

Conscientiousness does not moderate the association between political ideology and susceptibility to fake news sharing

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Abstract

Recent work suggests that personality moderates the relationship between political ideology and the sharing of misinformation. Specifically, Lawson and Kakkar (2021) claimed that fake news sharing was driven mostly by low conscientiousness conservatives. We re-analyzed their data and conducted five new preregistered conceptual replications to reexamine their claims ($N = 2433$; stopping rule determined via Bayesian sequential sampling). The results did not support their claim that conscientious conservatives shared less fake news; instead, their findings pertain to *overall* sharing rates (of both true and fake news), rather than specifically to *fake* news. That is, the association between conscientiousness and misinformation sharing (when it occurs) is explained by lower overall sharing instead of a particular resistance to fake news per se. Our results highlight the importance of distinguishing between overall sharing tendencies and the sharing of misinformation specifically, which have different theoretical and practical implications for how to combat the spread of misinformation.

Keywords: fake news; misinformation; conscientiousness; ideology; conservatism

Data and materials: <https://osf.io/972jm/>

Public Significance Statement

This research challenges the claim that misinformation sharing is mostly driven by low conscientiousness conservatives. We re-analyzed existing data and conducted five conceptual replication studies, which did not find evidence for this claim and showed that low conscientiousness conservatives tend to share more news overall and not more misinformation specifically. Our results highlight the need to distinguish between overall sharing tendencies and the sharing of misinformation.

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Why do people believe and share misinformation online? Recent work highlights the importance of ideology (Jost et al., 2018; Van Bavel et al., 2021). Not only do people tend to believe and share news that aligns with their political beliefs (Allcott & Gentzkow, 2017; Pereira et al., 2018; Pennycook & Rand, 2021a; Pennycook et al., 2020; Roozenbeek et al., 2022), but there is evidence of an asymmetry across party lines: Conservatives and Republicans are more likely to share misinformation than liberals and Democrats (Grinberg et al., 2019; Guay et al., 2022b; Guess et al., 2019; Osmundsen et al., 2021).

A recent set of preregistered studies by Lawson and Kakkar (2021) offered a more nuanced account. They found that conscientiousness robustly moderated the relationship between ideology and sharing, and claimed that fake news sharing was driven mostly by low conscientiousness conservatives. Critically, highly conscientious liberals and conservatives shared similar amounts of fake news. If true, these findings provide important theoretical insights into the (mostly neglected) role of personality in fake news sharing, and suggest that anti-misinformation interventions could focus on low conscientiousness conservatives.

However, Lawson and Kakkar's (2021) analyses focused on how conservatism and conscientiousness interact to predict the sharing of *all* types of news (i.e., both true *and* false news). In fact, their supplemental analyses and results showed that the interaction effect was also observed for true news (contrary to their implied claim that their findings were specific to fake news). Because they did not specifically examine sharing *discernment*, which is the tendency to share true news *more than* false news (Batailler et al., 2022; Pennycook & Rand, 2021a; Pennycook & Rand, 2021b), their conclusions shed light on *overall* sharing tendencies, but not the psychology of misinformation sharing in particular (see Guay et al., 2022a). That is, their findings could simply reflect a tendency for low conscientiousness conservatives to share more of all news (regardless of its veracity)—which is very different from a specific susceptibility to sharing misinformation. For their findings to pertain specifically to false news, it is necessary to observe that conscientiousness moderates sharing discernment (i.e., to observe a three-way conservatism-conscientiousness-veracity interaction).

We therefore re-analyzed their publicly available data (<https://osf.io/ahdsf/>) to examine whether conscientiousness influences overall sharing or sharing discernment. We also ran five new preregistered studies that aimed to conceptually replicate Lawson and Kakkar's (2021) findings. To ensure sufficient statistical power, our studies used Bayesian sequential sampling (Schönbrodt & Wagenmakers, 2018) to determine if there was sufficient evidence to reject null interaction effects (based on Bayes factors).

We sought to replicate the two-way conservatism-conscientiousness interaction on overall sharing and investigate the specificity of the interaction to false news by examining sharing discernment (i.e., testing the conservatism-conscientiousness-veracity interaction). As with Lawson and Kakkar (2021), we also fitted models where we controlled for relevant covariates, though they included only the covariates but not the covariate interactions, and omitting the latter could bias estimates and increase Type-I errors (Simonsohn, 2019; Yzerbyt et al., 2004).

