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Information Exposure and Opinion Formation

By

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Abstract

Are people biased in their exposure to, and processing of, political information? Do people only expose themselves to information that agrees with, and reinforces, their political dispositions; and do they process information to be consistent with their prior beliefs and opinions? This dissertation provides two studies that aim to answer these questions. The first study uses a seven-year panel survey to understand whether Americans' news consumption is a function of their partisanship and ideology, as well as whether exposure to a particular ideology of news sources shapes one's political identity. The second study uses an original survey experiment to assess: 1) individuals' tendencies to follow misinformation from copartisan elites about climate change; and 2) tendencies to reject corrections from science experts that rebut this misinformation. I find little evidence that individuals are biased in how they seek and respond to information. One's political identity does not inform the degree to which one is exposed to liberal or conservative news sources. Neither do individuals adhere to misinformation from copartisan elites when it is identified as such by scientists. Instead, I find that individuals seek out and respond to information in a largely unbiased manner: a boon for democracy and a well-informed public.

Acknowledgements

Thank you to Ben Highton, Cheryl Boudreau, and Amber Boydstun; this dissertation benefited greatly from your guidance and expertise. Special thanks to Ben Highton, whose tutelage and friendship grew me into the thinker, writer, and researcher that I am today.

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Chapter 1: Introduction

This dissertation investigates the relationship between exposure to political information and one's political identity and opinions. Lazarsfeld, Berelson, and Gaudet's (1948) observation that voters selectively expose themselves to campaign information that "[reinforces] the predispositions with which [they came] to the campaign," (p. 76) serves as the foundational impetus for the two empirical studies conducted here. In the first study, I evaluate people's tendencies to seek information that is already in agreement with their political identity and whether that identity is shaped by the information they are exposed to. In the second study, I investigate whether people are willing to engage with expert corrections to politically related misinformation, especially when the misinformation aligns with their prior beliefs about a subject and the correction conflicts with those beliefs. These studies test the extent to which individuals are biased in seeking congruent information and rejecting incongruent information.

The central question asked in the first study is, "In what direction does the causal arrow run between exposure to news media and one's fundamental political identity?" Understanding the answer to this question carries substantial implications concerning what moves the "unmoved mover" of party identification (Chen and Goren 2014; Johnston 2006) and its increasingly entangled sibling: symbolic (or self-reported) ideology (Camobreco 2016; Ellis and Stimson 2009). In investigating exposure to news and political identity as both causal forces and outcomes, this investigation also provides a substantial test of the theory derived from Lazarsfeld, Berelson, and Gaudet's (1948) observations that people only seek likeminded information that reinforces preexisting attitudes and dispositions.

Empirical tests of these relationships are not new by any stretch (see Barnidge and Peacock (2019) for a review), but my tests use two novel tools with the goal to provide new and more convincing insights. I use a seven-wave, six-year U.S.-representative panel survey to get substantial leverage in inferring causality; and I develop and test a considerably more nuanced measure of news media exposure than has been used before, with the goal that the measure provides a more internally valid assessment of exposure to a particular ideology of news information. Together, these tools arguably allow for a stronger test of hypotheses pertaining to selective exposure than have previously been conducted.

I find evidence that stronger exposure to more ideologically extreme news content produces a corresponding stronger political identity, such that greater exposure to more conservative (liberal) news media in one measurement period is associated with a more right-leaning (left-leaning) political identity in the next measurement period. However, I do not find strong support for the inverse; a directionally stronger political identity does not appear to produce greater exposure to more ideologically congruent news media. Together, these findings do not conform to recent accounts of a "reinforcing spirals" relationship between news exposure and other political quantities, where greater exposure to ideologically slanted news media is argued to feed into more extreme opinions and identities, which, in turn, lead to greater selective exposure (Dahlgren, Shehata, and Strömbäck 2019; Moeller, Shehata, and Kruikemeier 2018; Slater 2015). Instead, I only find evidence that political identity is "moved" by ideological news exposure and is not, in turn, a "mover." This is an especially surprising finding given several factors: the relative stability of political identity and its regularly recognized status as the root cause of many opinions and behavior in American politics (Huddy and Bankert 2017); arguments that we have entered a new era of minimal media effects (Arceneaux and Johnson

2010; Bennett and Iyengar 2008); and empirical support for theories related to attitude-congruent information-seeking (Bolsen and Palm 2019).

The central question asked in the second study is, "Are people motivated to adopt climate change opinions in line with their prior attitudes and partisanship, and do they reject information that conflicts with these dispositions?" Much like the first study, the second study tests hypotheses related to whether individuals are "directionally motivated" (Druckman 2012; Kunda 1990; C. Taber and Lodge 2013). While the first study looks for prior-disposition congruent information-seeking, the second study looks to see if people adopt opinions congruent with their prior dispositions and reject conflicting information. The findings from this study directly relate to pertinent and current questions about the extent of directionally motivated reasoning among the public, which remains an open area of debate (Groenendyk and Krupnikov 2021). Druckman and McGrath (2019) are especially skeptical of positive findings of directional motivation in climate change opinion formation; they argue that no related studies account for the possibility that individuals are actually motivated to form accurate opinions. If, for instance, Republicans trust information from copartisan elites – who may deny that the climate is changing due to human activity – more than climate scientists – who often refute these false claims – then it cannot be convincingly argued that Republicans adopt beliefs about climate denialism simply to be congruent with prior attitudes on the subject or to be aligned with their central political ingroup. Adoption of such beliefs may, instead, be a product of a difference in perceived credibility of the information sources.

The survey experiment I conduct *does* control for the perceived credibility of the sources of information participants are provided as experimental treatments. In presenting participants with misinformation about climate change from copartisan elites and a correction by scientists and subsequently gauging their opinions about the arguments presented and opinions about climate change, I also account for how credible they deem each source to be, measured before the treatment arguments are presented. To address Druckman and McGrath's (2019) concerns then, I evaluate perceived source credibility in conjunction with testing for manifestations of directional motivated reasoning.

A second contribution this study makes is that it evaluates motivated reasoning in climate change opinion among an understudied group: self-identified liberals and Democrats who are largely concerned about the threats posed by climate change. Existing studies of on the topic focus almost exclusively on climate change denialism and Republicans’ response to such claims and corrections. In contrast, I present – to a sample of self-identified Democrats – misinformation from a Democratic elite that exaggerates the severity of climate change and a correction by scientists that moderates the exaggeration. In focusing on motivated reasoning in climate change opinion formation among liberals and Democrats, this study aims to evaluate the generalizability of findings from previous studies and provide a more holistic impression of the extent of directional motivated reasoning in the public.

I find that participants in the study do not follow their copartisan elite’s message about the severity of climate change. In fact, I find the opposite: participants evaluate the correction by scientists more highly when the source of the misinformation is identified as a copartisan. The correction also reduces threat perceptions about climate change, regardless whether the misinformation source is identified as a copartisan. I also find that prior attitudes about the threat of climate change do not moderate the influence of the treatment on evaluations of the arguments or opinions about climate change. In short, I do not find evidence that participants form opinions and evaluations to be consistent with their prior attitudes or to be aligned with their partisanship in manners consistent with directional motivated reasoning.

While I do not find that perceived credibility of the informational sources moderates influence of the treatments in a way predicted by Druckman and McGrath (2019), I find generally that participants follow the experts – climate scientists – in a manner consistent with accuracy motivated reasoning. Participants evaluate the correction favorably only when the arguments presented are attributed to their respective sources. In sum, participants appear motivated to form accurate opinions about the arguments presented and climate change more generally, and appear skeptical of messaging from their own party. This evidence is observed despite conflicting information from copartisan elites and despite the fact that the scientists' correction conflicts with prior beliefs about the imminent threat of climate change.

These findings are in general alignment with more recent work on motivated reasoning that trends toward pushing back on earlier studies' implications of near-ubiquitous directional motivated reasoning in public opinion formation (Druckman and McGrath 2019; Groenendyk and Krupnikov 2021). However, the evidence I find of a copartisan backlash effect in evaluations of the treatment arguments goes beyond what is expected by accuracy motivated reasoning and has not been previously documented. I explore the implications of this finding and the results of the experiment more generally at the end of Chapter 5.

On the whole, the results of the two studies conducted in this dissertation carry normatively positive implications. I find that individuals' political identities and opinions do not influence information seeking and processing in a way that could be considered biased (i.e. done to conform to, and reinforce, prior dispositions). Instead, I find that individuals respond to information by forming identities and opinions directionally consistent with the content of the information. Further, when presented with conflicting information, I find that individuals adhere to the expert when evaluating the information and forming opinions on the topic at hand.

The methodologies I employ in my studies aim to bolster the weaknesses of the other and so provide confidence in the common set of implications drawn from their findings. In looking at change over time among the same set of individuals from a nationally representative six-year panel survey, the first study provides some assurance of generalizability over time and the population under study. Unlike the survey experiment conducted with a convenience sample of undergraduate students, where responses are gauged immediately following treatment, one can be confident that the trends observed in the panel study are not ephemeral nor particular to a single demographic group. However, as with any observational work, the first study cannot account for all possible factors that may be related to both the treatment and outcome (i.e. possible confounders in the relationship observed between political identity and news exposure). The second study complements this weakness by randomly assigning presentation of political information – as treatments – to participants. Through the randomized controlled trial design, the survey experiment overcomes the possibility of selection bias associated with observational studies, yielding confidence in a causal effect of information exposure.

The studies described in this dissertation generate clear and obvious extensions that would shore up their weaknesses and improve external validity. The panel survey I use includes many possible issue opinion variables that have been demonstrated in other studies to be related to news exposure, including those pertaining to climate change (Feldman et al. 2012, 2014a; Krosnick and Macinnis 2015). Taking advantage of the strengths offered by the panel survey I use to assess the relationship between ideological news exposure and climate change opinion would facilitate both broadening the generalizability of the findings from my panel study and as well as validating the findings from my survey experiment. Fielding my survey experiment to a broader swath of Democrats would similarly provide a stronger test of the generalizability of my findings; perhaps there is something particular to political science undergraduates or when I fielded the experiment that led to the observed data patterns, and that Democrats at large tend more toward following copartisan cues. I explore these avenues in greater depth in the discussion section of each chapter and holistically in the conclusion of the dissertation.

The remainder of the dissertation proceeds as follows. Chapter 2 begins the study on selective exposure and political identity using a coarse measure of ideological news exposure. Chapter 3 concludes the study using my own measure. Chapter 4 begins the study on motivated reasoning and climate change opinion, assessing one set of dependent variables related to evaluations of the treatments presented. Chapter 5 concludes the motivated reasoning study in assessing the second half of dependent variables related to opinions about climate change. Chapter 6 completes the dissertation with a synthesis of the studies' findings, implications, and future avenues of research.

Chapter 2: A panel study of the relationship between Fox News exposure and political identity

Introduction

This study seeks to assess the extent to which political groups engage in exposure to likeminded news and how this selectivity and one's political identity are causally related to each other. Chapter 2 operationalizes selective exposure to a particular news ideology through exposure to Fox News, a known conservative outlet. Chapter 3 seeks to provide nuance and sophistication to measuring news ideology by quantifying the ideological slants of a spectrum of news outlets.

Beginning with Lazarsfeld, Berelson, and Gaudet's (1948) observation that voters selectively expose themselves to campaign information that "[reinforces] the predispositions with which [they came] to the campaign," (p. 76), much theoretical and empirical work followed that documents how selective exposure bifurcates a wide array of political attitudinal and behavioral phenomena along partisan lines (Abelson 1968; Barnidge and Peacock 2019; Garrett and Stroud 2014; Iyengar et al. 2019; Knobloch-Westerwick 2014; Lau et al. 2017; Lazarsfeld, Berelson, and Gaudet 1948; M. Levendusky 2013; M. S. Levendusky 2013; Sears and Freedman 1967; Slater 2007, 2015; Stroud 2007, 2008, 2011; Zaller 1996; Zillmann and Bryant 2013). This study aims to form an account of the relationship between one's selective news exposure and one's political identity among a panel of the U.S. public between 2011 and 2017.

I first ask whether political groups are polarized in their news exposure and whether they have grown increasingly selective over time. I find that liberal Democrats reported viewing Fox

News at far lower rates than conservative Republicans by the end of 2011, when the panel survey began, and polarized further over the course of the survey. Republican viewership of Fox News increased significantly by 2018, while the proportion of Democrats that reported watching Fox News remained close to zero.

I then ask whether self-reported partisanship and ideology inform exposure to Fox News. Specifically, does a more conservative and Republican identity produce increased exposure to Fox News via a *selection effect* (Slater 2007, 2015)? Or, does the causal arrow run in the opposite direction; does more exposure to Fox News produce a correspondingly stronger rightward identity via a *media effect*? Or, does causal direction run both ways via Slater's (2007, 2015) *reinforcing spirals model*: a positive feedback loop wherein media and selection effects reinforce each other over time (Dahlgren, Shehata, and Strömbäck 2019; Feldman et al. 2014b; Noelle-Neumann 1974; Slater 2007, 2015; Song and Boomgaarden 2017)?

Using an extension of the cross-lagged panel model that estimates within-individual effects while accounting for time-invariant factors (e.g. race and gender), I find that past exposure to Fox News informs one's future political identity; heightened levels of exposure to Fox News significantly predicts a directionally stronger conservative/Republican identity in the future. This finding indicates that individuals' partisanship and symbolic ideology are, at least in part, products of the political news information to which they are exposed. However I do not find evidence that past political identity informs future Fox News exposure, indicating lack of an individual-level selection effect. Based on these findings, it appears that individuals' political identities are shaped by the news they are exposed to, but individuals do not, in turn, change their news exposure as a function of their political identity.

To begin, I review the state of the selective exposure literature, highlighting the existing gaps in empirical research. I find there to be little work that assesses the relationship between news exposure and the political identities of the same individuals over meaningful periods of time, so that the interplay between one's identity and their news consumption remains largely unknown. To address this gap, I use a nationally representative panel survey of U.S. adults that was fielded monthly between 2011 and 2018, The American Panel Survey (TAPS), conducted by the Weidenbaum Center at Washington University in St. Louis. TAPS provides repeated measures to the same individuals over meaningful lengths of time, allowing for up to a 7-wave panel design. Panel data provides advantages over time series and cross-sectional data in its ability to control for unobserved, time-invariant confounders, as well as the ability to assess the direction of causal relationships (Allison 1994; Allison, Williams, and Moral-Benito 2017; Angrist and Pischke 2008; Bartels 2006; Bell and Jones 2015; Halaby 2004; Wooldridge 2010).

I then begin my empirical investigation with a simple and coarse measure of selective exposure: as the extent to which respondents prefer Fox News as their television news source of choice. I document selective exposure to Fox News at the aggregate level, then evaluate the relationship between an individual's exposure to Fox News and their political identity using an extension of the cross-lagged panel model.

The process and findings for my Fox News analysis provides impetus and validation for introducing substantial nuance to my measure of selective news exposure. In Chapter 3, I assign news outlets numeric values corresponding to their respective ideological slants. Thus I am able to capture the full ideological spectrum of outlets' ideologies, and assess the extent to which exposure to a given news ideology is causally related to one's political identity. I again start at the aggregate level and look at changes in selective exposure over time, then conduct an analogous set of individual-level analyses. I then perform a similar assessment with my novel measure of ideological news exposure, which adds a substantial degree of resolution beyond what exposure to just Fox News can offer. I reserve discussion of the results of study until the end of Chapter 3.

Theory and hypotheses

The study of selective exposure can be broken down into two central components: 1) one's selection of media (e.g. the outlets and programs one chooses to watch on television and with what frequency), and; 2) one's relevant identities, opinions, and behaviors (Knobloch-Westerwick 2014; Zillmann and Bryant 2013). These components are linked in two ways.

First, individuals choose media in accordance with their prior identities and beliefs. Lazarsfeld, Berelson, and Gaudet (1948) first found that the public seeks political and campaign content that agrees with their prior political dispositions and they avoid content that disagrees with such dispositions. Festinger (1957) provides a cognitive-psychological basis for Lazarsfeld, Berelson, and Gaudet's (1948) findings and the theoretical impetus for work to follow (e.g. Klapper 1960); people inherently seek to avoid cognitive dissonance and they do so by avoiding information that conflicts with their prior dispositions. Tajfel et al. (1979) additionally provide a social-psychological basis for seeking information congruent with one's social identity: people have a strong need to delineate between social groups – specifically the group to which they identify with and others – and to bolster their self-esteem by favorably comparing their own group with relevant outgroups. Media provides individuals the opportunity to express and learn about ingroup and outgroups' values by attending to sources and content that reflect their ingroup's values (Harwood 1999; Slater 2007). Thus *selection effects* may be defined as the influence of relevant attitudes and behaviors on exposure to particular media (Slater 2007, 2015; Thomas et al. 2021).

In American politics, there is no political identity more important than one's self-identified partisanship (Abramowitz and Saunders 2006). It follows that this aspect of one's identity should guide political news media consumption so as to seek political information congruent with the opinions and policy preferences of one's party and to avoid news that conflicts with these opinions and preferences; or, more in line with social identity theory, one should seek news sources that one identifies as belonging to one's ingroup. In the present study, these motivations are observationally equivalent and they both amount to increased selectivity as a function of one's political identity.

Slater (2015) in particular theorizes that selectivity should grow "when social identity is under threat, [such as] during political campaigns or other times when rival ideologies are becoming salient, or at times of economic or social strain" (373). And between these periods, levels of selectivity should be maintained or diminished. This expectation coincides with findings that political preferences and affiliations typically become stronger around presidential election seasons (Erikson and Wlezien 2012; Page and Shapiro 2010). Song and Boomgaarden (2017) use simulations to test for media selectivity under political campaign environments and find support for Slater's hypothesis. Since two presidential election cycles occurred during the period in which TAPS was fielded, I am able to – at least descriptively – add to this discussion.

The second way in which the two aforementioned components of selective exposure are related is that exposure to particular media may influence related attitudes and behaviors: e.g. exposure to a public health campaign on smoking may produce modest behavioral effects related to quitting smoking (Snyder et al. 2004). *Media effects* are thus defined as: "the deliberate and nondeliberate short and long-term within-person changes in cognitions (including beliefs), emotions, attitudes, and behavior that result from media use" (Valkenburg, Peter, and Walther 2016, 316). Valkenburg, Peter, and Walther (2016) document the variety of significant media effects found in other empirical work: from exposure to violent videogames and subsequent aggressive behaviors to exposure to tobacco use on television and attitudes toward smoking.

There exists a bounty of work on the effect of political media on attitudes, policy preferences, and voting behavior (Arceneaux and Johnson 2010; DellaVigna and Kaplan 2007; Elo and Rapeli 2010; Feldman et al. 2014b; Lau et al. 2017; Schemer 2012; Stroud 2007). However, very few studies assess the effects of news selectivity on political identity, especially among the same individuals and over multiple measurement periods, and this study aims to help fill this empirical gap.

Slater (2007, 2015) proposes that selection and media effects may be part of a reinforcing cycle within the same individual over time, termed the *reinforcing spirals model* (RSM).

> "If some type of media use influences corresponding beliefs or behaviors […] and that belief or behavior in turn increases that type of media use […], then the process should be mutually reinforcing over time. Persons engaging in this process should tend toward continued or increased use of that particular media content. This should lead to the maintenance or strengthening of the attitude or behavior in question, leading in turn to continued or increased use of relevant media content." (Slater 2007, 284)

Slater provides three qualifications necessary for this process to occur; if it was happening for everyone all the time, the public and news media would contain only political extremists. First, presence of threat to identity is strong, such as during political campaigns or other national events that become polarized across partisan lines. Second, an individual's identification with a relevant social group is strong and the individual is not cross-pressured by other identities. Third, exposure to information that conflicts with one's relevant identity is limited by group norms or the national culture, such as if a government limited expression of dissenting opinions (i.e. a "closed" system rather than an "open" system where individuals are freely exposed to contrary information). As stated previously, I am able to descriptively assess for the presence of reinforcing spirals during the 2012 and 2016 presidential election cycles. I am also able to account for potential cross-pressuring identities as control variables in my panel models. With the survey data at hand, with all respondents a part of a single national media environment, I am unable to account for the effects of a closed or open system.

In sum, I test three central selective exposure hypotheses in the context of mass news media and political identities:

1. Selection effects: A stronger/more extreme political identity produces increased exposure to more ideologically congruent news media.

2. Media effects: Greater exposure to more ideologically congruent news media produces a stronger/more extreme political identity.

3. Selection and media effects feed into each other over time to produce an individual with a more extreme political identity and media consumption habits (under the right conditions).

Because these effects are theorized to occur within the same individual over time, I use panel data, and statistical models that exploit their advantages, to test them. Thomas et al. (2021) provide a sound description of the manifestation of the effects for the same individual over time:

> "Media and selection effects lead to fluctuations (or deviations) from individuals' continuous baselines (or expected scores). If an individual seeks for attitude-congruent information […], their media use is higher compared with their baselines. In turn, fluctuations in media use affect individuals' subsequent attitudes leading to fluctuations from the underlying baseline. If both effects are ongoing, a mutually reinforcing relationship establishes. As media and selection effects are processed and vary 'within one and the same media user' (Schemer, Geiß, and Müller 2019, 265), both effects can be perceived as intraindividual components (Scharkow and Bachl 2019)." (Thomas et al. 2021, 191).

To note, the scope of this study and its empirics is limited to "mass communication" news media (i.e. newspaper, radio, television) and not "mass self-communication" (i.e. internet news and social media) (Castells 2007; Valkenburg, Peter, and Walther 2016). There is much research on selective exposure in a social media environment (An, Quercia, and Crowcroft 2013; Barnidge and Peacock 2019; Fletcher and Nielsen 2018; Garrett 2009; Moeller, Shehata, and Kruikemeier 2018), and while my findings would be more generalizable had I the proper data at hand, TAPS only asked for respondents' preferred news sources for legacy news mediums, so I am unable to provide commentary on selective exposure in the context of internet-based mediums.

Preference for Fox news

Fox News is consistently cited as the having the strongest ideological slant among the major national news outlets, and exposure is found to contribute to more conservative policy preferences (Macinnis 2015; Morris 2005; Zúñiga, Correa, and Valenzuela 2012) and Republican voting behavior (DellaVigna and Kaplan 2007), so that it is a meaningful indicator of exposure to conservative news. Therefore in this section, selective exposure is operationalized as the extent to which political groups (i.e. conservative Republicans and liberal Democrats) differ in their exposure to Fox News.

In seven waves between December 2011 and February 2017,[1] TAPS includes the question: "Where do you get MOST of your news about national and international issues? [Family members, friends, or workmates; television; radio; newspapers; magazines; internet; other]" Respondents are allowed to give up to 2 different responses. If respondents choose "television," they are then asked "Which of these sources is your most common source of news from television? [Local station; ABC; CBS; NBC; CNN; MSNBC; Fox News; other[2]]"

Note that, because both ideology and partisanship are expected to be related to issue opinions and selective exposure in the same manner, and because they are highly correlated, I combine partisanship and ideology into a respondent's latent *political identity*. 7-point self-reported partisan identification (strong Democrat to strong Republican) and 7-point self-reported ideology (extremely conservative to extremely liberal) are correlated at .77 in the total sample. I

[1] Wave 1 is December, 2011. Wave 2 is June, 2012 (7 months difference). Wave 3 is June, 2013 (12 months). Wave 4 is November, 2014 (17 months). Wave 5 is March, 2015 (4 months). Wave 6 is October, 2015 (6 months). Wave 7 is February, 2017 (13 months).

[2] Respondents that chose "other" among their most common source of news from television, newspaper, or radio were given a follow-up text box where they were asked to type their preferred outlet. I capture an additional set of national news media sources from these responses.

measure a respondent's political identity in a given wave as the average of their partisanship and ideology in that wave. The measure is coded -1 (strong Democrat and extremely liberal) to +1 (strong Republican and extremely conservative). To produce these figures, respondents are nominally split into liberal Democrats (less than 0) and conservative Republicans (greater than 0). The trends observed in the following figures and the results from subsequent analyses are robust to alternative specifications of political identity (i.e. party identification or ideology alone).

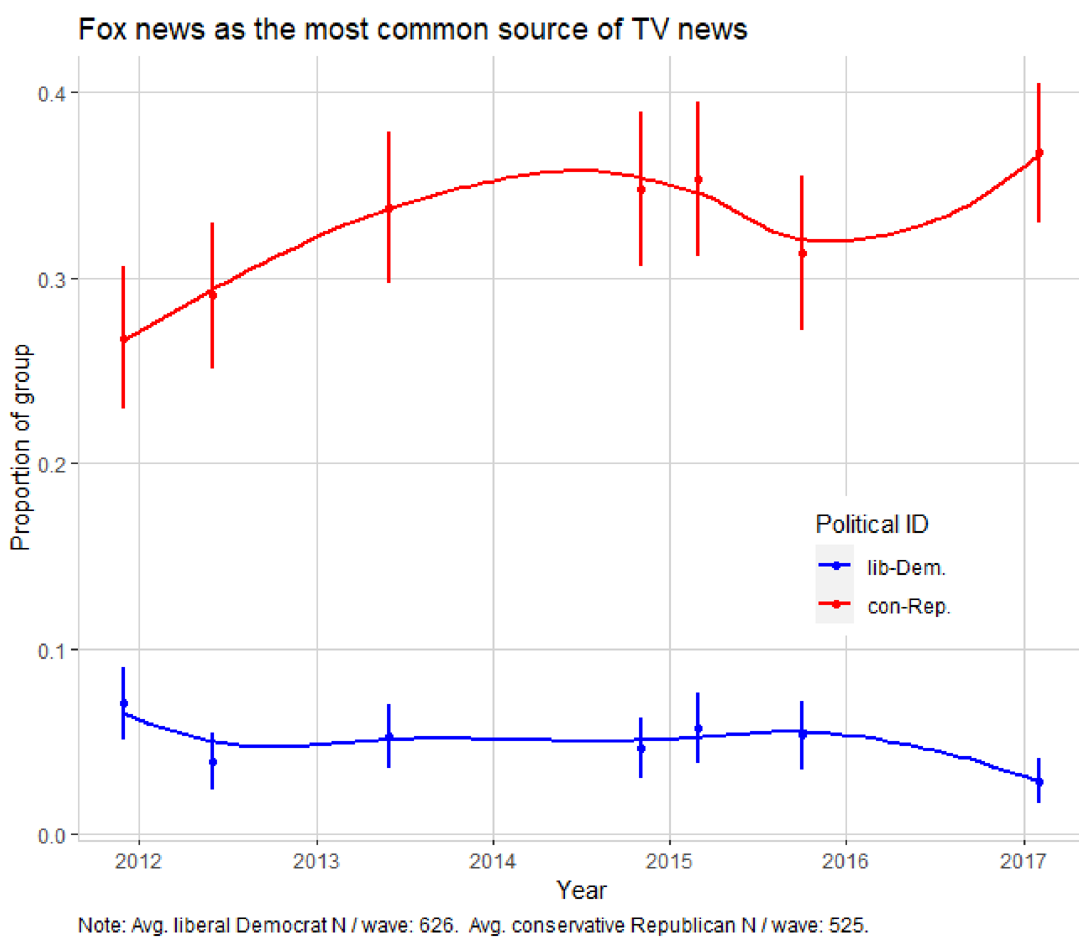

FIG. 2.1

Figure 2.1 shows the incidence with which liberal Democrats (referred to as lib-Dems for brevity) and conservative Republicans (con-Reps) reported that Fox News is one of their top two most common sources of news over the 6-year period. Proportions include all respondents in a given wave; i.e. the denominator includes respondents that did not state that their get their news from television. In the first survey wave in December 2011, 27% of con-Reps indicated that Fox News is their most common source of television news. This percentage rises through 2013, then dips slightly before rising again between October 2015 and February 2017, where it sits at 37%, a significant rise of 10%. Lib-Dems begin the survey at 7% and this number falls into 2013 and is maintained until it falls again between October 2015 and February 2017, ending at 3%, a significant decline from the end of 2011. To a moderated degree, the trend among lib-Dems mirrors the trend for con-Reps, but to a lesser degree.

Figure 2.1 does not necessarily indicate a panel effect. That is, it does not give a clear picture as to whether the same respondents tend to prefer Fox News is greater or lesser numbers over time. In the first survey wave in December 2011, TAPS successfully recruited 1,609 respondents. By the 63rd wave in February 2017, only 237 of the original respondents were retained. To combat panel attrition, TAPS recruited about 300 new survey respondents at four intervals over the course of the survey. This means that the trends observed in Figure 2.1 could, in part, be a product of individuals with weaker political leanings dropping out and being replaced by individuals with stronger leanings, who stay in the survey, artificially boosting the incidence of selective exposure.

Figure 2.2 illustrates a subsample of respondents; those that responded in all seven waves.

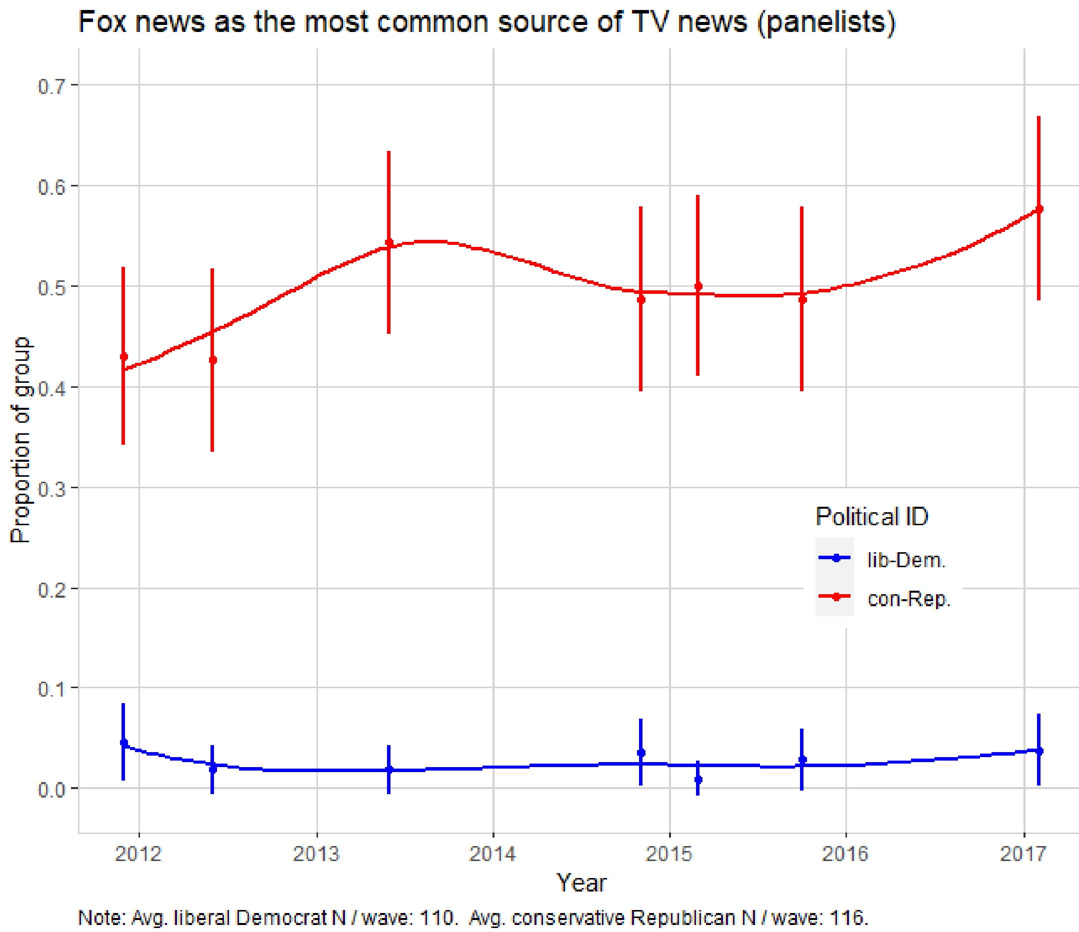

FIG. 2.2

While the proportion of lib-Dems that report their preferred source of television news is Fox News stays fairly constant over time, the incidence of con-Rep exposure to Fox increases from 43% to 58% between December 2011 and February 2017.

However, it may be that, rather than conservative Republicans moving to Fox News and liberal Democrats moving away from it over time, individuals adopted political identities to be congruent with their news. To provide an even clearer picture as to whether political groups have diverged in their exposure to Fox News over time, I take the average of a respondent's political identity over the 12 waves that partisanship and ideology questions were included. In

Figure 2.3 then, con-Reps and lib-Dems are respectively the same individuals in all seven waves, which provides a near-identical set of trends.[3]

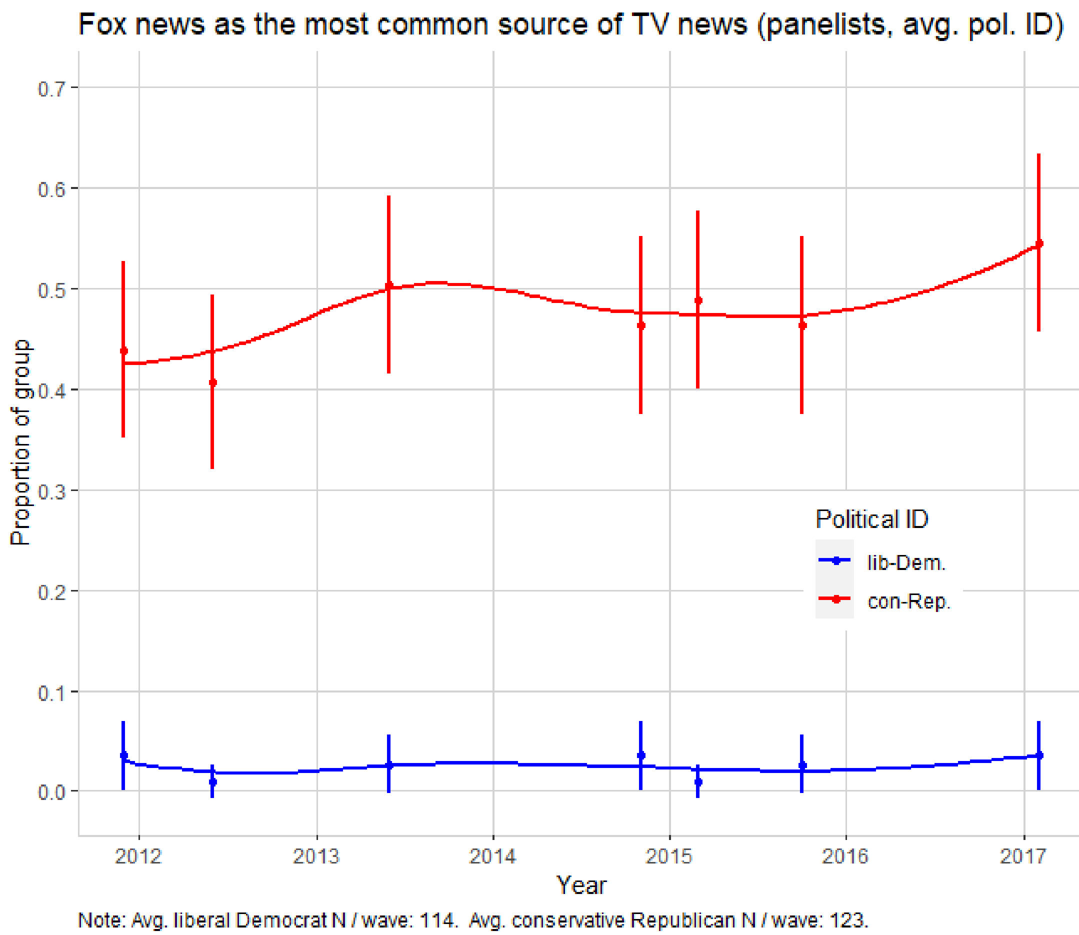

FIG. 2.3

Here it is clear that the greatest periods of change among con-Reps coincide with the 2012 and 2016 election cycles (waves 2-3 and waves 6-7), where the proportion of con-Reps that report to get most of their news from Fox News increases by about 10% and 8%, respectively. A certain floor effect prevents lib-Dems from diminishing their preference for Fox News further.

[3] When using respondents' first-wave political identity, I observe a near-identical set of trends.

An additional consideration related to the availability and use of panel data is that the survey respondents that were retained throughout the sample were more likely to engage in selective exposure than individuals that dropped out. While there is little difference in Fox News exposure between the overall lib-Dem sample and lib-Dems that were retained over the six-year period, the subset of con-Rep panelists started off watching Fox news at a 16% higher rate than the overall con-Rep sample and ended in 2017 at a 21% higher rate. There may be a selection bias in the con-Rep panelist observations toward more extreme selective exposure to Fox News. Related, survey respondents are generally more politically opinionated and have stronger political identities than those that do not respond to surveys (Brehm 2009), so it follows that respondents that stuck with a monthly survey for over six years have stronger tendencies toward selective exposure than individuals that only responded in one or two waves.

I find that respondents that dropped out of the survey have more moderate political identities than panelists retained over its six-year span. Respondents that responded to all seven waves (N = 237) have a mean absolute political identity (where 0: independent/moderate and 1: strong partisan/extreme ideology) of .54 ($\pm$ .03) in wave 1. Respondents that responded in wave 1, and excluding respondents that responded to all seven waves under analysis (N = 1050) – meaning that they dropped out some time before the last wave – have a mean absolute political identity of .48 ($\pm$ .02). This indicates that panel attrition and the resulting missing data is related to values of that data (i.e. more politically moderate individuals are more likely to drop out of the survey). Left unchecked, non-random missingness would bias estimates of models containing politically relevant parameters.

I address missing data and consequential estimate bias concerns in my individual-level models through use of the full information-maximum likelihood (FIML) method, which estimates model parameters directly using all information contained in the dataset; thus, partial observations (in this case, this mostly entails respondents that either dropped out, were introduced after the first wave, or a combination of the two) are retained as they are useful in parameter estimation. Most missing data methods, such as FIML and multiple imputation, rely on the assumption that data is missing at random. However, even in the case where the probability of missingness is correlated with values of relevant variables, "violation of the [missing at random] assumption does not seriously distort parameter estimates" (Collins, Schafer, and Kam 2001; Dong and Peng 2013, 3).

News attention

So far I have measured preference for Fox News among television news sources. It may be that some respondents that prefer Fox News watch it every day while others watch it once a week. This difference in the quantity of exposure to a given source should be related to one's political identity; more frequent exposure to Fox News should be tied to a stronger con-Rep identity.

In all seven waves that a respondent was questioned about their most common news sources, they were also asked, "How frequently do you pay attention to news about national and international issues? [every day (coded 1), several times a week (.8), once a week (.6), several times a month (.4), once a month (.2), less often (0), never (0)." I multiply a respondent's coded *news attention* value by their absence (0) or presence (1) of preference for Fox News in a given wave to form my measure of Fox News exposure. The net effect is maintained scores for respondents that report to attend to the news every day, and proportionately attenuated scores for less frequent viewers.[4]

Although there are ample measurement error issues associated with self-reports of news attention, such as over-reporting (Price and Zaller 1993; Prior 2005, 2007, 2009b, 2009a, 2013), there is not a comprehensive a work-around when using survey data. Zaller and colleagues (Price and Zaller 1993; Zaller 1992, 1996, 2003) developed a measure of news *reception* (based on correct answers to political knowledge questions) for use with survey data that captures "being exposed to a news story, attending to it, comprehending it, and remembering it" (Price and Zaller 1993, p. 135). While this measure employs some objective criteria for determining the degree to which individuals have attained political information from the news, there are a number of individual-specific factors – such as education and age – that influence the linkages between exposure to the news and eventual comprehension, memory, and application of news information (Zaller 1992, 1996). Because Zaller's measure of news reception does not assess exposure to the news per se (Prior 2009), I rely on self-reports of news source preference and exposure.

