ssColor Codes for comments

Matt (measurement and modeling + news attraction)

Dan (trait v. state)

Trevor (text data questions and misc. responses)

Completed responses

**Response Letter**

Paper Title: News ‘Attraction’ and Digital Inequalities: Incidental News Exposure and the Equalization or Stratification of Political Information

ID: 223259527

**Editor’s Comments**

**Comment:** We have now received the reviewers’ reports on your paper “News ‘Attraction’ and Digital Inequalities: Incidental News Exposure and the Equalization or Stratification of Political Information." Your paper has more reviews than the usual two. We initially received divergent reviews, which led as to reach out another two reviewers to make a robust assessment of your piece. That said, we apologize for the long wait this paper had to go through.  
  
In general, the reviewers had positive Comments about the manuscript, but there is still work to do for this piece to be ready for publication. As such, we are offering you the opportunity to revise and resubmit your paper addressing the suggestions made by the reviewers as well as the Digital Journalism editorial team.

Reviewer 1 invites you to better visualize your results, while Reviewer 2 would like you to reflect on some concepts and measures in the study. Reviewer 3 requests a better justification for presenting competing hypotheses, as well as stronger arguments to back up some conceptual definitions (such as news engagement or news attraction). This reviewer also has many methodological observations. Reviewer 4 would like you to discuss the context of your study, and invites you to provide more information for the methods section regarding sampling, variables, and models. This reviewer also provides helpful insight to strengthen your discussion section.

**Response**: Thank you to the editor(s).

**Comment:** In addition, the Digital Journalism editorial team would like you to respond to the following:

Sample and Data

The study relies on a cross-sectional survey of adult social media users in the US, but there is no explanation as of how these users were recruited. Did you hire a polling company, perhaps?

**Response**: Yes, Qualtircs

**Comment:** Also, the paper indicates that “survey responses were linked with social media content collected via Brandwatch (formerly Crimson Hexagon) and then validated by crosschecking content lists with CrowdTangle.” It’s not clear why survey responses needed to be validated, and how (what is that you validated, exactly?).

**Response**: We lacked precision here. We collected publicly available posts from major news organizations during the same period as the rolling cross-section collections. In this sense, the datasets are not directly related at the individual level. Though, in the news awareness items, we used the top story of the day as a quasi ‘stimuli.’

**Comment:** In addition, validating survey responses with social media data requires knowing the respondents’ usernames. Does this violate IRB concerns about anonymity/confidentiality? Please explain.

**Response**: Again, no personally identifiable information was collected.

**Comment:** Is there any reason to measure age as intervals instead of a continuous ratio variable?

**Response**: Precision beyond age categories was not needed to answer our research questions.

**Comment:** Why did you impute missing values in a 2,000-case sample? Did you have too many missing values? How big was your sample if you didn’t input missing data?

**Response**: We followed best practices for imputation using XX method according to YY authors. Certainly, we did not have to impute, as the sample was large enough, but recent advances in statistics in this are ensure a less biased measurement when missing data is imputed under the conditions of random missingness \*(MAR: perhaps report a simple test for randomness). If we fail the randomness test, consider running alternate analysis to show no real difference only if we fail the random missing test.

**Comment:** Exposure and Engagement; The methods section describes ‘trait-like’ and ‘state-like’ properties of news exposure, but none of this is explained in the front end, let alone the most adequate way to measure these properties. For instance, total exposure to political information was measured with Weeks et al., 2017’s selective exposure items, but it’s not clear why ‘trait-like’ properties should be measured as selective exposure. Please address the ‘trait-like’ and ‘state-like’ concepts in the lit review and explain the rationale to measure them.

**Response**: Thank you for raising this issue, as we view our theorizing and measurement of incidental exposure on both trait- and state-levels to be an important contribution. We have now offered clearer definitions of trait and state incidental exposure early in the paper (p. 4) and more consistently integrated this distinction throughout the introduction. These changes offer a stronger rationale for why we measure incidental exposure on these two levels (see p. 9).

**Comment:** Please indicate if you ran a factor analysis to create the high-effort engagement variable, to make sure the items loaded together.

**Response**: This comments gives us the opportunity to fine-tune our analysis. We performed an exploratory factor analysis and found that the items load onto a single factor. We had conceived of the high-effort variable as a conceptual robustness check, but considering the results of the factor analysis, we have decided to drop the variable from the paper completely. We would be happy to approach the issue differently at the suggestion of the editorial team.

