The manuscript addresses an important and regionally relevant topic, and the proposed WHI could be useful for wildfire hazard monitoring in the Andean and Amazonian regions of Peru. However, the Introduction, terminology, and methodology require clarification.

The Introduction provides useful background on wildfire activity, drought, vegetation conditions, and agricultural fire use, but the progression from the broad regional context to the specific research gap is not yet sufficiently sharp. The authors should state more clearly what is missing in the existing literature, why current wildfire hazard indices are insufficient or not directly transferable to the Peruvian Andean-Amazonian context, and what is novel about the proposed WHI compared with previous approaches.

The manuscript requires clearer and more consistent terminology. The authors frequently use the term “risk” throughout the manuscript, including in the abstract, but the analysis does not explicitly account for the additional components of risk beyond hazard, namely exposure and vulnerability. Therefore, the use of “risk” appears inappropriate unless these components are properly incorporated or discussed. Moreover, the term “hazard” may also be problematic in this context. In wildfire science, “fire hazard” is often associated with relatively static assessments of ignition and spread potential, fuel conditions, and potential fire behaviour. By contrast, the dynamic component of fire-prone conditions, particularly when driven by short- to medium-term environmental variability, is more commonly referred to as “fire danger.” The analysis presented by the authors seems to be more closely aligned with this latter concept (see for example the discussion at: https://jgpausas.blogs.uv.es/2017/08/05/fire-danger-fire-hazard-fire-risk/) These distinctions are important because the proposed WHI combines static and dynamic environmental variables with an anthropogenic fire-use proxy, and is validated against wildfire emergencies and MODIS hotspots, which represent observed fire activity rather than hazard or risk itself. As a result, the terminology used in the manuscript should be revised carefully. This issue appears throughout the manuscript, which is why I have not highlighted every occurrence individually. The authors should systematically review the use of terms such as “risk,” “hazard,” “danger”, ensuring that each term is used consistently and in line with the actual scope of the analysis.

In the methodological section, several aspects should be clarified in greater detail. This applies especially to some choices related to the indices, such as their rescaling to the 0-3 range and the thresholds used to assign values within this range. Although these choices may be plausible, they should be better justified and, where relevant, explicitly related to the Peruvian context. This would make the methodology more robust, transparent, and potentially replicable. Similarly, the final choice of weights assigned to the different components of the index should be more clearly justified, as noted in a more specific comment. Addressing these issues would substantially improve the conceptual clarity, reproducibility, novelty statement, and scientific robustness of the proposed index.

I added some specific comments in the attachment.

INTRODUCTION

Row 31: The Introduction is too broad at the beginning. It starts with global wildfire-regime changes, tropical rainforest loss, Brazil deforestation, and South American wildfire crises. These points are relevant, but the transition to the specific Peruvian Andean-Amazonian problem could be faster and more focused.

Row 37: The sentence defining “wildfire” interrupts the logical flow of the Introduction. If the authors wish to retain this definition, it would be better placed at the very beginning of the Introduction; otherwise, it could be shortened or removed.

Row 41: Which “wildfire indices” are intended, hazard indices? Risk indices? Specify.

Row 49-50: This sentence is broadly relevant, but its placement is not fully convincing. After discussing agricultural and livestock fire use as a primary driver, connection with the previous sentence should be clarified.

Row 61: The phrase “burns from escalating into wildfires” is potentially misleading, as it suggests that wildfire is simply a later stage of a burn. Moreover, the issue of agricultural fire use is better introduced in the previous paragraph. I would suggest linking the discussion more clearly.

Row 63-69: This paragraph is partly necessary, but in its current form it feels redundant and unfocused. Fire Weather Index is mentioned, but the paper does not use FWI.
Dry conditions are mentioned as important for wildfire hazard, but then many other factors not related to dry conditions are mentioned. Topographic factors such as elevation are mentioned, but topography is not clearly included in the WHI. Fuel treatment effectiveness seems less relevant for the Peruvian regional hazard-index model. Temporal and spatial patterns of fire occurrence are more related to validation or historical fire analysis, not directly to the input variables.