To summarize the key findings, we did not find evidence for a three-way conservatism-conscientiousness-veracity interaction in their and our studies, suggesting that their findings were not specific to false news discernment per se. That is, their findings pertain to overall sharing, but not specifically false news. Surprisingly, we also failed to find Lawson and Kakkar's (2021) two-way conservatism-conscientiousness interaction in any of our studies.

Method

Transparency and Openness

All studies were preregistered prior to data collection, and data and materials can be found here: <https://osf.io/972jm>

Sampling and Exclusion Criteria

We re-analyzed seven relevant Lawson & Kakkar (2021) studies (osf.io/ahdsf) and conducted five new preregistered conceptual replication studies (Tables 1 and 2). Following their recruitment and exclusion approach, we restricted our samples to only U.S. participants and excluded participants with suspicious IP addresses (Kennedy et al., 2020). Those who failed one or more (of three) bot screeners (e.g., “I’m not a bot” captcha) were not allowed to participate in the studies (number excluded in each study: 198, 15, 16, 18, 42). In addition, participants responded to four more attention-check questions throughout the survey. We did not exclude participants who failed these attention checks; instead, we computed an attention score for each participant (0: failed all checks; 1: passed all checks). For the studies conducted on Amazon Mechanical Turk, we also limited participants to those with a HIT acceptance rate of $\geq 94\%$ and had completed between 50 and 50000 HITS. Finally, following Lawson and Kakkar (2021), we did not exclude participants based on whether they had social media accounts.

In our preregistered studies, we used Bayesian sequential sampling to determine whether there was sufficient evidence to reject the null hypothesis (parameter estimate = 0) and whether to stop data collection. We used the sequential design with maximum participant approach (Bayes Factor Design Analysis approach; Schönbrodt & Wagenmakers, 2018) to recruit additional participants until either (a) the Bayes factor (BF) provided strong evidence for the null hypothesis ($BF < 0.1$) or alternative hypothesis ($BF > 10$), or (b) a total of 2000 participants had been reached (but recruited a minimum of 500 participants). We checked the Bayes factors whenever we recruited 250 additional participants. Specifically, we checked whether the BFs for the two-way conservatism-conscientiousness and three-way conservatism-conscientiousness-veracity interactions were > 10 or < 0.1 . In all studies, we stopped data collection at $n = 500$: Most of the BFs for the two- and three-way interactions clearly favored the null hypothesis, regardless of the conservatism measure used for the analysis for each study (Table S1, S2).

Summary of Studies

Study 1 recruited from Lucid and Studies 2 to 5 from Amazon Mechanical Turk (see Tables 1 and 2). Similar to Lawson and Kakkar (2021), the headlines in each study were always politically balanced such that there were equal numbers of liberal- and conservative-leaning headlines (except Study 5 which used mostly non-political COVID-19 headlines). For studies that had both true and false headlines, there were also equal numbers of each type of headline (Table 1). Studies 1 to 3 used stimuli/headlines used by Lawson and Kakkar (2021) and Studies 4 and 5 used headlines that were more “up to date” or relevant for when the studies were run. False headlines in the studies (including Lawson and Kakkar’s studies, which used headlines from previous studies (Pennycook, Bear, et al., 2020)) were determined by using popular fact-checking sites (e.g., snopes.com) and the true headlines

came from reputable mainstream news sources (for exact methodology, see Pennycook et al., 2021).

We measured participants' personality using the BFI-2 personality inventory employed by Lawson and Kakkar (Soto & John, 2017), which measures Big-5 personality factors using 12-items for each factor (5-point scale: Disagree Strongly to Agree Strongly). We included multiple conservatism and partisanship measures in every study but only Study 5 included Lawson & Kakkar's (2021) conservatism/ideology measure.

For the main analyses below, conservatism was measured using the mean of social and economic conservatism items in Studies 1 to 4 ("On social issues I am:" and "On economic issues I am"; 5-point scale: Strongly Liberal to Strongly Conservative), whereas Study 5 used Lawson and Kakkar's (2021) measure ("Please select the option that best describes your political orientation"; 7-point scale: Very Liberal to Very Conservative). In Study 5, Lawson and Kakkar's (2021) measure correlated strongly with our aggregate measure of conservatism ($r = .94$ [.93, .95], $p < .001$), a partisanship measure ($r = .83$; "Which of the following best describes your political preference?"; 6-point scale: Strongly Democratic to Strongly Republican), and an aggregate feelings thermometer (toward Democrat [Republican] voters) ($r = .80$; "How would you rate Democrat [Republican] voters?"; 101-point scale: Very Cold to Very Warm). Note that using these other measures of conservatism or partisanship did not change our conclusions (Supplementary Information).

