Use Your Inside Voice: Intra-Party Social Pressure and the Avoidance of Political Speech

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Abstract

Citizens increasingly report censoring their political beliefs to avoid social backlash from their in-group, yet research on partisanship largely ignores this dynamic, focusing solely on cross-party conflict. This oversight obscures a critical question: is self-censorship driven by partisan norms policing dissent or a general aversion to conflict that silences loyalists and dissenters alike? Using a nationwide survey experiment conducted during the 2024 primaries (N = 17,691), we find that partisans overestimate the likelihood of social sanctions for expressing their views. Experimentally correcting these exaggerated fears significantly reduces self-censorship for both loyalists and dissenters. However, a gap persists: after correction, dissenters remain less willing than loyalists to discuss their preferences with co-partisans. Our findings suggest that while both minorities and majorities face social pressure, minorities possess fewer positive motivations to share their beliefs. Reconciling the competing perspectives on intra-party social pressure offers important insights into partisan identity, polarization, and voter behavior.[1]

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[1]Artificial intelligence tools were used to check for typos in this manuscript and to verify the word count

Introduction

The extent to which partisan identities shape citizens' social preferences—thinning the network of people with whom they form relationships (Iyengar and Westwood, 2015; Klar, Krupnikov and Ryan, 2018), reside (Brown and Enos, 2021), and willingly discuss politics (Settle and Carlson, 2019; Minozzi et al., 2020)—is hotly debated across the social sciences. Research in this domain has been devoted almost exclusively to understanding how social contact *between* the political parties deteriorates. Yet, intra-party factional cleavages are a ubiquitous feature of partisan politics (Key, 1949; Cohen et al., 2016; Young and de Wit, 2025) and constitute a recurrent source of ideological division and animosity among otherwise like-minded peers. Whether co-partisans belonging to opposing factions embrace or reject conversations with one another has important ramifications for public opinion and parties' electoral success, raising the question: how does a shared partisan identity impede or facilitate speech *within* parties?

Theories of partisan norms and political communication offer competing intuitions. The former contends that partisan networks are governed by social norms dictating what right-thinking Democrats and Republicans should say and think (Groenendyk, Kimbrough and Pickup, 2023; Pickup, Kimbrough and de Rooij, 2022). By policing the specific content of speech, injunctive partisan norms are likely to result in disproportionate self-censorship among those who hold dissenting views that are unpopular within the party (Spelman et al., 2024). Social scientists have long warned that pressures to socially conform can impede deliberation by deterring minority opinion-holders from expressing their sincere opinions with peers (Noelle-Neumann, 1974; Huckfeldt and Sprague, 1987). Partisan social norms have indeed been shown to preserve support for the Democratic party among otherwise ideologically conservative Black voters (White and Laird, 2020) and to stigmatize support for fringe parties in Europe (Valentim, 2024*b*).

On the other hand, studies of political conversation have documented a sweeping aversion to discussing contentious issues that clearly transcends injunctive norms. Whether for fear of conflict or of appearing overzealous (Gerber et al., 2012; Eliasoph, 1998), people per-

ceive social repercussions to conversations with adversaries irrespective of their own minority or majority status (Carlson and Settle, 2022). From this perspective, self-censorship with co-partisan opponents may have little to do with the normative standing or popularity of one's belief, and the party faithful may be just as wary of the social costs to speech as party dissenters.

Understanding whether self-censorship within parties arises from social constraints imposed by the majority or from conflict avoidance offers important insights into the forces responsible for mass polarization and partisan voting behavior. Exposure to contrary perspectives produces better-informed opinions and reduces polarization by improving citizens' understanding of legitimate rationales for opposing viewpoints (Fishkin, 1991; Mutz, 2002). In an era where sincere deliberation between ideological and partisan opponents is rare (Gentzkow and Shapiro, 2011; Settle and Carlson, 2019) and partisan group cues dictate voters' receptiveness to factual information and opinion leadership (Barber and Pope, 2019; Coppock, 2023; Carey et al., 2025), norms that silence co-partisans' counter-partisan views may deprive citizens of a crucial source of cross-cutting exposure. Moreover, with factional division on the rise, cross-cutting exposure can accelerate party defection (Groenendyk, Sances and Zhirkov, 2020; Young and de Wit, 2025). How do parties keep the electoral effects of factional division in check?[2]

In this paper, we outline a theory of the pressures shaping intra-party discourse that strikes a middle ground. Consistent with the partisan norms theory, we show that party minorities are significantly more averse to discussing an upcoming election with peers in the party majority than vice versa. So pronounced is the gap that, whereas dissenters are more averse to speaking with the party majority than with out-partisans, party majorities are more *eager* to speak with dissenters than even their typical discussion network. This finding—which challenges theories rooted solely in conflict avoidance—suggests that willing-

[2]Anecdotal evidence from the 2024 U.S. presidential election suggests that intra-party social pressure deters speech that could threaten the party. Republican voters reported that they risked ostracism or even outright violence if they expressed misgivings about Donald Trump to peers (Tabet, 2023), so much so that the Democratic campaign implored women in Republican households to secretly vote for Kamala Harris (McCammon, 2024). Even so, it is not self-evident that minority factions within the parties faced greater social constraints on speech than majority factions. Both Never-Trump Republicans and uncommitted Democrats sustained highly vocal movements throughout the primary season (Aratani, 2024; Abdul-Hakim et al., 2024).

ness to speak depends on the normative standing of one's beliefs. However, consistent with the conflict avoidance view, we show that intra-party social pressure does not exclusively affect dissenters. Reducing the perceived risk of social sanctions renders party minorities and majorities alike less averse to discussing sensitive issues with co-partisan opponents. Therefore, social pressure is likely insufficient to explain the disproportionate self-censorship of party dissenters. We investigate how alternative psychological motivations could produce patterns of political speech consistent with the theory of injunctive partisan norms.

Our evidence hinges on a careful distinction between an asymmetry in party majorities and minorities' willingness to speak and the share of this asymmetry attributable to social pressure. Disentangling these requires an empirical strategy that differs from existing paradigms in studies of social pressure (e.g., Asch, 1951; Gerber, Green and Larimer, 2008; Tankard and Paluck, 2016). We show that respondents widely overestimate the frequency with which Americans at large face social sanction for political disagreement. Correcting these misperceptions via an "ask-tell" intervention (e.g., Braley et al., 2023; Mernyk et al., 2022) enables us to shape the reputational concerns brought to mind when respondents consider discussing their 2024 vote choice with co-partisans. This design directly intervenes on respondents' perceptions of the social cost to disagreeable speech, isolating social pressure from other motivations primed by social observation. To assess if social pressure can explain an asymmetry in self-censorship, we test whether this exogenous change in social pressure exhibits heterogeneous effects between dissenters and the party faithful.

This experiment was embedded in a large, nationwide survey ($N = 21,400$) fielded during the summer before the 2024 U.S. presidential election. Capitalizing on tensions surrounding the presidential primary, we define dissenters as partisans who did not support their party's 2024 nominee. Because dissenters constituted a small minority within each party, a large sample size is crucial for our assessments of treatment effect heterogeneity; many studies investigating speech pressures are simply unable to detect the type of sub-group effect that concerns this paper. To assess if the ask-tell correction improves willingness to speak, we used "compensation demand" or "willingness-to-accept" survey measures, which elicit the minimum price respondents would need to be paid to complete an assignment

(Settle and Carlson, 2019).

We find that on the topic of 2024 vote choice, members of the party faithful demanded substantially lower prices to speak with party dissenters they knew than vice versa. Exposure to the ask-tell correction reduced the prices party dissenters demanded to speak with co-partisan opponents, confirming that social pressure constrains intra-party speech for those in the minority. However, ask-tell exposure also reduced the prices demanded by members of the party faithful to speak with co-partisan opponents, with no significant difference in conditional average treatment effects between dissenters and the party faithful. We similarly find little evidence that being in the minority within one's private network is associated with a greater ask-tell effect. Further, ask-tell exposure reduced compensation demands by similar amounts for other peers in respondents' networks, such as out-partisans and people with whom respondents frequently discuss politics to begin with. Intra-party social pressure better resembles a general aversion to political conflict than the product of injunctive norms stigmatizing specific opinions.

Our conjecture that asymmetry in party majorities' and minorities' willingness to discuss politics cannot be fully explained by social pressure invites an alternative explanation. We demonstrate with a follow-up survey that when asked to consider the reasons why they may initiate discussions about the election with co-partisan opponents, dissenters are considerably less likely to see persuading their peers to change their mind as a possible benefit. Meanwhile, party dissenters and the party faithful are equally likely to cite damaging relationships as reasons for avoiding discussions about the election. We offer this as suggestive evidence that party majorities' unusual zeal for discussing the election with possible defectors is better explained by the instrumental benefits they derive from persuading wayward co-partisans than by injunctive norms shielding majorities from fears of social repercussion.

These findings add important nuance to theories of partisan identity. Existing research has demonstrated Americans' cognizance of injunctive partisan norms but not the mechanism by which these norms are enforced. Our evidence suggests that even as co-partisan networks favor speech conforming to injunctive norms, they lack the network density, institutions, or historical roots that render social pressure a powerful tool of norm enforcement for other

social identities (e.g., White and Laird, 2020). Notably, despite the durability of partisanship as social identity and the extreme favoritism voters show toward co- over out-partisans (Green, Palmquist and Schickler, 2002; Mason, 2018), partisanship ranks low on the list of identities Americans tend to name as personally important (Krupnikov and Ryan, 2022). Specific non-political social groups that overlap with partisan identity may be required to more effectively deter speech against the party line.

We begin by defining how social pressure differs from other considerations shaping openness to political speech, then illustrate how the majority influence and conflict avoidance views emerge as divergent intuitions from the existing literature. We subsequently describe why our empirical design differs from past studies. The remainder of the paper elaborates methods and results. We conclude with theoretical implications as well as a discussion of how our empirical design advances the study of social influence more broadly.

Defining Considerations Shaping Political Speech

The decision to engage in political speech reflects many motives, but here we focus on considerations related to the suppression of speech that raise particular normative concerns. From Nixon's appeals to a "Silent Majority" (Gillion, 2020, chapter 1) to contemporary coverage of "cancel culture" (Dias, Druckman and Levendusky, 2024), the worry that political debate may be stifled by citizens intolerant of their peers' opposing views is an enduring fixture of American politics. These concerns stem from social pressure: the psychological constraints that flow from "a basic human drive to win praise and avoid chastisement" (Green and Gerber, 2010, 331). This basic drive bears most on what scholars of social influence have termed "affiliative" motivations, in contrast to "accuracy" and "affirmation" goals (Cialdini and Goldstein, 2004; Carlson and Settle, 2022). Though this fear of scorn motivates members of the minority to heed norms dictating what those in their community deem appropriate speech, it can likewise deter speech from majority opinion-holders, who fear that arguments with the opposing faction will induce hostility or cause irreparable damage to important relationships. The causes of social pressure therefore include but are not limited to social

norms governing important speech.

Moreover, not all social norms operate through a social pressure mechanism. Scholars differentiate injunctive norms—which capture beliefs about acceptable behavior, and therefore often rely on social pressure for enforcement—from descriptive norms, which characterize beliefs about others' behavior (Deutsch and Gerard, 1955; Cialdini, Reno and Kallgren, 1990). Injunctive norms surrounding political participation and prosocial behavior explain why making people aware that their actions will be observed by peers has proven an effective means of inducing acts from electoral turnout (Gerber, Green and Larimer, 2008) to recycling (Schultz, 1999). Still, while social pressure implies external enforcement, injunctive norms that are internalized can continue to constrain behavior even in observers' absence. Descriptive norms can similarly constrain behavior without social pressure. Many experimental "norm change interventions" (Tankard and Paluck, 2016) act by altering descriptive norms: for example, information about whether a majority of one's peers will turn out to vote can alter electoral participation (Gerber and Rogers, 2009). People may imitate their surroundings not because they are pressured to do so but because they infer that, if others unanimously or even widely act this way, the behavior must be somehow sensible or efficient (Cialdini, Kallgren and Reno, 1991, 203).

In this paper, we seek to theoretically and empirically distinguish social pressure from other considerations that shape speech, including descriptive norms, non-social pressures, and the instrumental value of persuasion. Partisan minorities may be less willing to speak not because of social pressure, but because cross-cutting partisan and electoral preferences dampen their positive motivations to discuss their beliefs. Dissidents within a party may resemble the cross-pressured voters who, as Mutz (2002) argued, are less likely to share their beliefs, vote, or engage in political activities. Weaker partisan attachments may also correlate with socioeconomic, attitudinal, and informational traits associated with lower participation, with personality traits that increase the perceived cost of political speech, or with less informed or certain preferences (Brady, Verba and Schlozman, 1995; Gerber et al., 2012, 2013). In this paper, we assess the extent to which social pressure is specifically responsible for asymmetries in co-partisans' self-censorship.

Majority Influence View

Though partisanship's role as a social identity is well-established, there is debate about to what extent it is as powerful as other social identities at imposing norms of appropriate attitudes on party members. A set of theories argues that minority groups within parties face a particular social cost to expressing their beliefs to co-partisans. We describe this perspective as the "majority influence" view.

There is evidence that injunctive partisan norms serve to encourage the expression of the majority belief within parties. Groenendyk, Kimbrough and Pickup (2023) show that considering whether one's co-partisans would approve of them supporting various policy positions causes Americans to update their preferences towards the opinions held by majorities of co-partisans, suggesting voters are quite cognizant of partisan injunctive norms. This dovetails with evidence that strengthening partisan identity—for instance, through a randomized encouragement to formally register with a party—increases stereotypical partisan attitudes and improves evaluations of party leaders (Gerber, Huber and Washington, 2010). Spelman et al. (2024), in the paper perhaps most closely connected to our own, find that self-censorship is asymmetric between minority and majority opinion holders within each party; analogizing the special risks faced by dissenters to common forms of social ostracism.

Whether social pressure explains conformity to the party majority is far from evident. Asymmetry in party majorities' and minorities' self-censorship, for instance, could instead reflect dissidents holding fewer positive motivations to persuade the party faithful to change their vote than vice versa. While Spelman et al. (2024) illustrate how misperceptions about co-partisans' enthusiasm to deliver social sanctions predict this reluctance to speak, the causal role of these misperceptions is left unclear.

When partisanship overlaps with other group identities, these identities can supply the requisite mechanisms for norm enforcement. White and Laird (2020) argue that support for the Democratic Party is an injunctive social norm among Black Americans and that this norm preserves Democratic partisan identification, voting, and donations among otherwise ideologically conservative Black voters. Yet, this mechanism depends on within-group dy-

namics among Black Americans unlikely to be felt by partisans more broadly, especially given evidence that Black voters exhibit unusually resilient Democratic party attachments relative to other constituencies. The decline of locally rooted party organizations with a tangible presence in voters' daily lives means contemporary parties have limited tools for socially enforcing speech norms on their members (Schlozman and Rosenfeld, 2019, 2024). As Schlozman and Rosenfeld describe it, "Party identification drives American politics – but party loyalty, in the older sense of the term, has atrophied" (2019, 121).