**Comment:** Controls; What are the theoretical reasons to control for political ideology, party identity, and identity strength? These variables are significant in most of the models, so there is clearly something going on (especially if you are studying exposure to political information) but these significant findings are not discussed in the paper. You might want to give it some thought.

**Response**: Well OFC these have to be included as they are direct co-founds for models of news attention/exposure and various outcomes for political behavior in general (Cite). Need to add to the discussion section here.   
  
**Comment:** Limitations; Finally, you mention self-reported measures as a limitation, pointing out this is an endemic issue to survey research. However, some of the self-reported measures in this study are particularly problematic. For instance, how reliable is asking respondents whether they clicked on a story, scanned the headline or read it entirely, Commented, discussed, etc.? These actions are not easy to remember. The same might happen with network size (is this something you could cross validate with Brandwatch or CrowdTangle?). Please elaborate on this issue.

**Response**: Well, that is why we used the rolling cross-sectional design in the first place. We actually have several measures of attention/engagement this way. We chose the most widely circulated news of the day, for that last news cycle, then had them respond only under the condition that they were at least aware of that specific story. In this sense we are adding validity by jogging people’s memory. This approach is not novel, but it is not particular common either (see Citations). These measures are imperfect but rely on the best practices for survey research that we know of…

**Reviewer: 1**

**Comment:** The way in which the findings are presented in this paper will contribute significantly to the development of the field of incidental news exposure. The results are well structured and the methodology used is clear. The limitations of the study are also declared. It is suggested to incorporate tables that allow faster visualization of the results in the different variables used.

**Response**: Thank the reviewer for their time and response.

**Reviewer: 2**

**Comment:** This manuscript investigates whether the use of digital media reduces or exacerbates inequalities in news exposure and engagement based on survey data. The paper is very well-written and deals with an important topic. The literature review section is thorough and makes logical sense. The analyses are rigorous. Limitations (e.g., cross-sectional nature of the data) are also well-noted. Overall, I highly value this paper and recommend this paper be published in this journal after addressing the following concerns.  
  
**Response**: Thank the reviewer for their time and response.

**Comment:** While the author's way of measuring IE is more sophisticated compared to the previous way of measuring this concept which relied on a single item, the author(s) still need to acknowledge that it is still very difficult to measure IE with the survey. Survey respondents are not good at distinguishing to what extent their exposure was "accidental" or "purposeful." The author(s) can mention this point in the limitation section.

**Response**: Add limitation note.

**Comment:** I don't think the author(s) explained what story they chose and why they chose this topic. I need more information.

**Response:** Here, note the nature of news exposure in contemporary society, ala technology and heterogeneous content feeds and news as ‘flows’, etc. as well as the popularity to field as exemplified by the numerous studies and special issues (citations).

**Comment:** When measuring social media use, the author(s) used a single item. It is much more desirable to use multiple items when measuring this concept since a) different social media platforms have different functionalities and b) survey respondents may not take the term "social media" in the same way. For instance, some may think "YouTube" is not social media. Likewise, some may think of WhatsApp as social media (while some may not). So, the authors better use multiple items, or even if the authors end up using a single item, the authors need to at least add examples in the parenthesis such as "social media (e.g., x,y,z)."

**Response:** We agree with the premise of this comment and indeed try to rely on multiple-item measures, as well as more advanced reliability scales and techniques where appropriate (as evidenced by our approach to measurement of key IVs and DVs). However, in this case, that particularly survey item was validity checked against several other measures in a live, large N experiment that included both self-report and observed, live social media usage data (see XX). Thus, our decision here was purposeful, and it reflects the state of the art in this area.

\*Note, add a citation for that measure if it is not there…

**Reviewer: 3**  
  
**Comment:** This paper addresses how incidental news exposure and news engagement relate to the concept of “news attraction.” The authors attempt to explicate and measure this concept and then test it to better understand if incidental exposure can help reduce informational inequalities. My primary concerns lie in the presentation of the hypotheses and in the operationalization of the “news attraction” variable. I have presented my concerns in the order they appeared in the paper:

**Response**: Thank the reviewer for their time and response.

**Comment:** The quotes on page 6 need correcting to capture which portions of the sentence are directly quoted.

**Comment:** I’m not following the paragraph that leads to H1a and H1b. Why would overall exposure be equal for those who are high and low in news attraction? It seems by definition that those high in news attraction would have greater levels of overall exposure.  
There’s a typo in H1a and H1b as well as H2a and H2b (“and” should be “in”).