Each replication study differed from the previous study on one or two dimensions (see Tables 1 and 2). We introduced these incremental changes to each study to understand why our results differed from Lawson and Kakkar's (2021). For example, data quality in Study 1 might be lower because we recruited from Lucid and the headlines in Studies 1 to 3 (which were used in Lawson and Kakkar's (2021) studies) were likely outdated and irrelevant when we ran our studies. Study 5 was the most direct conceptual replication (Table 2) because the only key difference was the use of updated headlines.

Table 1
Study characteristics and descriptive statistics

Study	Platform	Sample	Headlines	Big-5 Reliability (α)	Preregistration	Attention Checks
L&K Study 1	MTurk	$n = 488$ (Mage = 39.6) (55% female, 1% other)	24 COVID-19 ^a	.87, .89, .85, .87, .92	osf.io/mvu2r	–
L&K Study 2	MTurk	$n = 484$ (Mage = 39.3) (46.3% female, 0.6% other)	24 political ^b	.89, .91, .87, .85, .91		–
L&K Study 3	MTurk	$n = 479$ (Mage = 40.4) (51.6% female)	24 COVID-19 ^a	.88, .89, .84, .85, .91	osf.io/bv2yg	–
L&K Study 4	MTurk	$n = 967$ (Mage = 39.2) (51.8% female)	24 COVID-19 ^a	.87, .88, .85, .86, .90	osf.io/ku6fq	–
L&K Study 6	MTurk	$n = 491$ (Mage = 40.4) (54.4% female)	24 COVID-19 ^a	.88, .90, .85, .85, .90	osf.io/98wx2	–
L&K Study S1	Prolific	$n = 954$ (Mage = 32.6) (51.7% female, 2.1% other)	12 false COVID-19 ^a	.87, .88, .86, .83, .91	osf.io/z46r3	–
L&K Study S2	MTurk	$n = 494$ (Mage = 40.9) (38.3% female)	20 false	.84, .89, .79, .84, .86		–
Study 1	Lucid	$n = 490$ (Mage = 46.3) (50.7% female, 2% other)	24 political ^b	.80, .88, .77, .80, .87	osf.io/k9yys	1%, 4%, 17%, 42%, 35%
Study 2	MTurk	$n = 484$ (Mage = 41.8) (49.7% female, 1.6% other)	24 political ^b	.90, .91, .90, .88, .94	osf.io/4hwcv	0%, 0%, 1%, 8%, 91%
Study 3	MTurk	$n = 465$ (Mage = 42.2) (46.5% female, 0.8% other)	12 false political ^b	.88, .91, .88, .87, .93	osf.io/4e8sg	0%, 0%, 0.2%, 9%, 91%
Study 4	MTurk	$n = 495$ (Mage = 41.9) (52.7% female, 2% other)	24 recent political	.88, .92, .88, .87, .94	osf.io/h6gzw	0%, 0%, 0.4%, 12%, 87%
Study 5	MTurk	$n = 499$ (Mage = 41.7) (52.2% female, 2.2% other)	24 recent COVID-19	.89, .89, .86, .84, .92	osf.io/8cnvx	0%, 0%, 1%, 10%, 89%

Note. Unless stated otherwise, half the headlines were true and the other half were false. Same superscripts indicate the same set of headlines. Big-5 five scale alphas are presented in this order: openness, conscientiousness, extraversion, agreeableness, neuroticism. Attention checks: Percentage of participants who passed 0, 1, 2, 3, and 4 (of 4) attention checks, respectively. L&K: Lawson & Kakkar (2021).

Procedure

Participants indicated whether they would share different headlines online ("Would you consider sharing this story online?" (responses: No, Maybe, Yes; coded 0, 1, and 1, respectively, following Lawson & Kakkar, 2021).

