Abstract

The issue of measurement error and response bias in self-reported measures poses a significant challenge to communication scholars. While such issues have been a central concern for many media exposure measures, there has been a lack of parallel attention to the accuracy of “exposure to disagreement” measures despite the growing importance of the concept in communication research. Using unique digital trace data combined with the panel survey, we first establish the evidence of over-reporting of self-reported “exposure to disagreement” measures. Second, we demonstrate that people's retroactive self-reports regarding their exposure to disagreement are systematically affected by their public opinion perceptions, but not by social desirability factors or cognitive abilities and motivations to recall their behaviors. Lastly, we further demonstrate the ultimate consequences of relying on (potentially imperfect) self-reported measure of exposure to disagreement using Monte Carlo simulations, evaluating the relative bias and inconsistencies of research findings based on self-reported measures.

*Keywords*: measurement error, response bias, survey self-reports, digital trace data, exposure to disagreement, Monte Carlo simulations

(abstract: 150 words)

**Assessing (In)accuracy and Biases in Self-reported Measure of Exposure to Disagreement: Evidence from Digital Trace Data**

“Much of what we know about human behaviour and the state of society––from dietary habits to the utilisation of health care services, the dynamics of poverty and the nation’s unemployment rate––is based on self-reports collected in representative sample surveys.”

-- Schwarz (2007, p. 282)

A bulk of research in communication science -- particularly the one from survey-based evidence -- has typically relied on a respondent’s self-reported measure of some media exposure. While this approach has been indispensable in advancing our understanding regarding the nature and extent of communication effects, such studies are often criticized based on its imperfect measurements. The issue of measurement error and response bias, particularly under- or over-reporting relative to the actual exposure, poses a significant challenge to communication scholars since, as Slater (2004) once claimed, “our ability to test theory and to establish media or campaign effects is a function of our ability to successfully [...] measure exposure to [...] communication, and to analyze effects of that exposure” (p. 168). Indeed, this issue has been a central concern in extant communication research over years -- from advertising exposure (Vavreck, 2007), political news and debate exposure (Guess, 2015; Prior, 2012; 2013), mobile phone use (Boase & Ling, 2013), general internet use (Araujo, Wonneberger, Neijens, & Claes, 2017; Scharkow, 2016), and to social media use regarding politics (Guess, Munger, Nagler, & Tucker, 2018).

In this contribution, we extend our focus to a largely parallel, yet often neglected, source of one’s information exposure -- interpersonal discussion of politics, especially focusing on exposure to disagreement during one’s political discussions. Despite many advancements, what we know about citizens’ daily interactions with their peers is still heavily relied on participants’ retroactive self-reports serving as a proxy of their actual behaviors. Yet better understanding how accurate one’s self-report of “exposure to disagreement” vis-a-vis actual exposure has genuine practical and theoretical consequences. While most prior research on this topic explicitly or implicitly assumes a focal respondent’s political perception of his/her peers is a valid and faithful representation of their actual political orientations (see Mutz, 2006, p. 110), evidence suggests that there is a substantial gap between perceived disagreement and actual disagreement (Eveland & Hutchens, 2013; Wojcieszak & Price, 2012), suggesting the need for understanding the nature of such bias. Second, as a central explanatory variable, our ability to accurately identify the “impact” of one’s exposure to disagreement ultimately contingent on the quality of measurements (e.g., Slater, 2004). While the issue of reliability of political discussion measures has only recently gained attention ( Hutchens et al., 2018), we know very little about whether one’s self-report of “exposure to disagreement” accurately reflects actual level of exposure to disagreement.

Against this background, our aim in this contribution is threefold: Using unique digital trace data coupled with a panel response, we first establish the evidence of over-reporting of (self-reported) “exposure to disagreement” measure. Second, we demonstrate that people's retroactive self-reports regarding their exposure to disagreement are systematically affected by their public opinion perceptions, but not by social desirability factors or cognitive abilities and motivations to recall their behaviors. Lastly, we further demonstrate the ultimate consequences of relying on (potentially imperfect) self-reported measure of exposure to disagreement utilizing a systematic Monte Carlo simulation.

**Exploring the Nature of Bias in Self-reported Measure of Exposure to Disagreement**

Traditionally, studies on citizens’ exposure to disagreement during political discussion -- either from representative surveys (e.g., Huckfeldt & Sprague, 1995; Klofstad, Sokhey, & McClurg, 2013; Mutz, 2006) or from whole-network data of small, well-defined communities of individuals (e.g., Lazer et al., 2010; Song, 2015) -- have heavily relied upon participants’ retrospective self-reports. While scholars have rarely differentiated the actual vs. self-reported (“perceived”) disagreement (Wojcieszak & Price, 2012), a thorough investigation on the issue of measurement errors and response bias of self-reported exposure to disagreement has yet to be conducted. This is mainly due to the fact that a direct measurement of the concept (i.e., “exposure to disagreement”) has been unavailable to researchers. However, recent advancements in the use of digital trace data in communication studies start to provide important insights regarding this issue. These studies -- typically comparing one’s self-report against the objective, behavioral benchmark of the same concept -- find that a retrospective self-reported measure of exposure tends to over-represent the actual degree of exposure (e.g., Araujo et al., 2017; Scharkow, 2016). Considering such common patterns across studies and measurements of various “exposure” measures, we first start with a confirmatory hypothesis:

**H1**: A retrospective self-reported measure of exposure to disagreement is upwardly biased (i.e., over-estimation) relative to an objective behavioral benchmark.

While extant studies generally agree that a retrospective self-report measure tends to poorly reflect the true extent of one’s exposure, they often disagree about the exact *nature* of such discrepancy. To date, there are at least three different explanations proposed regarding the possible *causes* of systematic over- or under-reporting in self-reported measures: (a) social desirability, (b) cognitive burdens and biases, and (c) the conceptual validity of the self-reported measure (i.e., a fit between the measurement instruments and the conceptualization it intends to measure). We discuss those each in turn below.

**Social Desirability Bias in Survey Reporting**

Prior literature on this topic commonly suggests a social desirability factor as one of the root causes of response inaccuracy. Self-reported measures of one’s “sensitive” behaviors often suffer from systematic under-reporting due to a respondent’s desire to be (socially) better perceived by others, whereas a systematic over-reporting is more common for “normatively desirable” behaviors (e.g., Kahn, Ratan, & Williams, 2014; Krumpal, 2013).

Sometimes, rather than monotonic under- or over-reporting patterns, only a subset of respondents -- those with a higher need for social approval or for impression management -- are likely to give more incorrect responses (e.g., Krumpal, 2013). Nevertheless, it appears that “disagreement is not a prohibitively painful or costly experience” for ordinary citizens (Huckfeldt, 2007, p. 979). Rather, the concept surely entails some desirable qualities associated with “good” citizens (e.g., tolerance and openness to others, exposure to balanced opinions, etc.). Therefore, we may expect that, if any, social desirability are likely to be associated with the propensity of over-reporting rather than under-reporting of behaviors:

**H2**: A social desirability bias is associated with the over-estimation of exposure to disagreement in the self-reported measure relative to the objective benchmark.