Figure 2.4 illustrates the distribution of responses to this item, per respondent and averaged across waves. The plurality of respondents report that they attend to the news every

[4] Incorporation of news attention into the Fox News preference variable means the variable must be treated as continuous, instead of categorical, in the models that follow, despite its largely bimodal distribution. While the modeling strategy I employ assumes multivariate normal distribution of the data, violation of this assumption should not severely bias parameter estimates (Allison, Williams, and Moral-Benito 2017; Rhemtulla, Brosseau-Liard, and Savalei 2012; Williams, Allison, and Moral-Benito 2018). Further, logistic regression with categorical endogenous parameters in structural equation models is incompatible with FIML to account for missing data, so I trade off use of an inefficient estimator (OLS) with the ability to account for missing data.

day, so that a respondent's measure of ideological news exposure does not – for the most part – differ greatly from their preference for Fox News.

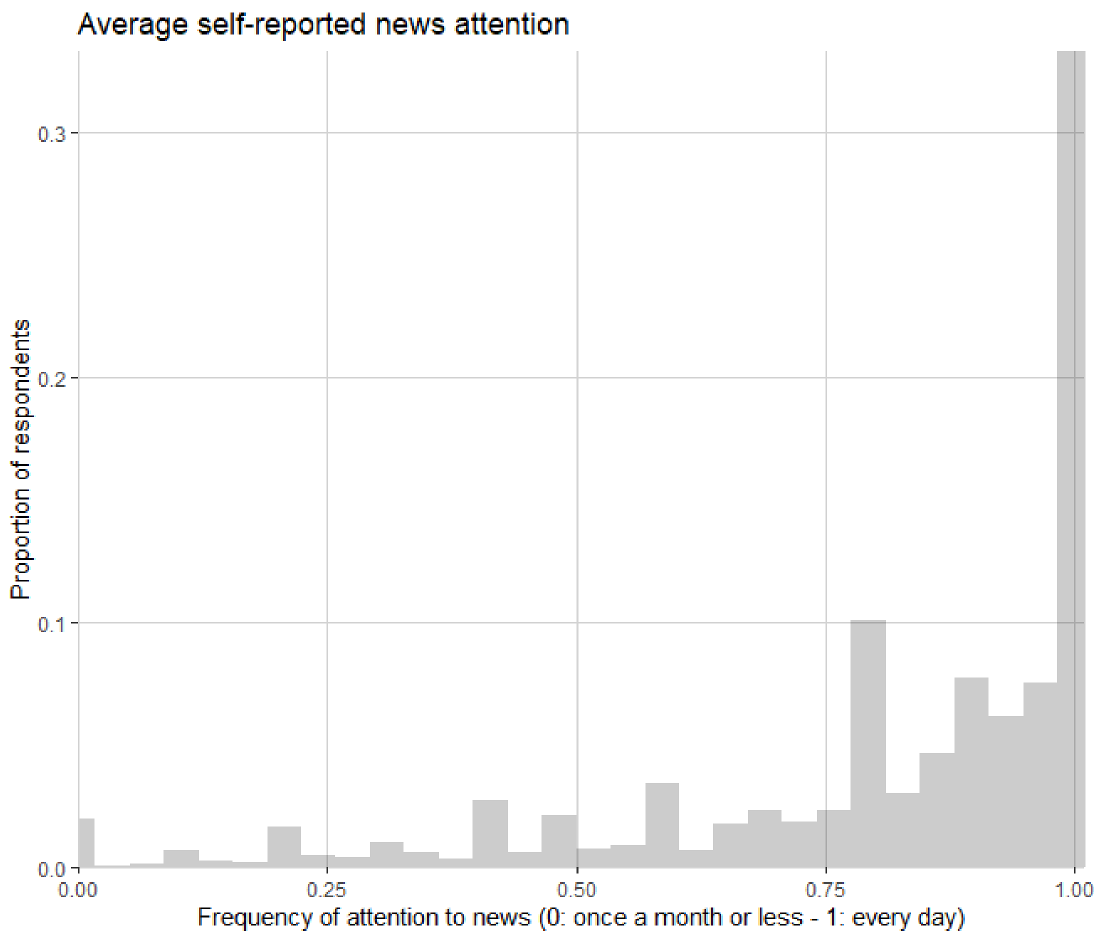

FIG. 2.4

Figure 2.5 reveals some small differences in news attention over time among the overall sample of respondents (the trend among panelists is the same but with wider confidence intervals). Consistent with the findings of Zaller (1992) and others (Dilliplane 2014; Strömbäck, Djerf-Pierre, and Shehata 2013), I document minor upticks in news attention around presidential election cycles.

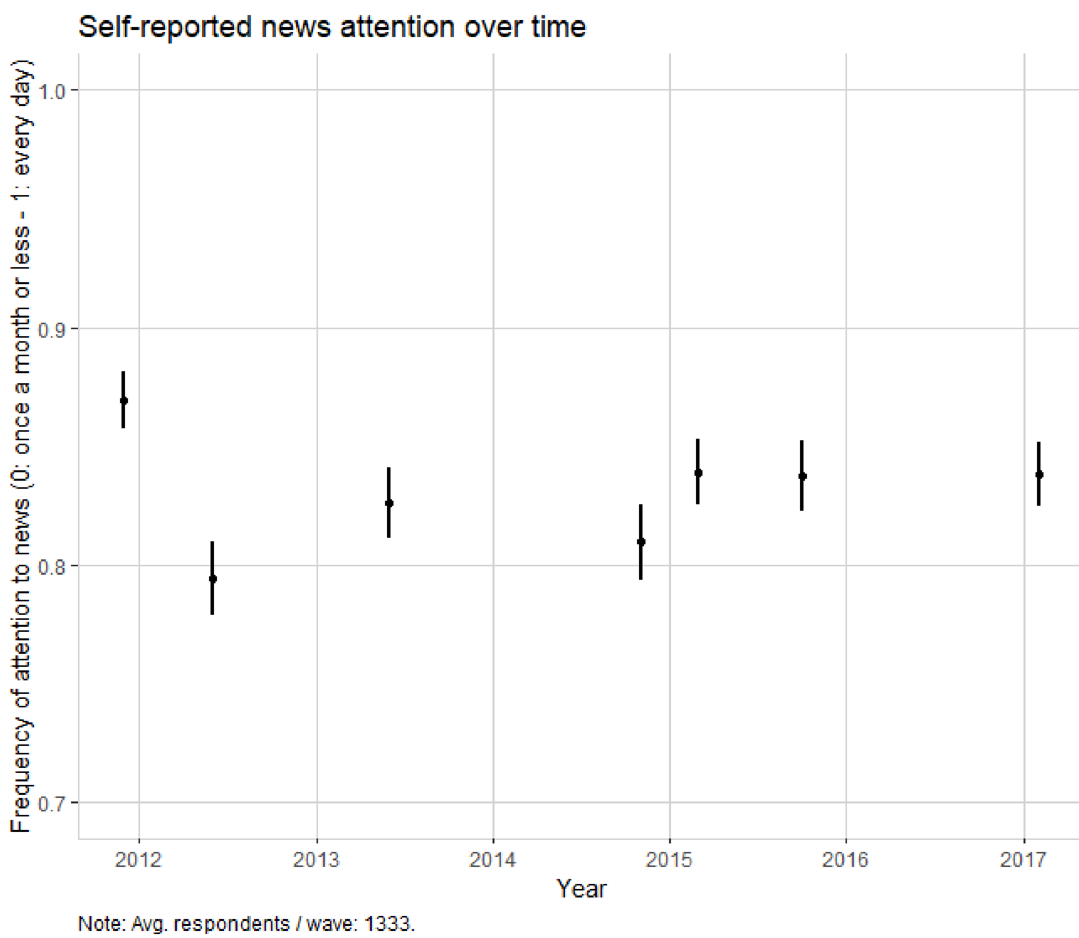

FIG. 2.5

Political identity

Before proceeding to the panel analysis, I describe the other key variable: political identity. Over the last 50 years, partisan identification and self-reported political ideology have become highly correlated and temporally stable constructs for most of the public (Abramowitz and Saunders 2006; Bafumi and Shapiro 2009; Huddy and Bankert 2017). Although, there still may be change in one's identity over time. I take the absolute value of one's political identity in a given wave in the panelist subsample to produce Figure 2.6, where 0 on the y-axis indicates independent and moderate identity and 1 indicates strong Democrat/Republican and extremely liberal/conservative.

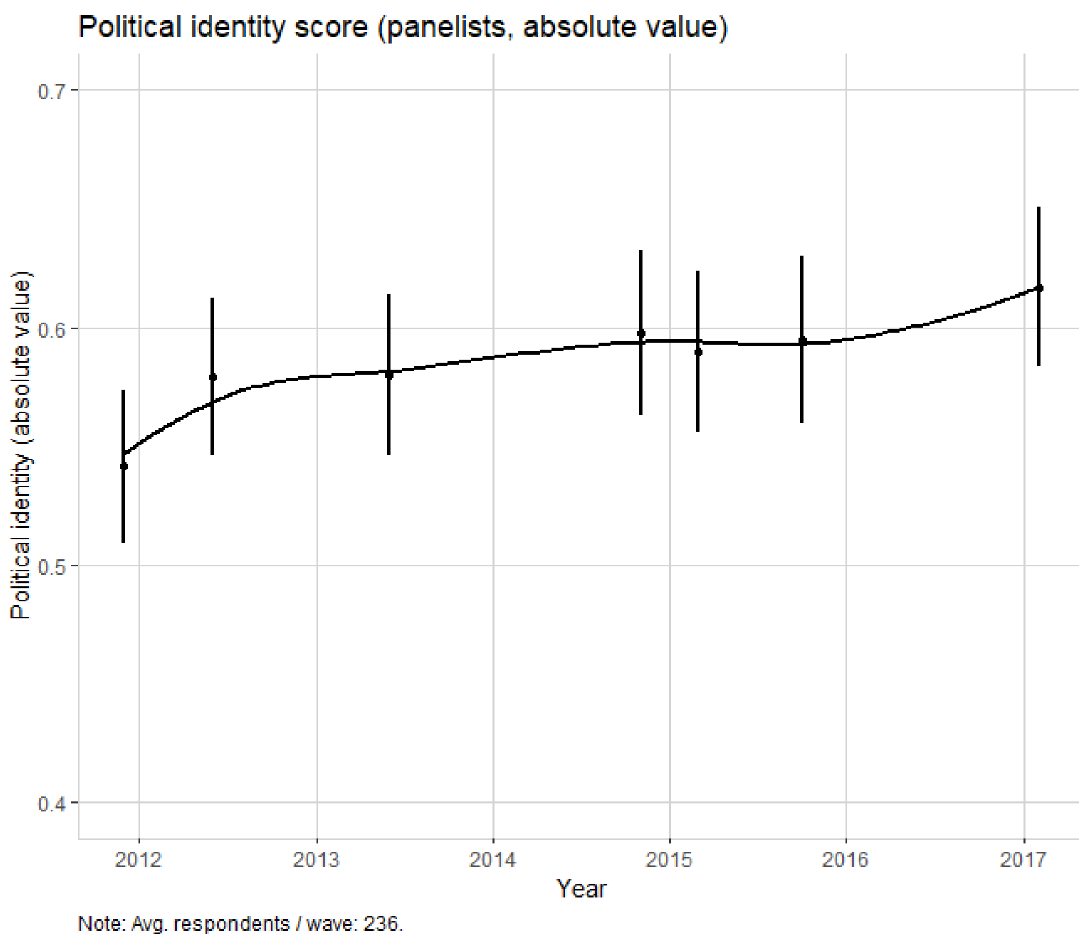

FIG. 2.6

There is a modest yet significant trend toward more extreme identities among the same group of individuals. In December 2011, the mean political identity score is .54 (± .03). In February 2017 it rises to .62 (± .03), a net increase of .12. Partisanship and ideology are both measured with 7-point scales and are averaged together to form political identity, so this is roughly the change in one response increment for each, such as moving from leaning Democrat to moderate Democrat *and* from moderate to slightly liberal. Thus there is change in individuals' political identity in the direction expected with increased selective exposure: a change over time to more extreme political identities that coincides with growing selective exposure to Fox News.

Methodology

I next describe my strategy to test for a causal relationship between one's political identity and their Fox News exposure. Because I am concerned with the causal direction of the relationship between ideological news exposure and political identity, I employ dynamic panel models to estimate a cross-lagged effect: i.e. y_t as a function of x_{t-1}.

While fixed effects (FE) models are considered the gold standard for removing estimate bias caused by time-invariant unobserved variables (Angrist and Pischke 2008; Wooldridge 2010), they rely on the assumption of strict exogeneity of the predictor variable. Thus they are unable to control for bias caused by a dynamic (i.e. reciprocal) causal relationship between outcome and predictor variables (Imai and Kim 2019). Lagging the dependent variable in FE models does not alleviate this bias (Bellemare, Masaki, and Pepinsky 2017).

Lagged first difference models relax the strict exogeneity assumption of FE models to be that of sequential exogeneity, but such models rest on the precise timing of lagged effects (Vaisey and Miles 2017). If the lag period specified in the model (8 months, on average in this case) is longer than the true lag, a coefficient estimate may be biased toward 0. In this case, if the effects of lagged selective news exposure on party identification are more instantaneous than can be measured with the data at hand – and informational effects tend to be fairly immediate (Chong and Druckman 2010; Druckman, Fein, and Leeper 2012; Hill et al. 2013) – then the true effect of the dependent variable cannot be measured.

Dynamic panel models attempt to solve endogeneity issues from reverse causality by including an autoregressive term (y_{t-1}) as a dependent variable. However, traditional cross-lagged panel models (CLPM), $y_t = y_{t-1} + x_{t-1}$, are unable to account for unobserved, time-invariant confounders. Traditional CLPM also cannot distinguish between within-unit change over time (such as FE does) and between-unit change. That is, researchers often interpret cross-lagged effects to be differences within individuals over time, but this cannot be statistically disentangled from differences between individuals (Thomas et al. 2021).

Moral-Benito (2013) introduced a CLPM with FE that can be estimated by maximum likelihood. This method assumes sequential exogeneity in the lagged x term, such that it allows for the possibility that x_{t-1} is influenced by y_{t-2}, y_{t-3}, and so on. P. D. Allison, Williams, and Moral-Benito (2017) then show that a structural equation modeling framework can be used to implement such an approach. The resulting method is termed maximum likelihood structural equation modeling (ML-SEM).

ML-SEM incorporates random intercepts unique to each individual. These intercepts represent mean differences between individuals and are treated as a set of latent or unobserved time-invariant effects on the dependent variable. When this intercept is correlated with time-variant variables, only within-individual variation remains (Allison, Williams, and Moral-Benito 2017; Bollen and Brand 2010). Further, the time-invariant variables can be identified and explicitly included in the model. Thus I am able to simultaneously estimate the between-individual effects of time-invariant variables, such as gender and race, and the within-individual deviation-from-one's-mean effects of time-variant variables of interest.

ML-SEM may still be susceptible to the issue of mis-specified temporal lags highlighted by Vaisey and Miles (2017), especially in the presence of aforementioned instantaneous and short-lived informational effects. Following Leszczensky and Wolbring (2019), I include a concurrent x term in models to account for the possibility that the effect of x on y is much closer to the concurrent measurement of y than the measurement of the lagged x term. As highlighted by Slater (2007, 284), "Concurrent paths can be conceptualized as correlations (or correlated disturbance terms) or as directional paths subject to problems of identification; concurrent paths should be conceptualized as time-ordered but with lags too small to measure in a given research design." Vaisey and Miles (2017) additionally find that ML-SEM provides biased cross-lagged estimates when only the cross-lagged term is included. Including a concurrent x term alleviates this bias.

Incorporating a concurrent x term helps solve a significant methodological issue, but simultaneously reveals a substantive issue concerning interpretation of the relationship between predictors and outcomes. It may be, for instance, that increased exposure to Fox News contributes to a more right-wing identity in a much shorter time period than the lagged measurement of Fox News can capture. The average length of time between measurement periods is 10 months. If the effect of Fox News exposure on political identity (or visa versa) occurs much closer to a few days or hours, the concurrent x term would likely capture any such effect. For instance, if an individual very recently considered themselves as a stronger Republican, they may watch more Fox News over the next week, at which point their news consumption habits may revert closer to their mean. The 10-month lagged term of a stronger Republican identity would not capture such a phenomenon, but if the individual responded to the TAPS survey soon after their identity shift, the concurrent x term would reflect this effect. Although, because x_t and y_t are measured at the same time, I am unable convincingly say that one came before the other. Shifts in identity and news exposure measured at the same time may simply go hand-in-hand, perhaps both affected by a third unobserved variable. I work to interpret the lagged and concurrent x term coefficients when I present the model estimates in the next section and again in the discussion section of the following chapter.

Two studies have employed ML-SEM to evaluate the reinforcing spirals model: 1) Moeller, Shehata, and Kruikemeier's (2018) 6-wave panel investigation of the relationship between internet use and interest in politics among adolescents, and; 2) Thomas et al. (2021)'s evaluation of various modeling strategies using the same data and variables. Thomas et al. recommend ML-SEM as a methodologically sound strategy to estimate intraindividual effects in the RSM context.

I use the *dpm* package (Long 2021) in *R* to perform the ML-SEM. This package serves as a user-friendly wrapper for ML-SEM with the *lavaan* package (Rosseel 2012). To account for missing data, I use a full information maximum likelihood approach, which has been shown to produce efficient and unbiased estimates in the presence of missing data and perform similarly to multiple imputation methods, while avoiding the modeling sensitivities of such methods (Hippel 2016; Lee and Shi 2021)[5].

A second missing data problem that I must contend with is that party identification, political ideology, and news exposure were not asked in all the same waves. For instance, ideology was not included as a question in the June 2012 survey (wave 2 in my models), though it was included in the May 2012 survey. I pool responses to these waves together assuming temporal autocorrelation; if ideology was included in the June 2012 wave, respondents would likely give very similar responses to what they gave in the preceding month. This process should

[5] Multilevel data, such as panel data, are particularly prone to sensitivity issues in specifying multiple imputation models, such that estimated values may vary greatly depending on small changes in model parameters (Allison 2012; Enders, Mistler, and Keller 2016; Lüdtke, Robitzsch, and Grund 2017).

downward bias parameter estimates as it introduces measurement error – and so noise – to the data. Combining a respondent's 7-point party identification and their 7-point ideology in a given wave into a single measure of political identity should help to reduce measurement error.

Consistent with previous research on both media use (Zillmann and Bryant 2013) and political dispositions (Bafumi and Shapiro 2009; Huddy and Bankert 2017) I include several time-invariant exogenous control variables in these models that may influence both political identity and news exposure: gender, race, educational attainment, religiosity, income, and age. These variables represent between-individual components that explain differences between each individual's outcome of interest (Slater 2015). While some of these variables are actually time-variant (e.g. age), TAPS only asks them in respondents' initial survey wave and only provides age in 4 age-range groups, so I cannot accurately measure age as time-variant. I am also only interested in the between-individual effects of age and am not concerned about how the within-individual aging process influences selective exposure or political identity.

All variables are coded between 0-1,[6] such that for ideological news exposure, 0 (1) represents high exposure to very liberal (conservative) news and for political identity, 0 (1) represents strongly liberal and Democratic (conservative and Republican) identification. Females, nonwhites, more educated, less religious, higher income, and younger individuals are expected to engage with more liberal news and to identify more strongly as lib-Dems than their counterparts. Exclusion of these variables from models do not substantially influence coefficients of the predictors of interest.

[6] Gender is coded male: 0, female: 1. Race is coded white: 0, nonwhite: 1. Educational attainment is coded less than high school: 0, high school: .33, some college: .66, bachelor's degree or more: 1. Religiosity is coded: never attend religious services: 0 to regularly attend religious services once per week or more: 1. Income is coded: less than $20,000: 0 to $250,000 or more: 1. Age is coded: 18-29: 0, 30-44: .33, 45:59: .66, 60 or more: 1.

Results

To begin, Table 2.1 provides results of ML-SEM models where coefficients are free to vary across waves. As mentioned previously, lagged coefficients depend highly on the time between waves, and because this time varies greatly across the seven waves where news exposure is measured (4 months to 17 months), it's likely that effects vary based on these time differences (Gollob and Reichardt 1987; Kuiper and Ryan 2018; Mulder and Hamaker 2020; Voelkle et al. 2012). Additionally, as seen in Figures 2.1-2.3, news selectivity rises mostly at the beginning and end of the timeseries: in proximity to presidential election seasons. Thus there may be contextual period effects that correspond with heterogenous coefficient estimates between waves. Specifically, it may be that cross-lagged effects are greatest around these politically charged periods and subside between them. Last, χ^2 model difference tests between constrained and unconstrained models reveal that constraining coefficients across time produces a significantly worse model fit (Satorra and Bentler 2001). However, in all models, fit indices indicate that the models fit the data well. That is, Comparative Fit Indexes (CFI) and Tucker-Lewis Indexes (TLI) are generally near 1, Root Mean Square Error of Approximation (RMSEA) is below .05, and Standardized Root Mean Square Residual (SRMSR) is generally below or near .01.

While there are strong advantages to leaving coefficients unconstrained, doing so produces six sets of results (for waves 2-7), which makes forming generalized interpretations difficult. I provide results where coefficients are constrained across time in order to provide an overall assessment of effects and comparisons between news exposure-political identity reciprocity. Standard errors are calculated in all models using the Huber-White estimator, so that they are robust to heteroskedasticity.[7]

Table 2.1: Political identity regressed on Fox News exposure (coefficients unconstrained across waves)

t = 2	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.020	0.015	1.279	0.201
Newsfox.exp	0.077	0.020	3.878	0.000
Female	-0.049	0.010	-5.086	0.000
Nonwhite	-0.082	0.012	-6.702	0.000
Age	-0.012	0.013	-0.965	0.335
Income	0.032	0.019	1.670	0.095
Religiosity	0.133	0.016	8.303	0.000
Education	-0.083	0.018	-4.670	0.000
Pol.ID (t - 1)	0.409	0.039	10.591	0.000

t = 3	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.033	0.014	2.447	0.014
Newsfox.exp	0.083	0.015	5.535	0.000
Female	-0.050	0.009	-5.519	0.000
Nonwhite	-0.094	0.011	-8.399	0.000
Age	-0.017	0.012	-1.402	0.161
Income	0.039	0.019	2.062	0.039
Religiosity	0.163	0.015	10.697	0.000
Education	-0.081	0.017	-4.889	0.000
Pol.ID (t - 1)	0.331	0.035	9.386	0.000

t = 4	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.022	0.014	1.526	0.127
Newsfox.exp	0.119	0.016	7.451	0.000
Female	-0.054	0.009	-6.152	0.000
Nonwhite	-0.080	0.011	-7.196	0.000
Age	-0.013	0.012	-1.144	0.253
Income	0.024	0.018	1.339	0.180
Religiosity	0.155	0.016	9.882	0.000
Education	-0.103	0.016	-6.521	0.000
Pol.ID (t - 1)	0.345	0.037	9.216	0.000

t = 5	Est.	S.E.	z val.	p

[7] Note I am unable to include sampling weights in models because some of this information is missing in TAPS data.

	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.060	0.016	3.733	0.000
Newsfox.exp	0.061	0.019	3.173	0.002
Female	-0.048	0.009	-5.522	0.000
Nonwhite	-0.097	0.011	-8.637	0.000
Age	-0.003	0.011	-0.263	0.792
Income	0.019	0.018	1.081	0.280
Religiosity	0.144	0.016	8.982	0.000
Education	-0.064	0.017	-3.776	0.000
Pol.ID (t - 1)	0.368	0.037	9.862	0.000

t = 6	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.010	0.019	0.549	0.583
Newsfox.exp	0.155	0.025	6.306	0.000
Female	-0.042	0.009	-4.487	0.000
Nonwhite	-0.099	0.011	-8.743	0.000
Age	-0.013	0.012	-1.013	0.311
Income	0.001	0.019	0.052	0.958
Religiosity	0.136	0.016	8.248	0.000
Education	-0.083	0.017	-4.767	0.000
Pol.ID (t - 1)	0.354	0.036	9.705	0.000

t = 7	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.169	0.025	6.778	0.000
Newsfox.exp	-0.007	0.016	-0.436	0.663
Female	-0.057	0.010	-5.735	0.000
Nonwhite	-0.110	0.012	-9.221	0.000
Age	0.003	0.013	0.230	0.818
Income	-0.010	0.021	-0.493	0.622
Religiosity	0.152	0.018	8.557	0.000
Education	-0.085	0.019	-4.571	0.000
Pol.ID (t - 1)	0.367	0.040	9.298	0.000

$X^2(26) = 158.749$, RMSEA = 0.041, 90% CI [0.035, 0.047], SRMR = 0.008, CFI = 0.994, TLI = 0.956

In waves (t) = 3, 5, and 7, lagged Fox News exposure (*newsfox.exp*) is significantly and positively related to political identity. Substantively in these waves, this cross-lagged effect indicates that: following a period of time during which an individual's Fox News exposure is relatively high (i.e. positively deviated from their mean), that individual's political identity becomes relatively more conservative and/or Republican in the following measurement period. This effect accounts for: 1) the possibility that a more conservative/Republican political identity in a previous wave (e.g. y_{t-2} or y_{t-3}) influenced Fox News exposure at t-1; 2) an individual's degree of Fox News exposure in the previous (e.g. x_{t-2} or x_{t-3}) and concurrent waves (i.e. *Newsfox.exp*), and; 3) all other stable, trait-like factors. The cross-lagged effect is largest in wave 7, where there is a 13-month period from wave 6. This is unexpected based on the largely ephemeral nature of media and informational effects, although the politically charged nature of this period – corresponding with the rise and election of Donald Trump to the presidency – may have a role in producing a large cross-lagged effect. The average cross-lagged effect size from waves in which the coefficient is statistically distinguishable from 0 is (.033 + .06 + .169) / 3 = .087.

The concurrent Fox News exposure coefficient (*Newsfox.exp*) either meets or greatly exceeds the magnitude and significance of its lagged counterpart in all measurement periods except the 7th. These coefficients certainly indicate that contemporaneous Fox News exposure and a right-leaning identity are generally highly correlated. The nature of these highly significant and large coefficients may also indicate that the bulk of the influence of Fox News exposure on political identity occurs quickly, and that there are often smaller, lingering effects of increased Fox News exposure captured by the lagged terms. As stated in the previous section, it is impossible to evaluate whether one or both of these interpretations are correct – given the data at hand – but both are certainly in line with the findings of previous research on selective exposure.

In all waves except 7, the cross-concurrent effect is also positive and significant, indicating that positive deviations from the mean between Fox News exposure and political identity are positively correlated. Because all variables are coded to range from 0 to 1, their relative effect sizes can be compared. The average cross-concurrent effect in waves where it is significant is .099, 14% larger than the average cross-lagged effect. The autoregressive effect is significant in all waves, and the average coefficient size is .359, 312% larger than the average cross-lagged coefficient.[8] In contrast to the cross-lagged and cross-concurrent coefficients, The autoregressive coefficients do not deviate much from each other across waves.

Outside of age and income, the time-invariant variable coefficients are significant and run in the expected direction, and their effect sizes do not vary greatly between waves. In Table 2.2, I show the reciprocal models: Fox News exposure regressed on political identity.

[8] The large magnitude of the autoregressive coefficient relative to the cross-lagged coefficient is expected in cross-lagged panel design (Kenny 1975, 2014; Wooldridge 2010).

Table 2.2: Fox News exposure regressed on political identity (coefficients unconstrained across waves)

t = 2	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.067	0.071	-0.934	0.351
Pol.ID	0.069	0.070	0.978	0.328
Female	-0.048	0.016	-2.937	0.003
Nonwhite	-0.068	0.017	-3.903	0.000
Age	0.060	0.021	2.817	0.005
Income	0.054	0.035	1.565	0.118
Religiosity	0.066	0.026	2.585	0.010
Education	-0.074	0.031	-2.418	0.016
Newsfox.exp (t - 1)	0.108	0.045	2.390	0.017

t = 3	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.110	0.071	-1.558	0.119
Pol.ID	0.193	0.072	2.661	0.008
Female	-0.035	0.017	-2.084	0.037
Nonwhite	-0.013	0.018	-0.714	0.475
Age	0.105	0.022	4.850	0.000
Income	0.032	0.035	0.920	0.357
Religiosity	0.017	0.028	0.600	0.548
Education	-0.041	0.030	-1.342	0.180
Newsfox.exp (t - 1)	0.113	0.045	2.530	0.011

t = 4	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.268	0.079	-3.400	0.001
Pol.ID	0.336	0.078	4.295	0.000
Female	-0.029	0.017	-1.658	0.097
Nonwhite	-0.035	0.019	-1.840	0.066
Age	0.092	0.023	3.981	0.000
Income	0.023	0.035	0.663	0.507
Religiosity	0.097	0.028	3.450	0.001
Education	-0.055	0.033	-1.669	0.095
Newsfox.exp (t - 1)	0.124	0.044	2.830	0.005

t = 5	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.121	0.096	1.261	0.207
Pol.ID	-0.017	0.089	-0.196	0.845
Female	-0.042	0.018	-2.256	0.024
Nonwhite	-0.001	0.021	-0.026	0.979
Age	0.095	0.024	3.982	0.000
Income	0.039	0.038	1.035	0.301
Religiosity	0.054	0.030	1.781	0.075

	Est.	S.E.	z val.	p
Education	-0.054	0.034	-1.598	0.110
Newsfox.exp (t - 1)	0.098	0.044	2.208	0.027

t = 6	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.096	0.090	1.063	0.288
Pol.ID	-0.086	0.090	-0.957	0.339
Female	-0.036	0.017	-2.091	0.037
Nonwhite	-0.003	0.020	-0.159	0.873
Age	0.092	0.021	4.325	0.000
Income	0.099	0.034	2.921	0.003
Religiosity	0.099	0.028	3.529	0.000
Education	-0.063	0.031	-2.056	0.040
Newsfox.exp (t - 1)	0.174	0.047	3.678	0.000

t = 7	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.078	0.071	1.094	0.274
Pol.ID	0.079	0.062	1.264	0.206
Female	-0.031	0.017	-1.889	0.059
Nonwhite	-0.037	0.017	-2.169	0.030
Age	0.142	0.022	6.511	0.000
Income	0.056	0.035	1.629	0.103
Religiosity	0.088	0.027	3.186	0.001
Education	-0.033	0.030	-1.083	0.279
Newsfox.exp (t - 1)	0.047	0.046	1.019	0.308

$X^2(36) = 158.131$, RMSEA = 0.033, 90% CI [0.028, 0.039], SRMR = 0.016, CFI = 0.994, TLI = 0.971

In only wave 4 is lagged political identity (*Pol.ID (t-1)*) significantly related to Fox News exposure, and the coefficient is perversely negative. This indicates that a directionally conservative deviation in wave 3 from one's expected political identity corresponds with less Fox News exposure in wave 4. This is likely a product of two factors. First, Figure 2.3 shows that the change in Fox News exposure between waves 3 and 4 is slightly negative among panelist con-Reps and positive among lib-Dems. Second, panel models produce highly sensitive estimates of intraindividual change in temporally stable variables. That is, small deviations from one's mean political identity are likely to produce outsized effects on Fox News exposure because political identity is relatively stable over time. Thus small, positive movement in an individual's political identity, and overall moderation in selective exposure to Fox News between waves 3 and 4, may account for the negative cross-lagged coefficient in wave 4. Concurrent political identity (*Pol.ID*) is only positive and significant in waves 3 and 4. This set of observations is also notably different from the concurrent x coefficients in the reciprocal model results in Table 2.1, where concurrent Fox News exposure coefficients are highly significant in almost all measurement periods. However in waves 3 and 4, where the concurrent political identity estimate is significant, it appears to be more closely related to Fox News exposure than lagged Fox News exposure. The average size of the autoregressive term when it is statistically different than zero (in all waves except 7) is .123, which reflects lower intraindividual stability over time in Fox News exposure than in political identity. Tables 2.3 and 2.4 provide parameter estimates for models where coefficients are constrained across waves.

Table 2.3: Political identity regressed on Fox News exposure (coefficients constrained across waves)

	Est.	S.E.	z val.	p
Newsfox.exp (t - 1)	0.018	0.007	2.583	0.010
Newsfox.exp	0.037	0.008	4.488	0.000
Female	-0.060	0.008	-7.343	0.000
Nonwhite	-0.106	0.010	-10.900	0.000
Age	-0.001	0.012	-0.070	0.944
Income	0.026	0.018	1.485	0.138
Religiosity	0.174	0.013	13.026	0.000
Education	-0.099	0.015	-6.781	0.000
Pol.ID (t - 1)	0.290	0.026	11.204	0.000

$X^2(71) = 271.494$, RMSEA = 0.03, 90% CI [0.027, 0.034], SRMR = 0.01, CFI = 0.991, TLI = 0.976

Lagged exposure to Fox News is positively related to political identity, indicating evidence of a media effect; individuals that recently watched more Fox News than usual adopt a more conservative/Republican identity than would otherwise be expected, controlling for both one's past political identity as well as other static factors. As expected, concurrent Fox News exposure has a larger effect size, perhaps indicating that more recent changes in news exposure are relatively more influential. Past political identity by far is the strongest indicator of present political identity. Gender, non-white status, religiosity, and education are all significantly related to political identity in the expected direction, while age and income are insignificant.

Table 2.4: Fox News exposure regressed on political identity (coefficients constrained across waves)

	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.031	0.045	-0.703	0.482
Pol.ID	0.081	0.040	2.015	0.044
Female	-0.038	0.012	-3.184	0.001
Nonwhite	-0.031	0.015	-2.076	0.038
Age	0.100	0.015	6.637	0.000
Income	0.052	0.023	2.294	0.022
Religiosity	0.077	0.023	3.379	0.001
Education	-0.057	0.022	-2.641	0.008
Newsfox.exp (t - 1)	0.121	0.031	3.828	0.000

$X^2(71) = 237.936$, RMSEA = 0.028, 90% CI [0.024, 0.032], SRMR = 0.025, CFI = 0.992, TLI = 0.980

Consistent with the unconstrained coefficients model, lagged disturbances in political identity appear to have no bearing on present levels of Fox News exposure. Directionally conservative/Republican deviations from one's concurrent mean political identity are positively correlated with elevated exposure to Fox News. Because concurrent Fox News exposure is also related to a more conservative/Republican identity, there may be evidence of a reinforcing spirals effect. However, because these reciprocal "effects" occur instantaneously (or rather, what must be inferred as instantaneous), it is impossible to know whether there is a causal feedback loop between the two, or whether these changes are products of a third time-variant factor that is unaccounted for in the model.

To summarize the findings: selective exposure to Fox News has grown over time. Lib-Dems were already highly unlikely to say they watch Fox News by the end of 2011, but preference for Fox News among con-Reps grew by around 10% between the end of 2011 and the beginning of 2017.

Using the modeling strategy at hand, I do not find that an individual's increased exposure to Fox News can be explained by past adoption of a more right-leaning identity. That is, I do not find strong evidence for a selection effect among con-Reps. Instead, there is more evidence for a media effect, whereby increased exposure to Fox News produces a stronger con-Rep identity in the future. Chapter 3 picks up with the introduction of my novel measure of ideological news exposure and goes on to perform the same set of hypothesis tests performed in this chapter.

Chapter 3: A new measure of ideological news exposure

Introduction and hypotheses

This chapter assesses the same set of hypotheses detailed in Chapter 3 using The American Panel Survey data and a novel measure of ideological news exposure, which is essentially a combination of exposure frequency and the ideological slant of news outlet respondents report being exposed to. I begin by briefly restating the selection effect, media effect, and reinforcing spiral hypotheses. I then describe the measure introduced – ideological news exposure – and descriptively explore its distribution in the sample and temporal trends. I summarize the results of the hypothesis tests and then conclude with a discussion of the findings from this and the previous chapter.

In looking at the aggregate-level movement of panelists over time, I find that liberal Democrats and conservative Republicans were already ideologically polarized in their news exposure by end of 2011. Consistent with the theoretical expectations of Slater (2015) and the findings of Song and Boomgaarden (2017), periods of increased selective news exposure roughly coincide with the 2012 and 2016 election cycles, and a period of relative stability lies between them.

Over the course of the timeseries, conservative Republicans moved double the distance rightward in their ideological news exposure than liberal Democrats moved leftward. I find indirect evidence that this asymmetric polarization is in part due to the ideological asymmetry in the national news media environment; there are a fewer number of right-of-center news outlets than left-of-center outlets, and those outlets that are on the right are, on average, more ideologically extreme than left-leaning outlets. Thus conservatives that wish to engage with mainstream conservative media are more constrained in their choices, and most available choices are highly conservative.

The hypotheses tested here as as follows: the selection effect hypothesis predicts that a more extreme or stronger political identity produces increased exposure to more ideologically congruent news media, and the media effect hypothesis predicts just the inverse relationship. The reinforcing spirals hypothesis predicts – under hightened political conditions – a positive feedback loop between identity and ideological news exposure.

Using the same modeling strategy identified in Chapter 2, I again find only support for the media effect hypothesis: more conservative (liberal) ideological news exposure at a previous point in time predicts a stronger right-leaning (left-leaning) political identity in the measured time interval. I find not only that the ideological news exposure measure introduced here produces results consistent with those produced with the Fox News exposure measure in Chapter 2, but also that the estimated coefficients are larger and more signfiicant, suggesting that the ideological news exposure measure is empirically valid and perhaps a superior measure of the underlying construct.

News ideology

The study described in Chapters 2 and 3 ultimately aims to assess polarization of ideological news exposure between political groups and to evaluate the relationship between these two constructs. Thus I move from using a measure that mostly captures whether or not someone watches programming from a particular cable news outlet to evaluating the degree to which one's exposure to a particular ideological slant of news is related to their political identity. Fox News exposure is simple and transparent, but there are limitations in using it as a measure of selective news exposure; a liberal Democrat who was already not watching Fox News cannot watch Fox News less if they adopt a stronger lib-Dem identity; there is a floor effect for lib-Dems that prohibits me from capturing the dynamics between their political identity and their selective news exposure when only using exposure Fox News as a stand-in for all news. There is a similar ceiling effect for con-Reps that already report to watch Fox News every day. I replicate the above analyses using a novel measure of ideological news exposure. This measure provides greater nuance and scope that should reduce measurement error and the likelihood of floor and ceiling effects. A similar set of findings should additionally help validate the measure; that is, if I again find evidence of a media effect and absence of a selection effect, there should be greater confidence that the measure captures ideological news exposure.

I first quantify the ideology of news outlets that respondents report to get their news through, using a set of four external resources that estimate the ideological slant of individual news outlets and programs. I term the measure *news ideology*. I then again move to incorporate the frequency with which individuals attend to the news, resulting in a novel measure of *ideological news exposure.* Next, I provide a set of figures that track ideological news exposure over time among a set of TAPS panelists. Finally, I analyze the relationship between ideological news exposure and political identity in a manner identical to the previous section.

