Dear Luigi,

Thank you very much for the candid and overall favorable review. We are very pleased that you enjoyed reading our perspective and appreciate your valid criticisms. We are grateful to have this stimulating open discussion with such an esteemed expert in the field.

I (Ben) fully accept your concern that I have been overly autoreferential and it is a fair point that there are several sentences where alternative (or additional) citations could be appropriate. However, I should note that the vast majority of my self-citations (15!) are led by students, postdocs, and other coauthors, whose work I am eager to draw more attention to. None-the-less, it is possible I have over-interpreted the "perspective" element of this piece, and of course I am most familiar with papers my coauthors and I have published. If this leaves you with an unpleasant impression, surely at least a few others will agree. To avoid this, I followed your suggestion and carefully read through to determine when I could work harder to find alternative citations, versus when a self-citation is indeed necessary or the most appropriate option. For example, we can cite alternative papers for the source-responsive model (i.e., Nimmo and Mitchell, *VZJ*, 2013 instead of Mirus and Nimmo, *WRR*, 2013); in contrast, there is no replacement for the Sitka dataset (Smith et al., *USGS,* 2023). As you noted, some self-citations "are justifiable (if not due)" for this type of perspectives piece, but I am confident that I could cut the number down considerably, especially limiting my own first-authored citations to just a few. Additionally, I will be sure that whenever possible, relevant self-citations are accompanied by any other complimentary works (i.e., in addition to Mirus and Loague, *WRR*, 2013, add Lanni et al., *ESPL*, 2013; or in addition to Lehmann et al., *GRL,* 2021, add Van Looy et al., *Rev. Geophys.*, 2017).

Regarding your other very constructive suggestion of adding more discussion on data-driven approaches, my coauthors and I agree that many of these approaches have considerable merit for process-based landslide forecasting (e.g., one self-reference to Orland et al., *GRL*, 2020). You are correct, these merit some further mention in our perspective. As you and I have discussed (e.g., at WLF6 in Florence), data-driven models are a great approach for replicating some observed relationships. Indeed, with enough data they are providing further insights and tools to quantify the spatial and temporal occurrence of landslides in well studied areas (e.g., Lombardo et al., *Earth Sci. Rev.*, 2020). While we may have underemphasized these approaches in our perspective, we also wanted to avoid a detailed discussion or voicing any criticism of methods we have not fully explored in our own research; the paper is a perspective, not a critique (or even a comment piece). However, as is perhaps more appropriate for this critical discussion, we take the opportunity here to point out that data-driven models can be great where there is data, but naïve machine learning models are strongly biased by the data they consider and thus may struggle to capture the observed hydrologic response and landslide triggering in areas *without* sufficient data. This data-bias effect may be less of an issue with physically based models, that capture the (incompletely) known physics of infiltration and drainage (albeit there are other biases we discuss related to the assumption of diffuse flow equations). Regarding the specific papers you mention, we have examined these and others to inform our responses to each set of papers below.

Yes, Stefan Steger's works (i.e., *NHESS,* 2023; *Front. Geosci*, 2024) are indeed very relevant, so are other space-time modeling studies authored by you and your colleagues (e.g., Lombardo et al., *Earth Sci. Rev.,* 2020) as well as contrasting efforts (e.g., Bordoni et al., *Landslides,* 2019). We had neglected to draw the connection between landslide forecasting used in early warning and promising potential shown by data-driven space-time modeling approaches such as these. As you all have noted, there is an issue of data bias and Stefan has proposed novel ways to deal with this (i.e., Steger et al., *Geosci. Front.*, 2024). However, some issues remain, including the need for extensive data on landslide timing, as well as the assumption that future triggering conditions will be controlled in a similar manner to past ones. In some cases these can be overcome, in others, more data is needed. We can revise the manuscript to reflect our perspective on the potential value of these novel contributions and acknowledge that our paper focuses on how to inject hydrologic understanding from in-situ monitoring and simplified process-based modeling studies.

You also note the excellent body of work by Stanley, Kirschbaum, and others, which we must also point out does not provide an explicit pathway for early warning of landslides, but rather for "nowcasting" of global landslides conditions. Still, they initially used the AR7 weighted rainfall index for LHASA V1 (Kirschbaum and Stanley, *Earths Future*, 2018) and then transitioned to a probabilistic approach using the 99% percentile of extreme daily rainfall for V2 (Stanley et al., *Front. Earth Sci*., 2021). Furthermore, in V2 they now consider SMAP data for antecedent conditions, but as we discuss, the satellite estimates of hillslope hydrology are often inappropriate for capturing hillslope drainage conditions (Thomas et al., *WRR,* 2019), not to mention the considerable latency of ~3 days. So, overall, theirs is great uniform approximation for generalizing the role of past rainfall in landslide triggering globally, but in our view this broad approach precludes practical application for local or regional landslide early warning that reflects an improved hydrologic understanding. While the global model output is interesting in that it can be applied everywhere to facilitate uniform risk and exposure assessments (e.g., Emberson et al., NHESS, 2020), it is potentially quite inappropriate for informing mitigation measures at local scales. For example, Marc et al. (*Earth Interactions*, 2022) nicely demonstrated that even after extensive testing a simple rainfall recurrence-interval based threshold has great promise for only about half of the events they considered. We suggest that for actually improving local or regional scale warnings that lead to actionable information, quantifying the local and regional factors that control antecedent conditions, infiltration, and drainage dynamics, is a promising path ahead. In the revised manuscript we will strive to stress these nuances for landslide warning without delving into the advantages or limitations of the LHASA model.

Thank you for pointing us and other readers to the study of Pudasaini and Krautblatter (2021), which is fascinating and indeed injects an unprecedented degree of mathematical formalism into landslide runout behavior. However, for this particular study we don't see a clear connection with our section on extrapolating through space and time, or how this relates to landslide warning, or even how it is relevant to the problem of *when* or *where* landslides will initiate. Perhaps you can more eloquently make this connection between improved runout modeling and landslide warnings in a future publication, but for the present manuscript we are not likely to include this in a meaningful way to add to our discussion about hydrologic information.

Overall, for the reasons discussed above regarding the current state of space-time modeling (e.g., Steger et al., 2024) and global-scale now-casting (i.e., Stanley et al., 2021), we advocate for a wider global network of hillslope hydrological monitoring, precisely so that there are more grounds for understanding and testing landslide hydro-climatology and how those may change in the future under climate change scenarios. Perhaps data-driven models are ultimately a good way to leverage that data more effectively than we have with more site-specific physically based or empirical thresholds models, but that is an argument for a later date when more of such data are available. For the present paper, I intend to read, and in some cases re-read the articles you suggest, and my coauthors and I will consider how to integrate further discussion on the advantages and drawbacks of these data-driven approaches.

Once again, we are very grateful for your time and keen insights, and certainly think an in-depth exploration of your own perspective would be equally worthwhile for some future publication. Personally, I am eager for further conversations with you on these topics moving forward.

Warmest regards,

Ben, Thom, Roberto, and Manfred