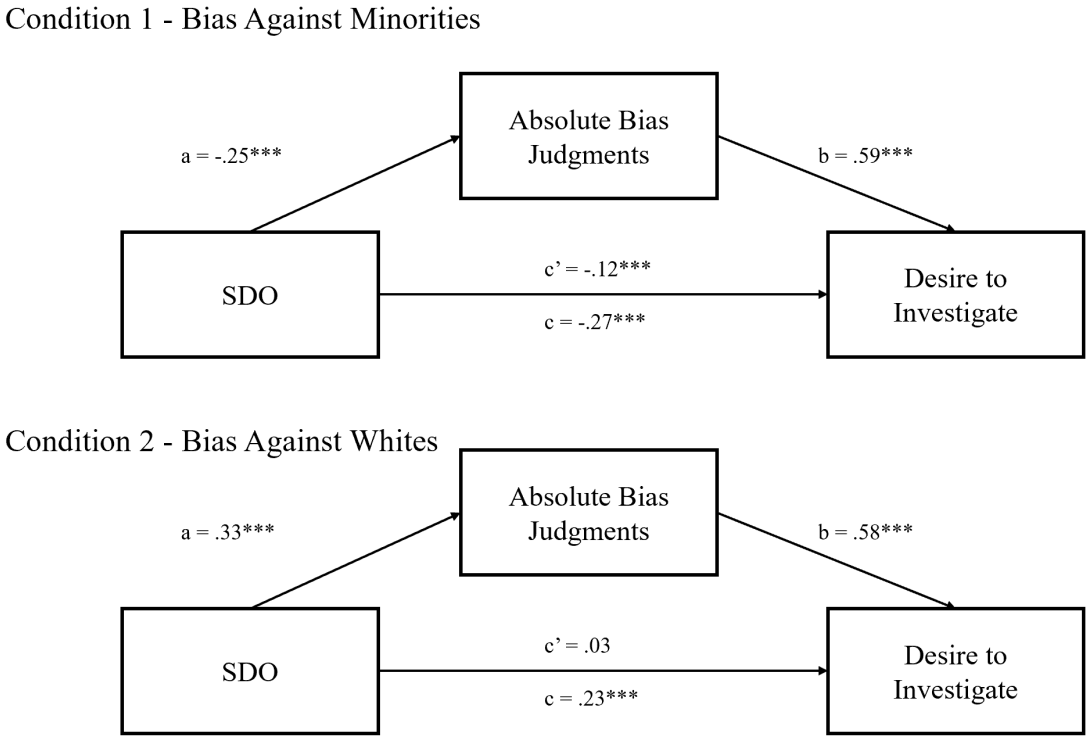


Figure A37. Mediation model linking SDO to the desire to investigate 'Connection Consulting' via *absolute bias judgments* across task condition. ***p <.001

Relative Bias Judgments. We entered SDO as the predictor, *relative bias judgments* as the mediator, and desire to investigate as the outcome measure, with bias condition as a moderator of each of the a, b, and c paths. In the anti-minority bias condition, there was a significant negative indirect effect of SDO on desire to investigate via *relative bias judgments*, b = -.16, SE = .02, 95% [-.21, -.12]. In contrast, in the anti-White bias condition, there was a significant positive indirect effect of SDO on desire to investigate via *relative bias judgments*, b = .04, SE = .02, 95% [.004, .08]. For individuals lower in SDO (-1 SD below mean), the indirect effect of task condition on desire to investigate via *relative bias judgments* was significantly negative, b = -.46, SE = .07, 95% [-.60, -.33]. For individuals higher in SDO (+1 SD above mean), the indirect effect of task condition on desire to investigate via *relative bias judgments* was significantly positive, b = .13, SE = .05, 95% [.04, .23].

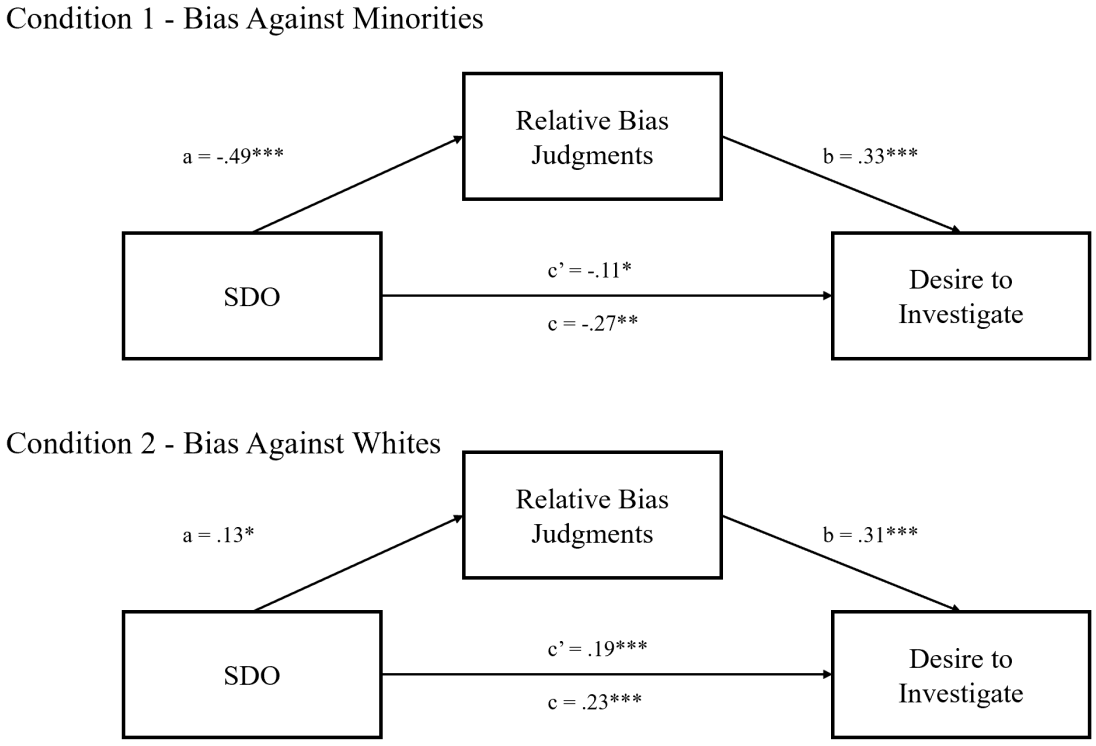


Figure A38. Mediation model linking SDO to the desire to investigate 'Connection Consulting' via *relative bias judgments* across task condition. ***p <.001