Unlike most of Lawson and Kakkar's (2021) studies, participants in our studies only indicated their sharing intentions, whereas their participants were exposed to other questions (e.g., to rate the subjective accuracy of a headline; see Table 1)—which prior work has shown can bias sharing decisions (Epstein et al., 2021). Lawson and Kakkar (2021) also acknowledged this issue: "With Study S2 we aimed to circumvent the concern that asking participants about the accuracy of stories first could bias their responses regarding their sharing intentions" (Supplement p.6). For example, prior research finds that a single accuracy prompt is sufficient to make the concept of accuracy salient and therefore decrease the sharing of false (relative to true) news (Lin et al., 2023; Pennycook, McPhetres, et al., 2020; Pennycook, Epstein, et al., 2021; Pennycook & Rand, 2022). Moreover, since their Study S2 showed that their effects replicated even when participants did not rate subjective accuracy prior to indicating sharing intentions, we therefore opted to ask for only sharing intentions in our studies to avoid biasing participants' responses.

Whereas Lawson & Kakkar (2021) counterbalanced the order in which participants completed the news sharing and personality questionnaires, participants in our studies always completed the news sharing task before completing the personality and demographic questionnaires (i.e., no counterbalancing, since it did not affect their results).

Table 2

Summary of whether key study characteristics were similar to Lawson and Kakkar's studies

Study	Sample	Headlines	Ideology Measure
Study 1	No (Lucid)	Yes (political)	No (5-point social/economic conservatism)
Study 2	Yes (MTurk)	Yes (political)	No (5-point social/economic conservatism)
Study 3	Yes (MTurk)	Yes (political, false)	No (5-point social/economic conservatism)
Study 4	Yes (MTurk)	No (recent political)	No (5-point social/economic conservatism)
Study 5	Yes (MTurk)	No (recent COVID-19)	Yes (7-point conservatism/ideology)

Note. Participants in our studies only indicated sharing intentions. Lawson & Kakkar (2021) also asked their participants to rate the accuracy of each headline before they indicated their sharing intentions. In their Studies 3 and 4, there were also fact-checker warnings or warnings when participants shared false headlines.

Analysis

Following Lawson & Kakkar's (2021) analytic approach, we fitted logistic regressions to model sharing intentions separately for each study. We used the R package *fixest* to cluster standard errors on participants (Bergé, 2018). In addition, we also fitted Bayesian generalized (logistic regression) mixed-effects models with the R package *brms* (Bürkner, 2017) to meta-analyze the studies with three-level multilevel models (observations nested

within participants, which were nested within studies; Studies 1 to 4 used our aggregate conservatism measure and Study 5 used Lawson & Kakkar's 2021 conservatism measure). For each model, we ran 20 Markov chain Monte Carlo chains with 2500 samples and discarded the first 1000 samples (as burn-in). For each effect in the Bayesian model, we report the mean of the posterior samples and the 95% highest posterior-density interval (i.e., narrowest interval containing the specified probability mass). All continuous predictors were z-scored within each study, but headline veracity was dummy-coded (0: false, 1: true).

Results

We first re-analyzed data from Lawson & Kakkar's (2021) seven relevant studies by modeling overall sharing intentions (true and false headlines) as a function of conservatism, conscientiousness, and the two-way conservatism-conscientiousness interaction (share ~ conservatism * conscientiousness). Our re-analysis of their data replicated their reported findings: The negative conservatism-conscientiousness interaction effect was significant in all their studies (Fig. 1). That is, the positive relationship between conservatism and overall sharing (of true *and* false headlines) was attenuated for people who are more conscientious.

However, critically, the negative two-way conservatism-conscientiousness interaction effect was not statistically significant in any of our five conceptual replication studies (Fig. 1). We varied the study characteristics along several dimensions (see Tables 1 and 2) but failed to find negative interactions across studies. That is, the lack of interaction emerged regardless of variation in sample characteristics (Study 1: Lucid; Studies 2-5: MTurk), headlines (Studies 1-3: Lawson & Kakkar's headlines; Studies 4-5: more recent political or COVID-19 headlines), and conservatism measure (Study 5: two different measures of conservatism). We also did not reliably find negative two-way interactions when using different measures of ideology or partisanship (Table S1).