I generally don’t like competing hypotheses. In some instances, competing hypotheses are used to ensure a supported hypothesis is possible no matter the results. I would prefer a stronger theoretical argument that leads to a specified prediction. Given that it’s difficult to do this ethically after data has been analyzed, I would like to see the authors do a better job justifying the decision to present competing hypotheses.

I would like to see a stronger argument for why “news engagement” matters in this context. For example, is engagement necessary for someone to learn from news content? It seems that simple exposure could prompt some knowledge gain. What exactly does engagement add to people’s knowledge gain process that is not available through exposure? Perhaps a stronger argument could be made for more in-depth processing (e.g., central route) occurring during “news engagement.”

**Comment:** The paragraph before H2a and H2b suggests interaction effects, but the hypotheses are not written to predict interaction effects. Please consider revising.

**Response**: We’ve taken your suggestion and revised the H2a and H2b to more explicitly predict interaction effects.

**Comment:** Was the trait-like scale for total exposure specific to social media use? It seems that specifying this information should come from social media would be important to this measure.  
Asking people to know if they came across information accidentally is asking a lot of their memory and accuracy. Are there prior studies that validate the use of this measure as an accurate way to know that someone encountered information accidentally? (The limitations of this self-report measure should be more thoroughly addressed in the paper – not just briefly touched on in the limitations paragraph.)

**Response**: Yes, the questionnaire did specify “information on social media.” Thank you for pointing out this oversight. We have clarified the measure on pg. 13.

We agree that self-reported measures are limited. However, we would offer three defenses of the approach.

First, the measures are indeed based on prior literature (Weeks et al., 2017), and this paper is citied in the measures section.

Second, the primary limitation of self-reported measures is inaccurate recall. Prior research shows that survey respondents tend to underestimate their exposure, producing point estimates that are systematically lower than the “true” population parameters (see González-Bailón & Xenos, 2020). This is a form of systematic measurement error. According to King et al. (1994), systematic measurement error will bias descriptive inferences. However, it does not necessarily bias causal inferences if all variables are affected in the same way. That is, if both the independent and dependent variables are systematically underreported, the estimates of the strength of the relationship will not be biased. Considering the nature of the variables in our study (attraction, exposure, engagement), there is a high likelihood that they are all biased in the same way, which counterintuitively means that the effects estimates are likely *not biased.*

We have expanded our discussion of these limitations in both the measures (pg. 11) and limitations sections (pgs. 25-26).

González-Bailón, S., & Xenos, M. (2020). Surveys underestimate online news exposure: a comparison of self-reported and observational data in nine countries. *SSRN Electronic Journal*.

King, G., Keohane, R. O., & Verba, S. (1994). *Designing social inquiry: Scientific inference in qualitative research*. Princeton university press.

Third, despite their limitations, we respectfully argue that self-reported measures are still the best way to measure news exposure in the general population. While advances in computational methods allow for direct observation of exposure, it is difficult for researchers to obtain reliable and externally valid data from social media companies (notably, Twitter is now charging $45k per month for their research API). This has been a particularly important issue for Facebook and other Meta platforms, as researchers have had a difficult time obtaining privacy-protected data directly from these companies. Web scraping techniques and third-party apps violate their terms of service. Collaborative research initiatives (i.e., Social Science One) ultimately failed, and research awardees never received the data they were promised due to a reversal from Meta. Finally, Meta’s proprietary platform (CrowdTangle) does not provide individual-level data. Rather, it only provides aggregate engagement metrics for particular posts. Thus, despite the fact that direct news exposure data exists, it is not readily available to academic researchers (including us).

Web-tracking software provides another approach that researchers can employ. However, participation rates in web-tracking studies are generally low, and combining survey data with web-tracking data results in study attrition that alters the contours of the sample. These studies are best treated as “small-N” or “phenomenon-driven” studies rather than as studies of the general population. Therefore, we respectfully assert that while self-reported measures are indeed limited by poor recall and critiquing them has become common in our field, they still provide valuable insights into the relationships between exposure and other variables of interest in the general population.

**Comment:** Why was total exposure multiplied by incidental exposure? Would the results change if these scales were not combined in this way (i.e., if the models were rerun with these variables assessed independently)?

**Response**: The incidental exposure question was designed as a corollary to the total exposure battery. That is, it’s placement in the survey (directly after the total exposure battery), means that respondents should have their previous responses at the top of their minds when answering the incidental question. Thus, the incidental question makes an indirect reference to the total exposure question and was not intended to be a stand-alone measure.