Appendix B

Chapter 3 Supplemental Information Appendix

Table of Contents

Preregistration Links

Sample 1a: https://aspredicted.org/3RQ_W6F
Sample 1b: https://osf.io/ysuw6/?view_only=53ce31a49fcc4fe9bec311e1f2c2bd4e
Sample 2a: https://aspredicted.org/182_TYY
Sample 2b: https://aspredicted.org/33P_5SV

Auxiliary Dependent Measures

Degree of Underestimation of Inequality: In addition to our dichotomous measure of whether or not participants underestimated the extent of unequal treatment faced by minority applicants, we examined the *Degree of Underestimation*, giving participants a score of '0' if they selected the pie chart which reflected the accurate distribution of hired White students to hired minority students or if they overestimated the extent of unequal treatment experienced by minority students. We gave participants a score of '1' on *Degree of Underestimation* if they selected the underestimating answer closest to the accurate response, a score of '2' if they selected the next closest underestimating response, and so on. Because there were 13 pie charts, of which 8 responses reflected underestimations, scores on *Degree of Underestimation* ranged from 0 to 8.

Degree of Overestimation of Inequality: We similarly examined *Degree of Overestimation*, giving participants a score of '0' on this measure if they were accurate or underestimated, a score of '1' if they selected the overestimating response closest to the accurate pie chart, and so on. Because there were 13 pie charts, of which 4 responses reflected overestimations, scores on *Degree of Overestimation* ranged from 0 to 4.

Relative Bias Judgments: After giving their open-ended response about their general impression of the task, we directly asked participants to indicate their level of agreement with each of the following statements on a 1 (*Strongly disagree*) to 7 (*Strongly agree)* scale: (a) 'Connection Consulting was biased against non-White (Asian, Black, Latino) students', and (b) 'Connection Consulting was biased against White students'. For this measure, *Relative Bias Judgments*, we examined results looking at perceptions of unequal treatment against the target who did encounter inequality (i.e., non-White students) while also incorporating assessments of bias against the target who did not face bias (i.e., White students). Thus, we calculated *Relative Bias Judgments* by subtracting self-reported ratings of bias against White students from ratings of bias against non-White students.

Pie Chart Selection: Participants selected from one of the following 13 pie chart distributions:

Option 1: 26 minority students: 2 White students
Option 2: 24 minority students: 4 White students
Option 3: 22 minority students: 6 White students
Option 4: 20 minority students: 8 White students

Option 5: 18 minority students: 10 White students
Option 6: 16 minority students: 12 White students
Option 7: 14 minority students: 14 White students
Option 8: 12 minority students: 16 White students
Option 9: 10 minority students: 18 White students
Option 10: 8 minority students: 20 White students
Option 11: 6 minority students: 22 White students
Option 12: 4 minority students: 24 White students
Option 13: 2 minority students: 26 White students

In addition to dichotomous measures of whether participants selected a pie chart accurately reflecting, underestimating, or overestimating the extent of inequality, we also created a continuous measure from 1 to 13 reflecting which pie chart participants selected.

Study 1

Sample 1a

Additional Conditions

Sample 1a also included an *Attention Nudge* condition. In this condition, prior to completing the mock hiring task, participants were given the explicit prompt to, "Please pay close attention to any inequality in hiring". Prior work on attention suggests that people selectively attend to the features of their environment that are *motivationally relevant*, that is, the environmental features that best facilitate goal achievement (Brosch & Van Bavel, 2012; Isaacowitz, 2006). We theorized that providing participants with the explicit goal of paying attention to any potential inequality in hiring would make participants more attuned to inequality and, thus, more likely to attend to and accurately detect inequality in the subsequent task.

Full Demographics

We aimed to collect data from approximately 800 participants per condition, for a total of roughly 2,400 participants after exclusions. In total, we collected data from 2,838 participants using Amazon's Mechanical Turk (M_{age} = 41.9, SD = 13.2; 55.7% Female, 43.7% Male, .5% Other; 1931 White, 154 Asian, 153 Black, 97 Latino, 9 Native American, 6 Middle Eastern, 50 Biracial, 5 Other) of whom 2,584 provided data on all focal variables (91.1% of full sample). As preregistered, we excluded participants who failed either of two attention checks, leaving us with a final sample of 2,415 participants. To maximize the range of SDO scores, we aimed to collect data from roughly equivalent numbers of Republicans and Democrats. Of the participants who provided data on all focal variables and were not excluded, 54.3% were Democrats or leaned Democrat and the remaining 45.7% were Republicans or leaned Republican.

Results

Table B1: *Output of Logistic Regression Predicting Key DVs from Condition and SDO*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Attention Nudge	.344*** (.142, .546)	.192+ (-.009, .393)	.002 (-.226, .230)	-.376** (-.655, -.098)
Information Treatment	.464*** (.263, .665)	-.288** (-.485, -.091)	.064 (-.162, .290)	.362** (.117, .608)
SDO	-.216** (-.367, -.066)	.122† (-.022, .266)	-.046 (-.212, .120)	-.150 (-.343, .042)
Attention*SDO	-.197† (-.409, .014)	.178† (-.032, .387)	-.180 (-.418, .058)	-.111 (-.407, .186)
Information*SDO	-.100 (-.305, .105)	.035 (-.162, .233)	.009 (-.218, .235)	-.036 (-.290, .218)
Constant	-.523*** (-.666, -.380)	.331*** (.191, .471)	-1.136*** (-1.297, -0.975)	-1.557*** (-1.739, -1.374)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B2: *Output of Linear Regression Predicting Auxiliary DVs from Condition and SDO*

