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The Contingent Reputational Benefits of Selective Exposure to Partisan Information

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Individuals often preferentially avoid information that contradicts and seek information that aligns with their prior beliefs—a tendency referred to as "selective exposure." Traditionally, prior research has focused on *intrapersonal* drivers of selective exposure, including avoidance of cognitive dissonance. We take a complementary approach by investigating the conditions under which *interpersonal* concerns drive selective exposure. Drawing on a large literature on impression management, we test a social signaling model of selective exposure, which predicts that (a) individuals shift their information selection decisions to signal to observers and (b) observers reward such shifts. We test this model in the domain of partisan politics in the United States across five financially incentivized, preregistered experiments (N = 3,598). Our results extend prior theory by identifying three key contingencies: the type of task on which observers expect to collaborate with actors, alignment of group membership between observers and actors, and the magnitude of demonstrated selective exposure. Overall, we find that tailoring one's information selection decisions can indeed have strategic value—but only under certain theoretically predictable conditions. Our work also identifies an actor–observer misalignment: While observers are sensitive to the type of future interaction with an actor, the actors themselves do not intuit this sensitivity. In the era of social media, when information selection decisions are more public than ever and the spread of misinformation is pervasive, understanding the ways in which reputational considerations shape decision making not only illuminates why selective exposure persists, but also suggests novel mitigation strategies.

Public Significance Statement
In the domain of partisan politics, we provide experimental evidence for the interpersonal drivers of selective exposure to information. We find that people shift their information selections to pander to observers and that observers subsequently reward people who select information aligned with the observers' ingroup.

Keywords: selective exposure, reputation, trust, cognitive dissonance, judgment and decision making

A large theoretical and empirical literature argues that consuming information from a diversity of sources improves judgment and decision-making (Akerlof, 1970; Blackwell, 1953; Galton, 1907; Golman et al., 2017; Janis, 1982; Mullainathan & Shleifer, 2005; Page, 2008; Peterson & Pitz, 1986; Stewart, 1988; Stigler, 1961; Sunstein, 2001; Surowiecki, 2005). Yet, individuals frequently select information that aligns with and avoid information that contradicts their prior beliefs—a phenomenon referred to as "selective exposure," or the "congeniality bias" (Adams, 1961; Akerlof & Dickens, 1982; Dorison et al., 2019; Freedman & Sears, 1965;

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Please find preregistrations, data, code, and survey materials on the Open Science Framework (OSF) page (https://osf.io/eu56v/?view_only=4aa7f5208d5048c5a8c019145b3065ac).

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Frey, 1986; Frey & Rosch, 1984; Frimer et al., 2017; Gentzkow & Shapiro, 2010; Hart et al., 2009; Iyengar & Hahn, 2009; Jonas et al., 2001; Lazarsfeld et al., 1948; Stroud, 2008).

Engaging in selective exposure has important consequences not only for judgment and decision making, but also for political polarization. Preferential consumption of ideologically aligned information—or avoidance of ideologically misaligned information—can increase divergence of political opinions (Lazarsfeld et al., 1948), create increasingly partisan information silos (Gentzkow & Shapiro, 2010; Sunstein, 2001), and prevent individuals from forming accurate beliefs about the world (de Benedictis-Kessner et al., 2019). Thus, understanding the causes and consequences of selective exposure continues to be a pressing concern for individuals, groups, and even democracy itself.

Prior research has focused on *intrapersonal drivers of selective exposure*, with explanations primarily centered around individuals' desire to avoid negative emotions (Adams, 1961; Dorison et al., 2019; Festinger, 2001; for review, see Hart et al., 2009; Sharot & Sunstein, 2020). Specifically, researchers have theorized that exposure to information that contradicts one's prior beliefs triggers the negative affective state of cognitive dissonance, which individuals are in turn motivated to avoid (Hart et al., 2009). However, a narrow focus on intrapersonal drivers may neglect other important causes of the phenomenon. For example, an extensive literature demonstrates that people care deeply about their reputations and the impressions they leave on others (Baumeister & Leary, 1995; Berman et al., 2015; Dorison, 2023; Dorison & Heller, 2022; Goffman, 1959; Jones & Pittman, 1982; Leary & Kowalski, 1990; Lerner & Tetlock, 1999; Mayer et al., 1995; Schlenker & Weigold, 1992; Tetlock, 2000, 2002; Westphal & Graebner, 2010). Indeed, much of our decisionmaking takes place in social settings, under the watchful eyes of both friends and foes.

The Present Research

In the present research, we thus examine the *interpersonal drivers of selective exposure*. Across five preregistered experiments, we assess both how social environments shape information selections (i.e., the reputational *causes* of selective exposure; Experiments 1 and 3) and how these selections are subsequently evaluated by observers (i.e., the reputational *consequences* of selective exposure; Experiments 2, 4, and 5). Our overarching goal is to test both the extent to which selective exposure is driven by interpersonal concerns and whether such concerns are justified.

We first draw upon conventional selective exposure paradigms to explore these questions (i.e., observing the choices participants make when offered a menu of information sources such as news articles or websites of politicians; Experiments 1–2). We then develop a stylized, novel, incentive-compatible paradigm, which allows us to explicitly consider a common trade-off between impression-manage ment goals and judgment accuracy (Experiments 3–5).

In these experiments, we also go beyond the prior literature to test three situational features that might moderate the reputational causes and consequences of selective exposure. First, we test the congruence of group membership (i.e., aligned vs. unaligned) between an actor selecting information and an observer evaluating the actor's choices. Second, we examine the type of future interaction between the actor and the observer (i.e., one requiring trust vs. judgment skill). Finally, we test whether the magnitude of selective exposure demonstrated by an actor moderates these effects. Together, our experiments explore whether engaging in selective exposure when others are watching can have strategic value. We further develop these hypotheses below.

Selective Exposure

Research on selective exposure boasts a rich history across multiple disciplines. As early as the 1940 United States presidential election, researchers documented partisan effects on Americans' media choices (Lazarsfeld et al., 1948). Later work in social psychology operationalized selective exposure in terms of alignment between an individual's information selection decisions and their personal beliefs (Adams, 1961; Stroud, 2014). And more recently, researchers have extended this framework by demonstrating that even in the case of consulting experts, people are more likely to favor those who share their views (Johnson et al., 2021).

Rather than focusing on alignment of sought-after information with *personal* beliefs, work in political science has focused on the alignment of information with *ingroup* beliefs—as one's ideology is often widely shared by the members of one's ingroup. For example, Iyengar and Hahn (2009) found that while Republicans preferred to read information from *Fox News* (a news source typically associated with a conservative viewpoint) compared to *CNN* and *NPR* (news sources typically associated with a liberal viewpoint), this pattern reversed among Democrats (for related work, see Mullainathan & Shleifer, 2005).

Notably, selective exposure has been defined both in terms of seeking confirming information *and* avoiding disconfirming information (Hart et al., 2009), in line with the daily experience of choosing from a variety of sources. To model this experimentally, selective exposure is frequently evaluated by offering participants a diverse menu of information choices (Dorison et al., 2019; Iyengar & Hahn, 2009; for review, see Stroud, 2014). And while the experimental results are open to interpretation as either evidence of people preferentially seeking confirming information or avoiding disconfirming information, measuring this relative preference for information continues to align with the experience of many individuals.

Importantly, across fields and different methodological and theoretical perspectives, research exploring the causes of selective exposure has predominantly focused on intrapersonal drivers of the phenomenon (e.g., Dorison et al., 2019; Frimer et al., 2017; Golman et al., 2017; for reviews, see Hart et al., 2009; Sharot & Sunstein, 2020; Stroud, 2014). In particular, this work has highlighted the idea that avoiding opposing views reduces cognitive dissonance, an unpleasant state of psychological tension evoked by the presence of contradictory thoughts, beliefs, or attitudes (Adams, 1961; Festinger, 2001; Frimer et al., 2017). Based on this theorizing, prior research has determined that selective exposure is likely to be most prominent when individuals expect conflict between new information and important views or decisions (Frey & Rosch, 1984; Jonas et al., 2001).

Social Signaling Model of Selective Exposure

In contrast to a large interdisciplinary literature on the intrapersonal drivers of selective exposure, a much smaller body of work has theorized that there could also be *interpersonal* drivers (Hart et al., 2020; Lundgren & Prislin, 1998). From browsing at a news

stand, to choosing who to "like" on social media, to deciding what events to attend, or which websites to open at work, many information selection decisions are made in full view of others. Given that individuals care deeply about their reputations, beyond simply attending to the instrumental value of information, people are likely to also be mindful of the impressions they are creating on those around them. Specifically, because members of ingroups are viewed more favorably on a variety of dimensions (Fiske, 2015; Foddy et al., 2009; Rand et al., 2009; Tajfel & Turner, 2001), people may choose to expose themselves to certain information in public to signal group membership. And even in cases where group membership is known, preferentially selecting ingroup-aligned information might signal the strength of one's affiliation.

Classic research in economics (Spence, 1973) proposes that if an individual has a characteristic that is desirable to others, (a) the individual will send a signal associated with the relevant characteristic and (b) others will reward the individual that sends such a signal. According to the model, this equilibrium is maintained because the reward reinforces the signal. This model has been used to explain a variety of behaviors, including but not limited to: paying for highly conspicuous goods to signal wealth and attain status (Veblen, 1899), cooperating with others without looking at costs to signal trustworthiness (Hoffman et al., 2015; Jordan, Hoffman, Nowak, & Rand, 2016), and escalating commitment to failing courses of action to avoid appearing unreliable (Dorison et al., 2021). Extending this logic to the domain of information, scholars have proposed that people selectively process specific information (Kahan, 2013) and express particular opinions (Chen et al., 1996; Earl et al., 2019; Ekstrom & Lai, 2021; Hussein & Wheeler, 2023; Silver et al., 2021) to manage the impressions they leave on valued observers.

Comprehensively testing social signaling hypotheses requires a two-step methodological approach (for recent examples, see Dorison et al., 2021; Jordan, Hoffman, Bloom, & Rand, 2016; Jordan, Hoffman, Nowak, & Rand, 2016; for review, see Leary et al., 2015; Schwardmann & van der Weele, 2019). First, to determine whether an individual is engaging in signaling behavior, researchers typically manipulate whether the focal behavior is conducted publicly or privately. If the person is more likely to undertake the behavior in public, then it is concluded that the behavior is at least partly attributable to a signaling motivation. Second, to determine whether the signaling behavior carries social benefits, researchers measure the reactions of observers toward individuals who do or do not engage in the focal behavior. Finally, a thorough understanding of the role of signaling in a particular context requires knowing under what contextual conditions both effects are most likely to emerge.

Prior research has not comprehensively examined this model in the domain of selective exposure. In an early experiment, Lundgren and Prislin (1998) tested the impact of three different goals (impression management, defense, or accuracy) on information selection behaviors. This study did not find any effect of impression motivations on information selection (Lundgren & Prislin, 1998). A later review article on selective exposure concluded that the research on impression-related motivations behind selective exposure did not "offer sufficient evidence" (Hart et al., 2009). In light of this, Hart et al. (2020) revisited this hypothesis, showing that giving participants an explicit goal of convincing an observer that they held a particular belief prompted participants to select more information sources aligned with that belief.

In the present work, we go beyond these prior tests to examine a comprehensive signaling model of selective exposure to information in the context of American politics. Specifically, we explore the following hypotheses: (a) People engage in selective exposure at least partly to send a signal to observers, and (b) observers reward those that send such a signal. Additionally, we consider whether the following elements impact people's information selection decisions and/or observer evaluations: the congruence of group membership between the actor and observer, whether the decision context under which an individual is being evaluated relies on trust or judgment skill, and the magnitude of exhibited selective exposure. We consider each of these hypotheses below.

Reputational Causes and Consequences of Selective Exposure

Prior research argues that any effect of observation on selective exposure "should depend on the characteristics of the audience that one intends to impress" (Schlenker, 1980; as cited in Hart et al., 2009). Within the political context, ingroup membership is widely beneficial: Interpersonally, people are more likely to collaborate with political ingroup members (Lelkes & Westwood, 2017); professionally, political ingroup members are believed to have superior professional judgment (Yeomans et al., 2020) and are more likely to be interviewed for desirable positions (Gift & Gift, 2015); economically, sellers will even offer lower prices to political ingroup members for the same good (Michelitch, 2015). Selecting information aligned with the observer's known group membership could serve as a signal of group alignment or the strength of one's affiliation. This suggests that selective exposure will be magnified when actors are surrounded by ingroup evaluators but attenuated (or even reversed) in the presence of outgroup evaluators.[1]

From the observer perspective, knowing whether someone is part of your ingroup is also valuable for predicting their behavior toward you (Brewer & Caporael, 2006; Yamagishi et al., 1998) as people are more cooperative toward ingroup members (Balliet et al., 2014). Consequently, we predict that observers will reward those actors who consume more of the observer's ingroup information.

Congruence of Group Membership

So far, we have theorized that (a) individuals will shift their information selection decisions to align with observer's beliefs, and (b) observers will reward them for doing so. However, might such patterns depend on whether the person's group membership is already known—or strongly suspected? In such situations, we argue that instead of signaling group membership, information consultation choices might instead signal the *strength* with which an individual identifies with their group (Abrams & Hogg, 1990).

For example, when a liberal observer is evaluating the behavior of an actor who is known to be a liberal, the actor could benefit by consuming liberal information sources if this signals their thorough commitment to liberal ideas. By contrast, if the actor is known to be conservative, consulting liberal sources might be a sign that they are receptive to a diversity of viewpoints (Minson et al., 2020). As one's initial expectations of a known outgroup member

[1] Importantly, this is a departure from economic theories of signalling, which have largely ignored the role of observer identity (Spence, 1973).

are likely to be negative (Moy & Ng, 1996; Tajfel & Turner, 2001), the latter case might powerfully violate observer expectations. Thus, signaling a willingness to seek out observer-aligned information may be particularly beneficial for outgroup members. To address this possibility, we systematically vary group membership (ingroup, outgroup, unknown) across studies.

Decision Context

Individuals evaluate ingroup members and those who more strongly subscribe to ingroup ideologies more favorably than outgroup members on several dimensions (Fiske, 2015; Foddy et al., 2009; Rand et al., 2009). However, the relevance of any given dimension of social evaluation naturally varies with the context. For example, in some interactions we might seek individuals who are "on our side," irrespective of any other skillset they may or may not possess. In other interactions, we might want to collaborate with an individual who is an astute perceiver of the world, even if their observations might lead them to unfavorable conclusions. This suggests that while signaling group affiliation is beneficial in some decision contexts, it may be less so in others.

However, prior literature does not offer clear predictions regarding whether actors or observers recognize these distinctions. For example, it could be the case that people engage in selective exposure to impress observers even in contexts where this would be inappropriate (i.e., contexts where observers value unbiased evaluation of all available information). Yet, this strategy may be effective if observers are insensitive to the fit between the characteristics being signaled (ingroup affiliation) and the characteristics necessary for success in a given context (ability to consider a variety of perspectives), and instead simply reward group affiliation across the board.

We address this question by examining signaling behavior across two contexts in which observers are likely to value different characteristics: one that relies on trustworthiness and one that relies on judgment accuracy. This allows us to test both whether actors anticipate that signaling ingroup affiliation is more or less beneficial in different environments, as well as whether observers are sensitive to these distinctions.

Magnitude of Selective Exposure

Whereas prior research has treated selective exposure as a bias, the signaling model suggests that some selective exposure might be appropriate when taking reputational rewards into account. In other words, a decrease in decision quality may, theoretically, be offset by benefits to one's reputation. However, what level of selective exposure will observers reward? In contexts where group affiliation is valued, should people select only ingroup information and avoid all counterattitudinal views to maximally signal group affiliation? Recent research demonstrates that people value copartisan perspective seekers (Heltzel & Laurin, 2021), which suggests there might be a benefit to maintaining a balance between signaling group affiliation and consuming a diverse information diet. However, do both actors and observers recognize this? And what should decision makers do in contexts when the identity of observers is unknown or when they represent a mix of ideological perspectives, as is often the case outside of a controlled laboratory setting?

While prior research has focused on the presence or absence of selective exposure, our paradigm allows us to assess reputational consequences as a function of the extremity of this tendency. By examining observers' reactions to actors who engage in different levels of selective exposure, we can evaluate not only the overall costs versus benefits of signaling through information selection decisions, but also identify the ideal amount of observer-aligned information that one should select to maximize social benefits.

Research Overview

In the present research, we experimentally test whether selective exposure is partly driven by people's desire to signal information to observers and under what conditions observers reward people for such signaling (i.e., we test a comprehensive "signaling model" of selective exposure). To do so, we conduct five preregistered, financially incentivized laboratory experiments. Experiments 1 and 3 examine the reputational causes of information selection, whereas Experiments 2, 4, and 5 examine the reputational consequences of such choices.

Experiments 1–2 test these hypotheses using traditional selective exposure paradigms. Specifically, Experiment 1 tests whether individuals differentially engaged in selective exposure when observed by political ingroup members, political outgroup members or in private. We examine this question across two types of information sources: information arising from an individual with a specific partisan identity (e.g., the website of a U.S. Senator) and information arising from an organization with a specific partisan identity (e.g., a news story from a specific outlet). Experiment 2 tests the contingent wisdom of this strategy by examining whether observers reward people based on their information selection.

