

### **General Comments:**

After reading through the authors responses, they put in a lot of effort to address the feedback from all three reviewers. They addressed all my comments from the first round. Furthermore, their new statistical analysis combined with drought indices adds more nuance to the study's original findings, which is a great to see.

I had one lingering general comment that is described in more detail in my line comments below. Briefly, I'd like the authors to add more discussion on whether/how the interpretation of their results may differ if they are to choose one of the more parsimonious models in Table S1.

Based on my general comment and the handful of line edits below, I recommend this manuscript be accepted with minor revisions. Thanks again to the authors for their interdisciplinary approach to looking at drought through the lens of newspaper media. I found the paper very interesting and am grateful for the opportunity to serve as a reviewer.

### **Line Edits**

Line 139. I suggest removing the word “package” as, from my reading, the authors are referring to the entirety of the R software, not a specific R package. The final sentence would then read: “The process was performed in R, version 4.3.2...”

Lines 153-159. Pointing out that this text is all underlined because I'm not sure if that was intentional. I do not think the underline is needed.

Line 178-179. Can the authors please provide a sentence explaining what R package they used to do the negative binomial regression analysis and cite the package? I believe it's, the MASS R package, correct?

Line 180. If AIC values are within two units of one another, they are not (statistically) significantly different. As a result, it's typical to choose the most parsimonious of the models tested within the two-unit difference. That said, I recommend the authors consider whether/how the findings of their work may differ if they were to use, for example, the model with only SPI12 and seasonality (Table S1 row 3). I'd like to see more discussion of this in the results section. It would also be great to include a third line for that model on Figure S2, if that helps to show the comparison in performance.

Lines 383-386. I think this point is very interesting and appreciate the authors adding this discussion of nuance here and in the abstract.

Table 1. I appreciate the authors including this table with the R model outputs and their interpretations, but I recommend they move this level of detail to the supplement.

Figure 2. Overall, this is an interesting visual! I wanted to point out that I do not see a lilac-colored grid on the top; it looks grey in the attached PDF. Since there is no color scale bar given, I recommend the author's explicitly give a description in the caption that lighter hue saturation indicate higher article counts and hydroclimatic anomalies (either larger negative or positive) whereas darker hue saturation indicate the opposite.

Table S1. Is there a way for the authors to include R squared or something similar in this table that would give a sense of how much variation in article counts each model explains?