Beyond the context of partisanship, however, there is evidence that minority views across contexts may face particular speech pressures. Social scientific research on conformity traces its lineage to the seminal line-judgment experiments of Asch (1956), which in the paradigmatic case asked subjects to publicly state whether two plainly different lines were of equal length after a room of confederates unanimously pronounced the lines identical. The idea that perceiving oneself to be in the minority constrains speech has since featured prominently in scholarship about public deliberation, particularly in Noelle-Neumann's (1974) "spiral of silence" theory, which posits that citizens maintain a "quasi-statistical" sense of the distribution of opinion in society and minority opinion-holders withhold their true preferences in public settings for fear of isolation.[3]

Decades of empirical study testify to a proclivity among Americans (as well as citizens of comparable democracies) to condone speech restrictions on political opponents who hold particularly stigmatized, minority beliefs (Sullivan, Piereson and Marcus, 1979; Chong, Citrin and Levy, 2024; Dias, Druckman and Levendusky, 2024). Drawing on the classic sociological work of Goffman (1963), Valentim (2024*b*) analogizes the source of social pressure constraining support for fringe candidates to political *stigma*, a judgment of "undesired differentness" in one's beliefs and by extension one's character (1384). Valentim (2024*a*,*b*) argues that radical right parties in Europe are subject to stigma, that other small parties do not face this concern, and that this mechanism can reduce vote share. This theory suggests

[3]Neither study should be interpreted as demonstrating that majority influence is ubiquitous. Asch emphasizes that only one-third of subjects in fact conformed in the maximal case. Further, empirical examinations have failed to substantiate the prediction that being of the minority view mechanically reduces people's willingness to express that opinion, as a narrow reading of the spiral of silence theory might imply, as a narrow reading of the spiral of silence theory might imply (Glynn, Hayes and Shanahan, 1997).

that minority status is a necessary but insufficient condition for social pressure. Instead, stigma is a property of specific beliefs that violate injunctive norms, and citizens are willing to punish others who violate these norms.

Recent work on "political correctness" in colleges extends evidence for majority influence. Ho and Huang (2024) show that undergraduates misread silence as endorsement of the majority view; correcting this mistaken inference raises political minorities' willingness to speak without changing majority behavior. The mechanism is descriptive rather than injunctive, since the intervention shifts second-order beliefs about peers' support for one's views. Similarly, making social-image concerns salient (by hinting that responses might be shared with peers) reduces willingness to speak among students holding minority opinions but not among majorities (Braghieri, 2024). Our task is to assess whether these behavioral patterns apply to one-on-one conversations between intra-party opponents.

Conflict Avoidance View

In contrast to the majority influence view, which predicts asymmetry between minorities and majorities, other literature suggests that fear of social sanction and broader willingness to speak might be considerably more uniform. For as long as scholars have recognized the role of majority influence in constraining speech, they have understood in equal measure that daily life is, for most people, not deeply steeped in politics (Berelson, Lazarsfeld and McPhee, 1954; Prior, 2005). In her canonical examination of why even privately political volunteers and activists project an apathetic attitude around their peers, Eliasoph (1998) argued that rather than reflecting true disinterest, avoiding politics often reflects a considered effort to assess and maintain the boundaries of "political manners" or "etiquette" (21). These injunctive norms governing etiquette are not substantive but rather apply to all, irrespective of the content of their beliefs. People recognize that, in addition to the inevitable discomfort raised with certain political topics, appearing excessively invested in politics may turn off those less politically involved (Krupnikov and Ryan, 2022). Social pressure to self-censor may derive from the simple desire to avoid experiencing discomfort or inflicting it upon others.

A related set of evidence suggests that disagreement alone increases the cost of political speech. Crucially, many of these papers are agnostic, both theoretically and empirically, about the role of majority status. Gerber et al. (2012) find that discussant-level disagreement predicts dramatically decreased frequency of political discussion. More broadly, surveys using self-reports of discussion frequency acknowledge that both network composition and discussion frequency are endogenous to tolerance for disagreement on political topics. These studies use correlational data or structural models to suggest that dyad-level disagreement leads to a diminished willingness to discuss politics (Huckfeldt, Mendez and Osborn, 2004; Huckfeldt and Mendez, 2008). Even within a given dyad, topic-level agreement affects reported discussion frequency (Gerber et al., 2012).

While the bulk of the literature on disagreement in discussion networks is observational, there is some causal evidence that disagreement alone generates social costs. Settle and Carlson (2019) show experimentally that respondents demand more compensation to speak with opposing partisans than with co-partisans, suggesting that agreement lowers the costs of speaking, though they do not assess the role of majority status. Experimental tests of affective polarization find that partisans are strongly unwilling to interact with each other across a range of contexts (Iyengar and Westwood, 2015). Recent evidence suggests similar tensions exist within parties (Young and de Wit, 2025). Whether this sort of social identity-driven conflict behaves asymmetrically between minority and majority groups remains an open question.

These theories suggest that the existence of political conflict introduces social repercussions for all forms of contentious political speech. If the concern that talking politics will damage existing affiliations hinges not upon the stigmatization of one's specific beliefs but upon a general wariness of political tension and a dislike of conflict, majority and minority opinion-holders should differ little in the extent to which they experience social pressure.

Measuring Willingness to Speak and Social Pressure

The alternating theoretical perspectives we discussed make predictions not only about which groups will feel higher costs to speech, including all possible mechanisms, but about which groups will specifically feel more social pressure. The *majority influence view* suggests not only that partisan minorities will be less willing to speak, but also that this gap will be because of social pressure; those who hold majority beliefs should in this view be subject to no social pressure. In contrast, the *conflict avoidance view* suggests uniformity. That is, both majorities and minorities will face comparable social pressure concerns.

Identifying the social pressure latent in respondents' real networks presents distinctive measurement challenges. One approach studies self-censorship by testing whether self-reported perceptions of an intolerant climate predict lower willingness to speak (Gibson and Sutherland, 2023; Gibson, 1992). While useful, such perceptions may be unreliable given the politicized nature of claims that free speech is stifled (Menzner and Traunmüller, 2023). These data are also blunt, offering little leverage on how pressure varies across specific conversational partners. Network surveys often use "name generators," in which respondents list "alters" with whom they regularly discuss politics (Burt, 1984; Huckfeldt and Sprague, 1987), but these designs provide no information about attitudes or social pressure in hypothetical conversations with individuals not named as discussants.[4]

In light of these methodological concerns, Settle and Carlson (2019) use a "compensation demand" measure, asking respondents their financial demands for participating in conversations with groups with randomized political identities. By inviting people to quantify the perceived cost of an activity, this approach closely resembles open-ended forms of the "willingness to accept" (WTA) measure developed in contingent valuation studies (Venkatachalam, 2004). Normally, accurately estimating such a nonmarket price would demand that researchers attend closely to incentives for strategic misrepresentation and carefully minimize anchoring effects (Arrow et al., 1993; Green et al., 1998). In addition, nonmarket

[4]Some surveys address this by using role-based generators that ask for alters who fill specific roles (e.g., neighbors or friends). We are not aware of recent surveys that pair role-based generators with measures of respondents' affiliative concerns about the named alters. Name generators can also lengthen and complicate instruments, limiting their use.

goods such as political conversations may produce inflated WTA values that diverge from what people would in fact be "willing to pay" for avoiding an equivalent activity (Hanemann, 1991; Shogren et al., 1994). These concerns are less pressing when, as in this paper, we are interested only in assessing whether different groups perceive differential costs, and not in an accurate price estimate for its own sake.

We build on Settle and Carlson's approach to evaluate whether party minorities and majorities perceive differential costs to honestly expressing their electoral views with co-partisan opponents. That paper notes that compensation demands (hereafter, CDs) sum across the full set of considerations bearing on one's choice to talk politics, not just costs but even perceived benefits such as persuading peers to share one's view or expressing deeply held commitments (Carlson and Settle, 2022, chapter 3). Further, the average difference in CD prices when varying the identity of the discussant can also reflect perceptions of a lower likelihood of productive debate and an increased risk of having to defend uncertain opinions.

Quantifying intra-party social pressure therefore requires a novel measurement strategy that can *decompose* the CD price for co-partisan conversation into its constitutive elements. We operationalize social pressure as the extent to which willingness to speak *responds* to an exogenous change in the risk of social sanction for speech, defining it as an essentially causal quantity: the amount by which speech concerns are alleviated when the threat of sanction is reduced. Examining whether an intervention that mitigates fears of social sanction subsequently reduces CD prices for hypothetical political conversations with discussants of various identities enables us to measure (a) whether social pressure is prevalent in co-partisan interactions and (b) whether heterogeneous treatment effects in response to social-pressure alleviation reveal symmetry or asymmetry in the degree to which party defectors and the party faithful experience social pressure in these contexts.

Methods

We embedded an experiment in a large, nationwide survey (N = 21,400) fielded through the Civic Health and Institutions Project (CHIP50).[5] The data were part of the broader wave 32 collection and were collected between June 18th and July 28th, 2024 by PureSpectrum, an online panel management platform. Respondents were collected from all 50 states and D.C. Following our pre-registration, we subset our main analyses to the 17,691 partisans, including self-identified partisans and Democrat- or Republican-leaning independents.[6]

The survey occurred at a crucial moment for intra-party minorities, as both parties' nominees appeared certain but influential factions—including Never-Trump Republicans and Democrats opposed to the Biden administration's handling of Israel-Palestine—spoke against their respective nominees. Within each group, respondents who said they would vote for their party's respective nominee (Biden for Democrats, Trump for Republicans) if the "election were held today" are considered part of the *party majority*, whereas respondents who gave any other response (including "Not sure" or "I would not vote") are categorized as the *party minority*. Due to the scarcity of party minorities (21.25% in our final data), a sample of CHIP50's size is crucial for testing whether supporters of and defectors from the party leaders alike experience pressures to withhold their true preferences from peers.[7]

Experimental Design

Our survey asked respondents to consider their willingness to share their political beliefs across a range of scenarios with randomly-assigned attributes. The survey began with standard demographic questions, and our module asked respondents to estimate the shares of their personal networks that are co-partisans, out-partisans, and co-partisans who "are hes-

[5]We thank the Civic Health and Institutions Project, a 50 States Survey (CHIP50), NSF Grants SES-2241884, SES-2241885, and SES-2241886, led by Matthew Baum, James Druckman, David Lazer, and Katherine Ognyanova, Principal Investigators.

[6]The remaining 3,685 "pure" independents were assigned to the survey wording for Democrats if they self-identified as ideologically liberal, assigned to the survey wording for Republicans if they self-identified as conservative, or randomized to one of the two conditions otherwise. These pure independents are dropped from the main analysis per the pre-registration.

[7]All but 27 responses (20 from partisans) were recorded before Biden announced his withdrawal from the race via his personal X account at 1:46pm ET on July 21: https://x.com/JoeBiden/status/1815080881981190320.

itant" to support their party's nominee before asking their beliefs about pressures to avoid sharing their political beliefs. In order to familiarize respondents with the compensation demand outcomes later in the survey, we first presented a pre-treatment CD question based on a more standard assignment in an online survey context. Respondents were asked for their CD price to "write a few paragraphs explaining why" they would or would not vote for their party's nominee.[8]

From here, respondents were asked to estimate the percentages of Americans who (i) "say they have been penalized at work for something they said on social media," (ii) "say they have recently had political disagreements with family or friends that hurt their relationship," and (iii) "say a person's political views tell you a lot about whether they are a good or bad person." These statements are based on real survey questions asked of representative samples of Americans in 2022, with the true percentages being respectively 3, 19, and 14 percent.[9] A clear majority of our respondents overestimated the correct percentage for each, indicating widespread misperceptions about the prevalence of social sanctions for disagreeable speech in everyday life. We refer to these beliefs as respondents' "priors" regarding the likelihood of social sanction and analogize our results to other areas where respondents hold exaggerated negative beliefs, including about the anti-democratic attitudes (Mernyk et al., 2022; Braley et al., 2023) and homogeneity (Ahler and Sood, 2018) of the opposing party.

Ask-Tell Treatment (τ_1) Our first randomized treatment corrected these misperceptions in the form of an "ask-tell" intervention (e.g., Mernyk et al., 2022), so-called because after all respondents were "asked" for their priors, respondents were randomized with 0.5 probability to be "told" the true percentages. In the tell condition, respondents were shown a table that contained their own answers, the true percentages based on real data, the average percent

[8]The aim of the baseline CD is both to reduce the likelihood that our later CD outcomes present hypothetical assignments that respondents have difficulty imagining and to mitigate concerns about respondents' unfamiliarity with the question format. To improve precision, we include this baseline CD price in our pre-registered vector of covariates. Despite being a source of bias in contingent valuation studies, anchoring is not a cause for concern here as we do not interpret the CD prices as inherently meaningful.

[9]The wording and true percentage for the first statement are adapted from the following 2022 Knight Foundation report: https://knightfoundation.org/wp-content/uploads/2022/01/KF_Free_Expression_2022.pdf. For the second and third statements, refer to the following October 2022 Times/Siena Poll: https://www.nytimes.com/interactive/2022/10/18/upshot/times-siena-poll-registered-voters-crosstabs.html.

across their three guesses, and the true average (12 percent). Finally, they read a summary statement: "Overall, Americans rarely punish others for their speech choices, and very few have ever lost a job or even a friend over their political opinions." In the control condition, respondents were shown a table only reminding them of their own estimates and average guess. This design builds on one often employed in studies of democratic meta-perceptions (e.g., Braley et al., 2023; Druckman et al., 2023), where respondents are asked priors about second-order beliefs about other Americans' views, then randomly corrected to investigate how false beliefs affect democratic attitudes. This paper uses the ask-tell to exogenously reduce perceived affiliative costs, which we verify using a series of mechanism checks.

Target Randomization (τ_2) We presented three CD outcomes following the ask-tell. The CHIP50 core survey items elicited respondents' 2024 vote intention. Accounting for this answer, we asked respondents to consider an assignment in which they would make a one-minute video explaining why they did or did not support their party's 2024 presidential nominee. They were then asked to think of three people they know (hereafter, "targets") and to write down their initials. The first target listed, which serves as a benchmark, was always "A person with whom you frequently discuss politics." The second and third were randomly selected from three possible targets: a co-partisan "opponent" who, unlike the respondent, did (did not) intend to vote for the party leader; a co-partisan "ally'' who, like the respondent, did not (did) intend to vote for the party leader; or an out-partisan. Each of these three possible targets was mentioned to two-thirds of the sample. This design allows for both between and within-subject comparisons. Two outcomes were subsequently measured:

- *Private Compensation Demands*: Respondents stated the lowest payments they would accept to send their one-minute video to a frequent discussant and two randomly selected targets. CDs were measured on a left-right integer scale from \$0 to \$300 USD.
- *Mechanism Check*: To assess if the ask-tell affects the CDs by altering beliefs about the likelihood of social sanction, we asked respondents to estimate what percentages of Americans and of people they know agree with the statement, "When people I know express political views with which I disagree, I lose some respect for them."

Our main hypotheses center around the treatments and outcomes described above. An anonymous version of the pre-analysis plan is provided in supplementary materials. We hypothesized that CDs would be higher for co-partisan opponents than for frequent discussants (H1A) or co-partisan allies (H1B) but lower for co-partisan opponents than for out-partisans (H1C). Party minorities, however, would report higher CDs for co-partisan opponents than would party majorities (H5A). We then predicted that the ask-tell correction would move our mechanism check outcomes by reducing the estimated shares of Americans and peers who would judge political opponents (H2A), diminish the perceived likelihood of social sanctions (H2B), and reduce all CDs on average (H3A). As our main test of intra-party social pressure, we hypothesized that the ask-tell correction would reduce CDs foor co-partisan opponents (H3C) and a co-partisan group discussion (H3D).