Experiments 3–5 develop a novel, incentivized, experimental–economic paradigm to introduce a competing accuracy benefit to consuming varied information. Experiment 3 uses this paradigm to test whether individuals are willing to forego useful information to impress an audience. Experiment 3 also examines whether any audience effect on selective exposure depends on the nature of the future collaborative task that the actor anticipates engaging in with the observer.

Experiments 4–5 turn to the observer's perspective, allowing us to test when and why observers reward actors for selective exposure—even in contexts where there are apparent trade-offs between accuracy and selective exposure. Whereas Experiment 4 leaves the actor's group membership ambiguous, in Experiment 5, we experimentally vary whether the actor belongs to the observer's ingroup. In both Experiments 4–5, we also assess how the magnitude of information selection impacted observer decisions. We thus identify important boundary conditions to the reputational benefits of selective exposure.

Transparency and Openness

We preregistered all studies before beginning data collection.[2] The preregistrations, data, code, and survey materials are available on the Open Science Framework (OSF) page. In each study, unless otherwise indicated, all analyses are preregistered and confirmatory

[2] Note that Experiments 1 and 3 used AsPredicted.org for the preregistration process. The other three studies had a time-stamped document on OSF prior to data collection; however, we did not hit the "Register" button until after the data collection process because we were unaware of this functionality.

(either primary or secondary). All other analyses are explicitly labeled as exploratory (either preregistered or not). We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in all experiments (Logg & Dorison, 2021; Simmons et al., 2012). Details about implementation details and recruitment of participants for all experiments can be found in Tables A1 and A2.

Experiment 1

Experiment 1 investigates the reputational causes of selective exposure to partisan information using a traditional selective exposure paradigm. Our procedure builds upon prior research methodologies that evaluate selective exposure by offering participants (whom we call "actors") a diverse menu of information choices (Dorison et al., 2019; Iyengar & Hahn, 2009; for review, see Stroud, 2014). All participants had the opportunity to select five out of 10 pieces of information, evenly balanced between information arising from ingroup versus outgroup sources. We systematically varied whether actors' information selection decisions[3] were made in private, observed by members of their political ingroup, or observed by members of their political outgroup. In both public conditions, actors learned that the observer would choose whether or not to send them money in a financially incentivized trust game (described below) based on their selections. In line with prior work (e.g., Dorison et al., 2019), we considered making selections where the majority of information is aligned with one's own prior beliefs or group affiliation to be evidence of selective exposure.

Based on the social signaling model, we hypothesized that actors being observed by ingroup members would select more ideologically aligned information than actors in the private condition, who would in turn select more ideologically aligned information than actors being observed by outgroup members. We predicted that these effects would be sustained for both types of information choices (news stories and senators' webpages). The private control condition allows us to assess whether shifts in behavior are due primarily to one type of observer (or both).

Method

Participants

We recruited a sample of U.S. residents from Cloud Research (Litman et al., 2017) to participate in a 3- to 4-min experiment in exchange for $0.50, with further opportunities for a bonus. Our final sample consisted of 602 participants ($M_{age} = 41.3$; 51.7% female, 47.8% male, 0.5% nonbinary; 59.5% liberal, 40.5% conservative). To achieve 80% power to detect a small to medium-sized main effect ($d = .40$) in each information type, we aimed to recruit at least 100 participants per experimental condition.

Procedure

Upon entering the study, participants first reported basic demographic information, including gender, age, and education. Following prior research (American National Election Studies, 2016), they indicated their political ideology on a 7-point scale from *extremely liberal* to *extremely conservative*. After reporting demographics, participants had the option to select five of 10 information sources presented to them. Those in the public ingroup and public outgroup conditions were told that an observer would evaluate their choices. At the end of the study, these participants reported how much money they would allocate to an observer in a trust game and were redirected to view their chosen information sources.

Dependent Variable. All actors were presented with a balanced set of five liberal sources and five conservative information sources. Adapting methodology from Dorison et al. (2019), actors' selection of the number of ingroup information sources to view served as our dependent variable. This number ranged from zero to five.

Independent Variables. Before making information selection decisions, participants were randomly assigned to one of six between-subjects experimental conditions in a 3×2 factorial design. For the first factor, we varied whether the actors' choices would be observed by a political ingroup member (ingroup condition), observed by a political outgroup member (outgroup condition), or made in private (private condition). Those participants in the two public conditions learned that their choices in the study would be communicated to an observer who would use that information to make decisions in a trust game (Berg et al., 1995). Specifically, the observer would have the opportunity to send the actor between 0–10 cents and that we (the experimenters) would triple whatever amount was sent by the observer. The actors would then have the opportunity to send back any amount of money they felt appropriate. Participants in the private condition were simply asked to select the information sources that they would like to view. Further implementation details can be found in Table A1.

In this manner, our paradigm closely tied the reputational gains that might arise from signaling specific information preferences to financial incentives for the actors. This method of tying reputational incentives to financial rewards via economic games such as the trust game is common (Dorison et al., 2021; Jordan, Hoffman, Nowak, & Rand, 2016), with previous research documenting lab and field generalizability (Camerer, 2011).

The second factor varied the type of information actors were asked to consider. Specifically, we instructed actors to select five information sources from a list of 10 current U.S Senators' press pages (senators condition) or from a list of 10 news stories (news condition). To ensure that participants were familiar with the senators, the list of choices included the five Democrats (e.g., Elizabeth Warren and Amy Klobuchar) and the five Republicans (e.g., Marco Rubio and Lindsey Graham) who had the most Twitter followers at the time of the study. For the news articles condition, we chose stimuli from the website allsides.com, which presents arguments for multiple perspectives on a variety of current policy issues. We selected a political issue (forgiving student loan debt) with at least five conservative and five liberal articles listed on allsides.com. In both conditions, we signaled the ideological slant of the choice as being a liberal or conservative senator or news source. Additionally, we provided participants with links to view their selected information sources at the end of the survey.

Analysis Plan

We used a linear regression, using the lm function in R to predict the number of ingroup information sources selected by participants

[3] For the ease of interpretation and consistency, while we acknowledge that selection and avoidance of information are inextricably linked in our design, throughout the methods and results sections, we generally refer to actor's "selection" of information.

as a function of experimental condition. The ingroup versus outgroup versus private conditions were represented by an unordered factor with the private condition set as the reference group. The second factor was dummy-coded such that *news* = 0 and *senators* = 1.

Results

Reputational Causes of Selective Exposure

We began by examining the level of selective exposure between the public conditions. If selective exposure was purely driven by individuals' affective and cognitive responses to the relevant information (c.f., Hart et al., 2009), observer group membership should not affect outcomes. However, if selective exposure was driven at least in part by individuals' desire to appeal to observers (e.g., Lerner & Tetlock, 1999; Tetlock, 2000, 2002), we should see actors in the ingroup condition consulting a greater number of ingroup advisors than actors in the outgroup condition.

We found evidence consistent with the latter hypothesis. On average, actors in the public ingroup condition selected 4.06 ingroup sources ($SD = 1.25$). In contrast, participants in the public outgroup condition selected just 2.36 ingroup sources ($SD = 1.63$), on average. When restricting to the two public conditions, in exploratory analyses, we find that the number of ingroup sources differs by whether actors were in the ingroup or outgroup condition, $t(376) = 11.73$, $p < .001$. See Appendix B for a replication of these results.

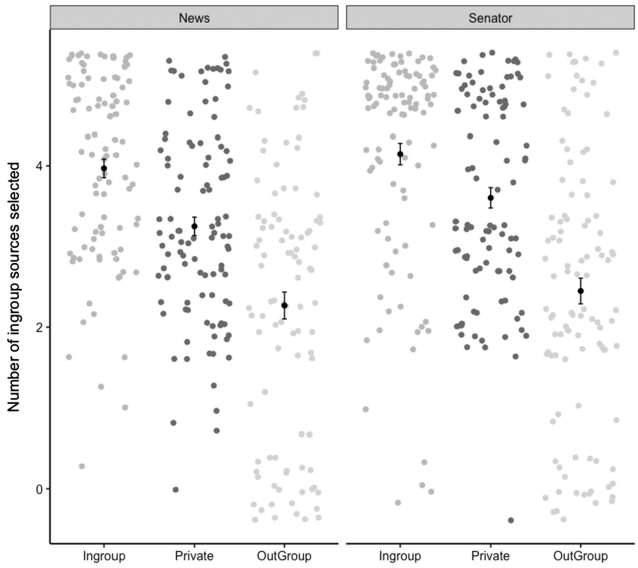
To put these results in perspective and think about the effect size, we ran an exploratory simulation in which we randomly drew 10,000 pairs of participants, one from the ingroup condition and one from the outgroup condition. For each pair, we then assessed how often the participant from the ingroup condition selected more ingroup sources than the participant from the outgroup condition (McGraw & Wong, 1992). Participants in the ingroup condition selected more ingroup sources than their randomly selected match in the outgroup condition 70.8% of the time, and the reverse just 13.1% of the time (the remaining 16.0% of pairs selected an equal number of ingroup sources). Taken together, our data are consistent with the hypothesis that, in partisan environments, reputational considerations powerfully drive information selection decisions.

Next, we turned to the primary question being tested in this experiment: comparing both public conditions to the private control condition. Actors in the private control condition, on average, selected 3.42 ingroup advisors ($SD = 1.21$). When we regress the number of ingroup sources selected on condition (private vs. ingroup vs. outgroup), where the private condition was set as the reference group, we see that actors in the public ingroup condition viewed an average of 0.64 more ingroup sources than those in the private control condition ($p < .001$). Additionally, those in the private control condition viewed 1.06 more ingroup sources than those in the public outgroup condition ($p < .001$), on average. Results are presented in Figure 1 and in Table A3 of the Appendix. Taken together, our data are consistent with the hypothesis that, in partisan environments, reputational considerations drive information selection decisions—and this is true when being evaluated by both ingroup and outgroup observers.

It is worth noting that, in exploratory analyses, the overall average number of ingroup information choices chosen was 3.28, which is significantly above 2.50, the amount that would have represented even exposure to ingroup and outgroup sources, $t(601) = 12.35$, $p < .001$.

Figure 1

Number of Chosen Ingroup Information Sources by All Conditions



Note. Error bars represent ± 1 *SE* of the group mean. Participants in the ingroup condition chose more ingroup information than those in the private condition, who chose more ingroup information than those in the outgroup condition, on average. There were no significant ($p = .05$) differences between the information type conditions.

However, in the presence of an outgroup observer, this mean falls to 2.36, and becomes indistinguishable from the 2.50 baseline, $t(201) = -1.21$, $p = .23$. This suggests that at least in this context, reputational incentives were powerful enough to eliminate the tendency toward selective exposure.

Information Source

We next confirmed that this pattern of results persisted for both types of information (i.e., senators' webpages vs. news stories), a secondary hypothesis in our preregistration. Specifically, we tested a regression of the number of ingroup sources selected on an unordered factor representing ingroup, outgroup or private condition (where the private condition was again set as the reference group). We repeated this regression both for those participants in the senators condition and the news condition. When restricting the data to the senators condition, we saw that actors in the public ingroup condition viewed 0.54 more ingroup sources than those in the private control condition ($p = .007$), in which actors viewed 1.16 more ingroup sources than those in the public outgroup condition ($p < .001$), on average. We observed similar results for the news condition: actors in the public ingroup condition viewed 0.72 more ingroup sources than those in the private control condition ($p < .001$), in which actors viewed 0.98 more ingroup sources than those in the public outgroup condition ($p < .001$), on average. Furthermore, in exploratory analyses using all of the data, we added a dummy coded variable for information type and found no evidence of an interaction between social incentives and information type ($\beta_{\text{ingroup} \times \text{source}} = -.18$, $p = .52$; $\beta_{\text{outgroup} \times \text{source}} = -.17$, $p = .53$). Thus, it appears that signaling

motivations for selective exposure generalize across multiple information types. We detail these results in Figure 1 and Table A3 of the Appendix.

Discussion

Experiment 1 supported the hypothesis that individuals' information selection decisions are sensitive to observation using a traditional selective exposure paradigm. Actors selected more ingroup information sources when observed by ingroup members in comparison to being observed by outgroup members, with actors in a private control condition falling between. Of note, this effect persisted across information associated with specific individuals (U.S. Senators) as well as more general information reported in news articles.

These results raise the question of whether there are in fact complementary reputational benefits accrued to those who engage in selective exposure. Do observers differentially respond to people's strategic information selection decisions, and if so, to what extent? We begin to address these questions in Experiment 2, where observers consider profiles of actors and choose whether or not to trust them in a financially incentivized economic game.

Experiment 2

In Experiment 2, we shifted our focus from testing whether individuals change their information selection decisions to signal to observers toward investigating whether observers reward such behavior. Participants in Experiment 2 (whom we call "observers") chose how much money to send to participants in a financially incentivized trust game based on the actor's information consumption choices. We were interested in whether observers were more likely to trust actors who selected more information sources congruent with the observer's own ingroup, across types of information.

To get observers to consider the behavior of real actors, we randomly exposed them to profiles of actors who had engaged in different levels of selective exposure (i.e., selected from 0 to 5 ingroup sources). These participants come from the replication study described in Appendix B, which had a parallel design to Experiment 1. This natural variation also allowed us to also examine whether there is an optimal level of selective exposure that observers prefer.

Method

Participants

We recruited a sample of U.S. residents from Cloud Research to participate in a 3- to 4-min experiment in exchange for $0.50, with further opportunities for a bonus. Our final sample consisted of 671 participants (M_{age} = 40.3; 56.3% female, 42.8% male, 0.9% nonbinary; 63.9% liberal, 36.1% conservative). To achieve 80% power to detect a small to medium-sized main effect ($d = .40$) across number of ingroup sources (collapsing across information type conditions), we aimed to recruit about 100 participants per experimental cell.

Procedure

Upon entering the study, participants first reported basic demographic information, including gender, age, and education. They again indicated their political ideology on a 7-point scale from *extremely liberal* to *extremely conservative*. After reporting demographics, participants read about the trust game and had to correctly answer two comprehension check questions before proceeding through the study. Participants were then shown their partner's information selections and played a trust game, described in detail below.

Dependent Variable. We informed participants that they could decide how much money to send to a partner based on the partner's choices in a previous task. Observers' choice of how much money to send to their partner (between 0 and 10 cents) served as our dependent variable. Further implementation details can be found in Table A1.

Independent Variables. Before viewing their partner's information selections, observers were randomly assigned to one of two between-subjects experimental conditions: senators versus news articles. These conditions corresponded to whether they saw the behavior of an actor from the senators or news articles conditions described in Experiment 1. Within each of these conditions, participants saw a partner who selected between 0 and 5 sources aligned with their own (observer's) ingroup perspective, chosen from the original list of 10.

Analysis Plan

Our primary dependent measure was the amount of money that observers chose to send to the actor whose choices they observed. This number ranged from zero to 10 cents. Thus, we used the lm function in R to regress how many cents the observer sent on the number of information sources aligned with the observer's ingroup that the actor selected (from 0 to 5). We predicted a positive relationship, such that increased consumption of ingroup-aligned information sources would increase trusting behavior.

Results

Reputational Consequences of Selective Exposure

We first examined whether observers sent more money to actors who chose more sources from the observer's ingroup. We found this to be the case: In a linear regression, choosing one more source from the observer's ingroup increased the amount sent by an average of .56 cents, $R^2 = .07$, $F(1, 669) = 54.75$, $p < .001$. Concretely, if an actor chose zero sources from the observer's ingroup, they were sent an average of 4.40 cents ($SD = 3.52$). However, if an actor selected all five sources from the observer's ingroup, this increased to an average of 7.13 cents ($SD = 3.14$). This effect persisted across both the news condition, $\beta = .54$, $R^2 = .07$, $F(1, 333) = 25.39$, $p < .001$, and the senators condition, $\beta = .58$, $R^2 = .08$, $F(1, 334) = 29.09$, $p < .001$.

We also used a logistic regression to examine whether the observer sent the actor any amount greater than zero. Here, we see that each additional ingroup source chosen multiplied the odds of the observer trusting the actor by 1.57 ($p < .001$). Thus, catering information selection decisions toward an observer's point of view enhanced perceptions of the actor's trustworthiness and increased the actors' chances of earning a bonus. See Table A4 in the Appendix for full results and statistics.

Information Source

In preregistered exploratory analyses, we examined whether there was a difference between the senators and news conditions. If observers were conditioning their trust on whether actors were information-seeking from individuals with a specific partisan

identity (senators condition) versus from information arising from an organization with a specific partisan identity (news articles condition), we would observe an interaction between the type of information source and trust. However, when using a linear regression to predict how much the observer sent to the actor on the number of sources selected from the observer's ingroup, a dummy-coded variable indicating news or senators condition, and their interaction, we found no evidence of such an interaction ($\beta_{interaction} = .04$, $p = .80$). See Table A4 in the Appendix for full results and statistics.