Beyond the binary of preference for Fox News, it is possible to model the relationship between political groups' exposure to a liberal, neutral, or conservative news source and their tendency toward political polarization. Some previous selective exposure studies have done just that (e.g. Bakshy, Messing, and Adamic 2015). However, collapsing a rich spectrum of ideological news content into three categories artificially limits information about selective news exposure and may produce imprecise or biased estimates. While both The Washington Post and The New York Times are frequently cited as liberal newspapers, the latter is consistently found to exhibit a more liberal slant in its content (Budak, Goel, and Rao 2016; Gentzkow and Shapiro 2010; Groseclose and Milyo 2005; Otero 2021). Related, while Fox News and CNN are often touted as polar ideological opposites in cable news, Fox News is often found to be more conservative in its content than CNN is found to be liberal, and Fox News viewership is found to be associated with more conservative policy opinions than CNN is associated with liberal opinions (Feldman et al. 2012; Hart 2008; Zúñiga, Correa, and Valenzuela 2012). Thus, in attempting to understand the factors that lead to, or are consequences of, selective exposure, it may be important to quantify and capture the full ideological space in which these news sources reside.

When TAPS asks "Where do you get MOST of your news about national and international issues? [Family members, friends, or workmates; television; radio; newspapers; magazines; internet; other]," respondents are allowed to choose up to two mediums. If respondents choose "television," they are asked for their "most common source of news [ABC, CBS, NBC, CNN, MSNBC, Fox News, local station, and other]." Respondents that choose radio are asked to then choose between talk radio, public radio, music radio, or "other" as their most common source of radio news. Respondents that choose newspaper are asked to choose between USA today, New York Times, Wall Street Journal, and "other newspapers."

I am able to measure a respondent's news ideology if in a given wave if they select at least one of "television," "radio," or "newspapers" among the two possible responses in the medium question, then select ABC, CBS, NBC, CNN, MSNBC, Fox News, talk radio, public radio, USA today, New York Times, or Wall Street Journal among the news source questions. Respondents that select "other" in either the medium or news source questions are given the option to write in answers. I extract other news outlets from these write-in responses to include among the list of news outlets for which ideology can be measured: Al Jazeera, BBC, CNBC, Los Angeles Times, and Washington Post.

News ideology cannot be measured in a given wave for respondents that choose family members, friends, or workmates, internet, or other (and do not provide an extractable text response) to both instances of the news medium question. That is, for respondents that select “friends” and “internet” when asked where they get most of their news, there is no useable information from which to measure their news ideology for that wave. The same is true for respondents that select “friends” and “television” then select “local station” or “other” in the followup television source question. However, if a respondent selects “friends” and “television” when asked where they get most of their news, then selects “ABC” in the followup television question, their news ideology for that wave is assigned to be the ideology of ABC News. Likewise, if a respondent selects “television” and “newspaper” then selects “ABC” and “New York Times” in the two respective followup news source questions, their news ideology is measured as the average ideology of those two news sources.

In the 9,387 total observations to the set of items pertaining to preferred news mediums and sources over the seven waves the question was asked, news ideology can be measured in 76% of observartions. Respondents for which I am unable to capture news ideology are not demographically or politically distinct from uncaptured respondents. That is, the 24% of observations missing due to responses from which news ideology cannot be measured should not seriously bias the temporal trends in the following figures or the parameter estimates from the models that follow.

To assign numeric values for the ideologies of each named news outlets, I average the estimates provided by four external sources: Ad Fontes Media (AFM), AllSides, Media Bias Fact Check (MBFC), and Ribeiro et al.'s (2018) media bias monitor (MBM). All four sources rate news outlets on a left-right/conservative-liberal scale according to their own content- or audience-based methodologies.[9] Each has been previously used in applied scholarly work pertaining to news media bias (Badawy, Lerman, and Ferrara 2019; Bovet and Makse 2019; Zhao et al. 2020).

I first rescale the sources' scales to run from -1 (most liberal/left in a respective source's database of news outlets) to +1 (most conservative/right), with 0 retained as ideologically neutral. Table 3.1 provides the Pearson's correlation coefficients for the ideology scores that each source assigns to the outlets named in TAPS. The sources' assessments of news outlets' ideologies are highly correlated, and averaging their scores across outlets should reduce measurement error toward reaching a true ideological slant for each outlet.

Table 3.1: news outlet ideology source correlations	Media Bias Monitor	Ad Fontes Media	AllSides
Media Bias Monitor			
Ad Fontes Media	0.85		
AllSides	0.76	0.94	
Media Bias Fact Check	0.81	0.94	0.91

[9] While Ribeiro et al.'s (2018) media bias monitor and Ad Fontes Media's media bias scale assign numeric values to the ideological slant of news outlets, Allsides and Media Bias Fact Check assign qualitative labels to news outlets: left, center-left, neutral, center-right, and right. I code these scales numerically, so that left = -1, center-left = -.5, neutral = 0, center-right = .5, and right = 1.

Figure 3.1 illustrates the ideological score for each news source, as the average taken by the aforementioned media bias estimator sources.[10] There is asymmetry with regard to the number of liberal and conservative news outlets named by TAPS and this asymmetry in the survey sample reflects the real world, where major and mainstream liberally slanted news outlets in the U.S outnumber conservative outlets (Budak, Goel, and Rao 2016; Mitchell et al. 2014; Ribeiro et al. 2018). Ideological asymmetry is also present; Fox News and talk news are both more conservative than the most liberal news sources. Additionally, while there exist many "center-left" news sources according to Allsides and MBFC, only the Wall Street Journal is classified as "center-right."

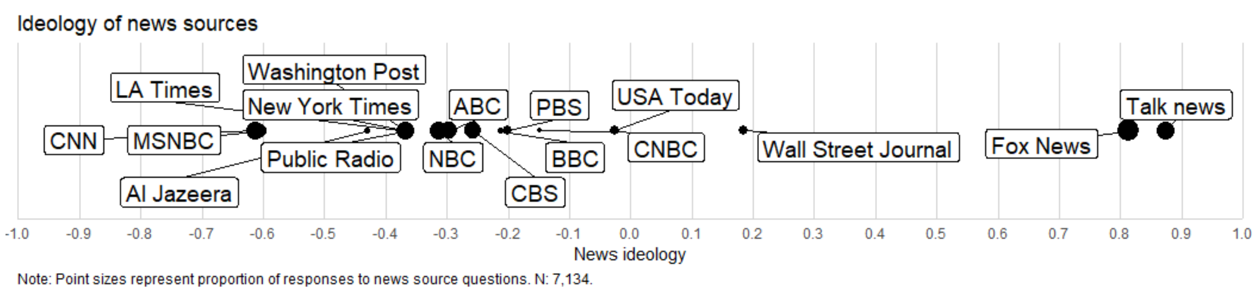

FIG. 3.1

This means that conservatives and Republicans that seek to attend to like-minded news are far more limited in their options than liberals and Democrats. It also means that con-Reps must often an take an ideological leap if they want to move to attend to more conservative news. That is, the move from CNBC (the most ideologically neutral television news outlet in the sample at -.14) to CNN (the most liberal at -.61) is .47 units. The move from CNBC to Fox News (the most conservative television outlet at .81) is .95, about double the ideological distance.

[10] To assign ideological scores for "talk radio" as a news source, I average the ideological scores for the top-two news programs during the 2010s: The Rush Limbaugh Show and The Sean Hannity Show (Top Talk Audiences : TALKERS.COM n.d.). "Public radio" is assigned the score of National Public Radio, which, while it is a network and not a program, Allsides, MBFC, MBM, and AFM rate as a whole.

Figure 3.2 gives some preliminary insight into the effect of this news outlet asymmetry. Con-Reps are noticeably more “cross-pressured” than lib-Dems in their ideological news preferences. While a higher proportion of con-Reps sit at the ideological extreme, there is also a considerable proportion of them that report getting most of their news from NBC, BBC, or public radio. In contrast, very few lib-Dems report getting their news from Fox or talk radio, and the large majority report getting their news from liberal sources.

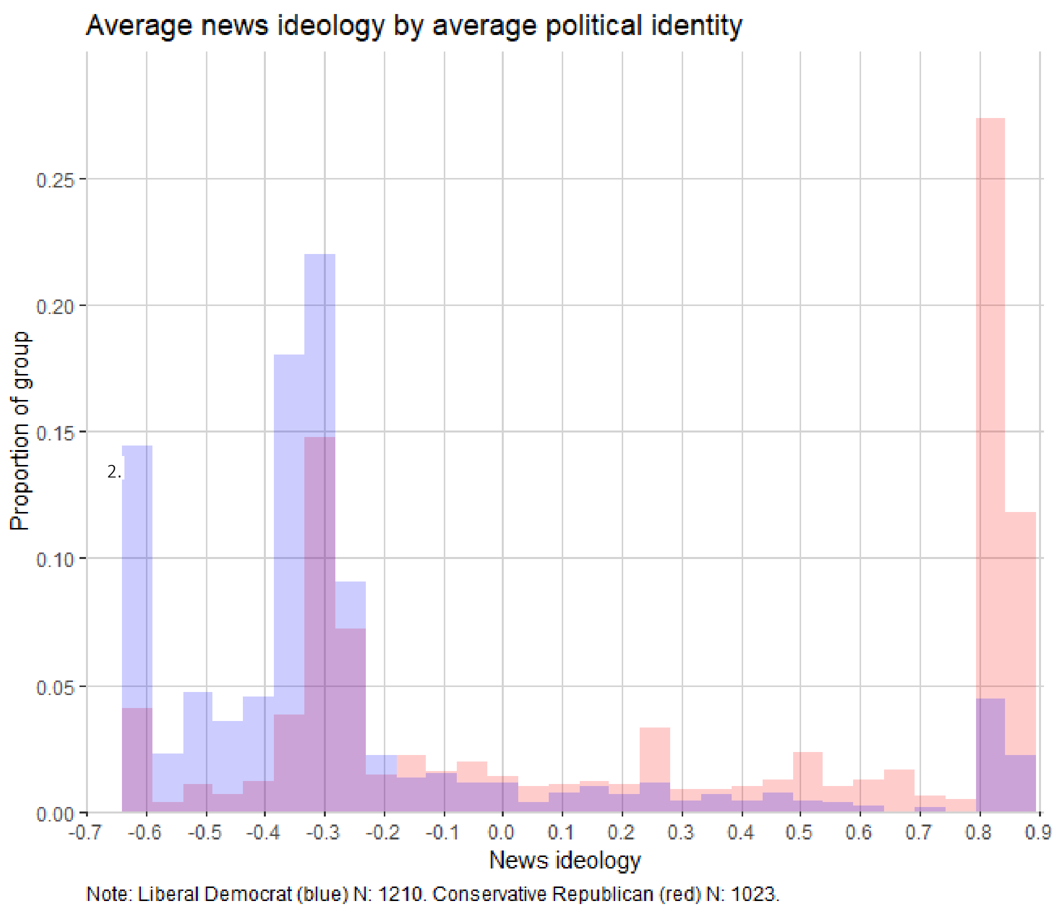

FIG. 3.2

Figure 3.3 shows this distribution over time for all observed responses in each wave. As with the previous times series figures, most of the polarization appears to occur between the October 2015 and February 2017 waves, though substantial rightward movement occurred for con-Reps between June 2012 and June 2013. Moreover, con-Reps moved more than double the distance in the conservative direction than lib-Dems moved in the liberal direction over the course of the timeseries. Lib-Dems moved from a position of -.23 to -.30, a difference of -.07. Con-Reps moved from a position of .21 to .38, a difference of .17.

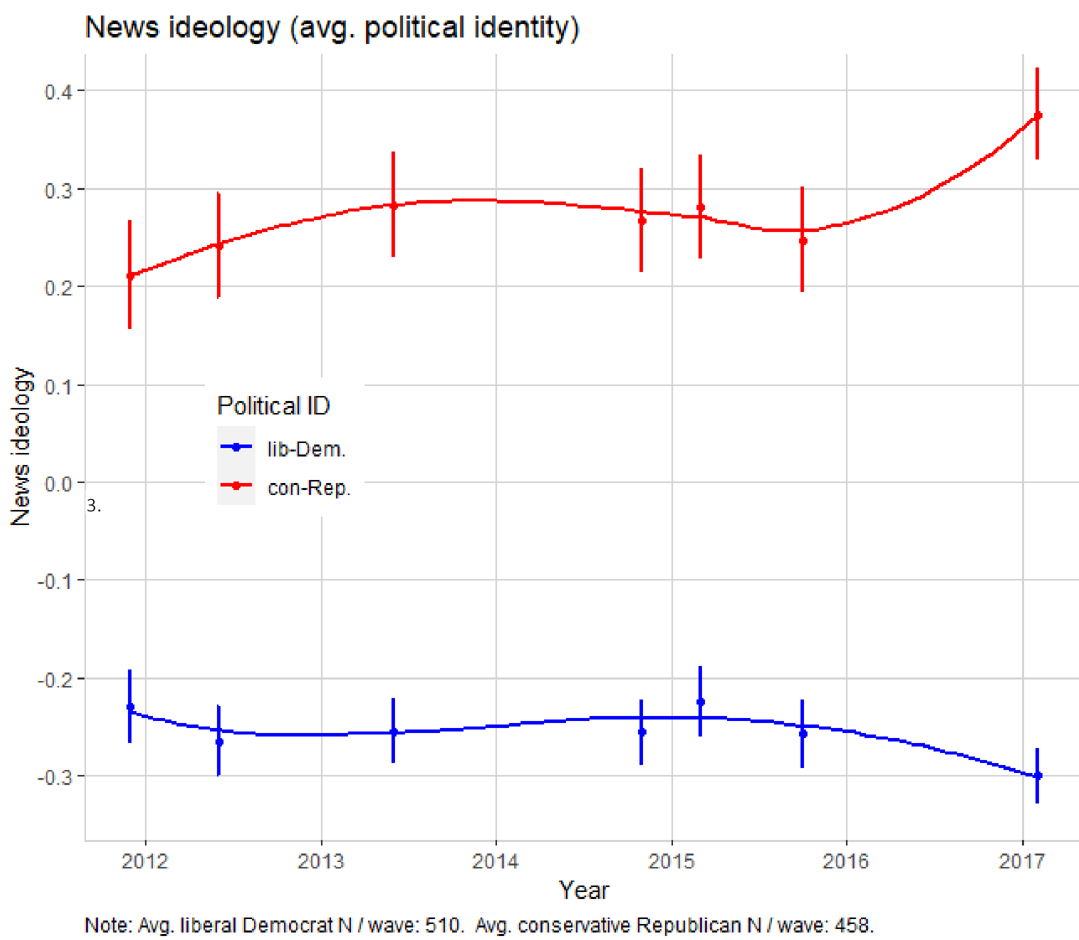

FIG. 3.3

I do not have access to data on the ideology of news outlets before 2020, when I collected this data, so I am unable to account for changes over time in the ideological slant of the news outlets themselves. I necessarily assume them to be constant over time, which many certainly are not. However, I believe that this limitation hinders my ability to find significant results, relative to the alternative where I am able to account for these changes. It's likely that the news sources under investigation have polarized in the ideological slant of their news content over this period; liberal sources such as CNN and MSNBC have become more liberal and Fox News and conservative talk news have become more conservative (Martin and Yurukoglu 2017). Thus by holding news ideology constant across time, changes only reflect changes in respondents' exposure to these sources, and not growing extremity in the news outlets themselves.

Figure 3.4 illustrates the same temporal trends among only panelists and shows that news ideology stayed fairly constant throughout, with most of the divergence between political groups occurring between the last two waves.

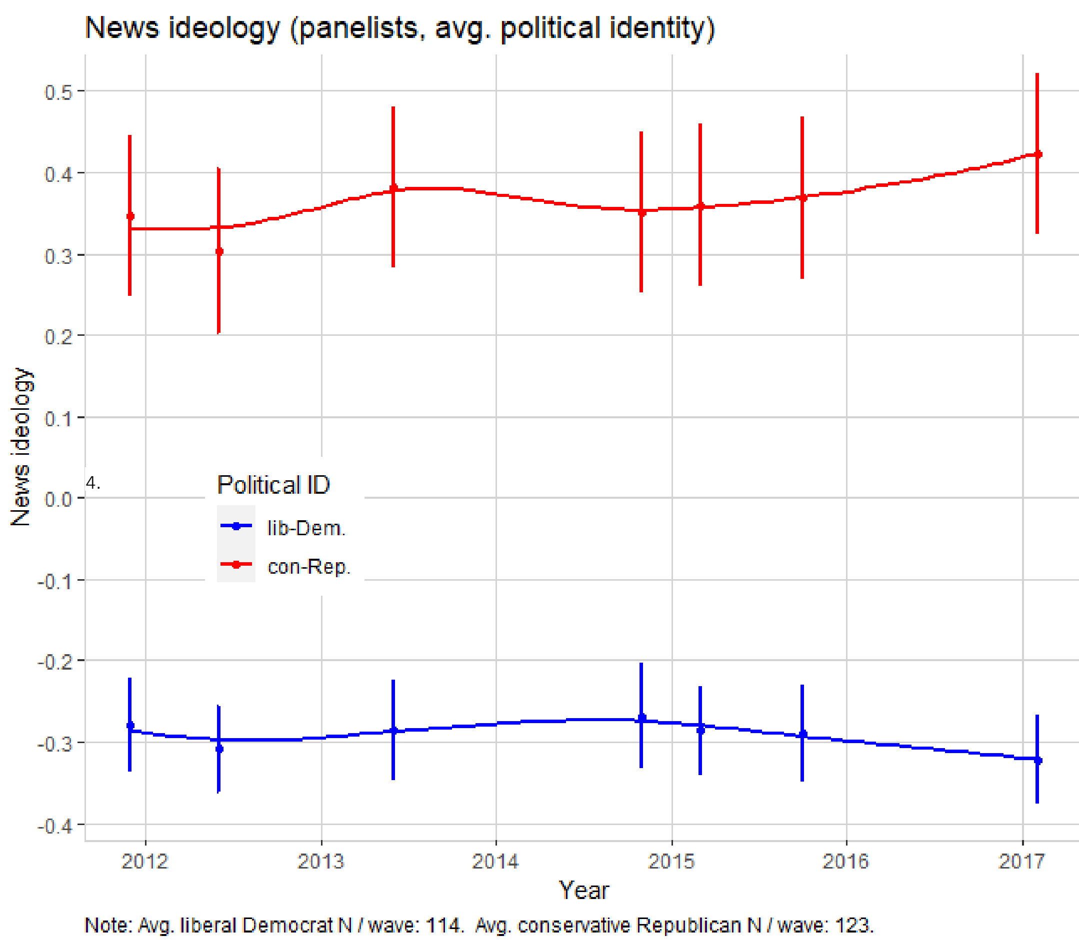

FIG. 3.4

News attention

The last step in formulating my measure of *ideological news exposure* is to incorporate respondents' frequency of news exposure in a manner identical to what was done for Fox News exposure. Figure 3.5 shows that, relative to the distribution of news ideology in Figure 3.2, incorporation of news frequency modestly increases variation and reduces the bimodality of the distribution.

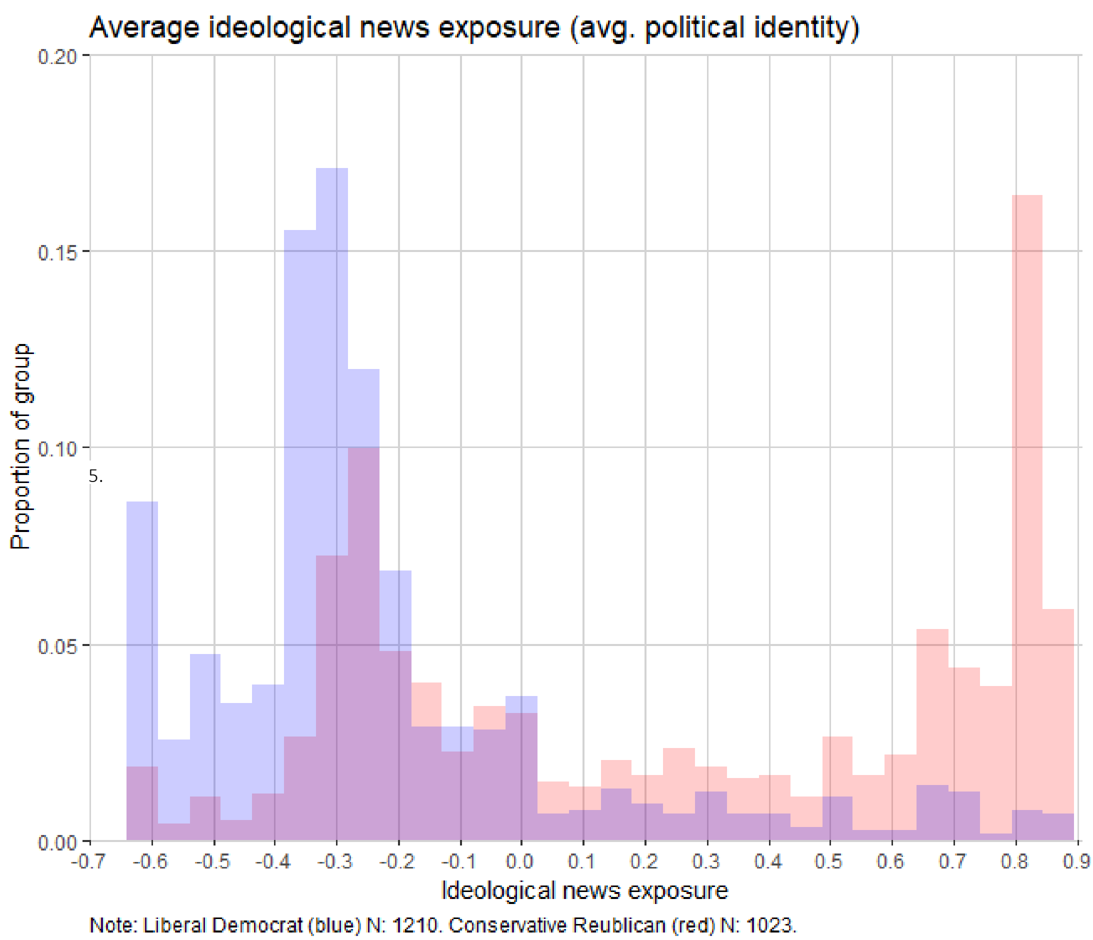

FIG. 3.5

Figure 3.6 shows the temporal trend for ideological news exposure between con-Reps and lib-Dems. The trends are almost identical to those observed in Figure 3.3 (news ideology).

Lib-Dems moved from a mean of -.22 in December 2011 to a mean of -.29 in February 2017, a change of -.07. Con-Reps moved from .19 to .36, a change of .17.

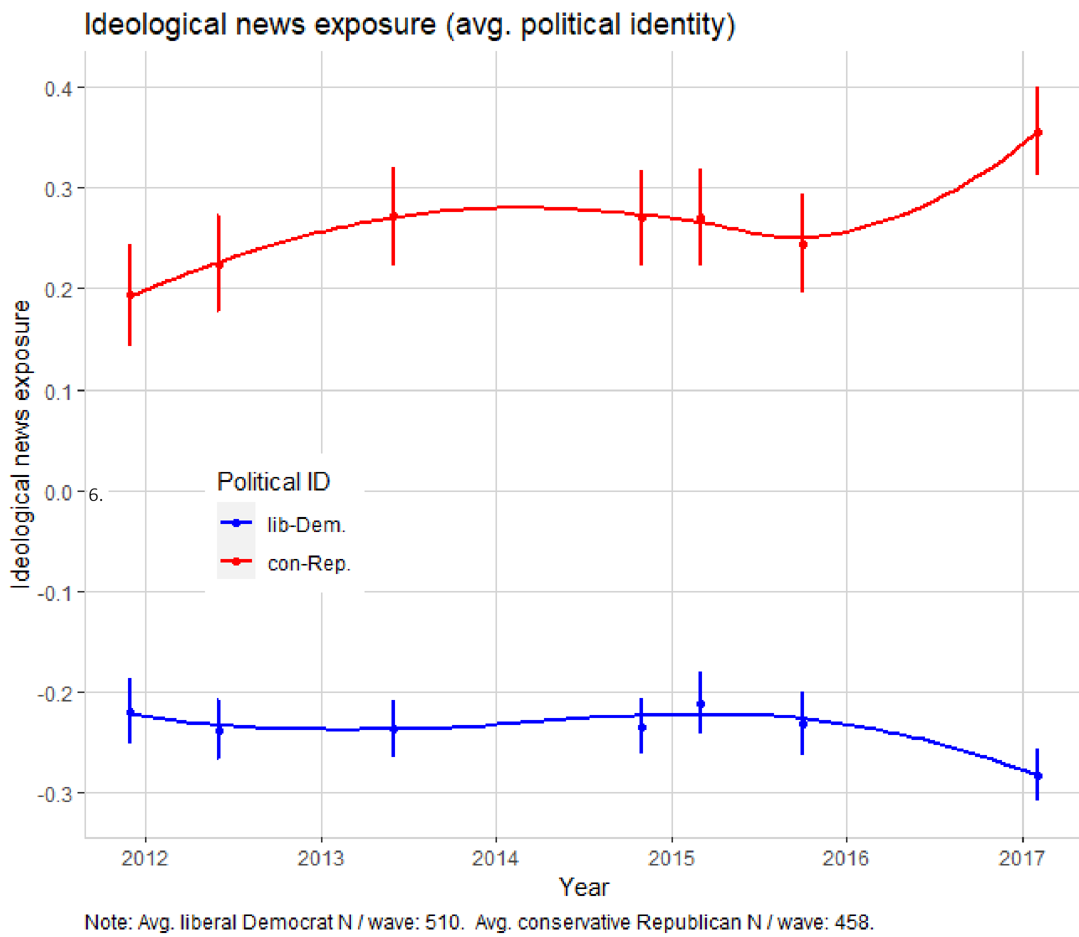

FIG. 3.6

Figure 3.7 illustrates the trends among panelists, and shows a slightly attenuated set of trends relative to those observed in Figure 3.4. Over the timeseries, lib-Dems' mean estimates move from -.27 to -.31, a change of -.04. Con-Reps move from .34 to .42, a change of .08. Consistent with selective exposure to Fox News, movement among con-Reps occurs primarily between waves 2 and 3 and between waves 6 and 7. In contrast, lib-Dems moderate their selective exposure through the end of 2014 and only then begin moving leftward through the end of the timeseries.

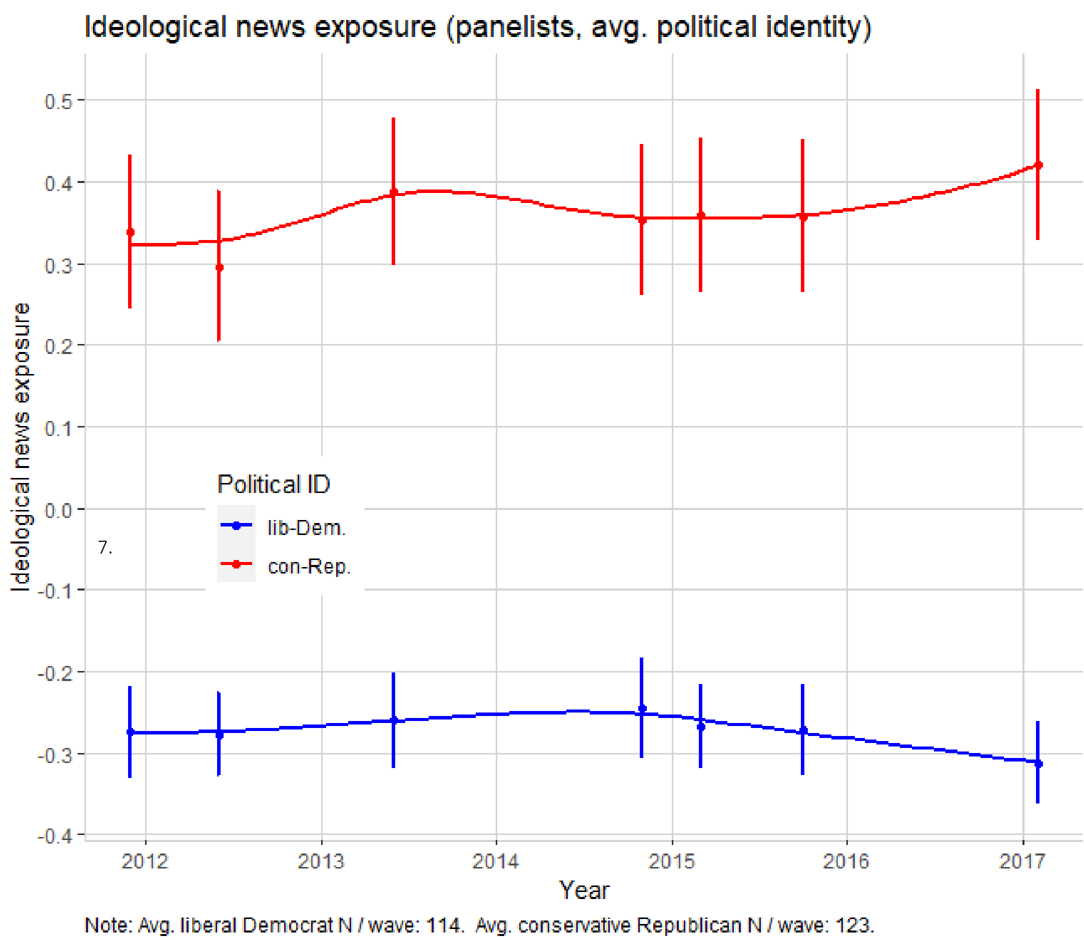

FIG. 3.7

Overall, polarization over time in self-reported exposure to ideologically congruent news is evident and has grown over time. However, a large portion of the divergence observed occurred between waves 2 and 3 (only among con-Reps), June 2012 – June 2013, and the last 2 waves, October 2015 – February 2017, indicating that periods of increased selectivity may be products of politically charged contexts, such as presidential elections. Additionally, these findings corroborate with those of Song and Boomgaarden (2017), who test Slater's (2007, 2015) reinforcing spirals model and find that national elections trigger periods of increased news selectivity.

Results

I next estimate a set of ML-SEM models in a manner identical to the Fox News exposure models in Chapter 2.[11] Table 3.2 reports results for political identity regressed on ideological news exposure, with coefficients unconstrained to be equal across time.

Table 3.2: Political identity regressed on ideological news exposure (coefficients unconstrained across waves)

t = 2	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.025	0.023	1.123	0.261
Ideo.News.Exp	0.120	0.027	4.420	0.000
Female	-0.041	0.010	-4.246	0.000
Nonwhite	-0.071	0.012	-5.815	0.000
Age	0.000	0.012	0.030	0.976
Income	0.031	0.019	1.661	0.097
Religiosity	0.116	0.016	7.111	0.000
Education	-0.076	0.017	-4.457	0.000
Pol.ID (t - 1)	0.437	0.041	10.657	0.000

t = 3	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.065	0.021	3.188	0.001
Ideo.News.Exp	0.125	0.023	5.396	0.000
Female	-0.039	0.009	-4.236	0.000
Nonwhite	-0.083	0.011	-7.395	0.000
Age	-0.001	0.011	-0.068	0.946
Income	0.036	0.018	1.973	0.049
Religiosity	0.143	0.016	8.905	0.000
Education	-0.068	0.017	-4.096	0.000
Pol.ID (t - 1)	0.343	0.037	9.300	0.000

t = 4	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.066	0.020	3.268	0.001
Ideo.News.Exp	0.147	0.022	6.606	0.000
Female	-0.044	0.009	-4.960	0.000
Nonwhite	-0.072	0.011	-6.430	0.000
Age	0.004	0.011	0.346	0.729
Income	0.020	0.017	1.195	0.232
Religiosity	0.133	0.017	8.003	0.000

[11] See the "Panel analysis methodology" section of Chapter 2 for a detailed explanation of the modeling strategy and rationale for the covariates included.

	Est.	S.E.	z val.	p
Education	-0.093	0.015	-6.037	0.000
Pol.ID (t - 1)	0.364	0.041	8.969	0.000

t = 5	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.059	0.025	2.393	0.017
Ideo.News.Exp	0.132	0.025	5.247	0.000
Female	-0.040	0.009	-4.570	0.000
Nonwhite	-0.088	0.011	-7.655	0.000
Age	0.012	0.011	1.142	0.253
Income	0.020	0.017	1.162	0.245
Religiosity	0.122	0.017	7.131	0.000
Education	-0.053	0.017	-3.160	0.002
Pol.ID (t - 1)	0.392	0.042	9.359	0.000

t = 6	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.041	0.028	1.469	0.142
Ideo.News.Exp	0.184	0.034	5.380	0.000
Female	-0.035	0.009	-3.678	0.000
Nonwhite	-0.085	0.012	-7.216	0.000
Age	0.009	0.011	0.796	0.426
Income	0.007	0.018	0.382	0.703
Religiosity	0.114	0.018	6.523	0.000
Education	-0.073	0.017	-4.243	0.000
Pol.ID (t - 1)	0.380	0.041	9.279	0.000

t = 7	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.225	0.031	7.359	0.000
Ideo.News.Exp	0.012	0.022	0.551	0.582
Female	-0.049	0.010	-4.948	0.000
Nonwhite	-0.097	0.013	-7.688	0.000
Age	0.024	0.012	1.908	0.056
Income	-0.004	0.020	-0.229	0.819
Religiosity	0.130	0.019	7.000	0.000
Education	-0.077	0.019	-4.141	0.000
Pol.ID (t - 1)	0.382	0.046	8.248	0.000

$X^2(26) = 152.036$, RMSEA = 0.040, 90% CI [0.034, 0.046], SRMR = 0.008, CFI = 0.994, TLI = 0.960

In waves 3, 4, 5, and 7, the cross-lagged term (i.e. ideological news exposure or *Ideo.News.Exp*) is positive and significantly related to political identity. The average size of the coefficient in these waves is .104. Similar to the estimates using the Fox News exposure measure, in all waves but the last the concurrent x term is positive and significant. The average coefficient of concurrent ideological news exposure is .142. These are notable increases in magnitude over the average Fox News exposure coefficients observed in Table 2.1, which are .087 and .099, respectively. Lagged Fox News exposure is additionally insignificant in wave 4, where lagged ideological news exposure is significant. Table 3.3 reports the results from the reciprocal relationship model.

Table 3.3: Ideological news exposure regressed on political identity (coefficients unconstrained across waves)

t = 2	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.065	0.078	-0.834	0.404
Pol.ID	0.134	0.082	1.633	0.103
Female	-0.064	0.018	-3.632	0.000
Nonwhite	-0.074	0.023	-3.194	0.001
Age	-0.040	0.022	-1.849	0.064
Income	0.012	0.033	0.366	0.715
Religiosity	0.097	0.031	3.186	0.001
Education	-0.059	0.032	-1.862	0.063
Ideo.News.Exp (t - 1)	0.094	0.044	2.106	0.035
t = 3	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.042	0.070	0.600	0.548
Pol.ID	0.029	0.094	0.308	0.758
Female	-0.072	0.017	-4.185	0.000
Nonwhite	-0.037	0.022	-1.671	0.095
Age	-0.012	0.021	-0.548	0.584
Income	0.040	0.034	1.196	0.232
Religiosity	0.090	0.032	2.796	0.005
Education	-0.099	0.031	-3.227	0.001
Ideo.News.Exp (t - 1)	0.148	0.047	3.155	0.002
t = 4	Est.	S.E.	z val.	p

Pol.ID (t - 1)	-0.191	0.074	-2.579	0.010
Pol.ID	0.218	0.085	2.571	0.010
Female	-0.045	0.017	-2.609	0.009
Nonwhite	-0.040	0.021	-1.917	0.055
Age	-0.004	0.020	-0.197	0.844
Income	0.028	0.033	0.854	0.393
Religiosity	0.139	0.032	4.330	0.000
Education	-0.043	0.030	-1.428	0.153
Ideo.News.Exp (t - 1)	0.145	0.043	3.372	0.001

t = 5	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.034	0.095	-0.359	0.720
Pol.ID	0.099	0.079	1.258	0.208
Female	-0.049	0.017	-2.845	0.004
Nonwhite	-0.031	0.021	-1.443	0.149
Age	-0.016	0.021	-0.751	0.453
Income	0.008	0.033	0.251	0.802
Religiosity	0.102	0.033	3.034	0.002
Education	-0.082	0.031	-2.677	0.007
Ideo.News.Exp (t - 1)	0.141	0.048	2.920	0.003

t = 6	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.283	0.110	2.561	0.010
Pol.ID	-0.318	0.147	-2.159	0.031
Female	-0.044	0.017	-2.640	0.008
Nonwhite	-0.043	0.021	-2.017	0.044
Age	-0.011	0.020	-0.522	0.601
Income	0.058	0.032	1.811	0.070
Religiosity	0.132	0.031	4.230	0.000
Education	-0.066	0.028	-2.338	0.019
Ideo.News.Exp (t - 1)	0.239	0.049	4.861	0.000

t = 7	Est.	S.E.	z val.	p
Pol.ID (t - 1)	-0.041	0.142	-0.289	0.773
Pol.ID	0.276	0.083	3.320	0.001
Female	-0.045	0.017	-2.626	0.009
Nonwhite	-0.049	0.021	-2.308	0.021
Age	-0.023	0.021	-1.111	0.266
Income	0.020	0.032	0.636	0.525
Religiosity	0.084	0.032	2.607	0.009
Education	-0.045	0.030	-1.500	0.134
Ideo.News.Exp (t - 1)	0.025	0.052	0.472	0.637

$X^2(26) = 101.832$, RMSEA = 0.031, 90% CI [0.025, 0.037], SRMR = 0.012, CFI = 0.997, TLI = 0.976

Similar to the Fox News exposure model results in Table 2.2, Table 3.3 reports that the cross-lagged coefficient is negative and significant in wave 4. However it is positive and significant in wave 6, indicating that directionally conservative/Republican deviations in wave 5 from an individual's mean political identity corresponds with above-average increased conservative news exposure in wave 6. This is what is expected from a selection effect. However, concurrent political identity is negatively related to ideological news exposure in wave 6, where in waves 4 and 7 it is significant and positive. Tables 3.4 and 3.5 report results for models where coefficients are constrained across waves.

Table 3.4: Political identity regressed on ideological news exposure (coefficients constrained across waves)

	Est.	S.E.	z val.	p
Ideo.News.Exp (t - 1)	0.036	0.010	3.477	0.001
Ideo.News.Exp	0.059	0.011	5.381	0.000
Female	-0.056	0.008	-6.923	0.000
Nonwhite	-0.101	0.010	-10.455	0.000
Age	0.007	0.011	0.626	0.531
Income	0.026	0.017	1.577	0.115
Religiosity	0.165	0.014	12.107	0.000
Education	-0.095	0.014	-6.649	0.000
Pol.ID (t - 1)	0.294	0.027	10.902	0.000

$X^2(71) = 268.913$, RMSEA = 0.030, 90% CI [0.026, 0.034], SRMR = 0.012, CFI = 0.991, TLI = 0.977

Again, similar to the Fox News exposure coefficients in Table 2.3, the cross-lagged and cross-concurrent effects are both significant and positive in the model results shown in Table 3.4.

However, the effect size for each is about double that of the Fox News exposure terms, which are .018 and .037, respectively. Thus it appears likely that the extra variation in ideological news exposure captured by this variable produces a stronger predictor of change in one's political identity.