	Pie Chart Selection (1)	Relative Bias Judgments (2)	Degree of Underestimation (3)	Degree of Overestimation (4)	Desire to Investigate (5)
Attention Nudge	-.183† (-.387, .021)	.021 (-.189, .231)	.121 (-.050, .293)	-.062† (-.129, .005)	-.214** (-.375, -.053)
Information Treatment	.298** (.094, .502)	.193† (-.018, .403)	-.164† (-.336, .008)	.134*** (.067, .201)	.110 (-.051, .271)
SDO	-.127† (-.275, .020)	-.659*** (-.811, -.507)	.109† (-.015, .233)	-.018 (-.067, .030)	-.456*** (-.572, -.340)
Attention*SDO	-.097 (-.304, .111)	.001 (-.213, .216)	.085 (-.090, .260)	-.012 (-.080, .056)	.012 (-.152, .176)
Information*SDO	-.041 (-.244, .162)	.101 (-.109, .310)	.012 (-.159, .182)	-.030 (-.096, .037)	-.0005 (-.161, .160)
Constant	7.744*** (7.601, 7.888)	1.427*** (1.278, 1.575)	1.510*** (1.389, 1.631)	.254*** (.207, .301)	3.723*** (3.610, 3.837)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B3: *Output of Logistic Regression Predicting Key DVs from Condition and Party*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Attention Nudge	.464*** (.196, .732)	.241† (-.026, .508)	.011 (-.285, .308)	-.419* (-.767, -.071)
Information Treatment	.545*** (.277, .813)	-.164 (-.430, .101)	-.130 (-.430, .171)	.359* (.049, .668)
Political Party	-.322* (-.609, -.034)	.657*** (.371, .942)	-.413* (-.741, -.085)	-.590** (-.968, -.212)
Attention*Party	-.251 (-.654, .152)	-.103 (-.512, .305)	-.025 (-.490, .440)	.149 (-.420, .719)
Information*Party	-.209 (-.611, .193)	-.263 (-.663, .137)	.438† (-.020, .895)	-.001 (-.511, .508)
Constant	-.367*** (-.558, -.176)	.032 (-.156, .220)	-.954*** (-1.163, -.744)	-1.302*** (-1.531, -1.073)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Political Party Coded such that 1 = Democratic/lean Democratic, 2 = Republican/lean Republican.

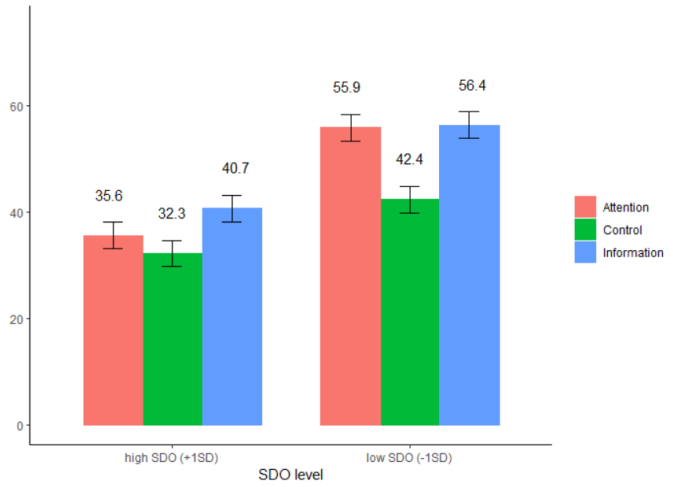
Table B4: *Output of Linear Regression Predicting Auxiliary DVs from Condition and Party*

	Pie Chart Selection (1)	Relative Bias Judgments (2)	Degree of Underestimation (3)	Degree of Overestimation (4)	Desire to Investigate (5)
Attention Nudge	-.211 (-.489, .066)	.108 (-.184, .401)	.120 (-.114, .353)	-.092* (-.183, -.001)	-.165 (-.386, .055)
Information Treatment	.345* (.068, .622)	.223 (-.069, .515)	-.172 (-.405, .061)	.173*** (.082, .263)	.151 (-.069, .371)
Political Party	-.377* (-.665, -.088)	-.774*** (-1.079, -.470)	.276* (.033, .519)	-.101* (-.195, -.006)	-.736*** (-.965, -.507)
Attention*Party	.052 (-.357, .461)	-.200 (-.631, .231)	.013 (-.331, .358)	.065 (-.069, .199)	-.126 (-.451, .199)
Information*Party	-.120 (-.530, .290)	-.122 (-.554, .309)	.030 (-.315, .375)	-.091 (-.225, .044)	-.138 (-.463, .188)
Constant	7.920*** (7.723, 8.116)	1.793*** (1.586, 2.000)	1.382*** (1.216, 1.547)	.301*** (.237, .365)	4.070*** (3.914, 4.226)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Political Party Coded such that 1 = Democratic/lean Democratic, 2 = Republican/lean Republican.

Figures

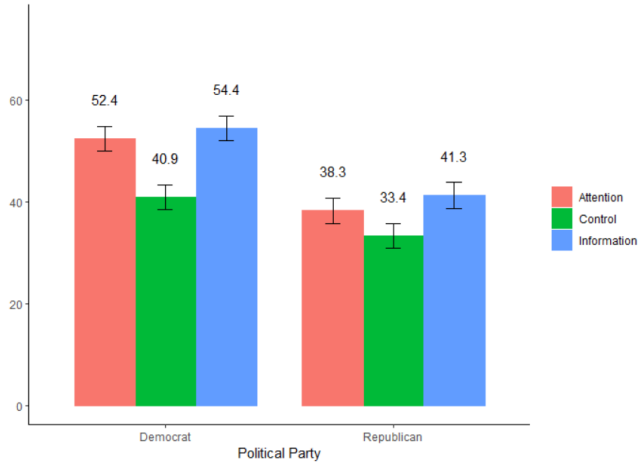
Figure B1: *% Noticing Bias – SDO*



SDO-level	Attention	Control	Information
Low SDO (-1SD)	55.9***	42.4	56.4***
High SDO (+1SD)	35.6	32.3	40.7*

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition. Figure error bars reflect standard error of the mean.

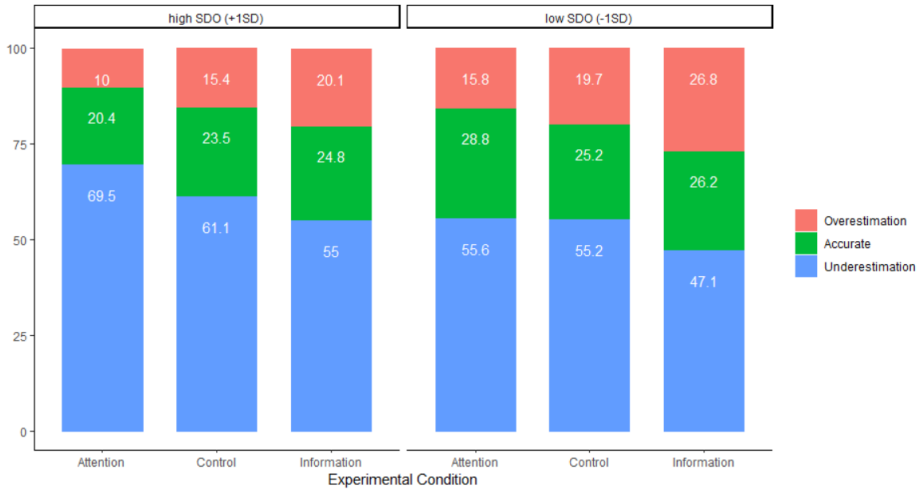
Figure B2: *% Noticing Bias – Party*



Political Party	Attention	Control	Information
Democratic	52.4***	40.9	54.4***
Republican	38.3	33.4	41.3*

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition. Figure error bars reflect standard error of the mean.