Magnitude of Selective Exposure

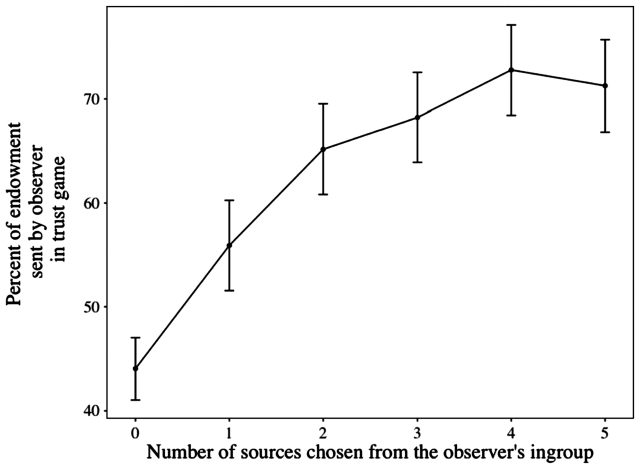
So far, our results have demonstrated that observers were more likely to choose actors who select information from sources consistent with the observer's ingroup perspective. However, does the extremity of the actor's preference matter? On the one hand, it could be the case that observers favor actors who fully embrace the observer's ingroup perspective at the expense of all outgroup perspectives. On the other hand, it could be the case that observers, to at least some extent, value actors who balance ingroup and outgroup information sources.

To address this question, we conducted an exploratory analysis to examine the amount of money that an observer sent to an actor based on the number of sources from the observer's ingroup that the actor selected (a number that could range from 0 to 5). We hypothesized that there might be a cost to selecting all sources from one group. Results are presented in Figure 2. To analyze the statistical significance of these results, we regressed how much the observer sent on a factor representing the number of observer's ingroup sources that the actor had selected. Furthermore, to test for differences between specific numbers of sources (e.g., three and four), we ran subsequent linear hypothesis tests.

Two results clearly stand out from this visualization and are borne out by inferential analyses. First, as described above, we see a generally increasing line, showing that observers demonstrated the expected preference for actors who selected more information from the observer's ingroup. Second, and perhaps more surprisingly, we found that there seem to be diminishing marginal returns to the number of observer's ingroup information sources selected. Specifically, if an actor selected zero sources from the observer's ingroup, then the observer only sent 44% of their endowment, on average. However, if the actor selected just one source from the observer's ingroup, the endowment sent significantly increased to 56% ($p = .007$). The endowment sent further increased to 66% with choosing two sources (linear hypothesis test compared to one source: $p = .04$). The amount sent continued to follow a positive trend when choosing three sources (68% of the endowment; linear hypothesis test compared to two sources: $p = .49$) and four sources (73% of the endowment; linear hypothesis test compared to two sources: $p = .31$) from the observer's ingroup. When the actor selected the maximum possible number of sources from the observer's ingroup (five), the amount sent to them slightly *decreased* to 71% from 73% of the endowment (linear hypothesis test compared to four sources: $p = .74$). Thus, while participants demonstrated a general preference for like-minded others, there seemed to be diminishing marginal returns to the social rewards based on number of the observer's ingroup sources chosen—returns that are eliminated, and directionally reversed, at the extreme. Full results and statistics presented in Table A5 of the Appendix.

Figure 2

Average Amount of Money Sent by the Observers in the Trust Game as a Function of the Number of Information Sources Aligned With the Observer's Ingroup Chosen by the Actor



Note. Error bars represent ± 1 *SE* of the group mean. Actors who chose more sources from the observer's ingroup were generally trusted more, although these benefits declined with more ingroup sources being chosen.

Discussion

Experiment 2 demonstrated the reputational consequences of information selection decisions, providing additional evidence for the signaling model of selective exposure. Specifically, observers were more likely to reward actors who selected more sources from the observer's ingroup. This result was not dependent on whether the actor was selecting information from a specific person (a senator) or an impersonal source (a news article).

Additionally, observers were responsive to the magnitude of selective exposure demonstrated by the actor. While observers tended to reward choosing more of their ingroup sources, there appear to be diminishing marginal returns to these choices. The recognition that although selective exposure is generally rewarded, there is a limit to such benefits, adds important nuance to our understanding of the phenomenon.

Thus far we have examined contexts in which the actors only had one incentivized goal: to earn the trust of observers. However, information often has instrumental value, helping individuals to make better decisions or hold more accurate opinions. Therefore, in Experiments 3–5, we develop a novel paradigm to explicitly include this trade-off between reputational and accuracy incentives related to information selection. Additionally, we vary the type of future task that the actors and observers expect to collaborate on to explore additional moderators of the signaling model.

Experiment 3

Experiments 1–2 drew on standard selective exposure paradigms to provide an initial test of the reputational causes and consequences of selective exposure. In Experiments 3–5, we develop a novel, stylized, incentive-compatible paradigm that directly pits reputational

incentives against the accuracy incentives that are typically present outside of laboratory settings.

Experiment 3 begins by examining the actor side of the model (parallel to Experiment 1). In this experiment, all participants (actors) made incentivized estimates about the proportion of respondents in a prior survey who supported specific policies. Critically, before making their estimates, actors had the opportunity to consult additional information from ideologically aligned versus unaligned others.

In addition to financial incentive for accuracy, Experiment 3 builds on Experiment 1 in another important way. While Experiment 1 revealed that incentives to appear trustworthy shifted behavior, trustworthiness is not the only characteristic that people use to select collaborators. Thus, in Experiment 3 we systematically manipulated whether participants expected to be evaluated for a future collaborative task that relied on trustworthiness or on quantitative judgment skill.

Based on the social signaling model, we hypothesized that actors being observed by ingroup members would select more ideologically aligned information than actors in the private condition, who would in turn select more ideologically aligned information that actors being observed by outgroup members.

Our procedure continues to build upon prior research methodologies that evaluate selective exposure by offering actors a diverse menu of information (Dorison et al., 2019; Iyengar & Hahn, 2009; for review, see Stroud, 2014). Importantly, however, some prior research has manipulated the perceived usefulness of information (e.g., by asking participants to write a pro- or antiattitudinal essay after making their information consumption choices), finding that an increased accuracy motivation decreases selective exposure (Freedman, 1965; Hart et al., 2009). We extend this prior methodology by providing actors with information which has a clear impact on the accuracy of their incentivized judgments—the opinions of individuals from the population whose attitudes they are trying to estimate. This allows us to capture the tension that individuals in the world may experience between the desire to form accurate beliefs and the desire to enjoy the reputational benefits of consuming information aligned with an observer's ingroup. Furthermore, we extend our prior studies by varying the type of future task for which the observers are considering the actors. Namely, we were curious whether actors would attempt to signal different characteristics when being evaluated for a task reliant on trustworthiness versus one reliant on judgment skill.

Method

Participants

We recruited a sample of U.S. residents from Cloud Research to participate in a 15-min experiment in exchange for $1.50, with further opportunities for a bonus. Our final sample consisted of 883 participants (M_{age} = 42.2; 52.4% female, 47.1% male, 0.5% nonbinary; 52.4% liberal, 47.6% conservative). In a pilot study, we observed a standardized effect size of approximately 0.30 for the main hypothesis of interest; our final sample thus achieved greater than 80% statistical power.

Procedure

Upon entering the study, actors reported basic demographic information, including gender, age, education and the name of their hometown. As in Experiments 1–2, they indicated their political ideology on a 7-point scale from *extremely liberal* to *extremely conservative*.

Participants then read and indicated their own opinion ("yes" or "no") on eight policy statements related to current social and political issues debated in the United States (e.g., "the death penalty should be abolished in all U.S. states"; see full list of issues in Table A6 of the Appendix). After indicating their own opinion, participants estimated the percentage of prior participants who reported agreeing or disagreeing with each of the eight policy statements. We truthfully informed participants that the people whose policy preferences they were estimating contained roughly equal proportions of liberal and conservative Cloud Research participants. Finally, when relevant, participants reported how much they would return to an observer in a trust game.

Dependent Variable. Actors then engaged in the central task of the study: selecting advisors and revising their estimates based on the advice from other Cloud Research participants. Specifically, we informed actors that to increase their accuracy, they could view the opinions of three advisors (participants from the sample whose opinion the participants were estimating). Their selection of which three advisors' opinions to view out of a possible set of six served as our dependent variable.

For each policy statement, we presented the actors with basic information about six advisors including name, age, hometown, and political affiliation. The actors were then required to select three advisors whose opinions ("yes" or "no") they could view before revising their estimate.[4] While the simple "yes" or "no" answer is devoid of much of the emotionally charged content in traditional selective exposure paradigms (like those used in Experiments 1–2), we aimed to isolate the causal effects of interest (e.g., the impact of social vs. accuracy incentives) as precisely as possible, in accordance with other laboratory experimental designs (Falk & Heckman, 2009). In this scenario, because the advisor is a member of the population whose beliefs on a policy topic are being estimated, their opinion has clear value for improving the accuracy of the actors estimate. In the General Discussion section, we explore contexts when this type of information may be representative of real-world selective exposure scenarios.

In each set of six potential advisors, half were labeled as "conservative" and the other half were labeled as "liberal." The distribution of the "yes" versus "no" opinions attributed to each of the liberal or conservative advisors matched the real distribution of opinions from the Cloud Research participants surveyed in the prior sample. For example, if a policy statement was supported by 67% of conservatives in the prior sample, then two out of three conservative advisors presented to participants also expressed support for the policy statement. The advisors were presented with fictional names, ages, and hometowns so that this information could be counterbalanced between the liberal and conservative advisors. We incentivized estimation accuracy by entering actors into a raffle for $100 for estimates

[4] In standard judge-advisor paradigms, the advisor typically tells the participant their estimate of the quantity in question. In contrast, rather than advisors giving an estimate of the percentage of people who agreed with particular policy statements, the actor was informed of the advisor's own opinion ("yes" or "no") on the policy statement. Given that in our experimental paradigm the advisor is a member of the population whose views are being estimated, this information has clear value for improving accuracy.

that were within 10% of the correct answer. Multiple accurate estimates led to multiple raffle entries.

Independent Variables. Before making their advisor selections, actors were randomly assigned to one of five between-subjects experimental conditions in a 2×2 factorial design with an extra untreated control condition.

For the first factor, actors were truthfully told that their selection of advisors would be shown to observers who would then decide whether to work with them or another participant on a future task. We varied whether the observer picking them for the future task was described as a political ingroup member (public ingroup condition) or a political outgroup member (public outgroup condition). We truthfully told actors that being chosen for the future task by the observer would lead to an additional raffle entry. Thus, while all actors were financially incentivized for accuracy, the experimental conditions systematically tied the reputational gains that might arise from signaling to additional financial incentives.

The second factor varied the type of future collaborative task for which the actors were told they were being chosen. Specifically, the observers chose an actor for a future collaborative task reliant on either trustworthiness (trust condition) or judgment skill (judgment condition). Actors in the trust condition read a description of the trust game (Berg et al., 1995) and actors in the judgment condition read a description of an estimation task similar to the one they had just completed. If chosen, they were told that they would play a subsequent trust or estimation game with the observer. Being chosen by the observer would lead to a further bonus opportunity. Further implementation details can be found in Table A1.

In the private control condition, actors were told that viewing the responses of the other participants could help them make more accurate estimates; however, they did not have to consider how their choices would be evaluated by another individual. Since there was no observer mentioned in this condition, we could not systematically vary the type of future collaboration task in this condition.[5] Therefore, this control condition only included the accuracy incentive and had no associated reputational incentives, allowing us to precisely identify the effect of those incentives on behavior.

In sum, actors in all conditions had an accuracy incentive. However, being observed by an ingroup or outgroup member also created a social incentive to the extent that actors viewed their selections as signaling information that may be relevant for a future bonus opportunity.

Analysis Plan

Our primary dependent measure was the number of ingroup advisors that actors consulted for each of the eight estimates. This number ranged from zero to three and was represented as an ordinal factor. Thus, as detailed in our preregistration, we used an ordinal logistic regression, using the clm function in R (Christensen, 2018). We also included participant-clustered *SE*s, necessary because each participant provided eight estimates. Since we had eight different policy topics, we used fixed effects to control for the effect of each.[6]

Results

Reputational Causes of Selective Exposure

We began by examining selective exposure in the public conditions. We found evidence consistent with the results of Experiment 1. On average, actors in the public ingroup condition selected 1.56 ingroup advisors ($SD = 1.00$). In contrast, participants in the public outgroup condition selected just 1.15 ingroup advisors ($SD = .92$), on average. Applying the analysis strategy described above and restricting the data to the two public conditions (employing dummy codes to contrast the public ingroup and public outgroup conditions), we found that participants in the public ingroup condition were on average 2.16 times more likely to consult an ingroup opinion than those in the public outgroup condition ($p < .001$). Results are presented in Table A7 of the Appendix, including robustness checks using linear regression analyses.

Actors in the private control condition, on average, selected 1.35 ingroup advisors ($SD = .94$). Interestingly, in exploratory analyses, the average number of ingroup advisors selected in the private condition fell somewhat below 1.5, or 50%, $t(1,399) = -5.78$, $p < .001$. On the one hand, this pattern is in line with prior research demonstrating that selective exposure is attenuated in the presence of accuracy goals (Freedman, 1965; Hart et al., 2009). It is also worth considering, however, that participants already had access to one ingroup opinion—their own. Thus, to maximally diversify the information available to them, these participants perhaps should have relied even more heavily on outgroup information.

We then compared information selection behavior in the private condition to the public conditions. Specifically, we used the analysis plan described above to regress the number of ingroup sources selected on a factor variable representing condition (private vs. ingroup vs. outgroup), where the private condition was set as the reference group. Here, we see that for participants in the public ingroup condition, the odds of picking more ingroup opinions was 1.470 times that for those in the private condition ($p < .001$), on average. Additionally, for participants in the private condition, the odds of picking more ingroup opinions was 1.475 times that of participants in the public outgroup condition ($p < .001$), on average. Results are presented in Figure 3 and in Table A7 of the Appendix, which includes robustness checks using linear regression analyses. Taken together, our data are consistent with the hypothesis that, in partisan environments, observations of both ingroup and outgroup observers drive information selection decisions.

Decision Context

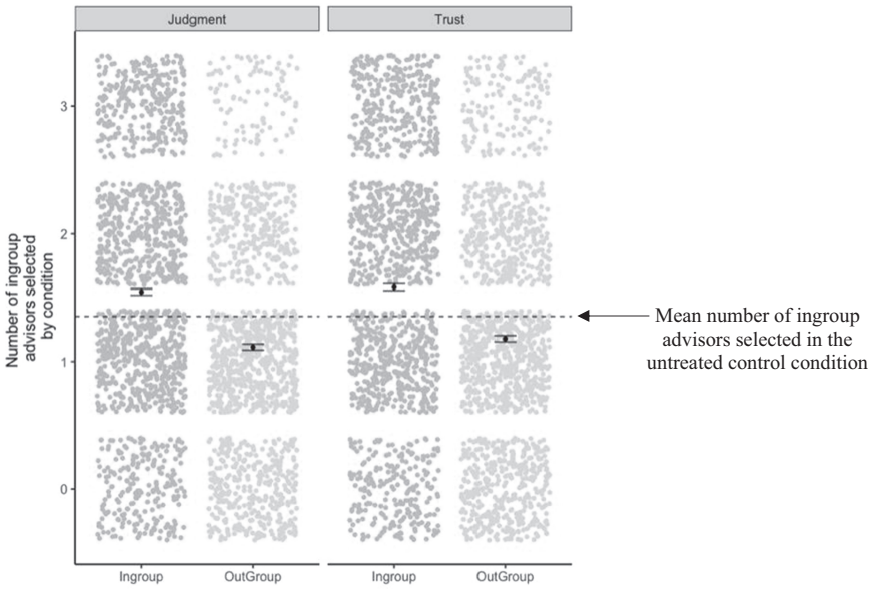
We next examined whether the actors' advisor selections were contingent on their expectations of being chosen for a future trust game versus a future judgment task. If actors were attempting to signal a

[5] Note that while a $2 \times 2 \times 2$ design might be expected here with the final factor varying whether an actor's choices of advisors are shown to observers or not, a mere presence of another can be enough to change behavior (Lerner & Tetlock, 1999). Therefore, we follow common practice in testing interpersonal explanations by not mentioning an observer at all in the private condition (Leary et al., 2015).

[6] To interpret the constant in the regression model (e.g., the private condition when of interest), we applied a simple effects coding contrast to the topic variable. This coding scheme allows us to interpret the intercept (or constant) of the model as the grand mean rather than the mean of a reference topic (Hardy, 1993). Without this, the intercept corresponds to the mean of a reference group (e.g., the intercept would correspond to a topic such as "The death penalty should be abolished in all U.S. states"). With the simple effects contrast, the intercept of the regression model becomes the grand mean of all topics in the reference condition (UCLA: Statistical Consulting Group, n.d.). This coding matrix does not impact the interpretation of any coefficients in the model other than the intercept.

Figure 3

Number of Ingroup Advisors Chosen by Condition



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant. The dotted line represents the mean number of ingroup advisors chosen in the private condition. Participants in the public ingroup conditions chose more ingroup advisors than those in the private condition, who chose more than those in the public outgroup conditions. However, there were no significant differences between game type or interaction between game type and observer identity.

specific characteristic such as trustworthiness or judgment skill, we would observe an interaction between game type and observation condition. We preregistered as a secondary hypothesis that the effect of choosing more ingroup information when being observed by an ingroup member might be stronger when actors expected to play a future trust game. To test this, we again used the ordinal logistic regression described above, again restricting our analysis to the public conditions and employing dummy codes to contrast the trust game and estimation game conditions. Here, we found no evidence of such an interaction ($\beta_{interaction} = .03$, $p = .88$).[7] Instead, actors selected more ingroup advisors when being observed by ingroup members, irrespective of the task for which they were being selected. Results are presented in Figure 3 and Table A7 of the Appendix, including robustness checks using linear regression analyses.

