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Reply on RC2

Sébastien Biass et al.

Author comment on "Insights into the vulnerability of vegetation to tephra fallouts from interpretable machine learning and big Earth observation data" by Sébastien Biass et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-79-AC2, 2022

We are grateful to Reviewer 2 for his reviews. Pleas find below a detailed answer to his comments.

Point 1

Earth Observation data provides a global observation network in space and time, which can be used as an indirect proxy to infer surface processes. EO sensors always balance three types of resolutions (i.e., spatial, temporal and spectral) to achieve specific purposes. For instance, Landsat provides 30 m pixels at a poor temporal resolution whereas MODIS provides 250 m pixels at a higher frequency. With this in mind, the EO community is entirely aware that satellite imagery is not able to resolve the complexity suggested by Reviewer 2, especially when, in our case, the methodology considers large areas and require dense time series. However, EO data enables the monitoring of vegetation over widespread areas, for which MODIS is often the preferred sensor. Therefore, we feel that i) the manuscript already extensively covers literature focusing on vegetation monitoring from EO data and ii) expressing and detailing this uncertainty is beyond the scope of the paper. Please note that all points stated in our discussion and future objectives target this exact purpose through validations and comparison with field mapping.

Point 2

We partly agree with this statement. We agree that impact mechanisms have been discussed for a long time in the literature, both from an ecological perspective (i.e., references proposed by Reviewer 2) and from an "impact" perspective (i.e., the dominance of references proposed in our manuscript), with the conclusion that no consensus is yet possible. One limitation to this is the opportunistic nature of studies in the field, which are too limited (both in number and in spatial coverage) to provide a

sufficiently large number of observations required to capture the full variability of the involved processes (e.g., eruption types, climates, crop and vegetation types, etc.). We would fully agree with Reviewer's 2 comments should our method ambition to replace these field-based studies. However, the motivation for our method is the realization that generalizable models of volcanic impacts – at least for disaster risk reduction perspectives – probably will never be developed using only field-based studies, and we therefore explore here an alternative way to generalize these in situ observations rather than replace them. In addition, we fully acknowledge the limitations of our methodology, and limit causal inference to specific case-studies where impact mechanisms suggested by various sources point to supporting our interpretation. We therefore feel that most issues raised by Reviewer 2 in this comment are comprehensively addressed in our manuscript and supported by more recent literature, although using an impact rather than an ecological perspective.

Point 3

This statement has been deleted. Please note that i) the timing of the eruption relative to the phenological cycle of the plant is mentioned in the same paragraph, for which we have added relevant references and ii) further investigations of this relationship is identified as the first point for future iterations of the method in the discussion section.

Point 4

The method (and the CDI) are indeed based on vegetation indices, which provide a proxy for biomass production. Here, we attempt to provide a proxy for impact. Following comments of Reviewer 1, the purpose and limitations is now more detailed and discussed in the perspective of existing techniques. In a nutshell, the CDI is designed to not only capture negative impacts but, on the longer term, to also capture the recovery. It was developed with the idea of quantifying impact as a budget (i.e., comparing short-term negative losses with potential long-term gains in fertility), and presents many advantages to estimates rates of impact and recovery compared to existing anomaly quantification methods. We believe that changes made to address Reviewer 1's comments also address this issue.

Minor comments

- Line 32 : Done
- Line 125: The map was reworked according to both reviewer's comments
- Line 175: Done
- Line 190: Done (and good to know!)
- Line 267: Done
- Line 269: Done
- Line 270: This sentence was rephrased following reviewer 1's comments
- Line 310: Done

- Line 800: Done