The remainder of the experiment was designed to assess the mechanisms and scope conditions governing intra-party social pressure. First, if social pressure is in fact rooted in reasoned judgments about the risk of social sanction, then the ask-tell effect should be larger for respondents with more pessimistic priors. Second, we address concerns that dissenters could show greater concern than the party faithful about social sanctions either (i) for speech posted to social media or (ii) in group conversations, even if social pressure is equally salient for these groups in a one-on-one environment. Finally, we test if our findings are limited to specific discussion topics or to a specific political party. For instance, we hypothesized that the ask-tell CATEs on prices demanded for co-partisan conversation may be larger for party minorities than for party majorities among Republicans alone (H5B), as Republicans have been the subject of most reports about intra-party censorship. For concision, our remaining hypotheses are described in the pre-registration. The experiment proceeded as follows:

Social Media Prime (τ_3) Respondents were asked to reflect on the concerns that came to mind as they contemplated sharing a one-minute video about the party nominee with peers. We randomly assigned two question wordings with equal probability. In the control, respondents considered these questions in the context of the individual CD targets previously described. Respondents assigned to the prime instead considered sharing the video to a social

media platform of their choice, "where many people you know could see it."

- *Sanction Likelihood*: On a 7-point Likert scale, respondents stated the perceived likelihood of: "suffering consequences for your career," "damaging relationships with people you care about," and "experiencing hostility from strangers or people you barely know."

Group Conversation Randomization (τ_4) The final grid in our module measured respondents' willingness to have five-minute discussions with a group of Republicans and a group of Democrats. We randomized question wording by varying whether the topic would be the 2024 presidential election (prob = 0.5), "economic issues like taxation and social security'' (prob = 0.25), or "social issues like abortion and immigration'' (prob = 0.25).

- *Group Conversation CDs*: We measured respondents' compensation demand for each of the two hypothetical discussion assignments on $0 to $300 scales.

Estimation

A full discussion of the model specifics for each outcome can be found in our pre-analysis plan. Here, we outline in broad strokes our two approaches to estimation. All models include an error term ϵ and control for a pre-registered vector $\mathbf{\Phi}_i$ containing party affiliation, the average of respondents' three "ask-tell" estimates, two measures of self-reported network diversity, baseline CD price, and the "core items" shared across CHIP50 modules. All models employ robust standard errors at the individual level, which serves to cluster standard errors for the estimates that rely on multiple observations per individual. For some iterations of these models, other prior treatment conditions are included in the set of control variables.

Model 1 applies to respondent-level outcomes. The treatment τ_1 refers alternatively to the ask-tell correction, the social media prime, or the content of a group discussion.

$$\text{Outcome}_i = \beta_0 + \beta_1 \tau_1 + \eta \mathbf{\Phi}_i + \epsilon_i \tag{1}$$

Model 2 accounts for the within-subjects nature of the private CD outcomes, measured at the respondent-target level. The co-partisan opponent, co-partisan ally, and out-partisan

are represented by dummy variables CD1, CD2, and CD3 respectively, while the frequent discussant CD serves as a baseline. Respondents are indexed by i and compensation demand observations by c.

$$\text{Outcome}_{ic} = \beta_0 + \beta_1 \text{CD1}_c + \beta_2 \text{CD2}_c + \beta_3 \text{CD3}_c + \eta \mathbf{\Phi}_i + \epsilon_i \tag{2}$$

Results

In Figure 1, we illustrate the stark differences between party majorities and party minorities in the compensations they demand for the private video-sharing assignment. For party minorities, Hypothesis 1 is partially affirmed: party minorities demanded higher prices for the co-partisan opponent they know than for any other target, including an out-partisan they know, though party majorities show a different pattern. More striking, Figure 1 confirms Hypothesis 5a. Not only did party minorities report higher demands for co-partisan opponents than did party majorities, but whereas party minorities demanded the highest CD for this target, party majorities demanded the *lowest* compensation for co-partisan opponents, even lower than people with whom they frequently discuss politics. The asymmetry between minorities and majorities in willingness to discuss the election is specific to co-partisan opponents and frequent discussants, not extending to co-partisan allies or out-partisans.

CDs for the group assignment exhibited asymmetry for co- and out-partisans alike. Figure 2 indicates that relative to majorities, party minorities demanded higher prices for the co-partisan conversation and lower prices for the out-partisan conversation. Unlike in the private CD results, minorities did not prefer speech with the opposing party to their co-partisans. A plausible explanation is that these survey items did not specify the distribution of preferences, meaning we left to respondents' imagination what proportion of the group would share their beliefs. Nonetheless, party minorities exhibited greater reluctance toward intra-party speech than party majorities in both one-on-one and group environments.

At first glance, the CD results offer strong support for the theory that self-censorship in the 2024 election cycle resulted from partisan injunctive norms favoring majority opinion.

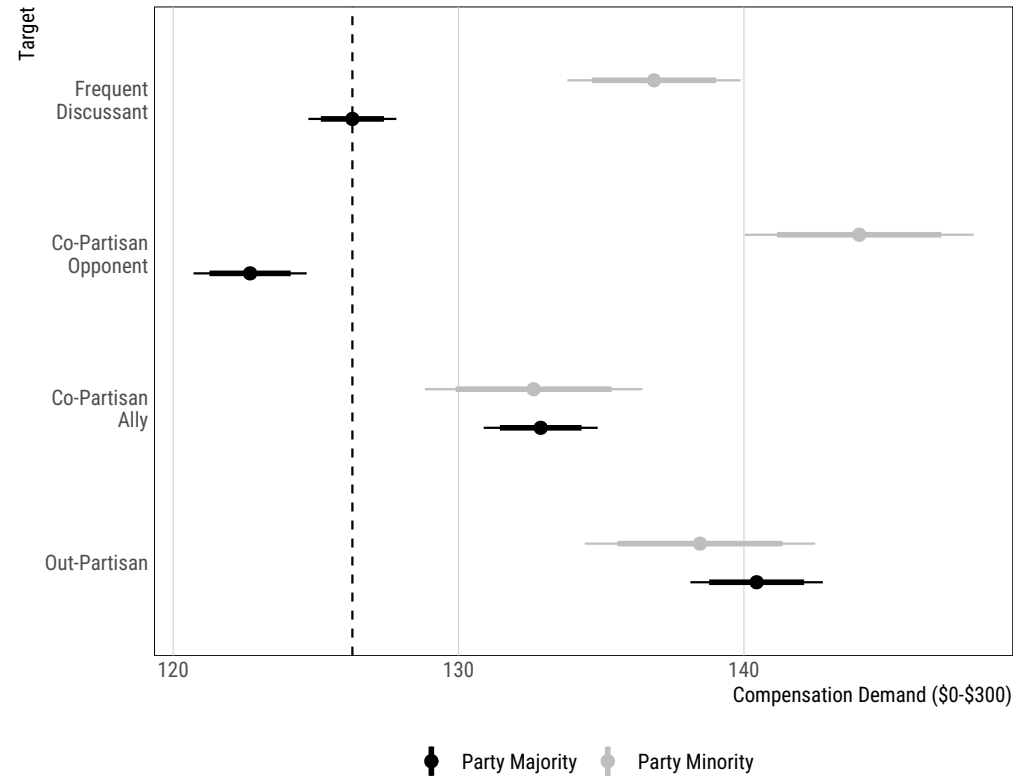

Figure 1: Private CDs by Party Majority/Minority, Full Model in A6.

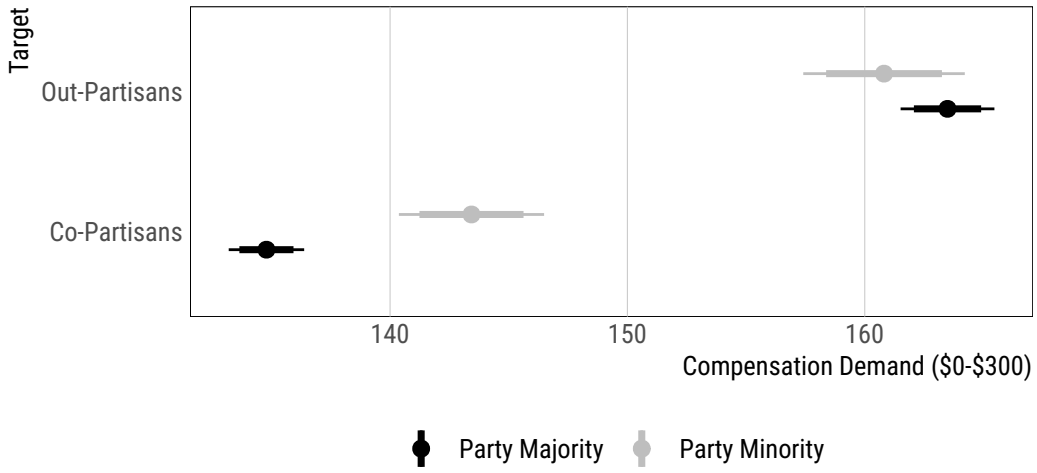

Figure 2: Group Conversation CDs by Party Majority/Minority, Full Model in Table A7.

Respondents were least willing to participate when they held reservations about their party's nominee and were asked to share these opinions with co-partisans in strong support of the nominee. However, as discussed in our theoretical section, price demands alone cannot isolate

Table 1: ATE on Mechanism Check Results with Interactions by Prior (Continuous)

	"All Americans" Manip Check		"People you know" M.C.		Sanction Mean
	(1) H2a	(2) H2c	(3) H2a	(4) H2c	(5) H2b
Ask-Tell	−11.455***	−2.368**	−7.643***	−1.490	−0.337***
	(0.383)	(0.912)	(0.399)	(0.909)	(0.021)
Prior	0.436***	0.536***	0.454***	0.522***	0.015***
	(0.011)	(0.014)	(0.011)	(0.015)	(0.001)
Ask-Tell:Prior		−0.197***		−0.134***	
		(0.019)		(0.020)	
Num.Obs.	16 650	16 650	16 477	16 477	16 901
R2	0.210	0.215	0.196	0.199	0.119

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001, Full Models in Table A1.

social pressure, instead representing a bundle of many considerations at once.

To determine whether this arises from an asymmetry in social pressure, we turn to the ask-tell correction which was designed to reduce fears of social sanction while leaving other motivations to speak unaffected. First, in Figure A9 in the Appendix, we plot the distributions of respondents' estimates for each of three survey items as well as their ask-tell prior. The dotted line in each faceted histogram corresponds to the true answer. The vast majority of our respondents overestimate the true percentage for each question, indicating widespread and consistently pessimistic misperceptions about the likelihood of social sanction for political speech in the US; the average answer was 46% compared to a true value of 12% - only 5.9% of respondents, on average, under-estimated the true level of social sanctions. Like other misperceptions that Americans hold regarding the opinions of their peers (Ahler and Sood, 2023; Braley et al., 2023), these beliefs may contribute to baleful outcomes—in our case, the suppression of political speech. Misperceptions were relatively uniform across lines of party (and minority status within a party), race, education and gender, suggesting that Americans almost ubiquitously overestimate the extent to which their fellow citizens may retaliate against political speech.

Do these misperceptions help explain why Americans widely avoid political discussion? Table 1 (column 1) and Figure 3 (top row) confirm that the ask-tell treatment substantially reduced respondents' estimates of the percentage of "All Americans" who would say they lose

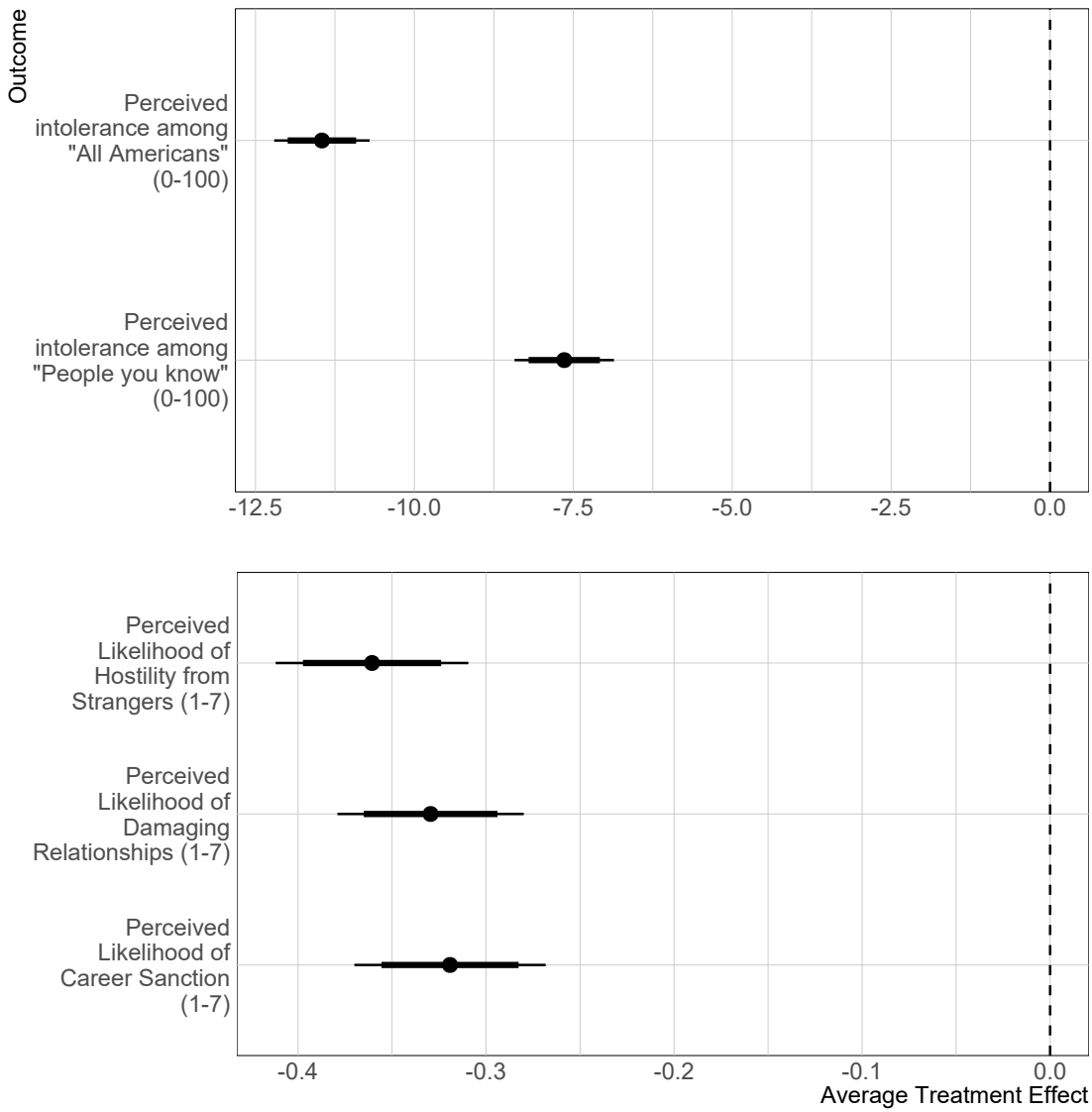

Figure 3: Ask-Tell ATEs on Mechanism Checks (Separate Regressions), Full Model in Table A8.

respect for people who express political views with which they disagree (-11.50 percentage points, SE = 0.38)—full models are presented in Appendix Table A1. Consistent with our intuition that people hold less diffuse but nonetheless malleable priors about familiars, exposure to the ask-tell also substantially reduced the estimated percentage for "People you know" (-7.71 pp, SE = 0.40), though the magnitude of this effect is smaller, as respondents likely have stronger priors about their social circles than "All Americans" in aggregate. With regard to the risks of specific sanctions for the private CD task, ask-tell exposure significantly reduced the perceived likelihood of all three outcomes as well as their mean (-0.34, SE =

0.02).