**Cognitive Bias and Burdens Leading Imperfect Recall**

Another commonly proposed explanation for inaccuracy of one’s self-report is a respondent’s cognitive bias and burdens in recalling his or her behaviors. Typically, a process of answering survey self-report involves following steps: (1) “understanding” the question, (2) recalling relevant instances of concerned behaviors through memory search, (3) correctly adding up recalled instances or roughly estimating the quantity in question (e.g., frequency or rates of the behavior) in order to arrive at their own answer, (4) mapping the derived answer to given response options, and finally, (5) reporting either honestly or modifying the answers based on social desirability (Schwarz & Oyserman, 2001; Tourangeau & Rasinski, 1988).

From the perspective of human information processing, people are thought of paying more attention to and weigh greater importance on negative than positive information since negative information is more novel and distinctive, therefore offering more diagnostic cues regarding a given situation (Pratto & John, 1991). Due to this evolutionary cause, the near-automatic, attention-grabbing nature of negative information not only makes one’s judgement to be heavily dependent upon such negativity (Ito et al., 1998; Soroka, 2014), but also makes them to be better remembered in one’s memory and being better retrieved later on (Fiske, 1980; Pratto & John, 1991).[[1]](#footnote-2) Hence, we would expect that individuals are more likely to recall counter-attitudinal (i.e., negative) than pro-attitudinal (i.e., positive) encounters.

In addition to this inherent negativity bias, literature on human memory processing often finds that frequent and repeated behaviors are poorly represented within one’s memory without detailed contextual markers such as time or locations (Schwarz, 2007; Schwarz & Oyserman, 2001). Therefore, an extensive memory search for such poorly remembered behaviors is cognitively taxing, requiring a non-trivial motivation and time to do so. Absent of motivation and ability, people often (mistakenly) assume that their most recent behaviors are representative of their long-term behaviors (Schwarz & Oyserman, 2001; Tourangeau & Rasinski, 1988). It is therefore expected that shifting the time frame of a concerned behavior to a more recent period would bring more accurate responses (e.g., Araujo, et al., 2017) due to reduced cognitive burdens. This explanation further leads us to expect that the self-reported measure of exposure to disagreement will be better correlated with a more recent time-framed behavioral benchmark than the one with a longer time frame. Therefore:

**H3**: The over-estimation in survey self-reports is reduced when a more recent time-framed behavioral benchmark is used, compared to the one with a longer time frame.

In addition to one’s cognitive ability, individuals have different propensities of how deeply and “effortfully” they would engage with the required memory search (Krosnick, 1991). In this regard, participants’ relevant motivations play a considerable role in explaining the variations in (in)accuracies of self-reported behaviors. While a respondent’s interest in the survey itself (e.g., Araujo et al., 2017) is often regarded as the main motivational factor, here we expect political knowledge and interest to be meaningfully correlated with a participant’s motivations in answering “exposure to disagreement” question. Political knowledge and interest in general well predict one’s political behaviors, along with one’s attention and engagement with political information (e.g., Fiske, Lau, & Smith, 1990). Therefore:

**H4**: Political knowledge (H4a) and interest (H4b) are negatively related to over-reporting of exposure to disagreement in retrospective self-report.

**The Role of Public Opinion Perception in Retrospective Inference**

To measure “exposure to disagreement” in a survey setting, researchers typically rely on questions asking the summary perceptions of whether they have interacted with peers with different political orientations, or whether they were being exposed to political views of others either online or offline (e.g., McLeod et al., 1999; Kwak et al., 2005; also see Hutchens et al., 2018, for review).[[2]](#footnote-3) This involves an (often unrealistic) assumption that people “accurately” perceive political orientations of others when retroactively determining what counts as “disagreeable” exposure (i.e., the first and the second step of the response process). Yet studies suggest that the actual accuracy of one’s network alter perceptions (i.e., whether one’s perception of their alter’s political orientation indeed matches with that alter’s actual orientation) is often not very high (Eveland & Hutchens, 2013, Eveland et al., forthcoming). Moreover, respondents are unlikely to maintain detailed recollections of *every* encounter due to reasons outlined above. Therefore, it is perhaps not surprising that “respondents usually resort to a variety of inference strategies to arrive at a plausible estimate” (Schwarz & Oyserman, 2001, p. 142), extrapolating their past behaviors from currently available information (Schwarz, 2007). With respect to this point, we posit that individuals may rely on perceived opinion climates (i.e., a summary judgment regarding the relative distribution of opinions) as a heuristic cue in retrospectively inferring their past “exposure to disagreement.” It is often the case that individuals closely monitor their opinion environment (e.g., Hayes, Matthes, & Eveland, 2013), and overall perceived opinion climate can indeed serve as an easily available heuristic cue (Tversky & Kahneman, 1974), serving as a reference point against which one can naively infer and “extrapolate” their past exposure to disagreement within such opinion distribution (i.e., the second step of the response process).

Based on this reasoning, we expect that those who perceive themselves to be in a majority position are *less* likely to retrospectively report exposure to disagreement. There are a couple of explanations of why this might be the case. First, probabilistically, the chance of encountering disagreeable opinion becomes lower as one perceives more prevalence of supportive opinions, whereas it becomes higher as the perceived prevalence of hostile opinions increases. Applying this logic, those who perceive themselves to be in a more favorable opinion environment would expect a lesser degree of (probable) exposure to disagreement than they would under more hostile opinion climate. Indeed, Huckfeldt’s (2007) analysis suggests that people’s naive expectations regarding the political preferences of one’s alters are likely to be in line with the perceived dominant opinion distribution.

Second, the literature on contrast bias and social judgment theory (Mussweiler, Ruter, & Epstude, 2004; Sherif & Hovland, 1961) assert that an individual perceives a target of a comparison to be more biased in favor of the opposing side. If one perceives that the typical opinion climate is biased against (for) them, this would trigger more (less) negative contrast in retrospectively “inferring” a probable exposure scenario during the naive inferences phase. This implies that (a) one’s retroactive perception of exposure to disagreement under a model opinion distribution is systematically conditioned upon the perceived opinion climate, and for that matter, (b) the (in)accuracy of one’s self-report should be also systematically correlated with one’s perception of opinion climates. Therefore:

**H5**: More favorable public opinion perception is related to a lesser degree of over-reporting of exposure to disagreement relative to the objective behavioral benchmark.

It is worth noting here that our explication of the nature and mechanisms of the over-reporting bias in self-reported exposure to disagreement measures, as outlined above, are by no means exhaustive. Also, it is not clear whether expected patterns of over-reporting are resulted only by one of those mechanisms, or by in any combination of them simultaneously. Regardless, each of the expectations outlined above gives us unique insights in understanding the exact nature of such bias, which has been poorly understood to date.

**Consequences of Over-Reporting Bias in Self-Reported Measure of Disagreement**

One of the most common concerns on the inaccuracy of self-reported exposure measure is that such bias can mask or distorts the true relationship between predictors (such as exposure to disagreement) and key outcomes. While some has been more vocal about this issue (e.g., Prior, 2012; 2013), others have suggested that monotonic over-reporting bias is less of a concern in most of the multivariate, covariational analysis, since an increase in the self-reported measure is indeed reflective of a relative increase in the true exposure as long as the two measurements are systematically correlated (Scharkow, 2016). While this general expectation is widely shared by scholars, the question of to what degree the conclusions from self-reported exposure to disagreement is systematically affected by its imperfect relationship with the true exposure have yet to be studied. Therefore, we ask following research question:

**RQ**: How the impact of self-reported exposure to disagreement on an outcome is affected by the degree of correlation between self-reports and the behavioral benchmark?