We then performed Bayesian multilevel analyses to compute aggregate meta-analytic estimates. The posterior estimate suggests a robust negative interaction effect in Lawson & Kakkar's (2021) seven studies ($b = -0.43 [-0.51, -0.35]$). However, we found a null effect in our five studies ($b = -0.06 [-0.14, 0.02]$). Across all twelve studies, we found a robust negative two-way conservatism-conscientiousness interaction ($b = -0.29 [-0.34, -0.23]$), suggesting that, overall, the positive relationship between conservatism and overall sharing was attenuated by conscientiousness. However, as per the analyses described above, this effect is driven by the studies from the original paper and not our studies (Fig. 1).

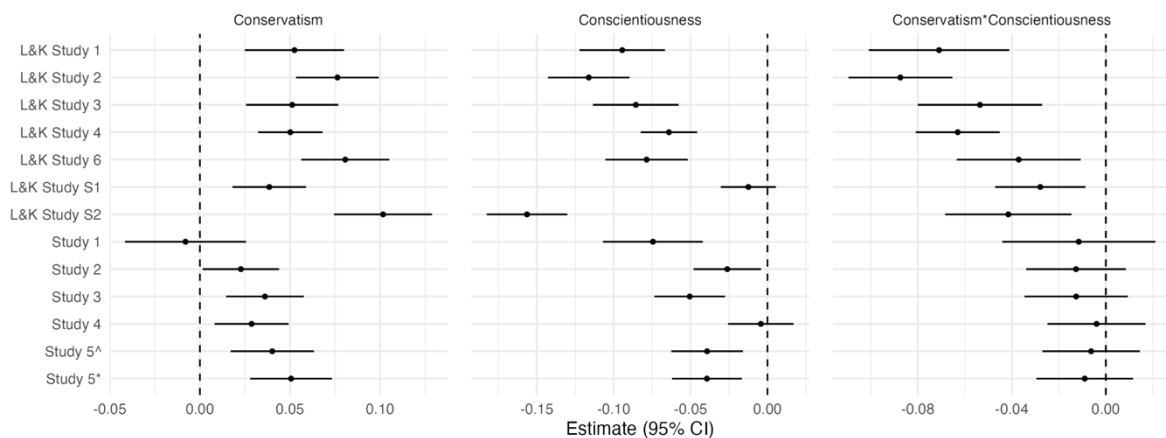


Figure 1. Estimates from two-way interaction logistic regression models predicting the sharing of true and false headlines. Error bars are 95% CIs. Study 5^a: conservatism was the

mean of social and economic conservatism (both measured using 5-point scales). Study 5*: conservatism was L&K's 7-point conservatism measure.

As discussed above, although the previous set of analyses shows that *overall* sharing (of both true and false headlines) depends on the interaction between conservatism and conscientiousness, they do not reveal whether the interaction effect was *specific to fake news*, which is one of Lawson and Kakkar's (2021) main goals (emphasis added): "Across eight studies ... we examine the joint effect of political ideology and personality on *fake news* sharing behavior" (Lawson & Kakkar, 2021, p. 4).

To directly test this key claim, we modeled sharing intentions as a function of conservatism, conscientiousness, headline veracity (coded 0 [false], 1 [true]), and their interactions (share ~ conservatism * conscientiousness * veracity). We did not find evidence indicating a three-way conservatism-conscientiousness-veracity interaction effect (Fig. 2, bottom right panel). A Bayesian multilevel model that aggregated the estimates across all studies led to similar conclusions ($b = 0.02 [-0.02, 0.05]$): Meta-analytically, the positive relationship between conservatism and sharing was attenuated for more conscientious people, but *to similar extents for both true and false headlines* (see also Lawson & Kakkar's (2021) Figure S1, which also shows this lack of three-way interaction). That is, Lawson and Kakkar's (2021) findings pertain to *overall* sharing rates (of true and false news), but not specifically to false news headlines or sharing discernment (i.e., difference in sharing between true and false news).