Though social pressure is well-understood as not merely a gut response but the product of a concrete if subconscious cost-benefit assessment (Cialdini and Goldstein, 2004), this intuition has yet to be empirically demonstrated to our knowledge. We offer compelling evidence that people indeed reason about the risks of social sanction in a concrete and considered manner. In columns 2 and 4 in Table 1, the negatively signed interaction effect between ask-tell assignment and ask-tell prior indicates that the ask-tell ATE substantially increased in magnitude for larger priors—that is, for respondents who more drastically overestimated the likelihood of social sanctions for political speech.

Turning to the main effects of the ask-tell, Figure 4 shows that ask-tell exposure reduced

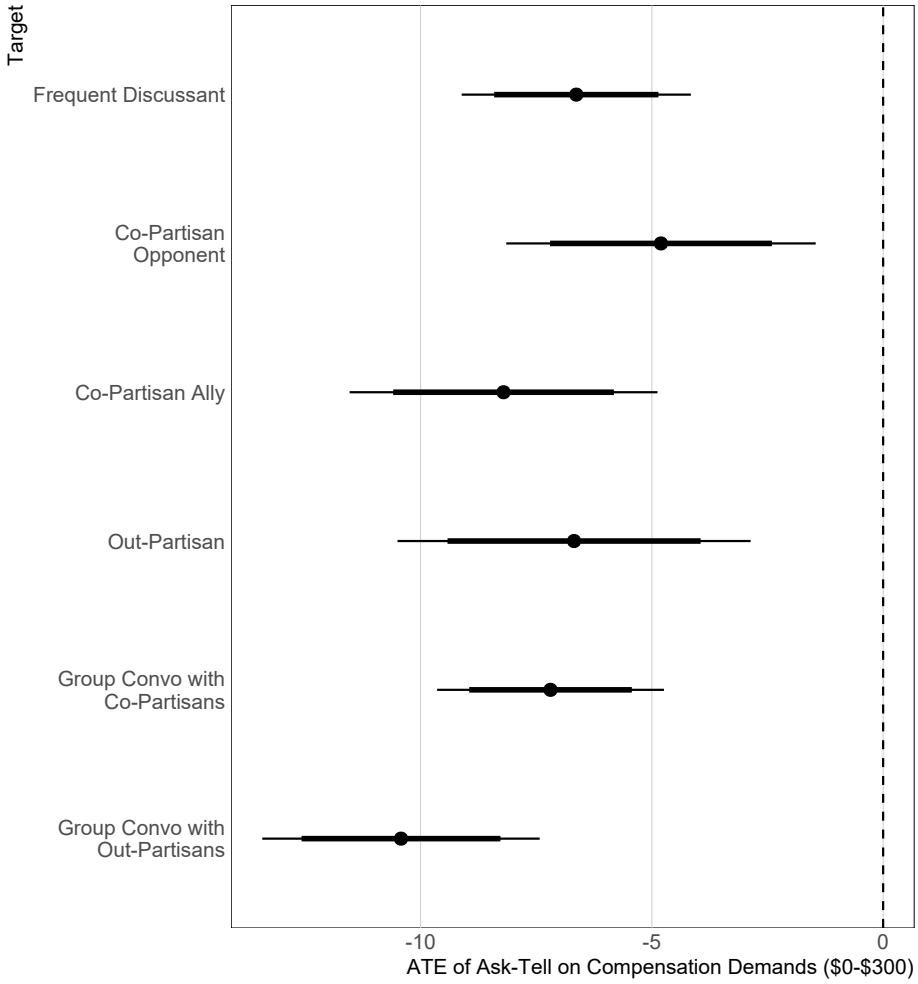

Figure 4: Ask-Tell ATE on Private and Group CDs, Full Models in Table A9.

compensation demands for discussing the election with co-partisan opponents, evincing the prevalence of social pressure in intra-party networks (full models in A9). The lower the perceived risk of social sanction, the more people were willing to speak about their party's nominee with co-partisan peers. Importantly, the ask-tell correction substantially reduced reluctance toward both one-on-one and group conversations (confirming H3a and H3b).

It must be noted, however, that ask-tell exposure reduced compensation demands not just for co-partisan CDs but across the board. Especially striking is that the ask-tell reduced CD prices for the frequent discussant and co-partisan ally, among whom we might expect ideological or partisan differences to be smaller or at least less salient. These results accord with an understanding of social pressure as less about the specific substance of one's beliefs than about a broader aversion to political discussion irrespective of one's own minority or majority status. Intra-party social pressure is real, but may not be rooted in a unique set of norms specific to one's party network.

Consistent with this interpretation, we find that the reduction in social pressure from the ask-tell is not driven solely by members of the party minority. Interacting the ask-tell treatment indicator with another for party minority status, the main effects listed in Table 2 indicate that for party majorities alone, our correction reduced compensation demands for discussing the election with co-partisan opponents privately (-\$3.79, SE = 1.89) and with a group of co-partisans (-\$6.43, SE = 2.39). Though the average effect sizes for party minorities more than double those of party majorities in magnitude, suggesting minorities may face increased social pressure, we cannot reject the null of no difference between the sub-group effects. Concerning the hypothesis that asymmetry in social pressure may be more pertinent for Republicans than Democrats, the effect is in the right direction: Table 3 shows that, for both outcomes, the difference in CATEs between party majorities and minorities is smaller for Democrats than for Republicans. But we again fail to reject the null of no difference across parties. While social pressure is not uniform, fear of social sanction serves to constrain intra-party speech across the board, irrespective of party, social context, or the normative status of one's beliefs.

Table 2: Do Ask-Tell Effects Vary Between Party Majority and Minority?

	Co-Partisan Opp. CD	Co-Partisan Group CD
Ask-Tell	−3.804*	−6.467**
	(1.889)	(2.384)
Party Min	21.609***	11.183**
	(3.184)	(4.161)
Ask-Tell:Party Min	−4.985	−7.348
	(4.364)	(5.646)
Num.Obs.	11 043	5490
R2	0.284	0.402

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001, Full Models in Table A2.

Table 3: Does Majority/Minority Asymmetry in Ask-Tell Effects Vary by Party?

	Co-Partisan Opp. CD	Co-Partisan Group CD
Ask-Tell Treatment	−4.549	−4.861*
	(2.786)	(1.946)
Party Minority	28.131***	14.830***
	(5.021)	(3.542)
Democrat	−9.135**	6.097*
	(3.197)	(2.374)
Ask-Tell:Party Min	−7.159	−6.260
	(6.999)	(5.044)
Ask-Tell:Dem	1.459	−4.158
	(3.787)	(2.773)
Party Min:Dem	−11.354+	−10.160*
	(6.453)	(4.640)
Num.Obs.	11 043	16 661
R2	0.285	0.402

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001, Full Models in Table A3.

Alternative Routes to Majority Influence

A limitation of our analyses is that they may may fail to uncover starker asymmetries between majorities and minorities because minority status is defined only with respect to a global distribution of opinion within the Democratic and Republican parties. Majority influence is said to occur through varied channels according to the literature on social pressure. Asch's (1956) line-judgment experiments, for instance, leveraged majority influence within an immediate group context, as do recent laboratory experiments (Ho and Huang, 2024).

The hypothetical assignment described in the group conversation CD was intended to bring such a context to mind, implying to party minorities that they would likely be in the minority if forced into conversation with a group of strangers. Majority influence may also operate through network composition. Perhaps perceived stigmatization is salient primarily for people who find themselves in the minority within their personal networks, not just the party writ large. Mutz (2002) famously argued that people who encounter interpersonal political disagreements the most often tend to subsequently withdraw from political activity as a result of cross-pressures; meanwhile Gerber et al. (2013) find that citizens less confident in their political judgments are less willing to discuss their vote choice. Or perhaps asymmetry is conditional upon the type of speech, such that party defectors disproportionately self-censor in online speech acts that would broadcast their opinions publicly to their social media network (Schulz, 2024), even if they act similarly to the party faithful in private exchanges or group conversations with strangers.

We leverage two components of our design to examine whether considerations about position within one's personal network may be a source of meaningful asymmetry in social pressure. First, we explore the effects of our social media treatment. As Table 4 illustrates, the social media prime heightened the perceived risk for all three sanctions, consistent with the intuition that public speech acts bring to mind a wide range of one's existing affiliations and the associated costs and benefits for honest speech. However, upon interacting exposure to the social media prime with party minority status, we find no evidence that the CATE was stronger for party minorities than majorities (Figure 5).

Second, we leverage pre-treatment questions which elicited self-reported network compo-

Table 4: Social Media Prime Main Effects

	(1) Sanction Mean (H6a)	(2) Career	(3) Affiliative	(4) Strangers
Social Media Prime	0.178***	0.131***	0.122***	0.283***
	(0.021)	(0.026)	(0.025)	(0.026)
Num.Obs.	16 901	16 925	16 931	16 930
R2	0.107	0.110	0.086	0.066

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, Full Models in Table A4.

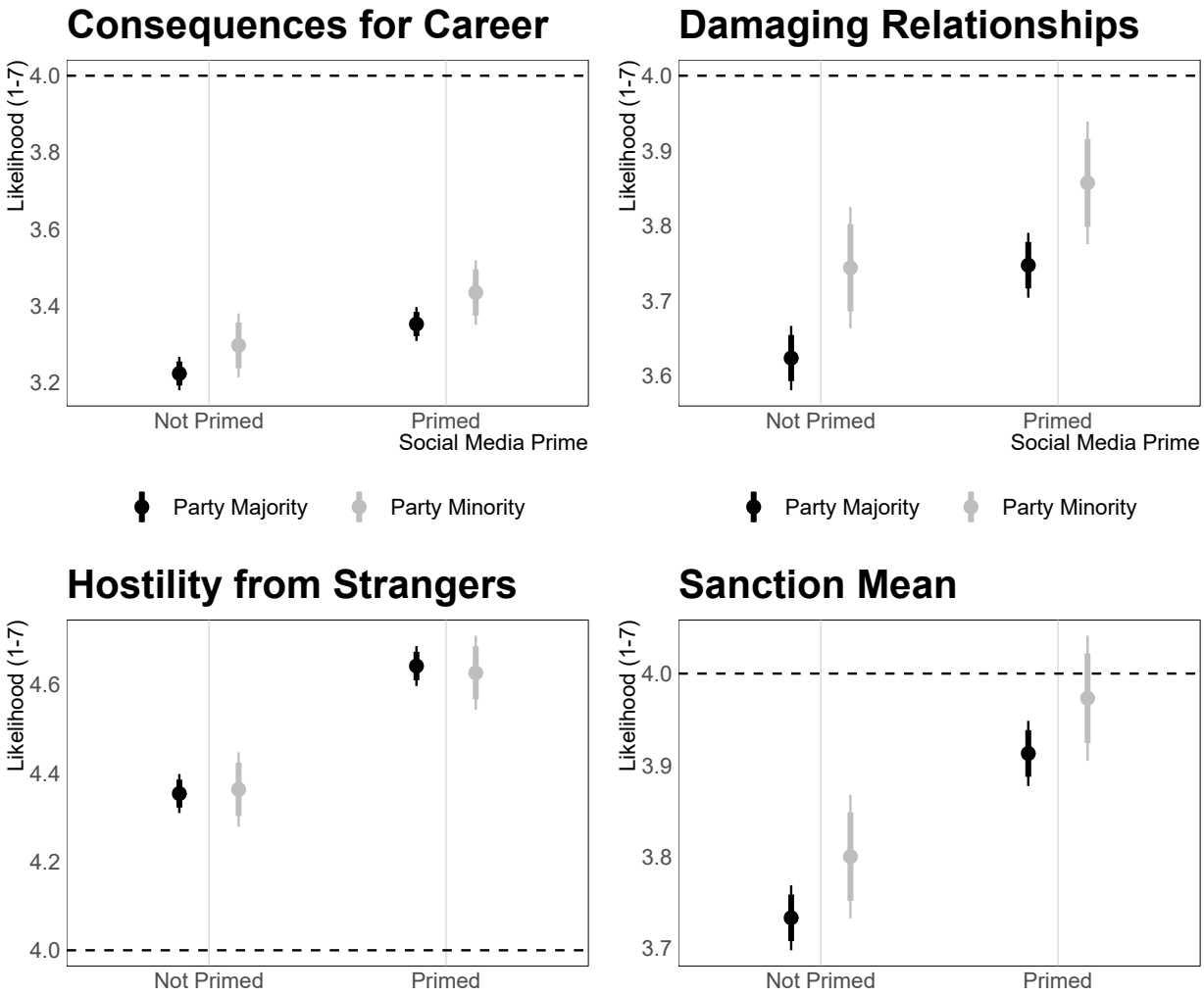

Figure 5: Estimated Marginal Means by Social Media Prime and Party Minority Status, Full Models in Table A10.

sition. Respondents provided estimates for the shares of their network who are co-partisans (N1), out-partisans (N2), and co-partisans hesitant to support the party nominee (N3). Responses were recorded on a 7-point scale (options: None, Almost none, A few, About half, A lot, Nearly all, All) then rescaled to be 0-1. Rescaled responses to N2 are employed as a rough proxy for the proportion of the respondent's network who are out-partisans. We construct a proxy for co-partisan opponent network share by calculating $\text{N1} * \text{N3}$ for party majorities and $\text{N1} * (1 - \text{N3})$ for party minorities.

Table 5 shows that both proxies for self-reported network share strongly predict the perceived risk of sanctions for the video-sharing assignment. That is, the perception of

risk increases with the share of out-partisans and share of co-partisan opponents in the respondent's network. Again, however, we find no evidence of interaction effects between the social media prime and either proxy of network share. If anything, the signs on the coefficients suggest respondents with more opponents in their network respond somewhat less strongly to the social media prime. Publicly sharing views appears no more costly for those with more adverse networks. These results run contrary to our expectation that individuals with more hostile networks would confront more aggressive concerns about publicly sharing a video, since public as opposed to private conduct implicates an individual's entire network.

Table 5: Interaction Effects for Social Media Prime by Network Proxies

	Sanction Mean
Social Media Prime	0.246***
	(0.052)
Party Minority	0.057
	(0.038)
Out-Partisan Network Share	0.975***
	(0.080)
Co-Opp Network Share	0.410***
	(0.087)
SM Prime X Party Minority	0.007
	(0.053)
SM Prime X Opponent Network Share	−0.095
	(0.112)
SM Prime X Co-Opp Network Share	−0.163
	(0.123)
Num.Obs.	16 901
R2	0.107

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$,
Full Model in Table A5.

Why do Party Minorities Speak Less if Social Pressure Acts on Majority and Minority Partisans?