Estimation Error

Finally, because our participants made estimates prior to consulting any additional information, we can also examine the consequences of their selections for the error of their final estimates. To calculate error, we took the absolute difference between each actor estimate and the true proportion of prior Cloud Research participants who expressed a particular policy opinion. We then *z*-scored these absolute errors within estimation topic and across conditions. Error scores ranged from −1.46 to 4.35, where lower numbers indicated lower error (i.e., greater accuracy).

A linear regression of the *z*-scored error on the number of ingroup advisors selected revealed that the selection of each additional ingroup advisor was associated with an increase in estimation error ($\beta = .07$, $p < .001$). Specifically, the lowest estimation error was associated with choosing all outgroup advisors, with a sharp increase in error associated with selecting just one ingroup advisor. Results are presented in Figure 4 and Table A8 of the Appendix. All following analyses in this section were exploratory.

Surprisingly, we did not observe differences in estimation error between conditions ($M_{ingroup} = .007$, $M_{outgroup} = .015$), $t(5{,}662) = -0.33$, $p = .740$. These results are intriguing given that (a) we observed differences in information selection decisions across conditions and (b) information selection decisions predicted estimation error. Why then, was there no effect of condition on accuracy?

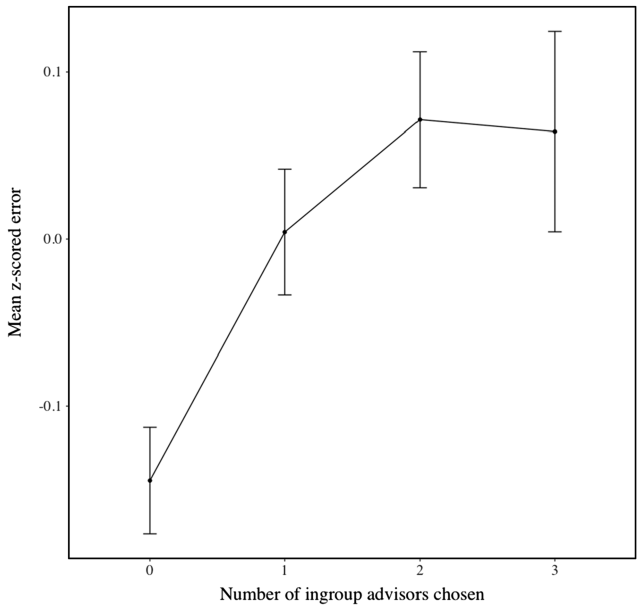
To investigate this null effect, we examined the weights that actors placed on the information they selected. Unsurprisingly, estimates were influenced by advice: For every chosen advisor that said "yes" in response to a policy question, the actors' estimate of the number of prior Cloud Research participants that said "yes" to that question increased by an average of 2.06 points ($SD = .48$).

Crucially, however, actors gave greater weight to the opinions of ingroup advisors than outgroup advisors. For each outgroup advisor that said "yes" to a policy question, actors' estimates increased by an average of 1.52 percentage points. By contrast, when an ingroup advisor said "yes" to a policy question, the actor's estimate increased

[7] While there is a directional main effect of the actor selecting more ingroup sources in the trust condition in comparison to the judgment condition, this pattern did not reach traditional levels of statistical significance.

Figure 4

Mean of the z-Scored Errors (z-Scored Within Topic and Across Conditions) by the Number of Ingroup Advisors Chosen



Note. A *z*-score of zero refers to the average level of error for that topic. Error bars represent ± 1 *SE* of the group mean, clustered by participant. Participants who chose a greater number of ingroup advisors produced estimates with higher error.

by an average of 2.79 percentage points—almost double. Similarly, for each outgroup advisor that said "no," to a policy question actors' estimates decreased by 2.30 percentage points. When an ingroup advisor said "no" to a policy question, however, the actor's estimate decreased by 4.13 percentage points. Taken together, these results indicate that participants placed greater weight on ingroup advice than outgroup advice (interaction between an advisor saying "yes" and the advisor being from the same side: $p < .001$; see Figure A1 and Table A9 of the Appendix). Importantly, updating one's estimate—as measured by the change in one's estimate from before to after seeing the opinion of the advisor—was associated with less *z*-scored error ($\beta = -.004$, $p < .001$).

In sum, while participants in the outgroup condition selected more outgroup sources, they also updated their estimates less based on these sources, foregoing the potential accuracy benefits. However, given our lack of statistical power to detect a difference in estimation error between conditions, this pattern should be interpreted with caution and presents a fruitful avenue for future research.

Discussion

Building on Experiment 1, Experiment 3 provided further support for the hypothesis that individuals' information selection decisions are sensitive to observation. Actors selected more ingroup advice when observed by ingroup members and more outgroup advice when observed by outgroup members—all compared to a private control condition. This effect persisted across tasks reliant on trustworthiness or judgment skill. Our novel incentivized design directly pitted an accuracy incentive against possible reputational benefits. Not surprisingly, we found that greater selective exposure carried accuracy costs.

In Experiments 4–5, we return to the observer side. Do observers respond to people's strategic information selection decisions, and if so, to what extent? Are observers sensitive to the type of task on which they expect to collaborate with the actor? Experiment 4's observers consider profiles of actors from Experiment 3.

Experiment 4

In Experiments 4–5, we shift back to examining whether actors' beliefs about the reputational benefits of selective exposure are correct. That is, rather than considering the reputational causes of selective exposure, we again consider the reputational consequences. Specifically, participants in Experiment 4 (observers) picked a partner for a future collaborative task from among pairs of actors. We were interested in whether observers were more likely to choose actors who selected a greater number of information sources from the observer's ingroup.

The extent to which observers are likely to favor collaboration partners who viewed more ingroup information could vary with the type of collaboration they expect to engage in. Therefore, in Experiment 4, we again varied the type of task for which the observers were picking partners (i.e., judgment skill vs. trust). As observers considered the behavior of real actors from Experiment 3, they were exposed to profiles of actors who had engaged in different levels of selective exposure (i.e., selected from 0 to 3 ingroup advisors). This natural variation allowed us to further examine whether the diminishing returns to the greater levels of selective exposure that we observed in Experiment 2 would be robust to our new paradigm.

Method

Participants

We recruited a sample of U.S. residents from Cloud Research to participate in a 5-min experiment in exchange for $0.50, with further opportunities for a bonus. Following our preregistration, our final sample consisted of 459 participants ($M_{age} = 43.1$; 53.4% female, 46.2% male, 0.4% nonbinary; 54.0% liberal, 46.0% conservative). We chose our sample size to achieve greater than 80% statistical power to detect the effect size found in prior studies we conducted which tested a similar hypothesis.

Procedure

In Experiment 4, we showed a new sample of participants (observers) the choices that actors made in Experiment 3. We then examined how the observers reacted to the actors' information selection decisions.

Dependent Variable. Observers learned that their goal in the study was to pick a partner for a future collaboration task from among two individuals. Recall that in Experiment 3, actors selected the advice of three advisors from a total of six possibilities. We randomly selected two actors from Experiment 3 and showed their choices of liberal versus conservative advisors to the observers in this study. The dependent variable in Experiment 4 was whether the observer chose to work with an actor who had selected a greater number of advisors from the observer's ingroup than their counterpart. Observers made eight choices corresponding to the policy topics used in Experiment 3. We truthfully told observers that we would implement the outcome of one of their eight choices (picked at random). Further implementation details can be found in Table A1.

Independent Variable. As in Experiment 3, we varied the future collaborative task for which the observer was choosing the actor. Participants either chose actors to be future collaborators on a task reliant on trustworthiness (trust condition) or a task reliant on judgment skill (judgment condition). Participants in the trust condition read a description of the trust game and learned that they would play a subsequent incentivized trust game with one of the eight actors who they picked. By contrast, participants in the judgment condition read a description of the estimation task from Experiment 3 and learned that their bonus would be tied to the judgment accuracy of one of the eight actors who they chose.

After making eight choices between potential partners, observers reported demographics, which included the same political ideology measure used in prior experiments: a 7-point scale from *extremely liberal* to *extremely conservative*.

Analysis Plan

We recoded the choices made by actors in Experiment 3 to reflect how many of their selected advisors belonged to the observer's ingroup (i.e., the participant in the present experiment). The two actors displayed to each observer were randomly chosen. There were 998 pairings where the two actors selected identical numbers of observer ingroup advisors, which were dropped from the subsequent analyses. This resulted in a final set of 2,674 choices made by observers.

Our primary dependent variable was binary (1 = *observer chose the actor who consulted more of the observer's ingroup advisors*, 0 = *observer chose the actor who consulted fewer of the observer's ingroup advisors*). Thus, in all of the analyses below, we used an ordinal logistic regression, using the clm function in R (Christensen, 2018). As in Experiment 3, we also included participant-clustered *SE*s, necessary because each participant made eight choices. Since we had eight different policy topics, we used fixed effects to control for the effect of each of these, again with use of a simple effects contrast matrix in order to interpret the intercept of the model as the grand mean rather than the mean of a reference topic.

Results

Reputational Consequences of Selective Exposure

We first examined whether observers preferred to collaborate with actors who consulted more advisors from the observer's ingroup. We found this to be the case 70.9% of the time, a frequency substantially greater than chance. When using the analytic strategy described above, the odds of actors who selected a greater number of the observer's ingroup advisors being chosen for future collaboration was 2.44 times that of their counterpart ($p < .001$). Thus, catering information selection decisions to the observer greatly enhanced actors' chances of being chosen for an additional bonus opportunity.

Decision Context

We next examined the effect of selecting advisors from the observer's ingroup when choosing a partner for a future trust game versus a future judgment task. To test this, we included a dummy-coded variable for task type in the regression model. Although participants picked the actor who consulted more ingroup sources at greater than chance levels in both conditions, this tendency was substantially more pronounced in the trust condition ($OR = 1.93$, $p < .001$). Specifically, in the trust condition, observers chose the actor who had selected more of the observer's ingroup advisors 77.1% of the time. However, in the judgment condition, observers chose the actor who had selected more of the observer's ingroup advisors only 63.5% of the time (see Figure 5 and Table A10 of the Appendix). This difference suggests that although individuals prefer collaborators who favor information from their ingroup, they recognized that judgment accuracy demands some exposure to a variety of perspectives.

Of note, actors in Experiment 3 appeared insensitive to this difference, expecting observers to equally favor selection of ingroup information for both tasks. Thus, while both actors and observers appreciated that selecting sources congruent with observers' preferences would yield reputational rewards, actors' choices appeared insufficiently sensitive to the context (in this case, the nature of the upcoming collaboration task), perhaps leading them to sacrifice judgment accuracy for little reputational gain. We discuss potential causes and consequences of such an asymmetry in the General Discussion section.

Magnitude of Selective Exposure

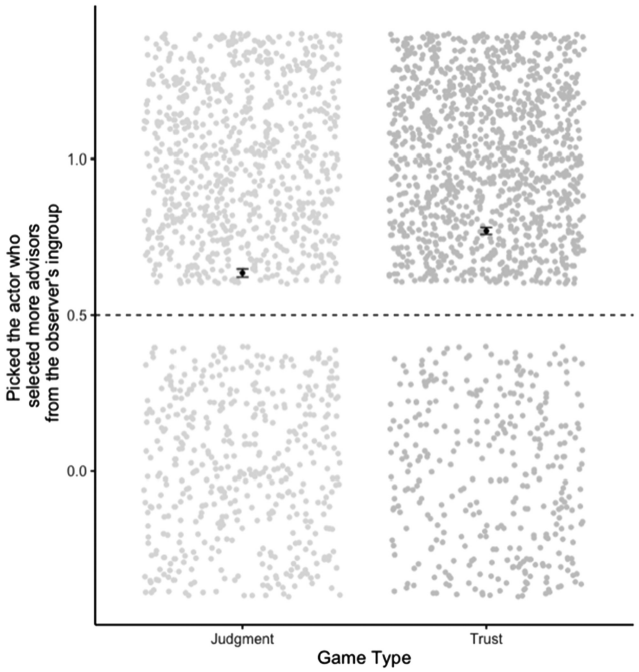
So far, our results have demonstrated that observers were more likely to choose actors who prefer advice from the observer's ingroup. However, does the extremity of the actor's preference matter? To address this question, in exploratory analyses, we examined the probability of an observer choosing an actor based on the number of advisors from the observer's ingroup that the actor selected (a number that could range from 0 to 3).[8] Results are presented in Figure 6. Using this alternative analytical approach, we again found that observers demonstrated the expected preference for actors who selected more advisors from the observer's ingroup, similar to the results reported above.

Perhaps more surprisingly, we also found that observers showed a preference for some information diversification—punishing those actors who selected all of their advisors from the observer's ingroup. Specifically, if an actor selected zero advisors from the observer's ingroup, then the probability that this actor was selected over their counterpart was only 18.7%. However, if the actor selected just one advisor from the observer's ingroup, this probability dramatically increased to 45.4%. The probability of being chosen further increased to 71.3% if the actor selected two advisors from the observer's ingroup. These increases demonstrate a clear social benefit to actors who consulted a greater number of advisors aligned with the observer. However, when the actor selected the maximum possible number of advisors from the observer's ingroup (three), the probability that they were chosen decreased to 58.1%. Thus, while participants demonstrated a general preference for like-minded others, this preference was tempered by a surprising willingness to reward those who selected at least some information from the outgroup. To test the statistical

[8] In our pre-registration, we detailed testing this question by comparing whether an actor's choice to view at least one information source from the observer's ingroup would be a better predictor than the difference score between actors' choices. However, upon reflection, we think that the below analyses and graphs are a clearer representation of our results. We include the preregistered analyses in the Appendix.

Figure 5

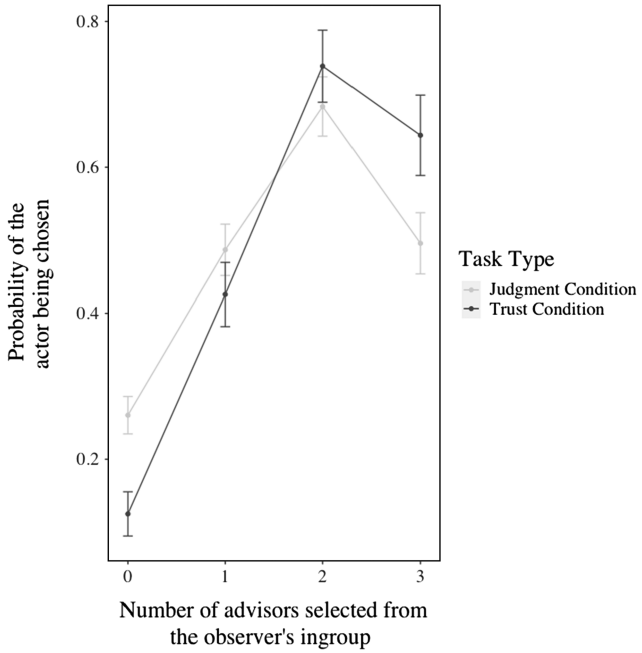
The Mean Probability That Observers Chose the Actor Who Selected a Greater Number of Advisors From the Observer's Ingroup, by Condition



Note. The dotted line at 0.50 represents chance levels. Error bars represent ± 1 *SE* of the group mean, clustered by participant.

Figure 6

Mean Probability That an Actor Was Chosen for a Future Task Based on the Number of Advisors That They Selected From the Observer's Ingroup, as a Function of Task Type



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad.

significance of these results, in exploratory analyses, we regressed a binary variable indicating whether or not the actor was chosen for the collaborative task on a factor representing the number of observer's ingroup advisors that the actor had selected, finding that each of these means were statistically different from the others (results presented in Table A11 of the Appendix).

Although this general pattern persisted across game type, actors received a greater benefit for each additional advisor selected from the observer's ingroup when expecting to play a future trust game than when expecting to play an estimation game (see Figure 6 and Table A11 of the Appendix). Furthermore, when expecting to play a trust game, actors who selected all of their advisors from the observer's ingroup were penalized less than in the estimation game. This suggests that observers valued an actor's information diversification more when anticipating performing a task reliant on the actor's judgment skill rather than their trustworthiness.

One concern with the above analysis is that the reported result could depend on the frequency with which observers evaluated particular pairings of actors. For example, actors might have more frequently chosen one advisor from the observer's ingroup than three. Since we randomly presented two real actors' selections of advisors to observers, this would imply that observers would be faced with the decision between an actor who selected one ingroup advisor and an actor who selected two ingroup advisors more frequently than the decision between an actor who selected two ingroup advisors paired with an actor who selected three ingroup advisors. Thus, a simple preference for the actor who chose more ingroup advisors could yield the graph above.