Table 3.5: Ideological news exposure regressed on political identity (coefficients constrained across waves)

	Est.	S.E.	z val.	p
Pol.ID (t - 1)	0.053	0.041	1.286	0.198
Pol.ID	0.138	0.040	3.480	0.001
Female	-0.043	0.012	-3.723	0.000
Nonwhite	-0.028	0.015	-1.905	0.057
Age	-0.015	0.014	-1.090	0.276
Income	0.019	0.021	0.911	0.363
Religiosity	0.079	0.022	3.539	0.000
Education	-0.047	0.020	-2.411	0.016
Ideo.News.Exp (t - 1)	0.141	0.037	3.824	0.000

$X^2(71) = 216.909$, RMSEA = 0.026, 90% CI [0.022, 0.030], SRMR = 0.024, CFI = 0.994, TLI = 0.983

For the last time, similar to Table 2.4, the cross-lagged coefficient observed in Table 3.5 is insignificant and the concurrent x coefficient is significant and positive. The size of the latter coefficient is considerably larger than it is using the Fox News measure in Table 2.4 (by about 60%). Again, the concurrent coefficient indicates that a more conservative/Republican-than-average political identity corresponds with a more conservative-than-average news exposure at the same time, but it is impossible to infer any causal relationship between these concurrent variables.

Discussion

Before moving to synthesize the findings presented between Chapters 2 and 3, there are two significant limitations of the findings presented in this study that should be highlighted first. The first limitation concerns generalizability: these findings cannot be safely generalized to news mediums beyond television, radio, or newspapers, nor beyond national and mainstream news outlets. There may be aspects of, for instance, internet news that are unique and influence political identity in a manner not found here (Kaid 2003). Second, the effect of ideological news exposure, while significant, is very small; in the time-constrained model, a one-unit deviation in selective news exposure – that is, moving from exposure to MSNBC everyday to exposure to talk news everyday – produces a 3.6% change toward a more right-wing identity. This small effect size may reflect the high stability of political identity and/or the ineffectual nature of mass legacy news media. Because news exposure and political identity are the only variables under consideration in this study, it is impossible to disentangle which is more likely.

The small and insignificant cross-lagged effects of ideological news exposure and political identity, respectively, could also be a product of the time constraints of the measurement periods. The average time between measurement periods is 10 months. Given this extreme length and consistent documentations of instantaneous and short-lived media and information effects (Chong and Druckman 2010; Druckman, Fein, and Leeper 2012; Hill et al. 2013), it is surprising that I any evidence for a lagged effect of news exposure on identity. Further, it is not at all surprising that I find very little evidence of an effect of lagged political identity on news exposure. If either selection or media effects tend to occur and dissipate quickly, it may be that the concurrent x coefficients reflect these effects. Thus these generally significant and relatively large cross-concurrent coefficients may be capturing a reinforcing spirals effect.

However, it is also possible that contemporaneously measured changes in ideological news exposure and political identity are caused by a set of variables unobserved in this analysis, so that the estimates are biased by omitted variables. Additionally, the modeling strategy used in this study aims to provide a tough test of the reinforcing spirals hypothesis, and so I consider the lagged coefficients to be the most consequential in testing this hypothesis. For these reasons, I move forward with the discussion concerning the cross-lagged effects and not the cross-concurrent effects.

On the whole, I present a set of results nearly identical to that presented in Chapter 2. I find that liberal Democrats and conservative Republicans are highly selective in the news they attend to; most members of these groups attend to likeminded news outlets. The polarization of news exposure has grown between December 2011 and February 2017, with most of the growth occurring around the time of presidential elections.

At least at the individual level, I do not find that change in selectivity of ideologically congruent news is a product of change in one's political identity. As individuals move toward either pole of the political identity spectrum, their selectivity to likeminded news does not grow in turn. Instead, I find that increased levels of selectivity in the past produces more extreme political identities in the future. That is, I only find strong evidence for a media effect, whereby attending to likeminded news leads one to view themselves as a stronger partisan and ideologue. Thus I do not find strong evidence in support of a reinforcing spirals model, as only one piece of the model is present.

Absence of a selection effect could be a function of several factors, such as the time period under observation. Democrats and Republicans were already highly polarized in their news consumption by the end of 2011. Ideological news exposure may have been a function of political identity prior to this period and plateaued due to the upper ideological bounds of available news sources.

Or, more likely, individuals' increasingly extreme identities lead to greater exposure to more ideologically extreme news content over time, but through increasingly ideologically slanted news content from the same sources. For instance, it may be that a stronger rightwing identity contributes to greater exposure to more conservative news over time simply because Fox News has grown more conservative in its content over time (Martin and Yurukoglu 2017). Individuals' media consumption habits could remain the same while becoming exposed to more ideologically extreme content. If this is the case, then the measures used in the panel study would not capture this selection effect. However, it is unclear as to whether "passive" selection of more ideologically extreme news qualifies as a selection effect in the way that scholars conceptualize it; individuals would not be seeking out more politically congruent information so much as maintaining their preferred news sources. On the contrary, individuals would be required to actively break their habits in order to maintain or decrease their exposure to increasingly ideologically extreme content. The interaction between the changing ideological slant of news outlets and consumers' exposure preferences should be given more focus in future work.

Last, absence of a selection effect may be due to TAPS' inability to capture preferred internet news sources. It could be that individuals seeking more ideologically extreme news to fit their political identity may be opting out of legacy mediums in favor of internet news. The internet facilitates a more fractionalized news environment that can cater to more extreme tastes than legacy mediums can typically afford to do (Garrett 2009; Kaid 2003). Because TAPS does not ask respondents where they get their internet news from, I am unable to track internet news ideology. Future surveys should incorporate higher resolution internet news preference questions as a continually growing proportion of the public gets their news from the internet.

Moving to my methodological contribution, the similar findings presented between Chapters 2 and 3 indicate that the novel measure of ideological news exposure is an empirically valid means of evaluating the degree to which an individual is exposed to a particular ideology of news media, at least as it relates to one's political identity. Relative to the models estimated with the Fox News exposure variable, the higher magnitude and more significant coefficients in the media effects models (Tables 3.2 and 3.4) provided in this chapter indicate increased certainty that such an effect is actually occurring. Further, the measure adds conceptual nuance that previous measures have not explored: the notion of quantifying the ideological slant of a news organization or program is not new, but simultaneously taking into account the frequency to which one exposed to ideologically slanted news media appears to be highly uncommon.   Future research may go even farther: in estimating the relationship between news exposure and topical political opinions, one may account for how well covered a given topic is in a given time period, with the idea that increased topic coverage should produce greater opinion change. Empirical questions remain as to whether exposure to more ideologically slanted news outlets produce greater opinion change given similar levels of issue coverage.

Future studies should also explore among which segments of the public these relationships may be stronger or weaker. For instance, there may be greater selective exposure or media effects among younger individuals whose news preferences and political identities may be less crystalized and stable than older individuals. Future work also ought to investigate a more diverse array of the factors that ideological news exposure may influence. In particular, no studies have assessed the relationship between more ideologically extreme news exposure and political behavior. Does increased exposure to Fox News or CNN produce a greater likelihood to turn out, or to volunteer with political campaigns? Or, perhaps, does more extreme news discourage consumers from participating in politics? These questions remain to be answered, and in an era of continued polarization, they seem increasingly relevant.

Chapter 4: An experiment on motivated reasoning and evaluations of arguments about climate change

Introduction

The research presented in Chapters 4 and 5 assesses the extent to which people are directionally or accuracy motivated when considering information and forming opinions about climate change. I investigate as to whether individuals adopt opinions about climate change and evaluations of messages by the actors involved in line with their prior opinions on the topic or their partisanship – or – do they aim to form an accurate set of opinions and evaluations in line with information provided by expert sources, even if this information goes against their own party's position?

The public is consistently found to follow the political opinions of copartisan elites (Bullock 2011; Bullock et al. 2013; Lenz 2013), even when those opinions are factually incorrect and subsequently corrected (Ecker and Ang 2019; Flynn, Nyhan, and Reifler 2017; Kahan 2015; Nyhan and Reifler 2010). Maintenance of factually incorrect beliefs in the face of contrary expert evidence is especially prominent on the topic of climate change, where Republicans follow their party leaders in denying climate change or underestimating its effects (Brulle, Carmichael, and Jenkins 2012; Jacques, Dunlap, and Freeman 2008; Kahan 2012, 2015; McCright and Dunlap 2011; Tesler 2018).

This study looks for motivated reasoning in climate change opinion among an understudied group: liberals and Democrats. There is virtually no published scholarly work that investigates motivated reasoning in climate change opinion among those on the left. Like those on the right, liberals and Democrats have been found to reject science messages that contrast with pre-existing beliefs, and dissonant science messages have been found to result in reduced trust of the scientific community (Kahan 2017; E. C. Nisbet, Cooper, and Garrett 2015). A primary question this study aims to answer then is: are Democrats motivated to follow copartisan elites? More specifically, who do Democrats follow when they are cross-pressured between conflicting information sources that they tend to trust – fellow Democrats and climate scientists – and who are normally in agreeance on climate change?

In the January 14th Democratic 2020 presidential primary debate, candidate and senator Bernie Sanders stated the following,

> "If we as a nation do not transform our energy system away from fossil fuel, not by 2050, not by 2040, but unless we lead the world right now — not easy stuff — the planet we are leaving our kids will be uninhabitable and unhealthy".

UCLA climate scientist Daniel Swain responded to the statement, saying,

> "There's not really a plausible climate change scenario in which the Earth becomes truly uninhabitable."

Richard Betts of the U.K.'s Met Office Hadley Centre added,

> "While it is clear that ongoing warming of the global climate would eventually have very severe consequences, the concept of the Earth becoming uninhabitable within anywhere near [the year 2100] is pure hyperbole" (Jackson et al. 2020).

This is a real-world example of credible, mainstream climate scientists refuting, what they argue is, an exaggerative and misinformative statement by a Democratic opinion leader concerning the imminent and existential threat of climate change. While exaggerative beliefs about the severity of climate change in the public are likely not nearly as prevalent as denialist beliefs, nor are the former nearly so detrimental to policy action on climate change, examining these beliefs may provide needed understanding of motivated reasoning in climate change opinion formation (Druckman and McGrath 2019). Additionally, the extent to which the public believes that the media and Democratic elites exaggerate the severity of climate change carries important policy implications. Such "Pandora's Box" frames (M. C. Nisbet 2009), which focus on catastrophic consequences of climate change, tend to polarize climate change opinion along partisan and ideological lines, as Republicans and conservatives believe such consequences are, rightfully sometimes, exaggerated. In 2018, Gallup reported that 69% of Republicans, but only 4% of Democrats, believed that climate change is generally exaggerated in the news (Kamarck 2019).

I look particularly at how Democrat-identified individuals respond to presentation of variations in the above passage. I use a convenience sample of undergraduate students; a population with demographic characteristics highly correlated with Democratic self-identification, belief in anthropogenic climate change, and support for climate policies: young, educated, and liberal (Egan and Mullin 2017; Hornsey et al. 2016). For the purpose of the hypothesis tests described in the following section, I look specifically at Democratic-affiliated respondents

I present these arguments as experimental treatments in, essentially, a 2-by-2 factorial design to understand the independent effects of 1) presence of a copartisan elite misinformation source cue; and 2) presence of a correction that follows the misinformation, on participants' opinions on the arguments themselves as well as their opinions about climate change. Chapter 4 focuses on presenting results of hypothesis tests concerning argument evaluations as dependent variables, while Chapter 5 tests the same set of hypotheses on climate change opinions.

I find scant evidence that individuals evaluate arguments about climate change as a function of their partisanship in a manner consistent with directional motivated reasoning. That is, participants in this study do not rate the misinformation argument more favorably (or the correction less favorably) when the source of the misinformation argument is identified as a copartisan elite. In fact, overall, copartisans respond more negatively toward the misinformation argument and more positively toward the correction when the misinformation argument is attributed to a copartisan senator. Even more, when the misinformation source is identified as a Democrat, stronger Democrats rate the misinformation argument *less* favorably than when the misinformation source is unaffiliated with a party, suggesting a copartisan backlash correlated with strength of copartisanship. I find similar evidence that participants that came into the experiment viewing climate change as highly threatening rate the correction more highly when the misinformation source is identified as a copartisan.

While evaluating for the presence of motivated reasoning, I seek to ensure that I am not simply capturing "disagreement over who constitutes a credible source" (Druckman and McGrath 2019, p. 116). Individuals may actually be motivated to form accurate opinions based on how credible they deem information sources to be, and observed divergence between individuals with different prior attitudes may instead be a product of how the same individuals differently view politicians and scientists as credible sources of information. In a review of existing studies on motivated reasoning in the formation of climate change opinion, Druckman and McGrath (2019) note that studies do not account for the possibility that individuals are motivated to form accurate opinions based on who they think are credible sources of information. The authors argue that perceived source credibility may be confounding the relationships observed between prior attitudes or partisanship and opinion formation. The present study seeks to address this issue by also accounting for individuals' prior perceived credibility of climate change information sources.

However, I also find little support for Druckman and Mcgrath's (2019) concern that perceived credibility of information sources actually accounts for observed directional motivated reasoning. In fact, perceived source credibility largely does not appear to influence evaluations of the arguments presented in a manner consistent with accuracy motivated reasoning. For instance, participants that came in viewing Democratic politicians as highly credible do not view the misinformation argument more favorably than their counterparts between experimental conditions when the source of the misinformation is unidentified and when it is identified as Democratic senator.

That being said, I find that individuals generally evaluate the arguments presented to them about climate change in a manner consistent with accuracy motivated reasoning. Compared to experimental conditions where participants are not offered the sources of the arguments presented, providing participants full information about the argument sources in a manner resembling the passage above produces lower evaluations of the misinformation argument, higher evaluations of the correction, and views that the correction is more persuasive. In other words, when Democratic participants are given information that the misinformation comes from a Democratic politician and the correction comes from climate scientists, as they would be provided in a typical news article, they tend to follow the expert source in their evaluations.

Theory and hypotheses

The observation that individuals tend to process and evaluate information in a manner that conforms to their prior dispositions is not new (Campbell et al. 1960; Lord, Ross, and Lepper 1979). Kunda (1990) is credited with developing political psychology's modern understanding of motivated reasoning: individuals can be motivated to be accurate in their judgements or to arrive at a particular conclusion (Groenendyk and Krupnikov 2021). Kunda finds that the latter occurs more frequently among individuals. For instance, when participants in Kunda's study were told that caffeine consumption has negative health effects, they reported lower consumption levels. Kunda theorizes that people engage less frequently in accuracy motivated reasoning because it is more cognitively costly than directionally motivated reasoning.[12] Individuals are constrained in their ability to arrive at a conclusion by their ability to make reasonable justifications to get there. People more quickly, and are more likely to, access memories and facts that support a desired goal.

Taber, Lodge, and Glathar (2001) brought Kunda's framework to political science, and expanded and popularized the concept of motivated reasoning in the formation of political opinions (Kraft, Lodge, and Taber 2015; Lodge and Taber 2005; Taber and Lodge 2006, 2013). Taber and Lodge (2006) describe three cognitive mechanisms by which directional motivated reasoning can occur. First, "a prior attitude effect, whereby people who feel strongly about an issue […] will evaluate supportive arguments as stronger and more compelling than opposing arguments" (757). Second, a disconfirmation bias, whereby "people will spend more time and cognitive resources denigrating and counterarguing attitudinally incongruent than congruent

[12] Although, other causal mechanisms have been posited (Lau and Redlawsk 2006; Redlawsk 2002; C. Taber and Lodge 2006).

arguments" (757). Third, a confirmation bias "such that when free to choose what information [people] will expose themselves to people will seek out confirming over disconfirming arguments" (757). These mechanisms are expected to result in attitude polarization after exposure to a politically relevant treatment. In Taber and Lodge's (2006) seminal study, these treatments are pro- and anti-affirmative action and gun control arguments, where they were found to polarize opinions about the treatment messages presented, as well as policy opinions about gun control and affirmative action in the directions congruent with their prior opinions about these topics.

This study evaluates for the presence of a *prior attitude effect* based on respondents' prior (i.e. measured before treatment) perceptions about the severity of climate change. Consistent with directional motivated reasoning, prior climate change threat perception should moderate the effects of the treatments (i.e. the misinformation and correction arguments) on the observed outcomes, which include evaluations of the treatment(s) and climate change opinions. Specifically, participants that come into the experiment viewing climate change as imminently threatening should be more accepting of the exaggerative misinformation argument and more resistant to the correction relative to participants with lower prior climate change threat perceptions. The relative acceptance and resistance to these treatment arguments should be reflected in argument evaluations as well as opinions about climate change measured after treatment. To note, while I present a set of hypotheses to be tested on both evaluations of the treatment arguments as well as hypotheses related to opinions about climate change, I confine the results of this chapter to argument evaluations. I present results pertaining to climate change opinions in Chapter 5.

A parallel line of research focuses on *partisan motivated reasoning*: partisanship serves as a "perceptual screen" by which individuals process political information and form opinions (A. Campbell et al. 1960, 133). Partisans evaluate climate change information in a manner that is beneficial to their own party, and are especially motivated to do so when cued that the information in question comes from a copartisan elite (Carmichael and Brulle 2017; Feldman and Hart 2018; Hart and Nisbet 2012). Of special relevance to the present study, people also tend to follow factual statements made by copartisan leaders, even when those statements are credibly disputed (Flynn, Nyhan, and Reifler 2017; Nyhan and Reifler 2010). I experimentally manipulate whether the misinformation argument comes from a generic politician (in this case a senator) or specifically a "Democratic senator." Thus respondents – who are exclusively self-identified Democrats – are expected to rate the misinformation argument more favorably and adopt stronger climate change threat perceptions and higher support for climate policies when the misinformation source is identified as a Democrat, and particularly when respondents are also confronted with a correction to the Democratic-sourced misinformation.

As a harder test of partisan motivated reasoning via response to the copartisan source cue, I expect that participants that more strongly identify with the Democratic party to engage in this aspect of partisan motivated reasoning more than their counterparts with weaker Democratic affiliations. That is, I expect that strength of copartisanship moderates the copartisan misinformation source cue effect.

In summary, consistent with a directionally motivated model of opinion formation, I expect individuals with higher prior climate change threat perceptions to view the misinformation argument relatively favorably and to be relatively resistant to the correction, such that argument evaluations and climate change opinions are distinct between individuals that come in viewing climate change as more and less threatening. In particular, when sources of the arguments are identified to participants in the treatments, a prior attitude effect should manifest in significantly greater differences in evaluations and opinions.

I also expect that participants will respond favorably to copartisan elite source cues, and that strength of Democratic partisanship will moderate the effect of the copartisan source cue; stronger Democrats are expected to evaluate the misinformation argument highly and to be particularly resistant to the correction argument when the source of the misinformation is identified as a Democratic elite. Related, stronger Democrats should also perceive climate change as more threatening and exhibit higher support for climate policies when the misinformation source is attributed to a Democrat.[13]

I simultaneously evaluate a competing hypothesis of accuracy motivated reasoning: participants' evaluations and opinions are the products of how credible they judge the sources of information to be. There is much scholarly skepticism about the extent of directional motivated reasoning[14] and a number of findings that individuals can be incentivized to engage in accuracy motivated reasoning (Bizer et al. 2006; Druckman, Peterson, and Slothuus 2013; Groenendyk and Krupnikov 2016, 2021). It may be that, consistent with an accuracy motivated model, perceived source credibility guides opinion formation when arguments are attributed to their respective sources, even when controlling for other directional factors. I expect that argument

[13] Consistent with a near-consensus of prior research on motivated reasoning, I would expect polarization to be greatest among more politically and scientifically knowledgeable (Kahan 2012, 2015) participants, as they theoretically have more informational ammunition and drive to protect their political dispositions (Bizer et al. 2006; Bolsen, Druckman, and Cook 2014; Groenendyk and Krupnikov 2016; Kunda 1990; Redlawsk 2002; C. Taber and Lodge 2006). Unfortunately, preliminary analysis revealed that the sample size of the present study is insufficient to subset the sample by political or science knowledge in hypothesis tests. Therefore I am unable to assess how prior attitude effects or party source cue effects differ across levels of knowledge.

[14] See Druckman (2012) for a review.

evaluations and climate change opinions are functions of prior perceived source credibility in conditions where arguments are attributed to the sources in question. In particular, there is good reason to believe that Democratic-affiliated individuals may prefer the arguments made a copartisan elite because they think copartisan politicians are highly credible sources of information (Druckman 2012). Therefore a copartisan source cue effect that draws in copartisan participants may not be directionally motivated – but instead accuracy motivated – and accounting for how credible participants deem Democratic politicians to be should enable me to disentangle these motivations.

Thus evidence of partisan motivated reasoning would be observation of Democratic partisanship strength moderating the effect of the copartisan cue (such that stronger Democrats would respond more favorably to the cue) *while* simultaneously accounting for the potentially moderating effect of perceived Democratic politician credibility. If, in fact, perceived Democratic politician credibility accounts for the difference in treatment effects observed between strong and weak/lean Democrats, then this would indicate absence of partisan motivated reasoning and presence of accuracy motivated reasoning. This also holds for the prior attitude effect hypothesis; it may be that prior climate change threat perceptions moderate the relationship between treatments and outcomes. Such that, perhaps, participants that came in viewing climate change as more threatening hold onto higher policy support when a correction is presented, relative to participants that view climate change as less threatening. Although, it may be that this high-threat group also does not view scientists as highly credible sources of information (however unlikely that may be). If it's found that prior perceived scientist credibility accounts for the difference in treatment effect between high- and low-prior threat groups, this would indicate an absence of a prior attitude effect, and presence of accuracy motivated reasoning.

As an easier test of accuracy motivated reasoning is whether the correction has an effect on outcome variables when sources are attributed. Even in the absence of evidence for prior perceived information source credibility as a mechanism of accuracy motivated reasoning, accuracy motivated reasoning may still be observed by differences in argument evaluations and opinion between conditions where argument sources are not attributed and conditions where the misinformation argument is attributed to a politician and the correction is attributed to scientists. In other words, it can be argued that respondents engage in accuracy motivated reasoning if they view the misinformation argument less favorably, view the correction more favorably, view climate change as less threatening, and are less supportive of climate policies between source-unattributed and source-attributed conditions, and between source-attributed conditions where no correction is presented and conditions where a correction by scientists is presented. These observations would indicate that respondents are following the arguments made by experts – climate scientists – and/or rejecting arguments made by non-experts – politicians.

Before I move on to describe the research design and variable measurement, I note several limitations of this study. Concerning its scope, the design of the experiment does not allow for evaluation of a confirmation bias, as participants do not have the ability to choose to be exposed to more information that agrees with their prior climate change dispositions. A disconfirmation bias is also not evaluated here. Taber and Lodge (2006), Schaffner and Roche (2017), and others (see Druckman 2012 for a review) measure disconfirmation bias by the time spent reading treatment arguments that conflict with participants' priors: the more time spent reading these arguments, the authors assume the more cognitive resources participants spends denigrating and counterarguing conflicting information. These studies were conducted in lab settings, where it can be reasonably argued that the time spent on a page reliably indicates the time spent reading, processing, and responding to information on that page. Unfortunately for the present study, the 2020 COVID-19 pandemic required the survey to be fielded remotely. Therefore, it is much less clear that the time spent on a given page accurately reflects the cognitive resources expended on reading, processing, and responding to conflicting treatment arguments. Instead, the time spent on a given page may be a product of several unrelated tasks performed by the participant.

Second, practical limitations do not allow evaluation of a second aspect of partisan motivated reasoning: polarization between partisan groups (Bartels 2002; Bisgaard 2015). Ideally, this study would investigate inter-party polarization on argument evaluations and climate change opinions between the control and experimental conditions. Unfortunately, preliminary analysis reveals that the sample size of Republicans (N = 74, across six conditions) is too small to obtain accurate and precise response estimates (this is also true of Independents. N = 73). Further, Republican responses are notably distinct from Democratic responses, so that their inclusion may confound any observed prior attitude effects. Thus the sample is limited to Democratic respondents and the scope of partisan motivated reasoning evaluated here is limited to Democratic copartisan source cue effects.

Concerning limitations of causal inference, I cannot randomly assign participants particular quantities of interest, namely prior perceived source credibility and prior climate change threat perceptions. My investigation largely concerns heterogenous treatment effects between members of these groups. Therefore any dimension of apparent heterogeneity may not be the true dimension of heterogeneity, and may well instead be a product of some correlated and unobserved variable.

Methodology and research design

Figure 4.1 describes the procedural design of survey experiment. I review each step in the design, describe how each set of items are used to measure the variables of interest, and explain how the combination of control and treatment conditions allow for the hypotheses described above to be tested.

FIG. 4.1: EXPERIMENTAL PROCEDURE

- Scientist and politician credibility items
- Political and science knowledge items
    - Perceived threat of climate change items
- Self-described partisanship and ideology items
- Demographic distraction task items (in treatment conditions)

<u>Control conditions</u>

Climate change items

Demographic
distraction task items

1. No correction condition
2. (Correction condition)

Misinformation argument
(Correction argument)

Argument evaluation
items

<u>Treatment conditions</u>

No correction conditions:
3. With party source cue
4. Without party source cue
(Correction conditions:
5. With party source cue
6. Without party source cue)

Misinformation argument

Argument evaluation
items

Climate change items

To begin, all participants are presented with a set of items pertaining to credibility. Per Pornpitakpan's (2004) review of work on source credibility, the constituent parts of information source credibility are expertise: how accurate is the information provided by the source, and trustworthiness: how willing the source is to provide information that the source believes to be accurate. I ask two sets of questions that seek to assess these respective constructs regarding the informational sources used in the treatment arguments: scientists and Democratic politicians.[15]

"How capable do you think _________ are of solving the nation's problems?
[Very capable / Somewhat capable / A little capable / Not at all capable]."

"How much of the time do you think you can trust _________ to do what is right?
[All of the time / Most of the time / Only some of the time / Never]."

The average correlation coefficient for responses to these items for each source is .38. I average responses to the two items together for each source in question in order to form an overall impression of pre-treatment perceived source credibility. This measure then serves as a mechanistic indicator of accuracy motivated reasoning. Specifically, I evaluate whether, between source-unattributed and source-attributed conditions, change in argument evaluation (or climate change opinion) is a function of prior perceived source credibility. I still assess for accuracy motivated reasoning more generally by gauging the change in participants' evaluations and opinions between source-omitted and source-attributed conditions.

[15] While I do experimentally manipulate as to whether the misinformation argument is attributed to an unaffiliated senator and a Democratic senator, using perceived *Democratic* politician credibility should provide additional leverage in identifying whether participants follow their copartisan elite as a function of how credible they deem Democratic politicians to be.

All participants are next provided a set of six general science knowledge questions. So as not to prime respondents prior to the treatment arguments, I "hide" two questions that I use to measure prior climate change threat perceptions within this set of science knowledge items:

> "When will sea level rise caused by climate change will be a serious problem for the United States? [This won't be a serious problem for the United States / In 50 years or longer / In 25 years / In 10 years / It is a serious problem right now]."
>
> "When will climate change start to seriously harm people in the United States? [This won't seriously harm people in the United States / In 50 years or longer / In 25 years / In 10 years / They are being seriously harmed now]."

The prior climate change threat perception items are modified versions of items included in Yale's Climate Change in the American Mind survey. They directly pertain to the treatment arguments, which topically concern both the level and timing of the threat posed by climate change. The correlation coefficient for these items is .41. I average responses together to form my measure of prior climate change threat perceptions.[16]

I next measure participants' self-identified partisanship using a standard five-point scale. Of respondents included in the main study (described in the following section), 74 are either weak/lean or strong Republicans, 73 are pure Independents, and 490 are either weak/lean or strong Democrats. Respondents that initially identify as "Independent" or "other" but said they leaned toward either party in a follow-up question are coded with "weak" party-identifiers as

[16] I then present six items to measure participants' general knowledge of contemporary U.S. politics and the political system. Although, as previously stated, the sample size in this study prohibits me from splitting the sample by political or science knowledge to assess the possibility for greater directional motivated reasoning among the more knowledgeable, so that these knowledge items unfortunately go unused.

weak/lean partisans. I separate strong and weak/lean Democrats in analyses pertaining to party cue effects to control for strength of Democratic affiliation.[17]

At this point in the survey participants are randomly assigned to one of six experimental treatment conditions. In two conditions participants are provided items pertaining to opinions about climate change before they are provided the treatment arguments and argument evaluation items. I call these *control* conditions because the information provided in the arguments cannot influence these participants' climate change opinion item responses. Thus responses in these conditions serve as a baseline by which to evaluate the effect of the arguments on climate change opinion in the *treatment* conditions, where the treatment arguments precede the climate change opinion items. This distinction is particularly relevant in Chapter 5, but the same language is used in this chapter for the sake of consistency.[18]

The climate change opinion items include the same two climate change threat perception items asked among the science knowledge items. I subtract the mean of a participant's post-treatment (or, in the control groups, the second time they are presented these items) climate change threat perceptions from the mean of their pre-treatment threat perceptions in order to measure intra-participant change in threat perceptions, akin to Taber and Lodge's (2006) experimental design.

---

[17] I also ask participants' political ideology on a five-point scale. However, 53 Democratic-affiliated participants identify as moderate and only 7 identify as slightly conservative. The remaining 430 identify as liberal, with 226 of these identifying as moderately liberal. Thus there is insufficient information in most experimental conditions to control for it in analysis.

[18] In retrospect, I would have the climate change opinion items follow the unattributed treatment arguments in the control conditions. That way, while in all conditions participants' climate change opinions may be influenced by the treatment arguments, I would be better able to isolate the effect of argument source attribution on climate change opinions. That being said, the mean estimates between the control and correction-omitted conditions are largely similar (shown in the following sections), so that it is unlikely that this change would result in any substantial difference in the control condition mean estimates.

I also include two more threat perception items among the others in the set of climate change opinion questions to reduce measurement error and to avoid drawing attention to the repeated threat perception items:

> "How worried are you about climate change? [Not worried at all […] Very worried]"

> "When will climate change will start to seriously harm you? [This won't seriously harm people in the United States / In 50 years or longer / In 25 years / In 10 years / I am being seriously harmed now]."

These four items are, on average, correlated at .46 (ranging from .32 to .63) and are averaged together to form a climate change threat perception scale.

I also include four environmental policy proposal items identical to those included in the Cooperative Congressional Elections Study survey. These ask "How much do you support or oppose the following policies [strongly oppose […] strongly support]:

1. Regulate carbon dioxide (the primary greenhouse gas) as a pollutant
2. Require electric utilities to produce at least 20% of their electricity from wind, solar, or other renewable energy sources, even if it costs the average household an extra $100 a year
3. Fund more research into renewable energy sources, such as solar and wind power
4. Set strict carbon dioxide emission limits on existing coal-fired power plants that would reduce climate change and increase the cost of electricity"

Responses to these four items are, on average, correlated at .53 (ranging from .44 to .65) and form a climate policy opinion scale. To note, because climate change policy is more distantly related to the treatment arguments than are climate change threat perceptions, evaluation of this variable offers a more difficult test of treatment effects. The resulting climate change threat perception and climate change policy scales are only weakly correlated – at .26 – and so responses to the items that constitute each scale are not averaged together to form a more holistic climate change opinion scale, and instead are analyzed separately.

Participants in the control conditions are then presented with several demographic items (gender, race/ethnicity) to serve as distractors prior to presentation of the treatment arguments. If participants in the control condition were to move directly from responding to items about climate change to evaluating arguments, the climate change items might inadvertently prime evaluations of the arguments. The demographic items serve as a "palate cleanser" to decrease the likelihood of such influence. These items precede the argument evaluations in the treatment conditions to the same effect.

In the control conditions, participants are then randomly assigned to either receive the misinformation argument alone or the misinformation argument followed by the correction argument. Neither argument is attributed to any source. Instead, responses to these items in the control conditions serve as a baseline by which to compare effects of source attribution in the treatment conditions.[19] Control condition participants in the *control, no correction* (termed *ctrl, no c* for brevity) condition are prompted with:

[19] Thus in the context of argument evaluations, the "control" conditions might instead be called "baseline" conditions or more accurately, "unattributed information source" conditions. I occasionally refer to the control conditions as such when discussing the argument evaluation results.

> “Next, I’ll provide an argument about climate change and ask your thoughts on it. Climate change refers to the long-term upward trend of the Earth’s temperature and its effects. Please read carefully:
>
> If we as a nation do not act to reduce our contribution to climate change by transforming our energy system away from fossil fuels – not by 2050, not by 2040 – but unless we lead the world right now, the planet we are leaving our kids will be unhealthy and uninhabitable.”

In the *control, correction* condition (termed *ctrl, c*), the misinformation argument is followed by,

> “There’s not really a plausible climate change scenario in which the Earth becomes truly uninhabitable. While it is clear that ongoing warming of the global climate would eventually have very severe consequences, the concept of the Earth becoming uninhabitable within anywhere near the year 2100 is pure exaggeration.”

The arguments in the *ctrl, c* condition are labelled “Argument 1” and “Argument 2” respectively, and referred to as such in the items that follow:

> “How much consideration do you think the [first / second] argument should be given when making climate change policy? [A great deal of consideration / A fair amount / A little / None at all]”.
>
> “How would you rate the strength of the [first / second] argument? [Very strong / Somewhat strong/ Somewhat weak / Very weak]”.

Responses to these two items are correlated at .51 for the misinformation argument and .72 for the correction argument, and so are averaged together to form an overall evaluation of each argument. In the *ctrl, c* condition, a third item follows:

> "Which argument do you think is more persuasive, the first or the second? [The first argument / The second argument / The arguments are equally persuasive]".

These argument evaluation items are also included in the treatment conditions. Because the arguments in the treatment conditions are attributed to sources, the argument evaluation items respectively refer to "The [Democratic] senator's argument" and "the scientists' argument."

In the treatment conditions, after the distraction task items are presented, participants are randomly assigned to receive one of four argument combinations: only the misinformation argument attributed to a generic senator (no party cue, and no correction: termed *no p, no c*); only the misinformation argument attributed to a Democratic senator (termed *p, no c*); the misinformation argument, attributed to a generic senator, followed by the correction argument attributed to climate scientists (termed *no p, c*); or the misinformation argument, attributed to a Democratic senator, followed by the correction argument attributed to climate scientists (termed *p, c*).

In the treatment conditions, consistent with previous studies (Berinsky 2015; Nyhan and Reifler 2010), arguments are presented in a news article format as a way to enhance the external validity of the treatments; i.e. the arguments are presented in a manner similar to how participants might be exposed in the real world. Unfortunately, formatting the arguments in this manner means that I cannot isolate the effect of source attributions on argument evaluations, per se, as there may be some correlated effect of the format itself, which is not present in the control conditions. However, it is difficult to conceive of a realistic news article without either an identified or “anonymous” source, and an “anonymous source” in journalistic reporting may carry unintended meaning for participants and so result in unintended effects on opinions and evaluations. Related, I name the climate scientists in the source-attributed condition but do not name the Democratic senator. It may be that participants view the correction more highly in the source-attributed condition partially because an unnamed senator may appear less credible. In retrospect, I would name the senator in the source-attributed conditions to account for this possibility.[20]

The prompt and arguments for the *p, c* condition is as follows:

> “Next, I’ll provide a segment of a recent news article about climate change. Climate change refers to the long-term upward trend of the Earth’s temperature and its effects. Please read carefully.
>
> **Climate Change: When Will It Hurt; When Will We Act?**
>
> By: CHRISTOPHER G. BENNETT
> Published: June 15, 2020
>
> WASHINGTON, DC – After a heated discussion on Capitol Hill among members of the Senate’s Committee on Energy and Natural Resources, a [Democratic] senator released the following statement about the imminent threat of climate change, “If we as a nation do not act to reduce our contribution to climate change by transforming our energy system away from fossil fuels – not by 2050, not by 2040 – but unless we lead the world right now, the planet we are leaving our kids will be unhealthy and uninhabitable.”
>
> Climate scientists reached out to dispute the truth of the [Democratic] senator’s statement. UCLA climate researcher Daniel Swain stated that, “There’s not really a plausible climate change scenario in which the Earth becomes truly uninhabitable.” Richard Betts, head of the U.K.’s Centre for Climate Prediction and Research, also argued against its accuracy, “While it is clear that ongoing

[20] Though I would not name the senator Bernie Sanders, as his identity may carry other unintended meaning for participants.

> warming of the global climate would eventually have very severe consequences, the concept of the Earth becoming uninhabitable within anywhere near the year 2100 is pure exaggeration." "

In the *no p, no c* and *no p, c* conditions, the "Democratic" descriptor is omitted. In the *no p, no c* and *p, no c* conditions, the second paragraph that contains the correction is omitted. Following presentation of the arguments, participants are presented with the same set of argument evaluation items as participants are in the control conditions. Finally, participants in the treatment conditions are presented with the same set of climate change opinion items as those presented in the control conditions.

In sum, the experiment effectively follows a 2-by-2 factorial design to understand the independent effects of a copartisan source cue and a correction that follows the misinformation, where each of the four conditions is some combination of either omitting or including the two independent variables . There are two additional conditions that manipulate the presence of a correction, but where neither source is attributed. Table 4.1 illustrates the experimental design, where each cell contains the abbreviated condition name.

Table 4.1: Experimental design

		Correction	
		Omitted	Included
Copartisan cue	Omitted	no p, no c	no p, c
	Included	p, no c	p, c
Sources unattributed		ctrl, no c	ctrl, c

Moving to the methodological design: because the prior attitude effect, the harder test of the partisan motivated reasoning, and the harder test of accuracy motivated reasoning are based on the moderating influence of prior threat perceptions and partisan strength, respectively, they are evaluated by determining whether statistically significant difference-in-differences are present between appropriate conditions. Specifically, these hypotheses are tested via interaction with a condition dummy variable in OLS regression. With this method, I am able to discern whether some baseline difference in mean argument evaluation estimates between, for instance, individuals with high prior threat perceptions and individuals with low threat perceptions in the *no p, no c* condition, is significantly less than the difference in mean estimates between these groups in a relevant treatment condition: in this case, the *no p, c* condition. If the difference between groups is significantly different between conditions in the direction expected, this marks evidence in favor of the corresponding hypothesis.

The easier test of partisan motivated reasoning, whereby the entire sample of Democrats is expected to express evaluations more favorable to – and opinions in line with – the misinformation argument when it is attributed to a copartisan elite, is more simply tested as a difference in means between appropriate conditions (e.g. between the *no p, c* and *p, c* conditions). Likewise, the easier test of accuracy motivated reasoning, where participants are expected to favor the correction significantly more when it is attributed to scientists, is also tested via difference in condition means (e.g. between the *ctrl, c* and *no p, c* conditions).

Data

To begin I describe the sample of respondents. A pilot study was conducted to determine the baseline strength for the treatment arguments (right column of Table 4.1). Respondents were randomly assigned to receive one of three argument conditions: only the misinformation argument (termed *ctrl, no m*), the misinformation followed by the correction, and only the correction argument. The arguments presented in the pilot study were unattributed and presented in a manner identical to those in the *ctrl, no c* and *ctrl, c* conditions. Only argument evaluation and political identity items were included in the pilot study.[21] In the main study described above, the *ctrl, no m* condition was omitted to increase the sample size of the remaining six conditions. The distribution of *ctrl, no c* and *ctrl, c* responses between the pilot study and the main study are not discernably different, so responses between studies are pooled when assessing argument evaluations and heterogenous treatment effects by partisanship.

Table 4.2 indicates that the conditions contain a roughly even number of respondents. Due to the small sample size in some cells, When appropriate, I pool conditions to increase statistical power. I discuss this pooling process as it becomes relevant in the following sections.