**Methods**

**Sample and Data**

The present study relies on the data collected during the 2012 Korean presidential election period (from November 23rd to December 23rd, 2012), using an online panel survey administered by a private firm *Embrain*. A total of 400 respondents were randomly recruited from a nationally representative opt-in panel maintained by the firm. Eligible panelists were invited to a custom-created online forum hosted on a research firm’s server, and were instructed to freely participate in the forum as they normally would in other online forums. Their activities in this forum were unobtrusively tracked, and this tracking data were merged with the three-wave panel survey, which were administered in the beginning, middle (2 weeks after the Wave 1 survey), and end (right after Election Day) of the study period (see Figure A1 in the appendix showing the detailed timeline of the data collection). This allows us to systematically compare their actual exposure patterns against their self-reports.[[3]](#footnote-4)

Excluding dropouts, the final dataset contained 341 samples who both completed the panel survey and stay participated in the online forum. While the final sample closely reflects the demographic profiles of the general Korean population in terms of gender (48% female) and age (*M* = 35.72, *SD* = 9.86), they tend to be slightly better educated (median = “college education”, *M* = 7.68, *SD* = 1.04) and more affluent (average household income: *M* = 4.99 [between $4000 to $5000 per month], *SD* = 1.88). In addition, at the beginning of the survey, participants were overall more likely to support a liberal presidential candidate (= 58.1%) relative to a conservative candidate more so than in general population (= 46.6%). Upon completion of the project, a monetary incentive of US$100 (approximately equivalent to 100,000 Korean Won) was provided to the participants in return for their participation.

**Measures**

**Behavioral Benchmark Measures.** We took the behavioral tracking data from the two weeks prior to each survey until the day before the survey in constructing our behavioral benchmark measure, aggregated the entire raw counts of disagreeable vs. total exposure during the respective two-weeks periods (W1: *M* = .41, *SD* = .23; W2: *M* = .44, *SD* = .19). Since the objective, behavioral tracking data is taken from the two weeks prior until the day before each survey date (W2 and W3, respectively), this establishes a clear temporal precedence between the behavioral benchmark and the self-reported measure (also see Figure A1 in the appendix for a detail).[[4]](#footnote-5) Here, “exposure to disagreement” is conceptualized as exposure to a message written by others who support a different candidate than one’s own, based on reported candidate preferences in Wave 1 and Wave 2 (where 0 = “supporting the conservative candidate” vs. 1 = “supporting the liberal candidate”).[[5]](#footnote-6) For instance, we regarded any exposure as “exposure to disagreement” whenever a liberal candidate supporter chose to view a message from a non-liberal candidate supporter (or vice versa).[[6]](#footnote-7)

**Self-reported Measures of Exposure to Disagreement**. During the second and the third wave of the panel survey, we asked as following: “what *percentage* of posts (out of 100%) you have read during the last two weeks in this forum can be classified as conservative vs. moderate vs. liberal in your opinion?” Accordingly, for liberal candidate supporters, perceived exposure (in percentage) to non-liberal (i.e., independent and conservative) messages were regarded as disagreement, while for conservative candidate supporters, perceived exposure to non-conservative (i.e., independent and liberal) messages was regarded as disagreement (Wave 2: *M* = .58, *SD* = .21, Wave 3: *M* = .59, *SD* = .21). More detailed information regarding the measurement instruments of key variables (including covariates) are reported in the online appendix.

**Social Desirability Bias.** A respondent’s *normative endorsement for disagreement* (W2: Cronbach α = .89, *M* = 5.59, *SD* = .90; W3: α = .89, *M* = 5.60, *SD* = .93) was assessed by asking five questions (all on a 7-point scale) including “people who are opposed to my opinion also have the right to participate in discussions,” “It is worth reading the opposite opinion,” etc. Also, a respondent’s *needs for social approval* (W2: α = .87, *M* = 3.86, *SD* = 1.30; W3: α = .87, *M* = 3.82, *SD* = 1.28) was assessed, based on three questions (all based on a 7-point scale) tapping their desire to be accepted by other peers (e.g., “I am sensitive to the evaluation of others on my writings in this forum”). Although these measures are rather an indirect measure, it nevertheless helps us account for possible social desirability bias.

**Political Interest and Knowledge*.*** For motivations and abilities, political interest (W2: Spearman-Brown = .94, *M* = 5.05, *SD* = 1.05; W3: Spearman-Brown = .95, *M* = 5.03, *SD* = 1.05, all ranged from 1 to 7) was assessed by two questions per each wave asking a respondent’s interest to politics and information seeking related to upcoming presidential election. For political knowledge, we also asked a total of 10 questions (only at the first wave of the survey, all coded as 1 = “correct” vs. 0 = “incorrect/DK”) regarding general political knowledge and candidate background/issue stance knowledge. A summary index (i.e., a sum of correctly answered questions) was derived (*M* = 5.74, *SD* = 2.15).

**Opinion Climate Perception.** Following a common approach operationalizing the perceived opinion climate in this area of research (e.g., Matthes et al., 2010; Matthes, 2015), one’s opinion climate perception was measured by asking whether a respondent perceives his or her own opinion to be in line with that of the majority (W2: *M* = 4.41, *SD* = 1.15; W3: *M* = 4.48, *SD* = 1.27, from 1 = “not at all” to 7 = “extremely”).[[7]](#footnote-8)

**Control variables.** We also control a host of variables in later regression models, including demographic variables as mentioned earlier (measured at Wave 1), the strengths of ideological self-placement (by folding the 7-point ideology scale, W2: *M* = 1.07, *SD* = .86; W3: *M* = 1.07, *SD* = .85, all range: 0 to 3), and media exposure (internet, TV, and newspaper exposure *in hours*, only measured at W2: *M* = 1.57, *SD* = 1.67). Since the distribution of candidate support was imbalanced across surveys, we also control for one’s actual candidate choice (dummy coded, 1 = “support for liberal candidate” as described above, W2: M = .68, SD = .46; W3: M = .62, SD = .48). Also, a respondent’s total volumes of message exposure at each wave was included. Since this measure was highly skewed, a set of log-transformed variables was created (W1: *M* = 4.28, *SD* = 1.41; W2: *M* = 4.28, *SD* = 1.47).

**Results**

**Preliminary Bivariate Analysis**

We first assess the bivariate relationship between our self-reported vs. behavioral benchmarks measuring exposure to disagreement, with the two possible different ways of constructing behavioral benchmarks based on our tracking data. Figure 1 below first displays the two sets of Q-Q plots, with the x-axis being actual exposure from tracking data in Wave 1 and Wave 2 and the y-axis being the self-reported survey measure in Wave 2 and Wave 3, respectively, along with the grey-dotted reference line indicating the one-to-one correspondence between the two measures.