To address this concern, we examined the observer's choice depending on the selections of both actors in a given pair (see Table 1). When one actor in a pair chose zero ingroup advisors, the other actor was more likely to be picked if they selected one or two advisors from the observer's ingroup than if they selected three. In other words, the other actor was worst off if they chose all three advisors from the observer's ingroup. When one actor chose one ingroup advisor, the other actor was again worst off by choosing three advisors from the observer's ingroup. Finally,

Table 1

Observer's Choice Depending on Selections of Both Actors in a Pair

Actor's choices of observers' ingroup advisors	0 vs. 1	0 vs. 2	0 vs. 3	1 vs. 2	1 vs. 3	2 vs. 3
Percent of observers who chose the actor with more ingroup cards	84	81	76	76	59	47
N	456	407	232	743	429	407

Note. The top row represents all possible combinations of the two actors' selections (e.g., in the "0 vs. 1" column, the observer chose between an actor who selected zero advisors from their ingroup and an actor who selected one advisor from their ingroup). The middle row represents the percentage of observers who chose the actor selecting a greater number of advisors from the observer's ingroup. The bottom row is the number of times each pairing appeared. Due to our random sampling strategy, observers were presented with the choice between an actor who selected one and an actor who selected two advisors from the observer's ingroup most often.

when one actor chose two advisors from the observer's ingroup, the same pattern persisted. Irrespective of the counterpart's behavior, there appears to be a reputational benefit to diversifying one's information selection decisions.

Discussion

Experiment 4 demonstrated the reputational consequences of information selection decisions, providing additional evidence for the signaling model of selective exposure. Critically, and in contrast to the apparent expectations of actors in Experiment 3, this relationship was contingent on the type of collaboration that observers expected to engage in. Specifically, observers were more likely to reward actors who selected advisors from the observer's ingroup for tasks reliant on trust (vs. judgment skill).

Additionally, observers were responsive to the magnitude of selective exposure demonstrated by the actor. While they tended to reward choosing more ingroup advisors, observers also seemed to display a preference for some information diversification. In neither game type did observers demonstrate a preference for actors who exclusively relied on advice from the observer's ingroup. This finding is important given that individuals make repeated information selection decisions in front of their families, friends, and colleagues. The recognition that selective exposure is rewarded, but to a limit, adds important nuance to our understanding of the phenomenon.

Of note, Experiment 4 examined a situation in which observers were not aware of the group affiliation of their two potential partners. Thus, observers might have been using the advisors that a given actor consulted to infer the actor's group identity. It could be the case that observers preferred to choose actors who had selected more ingroup advisors because they interpreted this to mean that the actor was an ingroup member as well. This interpretation aligns with the hypothesis that individuals engage in selective exposure to signal belonging to a particular group.

However, people often find themselves in scenarios in which they know quite a bit about the person with whom they are interacting. This may mean that either explicitly know the other person's group affiliation or have a strong prior belief about it. When one's group membership is already known, an actor's information selection decisions may be more a signal of *strength* of group affiliation.

For known ingroup members who begin in good standing, selecting even more ingroup information could signal high levels of group loyalty. For a known outgroup member, by contrast, selecting information from the opposing side could indicate openness to cooperation. In Experiment 5, we explore these questions by again assessing observer collaboration choices, while also directly and explicitly varying whether the actors under consideration are ingroup or outgroup members.

Experiment 5

In Experiment 5 we continued to examine the reputational consequences of information selection decisions. As in Experiment 4, participants (whom we will continue to call observers) chose a partner for a future collaborative task from among two actors who had participated in a prior study. We again varied the type of collaborative task.

In addition, the design of Experiment 5 varied whether the actors under consideration reported holding the same or opposing political ideology as the observer. Thus, whereas Experiment 4 allowed us to investigate contexts where an individual's group membership is ambiguous and their information selection decisions can function as a signal of that membership, Experiment 5 tests whether information selection decisions provide value above and beyond knowledge of group membership. On the one hand, when one's group membership is known, advisor selections from an ingroup member may be interpreted as a signal of strength of affiliation. On the other hand, to the extent that individuals have had more contact with (and thus stronger positive expectations of ingroup members), the information selection decisions of outgroup members may seem particularly informative. To the extent that we generally have negative expectations of outgroup members and expect them to be unwilling to learn about our perspective (Collins et al., 2022), a demonstrated willingness to select information from our side may send a particularly positive signal.

Method

Participants

We recruited a sample of U.S. residents from Cloud Research in August 2020 to participate in a 10-min experiment in exchange for $1.00, with further opportunities for a bonus. Following our preregistration, our final sample consisted of 983 participants (M_{age} = 42.2; 49.8% female, 49.7% male, 0.4% nonbinary; 51.1% liberal, 48.9% conservative). In a pilot study, we observed a standardized effect size of approximately 0.30 for the effect of whether observers preferred to collaborate with actors who selected a greater number of advisors from the observer's ingroup; our final sample thus achieved greater than 90% statistical power to test this hypothesis.

Procedure

Participants (observers) first answered demographic questions, which included reporting their political ideology on a 7-point scale from *extremely liberal* to *extremely conservative*. From here, Experiment 5 closely mirrored the procedure of Experiment 4 with participants learning that their task in the study was to choose a partner for a future collaboration task, and then proceeding to make eight partner choices, one of which would be implemented.

Building on Experiment 4, this study featured a between-subjects 2×2 factorial design. We again varied the future collaborative task for which the observer was choosing the actor—the trust game or the estimation game. Extending Experiment 4, we additionally varied whether the actors whose advisor choices the observers evaluated reported the same political ideology as the observer (ingroup condition), or the opposing political ideology (outgroup condition). Thus, observers in the ingroup condition viewed eight pairs of actors, all reporting their own political ideology, and chose one actor from each pair as a potential future collaboration partner. In contrast, observers in the outgroup condition viewed eight pairs of actors reporting the opposite political ideology. Further implementation details can be found in Table A1.

Analysis Plan

We followed the same analysis plan as in Experiment 4. We again dropped data from pairs of actors where both selected the same number of the observer's ingroup advisors. Our primary dependent

variable was again binary, such that for any pair of actors (1 = *observer chose the actor who consulted more of the observer's ingroup advisors*, 0 = *observer chose the actor who consulted fewer of the observer's ingroup advisors*). Thus, we used an ordinal logistic regression, using the clm function in R (Christensen, 2018). As in Experiments 3–4, we also included participant-clustered *SE*s, necessary because each participant provided eight estimates. Since we had eight different policy topics, we used fixed effects to control for the effect of each of these, again with use of a simple effects contrast matrix in order to interpret the intercept as the grand mean of the model rather than the mean of a reference topic.

Results

Reputational Consequences of Selective Exposure

We first examined whether observers preferred to collaborate with actors who selected a greater number of advisors from the observer's ingroup. Replicating the pattern of results in Experiment 4, we found this to be the case 66% of the time, a frequency greater than chance. When using the analytic strategy described above, the odds of the actors who selected a greater number of the observer's ingroup advisors being chosen for future collaboration was 1.98 times that of their counterpart ($p < .001$).

Congruence of Group Membership

We next examined a question that was new to Experiment 5: whether our effect differed for pairs of ingroup versus outgroup actors. To test this, we included a dummy-coded variable for congruence of group membership in the regression model. Although participants chose the actor who selected more ingroup advisors at greater than chance levels in both conditions, this tendency was substantially more pronounced when choosing among actors with the *opposing* political ideology ($OR = 1.52$, $p < .001$). In the ingroup condition, observers chose the actor who had selected more advisors from the observer's and actor's shared ingroup 61.7% of the time. In contrast, in the outgroup condition, observers chose the actor who had selected more advisors from the observer's ingroup 71.0% of the time (see Figure 7 and Table A12 of the Appendix). Thus, although individuals displayed a preference for collaborators who selected advice from their ingroup in both conditions, this was especially important for outgroup members.

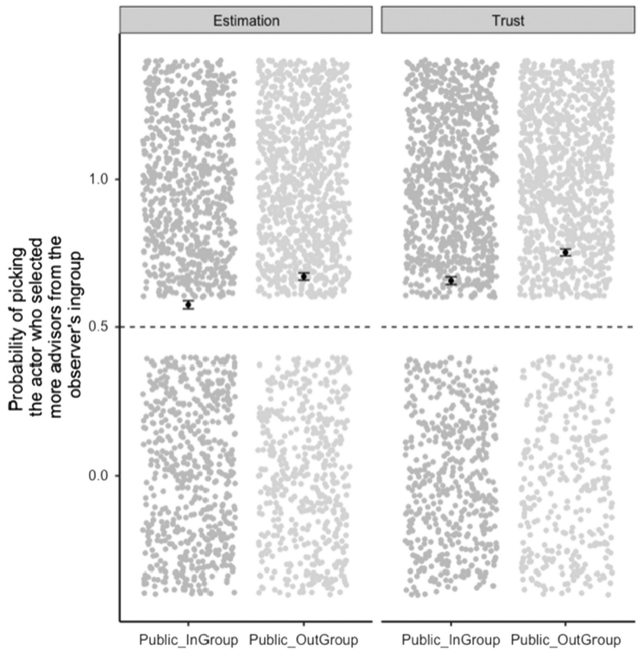
Given that people tend to hold overly negative beliefs about outgroup members (Lees & Cikara, 2021; Moore-Berg et al., 2020), seeing an outgroup member view ingroup information may have been an especially salient signal of open-mindedness and receptiveness to opposing views (Minson & Chen, 2022; Minson et al., 2020). Alternatively, if observers interpret actors' selections as a sign of their commitment to existing beliefs, this interaction could indicate that it's more important for observers to know that an outgroup member is malleable in their beliefs than it is to know that an ingroup member is fully committed. Examining this distinction would be a fruitful avenue for future research.

Decision Context

In examining the effect of selecting advisors from the observer's ingroup when choosing a partner for a future trust game or a future estimation game, we replicated the results from Experiment 4. To examine this hypothesis, we again included a dummy-coded variable for task type in the regression model. Although participants chose the actor who selected a greater number of advisors from the observer's ingroup at levels above chance in both conditions, this tendency was substantially more pronounced when choosing a partner for a trust game rather than an estimation game ($OR = 1.42$, $p < .001$; see Figure 7 and Table A12 of the Appendix).

Figure 7

The Probability That Observers Chose the Actor Who Selected a Greater Number of Advisors From the Observer's Ingroup, by Condition



Note. The dotted line at 0.50 represents chance levels. Error bars represent ± 1 *SE* of the group mean, clustered by participant. In all conditions, the actors who selected more of the observer's ingroup advisors were more likely to be chosen, but this tendency was more pronounced in the trust game and outgroup conditions.

Magnitude of Selective Exposure

In Experiment 5 we were again able to examine the effect of actors' information diversification on observer choices. While exploratory, these results are in line with those from Experiment 1 and directly replicate those from Experiment 4 with (a) that observers demonstrating a preference for actors who selected more advisors from the observer's ingroup, but also (b) showing a preference for diversification by punishing those actors who selected all of their advisors from the observer's ingroup. These results are presented in Figure A2 and Table A13 in the Appendix.[9]

[9] As in Experiment 4, in our preregistration, we detailed testing this question with different analyses, however we think that the presented analyses and graphs better represent our results. We include the preregistered analyses in the Appendix.

When examining the data by ingroup versus outgroup condition, observers were sensitive to ideological alignment when evaluating potential partners based on their advisor selections (see Figure 8 and Table A13 in the Appendix). Specifically, when evaluating an outgroup member, actors receive a greater benefit for each additional advisor selected from the observer's ingroup. However, for both ingroup and outgroup actor selections, observers clearly favored those who demonstrated some openness to outgroup information.

Trade-Offs Between Observers

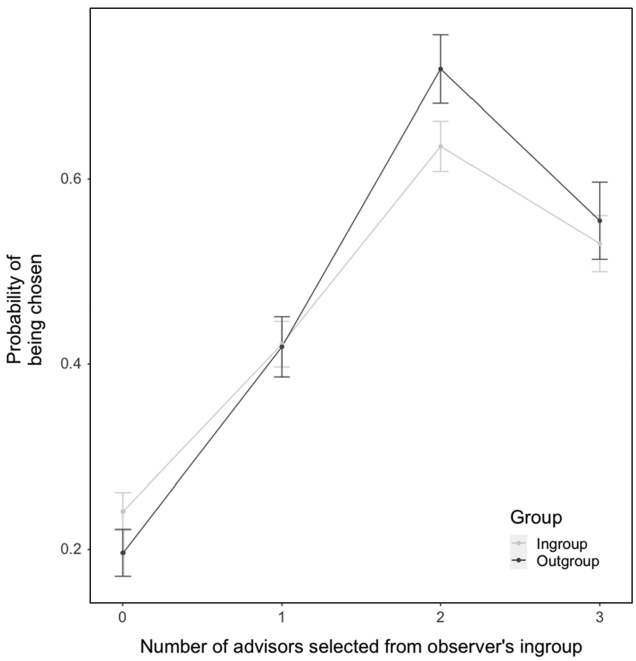
In a final set of exploratory analyses, we examined the reputational trade-offs that consumers of information face when confronted with an audience whose group affiliation is unknown. While participants in Experiment 3 had accurate information regarding whether the observer belonged to their ingroup or the outgroup, outside of controlled laboratory experiments, such information is often unavailable (e.g., on social media). Furthermore, audiences often include a mixed set of evaluators. In Experiment 5, our data capture how the range of possible information selection decisions of actors were evaluated by both ingroup and outgroup observers. This enables us to assess the information selection strategy actors might employ when audience affiliation is unknown.

Our results revealed a nuanced trade-off that actors must navigate in choosing whether to appeal to ingroup or outgroup observers. To the extent that observers reward actors who select information sources from the observer's own side, it seems impossible to please both. That is, selecting more sources from one group necessarily means selecting fewer from the other. Yet, our data suggest that it may be possible to improve on this zero-sum approach. Figure 9 plots evaluations of actors by both ingroup and outgroup observers as a function of advisor selection. The figure axes correspond to the probability of being chosen for future collaboration by ingroup and outgroup members, respectively. The points on the graph represent the number of information sources chosen by the actor belonging to the *actor's* ingroup. Note that this is a departure from Experiments 4 and 5, where we referred to the information sources belonging to the observer's ingroup as "ingroup sources." This departure is necessitated by the fact that Figure 9 presents observers from both the actor's ingroup as well as the actor's outgroup.

If we consider an actor who selected zero of their own ingroup information sources, we observe a 26% probability of this individual being chosen for future collaboration by their ingroup members, and a 55% probability of them being chosen by their outgroup members. However, if this actor selected a single ingroup source, their standing improved in the eyes of both types of observers, with the chance of being chosen by an ingroup member going up to 42% and the chance of being chosen by an outgroup member going up to 71%. Similarly, if we consider an actor who selected three of their own ingroup information sources, we observe a 53% probability of this individual being chosen for future cooperation by their ingroup members, and a 20% probability of them being chosen by their outgroup members. However, if this actor selected a single outgroup source, their standing improved in the eyes of both types of observers, with the chance of being chosen by an ingroup member going up to 64% and the chance of being chosen by an outgroup member going up to 42%.

Figure 8

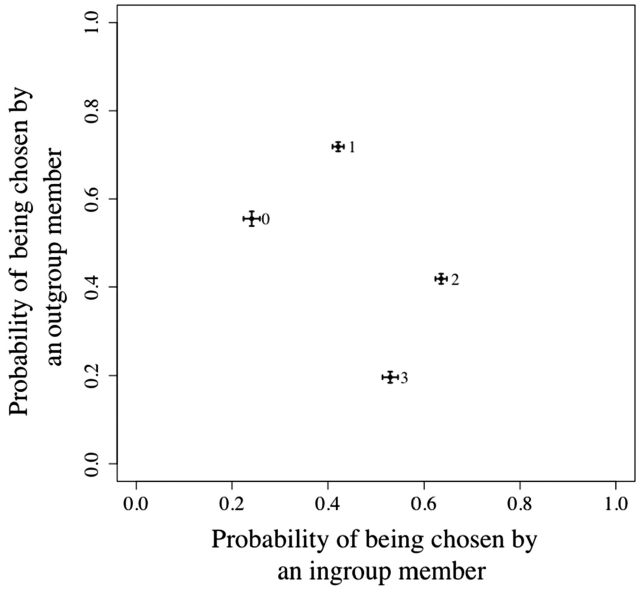
Probability That an Actor Was Chosen Based on the Number of Advisors That They Selected From the Observer's Ingroup, by Condition



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad.

Figure 9

The Points on the Graph Represent the Number of Selected Advisors From the Actor's Ingroup



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad. Note that points further from the origin are better, representing a clear case for diversification. For example, choosing one advisor from the ingroup is a pareto improvement over choosing zero. Similarly, choosing 2 advisors from the ingroup is a pareto improvement over choosing three.

This analysis demonstrates that selecting a diverse set of information sources yielded reputational benefits in all conditions. While observers had a general tendency to prefer those who selected more of their own ingroup information, we found that, surprisingly, the optimal response for individuals concerned with managing their reputation was to diversity their information portfolio—a strategy that is also likely to improve judgment accuracy under many circumstances.

Discussion

Experiment 5 provided further support for the hypothesis that observers reward actors who consult more advisors from the observer's ingroup. However, unlike in Experiments 2 and 4, the actor's group identity was known to observers. Our data demonstrate that even in situations where group affiliation is known (as is often the case when we interact with family members, friends, and colleagues), information selection decisions are perceived as informative. This finding provides an avenue for future research to examine whether actors anticipate this benefit above and beyond signaling group identity.