For that reason, we were not particular interested in modeling the interaction between the composite dimensions of our incidental exposure measure. Rather, our aim was to create a measure that is both internally and externally valid, and we think that our multiplicative measure accomplished that aim.

**Comment:** Why was a separate “high-effort engagement” variable created? This needs more justification both theoretically and operationally.

**Response**: Thank you for raising this issue. We had conceived of the high-effort variable as a conceptual robustness check. However, we performed a factor analysis at the request of the editors and found that the items load onto a single factor. Therefore, we decided to drop the high-effort variable from the paper completely.

**Comment:** It’s not clear to me how the second measure of news attraction, “self-reported interest,” is a news-related variable. Is political interest inherently part of news attraction? This measurement seems to suggest that someone who is interested in news and politics would necessarily get news on social media. Including interest as a covariate in the model makes sense, but it’s not clear to me why it is part of a measure of “news attraction.”  
Given the context of the study, I would encourage the authors to consider relabeling “news attraction” to “social media news attraction.” The crux of the argument surrounds exposure to news on social media.

**Response**: This is a valid concern, and we agree that it is important to be specific when measuring key variables. However, the three items are highly correlated and form a reliable scale, which reduces random measurement error in comparison to single-item measures. Additionally, the models are slightly less efficient (i.e., there is slightly more noise) if only the single item (i.e., the news interest item) is used, which is to say that the slope estimates are not substantively different, but the standard errors are slightly larger (but not large enough to alter the statistical significance of results). Therefore, we would prefer to leave the variable as is, and we have made specific mention of the measure in the limitations on pg. 25. That said, we would be willing to alter the variable if you believe this is the best approach.

**Comment:** Per my previous Comment, it seems entirely possible that someone could have high levels of political interest but only get news from non-social media sources.  
You address this previous point in the “regression analyses: exposure” section of the paper. I would like to see the overlap of non-social media news use and social media news use parsed out better in the literature review, specifically as it relates to “news attraction.” It wasn’t clear to me that “news attraction” would apply both to non-social media news as well as social media news, in part due to the way the variables in the news attraction variable were operationalized. I would like to see these decisions better justified (both conceptually and operationally).

**Reviewer: 4**  
  
**Comment:** This paper elaborates on the concept of ‘news attraction’ to examine the extent to which digital media users are exposed to news intentionally or accidentally, and how incidental exposure is related to engagement with news. It uses data collected through a cross-sectional online survey fielded in the USA during the 2020 elections. The results provide evidence of a paradox: while the online environment can be an equalizer of exposure to news across social groups, it can also stratify engagement with the news.  
  
There are many things to like about the paper. It tackles a relevant topic, namely, whether digital media reduces or exacerbates inequalities in passive and active news use. It is theoretically rich, covering the key works in the literature on incidental exposure. The statistical analysis is more sophisticated than what is typical for papers using cross-sectional data, as it includes a latent class analysis, estimates hierarchical models, etc. The appendix is helpful, too, as it enables readers to assess the representativeness of the sample and the robustness of some findings.  
  
Having said that, I see several areas for improvement, especially regarding the context of the study, the methods used, and discussion sections. Let me elaborate on these shortcomings.

**Response**: Thank the reviewer for their time and response.  
  
**Comment:** Somewhat ironically (as the authors make highlight the importance of studying the context of media exposure), I missed a discussion on the context of the study. Empirical findings are always bounded by cultural, temporal, and other forces. The polarized American media and political systems are rather unique in the world. I’m sure incidental exposure to political news and current events differs between election and nonelection years. All this is to say that it will greatly benefit the international audience of the journal if the authors include one or two paragraphs in the methods section about the particularities of the US case that are relevant for this particular study.

**Response**: You’re absolutely right on this point. We’ve taken your suggestion and added a “Context of Study” section after the hypotheses and before the Methods (pgs. 10-11).   
  
**Comment:** The methods section is lacking important pieces of information, and many of the authors’ choices are not justified or explained. This has the unfortunate consequence of making the statistical models less parsimonious and the results harder to follow.

**Response**: Thank you note and point to changes in the paper ala other responses to reviewers…

**Comment:** Why were the data collected using a rolling cross-sectional design (RCSD) instead of a single one-shot design? I’m asking considering that the longitudinal aspect of the survey does not seem of any relevance to this study.

**Response**: Beef-up the explanation of the RCS and explain the utility here (copy/paste from comment above to the editors about the tie-in of concurrent social media data). Add sources if not in the text.