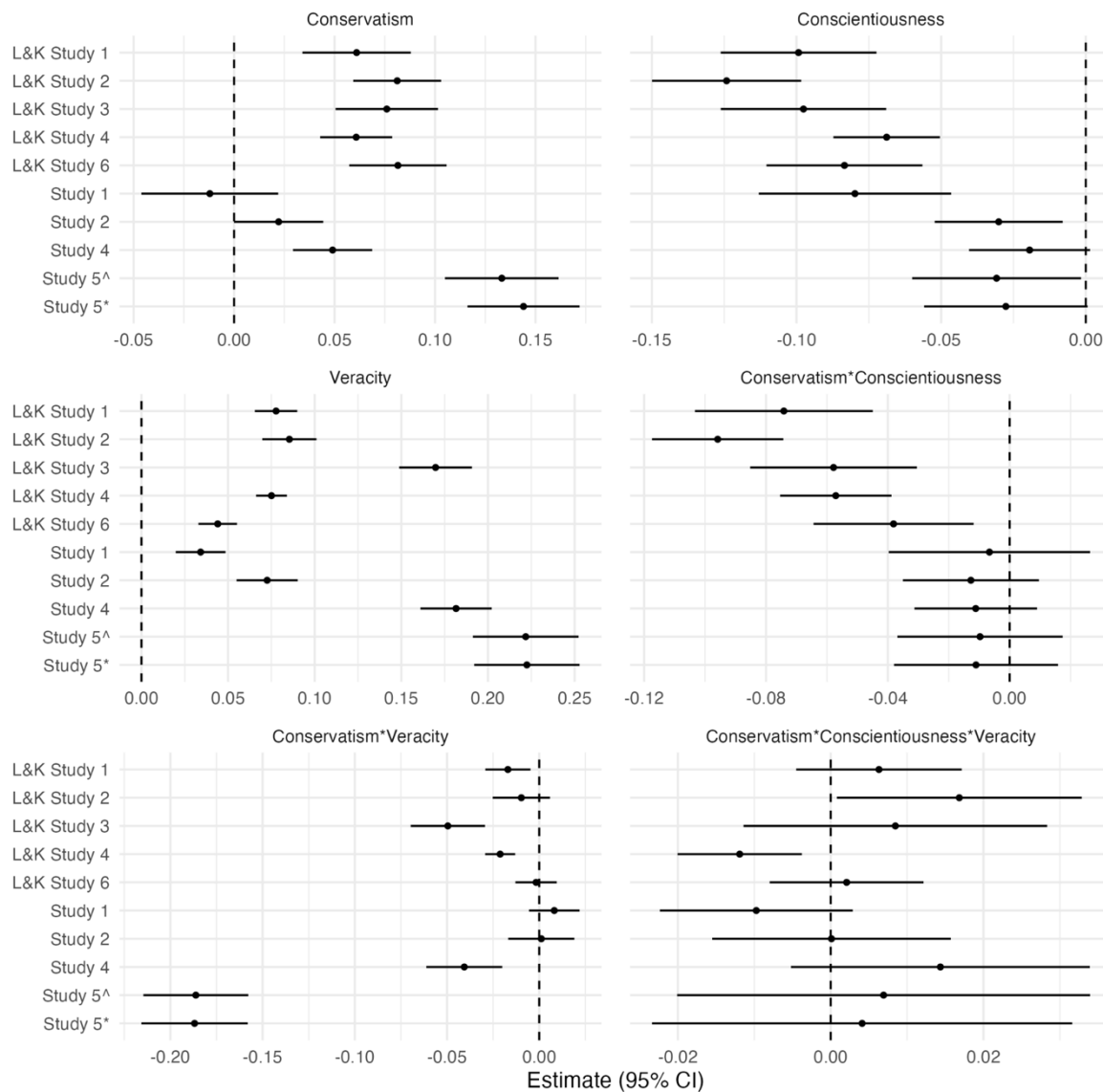


Figure 2. Estimates from three-way interaction logistic regression models predicting the sharing of true and false headlines. Headline veracity was coded 0 (false) and (true). Error bars are 95% CIs. Three studies (L&K Studies S1 and S2, Study 3) were excluded because they presented only false headlines. Study 5^A: conservatism was the mean of social and economic conservatism (both measured using 5-point scales). Study 5^{*}: conservatism was L&K's 7-point conservatism measure.

Finally, we fitted models to our studies to control for relevant covariates. Lawson and Kakkar (2021) included only covariates but omitted the covariate interactions, which could bias estimates and increase Type-I errors (Simonsohn, 2019; Yzerbyt et al., 2004). We therefore modeled sharing intentions as a function of conservatism, headlined veracity (coded 0 [false], 1 [true]), conservatism-veracity interaction, and their three-way interactions with the relevant and preregistered covariates (share ~ conservatism * veracity * (conscientiousness + openness + extraversion + agreeableness + neuroticism + age +

gender + education + attention + AOT¹). As shown in Figure 3, there is a lack of evidence for the two-way conservatism-conscientiousness interaction effect ($b = -0.06 [-0.20, 0.09]$) or three-way conservatism-conscientiousness-veracity interaction effect ($b = 0.04 [-0.07, 0.15]$).