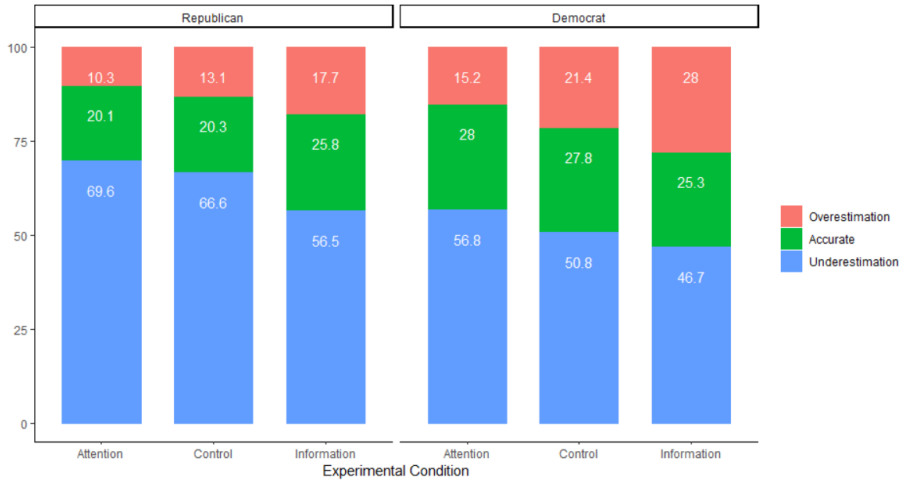
Figure B3: *% Accurately Detecting, Underestimating, and Overestimating Inequality – SDO*



	High SDO			Low SDO		
	Attention	Control	Information	Attention	Control	Information
Overestimation	10.0*	15.4	20.1†	15.8	19.7	26.8*
Accurate	20.4	23.5	24.8	28.8	25.2	26.2
Underestimation	69.5*	61.1	55.0†	55.6	55.2	47.1*

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition.

Figure B4: *% Accurately Detecting, Underestimating, and Overestimating Inequality – Party*



	Republican			Democrat		
	Attention	Control	Information	Attention	Control	Information
Overestimation	10.3	13.1	17.7†	15.2*	21.4	28.0*
Accurate	20.1	20.3	25.8	28.0	27.8	25.3
Underestimation	69.6	66.6	56.5**	56.8†	50.8	46.7

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition.

Sample 1b

Additional Conditions

Sample 1b also included a second bias condition where White (vs. minority) applicants were disproportionately less likely to be hired. We tested the effect of the information treatment (vs. control) in this condition as well. Participants in this condition received a '1' on *Accurate Pie Chart Selection* if they selected Option 5: 18 minority students: 10 White students, as this selection reflected the accurate response. Participants received a '1' on *Underestimation* if they selected a pie chart distribution that involved *less* inequality disfavoring White students than was actually present (i.e., if they selected Option 6 through Option 13) and a '0' otherwise. Participants received a '1' on *Overestimation* if they selected a pie chart distribution that involved *more* inequality disfavoring White students than was actually present (i.e., if they selected Option 1 through Option 4) and a score of '0' otherwise.

Full Demographics

We aimed to collect data from 800 participants per condition, for a total of roughly 3200 participants after exclusions. In total, we collected data from 3,481 participants using Amazon's Mechanical Turk and Prolific Academic (*M*age = 38.0, *SD* = 13.0; 54.7% Female, 44.7% Male, .4% Other; 2630 White, 200 Asian/Asian American, 282 Black/African American, 184 Latino/Hispanic, 18 Native American, 8 Middle Eastern, 110 Biracial, 22 Other). As preregistered, we excluded participants who failed either of two attention checks, leaving us with a sample of 3,193 participants (91.7% of full sample). Of the participants who were not excluded, 2,869 provided data on all focal variables (82.4% of our initial sample). We aimed to collect data from approximately equivalent numbers of Democrats and Republicans in order to maximize the range of SDO scores. Of the participants who provided data on all focal variables, 55.3% were Democrats or leaned Democrat and the remaining 44.7% were Republicans or leaned Republican.

Results

Table B5: *Output of Logistic Regression Predicting Key DVs from Condition and SDO*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Intervention	.142*** (.059, .224)	.050 (-.024, .123)	-.080† (-.163, .003)	.018 (-.077, .113)
Bias Condition	-.516*** (-.598, -.433)	.079* (.005, .153)	-.082† (-.165, .001)	-.019 (-.115, .076)
SDO	-.070* (-.132, -.007)	.045 (-.011, .101)	-.024 (-.087, .039)	-.039 (-.111, .034)
Int*Bias	-.031 (-.114, .051)	.060 (-.014, .134)	-.003 (-.086, .080)	-.097* (-.192, -.002)
Int*SDO	-.016 (-.078, .046)	-.025 (-.081, .031)	.030 (-.033, .094)	.004 (-.069, .077)
Bias*SDO	.217*** (.155, .279)	-.073* (-.129, -.017)	.041 (-.022, .105)	.065† (-.008, .138)
Int*Bias*SDO	-.014 (-.076, .048)	-.056* (-.113, -.0002)	.014 (-.049, .077)	.075* (.003, .148)
Constant	-.761*** (-.844, -.679)	.201*** (.128, .275)	-1.017*** (-1.100, -.933)	-1.501*** (-1.596, -1.406)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Bias Condition coded such that 1 = Bias Against Minority Students and 2 = Bias Against White Students.