**Comment:** The data was analyzed using multilevel modeling. According to the authors, this was justified: it helps to control for measurement invariance across the 17 sampling frames employed. But the authors should be aware that MLM are harder to interpret for the lay reader than a simple, straightforward OLS. Perhaps comparing the robustness of results across using different estimators would help answer the question of how dependent the results on MLM are.

**Response**: Thank you for raising this concern, as it gives us a chance to stress-test our analysis. We spot-checked robustness using Model 1 (DV: Incidental Exposure) and Model 3 (DV: Total Exposure) in Table 1. In both cases, model comparisons show that the MLM approach fits the data better than the OLS approach, with lower AICs and statistically significant log-likelihood tests (for Model 1, chi-square = 5.16, p = .023, indicating the MLM is a slightly better fit; for Model 3, chi-square = 29.17, p < .001, indicating MLM is a much better fit).

Furthermore, the coefficient estimates are similar in both cases. For Model 1 the key estimates are Low = 1.15 (MLM) vs. 1.14 (OLS); Medium—Unmotivated = 0.62 (MLM) vs 0.62 (OLS); Medium—Motivated = 0.36 (MLM) vs. 0.37 (OLS); and High = -0.16 (MLM) vs. -0.14 (OLS). The differences are slightly bigger for Model 3, but not big enough to alter the paper’s substantive conclusions: Low = 1.12 (MLM) vs. 1.09 (OLS); Medium—Unmotivated = 0.79 (MLM) vs. 0.82 (OLS); Medium—Motivated = 1.22 (MLM) vs. 1.30 (OLS); High = 1.59 (MLM) vs. 1.67 (OLS).

We would be happy to add a footnote to the manuscript describing these robustness checks if the reviewer feels this is appropriate and would clarify the findings for the readers.

**Comment:** How much data was missing to justify the use of multiple imputation using chained equations? Do the results change if missing data is not imputed? The same with the weighting scheme.

**Response**: Copy/paste comment from above. Need to perform quick MART test.

**Comment:** Do results change when using unweighted data? I’m asking because weights, while helping to address deviations from the population distribution, inflate standard errors, too.

**Response:** In this particular case, the weights do not inflate the standard errors. We spot-checked robustness using the same two models before, and the unweighted standard error estimates are very similar to the weighted estimates. We prefer to use the weighted models given the nature of the survey sample, and we would be happy to add a footnote explaining these robustness checks. Briefly, the key standard error estimates are (order = Low, Medium—Unmot, Medium—Mot, High) are:

Model 1 weighted: 0.05, 0.06, 0.08, 0.11

Model 1 unweighted: 0.05, 0.06, 0.08, 0.10

Model 3 weighted: 0.06, 0.05, 0.07, 0.10

Model 3 unweighted: 0.05, 0.06, 0.07, 0.10

**Comment:** More importantly, how were survey responses linked with social media content, exactly? And the validation of content lists with CrowdTangle, what was that and why was it necessary? And if most stories come from Fox News, what does this say about the representativeness of the media stimuli?

**Response:** Copy/paste note from above to the editors.   
  
**Comment:** I find it somewhat confusing why some IVs are described as covariates and others as control variables. Covariates predict variance in the DV that is clearly not attributable to the IVs of interest. That’s why socio-demographics are usually covariates. Control variables, however, are included to eliminate spurious relationships between the IV of interest and the DV that might otherwise be thought to be causal. Again, more explanation would be helpful.

**Response:** You’re right. Our terminology was needlessly confusing. We cut all mentions of covariates and now refer to all variables as controls.   
  
**Comment:** In the concluding section, I missed two central aspects. First, a discussion of how the study findings relate to the most important function of news: to produce informed readers. There is a larger debate on the consequences of the digitization of news and the rise of social media on citizen competence. Questions such as: what do people learn from they news? Why is social media news use related (or not) to misinformation? These are important questions, and I would like to know the implications of this study on those issues.

**Response:** Good questions. OFC not the focus of the study, but certainly there are implications for leaning. Need to add few sentences to the discussion.   
  
**Comment:** Second, for a paper that mentions repeatedly concepts such as datafication and algorithmic categorization, I expect some a more thorough discussion of what the study findings mean for the debate on the platformitization of news in the current media environment. The last paragraph of the paper hints at this. I’m sure the authors can elaborate more.

**Response:** We may need to reconsider or be more careful about this language.  
  
**Comment:** Last but not least, please proofread the manuscript. Some propositions are missing, there are spelling problems, etc.

**Response:** Do this and thank the author.