EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-494-RC2, 2022

Comment on egusphere-2022-494

Anonymous Referee #2

Referee comment on "Exploring TikTok as a promising platform for geoscience communication" by Emily E. Zawacki et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-494-RC2, 2022

Dear authors,
First, I would like to congratulate you on the 'Terra Explore' initiative. The metrics data shows that Tiktok can be a very promising platform to help geoscience outreach. Also, it explains some important features of TikTok to geoscientists unfamiliar with the platform. Your paper can have a lot of potentials, but it requires major revisions to critically reflect on the empirical data. You need to rethink your argument, research question, methodology, and scholarly contribution to increasing public exposure to and/or effective geoscience communication.
First of all, it's not clear which research gap this paper tries to address. The goal to increase the visibility of geosciences and geophysics on TikTok is achieved to some extent. The 2 million views in 4 months are impressive, but there is almost no description of why would and how the authors created those very popular videos in such ways. Are there any different considerations when the authors made those videos? Is there any hypothesis that led the authors to make those videos? Or the success videos are simply due to good luck? Above all, what has video creators learned from the process? I think that the authors should reflect on this process from a critical standpoint.
The authors listed two goals in the manuscript: To increase the visibility of geosciences and to determine the best strategies for effective geoscience communication on TikTok. However, the engagement is not discussed thoroughly, and the listed data are insufficient to analyze the geoscience communication's effectiveness on TikTok. Statistics of likes, views, and percentage of watched are useful metrics for showing the engagement of videos on TikTok but more importantly, stemming from the previous point, the authors should give their reasons. Although some design factors have been discussed loosely, like memes and music, the authors may want to give a more clear message about why some of the videos you made are more popular than others (at least your hypotheses and using your data and experiment design to test them). The current analysis and the reported data are not sufficient. I am wondering why the authors did not do any qualitative analysis of comments (considering you have so many comments and some of them are not relevant to learning). Lacking experiment design and the reported data as written hardly can give an insightful answer. I will recommend the authors analyze the contents of the comments and use a mixed method to critically discuss the potential reasons. This leads to another potential weakness: the literature review on how to design geoscience videos to engage the public on social media is insufficient.
Also, the authors may want to do a more thorough literature review about the existing geoscience efforts on TikTok and how your efforts differ from others (again, what research gap this paper tries to address). The current literature review gives a valuable introduction

to TikTok for geoscientists and communicators unfamiliar with TikTok, but this is not a research gap. Without this, this manuscript is not a rigorous research inquiry. The authors mentioned several possible theories, such as place-based video design (may want to use field-based or location-based design since the place-based can be misleading to place-based educational design) and geohazard event-based video design. However, there is no literature review about these designs or what is the template of the authors' video design. Very little is said about how this effort and results extend generalizable knowledge about how communicators can replicate the success of designs.

The authors may want to discuss what their hypothesis works, what may not, and how they predicted the videos' success on TikTok (considering there is no existing formula or clear pattern in the data). How is feedback incorporated into the development of new videos? Although this work shared valuable data to confirm and highlight the possibility of TikTok, especially the unique advantage of FYP and TikTok algorithm for new geoscience communicators, it is not novel enough to consider extending our knowledge boundary about geoscience communication on TikTok.

For example, what are the teaching goals of each video? why do some of the videos get so many views? While some others are not. Is it the content? The narrative? The visuals? The overall design? Why they were recommended more than others? Is it really because of the geohazard feature? If so, the authors should organize their information better. The GVM could be a good visualization, but the evidence is not enough to be convincing. The paper tested several metrics but didn't show convincing evidence of what factors can increase the visibility of geosciences and geophysics videos. The reported results have no clear pattern to determine what type of videos or design would be more popular. As written, the amount of GVM videos (9 videos) is not enough to discuss the statistical significance of the effectiveness of this design element. This is the same as the geohazard videos. (A minor suggestion: The authors may want to define the 'lecture-style video'. Is it a universal definition or just for TikTok?)

Moreover, two million views in 4 months with only 48 videos are amazing, but does the views of TikTok comparable to those of YouTube videos? Considering its extremely short durations (5-60sec), a video on the same topic can be much longer. This question also applies to the percentage of watched and the likes. On YouTube, IRIS channels also have many good geohazard videos, but it looks like they didn't get so many views. Is it the timeliness, the platform, the audiences, or the design? I think the authors have the special advantage in giving a good discussion about this.

Furthermore, the work provides some valuable data. But, many of the figures have little value and may be removed or revised. More importantly, the results alternate between assertions about the evidence presented, point-of-view statements that are not identified, and overreaching claims. For example, Line 278 to 280, "Videos 40 sec to 2 minutes in length received the highest engagement rates (Fig. 10)", Line 282 to 284 "High engagement rates on videos with lower view counts …a wider audiences..", I cannot find a clear pattern or empirical evidence supporting these claims.

Minor notes:

Line 30-32, 291-299: Interestingly, there is almost no literature review or efforts mentioned or discussion about using YouTube to communicate geosciences (the authors even talked about Twitter in the introduction). Shouldn't YouTube be a more comparable video-based social media platform to TikTok? Especially considering IRIS, UNAVCO, and Open Topography all have their YouTube channels. Is there a particular reason?

Line 60: I think the current limitation for TikTok videos is 10 minutes, not the 3mins anymore.

Literature Review is not enough. As mentioned at the beginning, the literature review introduces some interesting concepts and facts about TikTok and science communication on TikTok. Still, these do not carry over to the study's variables and measurement.

Regarding figure 5, why do you not report the statistics of shares? Have any of your videos been shared and viewed on Twitter or embedded on other websites? How will these be categorized? Will they be categorized into FYP, Personal Profile, or Unknown? This may affect how videos are being found and watched.

Line 350-355: If the gender of audiences on TikTok cannot be accessed or the data is unreliable, then a more thorough discussion of the limitation should be added.

I personally want to see more data about the timing of geohazard events and the release time of videos. Is there any relationship between the speed of release and the views of the video?

The demonstration using food could not be the major reason for different views. One possibility in my mind is the interests of the topic (e.g., more people have motivations in learning the magnitude of an earthquake than the types of faults), is there any useful comments to give insights about this?

The authors may want to clarify how this work contributes to broader theoretical debates like how geohazards affect or how place-based design affects engagements. Current discussions are not critically reflected on the empirical data (including yours and others).