Table B6: *Output of Linear Regression Predicting Auxiliary DVs from Condition and SDO*

	Pie Chart Selection (1)	Relative Bias Judgments (2)	Degree of Underestimation (3)	Degree of Overestimation (4)	Desire to Investigate (5)
Intervention	.082* (.011, .154)	.006 (-.072, .083)	.071* (.011, .130)	.009 (-.015, .034)	.010 (-.047, .068)
Bias Condition	-.944*** (-1.015, -.872)	-.461*** (-.539, -.384)	.019 (-.040, .079)	-.011 (-.036, .014)	-.362*** (-.419, -.304)
SDO	-.063* (-.117, -.008)	-.073* (-.131, -.014)	.050* (.005, .094)	-.016† (-.035, .003)	-.105*** (-.148, -.061)
Int*Bias	.060† (-.011, .132)	-.007 (-.085, .070)	.058† (-.001, .118)	-.023† (-.048, .001)	-.013 (-.071, .044)
Int*SDO	-.047† (-.101, .007)	-.055† (-.113, .004)	-.030 (-.075, .015)	-.005 (-.024, .014)	-.056* (-.100, -.013)
Bias*SDO	.067* (.012, .121)	.369*** (.311, .428)	-.046* (-.091, -.001)	.017† (-.001, .036)	.262*** (.218, .305)
Int*Bias*SDO	-.025 (-.079, .030)	.022 (-.036, .081)	-.021 (-.066, .024)	.025** (.007, .044)	.018 (-.025, .062)
Constant	7.018*** (6.947, 7.090)	1.018*** (.941, 1.096)	1.336*** (1.277, 1.396)	.279*** (.255, .304)	3.482*** (3.425, 3.540)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Bias Condition coded such that 1 = Bias Against Minority Students and 2 = Bias Against White Students.

Table B7: *Output of Logistic Regression Predicting Key DVs from Condition and Party*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Intervention	.104† (-.007, .215)	.050 (-.049, .149)	-.071 (-.182, .041)	.006 (-.121, .133)
Bias Condition	-.657*** (-.768, -.547)	.141** (.042, .240)	-.147** (-.258, -.036)	-.038 (-.165, .089)
Political Party	-.202* (-.367, -.038)	.107 (-.042, .255)	-.094 (-.262, .074)	-.049 (-.241, .142)
Int*Bias	-.064 (-.174, .047)	.109* (.010, .209)	-.048 (-.159, .063)	-.120† (-.247, .007)
Int*Party	.055 (-.109, .220)	.004 (-.144, .153)	-.032 (-.200, .136)	.036 (-.156, .227)
Bias*Party	.331*** (.167, .496)	-.144† (-.292, .005)	.154† (-.014, .322)	.039 (-.152, .231)
Int*Bias*Party	.068 (-.097, .232)	-.114 (-.262, .035)	.112 (-.056, .280)	.045 (-.146, .237)
Constant	-.661*** (-.772, -.551)	.154** (.055, .253)	-.976*** (-1.088, -.865)	-1.474*** (-1.601, -1.347)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Bias Condition coded such that 1 = Bias Against Minority Students and 2 = Bias Against White Students. Political party coded such that 1 = Democratic, 2 = Republican.

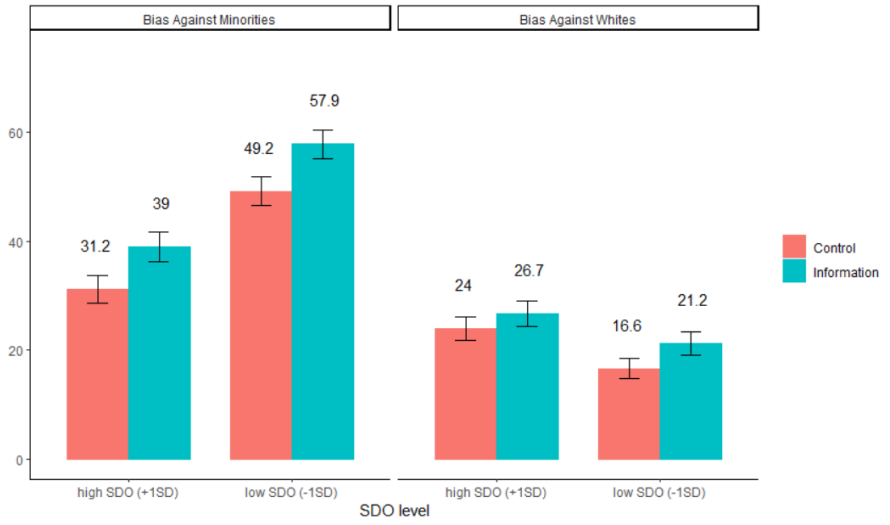
Table B8: *Output of Linear Regression Predicting Auxiliary DVs from Condition and Party*

	Pie Chart Selection (1)	Relative Bias Judgments (2)	Degree of Underestimation (3)	Degree of Overestimation (4)	Desire to Investigate (5)
Intervention	.120* (.023, .216)	.023 (-.082, .128)	.088* (.008, .168)	.017 (-.017, .050)	.031 (-.047, .109)
Bias Condition	-.995*** (-1.091, -.898)	-.831*** (-.936, -.726)	.037 (-.043, .117)	-.025 (-.058, .008)	-.560*** (-.638, -.482)
Party	-.075 (-.219, .070)	-.177* (-.334, -.020)	.081 (-.039, .201)	-.031 (-.081, .019)	-.385*** (-.502, -.268)
Int*Bias	.069 (-.027, .166)	-.032 (-.137, .073)	.083* (.003, .163)	-.036* (-.069, -.002)	-.044 (-.122, .034)
Int*Party	-.089 (-.233, .056)	-.068 (-.225, .089)	-.037 (-.156, .083)	-.017 (-.067, .032)	-.065 (-.181, .052)
Bias*Party	.113 (-.031, .257)	.836*** (.679, .993)	-.044 (-.163, .076)	.032 (-.017, .082)	.452*** (.336, .569)
Int*Bias*Party	-.018 (-.162, .126)	.057 (-.101, .214)	-.059 (-.179, .061)	.029 (-.020, .079)	.076 (-.041, .193)
Constant	7.050*** (6.953, 7.146)	1.093*** (.988, 1.198)	1.299*** (1.219, 1.379)	.293*** (.259, .326)	3.651*** (3.573, 3.730)

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Bias Condition coded such that 1 = Bias Against Minority Students and 2 = Bias Against White Students. Political party coded such that 1 = Democratic, 2 = Republican.