Given these findings, we conducted a follow-up survey on a different sample to investigate alternate explanations to the gap in willingness to speak to co-partisans between partisan minorities and majorities. In light of our observed treatment effects, we hypothesized that differential social pressure cannot explain the full scope of party minorities' disproportionate

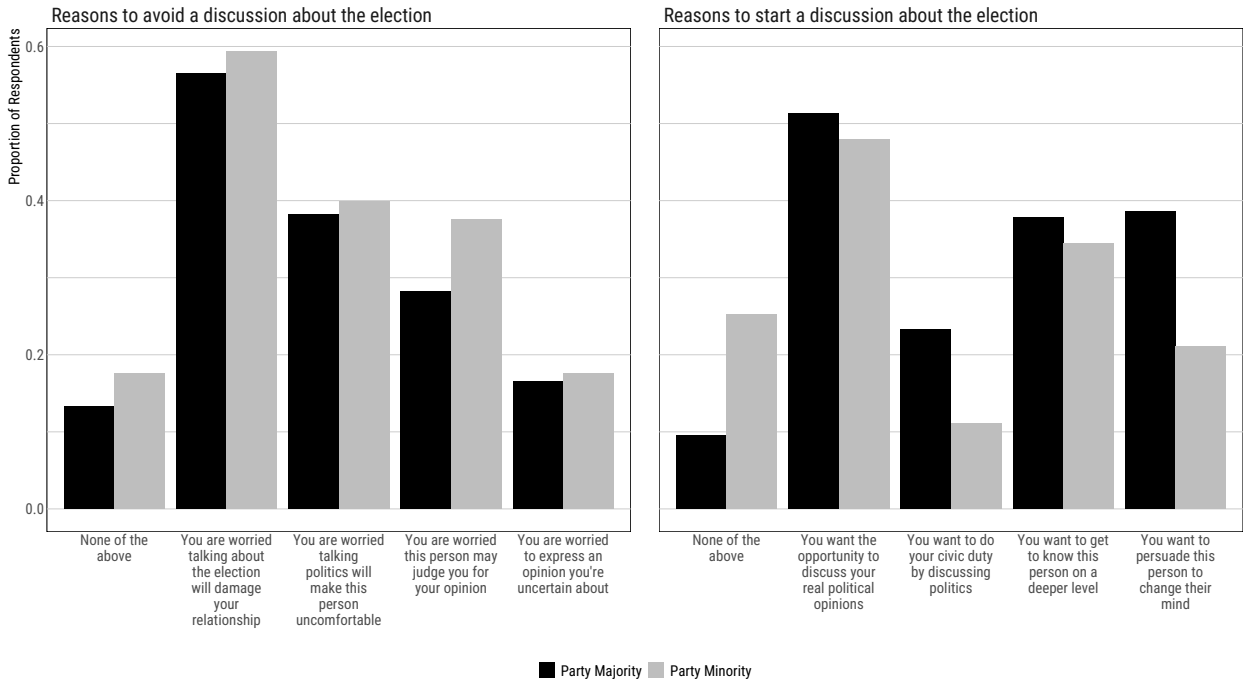

Figure 6: Considerations for Election Discussions with Co-Partisan Opponents (Know a Co-Opp), Full Models in Figures A11 and A12.

self-censorship. We fielded a short module in a survey with $N = 3,400$ partisans or partisan-leaners. Respondents were recruited through CloudResearch Connect between October 31 and November 4, just before Election Day on November 5. Respondents were asked whether they personally knew a co-partisan who disagreed with them about whom to vote for in the 2024 presidential election, to which 1,001 responded affirmatively. Respondents were then shown a list of four reasons why they might start a discussion about the election with this person (or a co-partisan opponent they envision) and asked to select all that apply or "None of the above." A similar question was asked containing four reasons why they might *avoid* a discussion about the election. Figure 6 visualizes the proportion of respondents who selected each reason, disaggregated for party majorities and minorities. This analysis includes only those respondents who said they know a co-partisan opponent, though the substantive conclusions of the analysis remain when all partisans are included, as shown in the Appendix.

The most salient difference between party majorities and minorities in terms of the considerations bearing on co-partisan speech concerned the opportunity to persuade the

discussant to change their mind. Regressing whether each reason is cited only on an indicator for party minority status, we find that party majorities were about 17.6 percentage points more likely to cite persuasion as a perceived benefit to starting a conversation on the election (SE = 0.03). Party majorities were also 12.1 percentage points more likely to cite a civic duty of discussing politics (SE = 0.02), potentially illuminating dispositional differences in engagement between the party faithful and party defectors (e.g., Krupnikov and Ryan, 2022). Party minorities were 15.7 percentage points more likely to say they see none of the listed considerations as potential benefits for speaking with co-partisan opponents (SE = 0.03).

By contrast, we find no statistically significant difference in the frequency with which the two groups cite concerns about damaging relationships or making the other discussant comfortable as a reason to avoid discussing the election. Among majorities and minorities alike, nearly 60 percent affiliative costs as a reason to avoid intra-party speech—by far the most salient consideration. Party minorities were more likely to mention only one concern: fearing judgment for their opinion (9.3 pp, SE = 0.03).

Differing strategic motivations, not a significant gap in social pressure, may be the principal explanation for the large asymmetry between party majorities and minorities in the compensations they demand for co-partisan opponents. This interpretation coheres with the striking finding that party majorities demanded lower prices for co-partisan opponents than even their frequent discussants.

Discussion

In this paper, we have presented evidence that minorities within their own party are profoundly unwilling to speak to co-partisans who toe the party line and that this disparity in self-censorship extends across parties and topics. Strikingly, individuals who dissent from their party's majority view on the 2024 presidential election in many cases prefer sharing their views with opposing partisans even over members of their party's majority. This result offers support for the majority influence view, which posits that minority opinion-holders within social groups as especially unwilling to share their counter-stereotypical beliefs.

In the ask-tell portion of our experiment, we documented how Americans across party lines overestimate the share of their fellow citizens who have either experienced or sought to inflict social sanctions in response to political speech. This misperception is substantially large, and future studies should examine how these misperceptions find their origin—for instance, in mass media or elite rhetoric. In addition, our design advances the study of social influence by demonstrating how scholars can elucidate the particular role played by social pressure in political behavior, distinguishing its causal effect from that of other social considerations. To date, studies of social pressure have almost ubiquitously employed group-level interventions (whether in the form of descriptive norm change or randomizing social observation itself), leading scholars to equate social pressure with majority influence. By manipulating the salience of social pressure directly, our intervention is able to demonstrate concretely that social pressure exists in people's one-on-one interactions, in line with the intuition that social pressure represents "a basic human drive to win praise and avoid chastisement" (Green and Gerber, 2010). This affiliative goal is present in all interpersonal interactions, not just those in group environments or among disagreeable conversants. While we cannot claim that our treatment eliminates social pressure, we show that it created substantially large shifts in perceptions of others' beliefs, in fears of social sanction and in respondent's willingness to share their beliefs. In contrast to similar research designs, this treatment served to vary social pressure concerns alone.

Using this empirical strategy, we find—contrary to the majority influence view—that fears of social sanction for political speech are not confined to any one group. Social pressure is prevalent across factional lines, with members of the party majority and minority alike exhibiting significant concern about affiliative costs to honest speech. Both groups were substantially more willing to speak when we reduced those concerns with an informational correction, even as effects differed modestly between sub-groups. This effect persisted over all possible conversation pairs, contexts, and topics, indicating that social pressure is ubiquitous in American political life, although there are some contexts in which it may be more severe. Even presumably congenial discussions between agreeable conversation partners are not free of social pressure concerns.

While there is partial evidence that minorities feel more social pressure (including in descriptive analyses of pre-treatment questions), this gap does not come close to explaining different groups' willingness to speak. Instead—as we demonstrated in our descriptive follow-up survey—partisan majorities have greater positive motivations to speak to co-partisan dissenters than vice-versa. That party majorities' willingness to discuss the election responds to ask-tell exposure and the CATEs for majorities and minorities differ minimally suggest intra-party social pressure is borne more from generalized aversion to a highly contentious political climate than from the perceived stigmatization of one's personal preferences.

While prior work finds that party identification can survive electoral defection in a general election (Green, Palmquist and Schickler, 2002), our findings suggest those who do not support their party's nominee find interactions with co-partisans costly, which may affect their long-term partisan identities through their selection into more congenial social ties and more congenial discussions within existing networks. This mechanism can transform electoral defection in a single election into longer-term shifts in partisan social identity. Our treatment effects serve only to measure a snapshot, holding fixed individuals' networks at a given moment in time, but future studies should examine how social isolation may produce cascading effects on partisan commitments over successive periods.

Our findings call attention to an important distinction in research on self-censorship and free speech. Both public media coverage, especially in partisan outlets, and some scholarship (e.g., Noelle-Neumann, 1974; Ho and Huang, 2024) have drawn attention to a form of social sanctioning that is carried out by one political faction to suppress the beliefs espoused by another, often making analogies to authoritarian repression more explicit. Through a large, nationwide sample, we show that this is not how most Americans experience pressure to self-censor. Not only are social fears not concentrated in any one faction, but they are also relevant to diverse inter- and intra-party contexts and extend to hypothetical discussions likely to be avoid or minimize substantive disagreement. In today's political climate, Americans across all party and factional lines fear social repercussions from political speech, even as party minorities may feel more pressure to avoid sharing beliefs that deviate from their group norms.

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Appendix

1.1 Full Models

We begin by showing full models, with all covariates, for all models used in the main analysis.

Table A1: Full Models, ATE on Mechanism Check Results with Interactions by Prior (Continuous) (Table 1)

	"All Americans"		"People you know"		Sanction Mean
	(1) H2a	(2) H2c	(3) H2a	(4) H2c	(5) H2b
Intercept	21.205***	16.625***	13.747***	10.622***	2.879***
	(1.323)	(1.380)	(1.359)	(1.417)	(0.072)
Ask-Tell	−11.455***	−2.368**	−7.643***	−1.490	−0.337***
	(0.383)	(0.912)	(0.399)	(0.909)	(0.021)
Dem	2.603***	2.640***	3.275***	3.305***	−0.033
	(0.522)	(0.521)	(0.533)	(0.532)	(0.029)
Demand Base	0.021***	0.021***	0.024***	0.024***	0.001***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.000)
Party Minority	−3.140***	−3.108***	−3.632***	−3.609***	0.060*
	(0.486)	(0.484)	(0.506)	(0.505)	(0.027)
Ideology	−0.532*	−0.529*	−1.002***	−0.996***	0.047***
	(0.251)	(0.250)	(0.261)	(0.260)	(0.014)
Pol. Int. (5-point)	1.355***	1.370***	1.466***	1.480***	0.009
	(0.204)	(0.203)	(0.210)	(0.210)	(0.011)
Education	−0.106	−0.107	−0.249	−0.252	0.066***
	(0.216)	(0.215)	(0.223)	(0.223)	(0.012)
Prior	0.436***	0.536***	0.454***	0.522***	0.015***
	(0.011)	(0.014)	(0.011)	(0.015)	(0.001)
Network Co-Opp	8.172***	8.148***	9.046***	9.024***	0.326***
	(1.132)	(1.134)	(1.188)	(1.188)	(0.062)
Network Opposing	1.370	1.398	1.730	1.736	0.927***
	(1.045)	(1.042)	(1.098)	(1.097)	(0.056)
Female	−2.791***	−2.830***	−2.307***	−2.334***	0.099***
	(0.397)	(0.396)	(0.415)	(0.415)	(0.022)
White	0.419	0.418	−0.346	−0.340	0.052*
	(0.475)	(0.473)	(0.496)	(0.495)	(0.025)
Married	0.568	0.574	0.084	0.092	0.054*
	(0.425)	(0.424)	(0.440)	(0.439)	(0.023)
Fully Employed	−0.162	−0.198	0.575	0.555	0.059*
	(0.457)	(0.455)	(0.479)	(0.479)	(0.025)
SM Prime					0.175***
					(0.021)
Ask-Tell:Prior		−0.197***		−0.134***	
		(0.019)		(0.020)	
Num.Obs.	16 650	16 650	16 477	16 477	16 901
R2	0.210	0.215	0.196	0.199	0.119

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A2: Full Models, Ask-Tell Effects by Minority Status (Table 2)

	Co-Partisan Opponent CD	Co-Partisan Group CD
Intercept	38.581***	47.233***
	(5.691)	(7.346)
Ask-Tell	−3.706*	−6.418**
	(1.888)	(2.380)
Dem	−11.059***	3.618
	(2.316)	(3.072)
Demand Base	0.510***	0.612***
	(0.010)	(0.012)
Party Minority	21.652***	10.938**
	(3.181)	(4.168)
Ideology	−3.044**	0.689
	(1.080)	(1.448)
Pol. Int. (5-point)	−2.812**	−4.448***
	(0.879)	(1.134)
Education	5.276***	−1.935
	(0.940)	(1.229)
Prior	0.329***	0.309***
	(0.046)	(0.056)
Network Co-Opp	31.755***	17.908**
	(4.845)	(6.115)
Network Opposing	19.974***	10.654+
	(4.439)	(5.642)
Female	4.447*	8.159***
	(1.764)	(2.246)
White	5.248**	−2.416
	(2.004)	(2.612)
Married	1.347	2.926
	(1.909)	(2.441)
Fully Employed	4.268*	7.636**
	(2.008)	(2.595)
Ask-Tell:Party Min	−5.203	−7.213
	(4.363)	(5.662)
Num.Obs.	11 043	5490
R2	0.283	0.401

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Full Models, Does Majority/Minority Asymmetry in Ask-Tell Effects Vary by Party? (Table 3)

	Co-Partisan Opp. CD	Co-Partisan Group CD
Intercept	38.581***	58.807***
	(5.771)	(4.361)
Ask-Tell	−4.464	−4.896*
	(2.786)	(1.945)
Dem	−9.234**	6.001*
	(3.193)	(2.371)
Demand Base	0.509***	0.619***
	(0.010)	(0.007)
Party Minority	27.884***	14.800***
	(5.032)	(3.542)
Ideology	−2.565*	0.539
	(1.100)	(0.833)
Pol. Int. (5-point)	−2.820**	−4.930***
	(0.878)	(0.649)
Education	5.159***	−0.391
	(0.942)	(0.702)
Prior	0.332***	0.292***
	(0.046)	(0.033)
Network Co-Opp	30.623***	14.458***
	(4.875)	(3.533)
Network Opposing	19.793***	6.014+
	(4.439)	(3.301)
Female	4.484*	2.926*
	(1.763)	(1.291)
White	5.332**	−2.406
	(2.005)	(1.506)
Married	1.278	3.544*
	(1.909)	(1.400)
Fully Employed	4.286*	3.294*
	(2.007)	(1.483)
Ask-Tell:Party Min	−7.197	−6.236
	(7.004)	(5.042)
Ask-Tell:Dem	1.486	−4.065
	(3.787)	(2.772)
Party Min:Dem	−10.835+	−10.250*
	(6.457)	(4.637)
Ask-Tell:Party Min:Dem	3.197	9.527
	(8.946)	(6.524)
Num.Obs.	11 043	16 661
R2	0.283	0.401

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Full Models, Social Media Prime Main Effects (Table 4)

	(1) Sanction Mean (H6a)	(3) Career	(4) Affiliative	(5) Strangers
Intercept	2.696***	2.857***	2.213***	3.021***
	(0.072)	(0.089)	(0.087)	(0.089)
Dem	−0.035	−0.084*	0.008	−0.029
	(0.029)	(0.035)	(0.035)	(0.035)
Demand Base	0.001***	0.001***	0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Party Minority	0.064*	0.078*	0.115***	−0.003
	(0.028)	(0.034)	(0.033)	(0.034)
Ideology	0.045**	0.114***	0.048**	−0.028+
	(0.014)	(0.017)	(0.017)	(0.017)
Pol. Int. (5-point)	0.011	−0.032*	−0.016	0.082***
	(0.011)	(0.014)	(0.013)	(0.014)
Education	0.065***	0.035*	0.056***	0.102***
	(0.012)	(0.015)	(0.014)	(0.015)
Prior	0.015***	0.015***	0.016***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)
Network Co-Opp	0.329***	0.364***	0.448***	0.176*
	(0.062)	(0.077)	(0.074)	(0.076)
Network Opposing	0.924***	0.668***	1.301***	0.792***
	(0.057)	(0.070)	(0.070)	(0.070)
Female	0.099***	0.005	0.107***	0.184***
	(0.022)	(0.027)	(0.026)	(0.027)
White	0.054*	−0.199***	0.145***	0.215***
	(0.026)	(0.032)	(0.031)	(0.032)
Married	0.049*	0.013	0.094***	0.041
	(0.023)	(0.029)	(0.028)	(0.029)
Fully Employed	0.060*	0.271***	0.000	−0.096**
	(0.025)	(0.031)	(0.030)	(0.031)
SM Prime	0.178***	0.131***	0.121***	0.283***
	(0.021)	(0.026)	(0.025)	(0.026)
Num.Obs.	16 901	16 925	16 931	16 930
R2	0.105	0.106	0.085	0.065

