M Gmail

<div align="right">Trevor Diehl <diehl.trevor@gmail.com></div>

Re: [EXTERNAL] 223259527 (Digital Journalism) A revise decision has been made on your submission

2 messages

Dan Lane <dlane@ucsb.edu>   Tue, Jan 31, 2023 at 6:28 PM
To: Matthew Barnidge <mhbarnidge@ua.edu>, Trevor Diehl <diehl.trevor@gmail.com>

phew.... a long wait and a long review. Please let me know how you want to tackle this and feel free to assign me any component you'd like!

Hope you guys are well...
D

On Tue, Jan 31, 2023 at 8:42 AM Matthew Barnidge <mhbarnidge@ua.edu> wrote:

Dear Prof. Westlund,

This is great news! Thank you for the opportunity to revise. We'll submit the revision as soon as possible. Thanks again!

Matt

Matthew Barnidge

Assistant Professor, Journalism & Creative Media

The University of Alabama
Reese Phifer 488B
Tuscaloosa, AL 35487
Phone 205-348-7544
mhbarnidge@ua.edu | https://jcm.ua.edu/

THE UNIVERSITY OF ALABAMA®

From: Digital Journalism <onbehalfof@manuscriptcentral.com>
Sent: Monday, January 30, 2023 1:01 AM
To: Matthew Barnidge <mhbarnidge@ua.edu>
Cc: oscarwestlund@gmail.com <oscarwestlund@gmail.com>
Subject: [EXTERNAL] 223259527 (Digital Journalism) A revise decision has been made on your submission

Dear Matthew,

Thanks for your patience, and apologies for the delay.

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.

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? 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?). In addition, validating survey responses with social media data requires knowing the respondents' usernames. Does this violate IRB concerns about anonymity/confidentiality? Please explain.
• Is there any reason to measure age as intervals instead of a continuous ratio variable?
• 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?

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

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.

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.

Below you will find the reviewers' comments; we hope you will find they assist you in the process of revising your paper.

I look forward to receiving your revised manuscript. Please ensure that it is in an editable format, preferably a Word or rtf file format. PLEASE BE SURE TO HIGHLIGHT AMENDED TEXT TO HELP THE EDITOR TO SEE HOW YOU HAVE RESPONDED TO THE REVIEWERS' COMMENTS. It may also be helpful to indicate in your response to reviewers how you responded to reviewer comments, as well as any that you chose not to address, highlighting your reason for doing so. This should be a separate file and should also be anonymous. Your title page should set out the Title, author name(s) and affiliation (department, institution, country) and e-mail address, and any Acknowledgements and Funding information. Tables and figures are to be sent as a separate document.

In accordance with our format-free submission policy, an editable version of the article must be supplied at the revision stage. Please submit your revised manuscript files in an editable file format.

To submit a revision, go to https://nam11.safelinks.protection.outlook.com/?url=https%3A%2F%2Frp.tandfonline.com%2Fsubmission%2Fflow%3FsubmissionId%3D223259527%26step%3D1&data=05%7C01%7Cmhbarnidge%40ua.edu%7Caf476dda5b2d4e418e6608db028fc95d%7C2a00728ef0d040b4a4e8ce433f3fbca7%7C0%7C0%7C638106588813549294%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzIiLCJBTiI6Ik1haWwiLCJXVCI6Mn0%3D%7C3000%7C%7C%7C&sdata=2as%2FzeIbby1GqGQdXpwXHRUX%2B6sxrrln2qhE27mlV1A%3D&reserved=0. If you decide to revise the work, please submit a list of changes or a rebuttal against each point which is being raised when you submit the revised manuscript.

If you have any questions or technical issues, please contact the journal's editorial office at RDIJ-peerreview@journals.tandf.co.uk.

IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.

Please use the attached template to indicate how you responded to reviewer comments, as well as any that you chose not to address, highlighting your reason for doing so. You can upload this as a separate file when submitting your

revised manuscript. This file should also be anonymous.

Because we aim for the timely publication of manuscripts submitted to Digital Journalism, please upload your revised manuscript as soon as possible and before 31-Mar-2023.

Once again, thank you for submitting your manuscript to Digital Journalism, it has been good to work with you.

Best wishes,

Oscar Westlund
Editor-in-chief, Digital Journalism
Professor, Oslo Metropolitan University

Reviewer(s)' Comments to Author
Reviewer: 1

Comments to the Author
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.

Reviewer: 2

Comments to the Author
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.

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.

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

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)."

Reviewer: 3

Comments to the Author
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:
The quotes on page 6 need correcting to capture which portions of the sentence are directly quoted.
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."

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

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.)

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)?

Why was a separate "high-effort engagement" variable created? This needs more justification both theoretically and operationally.

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. 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

Comments to the Author
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.

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.

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. 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. 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. 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. 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. 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?

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 sociodemographics 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.

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.

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 platformization of news in the current media environment. The last paragraph of the paper hints at this. I'm sure the authors can elaborate more.

Last but not least, please proofread the manuscript. Some propositions are missing, there are spelling problems, etc.

--
Dan Lane (he/him)
Assistant Professor
Department of Communication
University of California Santa Barbara
UCSB Digital Political Inequality Lab
Personal Website

Matthew Barnidge <mhbarnidge@ua.edu>   Thu, Feb 2, 2023 at 12:37 PM
To: Dan Lane <dlane@ucsb.edu>, Trevor Diehl <diehl.trevor@gmail.com>

Thanks, Dan! I'll make some time this week and next to organize the comments, and I'll get back in touch about next steps. Hope all is well with you and your family!

Matt

Matthew Barnidge

Assistant Professor, Journalism & Creative Media

The University of Alabama
Reese Phifer 488B
Tuscaloosa, AL 35487
Phone 205-348-7544
mhbarnidge@ua.edu | https://jcm.ua.edu/

THE UNIVERSITY OF ALABAMA®

From: Dan Lane <dlane@ucsb.edu>
Sent: Tuesday, January 31, 2023 5:28 PM
To: Matthew Barnidge <mhbarnidge@ua.edu>; Trevor Diehl <diehl.trevor@gmail.com>
Subject: Re: [EXTERNAL] 223259527 (Digital Journalism) A revise decision has been made on your submission

[Quoted text hidden]