Table 4.2: Sample sizes	Main sample N	Pilot sample N
Overall	490	234
High prior climate change threat perceptions	219	-
Low prior climate change threat perceptions	271	-
High prior perceived scientist credibility	264	
Low prior perceived scientist credibility	226	
High prior perceived Dem. politician credibility	331	
Low prior perceived Dem. politician credibility	159	
Control: No correction (ctrl, no c)	81	82
Control: correction (ctrl, c)	88	72
Control: no misinformation (ctrl, no m)	-	80
Treatment: no party cue, no correction (no p, no c)	86	-
Treatment: party cue, no correction (p, no c)	80	-
Treatment: no party cue, correction (no p, c)	72	-
Treatment: party cue, correction (p, c)	83	-

Note, all variables are coded zero to one unless otherwise noted, with higher values indicating higher perceived source credibility, higher perceived threat of climate change, more favorable argument evaluations, and greater support for climate change policies.

[21] I do not review the argument evaluations by participants in the pilot study alone. I instead incorporate those results into my review of the main study results.

Figure 4.2 illustrates the distribution of prior climate change threat perceptions. 44% of Democrats in the main study provided the highest prior threat responses to the two constituent items in the scale (i.e. "climate change-caused sea level rise is a serious problem for the U.S. right now," and; "people in the U.S. are being seriously harmed by climate change right now"), and the distribution is heavily left-skewed (mean = .83, median = .88). Therefore I dichotomize the variable into "high" and "low" prior threat perception groups for analysis; the high prior threat group contains participants that provided the highest threat responses to the two items, and the low prior threat group contains all other participants.

Figure 4.3 splits the Democratic sample by strength of party affiliation. While the mean prior climate change threat perceptions of strong Democrats is slightly higher than weak/lean Democrats (.85 and .82, respectively), a t-test reveals that there is no significant difference in prior threat perceptions between groups ($p = .15$).

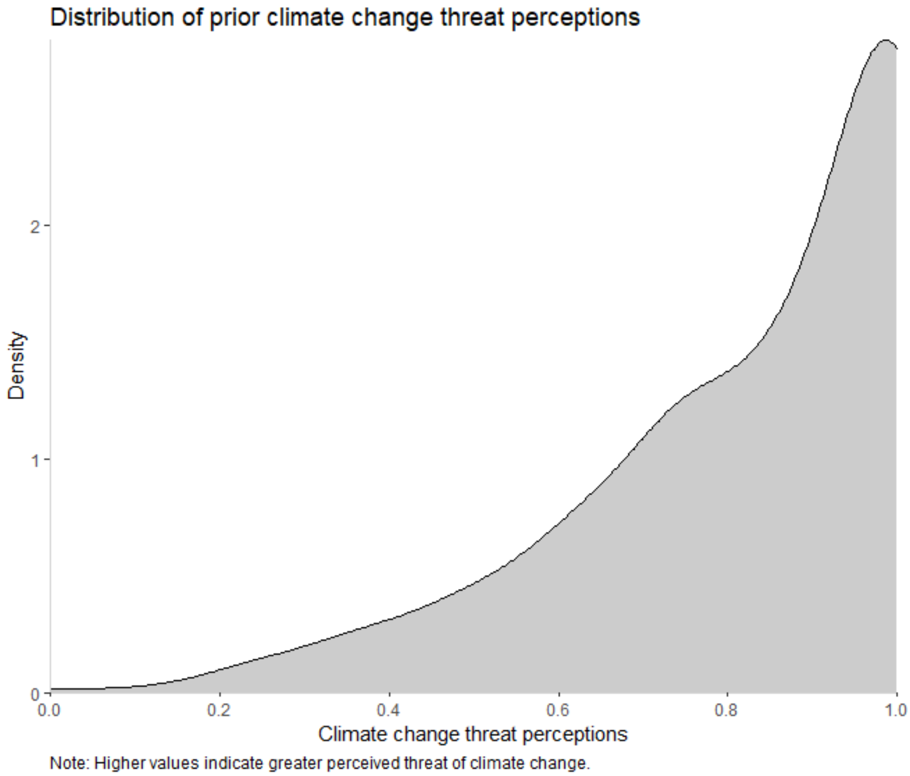
Distribution of prior climate change threat perceptions
Density
0
1
2
0.0
0.2
0.4
0.6
0.8
1.0
Climate change threat perceptions
Note: Higher values indicate greater perceived threat of climate change.

FIG. 4.2

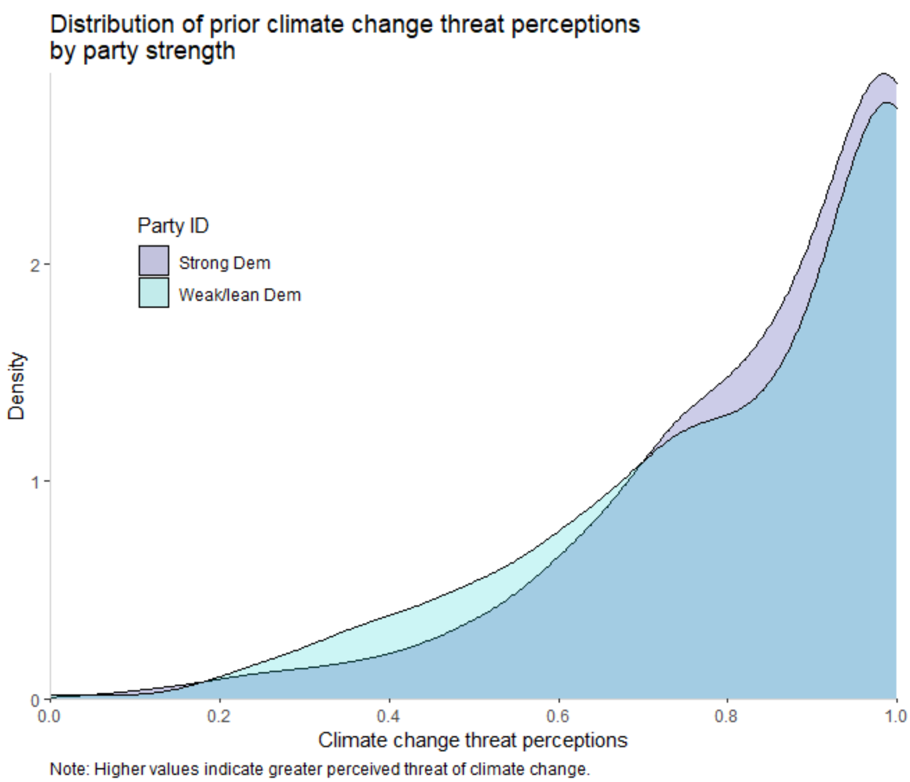
Distribution of prior climate change threat perceptions
by party strength
Party ID
Strong Dem
Weak/lean Dem
Density
0
1
2
0.0
0.2
0.4
0.6
0.8
1.0
Climate change threat perceptions
Note: Higher values indicate greater perceived threat of climate change.

FIG. 4.3

Figure 4.4 through Figure 4.7 provide the distributions of prior source credibility for Democratic politicians and scientists, respectively. Figure 4.4 reveals that the distribution of the former is normal, with both a mean and median of .5. However, Figure 4.5 illustrates that strong Democrats come in viewing Democratic politicians as more credible than weak Democrats (t-test p value < .05).

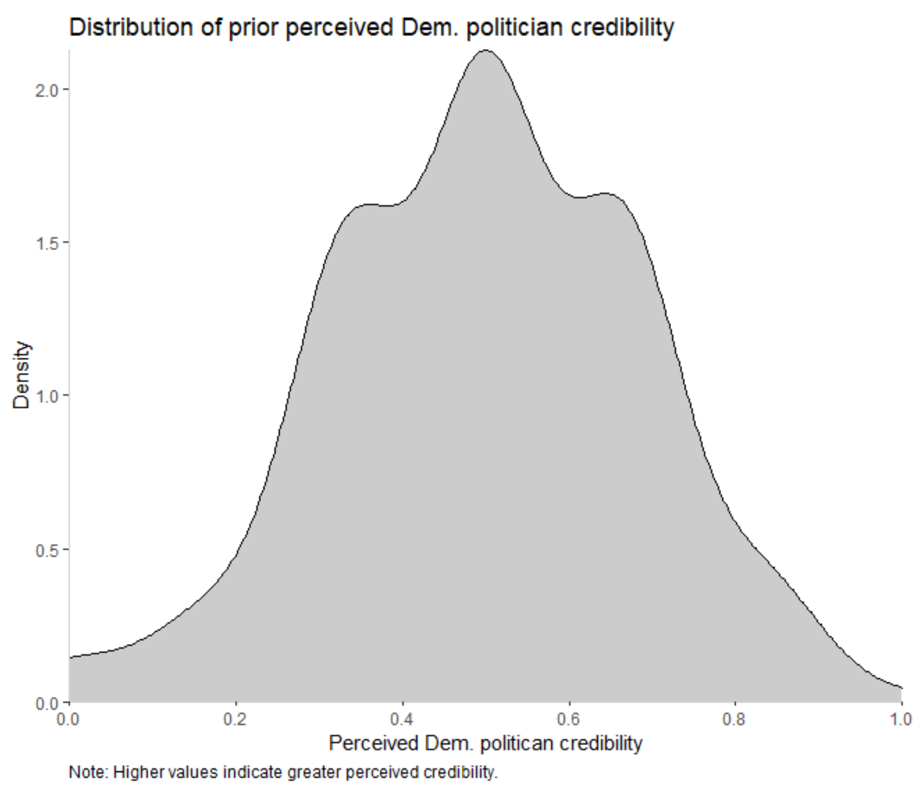

FIG. 4.4

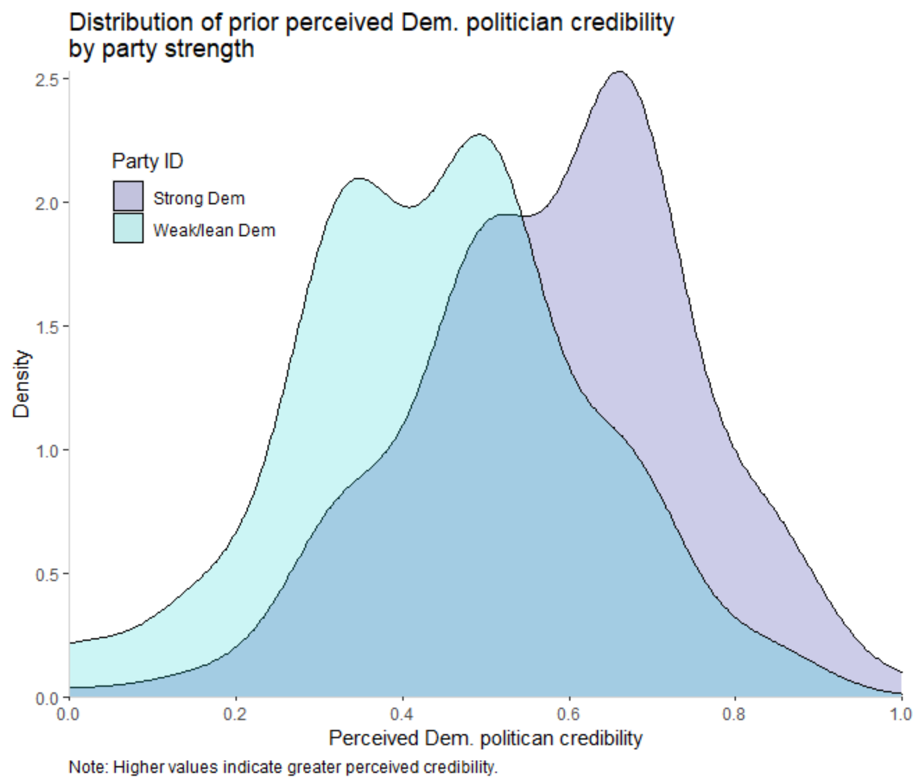

FIG. 4.5

Figure 4.6 shows that respondents come in viewing scientists as more credible than politicans (mean = .76, median = .83). Like they view politicans, strong Democrats' come in to the experiment viewing scientists as more credible than their weak/lean counterparts ($p < .05$). Thus in testing for the presence of accuracy-motivated reasoning, I make sure to account for the strength of participants' Democratic affiliations. Also, due to the relatively normal distributions of prior source credibility there is no obvious break by which to split respondents into "high" and "low" groups. I leave prior perceived source credibility as a continuous variable with the full range of values in analysis.

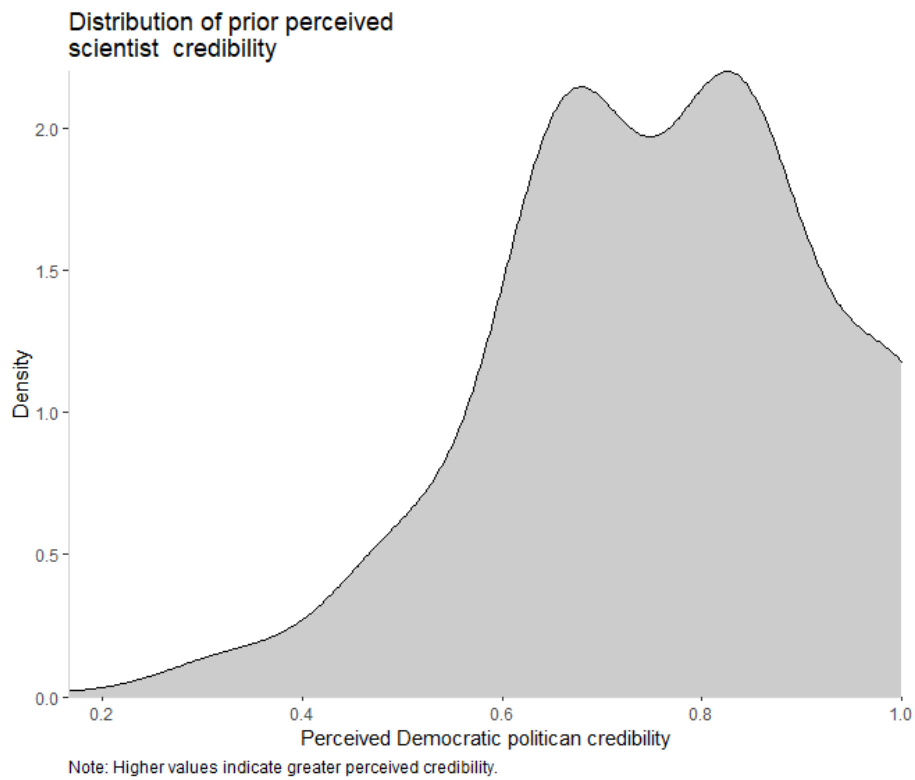
Distribution of prior perceived
scientist credibility
Density
2.0
1.5
1.0
0.5
0.0
0.2
0.4
0.6
0.8
1.0
Perceived Democratic politican credibility
Note: Higher values indicate greater perceived credibility.

FIG. 4.6

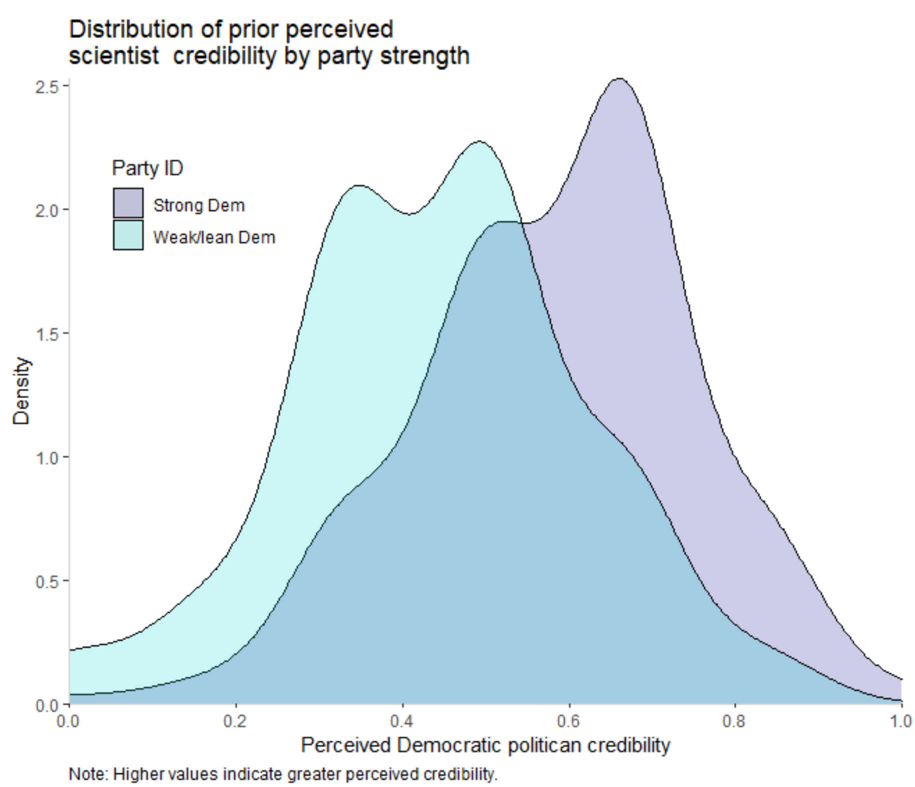
Distribution of prior perceived
scientist credibility by party strength
Party ID
Strong Dem
Weak/lean Dem
Density
2.5
2.0
1.5
1.0
0.5
0.0
0.0
0.2
0.4
0.6
0.8
1.0
Perceived Democratic politican credibility
Note: Higher values indicate greater perceived credibility.

FIG. 4.7

Results

I organize the results of my hypothesis tests across dependent variables. For each dependent variable, I first describe the whole-sample mean estimates. I then break the sample down by party strength, prior threat perceptions, and prior perceived source credibility, in turn in order to test my partisan motivated reasoning, prior attitude effect, and accuracy motivated reasoning hypotheses, respectively. I only test these hypotheses on argument evaluations here in Chapter 4. In the next chapter, I briefly describe and then test this same set of hypotheses on climate change opinions.

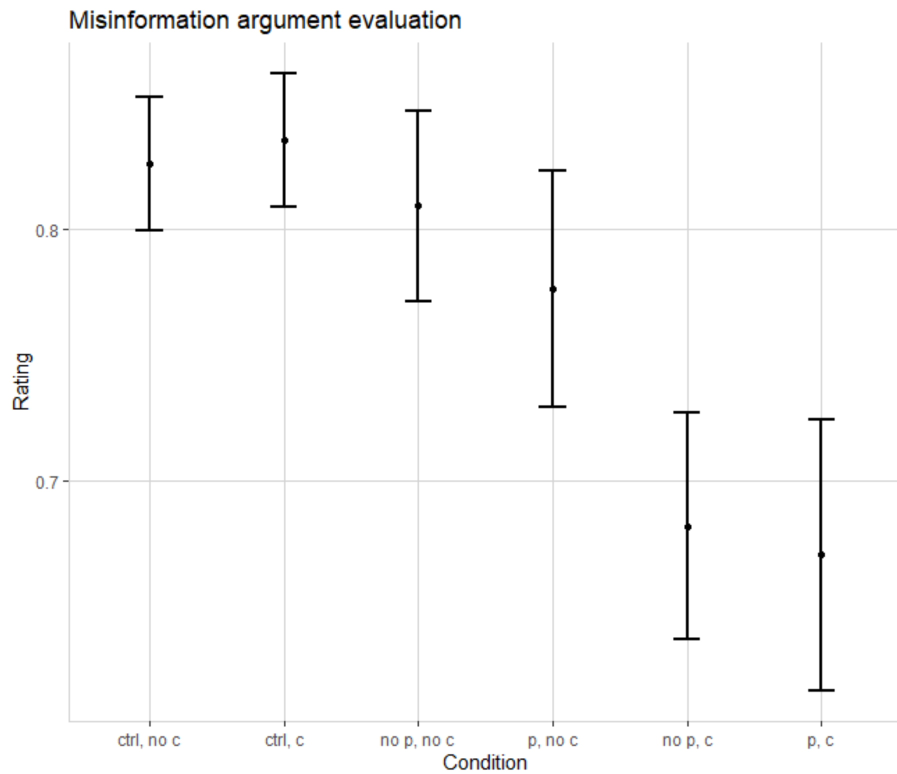

FIG. 4.8

Figure 4.8 illustrates the mean estimates (with 95% confidence intervals) of evaluation of the misinformation argument in the overall sample. Participants view the misinformation very favorably in the source-unattributed conditions; the mean evaluation between the *ctrl, no c* and *ctrl, c* condition is .83. There is no apparent difference in evaluation between source-unattributed conditions; adding a correction does not change favorability toward the misinformation argument when the arguments are unsourced. However, there is a large difference in evaluation between correction-omitted and correction-included conditions where the argument sources are attributed. In other words, only when arguments are attributed to their respective sources does the correction affect evaluation of the misinformation argument. Between the *no p, no c* and *no p, c* conditions, the mean evaluation drops by .14. Between the *p, no c* and *p, c* conditions, the mean evaluation drops by .11. On average (.13), these changes roughly amount to a one-unit shift toward a less favorable response option in one of the two items that constitute the argument evaluation scale (e.g. moving form viewing the argument as "very strong" to "somewhat strong").

The party cue seems to have a negative, but insignificant, effect on the aggregate sample. However, Figure 4.9 reveals heterogenous effects by Democratic strength in the source-attributed correction-included conditions: between the *ctrl, c* and *no p, c* conditions, then again between the *no p, c* and *p, c* conditions. Between the *ctrl, c* and *no p, c* conditions, strong Democrats appear to hold onto more favorable evaluations of the misinformation argument. Then, once the source of the argument is identified as a Democrat, between the *no p, c* and *p, c* conditions, strong Democrats drop their evaluation to nearly match that of weak/lean Democrats.

To statistically evaluate the presence of difference-in-difference effects concerning the quantity of interest (party strength, in this case) I regress the misinformation argument evaluation on experimental condition and party strength, and the interaction between the two. Both independent variables are treated as dummies in regression. Party strength is dichotomous, with "strong Democrat" as the base category. Although it is visually suggested in Figure 4.9, regression reveals no significant difference-in-in-difference between the *ctrl, c* and *no p, c* conditions (B = -.11, p = .1).[22]

However, Figure 4.10 highlights the coefficient estimates and 95% confidence intervals for a significant difference-in-difference effect between the *no p, c* and *p, c* conditions. The *no p, c* condition is the base experimental condition and “strong Democrat” is the base party strength category (i.e. both are treated as zero). The top-most term, *no p, c * weak/lean* is the difference between strong and weak/lean Democrats in the *no p¸ c* condition; weak/lean Democrats have a significantly less favorable rating of the misinformation argument in this condition. The middle term is the difference in evaluation for strong Democrats between the *no p¸ c* and *p, c* conditions, which approaches a significant decrease. The bottom-most term is the difference among strong and weak/lean Democrats in change in evaluation between these conditions, such that weak/lean Democrats experienced a significantly different (in this case, more positive) change in evaluation between conditions (B = .14, SE = .06).

[22] For the sake of brevity, I do not report regression coefficient plots of results that do not reach traditional levels of statistical significance (p < .05).

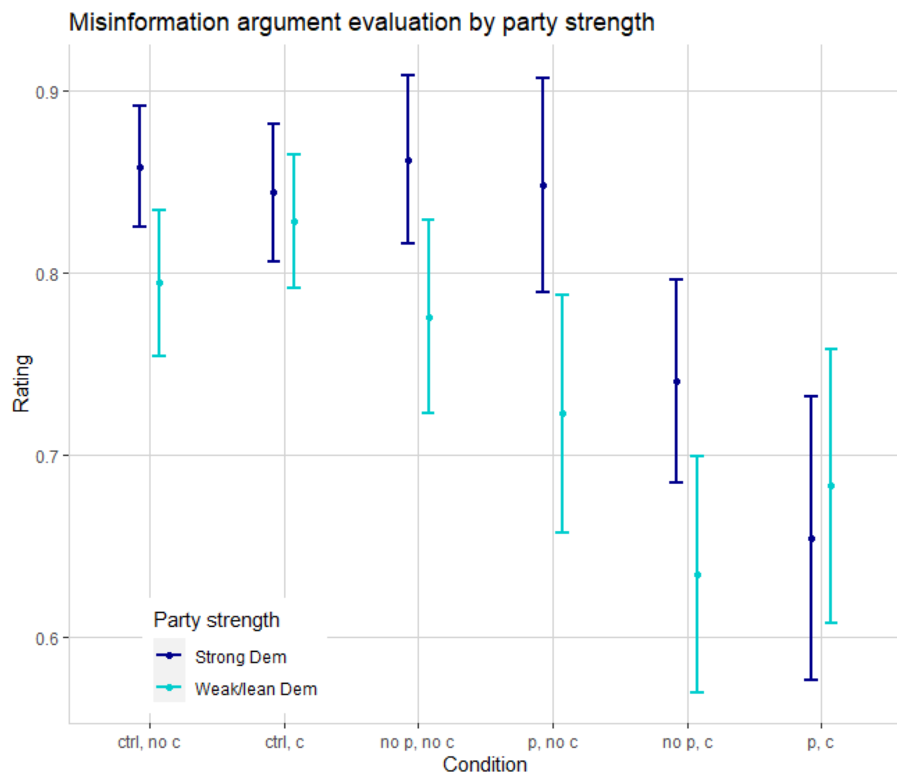

FIG. 4.9

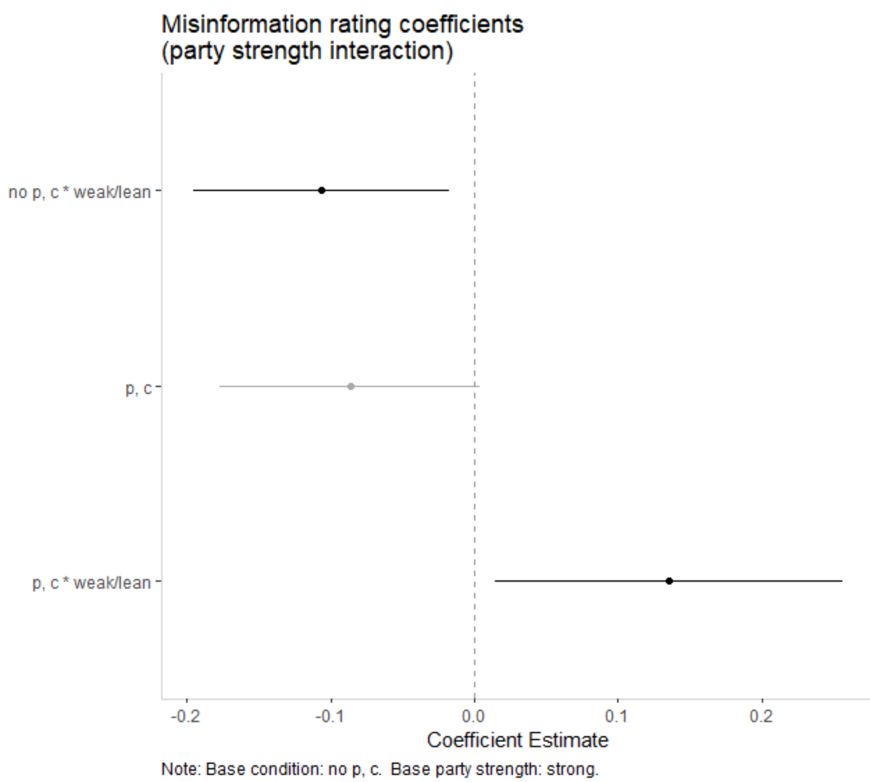

FIG. 4.10

This difference-in-difference effect provides evidence directionally opposed to the copartisan source cue hypothesis; party strength is not tied to more favorable evaluations of the misinformation argument when it is attributed to a copartisan elite. Instead, stronger Democrats experience lash back against their own party when the misinformation argument is identified as such: that is, in the presence of a correction by scientific experts.

To note, there are no significant heterogenous treatment effects related to party strength between a source-unattributed condition and a corresponding source-attributed condition. For example, there is no significant difference-in-difference between the *ctrl, c* and either the *no p, c* or *p, c* conditions.

Figure 4.11 splits estimates by prior climate change threat perceptions. As expected, there is no difference in evaluations between conditions that do and do not include a party cue. Figure 4.12 collapses conditions so that the *no c* condition aggregates estimates from both the *no p, no c* and *p, no c* conditions and *c* includes estimates from both the *no p, c* and *p, c* conditions.

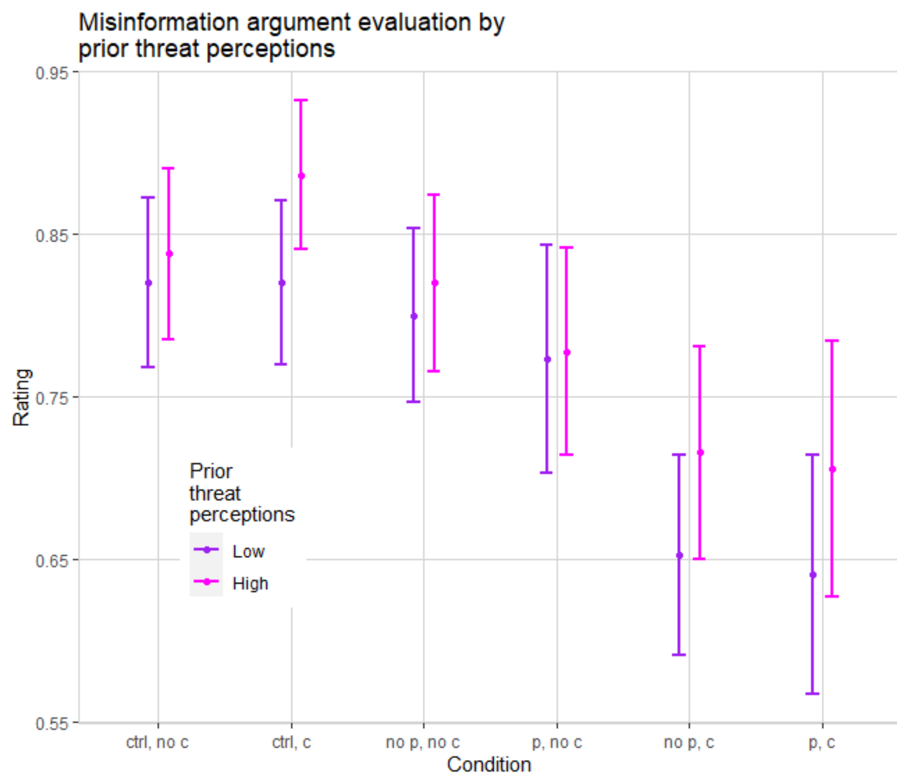

FIG. 4.11

Although Figure 4.12 shows that the "high-threat" group held onto slightly more favorable evaluations of the misinformation argument between the *no c* and *c* conditions, regression estimates (not shown here) verify that this difference-in-difference is not statistically significant ($p > .1$).

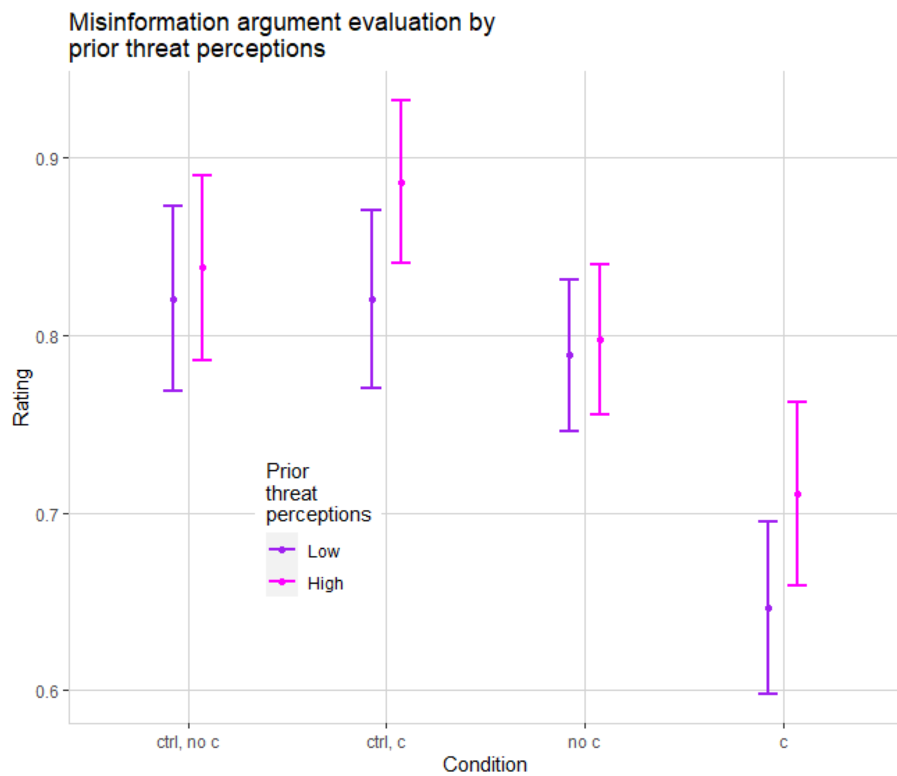

FIG. 4.12

Is there any support for an accuracy motivated model via perceived source credibility? Specifically, is there a heterogenous treatment effect by prior perceived Democratic politician credibility between corresponding (i.e. correction-omitted and correction-included) source-unattributed and source-attributed conditions? Figure 4.13 shows perceived Democratic politician credibility group mean estimates for each condition. For illustrative purposes, I dichotomize prior perceived source credibility at the mean into "low" and "high" groups at the mean. There appear to be no strong difference-in-differences between the *ctrl, no c* and *p, no c* conditions. Likewise, there is no obvious difference-in-difference between the *ctrl, c* and *p, c* conditions. Regression analysis, where perceived Democratic politician credibility is given the full range of values, confirms an absence of difference-in-difference effects between these conditions.[23]

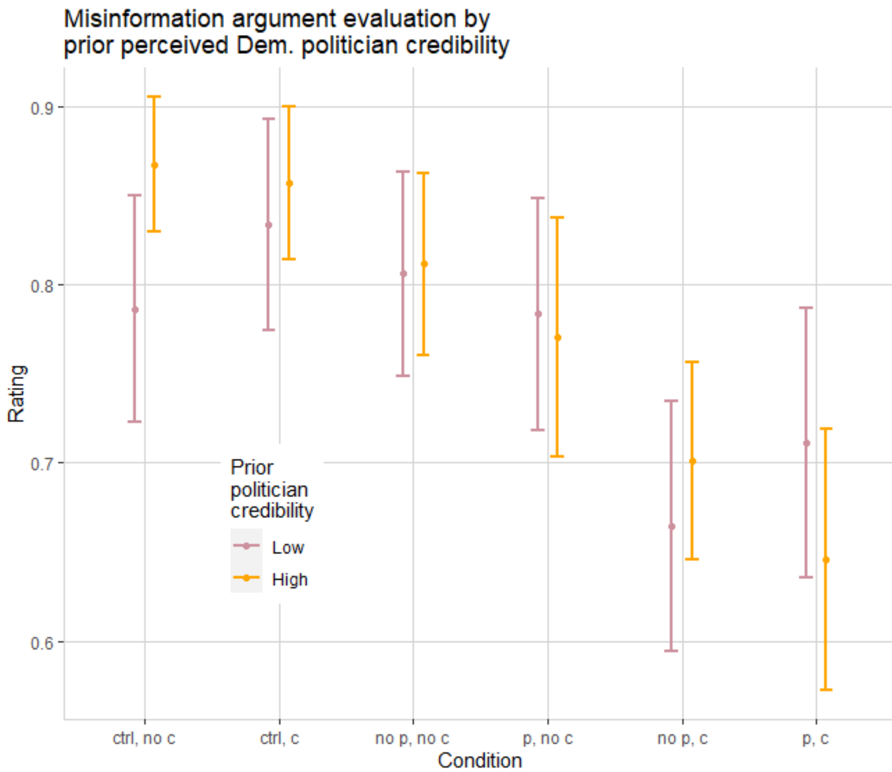

FIG. 4.13

Figure 4.14 splits condition samples by perceived scientist credibility and also reveals no difference-in-difference effect between respective source-unattributed and source-attributed conditions.

[23] The results of regression analyses where estimates of interest are highly insignificant are not provided here, as plotted mean estimates arguably provide sufficient evidence of insignificant difference-in-difference effects.

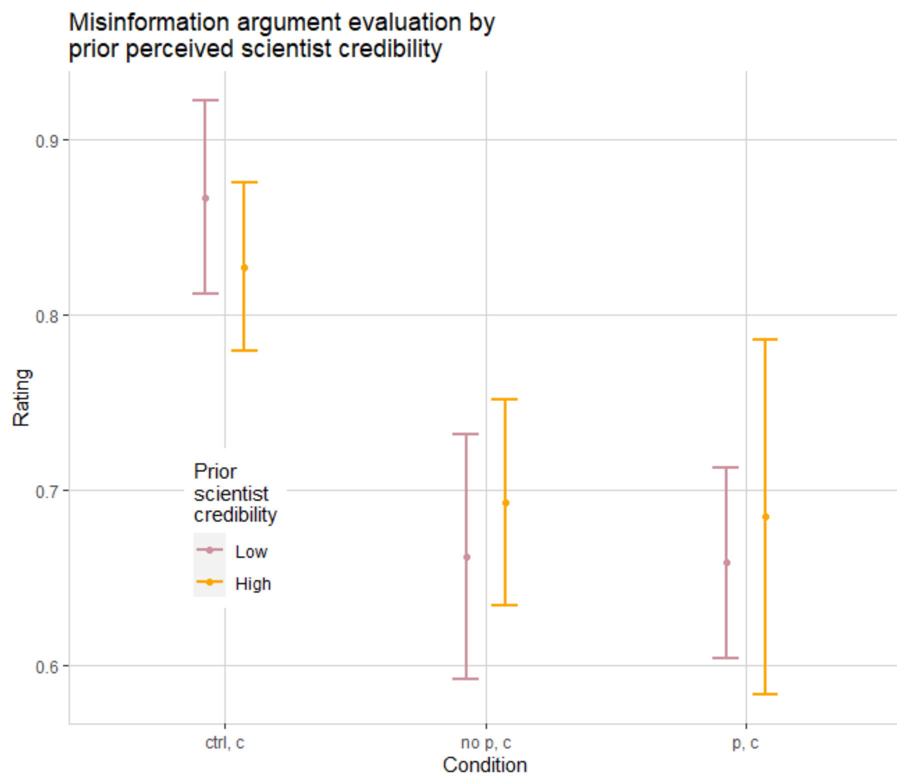

FIG. 4.14

Because accuracy motivated reasoning might be a function of *both* one's perceived scientist credibility and one's perceived politician credibility, and because the sources offer conflicting arguments, I subtract a respondent's perceived Democratic politician credibility from their perceived scientist credibility to create a measure of *net* perceived scientist credibility. I rescale the resulting measure to range from zero to one for comparability with the other measures. I dichotomize the measure to produce Figure 4.15. Again, regression analysis confirms no presence of a difference-in-difference effect.[24]

[24] When corresponding party cue-omitted and party cue-included groups are collapsed to improved statistical power, no further evidence of heterogenous treatment effects are present.

