

## **Response to Editor's Comments**

Dear Dr. Macedonio,

We would like to thank you for your careful and thorough review of our manuscript throughout all stages of the process, from the major revisions to this final minor revision stage. We have incorporated all the changes highlighted in your latest report.

Please find attached the revised manuscript with all modifications indicated in red. For clarity, we provide below a point-by-point response to the comments you raised.

Kind regards,

Helena and Martin

**1- Concerning the manuscript structure, the manuscript could be improved by moving methodological and study area descriptions (e.g., sensor types, rock permeability) out of the "Results and Discussion" section and into their respective dedicated sections to avoid interrupting the narrative.**

The corresponding descriptions have been relocated to Sections 2.2 and 3.2 and are highlighted in red in the revised manuscript.

**2- Reviewer #2 strongly suggests adding clear subsections within "Results and Discussion" (e.g., \*Precipitation\*, \*Tidal forcing\*, \*Degassing\*) to guide the reader. Ensure a sharper boundary between the "Introduction" and "Study Area".**

We have introduced clear subsections (Sections 4.1 to 4.3) within the "Results and Discussion" section to improve readability and better guide the reader.

**3-About the scientific claims, I invite you to slightly moderate your tone: Since the causal link between RWE variations and CO<sub>2</sub> emissions remains largely qualitative and complicated by differences in spatial and temporal resolution, please temper strong assertions like "excellent agreement" or "clear interplay."**

We have moderated the corresponding statements throughout the manuscript, replacing or removing expressions that could be interpreted as overly strong, and ensuring a more cautious interpretation of the relationship between RWE variations and CO<sub>2</sub> emissions.

**4- Be highly explicit about exactly what variables and stations are being compared across paragraphs.**

We identified an omission in Section 4.2 concerning the explicit identification of the variables and stations being compared, which has now been corrected in the revised manuscript.

**5-In addition, please, discuss whether the 3 cpd feature is location-dependent based on existing literature.**

We have added a discussion on the potential location dependence of the 3 cpd feature in Section 3.1.3. The revised text reads as follows:

*“The applicability of the 8-hour barometric tide in other regions is expected to depend on its spatial variability. While the 24-hour and 12-hour atmospheric tides exhibit relatively consistent large-scale patterns (e.g., McMillan et al., 2019), the 8-hour component is less well constrained and can vary significantly with longitude and regional atmospheric conditions (e.g., Moudden and Forbes, 2013), as it may arise from the superposition of multiple tidal components. Therefore, although the 8-hour signal provides a useful diagnostic in La Palma due to its spectral isolation, its detectability and amplitude may differ substantially elsewhere, and its applicability should be evaluated on a site-by-site basis.”*

**6- Furthermore, explicitly state the magnitude of the actual pressure change (e.g., in Pa or kPa) to help readers assess the method's feasibility in other environments.**

We have explicitly included the magnitude of the pressure variations in Section 3.1.3 to clarify the feasibility of the method.

**7- Concerning the barometric pumping, described in Material and Methods, probably, an explicit declaration that your method is based on this approach would be useful (see General Comments of reviewer #1).**

In the revised manuscript, barometric pumping is explicitly described in the Materials and Methods section.

**8-When detailing the effect of atmospheric tides on the vadose zone, broaden the scope to include the role of water (soil moisture and capillary water), rather than just the gas/air phase. This directly supports your later discussion of hydrological changes.**

We have revised the text in the Introduction to better reflect the multiphase nature of the vadose zone. In particular, we replaced “gas transport” with “fluid movement” to acknowledge that both air and water can respond to pressure variations in this region. While the focus of this study remains on gas-phase processes, this modification ensures consistency with the role of soil moisture and capillary water discussed later in the manuscript.

**9-Please, ensure the code availability link is explicitly provided within the manuscript text, and carefully proofread the manuscript to remove unwarranted hyphens and ensure correct punctuation.**

We have ensured that the code availability link is explicitly included in the manuscript. In addition, the text has been carefully proofread to correct punctuation and remove unnecessary hyphenation.

## Response to Referee #2

Dear Referee #2,

As in the previous round, we found all your comments helpful and believe they have improved our manuscript. Most of your general and minor comments were also raised by the Editor and have been addressed accordingly. We therefore provide below a response only to the remaining comment not covered in the Editor's report.

Kind regards,

Helena and Martin

- 1- I am wondering, why the authors present different cpd. In my opinion it weakens the arguments made by the authors. It seems there is no need for a specific pump to observe the features, because it is very similar across all selected bands.**

We thank the reviewer for this observation. The different cpd components are presented to illustrate that not all frequency bands exhibit the same behavior in the seismic response. In our results, the 3 cpd component stands out because it shows a clearer and more pronounced variability, particularly during periods that are consistent with enhanced degassing.

Importantly, the 3 cpd band corresponds to a known atmospheric tidal component, whereas nearby frequencies do not have a clear physical forcing. Its distinct behavior is therefore consistent with the physical framework described in the manuscript, supporting its interpretation as a meaningful diagnostic rather than a redundant feature.

## Response to Referee #3 Manuela Köpflí

Dear Dr. Köpflí,

We sincerely appreciate your comments, which we found constructive and helpful in improving the manuscript. We agree that a more concise presentation can enhance the impact of the work. At the same time, one of the reasons for choosing this journal was its allowance for a more extended manuscript length, which we considered appropriate for presenting our results in full detail.

For the remaining comments not addressed by the Editor, we provide responses below.

Kind regards,

Helena and Martin

**1- More importantly, while the method itself appears promising, the presented results do not yet convincingly demonstrate that volcanic degassing can be reliably monitored or quantified using RWE. The correlations between the ellipticity variations and CO<sub>2</sub> emisión remain qualitative. The differences in spatial coverage and temporal resolution make the comparison difficult to interpret.**

We acknowledge this limitation in the original version of the manuscript. In the revised version, we have strengthened the analysis by including a quantitative comparison between RWE variations and CO<sub>2</sub> emissions. Specifically, we now present Pearson correlation coefficients (see Figure A2 in the Appendix), which provide a quantitative assessment of the relationship between both datasets despite their differing spatial coverage and temporal resolution.

### **2 – If degassing prior to eruption is rare (l. 31-33), why do we care?**

We thank the reviewer for this comment. Although pre-eruptive degassing is not always observed and does not necessarily lead to an eruption, it constitutes a geohazard in itself and reflects changes in subsurface conditions that are relevant for volcanic monitoring. As stated in the abstract, this study proposes a complementary methodology to track such processes using seismic observations.

Our aim is not to establish degassing as a direct eruption precursor, but rather to provide a tool that can help volcanologists better characterize subsurface dynamics. In this context, we consider it important to explicitly acknowledge that not all degassing episodes lead to eruptions, in order to avoid misinterpretation, particularly for non-specialist readers.

We have clarified this point in the revised manuscript by explicitly stating that, although degassing anomalies prior to eruptions are rare, such episodes still constitute a geohazard and provide valuable information on subsurface processes, even in the absence of an eruption.