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: Full Model, Interaction Effects for Social Media Prime by Network Proxies (Table 5)

	Sanction Mean
Intercept	2.664***
	(0.075)
Dem	−0.035
	(0.029)
Demand Base	0.001***
	(0.000)
Party Minority	0.059
	(0.038)
Ideology	0.045**
	(0.014)
Pol. Int. (5-point)	0.011
	(0.011)
Education	0.065***
	(0.012)
Prior	0.015***
	(0.001)
Network Co-Opp	0.412***
	(0.087)
Network Opposing	0.967***
	(0.080)
Female	0.098***
	(0.022)
White	0.054*
	(0.026)
Married	0.049*
	(0.023)
Fully Employed	0.060*
	(0.025)
SM Prime	0.242***
	(0.052)
SM Prime:Party Min	0.009
	(0.053)
SM Prime:Network Out	−0.085
	(0.112)
SM Prime:Network Co-Opp	−0.165
	(0.123)
Num.Obs.	16 901
R2	0.105

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A6: Full Models, Private CDs by Majority/Minority Status (Figure 1)

	CD Price ($0-$300)
Intercept	31.465***
	(3.805)
Co-partisan Opponent	−3.584***
	(0.874)
Co-partisan Ally	6.599***
	(0.876)
Opposing Partisan	14.166***
	(1.116)
Dem	−3.807*
	(1.530)
Demand Base	0.558***
	(0.006)
Party Minority	10.569***
	(1.685)
Ideology	−1.620*
	(0.708)
Pol. Int. (5-point)	−1.556**
	(0.587)
Education	4.141***
	(0.635)
Prior	0.328***
	(0.031)
Network Co-Opp	8.326**
	(3.038)
Network Opposing	9.512**
	(2.930)
Female	3.798**
	(1.183)
White	6.918***
	(1.327)
Married	3.600**
	(1.274)
Fully Employed	2.218+
	(1.341)
Target Co-Opp: Party Min	10.776***
	(2.060)
Target Co-Ally: Party Min	−10.818***
	(1.898)
Target Out: Party Min	−12.553***
	(2.192)
Num.Obs.	49 868
R2	0.299
Std.Errors	by: id

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A7: Full Models, Group Convos (Figure 2)

	Co-Partisans	Out-Partisans
Intercept	56.209***	78.297***
	(4.255)	(5.109)
Dem	2.384	4.768*
	(1.738)	(1.992)
Demand Base	0.620***	0.481***
	(0.007)	(0.008)
Party Minority	8.640***	−2.675
	(1.681)	(1.885)
Ideology	0.233	−4.593***
	(0.819)	(0.961)
Pol. Int. (5-point)	−4.878***	−2.844***
	(0.650)	(0.788)
Education	−0.382	6.662***
	(0.702)	(0.861)
Prior	0.290***	0.367***
	(0.033)	(0.041)
Network Co-Opp	15.268***	−26.868***
	(3.505)	(4.392)
Network Opposing	6.045+	3.276
	(3.305)	(4.056)
Female	2.935*	8.985***
	(1.293)	(1.585)
White	−2.383	11.643***
	(1.508)	(1.808)
Married	3.498*	3.318+
	(1.401)	(1.705)
Fully Employed	3.348*	−1.763
	(1.485)	(1.789)
Num.Obs.	16 661	16 644
R2	0.400	0.215
Std.Errors	by: id	by: id

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A8: Full Models, Ask-Tell ATEs on Mechanism Checks (Figure 3)

	Career Sanction	Relation Sanction	Stranger Sanction	All Americans	People You Know
Intercept	3.031***	2.393***	3.217***	21.205***	13.747***
	(0.090)	(0.087)	(0.090)	(1.323)	(1.359)
Ask-Tell	−0.319***	−0.329***	−0.361***	−11.455***	−7.643***
	(0.026)	(0.025)	(0.026)	(0.383)	(0.399)
Dem	−0.081*	0.011	−0.026	2.603***	3.275***
	(0.035)	(0.035)	(0.035)	(0.522)	(0.533)
Demand Base	0.001***	0.001***	0.000	0.021***	0.024***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)
Party Minority	0.075*	0.112***	−0.007	−3.140***	−3.632***
	(0.034)	(0.033)	(0.034)	(0.486)	(0.506)
Ideology	0.116***	0.050**	−0.026	−0.532*	−1.002***
	(0.017)	(0.017)	(0.017)	(0.251)	(0.261)
Pol. Int. (5-point)	−0.034*	−0.018	0.080***	1.355***	1.466***
	(0.014)	(0.013)	(0.014)	(0.204)	(0.210)
Education	0.036*	0.057***	0.104***	−0.106	−0.249
	(0.014)	(0.014)	(0.015)	(0.216)	(0.223)
Prior	0.015***	0.016***	0.015***	0.436***	0.454***
	(0.001)	(0.001)	(0.001)	(0.011)	(0.011)
Network Co-Opp	0.361***	0.444***	0.172*	8.172***	9.046***
	(0.077)	(0.074)	(0.076)	(1.132)	(1.188)
Network Opposing	0.671***	1.305***	0.796***	1.370	1.730
	(0.070)	(0.070)	(0.069)	(1.045)	(1.098)
Female	0.006	0.108***	0.185***	−2.791***	−2.307***
	(0.027)	(0.026)	(0.027)	(0.397)	(0.415)
White	−0.202***	0.142***	0.212***	0.419	−0.346
	(0.031)	(0.031)	(0.032)	(0.475)	(0.496)
Married	0.018	0.098***	0.046	0.568	0.084
	(0.029)	(0.028)	(0.029)	(0.425)	(0.440)
Fully Employed	0.270***	−0.001	−0.097**	−0.162	0.575
	(0.031)	(0.030)	(0.031)	(0.457)	(0.479)
SM Prime	0.128***	0.119***	0.280***		
	(0.026)	(0.025)	(0.026)		
Num.Obs.	16 925	16 931	16 930	16 650	16 477
R2	0.113	0.094	0.075	0.210	0.196

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Full Models, Ask-Tell Effects (Figure 4)

	Group Out	Group In	Out-Partisan	Co-Ally	Co-Opponent	Discussant
Intercept	83.959***	60.115***	47.083***	48.896***	39.031***	25.641***
	(5.181)	(4.317)	(6.534)	(5.710)	(5.676)	(4.263)
Ask-Tell	−10.423***	−7.191***	−6.682***	−8.207***	−4.803**	−6.634***
	(1.530)	(1.251)	(1.947)	(1.698)	(1.706)	(1.264)
Dem	4.824*	2.440	2.248	−2.326	−11.047***	−3.720*
	(1.989)	(1.736)	(2.549)	(2.305)	(2.317)	(1.700)
Demand Base	0.480***	0.619***	0.452***	0.575***	0.510***	0.648***
	(0.008)	(0.007)	(0.010)	(0.009)	(0.010)	(0.007)
Party Minority	−2.800	8.565***	0.287	−0.192	19.056***	10.423***
	(1.883)	(1.679)	(2.371)	(2.225)	(2.293)	(1.696)
Ideology	−4.552***	0.269	−3.580**	0.945	−3.031**	−0.794
	(0.960)	(0.818)	(1.212)	(1.093)	(1.081)	(0.779)
Pol. Int. (5-point)	−2.904***	−4.919***	−1.236	−0.784	−2.808**	−1.434*
	(0.787)	(0.649)	(0.986)	(0.880)	(0.879)	(0.650)
Education	6.716***	−0.345	8.597***	−0.434	5.259***	3.537***
	(0.860)	(0.702)	(1.100)	(0.963)	(0.940)	(0.704)
Prior	0.367***	0.290***	0.407***	0.380***	0.329***	0.235***
	(0.041)	(0.033)	(0.052)	(0.046)	(0.046)	(0.034)
Network Co-Opp	−27.026***	15.149***	−15.846**	10.109*	31.802***	8.210*
	(4.389)	(3.503)	(5.345)	(4.764)	(4.844)	(3.391)
Network Opposing	3.423	6.122+	29.797***	−16.448***	19.992***	7.640*
	(4.050)	(3.300)	(5.154)	(4.420)	(4.437)	(3.244)
Female	9.011***	2.950*	6.668***	4.165*	4.448*	1.319
	(1.583)	(1.292)	(2.021)	(1.762)	(1.764)	(1.303)
White	11.558***	−2.448	20.463***	−0.063	5.234**	3.718*
	(1.806)	(1.507)	(2.303)	(2.025)	(2.004)	(1.483)
Married	3.463*	3.590*	5.847**	3.422+	1.398	3.664**
	(1.703)	(1.400)	(2.186)	(1.893)	(1.909)	(1.402)
Fully Employed	−1.805	3.324*	−2.508	4.212*	4.304*	2.548+
	(1.786)	(1.483)	(2.282)	(1.975)	(2.007)	(1.486)
Num.Obs.	16 644	16 661	11 037	11 091	11 043	16 697
R2	0.217	0.401	0.194	0.320	0.283	0.408

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Full Models, Social Media Prime and Minority Status (Figure 5)

	Career	Damage Relationships	Hostility Strangers	Sanction Mean
Intercept	2.858***	2.212***	3.018***	2.695***
	(0.089)	(0.087)	(0.089)	(0.072)
Dem	−0.084*	0.008	−0.029	−0.035
	(0.035)	(0.035)	(0.035)	(0.029)
Demand Base	0.001***	0.001***	0.000	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Party Minority	0.073	0.120**	0.009	0.067+
	(0.047)	(0.046)	(0.047)	(0.038)
Ideology	0.114***	0.048**	−0.028+	0.045**
	(0.017)	(0.017)	(0.017)	(0.014)
Pol. Int. (5-point)	−0.032*	−0.016	0.082***	0.011
	(0.014)	(0.013)	(0.014)	(0.011)
Education	0.035*	0.056***	0.102***	0.065***
	(0.015)	(0.014)	(0.015)	(0.012)
Prior	0.015***	0.016***	0.015***	0.015***
	(0.001)	(0.001)	(0.001)	(0.001)
Network Co-Opp	0.364***	0.448***	0.176*	0.329***
	(0.077)	(0.074)	(0.076)	(0.062)
Network Opposing	0.668***	1.301***	0.792***	0.923***
	(0.070)	(0.070)	(0.070)	(0.057)
Female	0.005	0.107***	0.184***	0.098***
	(0.027)	(0.026)	(0.027)	(0.022)
White	−0.199***	0.145***	0.214***	0.054*
	(0.032)	(0.031)	(0.032)	(0.026)
Married	0.013	0.094***	0.041	0.049*
	(0.029)	(0.028)	(0.029)	(0.023)
Fully Employed	0.271***	0.000	−0.096**	0.060*
	(0.031)	(0.030)	(0.031)	(0.025)
SM Prime	0.129***	0.124***	0.288***	0.179***
	(0.029)	(0.028)	(0.029)	(0.023)
SM Prime:Party Min	0.009	−0.011	−0.025	−0.007
	(0.065)	(0.063)	(0.065)	(0.052)
Num.Obs.	16 925	16 931	16 930	16 901
R2	0.106	0.085	0.065	0.105

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A11: Full Models, Perceived Benefits to Conversations with Co-Partisan Opponents

		Discuss Real		Get to Know	
	None	Opinions	Civic Duty	Discussant	Persuade
Intercept	0.096***	0.514***	0.232***	0.378***	0.386***
	(0.011)	(0.018)	(0.016)	(0.018)	(0.018)
Party Minority	0.157***	−0.035	−0.121***	−0.034	−0.176***
	(0.029)	(0.036)	(0.025)	(0.034)	(0.031)
Num.Obs.	1001	1001	1001	1001	1001
R2	0.040	0.001	0.018	0.001	0.027

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A12: Full Models, Perceived Costs to Conversations with Co-Partisan Opponents

		Damage	Make Discussant	Be Judged	Uncertainty
	None	Relationship	Uncomfortable	for Opinion	about Opinions
Intercept	0.132***	0.565***	0.382***	0.282***	0.165***
	(0.012)	(0.018)	(0.018)	(0.017)	(0.014)
Party Minority	0.044	0.029	0.016	0.093**	0.011
	(0.027)	(0.035)	(0.035)	(0.034)	(0.027)
Num.Obs.	1001	1001	1001	1001	1001
R2	0.003	0.001	0.000	0.008	0.000

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

1.2 Additional Pre-Registered Analyses

1.2.1 Hypothesis 3

Table A13: Full Models, Hypothesis 3

	(1)	(2)	(3)	(4)
Intercept	51.888***	49.214***	39.031***	60.115***
	(3.525)	(3.708)	(5.676)	(4.317)
Ask-Tell	−7.175***	−1.899	−4.803**	−7.191***
	(1.037)	(2.624)	(1.706)	(1.251)
Dem	−0.780	−0.757	−11.047***	2.440
	(1.398)	(1.397)	(2.317)	(1.736)
Party Minority	6.049***	6.060***	19.056***	8.565***
	(1.380)	(1.379)	(2.293)	(1.679)
Pol. Int. (5-point)	−2.691***	−2.682***	−2.808**	−4.919***
	(0.531)	(0.531)	(0.879)	(0.649)
Prior	0.324***	0.382***	0.329***	0.290***
	(0.028)	(0.038)	(0.046)	(0.033)
Network Co-Opp	2.162	2.161	31.802***	15.149***
	(2.752)	(2.751)	(4.844)	(3.503)
Network Opposing	7.399**	7.420**	19.992***	6.122+
	(2.670)	(2.671)	(4.437)	(3.300)
Female	4.911***	4.897***	4.448*	2.950*
	(1.072)	(1.072)	(1.764)	(1.292)
White	5.944***	5.948***	5.234**	−2.448
	(1.214)	(1.214)	(2.004)	(1.507)
Married	3.611**	3.614**	1.398	3.590*
	(1.154)	(1.154)	(1.909)	(1.400)
Fully Employed	1.893	1.876	4.304*	3.324*
	(1.222)	(1.222)	(2.007)	(1.483)
Num.Obs.	16 115	16 115	11 043	16 661
R2	0.444	0.445	0.283	0.401

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Models include covariate vector

1.2.2 Hypothesis 4

Table A14: Full Models, Hypotheses 4

	Group Demands	Private Demands
Intercept	56.184	35.621
	(4.055)	(3.866)
Co-partisan Ally		4.868
		(1.108)
Opposing Partisan		11.274
		(1.386)
Ask-Tell	−6.920	−6.800
	(1.548)	(1.272)
Dem	3.573	−3.783
	(1.572)	(1.528)
Party Minority	2.846	7.675
	(1.557)	(1.528)
Pol. Int. (5-point)	−3.884	−1.581
	(0.591)	(0.587)
Prior	0.329	0.328
	(0.031)	(0.031)
Network Co-Opp	−5.812	8.194
	(3.101)	(3.036)
Network Opposing	4.677	9.714
	(2.986)	(2.928)
Female	6.017	3.848
	(1.195)	(1.182)
White	4.561	6.861
	(1.371)	(1.325)
Married	3.538	3.709
	(1.288)	(1.273)
Fully Employed	0.752	2.228
	(1.362)	(1.340)
Ask-Tell:Topic Election	−1.084	
	(2.683)	
Ask-Tell:Target Out Partisans	−3.412	
	(1.938)	
Topic Election:Target Out Partisans	6.071	
	(2.492)	
Ask-Tell:Topic Election: Target Out Partisans	1.125	
	(3.450)	
Num.Obs.	33 305	49 868
R2	0.305	0.299
Std.Errors	by: id	by: id