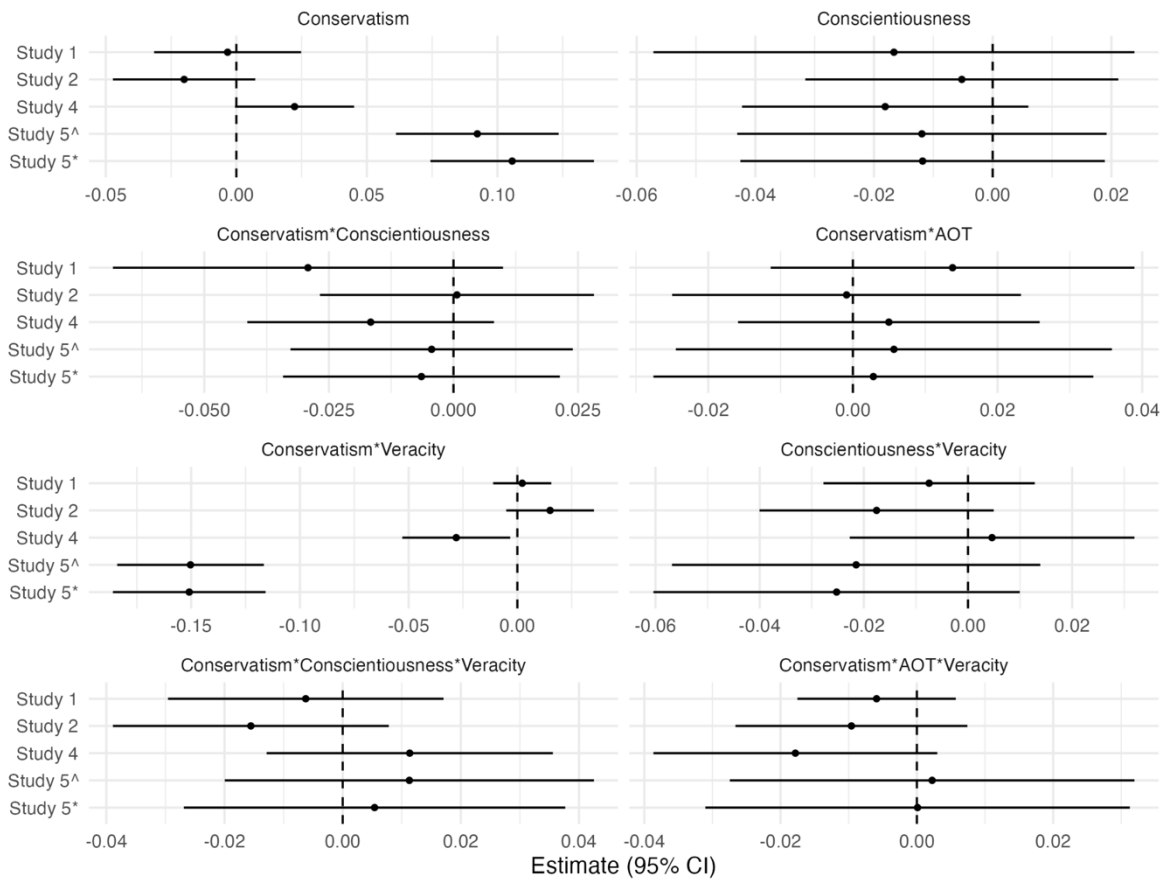


Figure 3. Estimates from three-way interaction logistic regression models predicting the sharing of true and false headlines, controlling for interactions with Big-5 factors, age, gender, education, attention, and actively open-minded thinking (AOT). Headline veracity was coded 0 (false) and (true). Error bars are 95% CIs. Study 3 was excluded because it only had false headlines. Study 5^A: conservatism was the mean of social and economic conservatism (both measured using 5-point scales). Study 5^{*}: conservatism was L&K's 7-point conservatism measure.