Experiment 5 also provided evidence that observers' preference for individuals who engage in selective exposure is moderated by congruence of group membership. Specifically, observers were more likely to reward the selection of advisors from the observer's ingroup when the actor belonged to the outgroup. An outgroup member consulting more of the observer's ingroup sources might be interpreted as a sign of persuadability. However, this result could also indicate that a willingness to cross the aisle is especially important when trying to make a positive impression on an outgroup member, and perhaps less reputationally consequential than demonstrating loyalty to one's own ingroup. Future research is needed to tease apart these explanations. Additionally, we replicated the results from Experiment 4, finding that the observer's preference was again moderated by the type of future collaboration they expected to engage in.

Finally, and perhaps most counterintuitively, we also found that observers demonstrated a preference for those actors who primarily, but not exclusively, selected information aligned with the observer's ingroup. This result provides a path forward for individuals who wish to balance judgment accuracy with the reputational benefits of demonstrating selective exposure as it appears that the reputational benefits of the latter are in fact bounded.

General Discussion

Across a range of personal and professional contexts, individuals must rely on diverse information to maximize the quality of their decision making. Yet, research suggests that they often avoid information that contradicts their prior beliefs. This phenomenon is especially pronounced in political contexts where such information avoidance can foster increased polarization and undermine the welfare of individuals and entire societies (Finkel et al., 2020). Why, then, do people fail to seek out the broadest possible set of facts and opinions?

Prior research in this area has primarily focused on intrapersonal answers to this question (most notably avoidance of cognitive dissonance). In the current work, we provide robust evidence for a social signaling model of selective exposure. We hypothesize and find that (a) people make information selection decisions at least partly to send a signal to observers, and (b) observers reward people who send such signals.

Across five well-powered, financially incentivized, preregistered experiments, our work simultaneously supports a social signaling model of selective exposure and paints a nuanced picture of information selection decisions and their interpersonal consequences under a variety of conditions. Specifically, our results reveal that while individuals are largely rewarded for selecting information aligned with the observer's ingroup, observers also attended to multiple other features of the situation: including (a) the type of future engagement they are likely to have with the actor, (b) the actor's known group membership, and (c) the magnitude of selective exposure demonstrated. Our experiments allowed us to capture a tension between individuals' desire to make accurate judgments and their desire to manage the perceptions of others—a tension that is present in many contexts outside of the laboratory. Our findings thus offer insights for understanding the basic drivers of selective exposure, as well as avenues for mitigating its occurrence.

Theoretical Contribution

Our approach extends prior theory on reputational influences on behavior and offers insights about the psychological underpinnings of selective exposure. We build on impression management research by applying this theoretical lens to information selection decisions. Across social science disciplines, impression management research has flourished as scholars recognize the role that reputational concerns play in an array of seemingly irrational behaviors (Dorison, 2022, 2023; Dorison et al., 2021; Jordan, Hoffman, Bloom, & Rand, 2016; Jordan, Hoffman, Nowak, & Rand, 2016; Tenney et al., 2019). However, thus far, explanations of selective exposure have been primarily focused on *intrapersonal* reasons, often rooted in avoidance of negative emotions (Dorison et al., 2019; Frimer et al., 2017). In the present research we provide evidence for a complementary *interpersonal* account.

By explicitly testing both sides of a social signaling model of information selection decisions, our work contributes to research on reputational accounts for behavior more generally. An examination of an interpersonal explanation requires looking at both reputational causes as well as reputational consequences of the behavior, something that prior research on selective exposure has not attempted to do. This functional approach allows us to answer whether a seemingly irrational behavior might actually be appropriate for a given environment.

The social signaling lens illuminates a key trade-off for decision makers. Selective exposure is traditionally considered to be a bias since information from a variety of sources maximizes judgment accuracy (Akerlof, 1970; Blackwell, 1953; Galton, 1907; Golman et al., 2017; Janis, 1982; Mullainathan & Shleifer, 2005; Page, 2008; Peterson & Pitz, 1986; Stewart, 1988; Stigler, 1961; Sunstein, 2001; Surowiecki, 2005). However, our results provide an important qualification to this traditional view. Specifically, we find that observers reward actors who select more of the observer's ingroup information. Thus, information selection decisions may be serving two purposes: maximizing judgment accuracy and maximizing reputational benefits. Our results suggest that when considering the relevant social rewards, tailoring one's information selection decisions to the audience may be a rational strategy.

Our results also identify theoretically derived conditions under which signaling is more versus less likely to be effective. We find that observers reward individuals more for consuming the observer's ingroup information when expecting to engage a future interaction

reliant on interpersonal trust than reliant on judgment skill. We also find that observers reward outgroup individuals more for consuming the observer's ingroup information than ingroup members. Neither of these factors have been considered in prior work relating to impression management and selective exposure (Hart et al., 2020; Lundgren & Prislin, 1998).

Whereas prior research has focused on documenting the presence or absence of selective exposure, we find that observers are also sensitive to the magnitude of the phenomenon. Both ingroup and outgroup observers displayed a preference for actors who signaled a willingness to engage with a variety of perspectives by choosing to view at least some information from both sides. Our work thus paints a more nuanced portrait of the reputational consequences of information selection decisions, identifying the conditions under which selective exposure is more or less socially rewarded.

Finally, our findings speak to why selective exposure is so persistent. Given that our results indicate that people tailor their information selection decisions to the identity of the *observer*, one might question how they speak to the most commonly studied operationalization of selective exposure—selecting information consistent with one's *own* identity. However, individuals spend most of their lives ensconced in neighborhoods and social networks comprised of politically like-minded others (Bakshy et al., 2015; Brown & Enos, 2021). Given that people are most often observed by ingroup members, our pattern of results illustrate a powerful social force leading to the persistence of selective exposure in the world.

Practical Contribution

Our work also holds important practical implications for individuals, organizations, and society. From the perspective of the individual decision maker, our results offer insights for managing the tension between judgment accuracy and reputational concerns across different contexts. Given that systematic avoidance of opposing views carries important accuracy costs, our results can also inform policy makers seeking to design interventions to encourage more balanced information consumption.

We consistently find that individuals can benefit reputationally from conspicuously consuming information aligned with the beliefs and values espoused by their audience. This result holds across multiple types of information and elicitations. However, our work goes beyond prior research by adding nuance to this basic strategy. The observers in our studies also attended to multiple other features of the signal: including the type of task, the actor's group membership, and the diversity of the selected set of information sources. Thus, actors benefitted more from consuming outgroup-aligned information sources when they sought to signal trustworthiness to ideological opponents. Importantly, however, irrespective of the observer's identity, individuals benefitted from diversifying their information selection decisions—which was valued by observers from both sides of the aisle.

In addition to informing the best strategy for an individual decision maker, our research holds important implications for leaders seeking to reduce selective exposure—an especially important concern given today's high levels of political polarization (Boxell et al., 2022). To the extent that individuals appear concerned with reputational consequences, leaders and policy makers may wish to design interventions to explicitly encourage specific behaviors. For example, communicating a preference for receptiveness to opposing views (Minson et al., 2020) or highlighting decision accuracy as a key goal may increase the range of information individuals consult, ultimately leading to less polarization, reduced spread of misinformation, and improvements in societal decision making.

Limitations and Future Directions

As our experiments test the specific predictions of the social signaling model, some key limitations and future research directions should be noted. We find that our results hold across multiple types of information (i.e., selection of politicians' web pages, news articles, and advisors). This is in line with prior research on selective exposure, which has been documented across a variety of behaviors ranging from who one discusses politics with, to the types of media outlets one chooses, to the content of messages one selects (Stroud, 2014). These information types differ across many dimensions including whether the information arises from a specific outgroup member (e.g., an individual's opinion) or supports an outgroup point of view (e.g., the results of a study). Thus, scholars should examine the generalizability of these key results in more naturalistic settings. Differences between information types might change the balance of social versus accuracy incentives in any given situation and therefore should be more thoroughly explored.

Additionally, Experiments 3–5 use a relatively stylized paradigm which was developed as an experimental test with precise control over the decision environment (Falk & Heckman, 2009) where reputational incentives were directly tied to financial benefits. This is common in the experimental economics literature, where the trust game, among others, is often used to measure social preferences (Levitt & List, 2007). Outside of the laboratory, there are situations where a single opinion might be sought or avoided. For example, imagine a liberal social scientist who wishes to form a clearer opinion on the risks of climate change. To do so, she might consider consulting with another colleague who is known for their contrarian opinions. In this case, her desire to understand both sides of the argument might be tampered by her concern about what other colleagues will think of her willingness to engage with a "climate denier." However, while there are a number of studies which link the generalizability of lab experiments to those in the field (Camerer, 2011), there are elements of reputation and impression management concerns which might not be captured by our use of financial incentives. For example, in our minimal information paradigm, participants do not have a personal relationship with their advisors which might increase or decrease the effect.

In designing our experiments, we follow prior selective exposure research methodologies by presenting participants with a balanced menu of choices and asking them to select a certain number of sources from among them. This method implicitly equates the selection of confirming evidence with avoidance of disconfirming evidence. This scenario parallels our era of information accessibility, wherein arguments on both sides of any issue are freely available. However, to the extent that information selection versus information avoidance decisions might be more observable to others, the reputational costs and benefits might differ. Future research should address this question. For example, turning off the TV to avoid *Fox News* programming might have different reputational benefits than choosing to turn on the TV to watch MSNBC.

Relatedly, whether observers reward or punish actors based on their information selection decisions is an interesting question for future research (for further discussion, see Dorison & Heller, 2022; Levitt

& List, 2007). In the experiments above, we focus on the use of the trust game or selection of someone for a future task, which participants likely view as rewards. although researchers know from extensive research on conditioning that both punishments and rewards impact behavior (Skinner, 1963), which is more impactful continues to be debated (Kubanek et al., 2015). Within groups, punishment can work to sustain cooperation (Fehr & Gächter, 2000), although the effectiveness of punishment is dependent on a variety of factors, including the characteristics of the group members (Gächter & Thöni, 2005). The U.S. political environment, where outgroup hate has recently been shown to overshadow ingroup love (Finkel et al., 2020), might be a particular scenario in which punishments might have a greater impact than rewards. Taken together, we suspect that the relative effectiveness of rewards versus punishments varies dramatically based on the information context.

Combining the above two points, there are four possible pathways for reputational incentives to play a role: (a) penalties for seeking outgroup information, (b) rewards for seeking ingroup information, (c) penalties for avoiding ingroup information, and/or (d) rewards for avoiding outgroup information. Given that selection decisions might be more observable than avoidance decisions, options one and two might be most consequential. However, future research is needed to disentangle the relative effects of these four options.

Furthermore, our experiments featured two decision contexts in which participants could easily signal certain characteristics (i.e., trustworthiness and judgment skill). As our theorizing suggests, the relevance of any given dimension of social evaluation naturally varies with the context. We chose to test two dissimilar decision contexts which could be operationalized in an incentive-compatible manner. Future extensions could also examine how information selection decisions might be strategically employed to signal other characteristics such as likability, cooperativeness, or intelligence.

Our results also raise intriguing questions about when actors can predict the social rewards associated with their information selection decisions and when they cannot. When looking across our five experiments to compare actor behavior with observer rewards, we see that actors adjust their information selection decisions depending on the identity of the observer. However, actors do not anticipate the varied social rewards based on the type of task the observer expects to engage with them on. Future research should thus further explore when and why actors can accurately predict the reputational benefits of specific information consumption choices.

Constraints on Generalizability

The experiments described above were all conducted with online samples of U.S. participants who reported identification with either a liberal or conservative ideology. We propose that the social signaling model presented here would apply to any group context with correlated belief structures (for related work, see Minson & Dorison, 2022), above and beyond the domain of partisan politics. While disagreement along the lines of political ideology builds on prior selective exposure research by leveraging naturally occurring ingroup versus outgroup belief structures, we think our paradigm and results could also be extended to feature other topics of disagreement. For example, many organizations must manage disagreement between groups organized around functional or geographic divisions. As this has important societal consequences, additional insight could be gleaned by examining other common topics of conflict such as those in organizations or in families.

Conclusion

Taken together, our results demonstrate the relevance of interpersonal factors in driving selective exposure to political information. In the era of social media and the rapid spread of misinformation and disinformation, when many choices are public than ever before, understanding the features of social contexts under which people are more or less likely to display selective exposure is crucial for both theory and practice. Our work extends prior thinking in this area and points to specific avenues toward greater engagement across ideological divides.

Context

This article fits into a program of research that considers the role of impression management concerns in the domain of information selection, consumption, and sharing. Specifically, we highlight how reputational concerns can rationally impact selective exposure to information, which has traditionally been studied as a bias. However, we also find hope in the result that observers value people who show at least some open-mindedness. This research is particularly timely given the high levels of political polarization in the world, helping us to best understand what drives decisions regarding media consumption and sharing, as well as the possibility of fostering cross-party interactions.

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(*Appendices follow*)

Appendix A

Supplemental Analyses

Table A1

Implementation Details for Each Experiment, Including Details About How Participants Were Paired Between Studies for Payment

Experiment number	Implementation details
Replication study	To avoid having to rerecruit participants, at the end of the actor study, participants were asked what percentage of the money sent to them they would be willing to return to an observer. Therefore, if an observer sent all 10 cents (which was then tripled by us, the experimenters) and an actor decided to return 50%, both the actor and the observer that they are paired with would end up with 15 cents. For payment, participants in the replication study (actors) were paired with participants from Experiment 2 (observers).
Experiment 1	To avoid having to rerecruit participants, at the end of the actor study, participants were asked what percentage of the money sent to them they would be willing to return to an observer. Therefore, if an observer sent all 10 cents (which was then tripled by us, the experimenters) and an actor decided to return 50%, both the actor and the observer that they are paired with would end up with 15 cents. For payment, participants in Experiment 1 (actors) were paired with participants from a pilot study of observers.
Experiment 2	Here, rather than randomly draw actor responses, we manipulated the number of selections for each of the people who the observers were evaluating directly. Therefore, observers were randomly assigned to evaluate an actor who had selected 0, 1, 2, 3, 4, or 5 ingroup responses. We were then able to randomly select one observer for each selection (e.g., an observer who evaluated an actor who chose four liberal sources and one conservative source) to accurately incentivize and pay actors from Experiment 1. For payment, participants in Experiment 2 (observers) were paired with participants from the replication study (actors).
Experiment 3	To avoid having to rerecruit participants, at the end of the actor study, participants were asked what percentage of the money sent to them they would be willing to return to an observer. Therefore, if an observer sent all 10 cents (which was then tripled by us, the experimenters) and an actor decided to return 50%, both the actor and the observer that they are paired with would end up with 15 cents. For payment, participants in Experiment 3 (actors) were paired with participants from Experiment 4 (observers)
Experiment 4	Here, participants (observers) were randomly shown eight real actor responses from Experiment 3, one of which was chosen for payout. As described above, we elicited actor's responses on the trust game (e.g., how much they would send back to their partner) at the end of their survey. For payment, participants in Experiment 4 (observers) were paired with participants from Experiment 3 (actors).
Experiment 5	Here, participants (observers) were randomly shown eight real actor responses from Experiment 3, one of which was chosen for payout. As described above, we elicited actor's responses on the trust game (e.g., how much they would send back to their partner) at the end of their survey. For payment, participants in Experiment 5 (observers) were paired with participants from a pilot study of actors.

Note. Gray rows report details of actor studies and the nonhighlighted rows report details of observer studies.

Table A2

Recruitment Details From Cloud Research for Each Experiment

Experiment number	Recruitment details
Replication study	We recruited using the following criteria: 90% HIT approval rate, greater than 500 HITs previously approved, and included on Cloud Research's approved participants list. Due to irregularities with Cloud Research, we collected responses from 420 people. Before random assignment to condition, we excluded 56 participants who reported their political ideology to be "middle of the road." Our final sample consisted of 364 participants.
Experiment 1	We recruited using the following criteria: 90% HIT approval rate, greater than 500 HITs previously approved, and included on Cloud Research's approved participants list. Before random assignment to condition, we excluded 132 participants who reported their political ideology to be "middle of the road." Our final sample thus consisted of 602 participants.
Experiment 2	We recruited using the following criteria: 90% HIT approval rate, greater than 500 HITs previously approved, and included on the Cloud Research approved participants list. Due to irregularities with Cloud Research, we collected responses from 844 people. Before random assignment to condition, we excluded 173 participants who reported their political ideology to be "middle of the road." Our final sample consisted of 671 participants.
Experiment 3	As per our preregistration, our goal was to collect data from approximately 1,000 participants, roughly balanced between liberals and conservatives. We recruited using the following criteria: 98% HIT approval rate, greater than 500 HITs previously approved, and included on Cloud Research's approved participants list. Due to irregularities with Cloud Research, we collected responses from 993 people. Before random assignment to condition, we excluded 110 participants who reported their political ideology to be "middle of the road." Our final sample consisted of 883 participants.
Experiment 4	As per our preregistration, our goal was to collect data from approximately 500 participants, roughly balanced between liberals and conservatives. We first recruited $N = 300$ using the following criteria: 98% HIT approval rate, greater than 500 HITs previously approved, and included on Cloud Research's approved participants list. We then excluded 66 participants who reported their political ideology to be "middle of the road." Next, in order to get roughly 250 participants of each political ideology, we directly recruited $N = 81$ self-reported liberals and $N = 185$ self-reported conservatives. From those, we excluded $N = 41$ who reported their political ideology to be "middle of the road." Our final sample consisted of 459 participants.
Experiment 5	As per our preregistration, our goal was to collect data from approximately 1,000 participants, roughly balanced between liberals and conservatives. We first recruited $N = 700$ using the following criteria: 98% HIT approval rate, greater than 500 HITs previously approved, and included on the Cloud Research approved participants list. We then excluded 90 participants who reported their political ideology to be "middle of the road." Next, in order to get roughly 500 participants of each political ideology, we directly recruited $N = 90$ self-reported liberals and $N = 300$ self-reported conservatives. From those, we excluded $N = 17$ who reported their political ideology to be "middle of the road." Our final sample consisted of 983 participants.