-- Figure 1 and Figure 2 About Here --

Figure 1 visually reveals that there is a rough correspondence between the self-reported vs. behavioral measures of exposure to disagreement, in a way that those who are more exposed to out-partisan (i.e., disagreeable) messages indeed report more exposure to disagreement in their self-reports. However, it also confirms that self-reported measure of exposure to disagreement often over-estimates the actual exposure (as the observations tend to be upwardly placed above the reference line). The survey response at Wave 2 was modestly and significantly correlated with this behavioral benchmark at Wave 1 (the left panel of Figure 1, *r* = .448), and the patterns was largely similar between Wave 2 tracking data and Wave 3 survey response (the right panel of Figure 1, *r* = 428). When we look at the simple differences between the self-reported measure and the behavioral measure (which quantifies the precise *degree* of overestimation), respondents are indeed likely to substantially overestimate their actual exposure to disagreement by as little as about 14.42% point on average (W2-W3 comparison, based on N = 20,000 nonparametric permutation test of differences, 95% CIs = [.1166, .1718]) up to 16.31% point on average (W1-W2 comparison, 95% CIs = [.1333, .1934]). A nonparametric permutation test additionally revealed that these differences were all statistically significant, as can be seen in their 95% confidence intervals.[[8]](#footnote-9)

Considering the fact that both the behavioral benchmarks and the subjective self-reported measure are designed to measure the exact same underlying concept, the modest correlation (from .428 to .448) is indeed surprising, suggesting the convergent validity of the self-reported measure is not high as often naively assumed in extant literature.

In the online appendix, we replicate our main analyses using the alternative behavioral benchmark using the averages of “daily proportions” measure instead of the cumulative proportion measure (see Figure A2 and Table A6 for details). Using this alternative behavioral benchmark did not change our findings and conclusions.

**What Explains Over-reporting of Exposure to Disagreement?**

Having established the evidence of over-reporting in one’s self-reported measure of exposure to disagreement, we now proceed to our evaluation of underlying factors of such discrepancies, as reported in Table 2 below. Here, we predict “inaccuracy” defined as the self-reported exposure *minus* behavioral benchmark measure as reported earlier (W2: *M* = .16, *SD* = .23, range = -.54 to .90; W3: *M* = .14, *SD* = .21, range = -.50 to .80).[[9]](#footnote-10) We have estimated a set of regression models with nonparametric bootstrapping (*N* = 10000) along with percentile bootstrapped CIs, in order to avoid the possibility of our conclusions driven by the peculiarity of our data and analytical choices. In online appendix, we present full results reported in the main manuscript (see Table A1 in the appendix for details). We additionally replicated the main analysis using Bayesian models (Table A2), using shorter time frame within which one’s behavioral benchmarks are derived (Table A4), and using alternative measurements of behavioral benchmark and public opinion climate perception variable (Table A6). Substantive conclusions remain unchanged.

-- Table 1 and Table 2 About Here --

The balance of evidence suggests that the social desirability bias (H2) and cognitive burdens accounts (ability and motivation, as measured by knowledge and interest: H4) have less likely to be a consequential factor explaining the apparent over-reporting patterns. In OLS regression results (“OLS” columns in Table 1), the social desirability factors did not significantly predict the inaccuracy of self-reported measure, so does political interest and knowledge. This was also true when we dichotomize the continuous “inaccuracy” variable against the value of zero and predict the probability of being assigned in “over-reporting” category (“GLM” columns in Table 1). In contrast, we found a much consistent impact of an opinion perception variable (H5), such that the more respondents perceive their own opinions to be in line with the overall opinion of the majority, they are less likely to over-report their exposure to disagreement (*b* = -.284 to -.021, all 95% percentile CIs do not contain zero). Indeed, consistent with our expectation, those who perceive more favorable opinion climate were less likely to retroactively report the exposure to disagreement controlling the actual exposure (see Appendix Table A3 for a detail). Ironically, considering that respondents typically exaggerate their baseline of disagreement exposure, the negative impact of public opinion perception on perceived exposure likely have reduced the discrepancy between one’s self report and the actual exposure, making them become more “accurate” in self-reports.

Next, our H3 stated that -- based on the cognitive burden accounts -- the over-reporting bias in self-reported measure would be reduced when behaviors from a more “recent” period are used as a benchmark. The Figure 2 above displays the additional Q-Q plot using the three most recent days of tracking data for the behavioral benchmark.[[10]](#footnote-11) Comparing the earlier patterns reported in Figure 1 (which is based on the entire two-weeks time frame), it is apparent that there is no substantial improvements in reporting accuracies when more recent behavioral data is used in comparison of one’s self-reports. Indeed, for the W2, zero-order correlation between self-reported exposure vs. behavioral benchmark (based on entire range of data) was *r* = .448, and this pattern only slightly changes when the three most recent days of tracking data is used for the behavioral benchmark (*r* = .391).[[11]](#footnote-12) The *difference* of two correlations was itself significant (*diff* = -.057, 95% CIs = [-.0924, -.0250]), yet the fact that correlation becomes *lower* when we use most recent data further suggesting the evidence *against* the cognitive burden explanation. For the W3 as well, the zero-order correlation between self-reported vs. behavioral benchmark does not significantly change when we use the three most recent days of tracking data for the behavioral benchmark (differences in *r* = -.0199, 95% CIs = [-.0574, .0167]). Table 2 above additionally reports replications of regression models from Table 1 (but using only OLS models), based on this alternative behavioral benchmark in deriving the inaccuracy variable (i.e., self-report *minus* behavioral benchmark: see Table A4 in the appendix for a full result). As can be seen in Table 2, there is virtually no change in terms of the magnitudes of regression coefficients and their significance levels across predictors, suggesting the evidence *against* H3. In sum, our analyses show that the over-reporting of exposure to disagreement does not likely to be driven by either social desirability or by participants’ cognitive burdens and motivations, but rather based on biased perceived opinion climate perception.

**Consequences of Using Self-Reported Exposure to Disagreement**

In order to demonstrate the ultimate consequences of relying on (potentially imperfect) self-reported “exposure to disagreement” measure, we examine the predictive validity of self-reported vs. behavioral measures of exposure to disagreement in relation to a focal outcome variable. Based on extant research suggesting that exposure to disagreement may undermine one’s attitude strengths (e.g., Levitan & Visser, 2009; Song & Eveland, 2015), we inspect the relationship between our two disagreement variables (the self-reported vs. the behavioral measure) as a focal predictor, one at a time in regression models, and the attitude “certainty” (Matthes et al., 2010; Matthes, 2015) as the focal outcome variable. A large-scale Monte Carlo simulation evaluation was followed in order to offer some additional contexts of this evaluation, primarily focusing on the sample size of regression models and the degree of correlation between self-reported and behavioral benchmark measures.