Figures

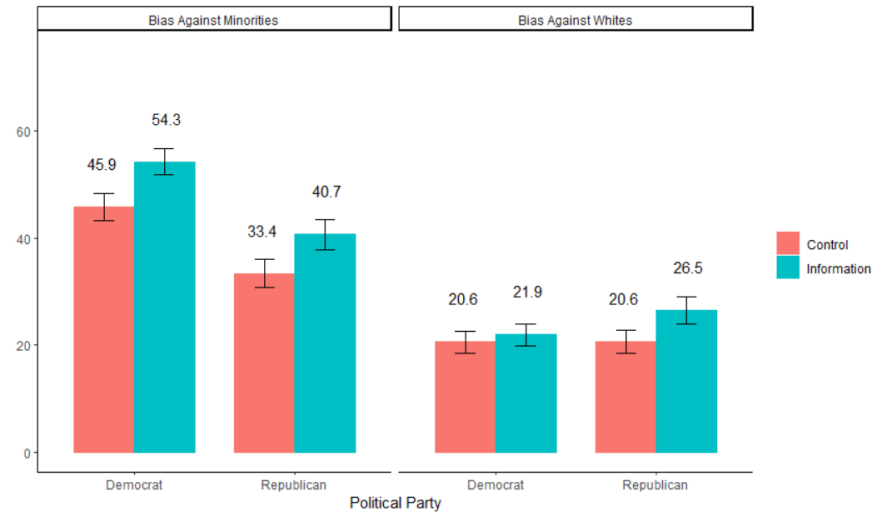
Figure B5: *% Noticing Bias as a Function of Experimental Condition, Bias Condition, and SDO*



	Bias Against Minority Students		Bias Against White Students	
	Control	Information	Control	Information
Low SDO (-1SD)	49.2	57.9*	16.6	21.2
High SDO (+1SD)	31.2	39.0*	24.0	26.7

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition. Error bars on figure reflect standard error of the mean.

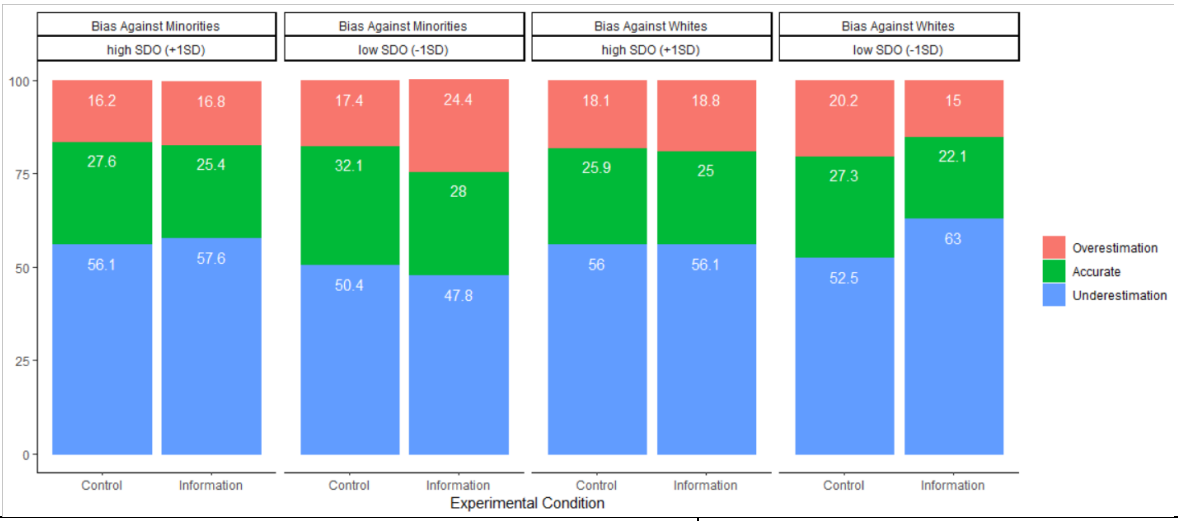
Figure B6: *% Noticing Bias a Function of Experimental Condition, Bias Condition, and Party*



	Bias Against Minority Students		Bias Against White Students	
	Control	Information	Control	Information
Democratic	45.9	54.3*	20.6	21.9
Republican	33.4	40.7†	20.6	26.5†

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition. Error bars on figure reflect standard error of the mean.

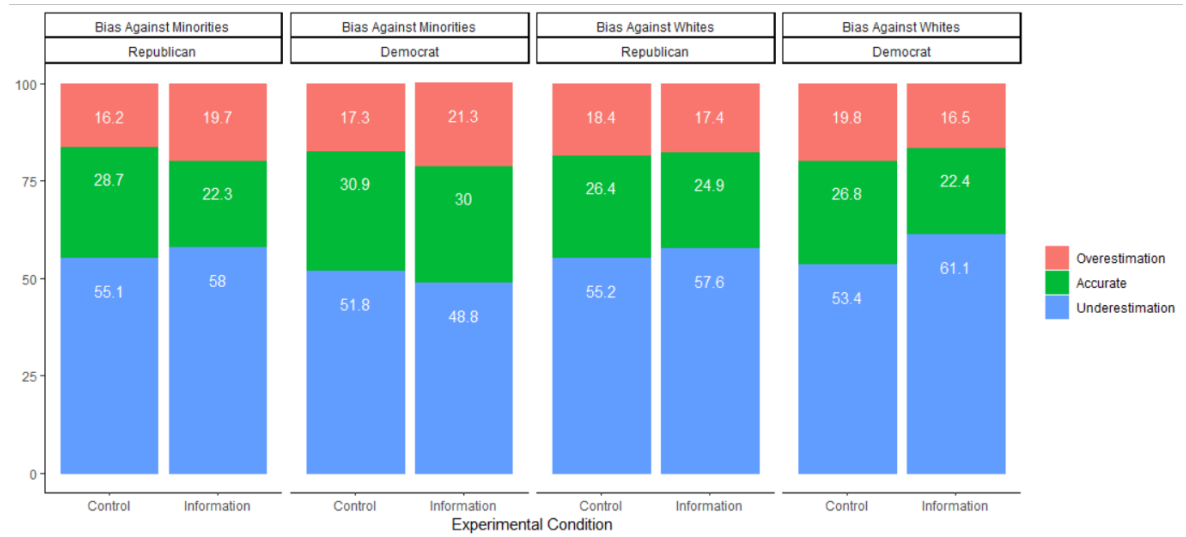
Figure B7: *% Accurately Detecting, Underestimating, and Overestimating Inequality*



	Bias Against Minority Students				Bias Against White Students			
	High SDO (+1SD)		Low SDO (-1SD)		High SDO (+1SD)		Low SDO (-1SD)	
	Control	Information	Control	Information	Control	Information	Control	Information
Overestimation	16.2	16.8	17.4	24.4*	18.1	18.8	20.2	15.0†
Accurate	27.6	25.4	32.1	28.0	25.9	25.0	27.3	22.1†
Underestimation	56.1	57.6	50.4	47.8	56.0	56.1	52.5	63.0**

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition.