1.2.3 Hypothesis 5

Table A15: Full Models, Hypothesis 5

	Group Demands	Private Demands	Group Demands
Intercept	38.581	31.465	56.457
	(5.771)	(3.805)	(4.290)
Co-partisan Ally		6.599	
		(0.876)	
Opposing Partisan		14.166	
		(1.116)	
Ask-Tell	−4.464		
	(2.786)		
Dem	−9.234	−3.807	2.388
	(3.193)	(1.530)	(1.739)
Party Minority	27.884	10.569	9.067
	(5.032)	(1.685)	(2.000)
Pol. Int. (5-point)	−2.820	−1.556	−4.886
	(0.878)	(0.587)	(0.650)
Prior	0.332	0.328	0.290
	(0.046)	(0.031)	(0.033)
Network Co-Opp	30.623	8.326	15.243
	(4.875)	(3.038)	(3.506)
Network Opposing	19.793	9.512	6.058
	(4.439)	(2.930)	(3.305)
Female	4.484	3.798	2.934
	(1.763)	(1.183)	(1.293)
White	5.332	6.918	−2.384
	(2.005)	(1.327)	(1.508)
Married	1.278	3.600	3.491
	(1.909)	(1.274)	(1.401)
Fully Employed	4.286	2.218	3.352
	(2.007)	(1.341)	(1.485)
Ask-Tell:Party Min	−7.197		
	(7.004)		
Ask-Tell:Dem	1.486		
	(3.787)		
Party Min:Dem	−10.835		
	(6.457)		
Ask-Tell:Party Min:Dem	3.197		
	(8.946)		
Target Co-Opp: Party Min		10.776	
		(2.060)	
Target Co-Ally: Party Min		−10.818	
		(1.898)	
Target Out: Party Min		−12.553	
		(2.192)	
Num.Obs.	11 043	49 868	16 661
R2	0.283	0.299	0.400
Std.Errors		by: id	

1.3 Additional Exploratory Analyses

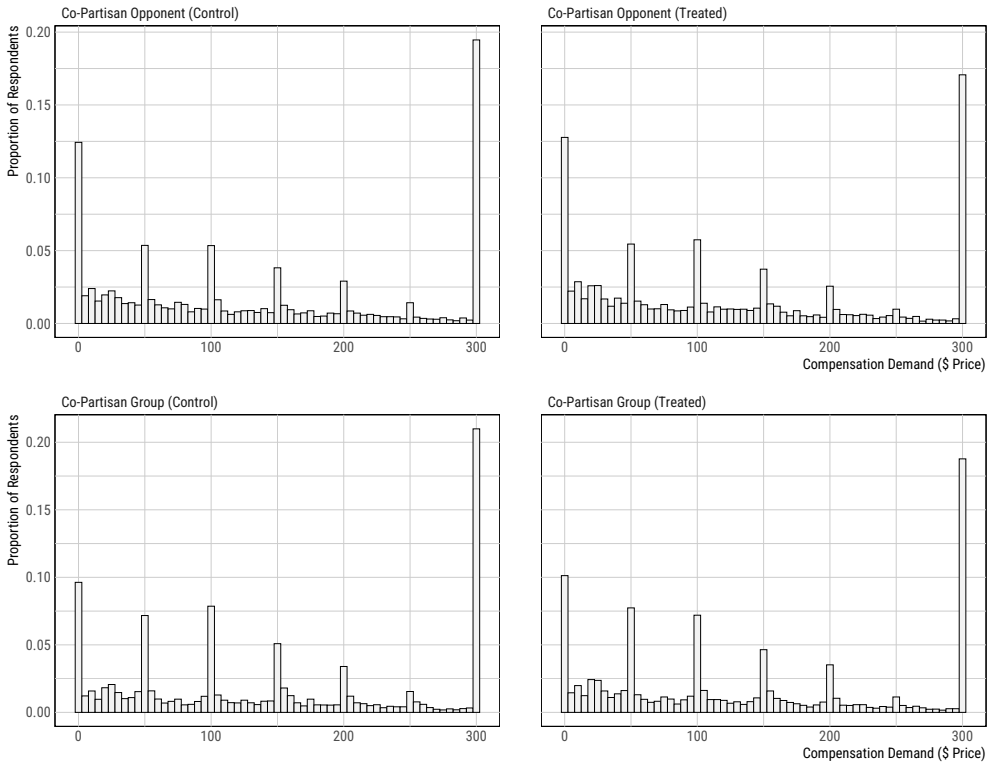

Figure A1: Distribution of Co-Partisan Compensation Demands

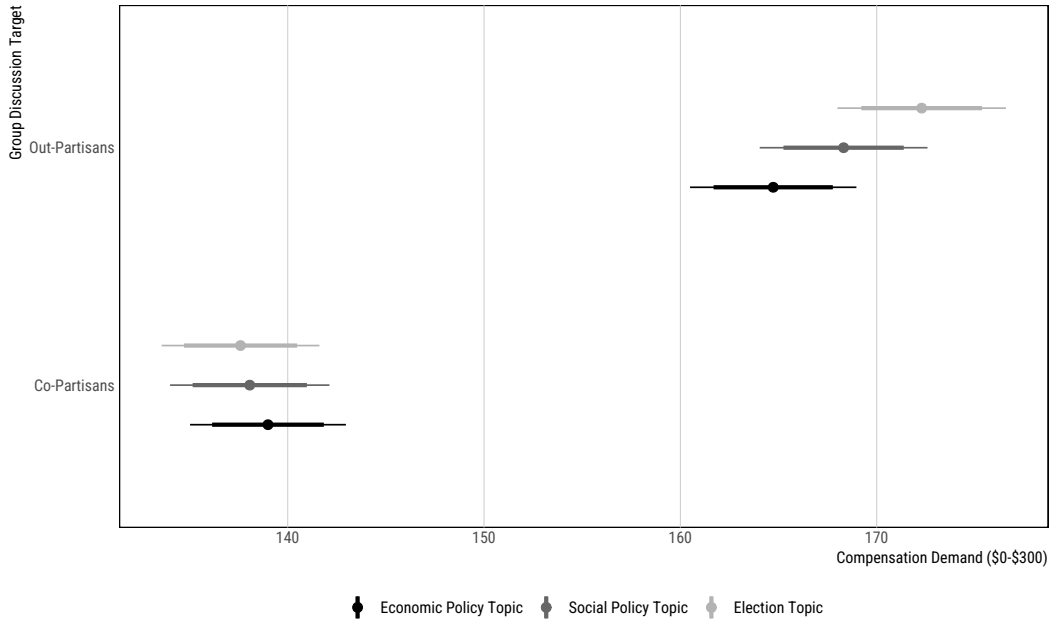

Figure A2: Group Conversation CDs by Topic

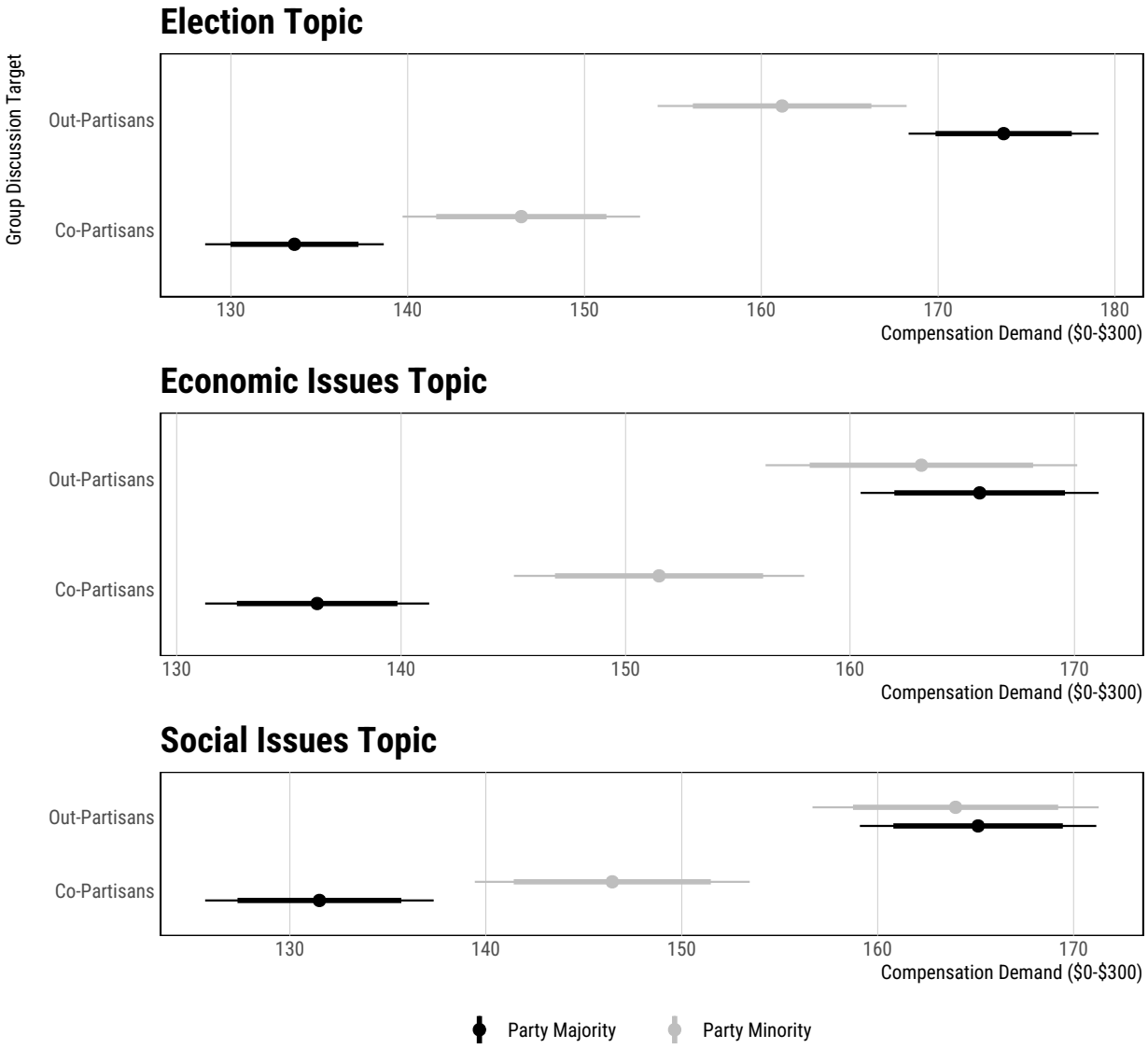

Figure A3: Group Conversation CDs by Topic and Party Majority/Minority

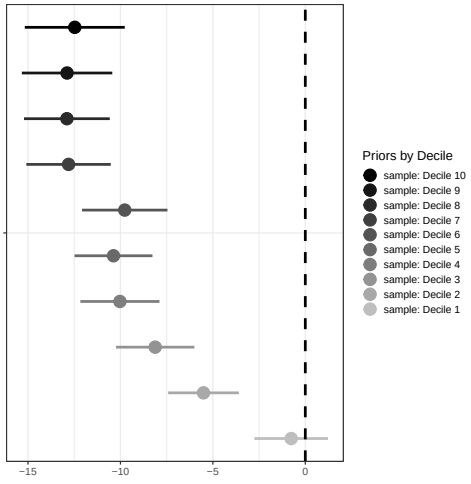

Figure A4: CATEs by Priors: Manipulation Index

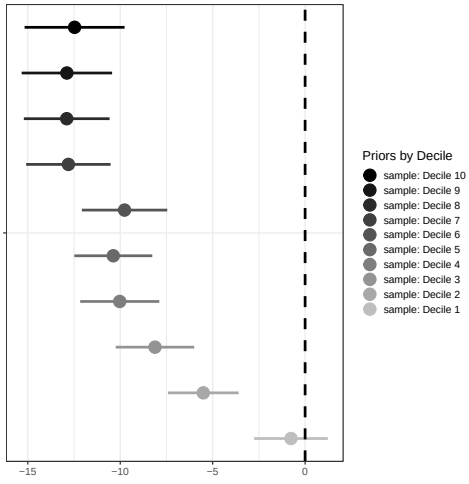

Figure A5: CATEs by Priors: Main Outcome Measure

1.4 Sample Composition and Balance Tables

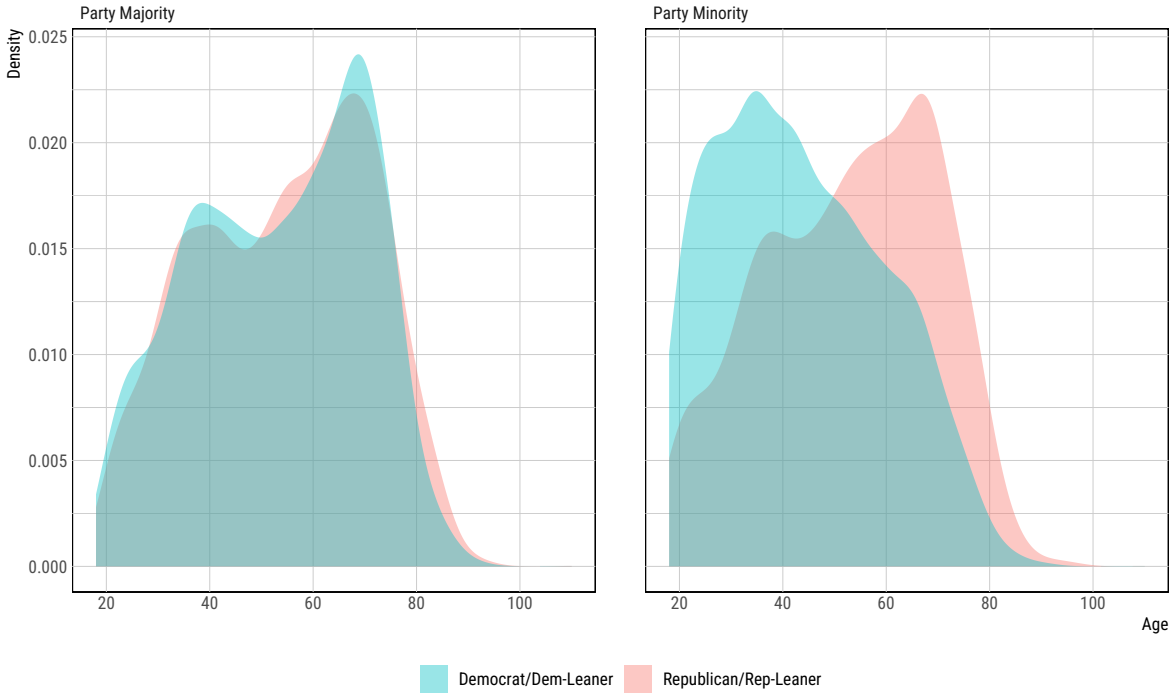

Figure A6: Density of Age by Party Minority Status

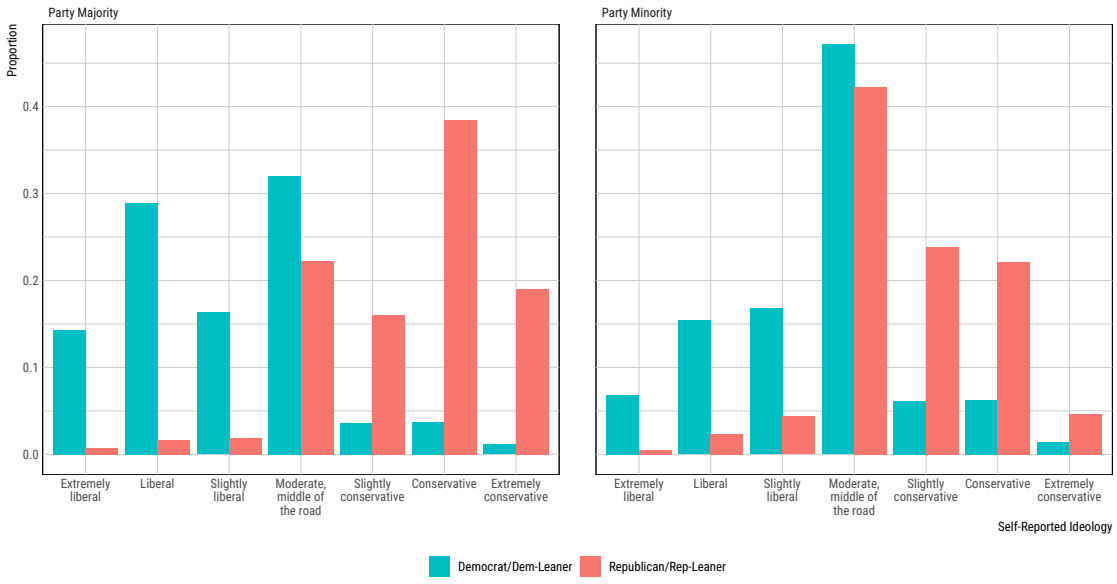

Figure A7: Self-Reported Ideological Identification by Party Minority Status

Here, we explore balance across pre-registered covariates for all four randomizations. Of

note, the "target" randomization includes 3 observations per individual so there is overlap across the assignments.