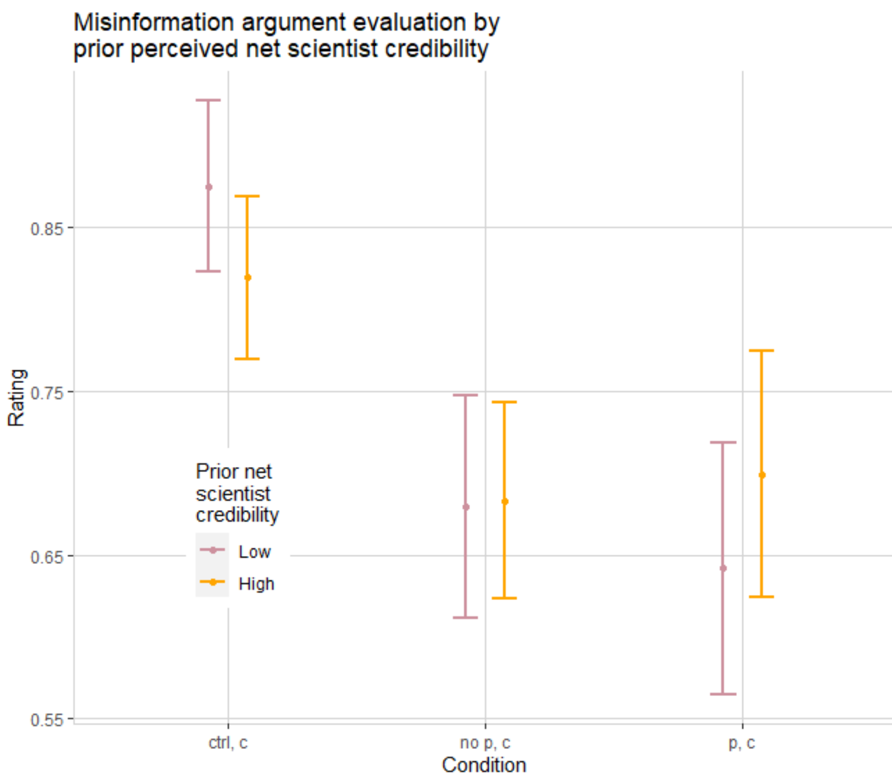

FIG. 4.15

Figures 4.13-4.15 do not provide support for information source credibility as a mechanism or indicator or accuracy motivated reasoning. Because there are no independent heterogenous treatment effects by perceived source credibility, I do not test regression models that check the robustness of the party strength effect illustrated in Figures 4.9 and 4.10; specifically, I do not test a model that includes an experimental condition * party strength interaction as well as an experimental condition * source credibility interaction.

Concerning an overall impression of accuracy motivated reasoning, Figure 4.8 shows that, among source-attributed conditions, including a correction significantly lowers evaluations of the misinformation. In contrast, there is no change in evaluation between the *ctrl, no c* and *ctrl, c* conditions. While prior perceived credibility of the information sources may not explain these observations, some unidentified factor pertaining to the sources appears to influence the effect of the correction on evaluation of the misinformation argument.

I next present results for the correction argument evaluation in the same format as presented for evaluation to the misinformation argument. Figure 4.16 provides the mean condition estimates for the whole sample. In contrast to evaluation of the misinformation argument in the source-unattributed conditions, participants view the unattributed corrections highly unfavorably; the mean evaluation between the *ctrl, no m* and *ctrl, c* conditions is .27. Participants in the *ctrl, c* condition, on average, rate the misinformation argument about three times more favorably than they rate the correction argument.

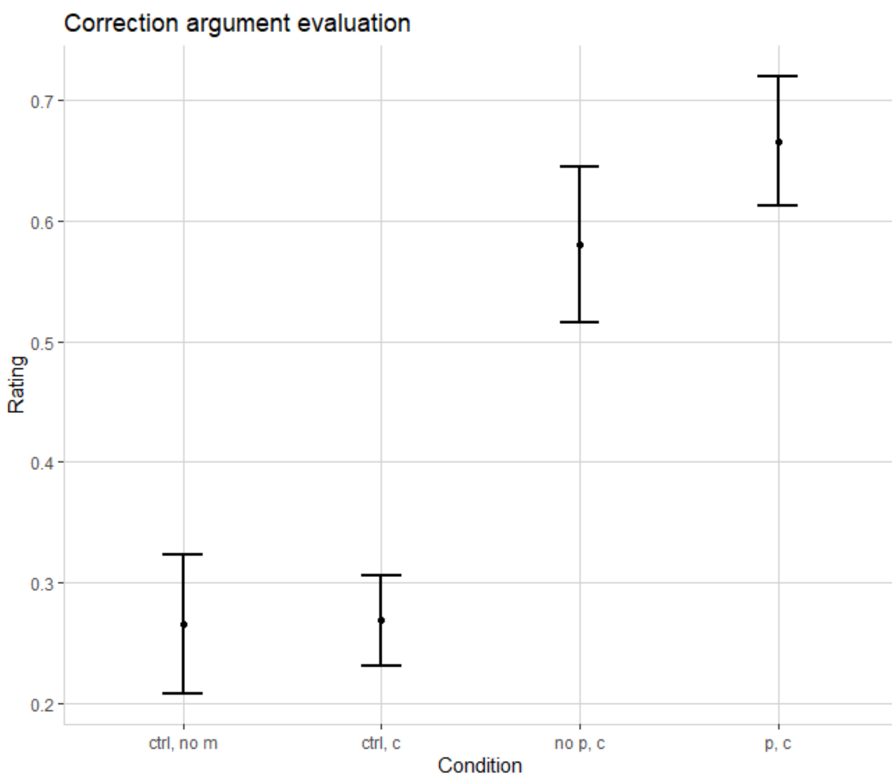

FIG. 4.16

Participants in the source-attributed conditions view the correction dramatically more favorably; the average change in evaluation between the source-unattributed and source-attributed conditions is .34. Identifying the source of the misinformation argument as a Democrat produces an additional .09 bump in favorability from the cue-omitted condition. Linear regression estimates reveal that this difference is statistically significant at $p < .05$.

However, unlike evaluations for the misinformation argument, the change in favorability for the correction between the *no p, c* and *p, c* conditions is not driven by strong Democrats. Figure 4.17 shows that both strong and weak/lean Democrats increase their evaluation of the correction between these conditions.

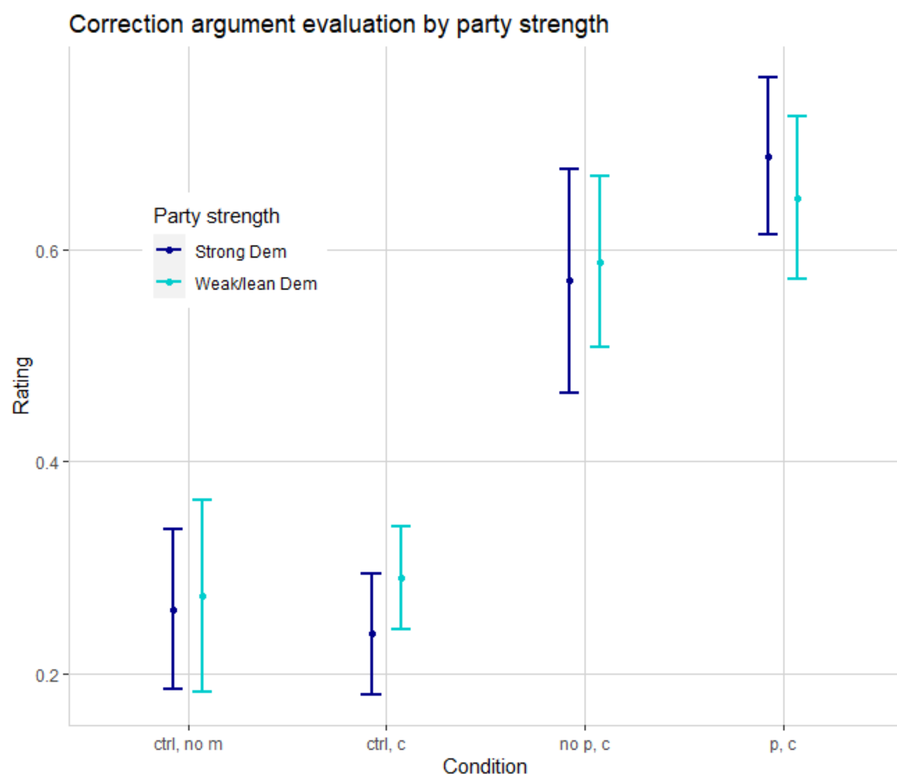

FIG. 4.17

Figure 4.18 breaks evaluations down by prior threat perception.[25] Between the *ctrl, c* and *no p, c* conditions, participants that came in viewing climate change as imminently threatening

[25] Because the pilot study did not include questions pertaining to climate change threat perceptions, I am unable to estimate these heterogenous treatment effects in the *ctrl, no m* condition, which was only included in the pilot study.

held onto lower views of the correction than participants with lower prior threat perceptions. However, regression estimates reveal that this difference-in-difference is only significant at p = .07. Between the *no p, c* and *p, c* conditions, the high-threat group increases evaluations while the low-threat group's mean estimate decreases slightly.

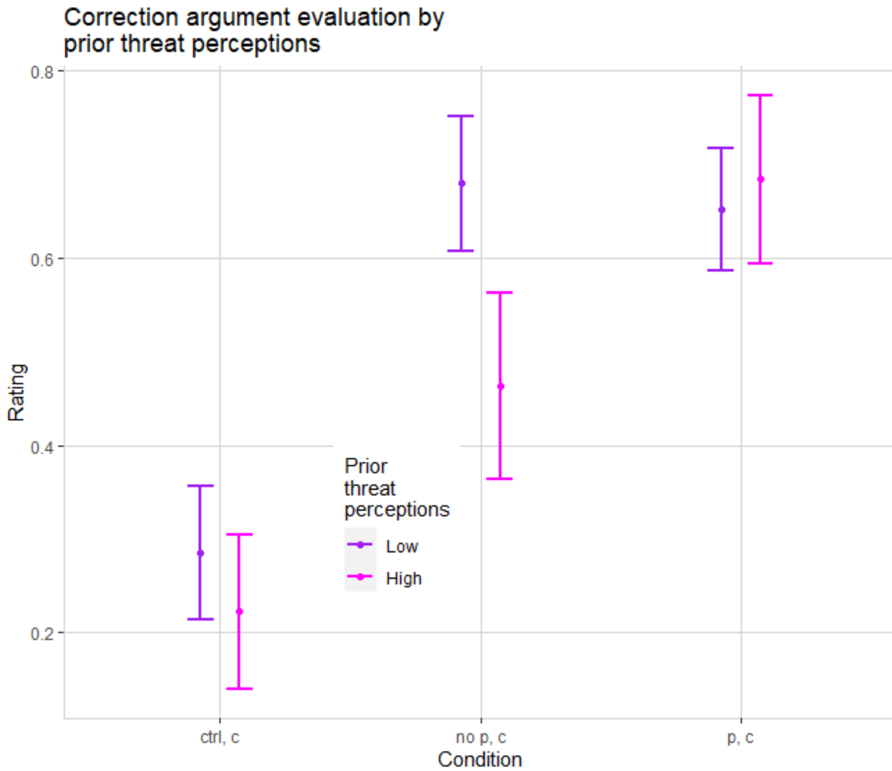

FIG. 4.18

Figure 4.19 shows estimates from a regression model that interacts prior threat perception and experimental condition, with the base condition set as *no p, c* and the base threat group set as "low." The *ctrl c * high threat* term indicates that the difference in change among threat groups between the *ctrl, c* and *no p, c* conditions doesn't quite reach the threshold of statistical significance. The significant coefficient of *p, c * high threat* does verify that high-threat perceivers increased their evaluation significantly more than their low-threat counterparts between the *no p, c* and *p, c* conditions (by .25). Together, Figures 4.18 and 4.19 suggest that participants that came in viewing climate change as less threatening view the correction favorably once the misinformation source is attributed to a politician and the correction source is attributed to cliamte scientists, regardless whether the former is identified as a copartisan. However, high-threat perceivers hold on to relatively lower evaluations until the misinformation source is identified as a copartisan. This is similar pattern to that observed in Figures 4.10 and 4.11, concerning a backlash among stronger copartisans in evaluating the misinformation argument. To note, as seen from Figure 4.17, partisan strength is not responsible for these heterogenous treatment effects.

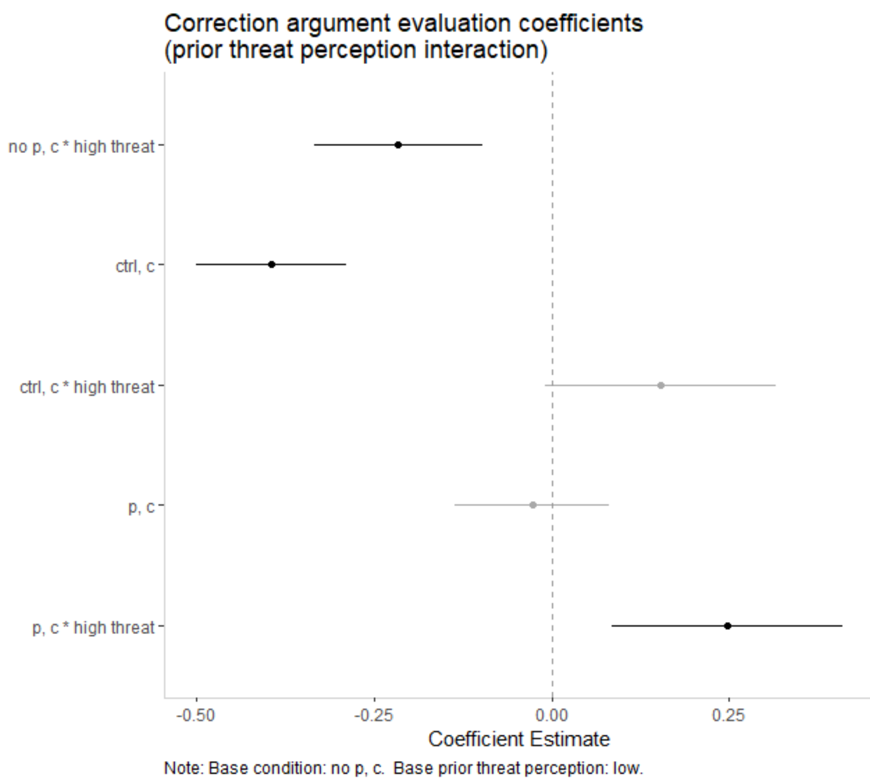

FIG. 4.19

Figures 4.20-4.22 break condition mean estimates down by prior perceived Democratic politician, scientist, and net scientist (scientist – Democratic politician) credibility, respectively. Again, prior perceived source credibility is dichotomized into low and high levels only in order to provide mean estimates in graphical form. Figures 4.20 and 4.22 show no heterogenous treatment effects by prior source credibility and regression estimates corroborate the absence of effects.

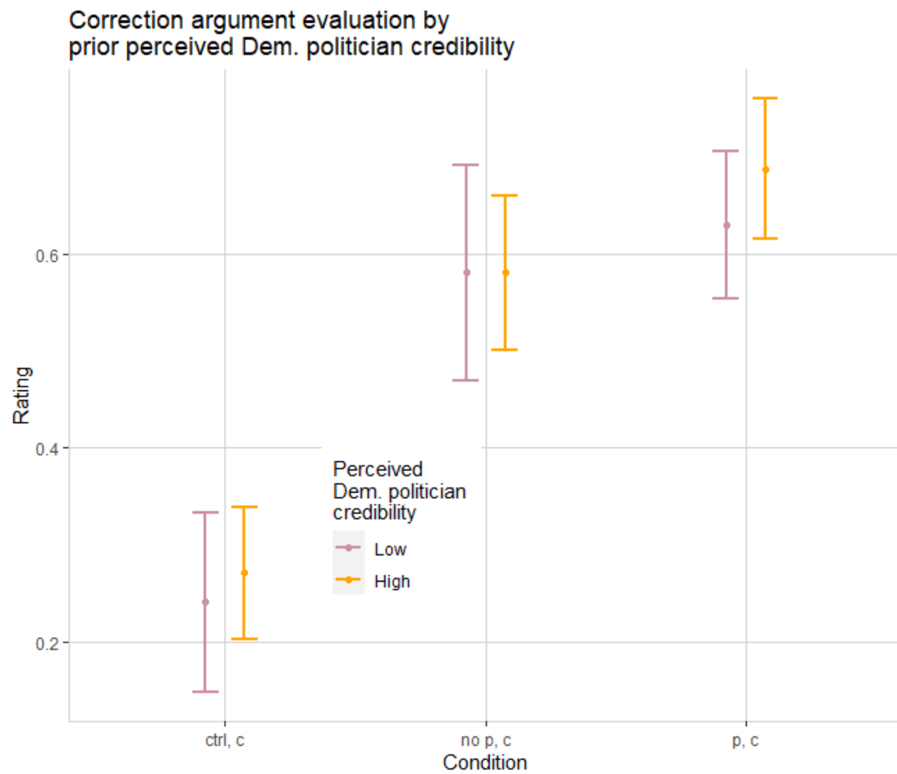

FIG. 4.20

However, Figure 4.21 shows that participants that came in viewing scientists as highly credible evaluate the correction significantly lower than their counterparts when sources are unattributed. When sources are attributed, the high-credibility group catches up so that the group estimates are near-identical. Figure 4.23 provides regression estimates that confirm a heterogenous treatment effect between the *ctrl, c* and *p, c* conditions, and a near-significant effect between the *ctrl, c* and *no p, c* conditions. This finding suggests that participants evaluate the correction argument partially as a function of how credible they view the source of the correction.

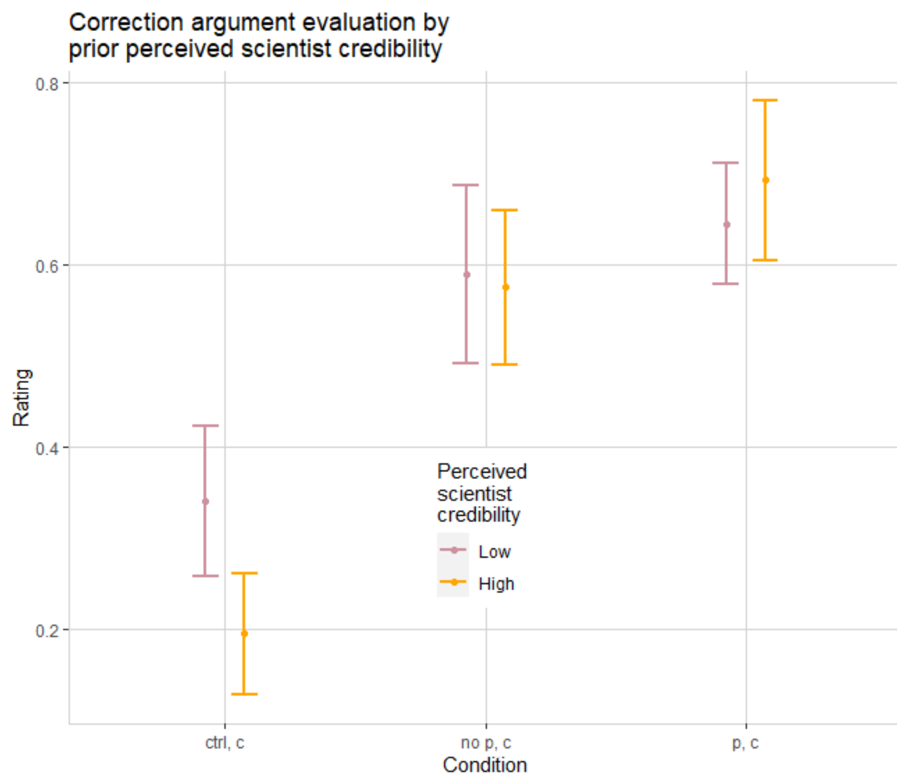

FIG. 4.21

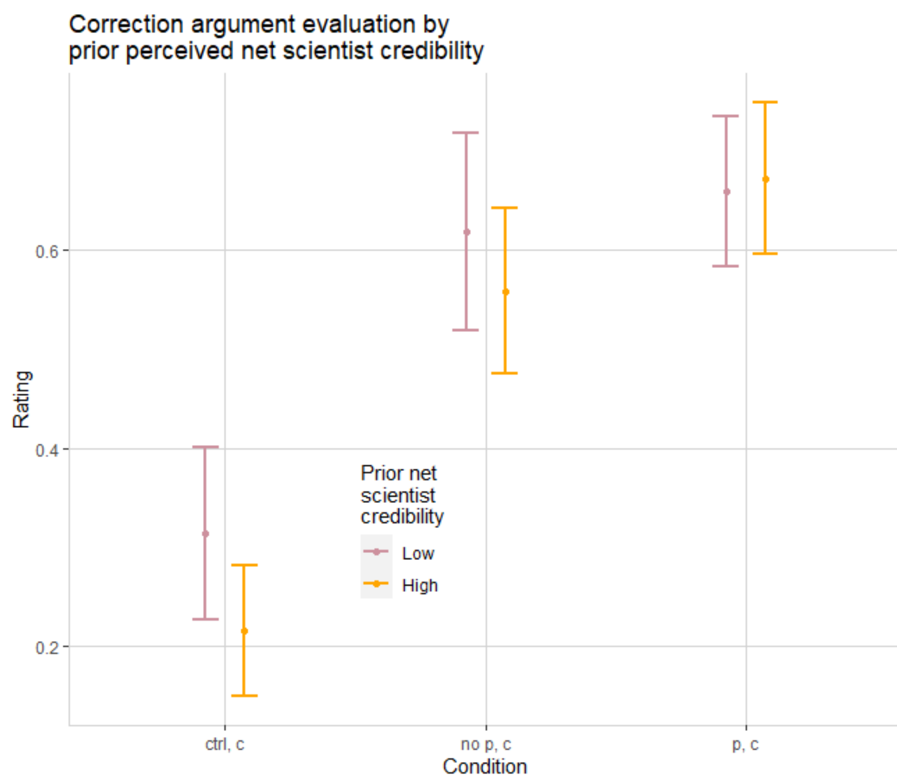

FIG. 4.22

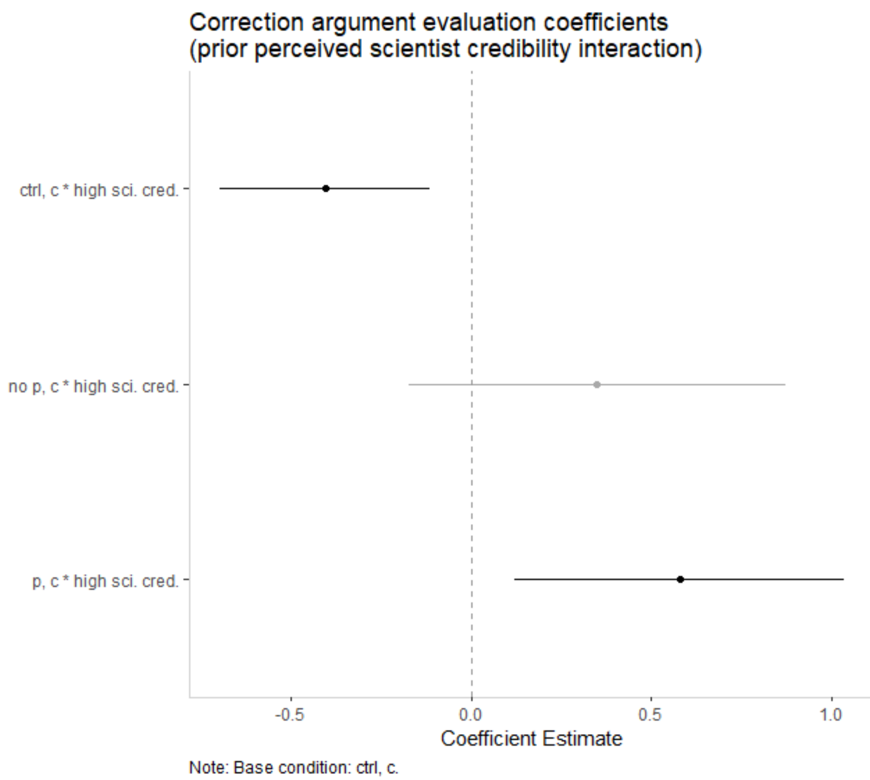

FIG. 4.23

I last investigate responses to the item that asks respondents to directly compare the persuasiveness of the misinformation and correction arguments.[26] Higher values indicate that the correction is more persuasive. Participants in the *ctrl, c* condition view the misinformation as much more persuasive than the correction. When sources are attributed but no party cue is present, participants significantly move toward viewing the correction as more persuasive, but still on average view the misinformation argument as more persuasive. When the party cue is present, copartisans significantly move again, in absolute terms, to view the arguments as nearly equally persuasive.

[26] Observations concerning the direct argument comparison closely track with net correction evaluations (i.e. evaluation of the correction argument – evaluation of the misinformation argument). Therefore I do not present findings on net correction evaluations.

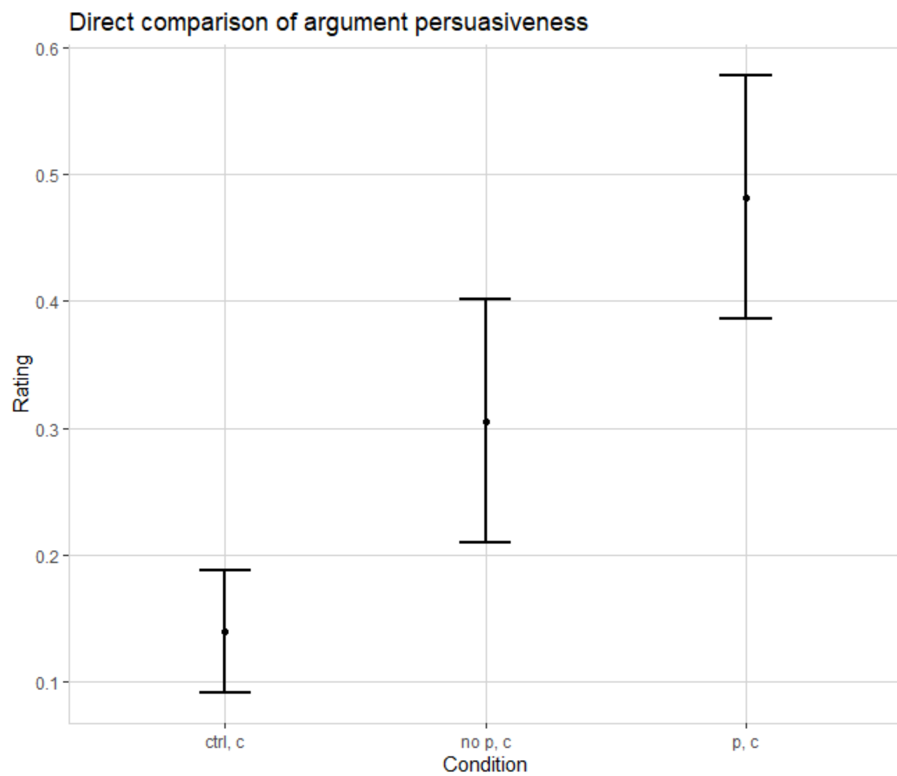

FIG. 4.24

Similar to observations of correction argument evaluations, Figure 4.25 indicates that the change in mean response between the *no p, c* and *p, c* conditions is not driven by stronger Democrats. Figure 4.26 also provides a pattern similar to that observed in the correction argument evaluations; participants with stronger prior climate change threat perceptions held onto a more favorable view of the misinformation argument in the source-attributed conditions. However, the difference-in-difference between the *ctrl c* and *no p, c* or *p, c* conditions is not significant ($p > .1$).

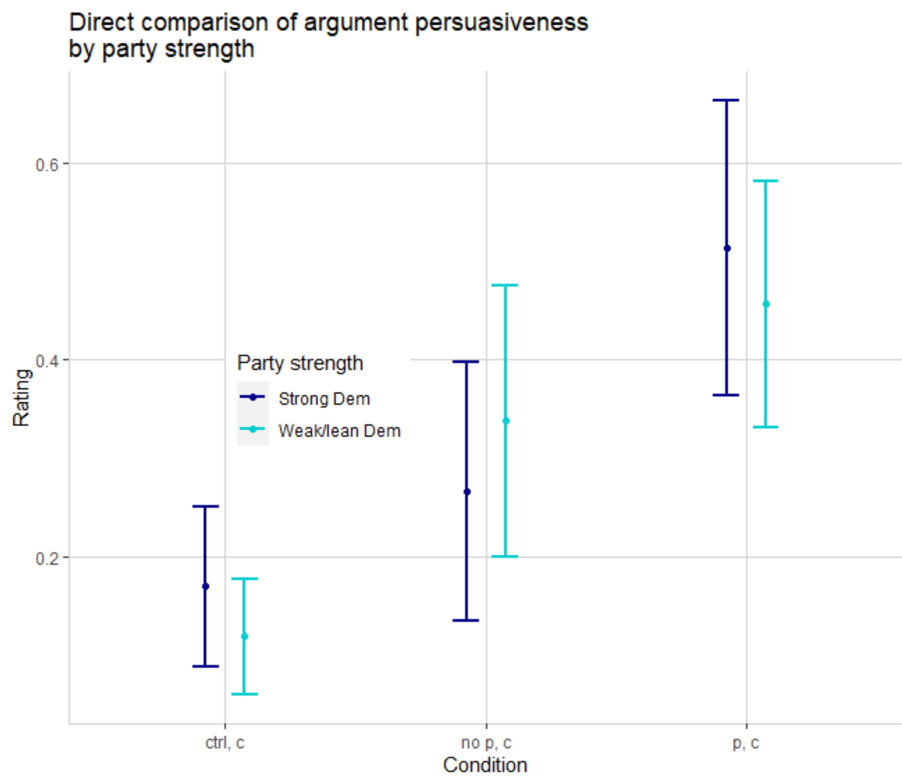

FIG. 4.25

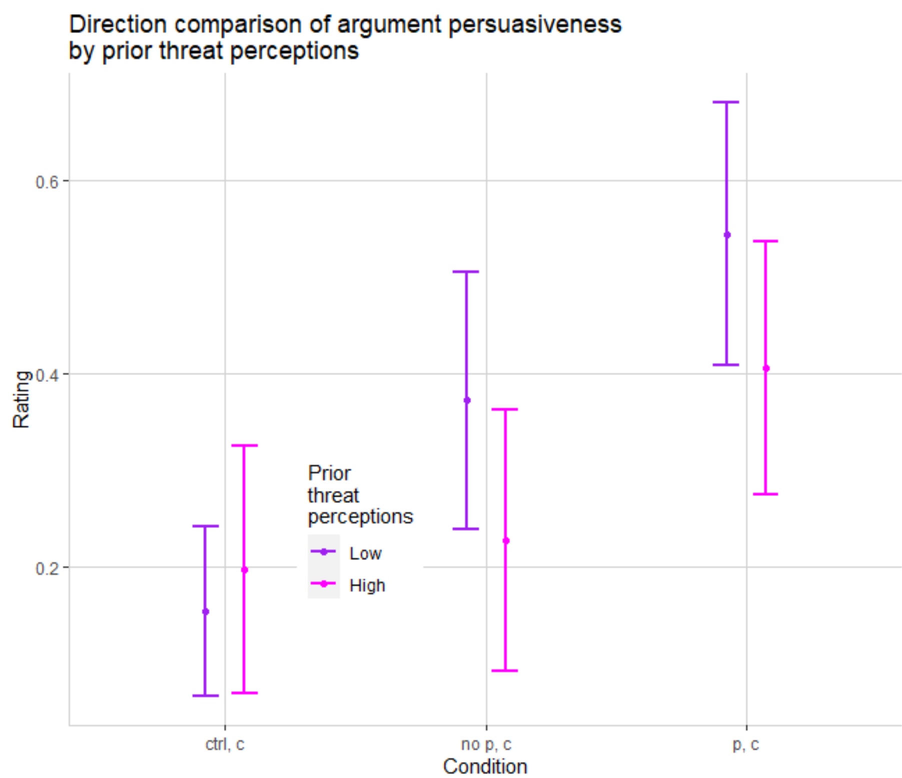

FIG. 4.26

Concerning the effect of prior source credibility, Figure 4.27 shows (and regression analysis corroborates) no heterogenous treatment effect between source-unattributed and source-attributed conditions. Interestingly, participants that came in viewing copartisan politicians as highly credible view the correction as significantly more persuasive between the *no p, c* and *p, c* conditions, while those that came in with lesser views of politicians view the misinformation argument as slightly more persuasive. Regression confirms that this difference-in-difference is significant (p < .05). However, it is wholly unclear as to how a heterogenous treatment effect between these two source-attributed conditions relates to the accuracy motivated reasoning hypothesis. Additionally, that participants with higher perceived Democratic politician credibility view the correction more favorably than their counterparts in any source-attributed condition is counterintuitive.

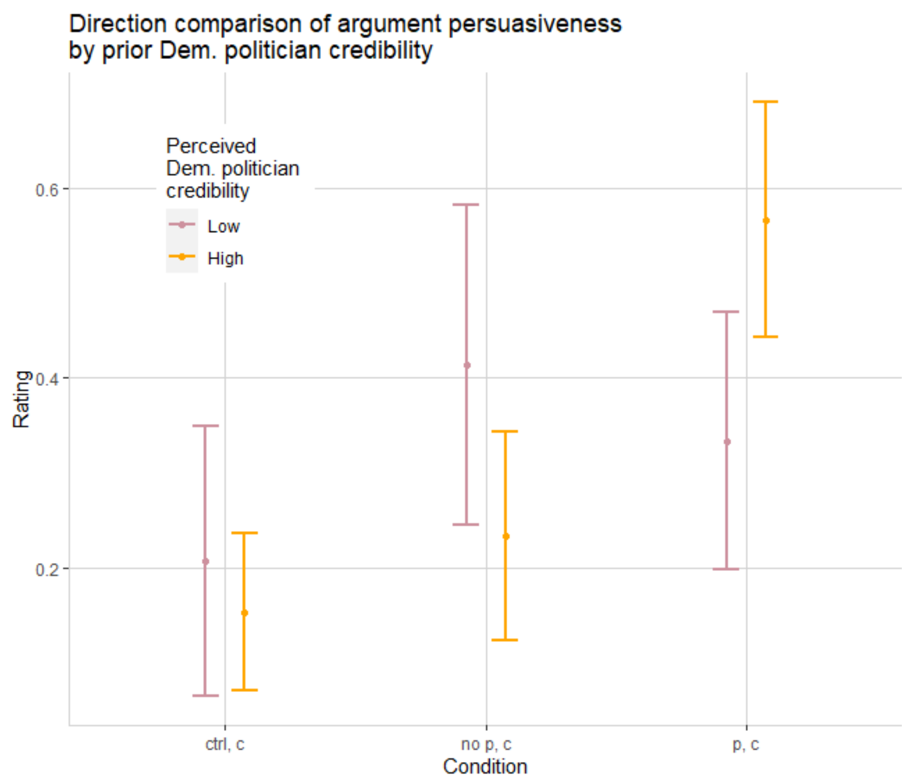

FIG. 4.27

Figures 4.28 and 4.29 respectively show no sign that prior perceived scientist source credibility influences how participants compare the persuasiveness of each argument.

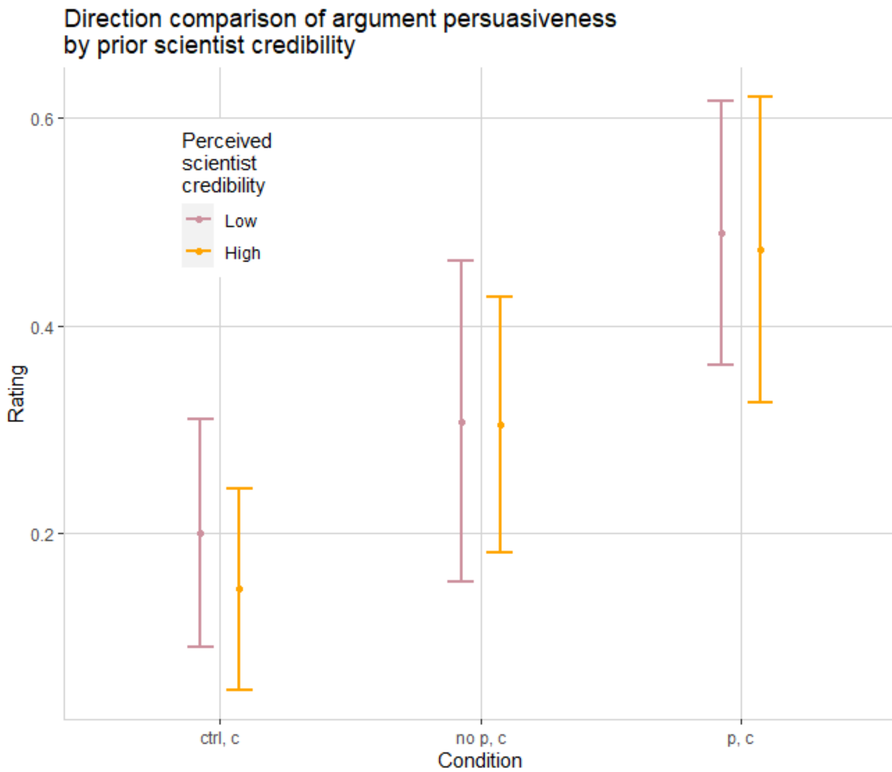

FIG. 4.28

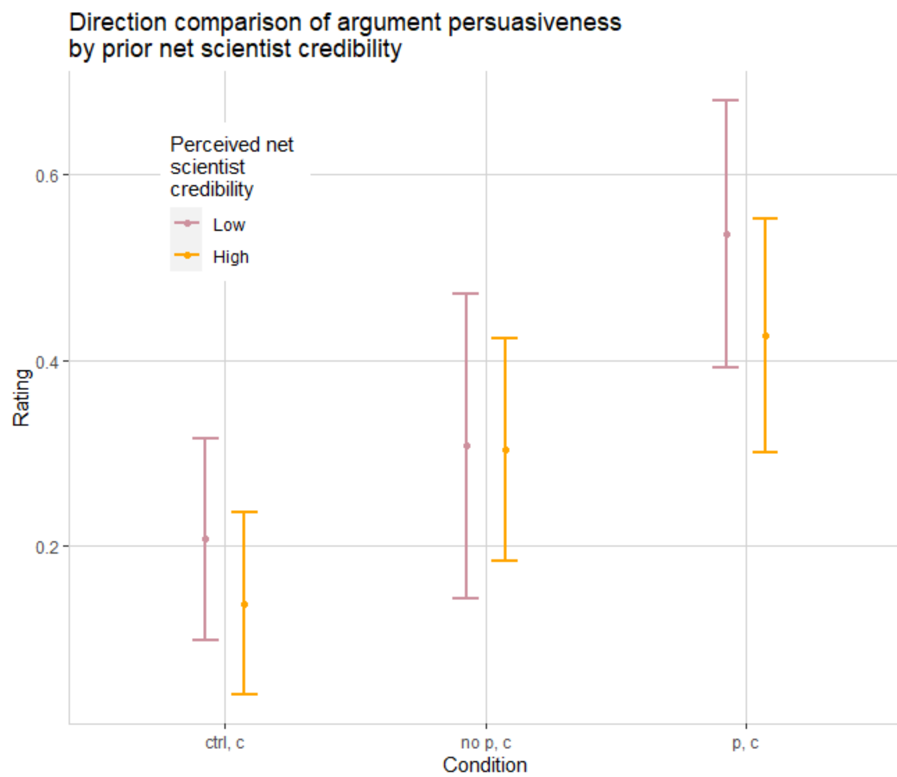

FIG. 4.29

Discussion

Overall, participants do not respond to the treatment arguments in a manner consistent with partisan motivated reasoning. As participants are provided, in progression, information that the misinformation argument comes from a politician; a copartisan cue, and a correction by scientists, they view the misinformation argument more negatively. Similarly, participants view the correction more favorably as they are provided source attributions, and then additionally a misinformation argument copartisan cue. It could reasonably be argued that these trends are consistent with accuracy motivated reasoning – participants follow the experts when they are identified as such and when their arguments conflict with non-experts – but it is certainly inconsistent with partisan motivated reasoning.

