Robert Buchwaldt 04/21/2025

In his submitted paper, the author attempts to explore a volcanic hazard assessment of the Otavalo Canton, which is surrounded by two active volcanoes: Imbabura and Cuicocha. To achieve this goal, the author uses a georeferenced dataset of wind direction combined with economic census data.

Nevertheless, while this is a worthwhile regional endeavor, the paper in its current form is far from being understandable or publishable in any academic journal.

I understand that none of the authors are native English speakers, but the paper is filled with spelling and grammatical mistakes to the extent that several sections are unreadable and difficult to comprehend. A basic spell and grammar checker—readily available in most writing programs—would significantly improve the structural form of the paper as a first step. However, more importantly, the authors must put in substantial effort to improve the language throughout the manuscript.

Beyond the language issues, the paper is difficult to follow. The introduction is overly wordy and does not get to the point. This is a scientific paper, not a historical pamphlet about volcanoes. At no point do the authors introduce the Otavalo region, the motivation for its volcanic hazard assessment, its relevance to a broader audience, or what novel contributions this paper offers to the field of hazard risk assessment. I still do not understand what Mount Vesuvius has to do with this study.

The authors also fail to explain and justify the methodology they used. What are the advantages of their approach? What are the new insights brought into the discussion of hazard and risk assessment?

It is unclear whether some of the references cited in the paper are appropriate or even relevant. The authors should thoroughly review all references and ensure that only essential and directly related sources are included, rather than a random collection of papers that might appear relevant but are not tied to the paper's actual content.

In the "Methods and Materials" section, please number all equations and explain the meaning of their components. Justify the use of the published equations—why are these models applicable to the problem at hand, a problem that, by the way, is never clearly defined in the lengthy introduction? Additionally, it seems the author may not fully understand GIS—ArcGIS is the name of the software suite, and ArcMap is one component of that suite. How were the model equations applied to the specific area and issue under study?

Figure 2 is meaningless. There is no discussion on how this assessment applies to the study region or why wind directional data can be applied as it is. The entire section is incoherent and confusing. Additionally, some numbers in brackets are unexplained, and I could not determine what they refer to.

The pyroclastic flow assessment is rudimentary. While the author digitized and georeferenced a map, where is the morphological data? Where are the flow models for different Volcanic Explosivity Index (VEI) scenarios? Where are the lahar assessments? Numerous studies show that ground flows are among the most destructive components of pyroclastic eruptions—yet here, they are treated as a side note, while ashfall is presented as the primary danger. This imbalance needs to be addressed and explained.

The discussion of the economic models in this section is marginally better—you do at least explain the parameters used—but again, number your equations. In the earlier section, you used GIS to create spatial separations, but here, you calculate the average impact across the entire area, both in terms of destruction and reconstruction. At no point did you georeferenced the economic impacts or address the issue at a spatial resolution. You mention land use cover, but in your model, you simply used the sum or integral over the area, which oversimplifies the issue.

RESPONSE TO THE EXPERT REVIEWER 2, DR. ROBERT BUCHWALDT

The first observation from the reviewer is that our manuscript is difficult to understand, and as a result, it may not be suitable for publication in any academic journal. He is right in the sense that we lacked to make it particularly clear what the objective of our study has been, and we included references to other areas of volcanoes' science that were not directly related to our predominant objective. We rewrote in a specific way the real scope of our study through the entire reformulated introduction in order to make clear the actual objective of this study. Therefore, we changed our original title and concentrated on the particular economic issues related to the two studied Ecuadorian volcanoes and their corresponding hazards.

The reviewer mentioned that at no point we introduced the Otavalo region. This observation is not really clear to us and kind of unjustified, because section 2.1 is dedicated exclusively to introduce the study area.

The third observation is related to difficulties to follow because of language issues. This particular observation was taken seriously, and the paper's new version has been reviewed by a further native English speaker. In addition, we used Gemini AI from Chrome to support us with an academic edition of the paper text. Correctly, the expert reviewer also asked why Mount Vesuvius was mentioned in our study, as it may have had an indirect associated aspect to be referred to, so we eliminated its presence and the corresponding reference.

The fourth observation has been about a certain lack of explanation of the apllied methodology. This observation remained not clear to us, because the methodology is explained in detail in sections 2.2 through 2.4. We may make it clear again, that the present study was not about hazard and risk management assessment. We may particularly point out, that this study is about the direct economic impact of volcanic eruptions and this has been realized based on a limited amount of literature that concentrates on this important area of knowledge, although it is a key and strategic issue to policy and decision-making for corresponding authorities.

The fifth observation is related to some of the references cited. This observation is welcome and has led to a new review of all the used and cited references, focusing to use just those directly related to the main objectives of our study.

The sixth observation was considered, and we numbered all equations and explained all their components.

The reviewer also mentions that Figure 2 is meaningless and how wind directional data can be applied to it as it was applied. In section 2.3, line 150, we explained how this method is applied and how it is related to the potential ash fall of the Cuicocha and Imbabura volcanoes. Figures 5 and 6 illustrate the potential ash fall direction of these volcanoes.

Eighth observation is related to Figure 3, and the reviewer evaluated that it to be rudimentary, asking where the morphological data is, as well as different scenarios for the used VEI of the two studied volcanoes. In our case, we do not need different scenarios because, as mentioned before, our study is based on the worst-case scenario. The figure may be seen for some as rudimentary, but in a more detailed view it absolutely meets our main study objective, which is the economic assessment of potential volcanic eruptions. This study was not prepared to demonstrate the

potential pyroclastic flow of the Cuicocha and Imbabura volcanoes, which may require different models to cover all potential directions. This study was exclusively designed to help to develop policies to prevent enormous potential losses in an area where no prevention or any other kind of risk management measures have taken place so far.

The final observation is related to the economic assessment and why it lacked to use GIS to georeference the economic impact. The reviewer is correct in the sense that locating the economic impact may help to explain it in particular spots. However, in the current study, with all the different potential impacts, some of them we considered the macroeconomic impact in terms of policymaking. In that sense, georeferencing the impacts did not really have impact on the results or outcome of the main objective of our study.

Once again and with all due respect, we are very thankful for the comments and corrections of Dr. Robert Buchwaldt, which helped to see a few unclear parts and or even faults of our side within our manuscript. With his comments we were able to smooth the text, clarify missing parts or wrong spellings, which resulted to an improved and much better than the initial version of this current study.

Thanks a lot on behalf of all authors