Below Table 3 reports the key results of two lagged dependent variable regression models predicting attitude certainty (regarding their candidate support) at Wave 3 as a function of exposure to disagreement (one using self-reported measure at Wave 3 and the other using behavioral measure at Wave 2, yet both of measures refer their respective behaviors at Wave 2), controlling for a host of covariates (see Table A5 in the online appendix for a full result). Results reveal that the magnitude of regression coefficient based on self-reported measure is slightly smaller than that of the objective measure, and the coefficient itself is not statistically significant (*b* = -.520, *SE* = .266, *p* > .05), although it shows the identical direction of the effect as to the objective measure. The objective behavioral measure, in contrast, registered a slightly stronger effect and statistically significant (*b* = -.769, *SE* = .340, *p* <. 05), consistent with the evidence suggested by a number of prior studies (e.g., Levitan & Visser, 2009; Song & Eveland, 2015). While the result shows that the predictive validity of the concept is more stronger for the behavioral measure of exposure than that of the self-reported measure, one may interpret this finding as the justification that the use of moderately-correlated subjective measure (again, zero-order *r* = .41 in our case) provides a largely consistent result against the behavioral measure. This appears encouraging at first glance, considering that studies typically report the zero-order correlation between self-reported vs. behavioral measures as low as 0.3 (e.g., Araujo et al., 2017; Scharkow, 2016) up to as high as .6 (e.g., Wonneberger & Irazoqui, 2017).

However, our simulation results, which appear in Figure 2 below, shows that this naïve conclusion is indeed less likely to be true. Using MC simulation, we compare two unstandardized coefficients of “exposure to disagreement” variables (the one based on the self-reported measure vs. the other based on the behavioral benchmark) in regression models predicting attitude certainty, and observe (1) their *relative size* (i.e., the ratio of two coefficients), (2) their *absolute bias* (i.e., the absolute difference of two coefficients), and (3) whether the statistical significance of two coefficients agree with each other. We systematically vary the size of the simulated data (*N* = 341, 1000, and 5000) and the zero-order correlation between two measures of disagreement (*r* = from 0.00 to 0.95 by 0.05 interval, so 20 zero-order correlations), yet residual correlations of two exposure variables with other covariates were kept constant as to our observed data (see the appendix for a more detail regarding the setup). The final MC simulation therefore generated 60 scenarios along with 1000 replications per each scenario.

-- Table 3 and Figure 3 About Here --

Results of MC simulations reveal that a regression coefficient based on self-reported measure (compared to the regression coefficient based on behavioral measure) is likely to indeed perform much worse, possibly more so than our empirical findings reported in Table 3. For the identical scenario as to our empirical case (i.e., the sample size *N* = 341, with the zero-order correlation *r* = 0.41 between the two exposure measures), only about 40% of replications (out of 1000) produced results in which the significance of two regression coefficients agreeing with each other (as in the green line in panel A of Figure 3). With a more number of sample size, the proportion of such results rapidly drops to 8.1% (sample *N* = 1000, the orange line) and further to 1% (sample *N* = 5000, the purple line), suggesting that only modestly correlated self-reported exposure measure tends to *always* disagree with the behavioral measure in terms of their statistical significance. As expected, the stronger the correlation between the two exposure variables, the higher the proportion of replications in which the statistical significance of two regression coefficients in line with each other. Yet this only start to happen at a much stronger correlation -- for instance, when the zero-order correlation between two exposure measure is equal or greater than 0.60 (*N* = 341 or *N* = 5000), or than 0.75 (*N* = 1000), the statistical significance of two regression coefficients agree with each other at least in 50% of replications.

The results concerning absolute bias (Panel B of Figure 3) and relative size (Panel C of Figure 3) also suggest a similar picture. Under the same condition as to our empirical case, the median expected *relative size* of the coefficient (= 0.097) from the self-report measure (vis-a-vis the coefficient from the behavioral measure) based on simulations is likely to be much smaller than our empirical finding (= 0.676), while the median expected *absolute bias* (*=* 0.713) of the coefficient based on simulations are likely to be much bigger than our empirical finding (= 0.248, respectively). Collectively, our results reported in Table 3 and comparisons of the simulation results (as in Figure 3) suggests that the likelihood of arriving at a wrong conclusion (regarding the impact of exposure to disagreement) is substantially higher when the self-reported measure is at best modestly correlated with the actual exposure.

**Discussion**

The reliability and validity of a self-reported exposure measure has long been recognized as “the” foundational issue shaping our collective ability as a field in establishing the valid empirical evidence regarding the impact of such exposure. While the importance of valid and reliable measure of “media” exposure has been indeed widely discussed (e.g., Prior, 2013; Slater, 2004), there has been relatively a lack of parallel attention to the issue of measurement of “interpersonal” exposure, especially in a form of exposure to disagreement during one’s political discussion, despite the growing importance of the concept in contemporary communication scholarship and its implication for deliberative democracy (e.g., Huckfeldt & Sprague, 1995; Mutz, 2006; McLeod et al., 1999; Kwak et al., 2005). Combining unique digital trace data and the multi-wave panel survey responses, the present study makes a number of contributions to the rapidly growing literature of citizens’ exposure to disagreement, and further, its accuracy of self-reports.

By comparing one’s retroactive self-reports and (unobtrusively-logged) behavioral measures of exposure to disagreement, we found that there was a clear and discernible tendency of over-reporting -- typically as little as approximately from 15% to 35% on average, depending on specific behavioral benchmarks being used -- in self-reported measure of “exposure to disagreement.” Although the two measures of exposure to disagreement are reasonably correlated, the correlation was at best only modest. This is largely consistent with the general tendency reported in prior studies examining the over-reporting in one’s self-reports of media and online exposure (Guess, 2015; Prior, 2012; Scharkow, 2016).

Above and beyond this apparent tendency of over-reporting, our findings also illuminate the possible *nature* of such bias. That is, our results reveal that response errors and reporting inaccuracies were unable to be explained by one’s social desirability factors or a lack of motivation and ability to recall their behaviors. More importantly instead, we found a systematic and stable influence of one’s public opinion perception, in a way that the more a respondent perceives his or her opinion to be in line with the opinion of majorities, the more one becomes “accurate” in terms of their self-reported exposure to disagreement vis-a-vis their actual behaviors. Consistent with our expectation, additional analysis (reported in Table A3 in the appendix) revealed that those who perceive themselves more in line with the majority opinion were indeed *less* likely to (retroactively) report exposure to disagreement. Our study is among the first to suggest that one’s self-reported (perceived) exposure to disagreement is systematically influenced by how one gauges their overall climate, revealing the importance of additionally considering one’s public opinion perception in this process. Importantly, this effect largely remained stable and consistent across different analytical models, regardless of how we utilize different measurement constructions of several key concepts in our analytical model (e.g., different durations of exposure measurements, different ways of aggregating exposure measures, or different operationalization of public opinion perceptions, etc.), speaking to the robustness of our findings against possible different analytical decisions.

Lastly, our evaluation of predictive validity of each “exposure to disagreement” measure and their relationships with a focal criterion variable (using the candidate preference certainty as a specific context) reveals further practical implications. Although our initial empirical finding (as in Table 3) documented that the two exposure measures -- one using self-reports and the other using the behavioral data -- performed largely similar to each other except their statistical significance, our MC simulation additionally uncovered that such seemingly-encouraging conclusion is rather unfounded, especially when it comes to understanding the *impact* of the exposure variables on outcome measures. The simulation showed that a typical result from the self-reported measure of exposure to disagreement may be systematically biased -- perhaps much more so than what is typically assumed -- under the typical magnitude of zero-order correlation between subjective vs. behavioral measures. The common practice of explicitly or implicitly assuming a respondent’s political perception of his or her alters as a valid and faithful representation of alters’ political orientations therefore constitute a serious threat to the validity of exposure to disagreement concept based on self-reported measure and the conclusion drawn from such a measure.