The heterogenous treatment effects observed here also indicate a limited and inconsistent trend of directional motivated reasoning, which, when a correction follows the misinformation, is resolved by cueing respondents that the misinformation comes from a copartisan. Concerning the misinformation argument, there is suggestive evidence that partisan strength moderates the influence of the treatment between the *ctrl, c* and *no p, c* conditions. Once a party cue is present, however, strong Democrats' evaluations decrease significantly more than weak/lean Democrats, so that the mean estimates between both groups of partisans are statistically indistinguishable. An identical pattern of heterogenous treatment effects is observed in the correction between participants with higher and lower prior climate change threat perceptions. The high-threat group retains suggestively lower views of the correction between the *ctrl, c* and *no p, c* conditions, but raises their evaluation significantly more than the low threat group between the *no p*, *c* and *p*, *c* conditions.

In sum, attributing the misinformation argument to an unaffiliated senator and the correction to climate scientists results in increased difference in argument evaluations consistent with directional motivated reasoning. However, identifying the senator as a Democrat significantly reduces this difference by leading strong Democrats to lower their opinion of the misinformation argument to join weak/lean Democrats, and by leading high-threat perceivers to increase their opinion of the correction to join the mean estimate of the low-threat group.

No prior threat-based heterogenous treatment effects are observed concerning the misinformation argument, and no party strength-based difference-in-differences are seen in evaluations of the correction or direct comparison of persuasiveness between arguments. One could partially rationalize these observed inconsistencies through the following; because the Democratic cue is attached to the source of the misinformation per se, a party strength effect should be most apparent in evaluation of the misinformation argument. And because the correction focuses on disputing the severity of climate change – and the correction does not comment on a call to action like the misinformation argument does – any prior threat effect should be strongest in evaluation of the correction. However, it's difficult to conceive of establishing these particularities a priori, and instead they may lean towards post hoc justifications.

The copartisan backlash effect is particularly striking and worth taking note of, since it goes against most previous findings concerning the positive effects of copartisan source cues (e.g. "following the leader" a la Lenz (2013)). Including a party cue pushes copartisans away from the misinformation argument, and perversely does so among strong Democrats. The cue also pushes all copartisans toward the correction. It is not obvious as to why the party cue causes these effects. It's possible that participants are skeptical of the motivations of any political party when calling for policy action. In which case, experimentally alternating the party source cue between Democrat and Republican may provide further insight. It is also entirely possible that participants recognized the research motivation for including the cue and reacted against it in their argument evaluations; they may not want to be seen as following the leader and so correspondingly modified their evaluations. Replicating this experiment among a wider swath of the Democratic public may allow for identification of possible survey response desirability bias in this particular sample of respondents.

While there is limited evidence of directional motivated reasoning, there is even less *direct* support for the notion of information source credibility as a mechanism of accuracy motivation. The extent of direct evidence for this mechanism is that participants that came in viewing scientists as relatively more credible evaluate the correction more highly once it is attributed to climate scientists. However, this does not extend to the direct comparison of argument persuasiveness, nor is misinformation argument evaluation a function of prior perceived credibility of either information source. Thus Druckman and McGrath's (2019) concerns that previous findings of directional motivated reasoning may be invalidated by an unobserved confounder are not substantiated here.

That being said, there is substantial *indirect* support for the influence of perceived information source credibility in producing evaluations consistent with accuracy motivated reasoning. The correction has a significant effect on misinformation argument evaluation only when sources are attributed. Likewise, attributing argument sources substantially improves evaluations of the correction and its perceived persuasiveness.

The discrepancy between absence of direct support via the items use to measure information source credibility and the clear influence that the source attributions have on argument evaluations may be explained by the quality of the instrument used to measure source credibility. The "trust" and "expertise" items that constitute the measure may not effectively capture the desired constructs. Alternatively, as stated previously, the experimental treatments do not perfectly allow me isolate the influence of argument source attributions; it may be some factor unique to differently presenting the information in a newspaper format in the source-attributed conditions that affects argument evaluations.

In total, it appears that respondents evaluate arguments in a manner largely consistent with accuracy motivated reasoning. There are some differences in argument evaluations between respondents with different priors, but again these are resolved once respondents are provided information most closely resembling that which they would receive in a news report of political events: i.e. the identity of the actors involved, including the partisanship of political actors. In other words, results involving the *p, c* condition, where the treatment roughly mirrors a full news article, indicate that respondents follow the experts.

This result stands in contrast to previous work that finds Republicans follow denialist climate change statements from copartisan elites, even when this misinformation is debunked by climate scientists (Brulle, Carmichael, and Jenkins 2012; Jacques, Dunlap, and Freeman 2008; Kahan 2012, 2015; McCright and Dunlap 2011; Tesler 2018). Given that this is the only climate change opinion study of its kind to look at Democrats' tendencies to follow Democratic elites, it is difficult to isolate the factors that might lead to different outcomes between partisans. However, because most other studies examine *opinions* about the issue at hand, rather than evaluations of the information provided, I will reserve speculation until the discussion in the next chapter. Chapter 5 picks up with presenting an analogous set of hypotheses and results pertaining to climate change opinions, then concludes with a wholistic discussion of the results from both Chapters 4 and 5.

Chapter 5: Experimental assessments of causes of climate change opinions

Introduction and hypotheses

In this chapter I provide the second half of the results of my motivated reasoning survey experiment. I restate then test the hypotheses presented in Chapter 4 on three dependent variables related to opinions about climate change: intra-participant change in climate change threat perceptions, a larger scaled set of threat perception items, and support for federal policies aimed at reducing greenhouse gas emissions. I then discuss the implications of the combined set of experiment results: those pertaining to treatment argument evaluations and climate change opinions.

I assess for the presence of directional motivated reasoning pertaining to participants' partisanship and prior attitudes about climate change. The partisan motivated reasoning hypothesis predicts that Democratic participants (which constitutes the entire sample) follow the misinformation argument by heightening their climate change threat perceptions and support for mitigation policies when the exaggerative misinformation argument is attributed to a copartisan. As a harder test of partisan motivated reasoning, I also expect that participants' strength of Democratic affiliation moderates the influence of the copartisan cue on responses to the dependent variables; strong Democrats should be more responsive to the cue than their weak/lean counterparts.

The prior attitude effect hypothesis predicts that participants' prior climate change threat perceptions moderates the effect of the treatment arguments on participants' formation of climate change opinions. In particular, participants with high prior threat perceptions should be resistant to the correction by retaining or increasing their threat perceptions and support for climate change policies relative to participants with low prior threat perceptions.

I also make a competing hypothesis that participants' opinion formation is motivated by accuracy goals: participants should respond to the correction by lowering their threat perceptions and policy support. The theorized mechanism of accuracy motivated reasoning is how credible participants view the sources of information prior to treatment, with the expectation that participants that view scientists as more credible – and/or Democratic politicians as less credible – should respond more to the correction-included treatments than their counterparts in a manner consistent with accuracy motivated reasoning. Prior perceived argument source credibility is expected to moderate the influence of the correction on climate change opinions. A harder test of the accuracy motivated reasoning hypothesis is then assessing for the presence of this moderating influence.

Evaluating the influence of prior perceived source credibility has the added benefit of accounting for what Druckman and McGrath (2019) theorize is frequently an unobserved confounder of directional motivated reasoning observed in previous studies. They argue that, without also accounting for "disagreement over who constitutes a credible source" (Druckman and McGrath 2019, p. 116), extant findings of directional motivated reasoning may be spurious, as moderators associated with directional motivated reasoning are likely correlated with perceived source credibility. For instance, Democrats may follow copartisan elites because they deem them credible sources of information, so by controlling for prior perceived source credibility, I am conducting a sounder test of the partisan motivated reasoning hypothesis.

Hypothesis tests concerned with the moderating influence of participant characteristics measured before treatment (i.e. party strength, prior threat perceptions, and prior perceived source credibility) are tested through difference-in-difference regression estimates; I interact a treatment condition dummy variable with the relevant moderator and evaluate the significance of the interaction term coefficient. In other words, I evaluate whether the influence of the moderator changes significantly between some baseline treatment condition (often the control group) and the treatment condition relevant to the hypothesis test.

I find little evidence of partisan motivated reasoning; the copartisan cue has small influence on climate change opinion in the overall sample. However I do find heterogenous treatment effects between strong and weak/lean Democrats in the condition that includes both a party cue and a correction: strong Democrats hold onto higher threat perceptions than weak/lean Democrats in this case, but the same difference-in-difference effect does not hold for policy support. I find that prior threat perceptions do not influence the effect of the treatment on threat perceptions or policy support, so that, taken together, there is relatively weak and inconsistent evidence of directional motivated reasoning.

Consistent with the “easier” test of the accuracy motivated reasoning hypothesis, I find that the correction results in significantly more negative intra-participant change in threat perceptions, such that participants in the correction-included conditions perceive climate change as significantly less threatening after treatment, relative to participants in the control condition. Although, it does not appear that prior perceived source credibility acts as a mechanism or indicator of accuracy motivated reasoning. The effect of the correction does not extend to support for climate change policies, so that participants maintain support for policies aiming to mitigate climate change in spite of lowered threat perceptions. I discuss the implications of the combined set of findings from this chapter and the previous chapter in the discussion section.

Results

I first evaluate the change in response to two climate change threat severity items: one asks when sea level rise will be a serious problem for people in the U.S. and the second asks when climate change generally will be a serious problem. These items were included among the science knowledge battery in all conditions, and the average to these pre-treatment items constitutes the prior climate change threat perception scale used here and in the previous chapter. These items were included again among the set of climate change opinion items: in the control (*ctrl*) conditions,[27] they preceded presentation of the treatment arguments. In the treatment conditions, they were provided after the treatment arguments, so that the arguments could affect responses in these conditions. A change in threat perception value is obtained for each respondent by subtracting their initial threat perception score from their second one. The resulting value can range from -1 (indicating movement from viewing climate change as an imminent threat to no threat at all) to 0 (indicating no change) to +1.

Distributions for responses to the second and initial threat perception items are both heavily left skewed; 219 participants responded with the highest level of threat perceptions in the initial questions, and 176 of these participants maintained this level of threat perceptions in the second set of questions. Given that 45% of participants can only decrease or maintain their threat perceptions, and given the strong intra-participant stability of responses to these items, this dependent variable provides particularly difficult tests of both directional and accuracy motivated reasoning.

[27] The correction-omitted and correction-included control conditions (*ctrl, no c* and *ctrl, c,* respectively) are pooled here because the treatment arguments can have no bearing on participants' climate change opinions in these conditions.

Figure 5.1 provides the condition-based mean estimates for the change in climate change threat perceptions. As seen in the *ctrl* condition estimate, simply asking the questions a second time, and/or perhaps, asking them in a different context, elicits responses that indicate a less threatened perception of climate change. Any remaining difference between conditions should be a product of exposure to the treatment arguments. While attributing the misinformation argument to an unaffiliated senator does not prompt a substantial change in threat perceptions (*no p, no c* condition), adding a copartisan source cue produces a lower threat perception relative to the control (*no p, c* condition).

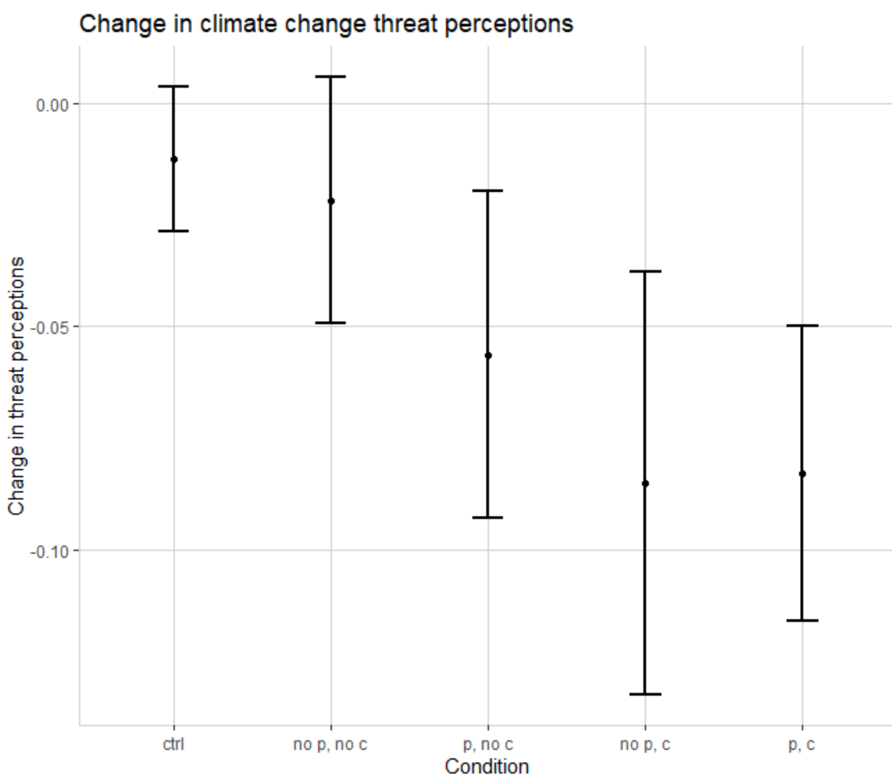

FIG. 5.1

This difference is verified as statistically significant ($p < .05$) in Figure 5.2, which illustrates coefficient estimates from regression analysis where experimental condition is treated a dummy variable, with the *ctrl* condition as the reference category. Including a correction by scientists lowers threat perceptions even more, though an additional misinformation argument party cue does not affect change in threat perceptions (*no p, c* and *p, c* conditions, respectively). Although not shown here, regression analysis using the *no p, no c* condition as the base dummy condition also reveals significant differences in estimates between the base condition and the two correction-included conditions (B = -.06, SE = .02, $p < .05$).

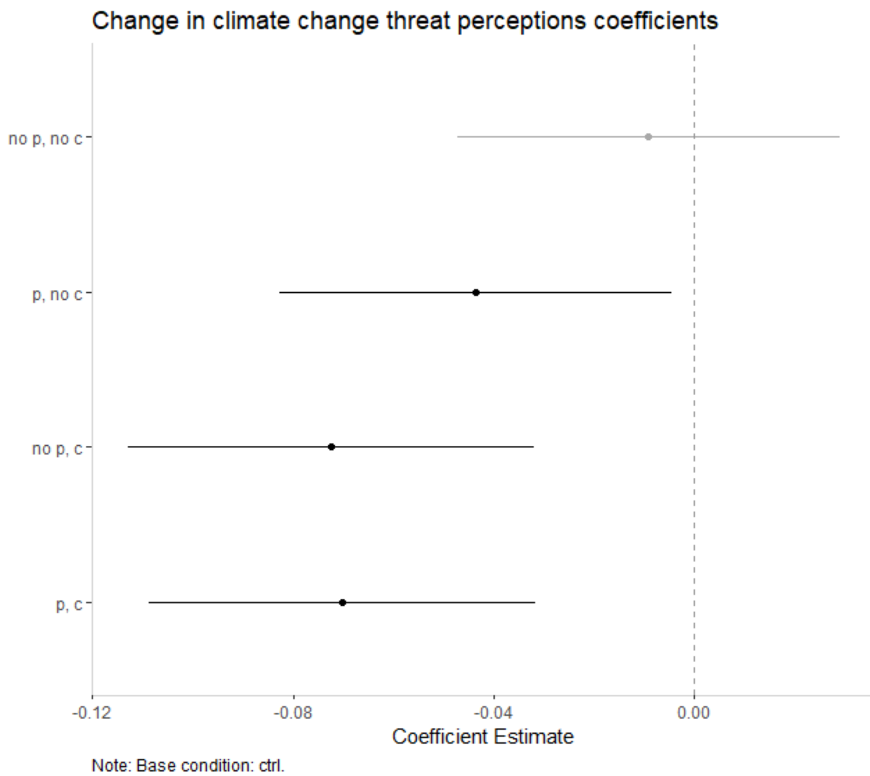

FIG. 5.2

Figure 5.3 shows change in threat perceptions by strength of Democratic partisanship. From left to right across conditions, there is a consistent downward shift in threat perceptions among weak/lean Democrats, while strong Democrats hang on to higher threat perceptions as the arguments are attributed and corrections are included.

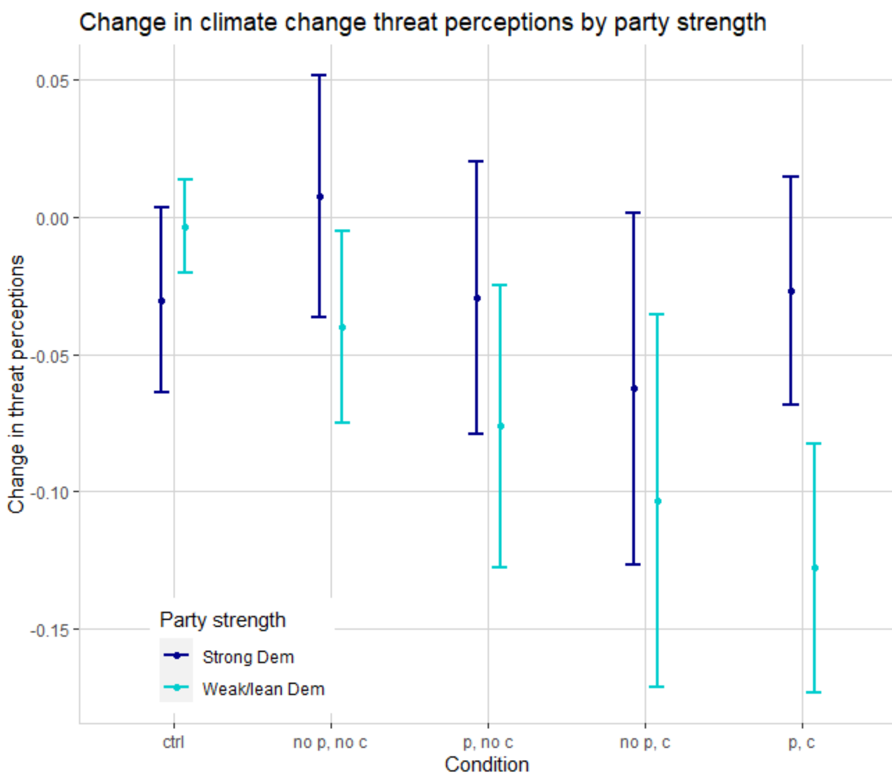

FIG. 5.3

Figure 5.4 shows that, between *ctrl* and *p, c* conditions, there is a significant difference-in-difference whereby weak/lean Democrats' threat perceptions decline significantly more than their strongly affiliated copartisans (B = -.13, SE = .04).

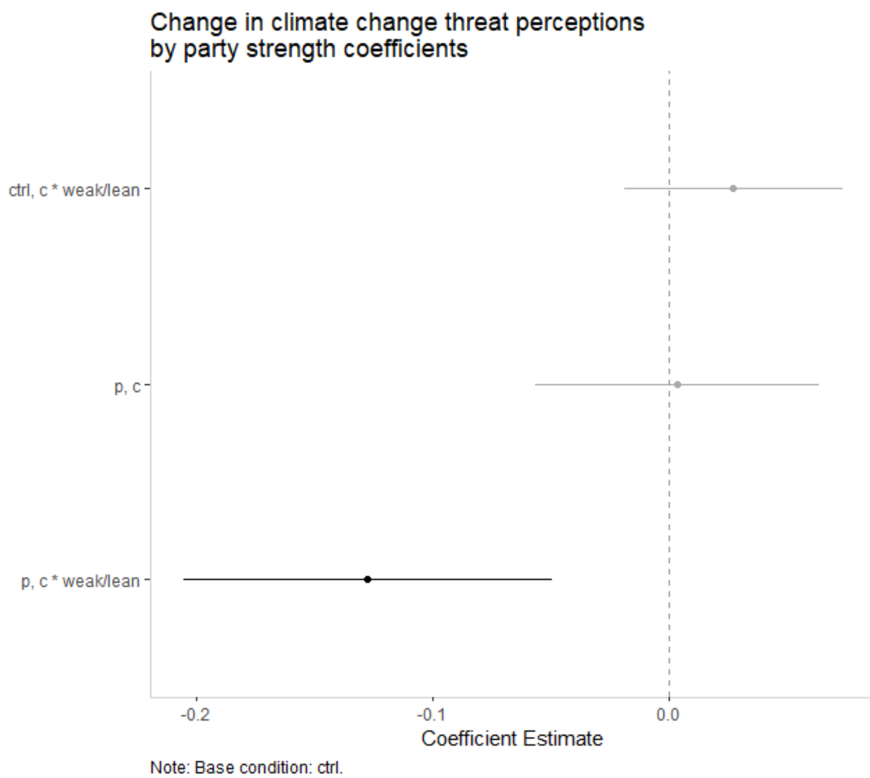

FIG. 5.4

There are no significant heterogenous treatment effects by party strength between party cue-omitted and party cue-included conditions, or between any two treatment conditions more generally. Holistically then, it might be that source attribution, a party cue, and a correction are all necessary to produce an effect consistent with the partisan motivated reasoning hypothesis.

Figure 5.5 illustrates threat perception change by prior threat perceptions. There is no evidence that individuals that came in viewing climate change as imminently threatening tend to hold onto their threat perceptions significantly more than their counterparts in any experimental condition. Nor is there any indication of a heterogenous treatment effect across conditions. Nor does collapsing corresponding party cue-omitted and party cue-included conditions produce substantively different results.

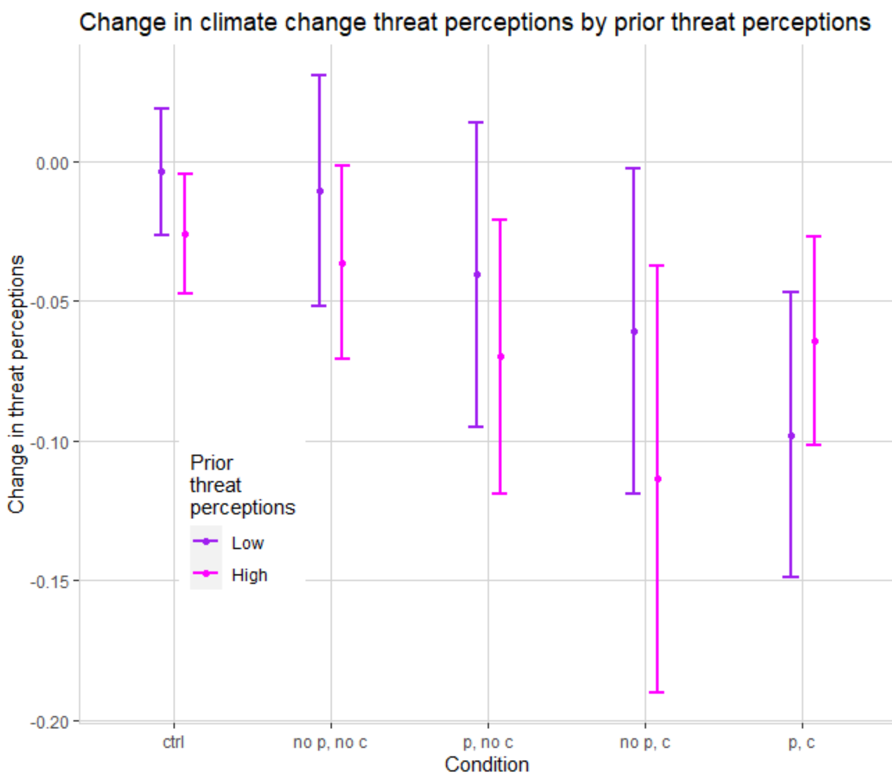

FIG. 5.5

Figures 5.6-5.8 show change in threat perceptions across conditions and split by high and low perceived information source credibility. Per the accuracy motivated reasoning hypothesis, between conditions where an argument is not attributed to a given source and a condition where it is attributed to a source, difference in change in climate threat perceptions between these conditions should be a function of how credible participants came in viewing that source. For instance, because the misinformation argument expounds climate change as an imminent and existential threat, between the *ctrl* and *p, no c* conditions, individuals that came in viewing Democratic politicians as highly credible should maintain high levels of threat perceptions, while their counterparts that have low prior perceived Democratic politician credibility should have a significantly more negative change in threat perceptions in the *p, no c* condition compared to in the *ctrl* condition.

Figure 5.6 reveals no differences in how threat perceptions change by whether individuals came into the experiment viewing Democratic politicians as highly credible or less credible. Regression analysis, where prior perceived source credibility is given the full range of possible values and is treated as a continuous variable, confirms no significant differences across conditions.

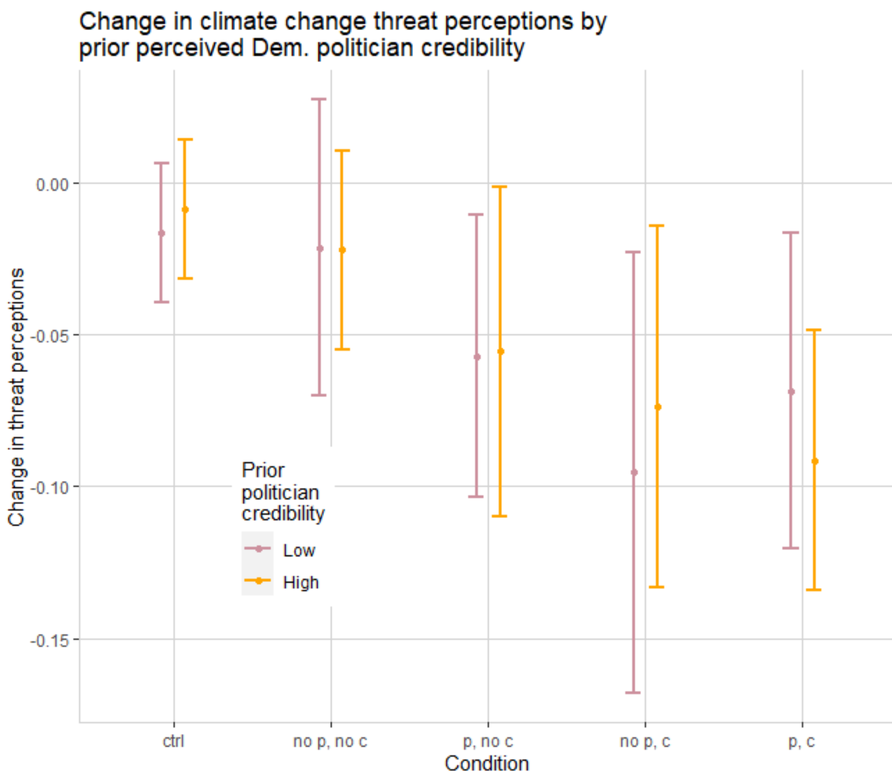

FIG. 5.6

Similarly, Figure 5.7 shows no heterogenous treatment effects by prior perceived scientist credibility.

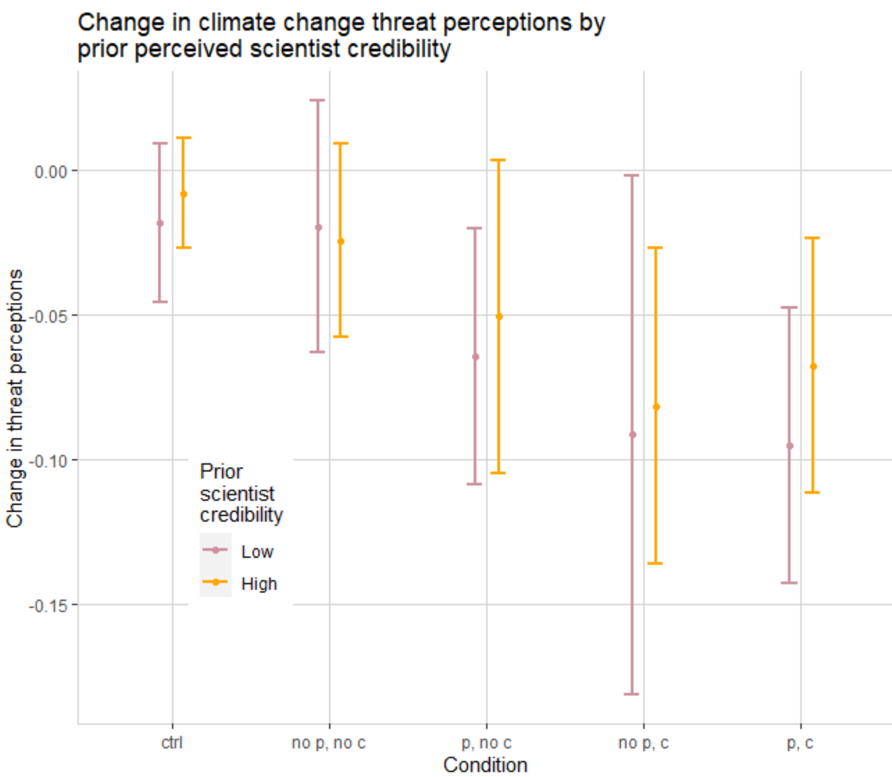

FIG. 5.7

Revealed by Figure 5.8, when a participants' perceived scientist credibility is subtracted from their perceived Democratic politician credibility to form a measure of "prior perceived net scientist credibility," there is an interesting difference-in-difference effect between the *no p, no c* and *no p, c* conditions. Between the *no p, no c* and *no p, c* conditions, regression confirms that higher relative perceived scientist credibility correlates in a greater drop in threat perceptions, relative to participants that came in viewing Democratic politicians as more credible (B = -.12, SE = .05). Because the correction by scientists argues for a more moderate threat posed by climate change, this difference-in-difference provides evidence in favor of the accuracy motivated reasoning hypothesis.

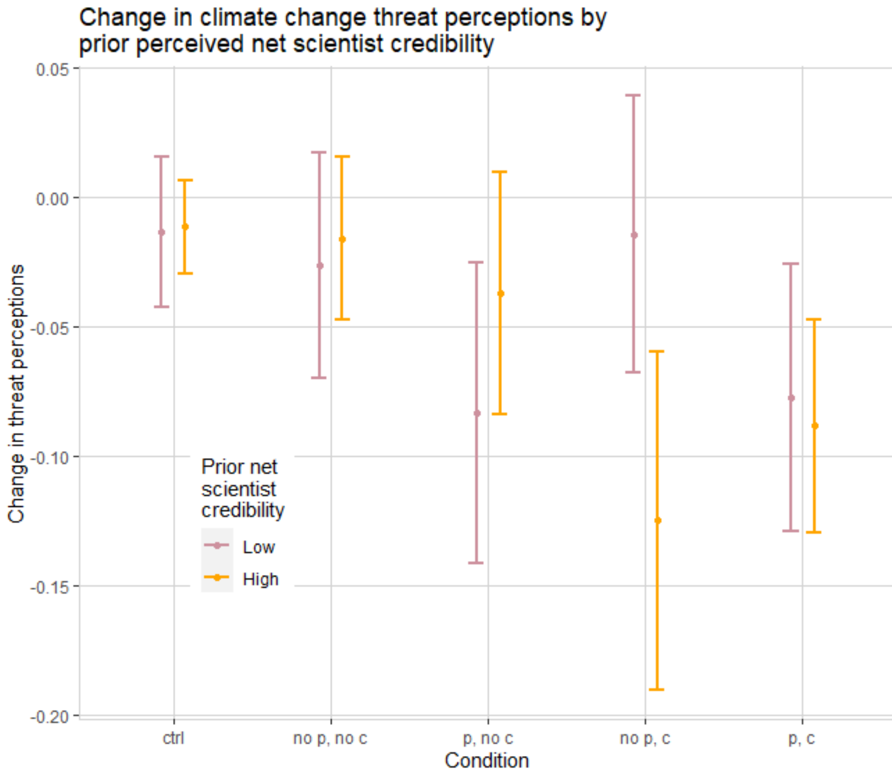

FIG. 5.8

However, once the senator is identified as a Democrat in the *p, c* condition, the significant difference observed in the *no p, c* condition disappears, as change in threat perceptions becomes relatively more negative among participants that view Democratic politicians as relatively more credible. The difference-in-difference between *no p, c* and *p, c* conditions is directionally opposite to what is expected by the accuracy motivated reasoning hypothesis, where participants with relatively higher prior perceived Democratic politician credibility are expected to have relatively higher threat perceptions when the misinformation argument source is identified as a copartisan.

I next present results for the four-item climate change threat perception scale, which is formed from the average of the two items used to measure prior threat perceptions as well as two more threat perception items included among the post-treatment set of climate change opinion items. I refer to the scale as "climate change threat perceptions" to differentiate it from *prior* threat perceptions and *change* in threat perceptions. While the mean threat perception score is still quite high (mean = .8, median = .88) Figure 5.9 shows that there is room for some respondents to increase their threat perceptions relative to the control condition. Regression results indicate that the only significant difference in mean estimates occurs between the *no p*, *no c* and both correction-included conditions (for both differences, B = -.07, SE = .03). The .05 decreases in threat perceptions between the control and the correction-included conditions do not quite reach statistical significance (p = .13 for the *no p, c* condition and p = .11 for the *p, c* condition).

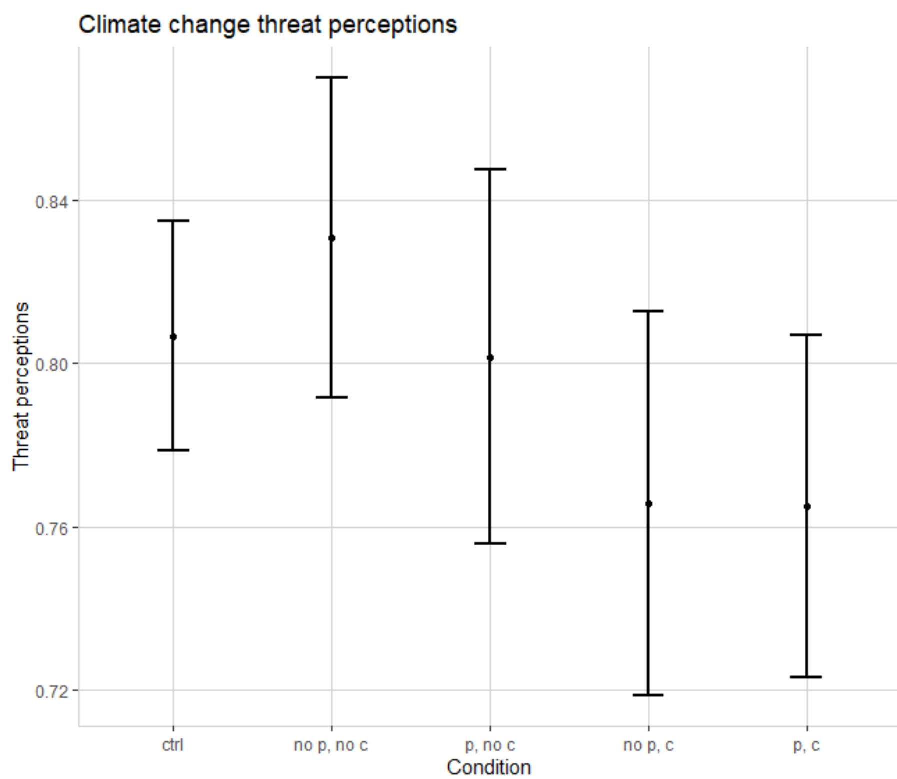

FIG. 5.9

Figure 5.10 illustrates a small heterogenous treatment effect by party strength between the control and treatment conditions. In contrast to the change in threat perceptions estimates highlighted in Figures 5.3 and 5.4, however, there is no indication that presence of a copartisan cue grows the difference in mean estimates between strong and weak/lean Democrats. Moreover, none of the difference-in-differences are statistically significant.

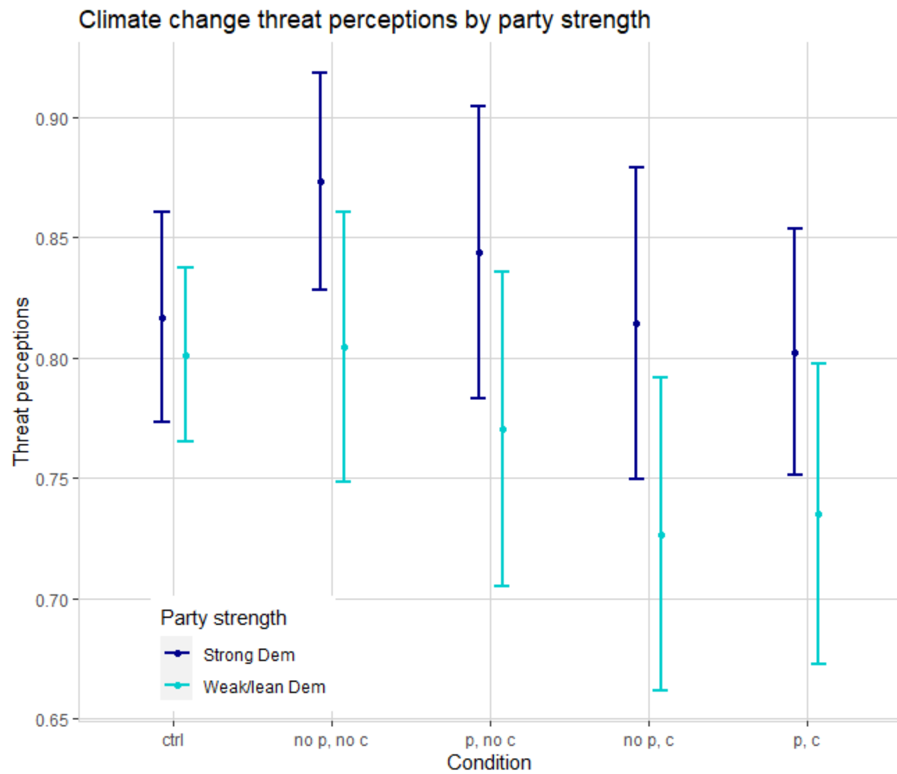

FIG. 5.10

Figure 5.11 shows no evidence of heterogenous treatments by prior climate change threat perceptions. The large gaps between high- and low-threat groups is a product of the intra-participant stability in perceived threat perceptions observed in Figure 5.5.