Figure B8: *% Accurately Detecting, Underestimating, and Overestimating Inequality*



	Bias Against Minority Students				Bias Against White Students			
	Republican		Democrat		Republican		Democrat	
	Control	Information	Control	Information	Control	Information	Control	Information
Overestimation	16.2	19.7	17.3	21.3	18.4	17.4	19.8	16.5
Accurate	28.7	22.3†	30.9	30.0	26.4	24.9	26.8	22.4
Underestimation	55.1	58.0	51.8	48.8	55.2	57.6	53.4	61.1*

Note: † p < .1. * p <.05. ** p < .01. *** p < .001. Indicates difference between control condition and intervention condition.

Pooled Analyses

Table B9: *Correlation Matrix for Pooled Sample – Both Conditions (Control and Information)*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) SDO	2.56 (1.34)								
(2) Party	1.44 (.50)	.505***							
(3) Noticing Bias	.43 (.50)	-.146***	-.118***						
(4) Accurate Detection	.26 (.44)	-.029	-.040*	.124***					
(5) Underestimation	.54 (.50)	.076***	.095***	-.297***	-.645***				
(6) Degree of Underestimation	1.39 (1.72)	.072***	.056**	-.233***	-.477***	.740***			
(7) Overestimation	.20 (.40)	-.063***	-.075***	.235***	-.294***	-.541***	-.400***		
(8) Degree of Overestimation	.31 (.73)	-.051**	-.078***	.199***	-.252***	-.463***	-.343***	.856***	
(9) Desire to Investigate	3.81 (1.70)	-.269***	-.242***	.502***	.082***	-.287***	-.169***	.269***	.258***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001. SDO range: 1 to 7. Party: 1 = Democrat/lean Democrat and 2 = Republican/lean Republican. Degree Underestimation range: 0 to 8. Degree Overestimation range: 0 to 4. Desire to Investigate range: 1 to 7.

Table B10: *Correlation Matrix for Pooled Sample – Control Only*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) SDO	2.55 (1.34)								
(2) Party	1.45 (.50)	.504***							
(3) Noticing Bias	.38 (.49)	-.117***	-.093***						
(4) Accurate Detection	.27 (.44)	-.037	-.050*	.129***					
(5) Underestimation	.56 (.50)	.059*	.096***	-.296***	-.682***				
(6) Degree of Underestimation	1.44 (1.70)	.069**	.049†	-.230***	-.509***	.745***			
(7) Overestimation	.17 (.38)	-.034	-.067**	.240***	-.274***	-.516***	-.385***		
(8) Degree of Overestimation	.26 (.66)	-.012	-.050*	.201***	-.236***	-.444***	-.331***	.860***	
(9) Desire to Investigate	3.77 (1.69)	-.245***	-.217***	.489***	.082**	-.265***	-.158***	.253***	.239***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001. SDO range: 1 to 7. Party: 1 = Democrat/lean Democrat and 2 = Republican/lean Republican. Degree Underestimation range: 0 to 8. Degree Overestimation range: 0 to 4. Desire to Investigate range: 1 to 7.

Table B11: *Correlation Matrix for Pooled Sample – Information Treatment Only*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) SDO	2.57 (1.35)								
(2) Party	1.44 (.50)	.508***							
(3) Noticing Bias	.48 (.50)	-.177***	-.249***						
(4) Accurate Detection	.25 (.44)	-.020	-.029	.122***					
(5) Underestimation	.52 (.50)	.093***	.093***	-.293***	-.610***				
(6) Degree of Underestimation	1.34 (1.75)	.076**	.063*	-.233***	-.448***	.735***			
(7) Overestimation	.22 (.42)	-.091***	-.081**	.223***	-.313***	-.562***	-.413***		
(8) Degree of Overestimation	.36 (.79)	-.085**	-.101***	.188***	-.267***	-.480***	-.353***	.854***	
(9) Desire to Investigate	3.86 (1.71)	-.293***	-.266***	.514***	.083***	-.308***	-.178***	.282***	.274***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001. SDO range: 1 to 7. Party: 1 = Democrat/lean Democrat and 2 = Republican/lean Republican. Degree Underestimation range: 0 to 8. Degree Overestimation range: 0 to 4. Desire to Investigate range: 1 to 7.

Table B12: *Output of Pooled Logistic Regression Predicting Key DVs from Condition and SDO*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Information Treatment	.411*** (.269, .552)	-.163* (-.302, -.024)	-.062 (-.219, .095)	.323*** (.147, .498)
SDO	-.252*** (-.356, -.148)	.120* (.021, .219)	-.084 (-.195, .028)	-.095 (-.227, .037)
Information*SDO	-.112 (-.257, .033)	.068 (-.072, .207)	.038 (-.120, .196)	-.133 (-.313, .047)
Constant	-.492*** (-.594, -.391)	.253*** (.154, .351)	-1.015*** (-1.126, -.905)	-1.581*** (-1.711, -1.451)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B13: *Output of Pooled Linear Regression Predicting Auxiliary DVs from Condition and SDO*

	Relative Bias Judgments (1)	Degree of Underestimation (2)	Degree of Overestimation (3)	Desire to Investigate (4)
Information Treatment	.129 (-.028, .287)	-.093 (-.212, .025)	.106*** (.056, .156)	.104+ (-.009, .217)
SDO	-.562*** (-.673, -.451)	.117** (.034, .201)	-.009 (-.044, .027)	-.414*** (-.494, -.334)
Information*SDO	-.076 (-.233, .080)	.015 (-.103, .133)	-.058* (-.108, -.008)	-.083 (-.196, .029)
Constant	1.415*** (1.303, 1.526)	1.437*** (1.353, 1.520)	.258*** (.222, .293)	3.757*** (3.677, 3.837)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B14: *Output of Pooled Logistic Regression Predicting Key DVs from Condition and Party*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Information Treatment	.474*** (.288, .661)	-.154 (-.339, .031)	-.103 (-.310, .104)	.341** (.118, .565)
Political Party	-.392*** (-.596, .189)	.388*** (.190, .588)	-.227* (-.451, -.004)	-.365** (-.632, -.101)
Information*Party	-.173 (-.458, .112)	-.009 (-.290, .271)	.088 (-.230, .407)	-.035 (-.393, .325)
Constant	-.306*** (-.439, -.173)	.077 (-.055, .209)	-.914*** (-1.601, -.770)	-1.421*** (-1.590, -1.257)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001. Political party coded such that 1 = Democratic, 2 = Republican.