A few important conceptual and methodological issues deserve further mention. First, our sample is based on rather small, “opt-in” panel therefore the generalizability of our findings may be limited. However, given our findings are much consistent with other studies employing a much larger, representative sample in a considerably different political and geographical context (e.g., Araujo et al., 2017; Guess et al., 2018; Scharkow, 2016), we are confident that basic dynamics regarding the response inaccuracy documented in the present study may be equally applicable to other contexts.

Second, our measurement construction of “exposure to disagreement” is based on what is called a “summary” perception measure (see Hutchens et al., 2018), the approach of which widely adopted in many survey-based measures of the concept. As discussed, this approach usually involves asking a focal respondent’s “summary” perception regarding their different types of social contacts, or across some contexts, without eliciting much detailed information of such exposure. As such, our results do not directly speak to another popular way of measuring disagreement -- a “name generator” based, egocentric instrument (e.g., Klofstad, Sokhey, & McClurg, 2013). In this alternative approach, researchers typically solicit three to five alters with whom an ego routinely and frequently discusses “important” or “political matters,” along with ego’s perception of such alters’ political orientations. While name generator tend to elicit one’s core discussion network of one’s family or close friends (Mutz, 2006), research suggests that perceptual accuracy (and therefore the exposure to disagreement for that matter) involving one’s such close, significant alters are likely to be much accurate than the present case, let alone routine interactions between such socially close dyad makes the perceptual accuracy much higher (Eveland & Hively, 2013).

Third, in determining exposure to “disagreement,” we did not directly utilize the valence and partisan bias of each message, but rather relied on the message poster’s candidate support as the proxy of such valence and partisan bias. While using a message source’s political orientation as the criterion of determining “disagreement” is indeed a very common approach in observational studies of this kind (e.g., Bakshy, Messing, & Adamic, 2015; Bond & Sweitzer, 2018), this implicitly assumes that every out-partisan message exposure involves political disagreement, while any in-partisan message exposure does not. Moreover, such approach does not take into account the possible ambiguities and individual differences in actually “recognizing” disagreement (e.g., “opportunities” of disagreement vs. actual, “experienced” disagreement: see Klofstad et al., 2013, for a similar discussion on this issue). While we acknowledge our measurement strategies -- especially for the objective, behavioral benchmark measures -- may not be the theoretically best-possible operationalization of the concept, doing so nevertheless provides two crucial inferential advantages. First, doing so allow us to overcome the issue of arbitrarily and subjectively determining the valence of each message. Second, our behavioral measure effectively taps maximum possible “opportunities” of disagreement afforded by such exposure -- especially under the assumption that every message conveys an unambiguous, clear signal of partisan bias of the source (poster) of the message, while receivers of such messages also correctly recognize such partisan biases without exception. Therefore, our behavioral benchmark measurements may actually over-represent the actual, if any, experienced disagreement. However, given the fact that one’s self-reported exposure to disagreement already substantially over-estimates this “maximum possible” disagreement exposure, our approach of using a message source’s political orientation as the criterion of “disagreement” makes our overall setup a far more conservative test of hypotheses, providing additional confidence regarding our findings.

Relatedly, we have assumed the behavioral benchmark of “exposure to disagreement” as unbiased, *the* gold-standard against which a respondent’s subjective self-reports are to be evaluated. Unlike other prior studies relying on the client-side access log data (e.g., using the tracking software installed on clients’ PCs or phones: Guess, 2015; Scharkow, 2016), our tracking data are directly available from the access logs collected at the server-side, therefore suffers less from a differential selection bias due to software- or device-specific differences arising from the use of such a client-side data (see Jürgens et al., 2019, for a detailed discussion on this issue). However, all digital trace data is, to some degree, algorithmically confounded with the architecture of the platform that generates such data. A participant’s browsing behavior and resulting access logs -- as we have utilized in this paper -- not only reflect one’s actual exposure to (dis)agreeable information, but also partly imposed and shaped by the architecture of the forum that they rely on to access such information. Had it been based on a different structure (e.g., one similar to Twitter instead of Reddit-like structure), their browsing behaviors could have been manifested in a different way. In this regard, digital trace data also exhibit its own bias and limitations (Jungherr, 2019; Scharkow, 2016), and readers should bear in mind this point in interpreting our findings although a more thorough discussion of this issue is beyond the scope of this paper.

Despite those limitations and caveats, the results of our study reveal several important observations regarding the nature of exposure to disagreement and the accuracy of one’s self-report concerning such exposure. On a more broad level, our results also illuminate the exact *nature* of “perceived” disagreement vis-a-vis actual disagreement. In one of the few prior studies directly tapping the differences between perceived vs. actual disagreement, Wojcieszak and Price (2012) observed that individual characteristics affecting information processing -- in their study, one’s extremity of opinion and emotional intensity -- may systematically color their perception of exposure, leading to differential effect of actual exposure to disagreement on perceived disagreement. Indeed, as an end-product of one’s information processing from actual exposure, perceived disagreement may additionally tap into message “engagements” rather than mere actual exposure alone, the antecedents and consequences of which may be systematically differ from that of mere exposure. Rigorously exploring this discrepancies give us more finer understanding of the interplay between how one’s message environments and one’s cognitive processing affects one’s perception as well as their impact on outcome, let alone the opportunities to directly compare one’s self-reports with the observed behavior is still rare. As Guess and colleagues (2018) once noted, “such cases provide us with a reference standard that is extremely valuable in validating the use of self-reports, as well as in providing appropriate cautions.” (p. 15).

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*Table 1. Predictors of inaccuracy in self-reported exposure to disagreement as a function of social desirability bias, cognitive burden, and public opinion perception (N = 341).*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | **DV: Inaccuracy W2** | | **DV: Inaccuracy W3** | |
| **OLS** | **GLM logit** | **OLS** | **GLM logit** |
| **Social desirability** |  |  |  |  |
| Discussion norm W2/W3 | .016  [-.011, .044] | .379  [-.009, .846] | .017  [-.012, .047] | .234  [-.056, .577] |
| Need for approval W2/W3 | -.003  [-.019, .014] | -.084  [-.368, .179] | -.017  [-.036, .005] | -.076  [-.318, .162] |
| **Cognitive burdens** |  |  |  |  |
| Political interest W2/W3 | .019  [-.002, .040] | .066  [-.260, .424] | .007  [-.018, .035] | .017  [-.279, .329] |
| Political knowledge W1 | -.003  [-.013, .008] | .041  [-.104, .202] | .002  [-.009, .013] | .123  [-.004, .272] |
| **Opinion Climates** |  |  |  |  |
| Prcvd Op Climate W2/W3 | **-.021**  [-.041, -.002]\* | **-.304**  [-.642, -.037]\* | **-.031**  [-.053, -.009]\* | **-.284**  [-.591, -.023]\* |
| R2 (Adj. R2) / Nagelkerke | 0.340 (0.313) | 0.271 | 0.127 (0.093) | 0.124 |
| RMSE | .196 |  | 0.207 |  |
| AIC |  | 339.957 |  | 387.246 |
| BIC |  | 393.604 |  | 440.892 |

***Note****:* DV = reporting inaccuracy (i.e., perception minus behavioral benchmark, using cumulative proportion as reported in Table 1 above. \* = 95% percentile bootstrapped CIs statistically significant (bootstrap N = 10000). All models control for candidate preference, strengths of ideological self-placement, demographics, media exposure, and total amount of exposure (logged) in respective waves. Full results are presented in the online Appendix, Table A1.