Table A16: Balance Tables Across Targets

	discussant (N=21400)		coopp (N=14237)		coally (N=14300)		out (N=14263)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
age	51.6	17.1	51.5	17.0	51.6	17.0	51.7	17.1
ask_1	38.2	25.5	38.2	25.6	38.3	25.5	38.0	25.4
ask_2	51.3	26.2	51.3	26.3	51.3	26.2	51.3	26.0
ask_3	47.6	27.4	47.5	27.5	47.6	27.4	47.7	27.3
dem	0.5	0.5	0.5	0.5	0.5	0.5	0.5	0.5
education	3.3	1.1	3.3	1.1	3.3	1.1	3.3	1.1
race_white	0.7	0.5	0.7	0.5	0.7	0.5	0.7	0.5

Table A17: Balance Tables for Ask-Tell Randomization

	control (N=10717)		treat (N=10683)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
age	51.8	17.1	51.4	17.0	-0.4	0.2
ask_1	38.0	25.4	38.3	25.6	0.3	0.4
ask_2	51.4	26.1	51.2	26.2	-0.1	0.4
ask_3	47.4	27.3	47.8	27.5	0.4	0.4
dem	0.5	0.5	0.5	0.5	0.0	0.0
education	3.3	1.1	3.3	1.1	0.0	0.0
race_white	0.7	0.5	0.7	0.5	0.0	0.0

Table A18: Balance Tables for Conversation Topic Randomization

	Econ issues (N=5774)		Social issues (N=5833)		2024 Election (N=5751)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
age	52.4	17.2	52.3	17.1	52.4	17.1
ask_1	38.1	25.3	38.0	25.4	37.9	25.6
ask_2	51.2	25.8	52.2	25.9	51.9	26.2
ask_3	47.9	27.4	48.6	27.4	48.2	27.3
dem	0.5	0.5	0.5	0.5	0.5	0.5
education	3.4	1.1	3.4	1.0	3.4	1.0
race_white	0.7	0.5	0.7	0.5	0.7	0.5
party_minority	0.2	0.4	0.2	0.4	0.2	0.4
turnout	0.0	0.2	0.0	0.2	0.0	0.1
interest	3.5	1.1	3.5	1.1	3.4	1.1

1.5 Reexamining Surveys about Self-Censorship

Respondents were asked to rate their agreement toward three statements. The wordings come from two public survey reports which provide conflicting accounts about the degree to which Democrats and Republicans asymmetrically engage in self-censorship and avoid political discussion. The first pair resembles survey language used by APM Research Lab, while the last statement mirrors survey language used by the Cato Institute.

1.6 Alternate Specifications

We first explore heterogeneity by a more stringent definition of intra-party conflict - defining minorities as respondents who say they will vote for the opposing party's nominee (as opposed to the broader pre-registered definition). Using this alternate specification we replicate two results: the baseline gap in willingness to speak (Figure 1) and the response to the correction treatment (Figure 6). We note, consistent with expectations from public polling and academic literature, that far smaller fractions of self-identified partisans affirmatively supported the opposing party's nominee as opposed to merely declining to support their own party's candidate.

Next, we explore heterogeneity by expressed turnout intention. While these self-reported answers are likely over-estimates of realized voter behavior (Jackman and Spahn 2019), we might expect that individuals who do not intend to vote have less strong feelings about their preferred candidate. Consistent with this interpretation, 76% of the sample reported a certainty to vote, compared to 64% of the American population that voted in 2024. Below, we compare the certain voters to the rest of the sample for out main outcomes. Further, most of the self-reported non-voters in our sample were pure independents leaving us with only 3064 partisans who expressed anything other than certainty to vote. We present results for certain-to-turnout voters and possible abstainers. The ask-tell correction operates similarly for both groups but abstainers have lower compensation demands to speak to opposing partisans and co-partisan allies.

Similarly, we explore heterogeneity by political interest. Our political interest variable

Table A19: Target and Ask-Tell Effects by Turnout Intention

	Voters	Abstainers
Co-partisan Ally	4.833***	1.850
	(0.849)	(1.943)
Opposing Partisan	12.876***	4.761*
	(1.078)	(2.167)
Ask-Tell Treatment	−6.466***	−6.915*
	(1.254)	(2.805)
Num.Obs.	41 192	8594
R2	0.307	0.276
Std.Errors	by: id	by: id

is on a 5 point scale. Theoretically, we might expect that more interested subjects are less responsive to our treatment because they are less effected by the etiquette mechanism we described. While exploratory, this analysis confirms our intuition, albeit directionally. Across all targets, each 1 point increase in political interest (SD = 1.1) is associated with a $2 decline in the effectiveness of the correction.

Table A20: Ask-Tell Effects by Target and Political Interest

	All Targets	Discussants	Out-Partisans	Co-Ally	Co-Opponent
Ask-Tell Treatment	−10.435**	−14.272**	−5.949	−11.323	−16.627*
	(3.983)	(5.392)	(8.140)	(7.163)	(7.216)
Political Interest	−2.127**	−2.615*	−1.489	−1.401	−4.496**
	(0.803)	(1.018)	(1.568)	(1.404)	(1.419)
Ask-Tell:Interest	1.111	2.086	−0.194	1.041	3.189+
	(1.088)	(1.416)	(2.160)	(1.901)	(1.900)
Num.Obs.	49 868	13 787	9135	9157	9113
R2	0.298	0.421	0.193	0.339	0.287
Std.Errors	by: id	by: id	by: id	by: id	by: id

1.7 Placebo and Spillover Tests

Does the presence of a target affect the CD for the other target? To test this within-subject stable-unit-treatment value assumption (which is that the potential outcomes of observation c for individual i are unchanged for different assignments to the same individual for observations $c+1$ and $c+2$) we compared CDs for each of the three categories as follows: does the CD of Target 1 differ if the other randomly assigned Target is Target 2 compared to Target 3? We present these results for each of the three randomly assigned targets. We can see that there is no statistically or substantively significant violation of our assumptions. In Table A21, the baseline category for the omit variables is "co-partisan ally" and the baseline target is the frequent discussant. The first two coefficients show the change in the "frequent discussant" CD when the co-opp and out-partisan targets are omitted while the next three interaction terms show how the treatment effect of those targets is modified depending on the omitted category.

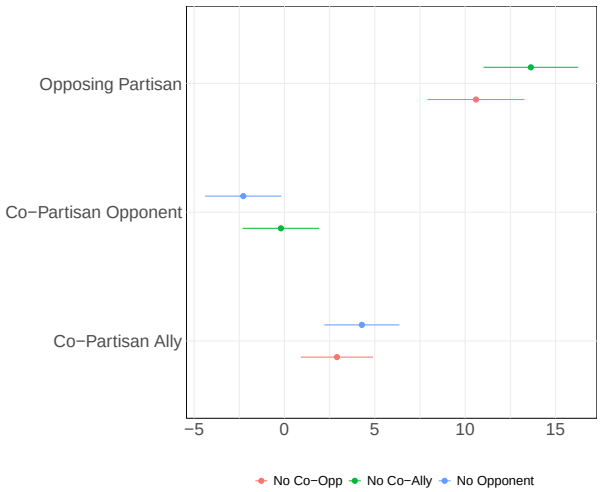

Figure A8: Comparison of Target Effects by Omitted Category

Table A21: Spillover Effects Model

	(1)
Omit Co-opp	1.587
	(1.557)
Omit Out	2.644^{+}
	(1.562)
Coally:Omit Co-Opp (baseline = Omit Out)	−1.379
	(1.473)
Opponent:Co-Opp (baseline = Omit Co-Ally)	−3.078
	(1.913)
Coally:Co-Opp (baseline = Omit Co-Ally)	−2.094
	(1.526)
Num. Obs.	49 868
R^2	0.300
Std. Errors	by: id

1.8 Descriptive Statistics and Self-Reported Censorship Fears

We next examine how partisanship and party-minority status relate to pre-treatment self-censorship questions. Although these questions are less effective at capturing true self-censorship than our randomized ask-tell design — because of the risk partisan cheerleading about out-parties and factions — they still provide meaningful self-reported indicators and contextual evidence for our interpretation. We asked three items: one about general self-censorship, one about fears of Republicans, and one about fears of Democrats.

Using these measures, we find that party minorities express somewhat greater self-

censorship concerns, but the effects are small and inconsistent across parties: less than one-tenth of a standard deviation for Democrats, and a slight negative correlation for Republicans. Minority partisans do report notably higher concern about co-partisans than do majorities, but contrary to common partisan narratives, we find no meaningful differences in overall self-reported censorship by minority status within either party, as shown in Tables A22 and A23.

Table A22: Descriptive Statistics for Censorship Fears among Dems

	General Censorship	Fear of Republicans	Fear of Democrats
Democrat	2.481***	3.346***	3.993***
	(0.157)	(0.157)	(0.165)
Partisan Minority	0.042	−0.164**	0.345***
	(0.052)	(0.050)	(0.055)
Network Copartisan Opponent	0.553***	−0.099	0.982***
	(0.128)	(0.129)	(0.139)
Network Opposing	1.375***	1.014***	0.663***
	(0.117)	(0.113)	(0.119)
Num.Obs.	8998	8993	8987
R2	0.087	0.136	0.095

Table A23: Descriptive Statistics for Censorship Fears among Reps

	General Censorship	Fear of Republicans	Fear of Democrats
Partisan Minority	0.028	0.464***	−0.076
	(0.061)	(0.062)	(0.062)
Network Copartisan Opponent	0.645***	1.444***	0.152
	(0.130)	(0.131)	(0.137)
Network Opposing	0.731***	0.760***	0.825***
	(0.130)	(0.126)	(0.128)
Num.Obs.	7936	7928	7929
R2	0.057	0.102	0.055

1.9 Descriptives of Mediators and Outcomes

1.10 Ethical Statement

The authors declare that the human subjects research in this article was reviewed by the pertinent universities' Human Subjects Committees. All aspects of this project adhered to the ethical principles outlined by the American Political Science Association.

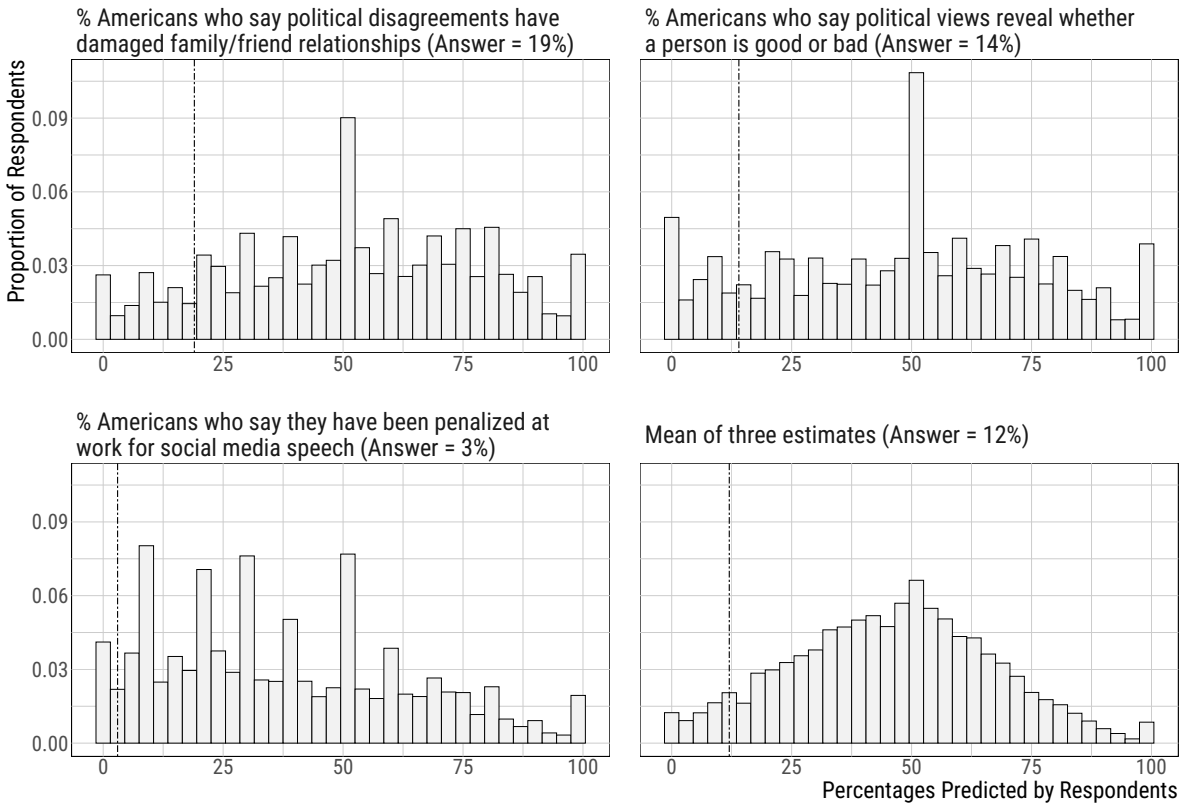

Figure A9: Respondents' "Ask-Tell" Prior Estimates

Participants were recruited through two widely used online platforms—PureSpectrum and CloudResearch—which provide panels for academic research. The PureSpectrum component was part of a larger study through the Civic Health and Institutions Project. All participants were required to provide informed consent prior to participation. The consent forms explicitly disclosed that participation was voluntary and that respondents had the right to withdraw at any point without penalty.

To the extent our study affected participants' beliefs about the real world, we provided correct information about the share of Americans reporting or seeking to inflict social repercussions for political speech, and this correction reduced harmful beliefs for the vast majority of participants. While the study explored preferences surrounding the 2024 presidential election, at no point were participants asked to share their political beliefs in their real social circles or to take any political action that affected real-world political processes.