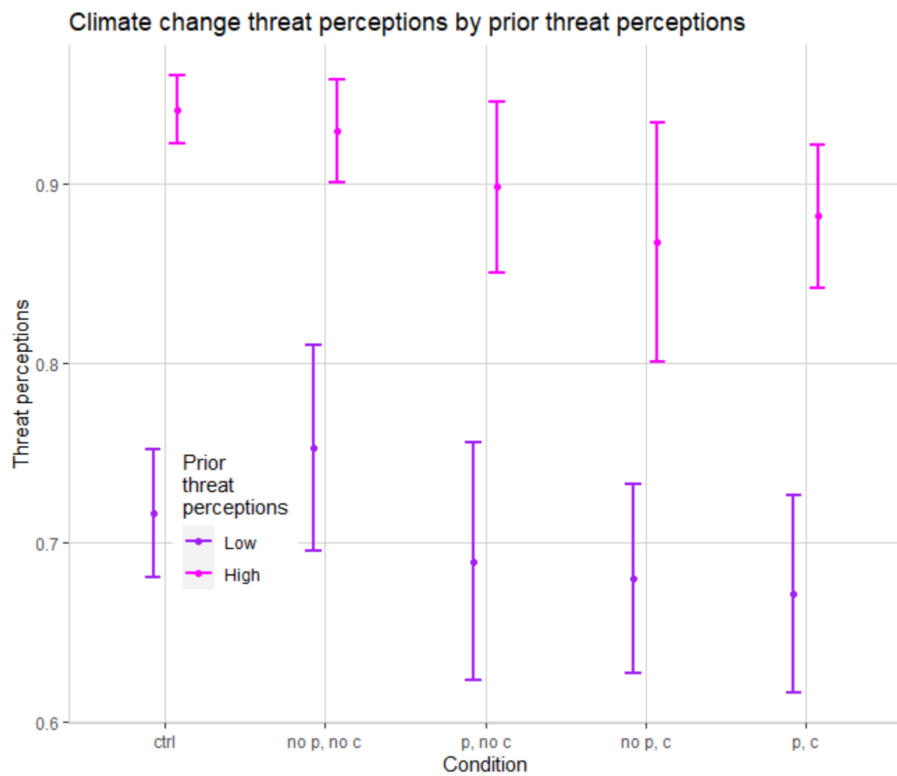

FIG. 5.11

Figures 5.12-5.14 illustrate condition mean estimate breakdowns by prior perceived politician, scientist, and net scientist credibility, respectively. While Figures 5.12 and 5.13 do not reveal any obvious heterogenous treatment effect (and regression confirms this absence), the difference between perceived scientist and politician credibility shows stark difference-in-differences. These are similar to those observed concerning change in climate change threat perceptions in Figure 5.8 but are somewhat more extreme.

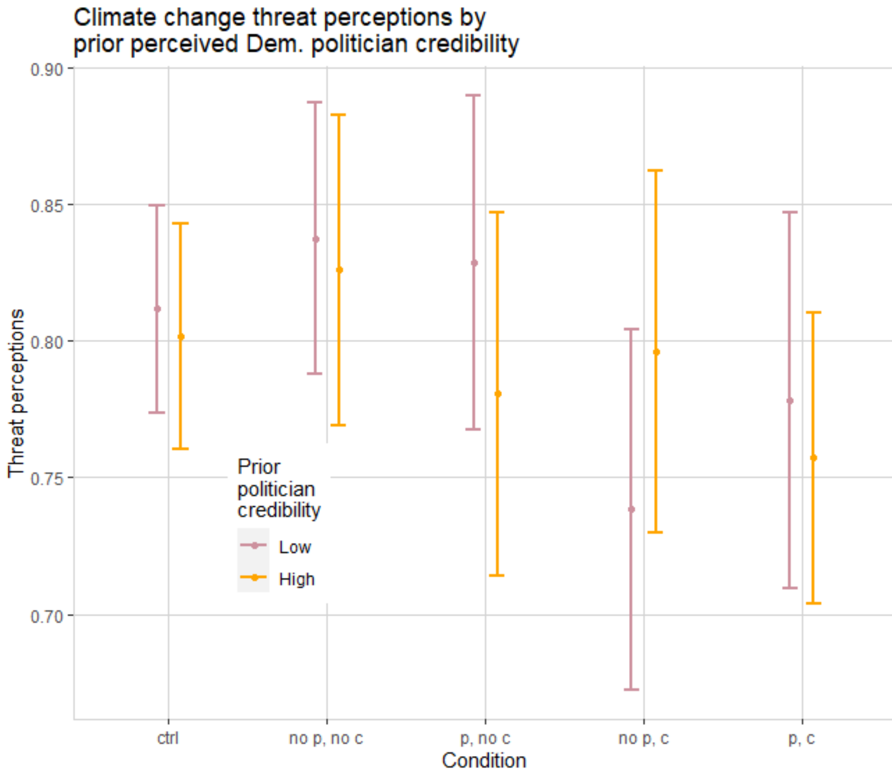

FIG. 5.12

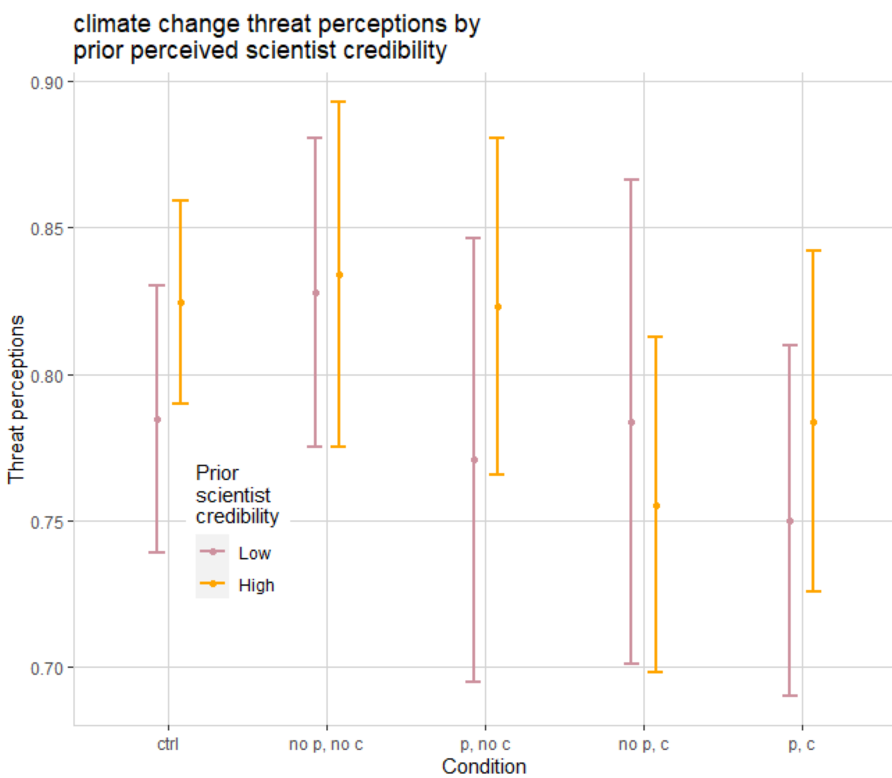

FIG. 5.13

Figure 5.14 suggests that adding a copartisan cue to the misinformation argument when a correction is not included (i.e. between the no *p, no c* and *p, no c* conditions) unexpectedly leads participants that perceive Democratic politicians as relatively more credible to have lower threat perceptions. Although, regression analysis reveals that this difference-in-difference is not significant, highlighted by the coefficient estimate of the *p, no c * net sci. cred.* term in Figure 5.15.

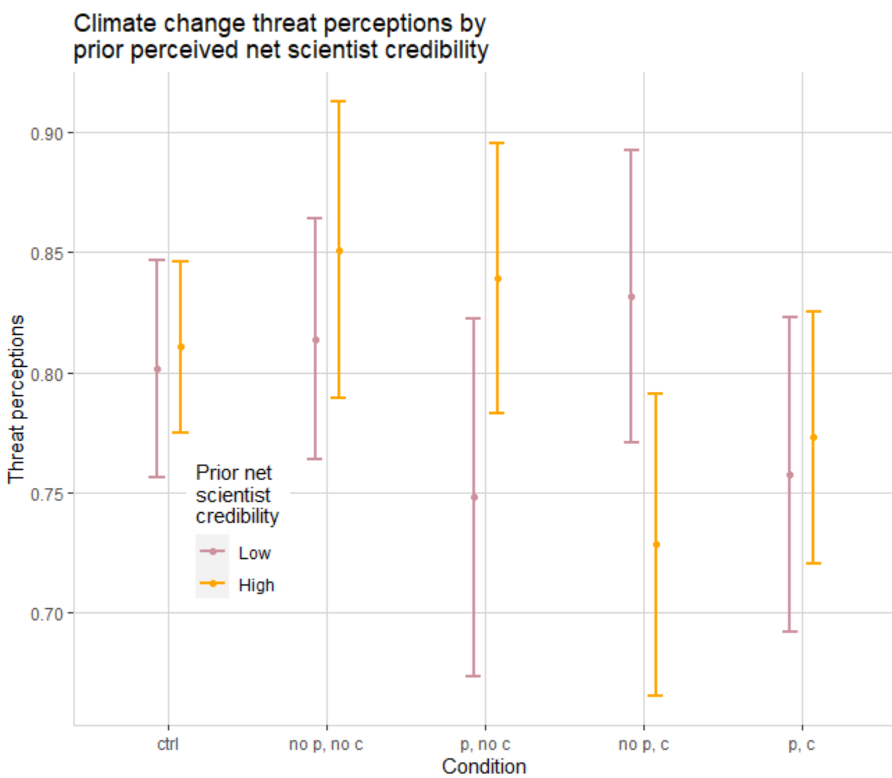

FIG. 5.14

Figure 5.15 does confirm that adding a correction in the absence of a copartisan cue produces a heterogenous treatment effect consistent with the accuracy motivated reasoning hypothesis. Participants that came in viewing scientists as relatively more credible have lower threat perceptions once they are presented with an argument by scientists that moderates the threat severity of climate change (relative to the misinformation argument).

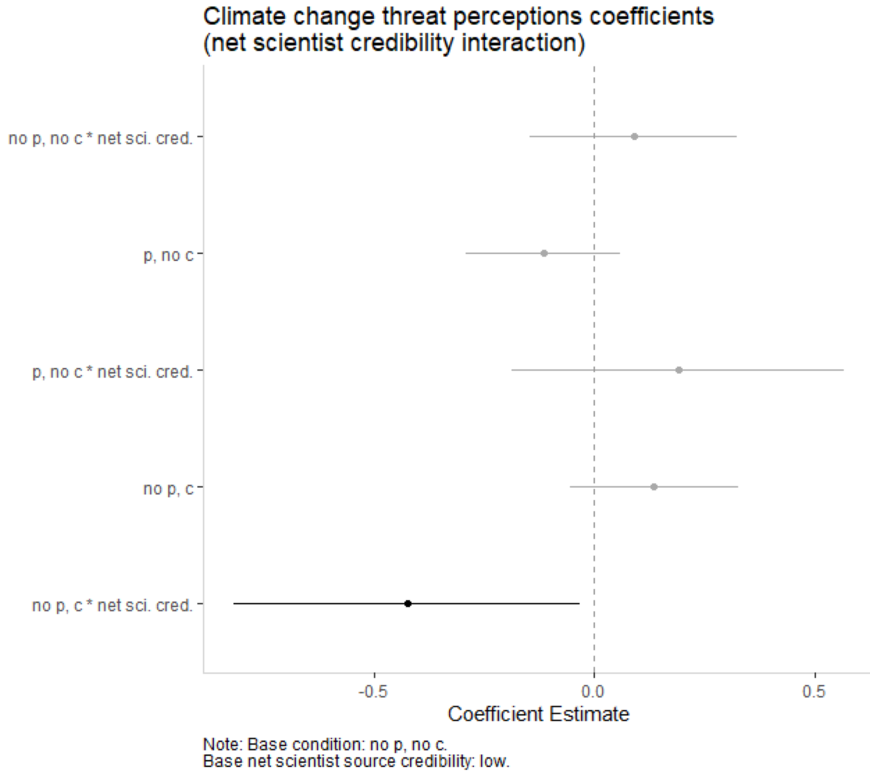

FIG. 5.15

Figure 5.14 also suggests that adding a correction in the presence of a copartisan cue similarly produces a heterogenous treatment effect consistent with accuracy motivated reasoning (*p, no c* to *p, c*), but regression shows this difference to be insignificant (p > .1). Figure 5.16 shows that adding a party cue in the presence of a correction (*no p, c* to *p, c*) leads to significantly lowered threat perceptions among participants that view Democratic politicians as relatively more credible, while their counterparts' perceptions rise. This is the opposite of the expected party cue effect in the context of accuracy motivated reasoning.

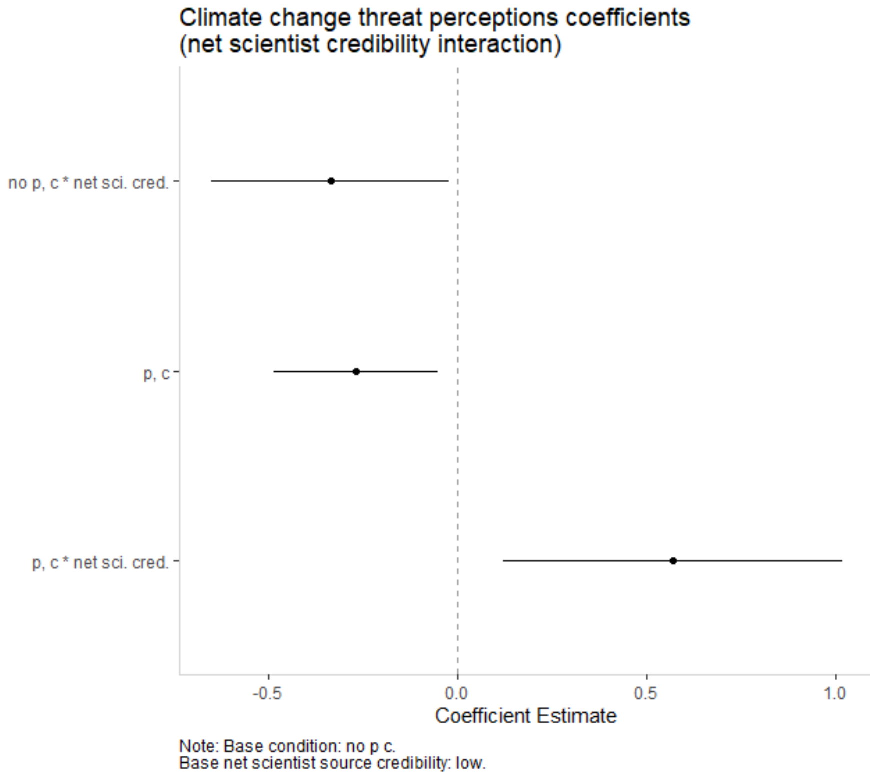

FIG. 5.16

The last set of results presented concern responses to the four climate change policy opinion items, which are scaled together to form an overall impression of support for governmental climate change mitigation efforts (higher values indicate greater support). Figure 5.17 shows the condition mean estimates; there is a pattern nearly identical to the climate change threat perception estimates shown in Figure 5.9. However, regression reveals no significant difference in means between any condition, even when correction-omitted and correction-included conditions are pooled together.

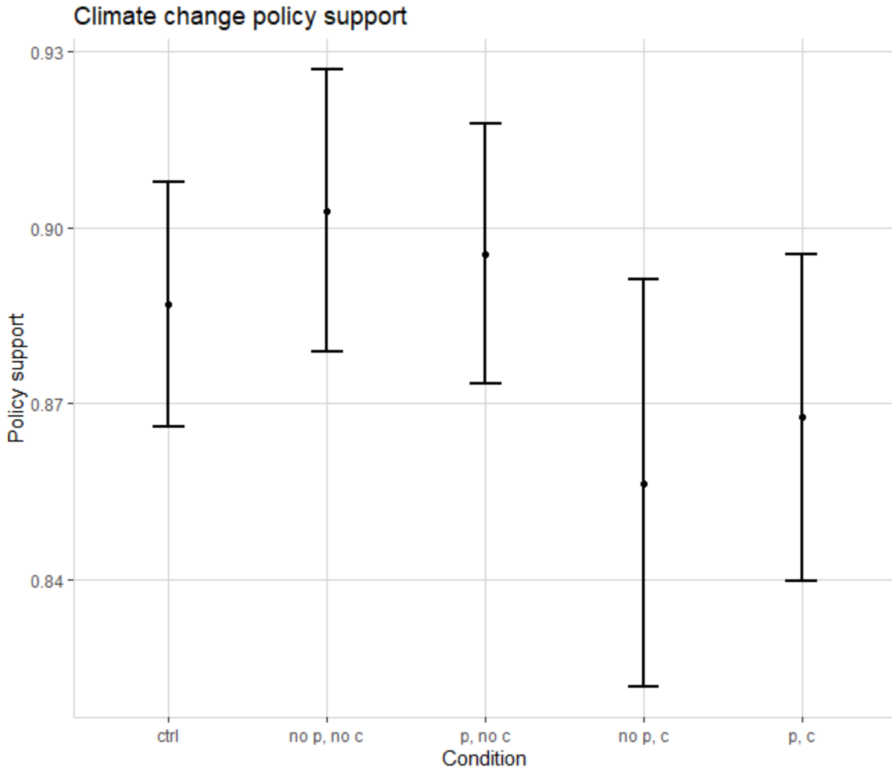

FIG. 5.17

Figure 5.18 reveals an interesting dip in support among weak/lean Democrats in the *no p, c* condition, but as this condition does not contain a party cue, it does not relate to the party cue effect expected by the partisan motivated reasoning hypothesis. Further, weak/lean Democrats increase support between the *no p, c* and *p, c* conditions, which is inconsistent with expectations of a copartisan cue effect. Otherwise, there is little difference-in-difference between the control and treatment conditions, or between other treatment conditions.

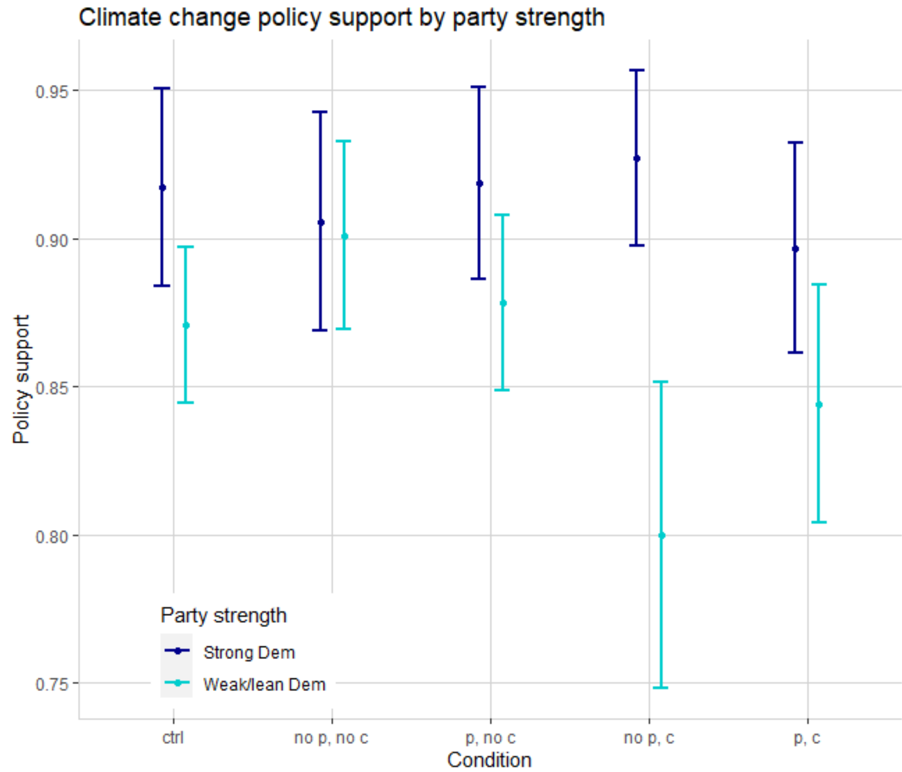

FIG. 5.18

Figure 5.19 shows no discernable difference in policy support between participants that came in viewing climate change as more or less threatening in any condition.

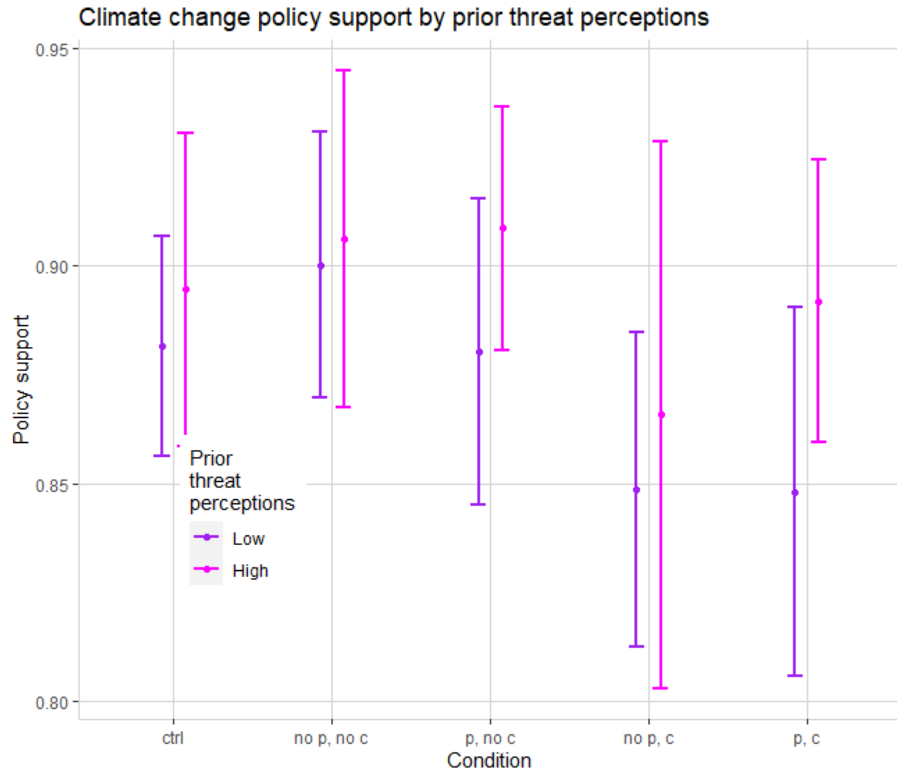

FIG. 5.19

Figure 5.20, which illustrates condition estimates split by prior perceived Democratic politician credibility, shows a dip in support among participants that view copartisan politicians as less credible in the *no p, c* group. This pattern is identical to that seen in Figure 5.18 among weak/lean Democrats, which makes sense because there is substantial overlap between participants that view Democrats as less credible and weak affiliation with the Democratic party. Once again, however, there is no theoretical rationale for why climate change policy support should be substantially lower among participants with low perceived Democratic politician credibility between the control condition and a treatment condition that does not include a copartisan cue. While Figure 5.20 also suggests a heterogenous treatment effect between the *p, no c* and *p, c* conditions, regression reveals no statistical significance in this difference-in-difference (p = .25).

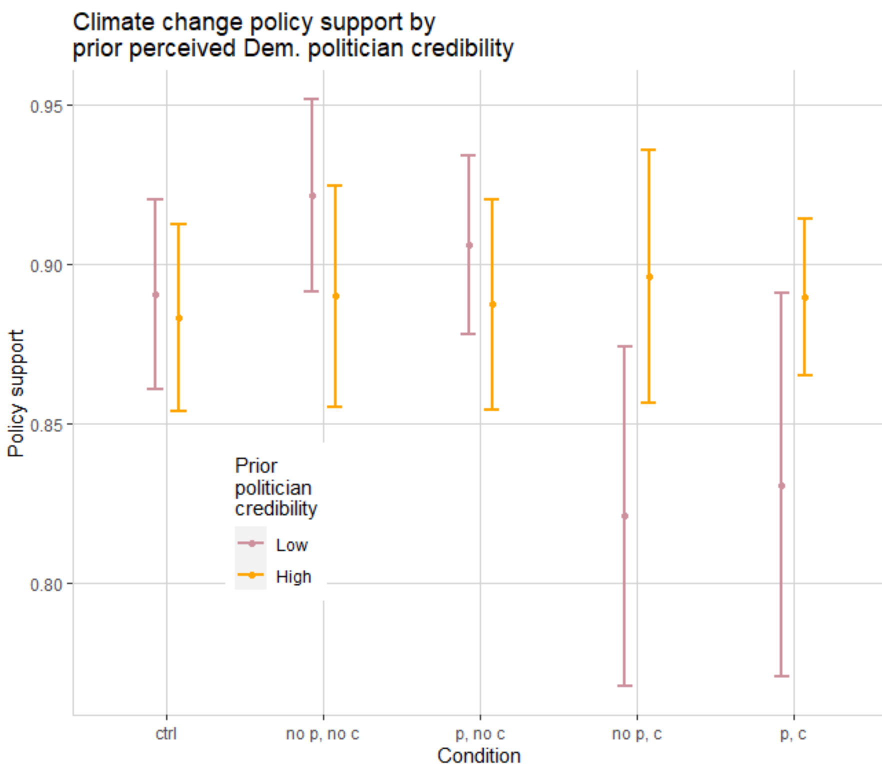

FIG. 5.20

Figure 5.21 suggests, and again regression confirms, that, compared to conditions where there is no correction attributed to scientists, participants do not support climate change policies as a function of their prior perceived scientist credibility substantially more when the correction is attributed to climate scientists. Figure 5.22, which breaks condition means down by net perceived scientist credibility, offers nothing more than what has been previously shown in Figures 5.20 and 5.21.

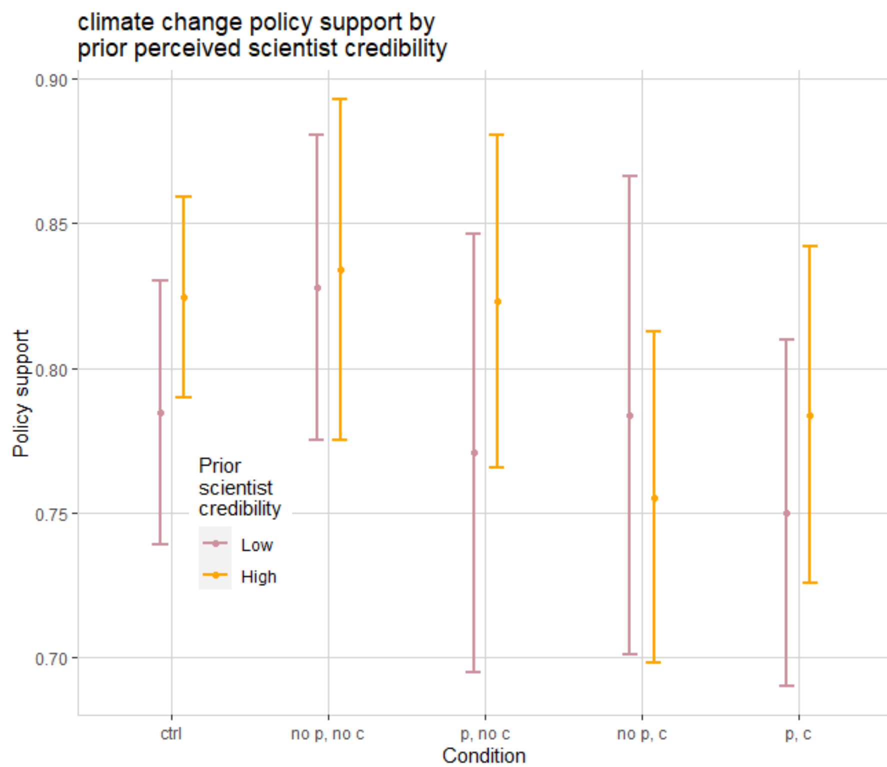

FIG. 5.21

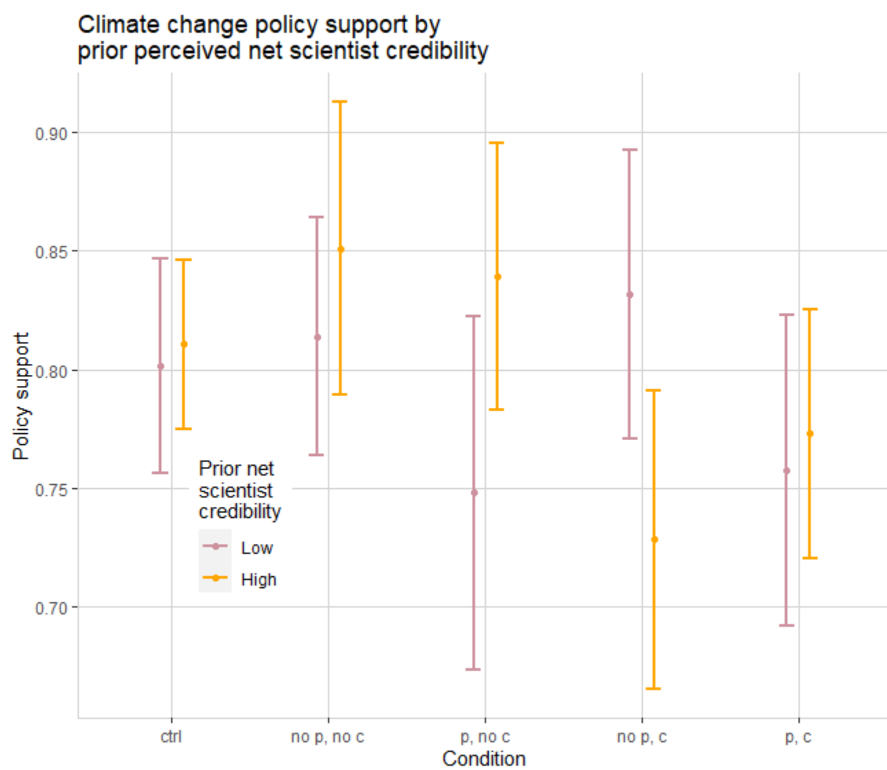

FIG. 5.22

As expected, the treatments do not have much bearing on climate change policy support, nor are there substantial heterogenous treatment effects consistent with hypotheses.

Discussion

On the whole, the treatments have little effect on opinions related to climate change. There is a net absence of support for the directional motivated reasoning hypotheses as well as for any heterogenous treatment effects, and suggestive support for accuracy motivated reasoning. The most notable treatment effects occur in changing threat perceptions, where the party cue, the correction, and the combination of the two produced more negative changes in threat perceptions relative to the control condition. However, the largest difference amounts to only three percent of the total possible difference (.06 points of difference divided by a total possible change of 2 points), so that the magnitude of the effect is quite small. While Figures 5.9 and 5.17 suggest that including a correction by scientists negatively influences threat perceptions and policy support, respectively, the difference in condition mean estimates do not reach traditional levels of statistical significance.

These trends are largely in accordance with the argument evaluation estimate trends in Chapter 4, but to a muted degree. In evaluating the arguments, attributing the arguments to their respective sources, inclusion of a copartisan cue, and inclusion of a correction produced progressively more unfavorable (favorable) evaluations of the misinformation (correction) argument. Here, the inclusion of the correction is the only reliable driver of opinion change, the direction of which is consistent with the climate scientists' moderating rebuttal.

There is also little support for a copartisan backlash among the dependent variables examined in this chapter; Figure 5.10 does suggest that inclusion of a copartisan cue in the correction-omitted condition results in a more negative change in threat perceptions, but this difference is not significant. Otherwise, there is little difference in estimates between corresponding cue-omitted and cue-included conditions.

Concerning moderating effects of partisanship strength, strong Democrats hold on to higher threat perceptions relative to their weak/lean copartisans as the copartisan elite misinformation source cue and correction are included in the treatment, suggesting mechanistic support for partisan motivated reasoning. Further, since prior perceived source credibility does not follow the same trends, it does not appear to be a confounder of this relationship. That being said, while strong Democrats have generally higher threat perceptions and support for climate change-related policies, as suggested by Figures 5.10 and 5.18, there are no heterogenous treatment effects that suggest partisan motivated reasoning among these variables.

Thus between the results pertaining to argument evaluations and those pertaining to climate change opinion, I find – on balance – absence of support for the partisan motivated reasoning hypothesis. Even Democrats that identify as strongly partisan do not follow their copartisan elites, *especially* when confronted with a contradictory statement by climate scientists. Instead, it appears as though participants follow the correction made by scientists.

There is also no support for a prior attitude effect among the climate change opinion variables assessed here. Participants with low and high prior threat perceptions respond similarly to each treatment. This is also largely the case among the argument evaluations. It appears that how threatening participants viewed climate change before the experiment has little bearing on how they process and respond to the arguments presented.

I also find little support for Druckman and McGrath's (2019) concern that prior perceived information source credibility confounds any moderating influence of prior attitudes or partisan strength on treatment effects. In the rare occurrences where I do find heterogenous treatment effects by partisan strength or prior threat perceptions, it is clear that prior perceived source credibility is not acting as an unobserved confounder. Moreover, I find that prior perceived source credibility does not moderate treatment effects in a manner consistent with the harder test of the accuracy motivated reasoning hypothesis (i.e. I do not empirically validate it as a mechanism of accuracy motivated reasoning). Instead, participants that came in with low opinions of scientists and/or Democratic politicians respond to treatments similarly to their high-opinion counterparts in their formation of argument evaluations and climate change opinions.

I find some support for the easier test of the accuracy motivated reasoning hypothesis: that relative to the control group, presentation of a correction of a correction by climate scientists – who are unarguably experts on the severity of climate change – produce climate change threat perceptions corresponding with the moderating nature of the corrective argument. The correction produces a significant and negative change in threat perceptions relative to the control condition. As expected at the outset, these findings are considerably weaker than those concerning the argument evaluations, likely because climate change opinion is conceptually more distant from the arguments themselves and also perhaps because the climate change opinion items were presented after the argument evaluation items so that the influence of the treatments likely dissipated.

Between the results presented in Chapters 4 and 5, it seems that participants readily accept climate change arguments made by expert sources, despite the fact that the content of the argument conflicts with the prior beliefs of many participants, and in fact view the correction by scientists more favorably when it is made against an elite of their own party. This correction shapes participants' views about the severity of climate change but does not go so far as to lower their support for mitigation policies.

As stated in the previous chapter's discussion, these findings contradict those of previous studies looking at Republicans' reticence to accept the reality of climate change, as well as those that generally find that copartisan cues prompt individuals to adopt similar political opinions (Carmichael and Brulle 2017; Kam 2005; Lenz 2013; Nicholson 2011; Turner 2007). There are a couple potential sample-particular explanations for the unexpected results of this experiment. Most previous studies use a representative sample of the American public, whereas this study uses a sample that is both more educated and younger than the average member of the public. Kahan and colleagues find that higher levels of education and scientific knowledge are associated with greater trust and perceived expertise of scientists (Kahan 2012, 2014, 2017b, 2017a; Kahan, Jenkins-Smith, and Braman 2011). Thus a concentrated sample of college undergraduates may push the effect of the correction to be more in line with accuracy motivated reasoning than would be expected with a representative sample of U.S. adults. Interestingly, however, Taber and Lodge (2013) find that more politically knowledgeable individuals tend to exhibit more pronounced directional motivated reasoning, so that these factors may push in opposing directions. Future work in this area that uses a representative sample of the public should seek to disentangle political knowledge from science knowledge in their moderating effects on motivated reasoning.

Second, the participants in this study are much younger than the average U.S. adult. Weaker partisan attachments among younger generations is well-documented (Arzheimer, Evans, and Lewis-Beck 2016), but it remains unclear as to whether younger "strong" Democrats are as strongly affiliated with the party as their older counterparts. That is, different age cohorts may differently conceptualize what it means to strongly or weakly self-identify as a member of a given party. It may be that strongly Democratic college-age individuals are more weakly tied to the Democratic party than an average-age strong Democrat, and so may be more willing to break from following the argument set by a copartisan elite. This, of course, is purely speculative, but more broadly it's possible that the unrepresentative characteristics of the sample used in this study may have led to results dissimilar from previous work.

If the results of this study are at least generalizable to climate change opinion formation among those on the left, then their implications may be regarded as normatively positive. If scientists are willing to step in and bring back to reality potentially alarmist claims made by Democratic elites, copartisans will have a more accurate understanding of the consequences of climate change, and may be less likely to engage in espousing catastrophic "Pandora's Box" frames that turn Republicans away from supporting climate change efforts (Nisbet 2009). That corrections do not lower support for mitigation policies is also a normative boon, insofar as federal policies aimed at reducing greenhouse gas emissions are necessary to maintain and grow the quality of life for the human species (IPCC and Edenhofer 2014).

Chapter 6: Conclusion

The studies here broadly focus on inferring the causes and effects of exposure to political information. Lazarsfeld, Berelson, and Gaudet (1948) observed that people primarily seek out political information during campaign season that is consistent with their dispositions, and that this information only serves to reinforce their dispositions. I do not find evidence to support the first half of the statement: over seven years among the same set of Americans, individuals do not appear seek out more ideologically extreme news content after their partisanship and ideology become more extreme.

I acknowledge that this absence of evidence in support of a selection effect may be because the ideological slant of news outlets has grown during the period under observation, especially among news outlets that were already catering to more ideologically extreme audiences (e.g. MSNBC and Fox News). In which case, it is likely that individuals are exposed to more ideologically extreme news content even if they do not report a change in their preferred news sources. My research design likely then underestimates a selection effect that is probably occurring in reality. Although again it is unclear what constitutes a "selection effect" if the selection of a given news source isn't changing. Future theoretical work should tackle what it means to select exposure to a given information source if relevant qualities of that information change over time. Additionally, future longitudinal studies of the sort conducted here should incorporate the change over time in the ideology of news sources. It may be that some segments of the public actively resist exposure to increasingly ideologically extreme news, while others are passively exposed to more ideologically extreme news by maintaining similar consumption habits over time.

I do find that greater exposure to more ideologically extreme news is related to a subsequent directionally consistent growth in the extremity of one's political identity. In other words, I find evidence of a media effect. This could be taken to mean that the second half of Lazarsfeld, Berelson, and Gaudet's (1948) observation is borne out here: exposure to ideologically extreme news reinforces political dispositions. However, in the absence of a selection effect, it is difficult to argue that participants are responding to the information in a biased manner or that the implications are normatively negative. A rational individual processes information and updates relevant factors to be consistent with that information, as, on average, TAPS participants did with regard to their core political identities.

I find an analogous set of results concerning individuals' responses to information about climate change. Participants' threat perceptions decline when presented information by climate scientists the threat of climate change is not as existentially imminent as suggested by a previously presented senator's statement. While participants do not increase their threat perceptions when presented with information by a senator – or specifically a member of their own party – that the threat of climate change is imminent and existential, this is likely due to the fact that most participants exhibited very high prior threat perceptions, so that they could not rise substantially more.

Further, individuals appear to respond to expert sources of information; participants in the climate change survey experiment highly evaluate the correction and lower their evaluation of the misinformation once sources are attributed. Although I did not find that perceived credibility of information sources guides these trends, it's likely that some aspect of the relative expertise of politicians and scientists plays a part in informing respondents' evaluations. In a similar vein, individuals may view preferred news outlets and their reporters as credible sources of information. Future work should evaluate who individuals follow when presented competing information between copartisan elites and preferred news sources; or who they follow when preferred news sources criticize copartisan elites.

The studies conducted here to form this dissertation aim to further understanding of the motivations and effects of exposure to political information. I develop a novel measure of ideological news exposure to test the reinforcing spirals model over six years and seven waves of the same representative segment of the public. I also evaluate motivated reasoning in climate change opinion formation among a novel segment of the public, and test the concerns that perceived source credibility might confound evidence of motivated reasoning. My findings are positive both methodologically and substantively: my measure appears to function as intended and provides empirical nuance beyond what has been used previously; perceived source credibility does not appear to be so highly correlated with outcomes of interest to the degree that it is a confounder, giving credence to prior studies on motivated reasoning; and individuals seek and process information in a manner largely consistent with accuracy motivated reasoning.

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