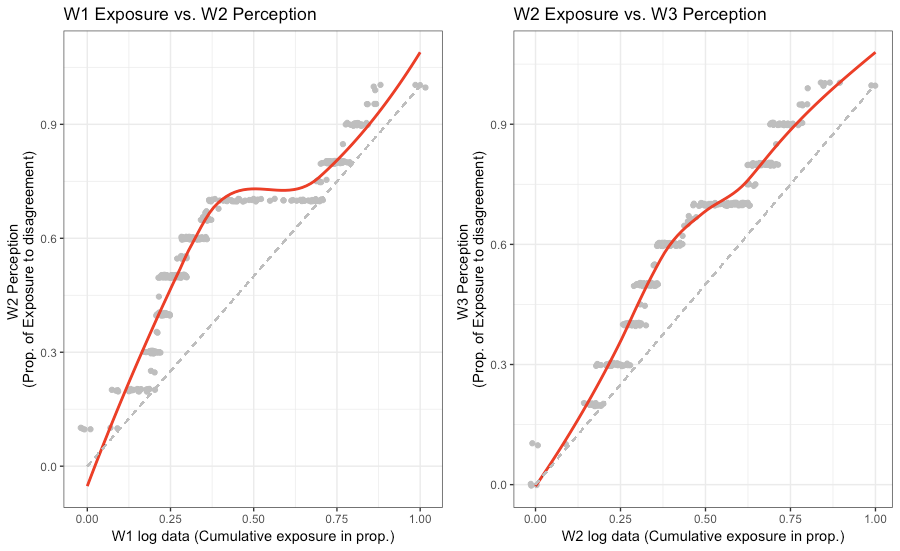
*Table 2*. *Predictors of inaccuracy in self-reported exposure to disagreement, when the three most recent days are used as a benchmark against the self-reported measures (N = 341).*

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
|  | **DV: Inaccuracy W2** | | **DV: Inaccuracy W3** | |
| **Two-weeks** | **3 most recent** | **Two-weeks** | **3 most recent** |
| **Social desirability** |  |  |  |  |
| Discussion norm W2/W3 | .016  [-.011, .044] | .025  [-.006, .057] | .017  [-.012, .047] | .020  [-.011, .052] |
| Need for approval W2/W3 | -.003  [-.019, .014] | .005  [-.012, .023] | -.017  [-.036, .005] | -.011  [-.031, .011] |
| **Cognitive burdens** |  |  |  |  |
| Political interest W2/W3 | .019  [-.002, .040] | .018  [-.003, .041] | .007  [-.018, .035] | -.004  [-.029, .024] |
| Political knowledge W1 | -.003  [-.013; .008] | -.003  [-.015, .008] | .002  [-.009; .013] | .002  [-.010, .014] |
| **Opinion Climates** |  |  |  |  |
| Prcvd Op Climate W2/W3 | **-.021**  [-.041, -.002]\* | **-.023**  [-.046, -.001]\* | **-.031**  [-.053; -.009]\* | **-.033**  [-.055, -.011]\* |
| R2 (Adj. R2) | 0.340 (0.313) | 0.263 (0.234) | 0.127 (0.093) | 0.154 (0.120) |
| RMSE | 0.196 | 0.218 | 0.207 | 0.216 |

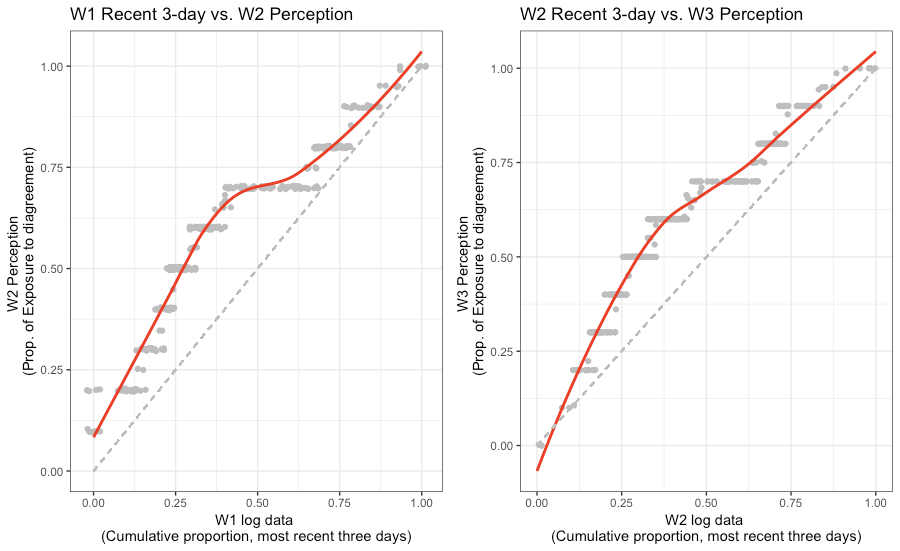
***Note****:* \* = 95% percentile bootstrapped CIs statistically significant (bootstrap N = 10000). All models control for candidate preference, strengths of ideological self-placement, demographics, media exposure, and total amount of exposure (logged) in respective waves. Full results are presented in the online Appendix, Table A4.

*Table 3****.*** *Lagged dependent variable regression models predicting attitude certainty as a function of exposure to disagreement (N = 320).*