Discussion

Recent work by Lawson and Kakkar (2021) argues that conscientiousness plays a key role in moderating the relationship between ideology and the sharing of misinformation. We re-analyzed their publicly available data and conducted five new preregistered studies to reexamine their claims. Our studies were conceptual replications and differed from the original studies in several ways (see Tables 1 and 2), but we also included a very close

¹ Actively Open-minded Thinking (AOT) is a measure of intuitive-analytic thinking styles (Baron, 2008; Baron, 2019; Pennycook, Cheyne, et al., 2020) that correlates with news discernment (Bronstein et al., 2019; Newton et al., 2021; Roozenbeek et al., 2022). People who score higher on this measure can better discern true from false news (Bronstein et al., 2019; Pennycook & Rand, 2019a), even for news that aligns with their political ideology (Pennycook & Rand, 2019b).

replication (Study 5; only difference is that we used more “up-to-date” headlines). We did not find evidence for the central claims made in their paper in any of our five studies.

Lawson and Kakkar (2021) found robust two-way conservatism-conscientiousness negative interactions in their studies, which led to the conclusion that “the sharing of fake news is largely driven by low conscientiousness conservatives” (p.1). But we found that their claims were *not* specific to false news, even in their data. The conservatism-conscientiousness interaction effect was not significantly different for true and false headlines in their studies (i.e., no conservatism-conscientiousness-veracity interaction was observed; see also Supplementary Information in Lawson & Kakkar, 2021, Figure S1, Table S1). Thus, their findings pertain to *overall sharing*, but not specifically *false news* (i.e., sharing discernment): Low conscientiousness conservatives are more likely to share *all* news (i.e., true *and* false news) than high conscientiousness conservatives or liberals. Results from our conceptual replications provide further evidence against their claims that their studies addressed specifically *misinformation* sharing (or sharing discernment).

Surprisingly, despite the robustness of the two-way conservatism-conscientiousness interaction effect on *overall* news sharing in Lawson and Kakkar’s (2021) studies, this interaction effect was not significant in any of our five conceptual replication studies. Nevertheless, the meta-analytic effect across all nine studies shows that conscientiousness attenuates the positive relationship between conservatism and overall news sharing, but this effect was driven by Lawson and Kakkar’s (2021) studies.

Recent work that investigates who believes or shares misinformation have focused largely on cognitive factors like analytic and actively open-minded thinking (Bronstein et al., 2019; Newton et al., 2021; Roozenbeek et al., 2022). Thus, Lawson and Kakkar’s (2021) alternative personality account, if supported by data, could provide important theoretical insights into how personality traits like conscientiousness are associated with misinformation sharing, which could lead to practical recommendations on who anti-misinformation interventions should focus on.

However, data from their and our studies do not provide evidence for the claim that the personality trait they have identified—conscientiousness—was associated with specifically misinformation sharing. Our findings highlight an important conceptual issue: The factors that influence overall sharing (e.g., conscientiousness) may be different from those that influence the sharing of misinformation per se. As such, factors that influence *overall* sharing tendencies may or may not be related to *misinformation* sharing *in particular* (Pennycook & Rand, 2021a; Pennycook & Rand, 2021b).

Our study designs differed from Lawson and Kakkar’s (2021; see Tables 1 and 2) in several ways because we systematically tweaked our designs to investigate and understand why we could not replicate their findings. These differences and our failure to replicate their findings across our five conceptual replication studies suggest a lack of generalizability (Yarkoni, 2020). Crucially, we could not replicate their findings even in Study 5, which was the most direct replication. Our results indicate that conscientiousness and political ideology may not have reliable effects even on overall news sharing.

Constraints on Generality

Only Study 1 recruited representative samples (from Lucid). Studies 2 to 5, however, followed Lawson and Kakkar (2021) by using convenience samples from Amazon Mechanical Turk (they also used non-representative samples from Prolific). When the main variable of interest is political ideology, it may be better to recruit representative samples that

are quota-matched to the US population on different demographic dimensions (e.g., Pennycook & Rand, 2022). Our results might be specific to the political context in the United States, which is unique among Western democracies for its two-party system and extreme polarization.

In summary, our findings highlight the importance of distinguishing between overall sharing tendencies and the sharing of fake news or misinformation. Investigating the individual difference variables that influence overall sharing versus the sharing of misinformation are equally important, but the latter is more likely to provide specific insights into who spreads misinformation and why certain people are more likely to share it.

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