Note. Gray rows report details of actor studies and the nonhighlighted rows report details of observer studies. HIT = human intelligence task on cloud research.

(*Appendices continue*)

Experiment 1 **Pilot Study**

Table A3
Regression Results for the Number of Ingroup Sources Chosen by Condition

	Dependent variable			
	Number of ingroup sources chosen			
Predictors	(1) News	(2) Senators	(3) All	(4) All
Senators				0.354*
				(0.195)
Ingroup	0.719***	0.541***	0.640***	0.719***
	(0.188)	(0.201)	(0.138)	(0.194)
Outgroup	−0.982***	−1.157***	−1.059***	−0.982***
	(0.188)	(0.200)	(0.137)	(0.194)
Senators × Ingroup				−0.178
				(0.275)
Senators × Outgroup				−0.175
				(0.275)
Constant	3.250***	3.604***	3.420***	3.250***
	(0.130)	(0.145)	(0.097)	(0.135)
Observations	298	304	602	602
R^2	.213	.205	.207	.213

Note. Ingroup (outgroup) indicates being in the ingroup (outgroup) condition. Senators is an indicator for being in the senators condition. Column 1 restricts data to those in the news condition. Column 2 restricts data to those in the senators condition. Columns 3–4 include data from all participants. All columns report the results of linear regressions using the lm function in R.
* $p < .1$. *** $p < .01$.

Experiment 2 **Experiment 3**

Table A4
Regression Results for the Observer's Trust in the Actor Based on Their Number of Sources Chosen From the Observer's Ingroup

	Dependent variable				
	Trust				
	OLS				CLM
Predictors	News (1)	Senators (2)	All (3)	All (4)	All (5)
Senators				−0.139	
				(0.449)	
Number of ingroup sources	0.539***	0.578***	0.558***	0.539***	0.451***
	(0.107)	(0.107)	(0.075)	(0.108)	(0.085)
Number of Ingroup Sources × Senators				0.038	
				(0.151)	
Constant	4.959***	4.821***	4.894***	4.959***	
	(0.305)	(0.330)	(0.224)	(0.307)	
Observations	335	336	671	671	671
R^2	.071	.080	.076	.076	
F statistic	25.389*** ($df = 1; 333$)	29.089*** ($df = 1; 334$)	54.755*** ($df = 1; 669$)	18.231*** ($df = 3; 667$)	

Note. Senators is an indicator for the senators condition. Number of ingroup sources refers to the number of sources an actor chose from the observer's ingroup (ranging from 0 to 5). Columns 1–4 report the results of linear regressions using the lm function in R, where trust is measured as the number of cents sent to the actor. Column 1 restricts the data to those in the news condition. Similarly, Column 2 restricts the data to those in the senators condition. Columns 3–5 include all data. Column 4 reports the results of a logistic regression using the clm function in R, where trust is measured as a binary variable of whether or not the observer sent any amount of money to the actor or not. OLS = ordinary least squares linear regression; CLM = cumulative link model for ordinal regression.
*** $p < .01$.

(*Appendices continue*)

Table A5

Linear Regression Results for the Observer's Trust in the Actor (Measured as Percent of Endowment Sent) Based on a Factor Variable Representing the Number of Sources an Actor Chose From the Observer's Ingroup

Predictors	Dependent variable Trust
One ingroup source	11.877***
	(4.356)
Two ingroup sources	21.150***
	(4.356)
Three ingroup sources	24.213***
	(4.315)
Four ingroup sources	28.761***
	(4.345)
Five ingroup sources	27.242***
	(4.445)
Zero ingroup sources	44.032***
	(2.986)
Observations	671
R^2	.088
F statistic	12.883*** ($df = 5; 665$)

Note. Trust is measured as the proportion of the observer's endowment sent to the actor. Here, the model intercept has been renamed "zero ingroup sources" for ease of interpretation.
*** $p < .01$.

Experiment 4

Table A6

Topics Used in the Pilot Study and the Proportion of Self-Identified Liberals (Column 2) and Conservatives (Column 3) Who Reported Agreement With Each Topic

Topic	Liberals	Conservatives
The death penalty should be abolished in all U.S. states.	0.72	0.28
I approve of the job that Joe Biden is currently doing as president.	0.78	0.22
I support the national legalization of marijuana for recreational use.	0.67	0.33
All U.S. civilians should have to undergo a psychiatric evaluation before purchasing a firearm.	0.61	0.39
If a woman wishes to terminate a pregnancy, she should first be required to undergo a fetal ultrasound in order to make a fully informed decision.	0.23	0.77
The public reaction to recent confrontations between police and minority crime suspects has been overblown.	0.10	0.90
Although the #metoo movement has provided a useful forum for women to discuss sexual harassment, it is also creating a zero-tolerance mentality of blame and finger-pointing.	0.36	0.64
A physical barrier along the southern border will have no effect on illegal immigration.	0.79	0.21

Note. These proportions were estimated by actors in Experiment 3.

(*Appendices continue*)

Table A7
Regression Results for the Number of Ingroup Advisors That the Actor Chose by Condition

	Dependent variable					
	Number of ingroup advisors chosen					
Predictors	OLS (1)	CLM (2)	OLS (3)	CLM (4)	OLS (5)	CLM (6)
Public ingroup			0.208***	0.385***		
			(0.057)	(0.107)		
Public outgroup	−0.415***	−0.768***	−0.206***	−0.388***	−0.431***	−0.784***
	(0.047)	(0.089)	(0.056)	(0.106)	(0.067)	(0.126)
Trust game					0.037	0.077
					(0.069)	(0.131)
Public Outgroup × Trust Game					0.028	0.027
					(0.095)	(0.182)
Topic fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	1.563***		1.354***		1.544***	
	(0.035)		(0.045)		(0.048)	
Intercept: 0 ingroup advisors | one ingroup advisor		−1.730***		−1.357***		−1.693***
		(0.083)		(0.096)		(0.101)
Intercept: one ingroup advisor | two ingroup advisors		−0.054		0.327***		−0.017
		(0.065)		(0.084)		(0.090)
Intercept: two ingroup advisors | three ingroup advisors		1.389***		1.793***		1.427***
		(0.084)		(0.097)		(0.105)

Note. *Public ingroup is an indicator* = 1 for being in the public ingroup condition. *Public outgroup is an indicator* = 1 for being in the public outgroup condition. *Trust game is an indicator* = 1 for being in the trust game condition and = 0 for being in the judgment game condition. Topic fixed effects account for the effect of the eight different policy topics. All regressions include clustered *SE*s by participant. Columns 1, 2, 5, and 6 are restricted to those in the public conditions. Columns 3 and 4 include data from all participants. All columns include simple effects coding for the topic variable in order to interpret the intercepts of the model as the grand mean of topics. Columns 1, 3, and 5 report the results of linear regressions using the lm function in R as a robustness check. Columns 2, 4, and 6 report the results of logistic regressions using the clm function in R. *SEs clustered by participant.* OLS = ordinary least squares linear regression; CLM = cumulative link model for ordinal regression.
*** $p < .01$.

Table A8
Linear Regression Results for z*-Scored Error*

	Dependent variable	
	z-scored error	
Predictors	(1)	(2)
Ingroup advisors chosen	0.073***	0.063*
	(0.017)	(0.038)
Public ingroup		−0.005
		(0.070)
Public outgroup		0.069
		(0.072)
Ingroup Advisors Chosen × Public Ingroup		0.027
		(0.045)
Ingroup Advisors Chosen × Public Outgroup		0.003
		(0.048)
Constant	−0.099***	−0.130**
	(0.027)	(0.058)

Note. Ingroup advisors chosen represents the number of ingroup advisors that the actor chose (0–3). *Public ingroup is an indicator* = 1 for being in the public ingroup treatment. *Public outgroup is an indicator* = 1 for being in the public outgroup treatment. The constant represents those in the private control treatment. All regressions include clustered *SE*s by participant.
* $p < .1$. ** $p < .05$. *** $p < .01$.

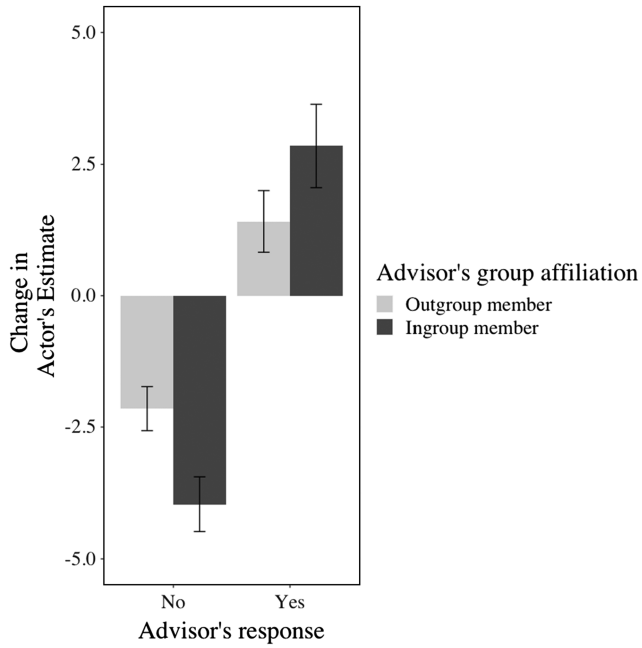
(*Appendices continue*)

Table A9
Linear Regression Results for the Change in the Participant's Estimate (Second Estimate–First Estimate)

	Dependent variable	
	Change in estimate	
Predictors	(1)	(2)
Advisor said yes	5.031***	3.563***
	(0.479)	(0.587)
Ingroup advisor		−1.814***
		(0.516)
Advisor Said Yes × Ingroup Advisor		3.245***
		(0.789)
Constant	−2.975***	−2.150***
	(0.335)	(0.419)

Note. *Advisor said yes is an indicator* = 1 when the advisor selected agreed with the policy statement and = 0 if they did not. *Ingroup advisor is an indicator* = 1 when the selected advisor belonged to the actor's ingroup and = 0 if they did not. All regressions include clustered *SE*s by participant.
*** $p < .01$.

Figure A1
Mean Change in Actor's Estimate by Condition and Advisor's Response



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant. This indicates that when an ingroup advisor says "no," actors lower their estimate by more than they do when outgroup advisors say "no." When an ingroup advisor says "yes," actors increase their estimate by more than if an outgroup advisor says "yes." These results indicate greater weight being placed on ingroup rather than outgroup advice.

(*Appendices continue*)

Table A10
Regression Results for Whether the Observer Chose the Actor With More Ingroup Advisor Selections

	Dependent variable			
	Chose actor with more ingroup advisor selections			
Predictors	OLS (1)	CLM (2)	OLS (3)	CLM (4)
Trust game			0.135***	0.655***
			(0.025)	(0.126)
Topic fixed effects	Yes	Yes	Yes	Yes
Constant	0.209***	0.892***	0.136***	0.557***
	(0.013)	(0.063)	(0.019)	(0.083)

Note. *Trust game is an indicator* = 1 when the participant was in the trust game condition and = 0 when the participant was in the estimation game condition. Topic fixed effects account for the effect of the eight different topics. All regressions include clustered *SE*s by participant and simple effects coding. Columns 1 and 3 report the results of linear regressions using the lm function in R. Columns 2 and 4 report the results of logistic regressions using the clm function in R. OLS = ordinal least squares linear regression; CLM = cumulative link model for ordinal regression.
*** $p < .01$.

Table A11
Regression Results for Whether the Observer Chose the Actor With More Ingroup Advisor Selections, by Number of Ingroup Advisors Selected

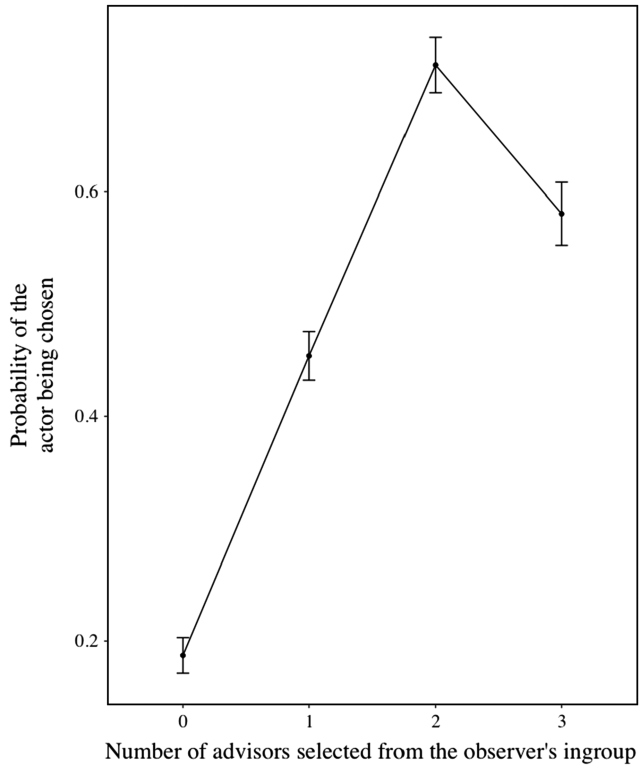
	Dependent variable			
	Chose actor with more ingroup advisor selections			
Predictors	OLS (1)	CLM (2)	OLS (3)	CLM (4)
One ingroup advisor	0.268***	1.288***	0.227***	0.994***
	(0.022)	(0.119)	(0.035)	(0.167)
Two ingroup advisors	0.527***	2.384***	0.424***	1.818***
	(0.025)	(0.144)	(0.041)	(0.201)
Three ingroup advisors	0.394***	1.798***	0.236***	1.031***
	(0.028)	(0.144)	(0.042)	(0.190)
Trust game			−0.136***	−0.905***
			(0.030)	(0.202)
One Ingroup Advisor × Trust Game			0.075*	0.658***
			(0.044)	(0.240)
Two Ingroup Advisors × Trust Game			0.191***	1.174***
			(0.049)	(0.286)
Three Ingroup Advisors × Trust Game			0.284***	1.512***
			(0.055)	(0.291)
Topic fixed effects	Yes	Yes	Yes	Yes
Constant	0.192***	−1.448***	0.265***	−1.022***
	(0.016)	(0.101)	(0.026)	(0.133)

Note. One, two, and three ingroup advisors are indicators for the number of advisors from the observer's ingroup that the actor selected. *Trust game is an indicator* = 1 for being in the trust game condition and = 0 for being in the estimation game condition. Topic fixed effects account for the effect of the eight different policy topics. All regressions include clustered *SE*s by participant and simple effects coding. Columns 1 and 3 report the results of linear regressions using the lm function in R. Columns 2 and 4 report the results of logistic regressions using the clm function in R. OLS = ordinal least squares linear regression; CLM = cumulative link model for ordinal regression.
* $p < .1$. *** $p < .01$.

(*Appendices continue*)

Figure A2

The Mean Probability That an Actor Was Chosen for a Future Incentivized Task Based on the Number of Advisors That the Actor Selected From the Observer's Ingroup



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad. As seen in this figure, the probability that observers chose a given actor increased when the actor goes from choosing zero to selecting one ingroup advisor (log odds = 1.28, $p < .001$) and when the actor goes from selecting one to selecting two ingroup advisors, $\chi^2(1, N = 5{,}348) = 213.75$, $p < .001$. However, there was a penalty for selecting three advisors from the observer's ingroup rather than two, as the probability of the actor being chosen for the bonus opportunity significantly decreased, $\chi^2(1, N = 5{,}348) = 49.14$, $p < .001$. (This "U-shaped" relationship was confirmed by a two-line test, indicating that there is indeed a positive relationship between selecting a lower number of ingroup advisors and being chosen by the observer, but that this relationship reverses when selecting a greater number of ingroup advisors [Simonsohn, 2018])

Preregistered Analysis for Magnitude of Selective Exposure Hypothesis

In our preregistration, we predicted that "an actor's choice to view at least one information source from the observer's ingroup will be a better predictor than the difference score between the two actor's choices." To test this, we ran the following regression, where

$$\text{Logit}(\text{ChoseMoreInGroup} - .5)_i = \beta_0 + \beta_1(\text{includeZero}_i) + \beta_2(\text{differenceScore}_i) + Z_i + \varepsilon_i \quad \text{(A1)}$$

- *IncludeZero* was binary variable representing when one of the actors chose zero ingroup sources versus neither actor chose zero ingroup sources.
- *differenceScore* was an ordinal variable with three levels (3 = *one actor chose three more ingroup sources than the other*, 2 = *one actor chose two more ingroup sources than the other*, 1 = *one actor chose one more ingroup source than the other*).