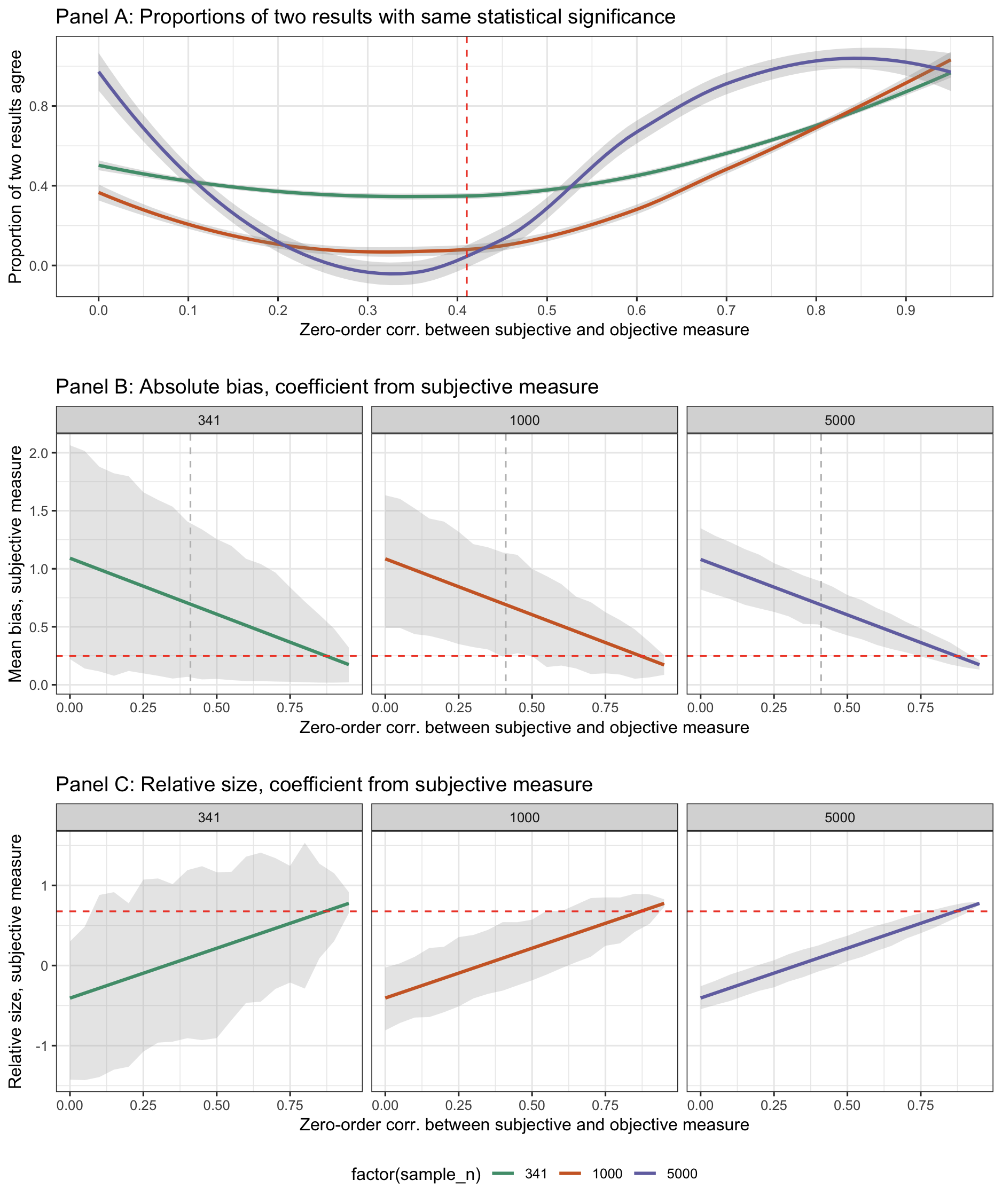
|  |  |  |
| --- | --- | --- |
|  | **Preference Certainty W3** | |
| **Lagged DV** |  |  |
| Preference Certainty W2 | .517 (.046)\*\*\* | .523 (.046)\*\*\* |
| **Focal predictor** |  |  |
| Exposure to Disagreement (Self-report) | -.520 (.266) |  |
| Exposure to Disagreement (Behavioral) |  | -.769 (.340)\* |
| R2 | 0.412 (0.387) | 0.414 (0.389) |
| RMSE | 0.858 | 0.856 |
| \*\*\**p* < 0.001, \*\**p* < 0.01, \**p* < 0.05. All models control for candidate preference, strengths of ideological self-placement, internal efficacy, demographics, media exposure, and total amount of exposure (logged). Full results are presented in the online Appendix, Table A5. | | |



*Figure 1. Quantile-Quantile plots of actual vs. perceived exposure to disagreement.*

****

*Figure 2. Quantile-Quantile plots of actual vs. perceived exposure to disagreement, using the most recent three day of exposure as the benchmark.*

****

*Figure 3. Monte Carlo Simulation Results (simulation N = 1000).*

Note: Panel A shows the proportion of replications (out of 1000) of which two regression coefficients (i.e., “exposure to disagreement” variable using self-reported vs behavioral measures) agree with each other in terms of their statistical significance (*p* < .05 for *N* = 341, *p* < .01 for *N* = 1000, and *p* < .001 for *N* = 5000). For The red dashed line indicates the observed zero-order correlation between two exposure measure (*r* = .41, based on listwise deleted *N*). In Panels B and C, the distribution of absolute bias and relative size from simulations are plotted along with each colored line representing median values from the simulations, with the red dashed line indicating the observed level of bias (= 0.248) and the relative size (= 0.676) when zero-order correlation *r* = .41, as reported in Table 3.

1. It should be noted that individuals are not necessarily always “correctly” remember negative information. Research suggests that while negative information is in general more likely to be remembered, individuals may also “falsely” remember them (i.e., falsely recalling negative information that is indeed not encountered). Nevertheless, negativity bias is showend to be also pronounced in false memory than its positive counterparts (Norris, Leaf, & Fenn, 2018). [↑](#footnote-ref-2)
2. Here, we do not consider another popular approach of measuring “exposure to disagreement” -- the one involving egocentric network survey (e.g., Huckfeldt & Sprague, 1995; Mutz, 2006). We return to this point in the discussion section. [↑](#footnote-ref-3)
3. The structure and interface of the online forum were adopted from the typical format (such as in Reddit). The main page -- which is displayed once participants log in to the website -- displayed the titles of the threads (the latest one at the top), along with the user ID of the poster, click-view counts, and the comments counts (if any). [↑](#footnote-ref-4)
4. Results were unchanged when we utilize different construction of behavioral benchmark measure. Detailed results utilizing this alternative benchmark are presented in the online appendix. [↑](#footnote-ref-5)
5. It should be emphasized that we use the message poster’s reported candidate support as a proxy for actual message valence. While this approach (i.e., using a message source’s political orientation as the criterion determining “disagreement” exposure) is indeed a very common approach in observational studies of selective exposure (e.g., Bakshy, Messing, & Adamic, 2015; Bond & Sweitzer, 2018), we acknowledge this is rather an indirect method. We discuss this issue in the later discussion section. [↑](#footnote-ref-6)
6. Results remain unchanged when we exclude those who supports a third-party candidates or reports that they are undecided from the analysis (excluded *N* = 16, 4.6%). [↑](#footnote-ref-7)
7. Due to the single-item measurement of the concept, we additionally validated our measurement of the opinion perception against several alternative measures; we also performed a series of robustness checks of our main results using such alternative measures. The results of these robustness checks, presented in the Appendix, provided largely consistent results. [↑](#footnote-ref-8)
8. 95% CIs reported here are based on nonparametric resampling-based permutation test (*N* = 20000), where we randomly reshuffle the self-reported measure and the behavioral benchmark measure, derive the null distributions of differences, and compare this null distribution with the observed difference. [↑](#footnote-ref-9)
9. The higher (lower) value of this measure therefore indicates the over (under)-reporting of exposure to disagreement relative to the behavioral benchmark measure. [↑](#footnote-ref-10)
10. It is important to note here that not every user have accessed the forum every same day. Therefore, instead of using the identical three-way time window for every user (which may bias the results since some users might have not at all or only partially accessed the forum in a given time window), we selected the three most recent days *per* each user. [↑](#footnote-ref-11)
11. In the online appendix, we additionally probe the zero-order correlations between behavioral measure and perceptual measures of exposure to disagreement with varying time windows within which behavioral tracking data is corrected (from 14-day windows to 1-day window), yet the results remain substantially the same. See Section 6 of the online appendix for details. [↑](#footnote-ref-12)