Table B15: *Output of Pooled Linear Regression Predicting Auxiliary DVs from Condition and Party*

	Relative Bias Judgments (1)	Degree of Underestimation (2)	Degree of Overestimation (3)	Desire to Investigate (4)
Information Treatment	.185† (-.029, .399)	-.115 (-.274, .044)	.145*** (.078, .213)	.164* (.012, .317)
Political Party	-.834*** (-1.061, -.607)	.166† (-.003, .335)	-.067† (-.138, .005)	-.736*** (-.898, -.574)
Information*Party	-.177 (-.499, .144)	.057 (-.182, .296)	-.094† (-.195, .007)	-.182 (-.411, .047)
Constant	1.802*** (1.649, 1.955)	1.361*** (1.247, 1.474)	.288*** (.240, .336)	4.099*** (3.990, 4.208)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001. Political party coded such that 1 = Democratic, 2 = Republican.

Study 2

Sample 2a

Table B16: *Results of Intervention (vs. Control) on Key DVs*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Reframing Intervention	.198 (-.086, .483)	-.242† (-.515, .03)	.196 (-.117, .509)	.165 (-.197, .528)
Constant	-.813*** (-1.016, -.614)	.514*** (.325, .707)	-1.250*** (-1.477, -1.032)	-1.723*** (-1.989, -1.473)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B17: Results of Intervention (vs. Control) on Auxiliary DVs

	Relative Bias Judgments (1)	Degree of Underestimation (2)	Degree of Overestimation (3)	Desire to Investigate (4)
Reframing Intervention	-.064 (-.340, .213)	-.179 (-.448, .090)	-.0004 (-.112, .112)	.030 (-.182, .242)
Constant	.908*** (.717, 1.100)	1.853*** (1.667, 2.039)	.310*** (.232, .387)	3.423*** (3.276, 3.570)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Sample 2b

Table B18: *Results of Intervention (vs. Control) on Key DVs*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Reframing Intervention	.567** (.171, .968)	-.539** (-.919, -.162)	.451* (.032, .875)	.333 (-.188, .863)
Constant	-.995*** (-1.300, -.707)	.602*** (.332, .878)	-1.233*** (-1.554, -.929)	-1.916*** (-2.326, -1.544)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B19: *Results of Intervention (vs. Control) on Auxiliary DVs*

	Relative Bias Judgments (1)	Degree of Underestimation (2)	Degree of Overestimation (3)	Desire to Investigate (4)
Reframing Intervention	-.103 (-.482, .276)	-.089 (-.387, .210)	.034 (-.075, .143)	.145 (-.153, .443)
Constant	1.018*** (.750, 1.285)	1.412*** (1.201, 1.622)	.195*** (.118, .272)	3.312*** (3.101, 3.522)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Pooled Analyses

Table B20: *Correlation Matrix for Pooled Sample – Both Conditions (Control and Reframing)*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)
(1) Noticing Bias	.33 (.47)						
(2) Accurate Detection	.25 (.43)	.196***					
(3) Underestimation	.59 (.49)	-.311***	-.694***				
(4) Degree of Underestimation	1.63 (1.89)	-.264***	-.496***	.715***			
(5) Overestimation	.16 (.37)	.187***	-.250***	-.524***	.374***		
(6) Degree of Overestimation	.28 (.76)	.115***	-.209***	-.438***	.313***	.836***	
(7) Desire to Investigate	3.42 (1.59)	.382***	.093***	-.255***	.085**	.234***	.218***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B21: *Correlation Matrix for Pooled Sample – Control Condition Only*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)
(1) Noticing Bias	.29 (.46)						
(2) Accurate Detection	.22 (.42)	.226***					
(3) Underestimation	.63 (.48)	-.327***	-.703***				
(4) Degree of Underestimation	1.70 (1.88)	-.249***	-.486***	.691***			
(5) Overestimation	.14 (.35)	.181***	-.220***	-.539***	-.372***		
(6) Degree of Overestimation	.27 (.79)	.104**	-.185***	-.452***	-.313***	.840***	
(7) Desire to Investigate	3.39 (1.63)	.374***	.128***	-.310***	-.104**	.274***	.253***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B22: *Correlation Matrix for Pooled Sample – Reframing Condition Only*

	Mean (SD)	(1)	(2)	(3)	(4)	(5)	(6)
(1) Noticing Bias	.37 (.48)						
(2) Accurate Detection	.28 (.45)	.161***					
(3) Underestimation	.55 (.50)	-.288***	-.683***				
(4) Degree of Underestimation	1.55 (1.90)	-.276***	-.504***	.738***			
(5) Overestimation	.17 (.38)	.188***	-.283***	-.507***	-.374***		
(6) Degree of Overestimation	.28 (.74)	.127**	-.237***	-.425***	-.314***	.838***	
(7) Desire to Investigate	3.45 (1.55)	.390***	.055	-.195***	-.062	.192***	.176***

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B23: *Results of Intervention (vs. Control) on Key DVs*

	Noticing Bias (1)	Underestimation (2)	Accurate Detection (3)	Overestimation (4)
Reframing Intervention	.324** (.093, .556)	-.345** (-.566, -.124)	.289* (.039, .540)	.218 (-.079, .517)
Constant	-.872*** (-1.040, -.708)	.543*** (.388, .701)	-1.244*** (-1.428, -1.066)	-1.785 (-2.006, -1.575)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.

Table B24: *Results of Intervention (vs. Control) on Auxiliary DVs*

	Relative Bias Judgments (1)	Degree of Underestimation (2)	Degree of Overestimation (3)	Desire to Investigate (4)
Reframing Intervention	-.076 (-.299, .148)	-.154 (-.359, .051)	.010 (-.073, .093)	.068 (-.104, .241)
Constant	.945*** (.789, 1.101)	1.705*** (1.562, 1.848)	.271*** (.214, .329)	3.385*** (3.265, 3.506)

Note. † p < .1. * p <.05. ** p < .01. *** p < .001.