Specifically, we predicted that β_1 would be larger than β_2 analyzed using a linear hypothesis test where the null hypothesis is that $\beta_1 - \beta_2 = 0$. We found evidence consistent with this hypothesis ($\chi^2 = 50.48$, $p < .001$), however decided that there was a more natural way to test this question by simply looking at the probability that an actor was chosen based on the number of sources that they selected, reported in the main text.

(*Appendices continue*)

Experiment 5

Table A12

Regression Results for Whether the Observer Chose the Actor With More Ingroup Advisor Selections

	Dependent variable					
	Chose actor with more ingroup					
Predictors	OLS (1)	CLM (2)	OLS (3)	CLM (4)	OLS (5)	CLM (6)
Constant	0.164***	0.684***	0.125***	0.514***	0.117***	0.477***
	(0.010)	(0.043)	(0.013)	(0.057)	(0.014)	(0.060)
Trust game			0.078***	0.354***		
			(0.019)	(0.086)		
Public outgroup					0.093***	0.420***
					(0.019)	(0.086)
Topic fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note. *Trust game is an indicator* = 1 for being in the trust game condition and = 0 for being in the estimation game condition. *Public outgroup is an indicator* = 1 for being in the outgroup condition and = 0 for being in the ingroup condition. Topic fixed effects account for the effect of the eight different topics. All regressions include clustered *SE*s by participant and simple effects coding. Columns 1, 3, and 5 report the results of linear regressions using the lm function in R. Columns 2, 4, and 6 report the results of logistic regressions using the clm function in R. OLS = ordinal least squares linear regression; CLM = cumulative link model for ordinal regression.

*** $p < .01$.

Table A13

Regression Results for Whether the Observer Chose the Actor With More Ingroup Advisor Selections, by Number of Ingroup Advisors Selected

	Dependent variable					
	Chose actor with more ingroup					
Predictors	(1)	(2)	(3)	(4)	(5)	(6)
One ingroup advisor	0.209***	0.992***	0.210***	0.944***	0.183***	0.843***
	(0.016)	(0.086)	(0.024)	(0.117)	(0.025)	(0.124)
Two ingroup advisors	0.466***	2.057***	0.425***	1.832***	0.397***	1.713***
	(0.019)	(0.101)	(0.027)	(0.139)	(0.027)	(0.138)
Three ingroup advisors	0.329***	1.476***	0.241***	1.068***	0.291***	1.278***
	(0.021)	(0.103)	(0.029)	(0.136)	(0.030)	(0.145)
Trust game			−0.068***	−0.411***		
			(0.025)	(0.151)		
Public outgroup					−0.044*	−0.260*
					(0.025)	(0.148)
One Ingroup Advisor × Trust Game			0.0002	0.131		
			(0.032)	(0.173)		
Two Ingroup Advisors × Trust Game			0.087**	0.495**		
			(0.037)	(0.201)		
Three Ingroup Advisors × Trust Game			0.174***	0.840***		
			(0.041)	(0.207)		
One Ingroup Advisor × Public Outgroup					0.041	0.247
					(0.033)	(0.170)
Two Ingroup Advisors × Public Outgroup					0.128***	0.644***
					(0.037)	(0.200)
Three Ingroup Advisors × Public Outgroup					0.070*	0.365*
					(0.042)	(0.204)
Topic fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.211***	−1.315***	0.244***	−1.132***	0.240***	−1.153***
	(0.013)	(0.076)	(0.019)	(0.100)	(0.020)	(0.110)

Note. One, two, and three ingroup advisors are indicators for the number of advisors from the observer's ingroup that the actor selected. *Trust game is an indicator* = 1 for being in the trust game condition and = 0 for being in the estimation game condition. *Public outgroup is an indicator* = 1 for being in the outgroup condition and = 0 for being in the ingroup condition. Topic fixed effects account for the effect of the eight different policy topics. All regressions include clustered *SE*s by participant and simple effects coding. Columns 1, 3, and 5 report the results of linear regressions using the lm function in R. Columns 2, 4, and 6 report the results of logistic regressions using the clm function in R.

* $p < .1$. ** $p < .05$. *** $p < .01$.

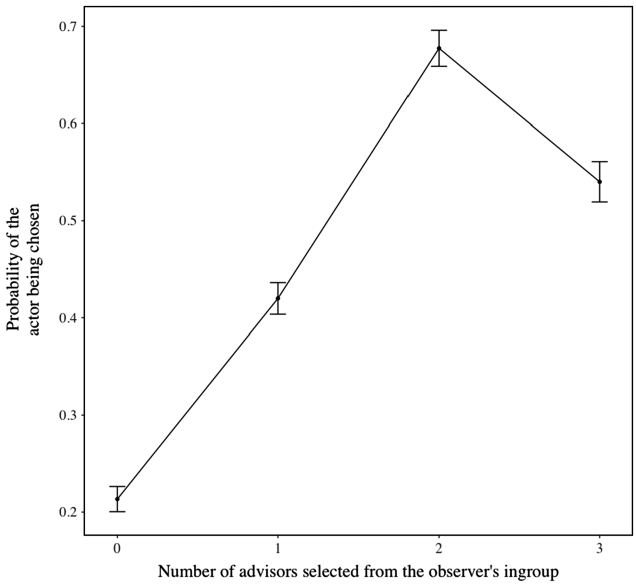
(*Appendices continue*)

Table A14
Results as a Function of the Information Selection Decisions Made by Both Actors in a Given Pair

Actor's choices of observers' ingroup advisors	0 vs. 1	0 vs. 2	0 vs. 3	1 vs. 2	1 vs. 3	2 vs. 3
Percent of observers who chose the actor with more ingroup cards (%)	82	78	74	72	57	46
N	659	658	295	1,959	885	1,062

Note. The top row represents the two actors' selections (e.g., in the "0–1" column, the observer chose between an actor who selected zero advisors from their ingroup and an actor who selected one). The middle row represents the percentage of observers who chose the actor who selected a greater number of advisors from the observer's ingroup. The bottom row is the number of times each pairing appeared. Due to our random sampling strategy, observers were presented with the choice between an actor who selected one and an actor who selected two advisors from the observer's ingroup most often.

Figure A3
Probability That an Actor Was Chosen Based on the Number of Advisors That the Actor Selected From the Observer's Ingroup

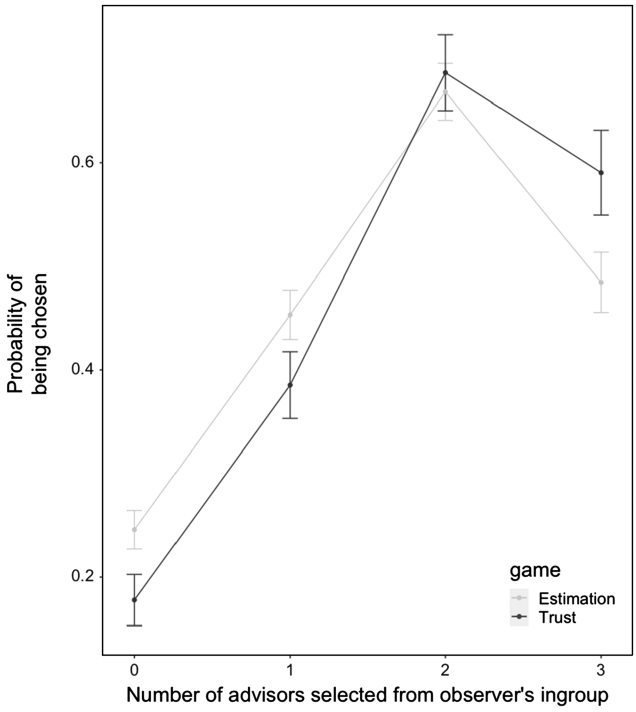


Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad.

(*Appendices continue*)

Figure A4

Probability That an Actor Was Chosen Based on the Number of Advisors That the Actor Selected From the Observer's Ingroup, by Game Type



Note. Error bars represent ± 1 *SE* of the group mean, clustered by participant and actor dyad.

Preregistered Analysis for Magnitude of Selective Exposure Hypothesis

In our preregistration, we predicted that "an actor's choice to view at least one information source from the observer's ingroup will be a better predictor than the difference score between the two actor's choices." To test this, we ran the following regression, where

$$\text{Logit}(\text{ChoseMoreInGroup} - .5)_i = \beta_0 + \beta_1(\text{includeZero}_i) + \beta_2(\text{differenceScore}_i) + Z_i + \varepsilon_i \quad \text{(A2)}$$

- *IncludeZero* was binary variable representing when one of the actors chose zero ingroup sources versus neither actor chose zero ingroup sources.
- *differenceScore* was an ordinal variable with three levels (3 = *one actor chose three more ingroup sources than the other*, 2 = *one actor chose two more ingroup sources than the other*, 1 = *one actor chose one more ingroup source than the other*).

Specifically, we predicted that β_1 would be larger than β_2 analyzed using a linear hypothesis test where the null hypothesis is that $\beta_1 - \beta_2 = 0$. We found evidence consistent with this hypothesis ($\chi^2 = 86.59$, $p < .001$), however decided that there was a more natural way to test this question by simply looking at the probability that an actor was chosen based on the number of sources that they selected, reported in the main text.

(*Appendices continue*)

Appendix B

Replication Study

The replication study investigates the reputational causes of selective exposure to partisan information using the traditional selective exposure paradigm described in Experiment 1. All participants had the opportunity to select five out of 10 pieces of information, evenly balanced between information arising from ingroup versus outgroup sources. We systematically varied whether actors information selection decisions were observed by members of their political ingroup or members of their political outgroup. In both conditions, actors learned that the observer would choose whether or not to send them money in a financially incentivized trust game based on their selections. Based on the social signaling model, we hypothesized that actors being evaluated by ingroup observers would select more ideologically aligned information and less ideologically misaligned information than actors being evaluated by outgroup observers for both types of information choices (news stories and senators' webpages).

Method

Participants

We recruited a sample of U.S. residents from Cloud Research (Litman et al., 2017) to participate in a 3- to 4-min experiment in exchange for $0.50, with further opportunities for a bonus. Our final sample consisted of 364 participants ($M_{age} = 39.7$; 47.5% female, 51.9% male, 0.5% nonbinary; 64.0% liberal, 36.0% conservative). To achieve 80% power to detect a small- to medium-sized main effect ($d = .40$) across each information type, we aimed to recruit 100 participants per experimental condition.

Procedure

Upon entering the study, participants first reported basic demographic information, including gender, age, and education. Following prior research (American National Election Studies, 2016), they indicated their political ideology on a 7-point scale from *extremely liberal* to *extremely conservative*. After reporting demographics, participants had the option to select five of 10 information sources presented to them and were told that an observer would evaluate their choices. At the end of the study, participants reported how much they would return to an observer in a trust game and were redirected to view these information sources.

Dependent Variable

All actors were presented with a balanced set of five liberal sources and five conservative information sources. Actors' selection of the number of ingroup information sources to view served as our dependent variable. This number ranged from zero to five.

Independent Variables

All participants learned that their choices in the study would be communicated to an observer who would use that information to make decisions in a trust game (Berg et al., 1995). Specifically, the observer would have the opportunity to send the actor between 0–10 cents and that we (the experimenters) would triple whatever amount was sent by the observer. The actors would then have the opportunity to send back any amount of money they felt appropriate. Further implementation details can be found in Table A1.

Before making information selection decisions, participants were randomly assigned to one of four between-subject experimental conditions in a 2×2 factorial design. For the first factor, we varied whether the observer was described as a political ingroup member (ingroup condition) or a political outgroup member (outgroup condition).

The second factor varied the type of information actors were asked to consider. Specifically, as in Experiment 1, we instructed actors to select five information sources from a list of 10 current U.S. Senators' press pages (senators condition) or from a list of 10 news stories (news condition). We provided participants with links to view their selected information sources at the end of the survey.

Analysis Plan

We used a linear regression, using the lm function in R to predict the number of ingroup information sources selected by participants as a function of experimental condition. Each condition was dummy coded such that $ingroup = 0$ and $outgroup = 1$ for the first factor and $news = 0$ and $senators = 1$ for the second factor.

Results

Reputational Causes of Selective Exposure

We began by examining the level of selective exposure between the ingroup and outgroup conditions. On average, collapsing across information type, actors in the ingroup condition selected 4.14 ingroup information sources ($SD = 1.10$). In contrast, participants in the outgroup condition selected 2.37 ingroup information sources ($SD = 1.58$), on average. This difference was substantial and significant for both those in the senators condition, $t(163) = 8.67$, $p < .001$, and those in the news condition, $t(156) = 9.00$, $p < .001$. Results are presented in Figure B1 and Table B1.

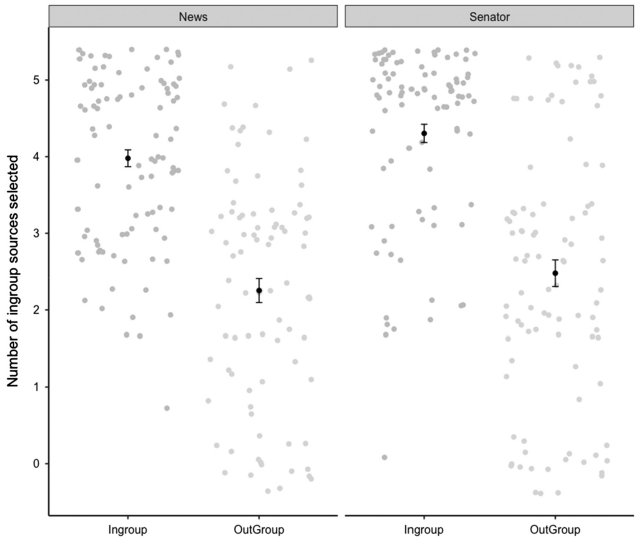
To put these results in perspective and think about the effect size, we ran an exploratory simulation in which we randomly drew 10,000 pairs of participants, one from the ingroup condition and one from the outgroup condition. For each pair, we then assessed how often the participant from the ingroup condition selected more ingroup sources than the participant from the outgroup condition (McGraw & Wong, 1992). Participants in the ingroup condition selected more ingroup sources than their randomly selected match in the outgroup condition 72.9% of the time, and the reverse just 11.7% of the time (the remaining 15.5% of pairs selected an equal number of ingroup sources). Taken together, our data are consistent with the hypothesis that, in partisan environments, reputational considerations powerfully drive information selection decisions.

In exploratory analyses, the overall average number of ingroup information choices chosen was 3.26, which is significantly above 2.50, the amount that would have represented even exposure to

(*Appendices continue*)

Figure B1

Number of Chosen Ingroup Information Sources by Condition



Note. Error bars represent ± 1 *SE* of the group mean. Participants in the ingroup conditions chose more ingroup information sources than those in the outgroup conditions; however, there were no significant differences between the information type conditions.

ingroup and outgroup sources, $t(363) = 8.92$, $p < .001$. However, in the presence of an outgroup observer, this mean falls to 2.37 and becomes indistinguishable from the 2.50 baseline, $t(180) = -1.10$, $p = 0.27$. This suggests that at least in this context, reputational incentives were powerful enough to eliminate the tendency toward selective exposure.

Table B1

Regression Results for the Number of Ingroup Sources Chosen by Condition

	Dependent variable			
	Number of ingroup sources chosen			
Predictors	(1) Senators	(2) News	(3) All	(4) All
Senators				0.325
				(0.201)
Outgroup	−1.825***	−1.726***	−1.766***	−1.726***
	(0.213)	(0.189)	(0.143)	(0.202)
Senators × Outgroup				−0.099
				(0.285)
Constant	4.303***	3.979***	4.137***	3.979***
	(0.152)	(0.131)	(0.101)	(0.140)
Observations	183	181	364	364
R^2	.289	.317	.297	.305
F statistic	73.608*** (*df* = 1; 181)	82.954*** (*df* = 1; 179)	153.006*** (*df* = 1; 362)	52.550*** (*df* = 3; 360)

Note. *Outgroup is an indicator* equal to 1 for being in the outgroup condition. *Senators is an indicator* equal to 1 for being in the senators condition. Column 1 restricts data to those in the senator condition. Column 2 restricts data to those in the news condition. Columns 3–4 include all participants. All columns report the results of linear regressions using the lm function in R.
*** $p < .01$.

(*Appendices continue*)

Information Source

We next conducted an exploratory analysis of whether the pattern of effects described above was contingent on the type of information (i.e., senators' webpages vs. news stories). This did not appear to be the case. When using a linear regression to predict the number of ingroup information sources selected by actors as a function of whether the participant was in the ingroup or outgroup condition, whether the participant was in the news or senators condition, and their interaction, we found no evidence of an interaction between the social incentive treatment and information type ($\beta_{\text{information source} \times \text{group}} = -.10$, $p = .73$). Thus, it appears that the signaling motivation for selective exposure generalizes across multiple information types. We detail these results in Figure B1 and Table B1. Additionally, in exploratory analyses using a *t*-test to compare the average number of ingroup sources chosen between information source conditions, we found no evidence of a significant main effect ($M_{\text{news articles}} = 3.15$, $M_{\text{senators}} = 3.37$), $t(359) = 1.28$, $p = .20$, on selective exposure. In other words, people were no more averse to engaging with information arising from a specific individual versus information that was broadly supportive of a point of view, but not specifically associated with one person.

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