Row 71-76: Important paragraph, but the research gap and novelty should be stated more clearly. “Their applicability” is ambiguous.

STUDY AREA

Row 107-108: It is not clear whether this information is actually relevant to the argument being developed. In its current form, it appears somewhat disconnected from the surrounding discussion and should either be better integrated into the text or removed.

Row 111: Instead of “prevention methodologies” it would be better to say “wildfire hazard monitoring methodologies”, you can use for example “hazard assessment tools.”

Row 112-113: Beyond punctuation, the sentence also feels conceptually incomplete. I would suggest to rephrase or better introduce the concept.

DATA AND METHODOLOGY

Row 160: Why were this index, the other component indices, and consequently the overall WHI rescaled to the 0-3 range? The authors should justify this choice, explaining whether this range has

a methodological basis, follows previous studies, or was selected for interpretability/classification purposes.

Row 164: The statement that “t refers to the annual MODIS availability” is rather vague. Only later in the manuscript is it clarified that the model has an 8-day temporal resolution; therefore, I assume that *t* refers to this 8-day time step, or to a quasi-weekly temporal unit. This should be clarified from the beginning of the model description, so that the temporal structure of the analysis is immediately understandable.

Row 169: Does the NDVI rescaling account for vegetation type or land-cover class? Using a single maximum value over the entire study area may be misleading, especially given the land-cover heterogeneity described earlier. Different vegetation types are unlikely to share the same NDVI variability range. The authors should clarify whether the rescaling is performed globally or separately by vegetation/land-cover class, and justify this choice accordingly.

Row 182: The authors should justify the choice of normalizing agricultural activity using the 33.3% and 66.6% terciles. It is currently unclear why this classification scheme was selected and whether it has a methodological or empirical basis. They should explain whether these thresholds are supported by previous studies, by the distribution of the data, or by a specific rationale related to agricultural use patterns.

Row 183: It is unclear in what sense the analysis is restricted to agricultural areas. Does this restriction apply only to this specific index, or does it affect other parts of the analysis as well? In addition, the meaning and role of the 12 maps mentioned afterwards are not sufficiently clear. Overall, the use of this variable is difficult to follow and should be clarified, including how it is calculated, where it is applied, and how it contributes to the final model.

Row 210: The weighting procedure requires more transparency. The authors state that an AHP-based pairwise comparison and sensitivity analysis were used, but the manuscript should provide the pairwise comparison matrices and the criteria used to select the final optimized weights for each region. Without these details, the weighting process is difficult to reproduce and appears partly subjective. Then, it is not clear in the Results section which set of weights was used to produce the results shown. Are these the weights reported in Table 3? If so, I would suggest presenting Table 3 earlier in the manuscript, before the results are discussed, so that the reader can more easily understand the basis of the analysis.

Table 1: Although I fully understand the need to provide a descriptive interpretation of the final classes, the rationale behind each description is not sufficiently clear. Since the final value results from the combination of several components, it is unclear why the class descriptions emphasize only some of them. In addition, the descriptions seem to focus mainly on relatively static features, such as herbaceous ecosystems, whereas the proposed index is presented as dynamic. This aspect should be better explained and justified, clarifying how the different components contribute to each class and why specific characteristics are highlighted in the interpretation. Moreover, the basis on which the hazard classes were defined should be clearly explained.

RESULTS

Row 223: The opening paragraph of the Results section is too introductory and does not immediately present the study’s results.

Figure 8: I would suggest moving Figure 8 earlier in the manuscript, so that the reader can immediately understand what type of outputs are being discussed. Alternatively, the authors should at least provide an example of these maps before discussing the results in detail.

Figure 3: The procedure used to construct the ROC curves is not sufficiently clear. In particular, the manuscript should explain how the hazard classes were related to the observed fire occurrences in order to derive the ROC curves. I would recommend that the authors describe this step in more detail.

DISCUSSION

Row 413: This statement does not seem to be adequately supported by the results, since no spatial analysis of the distribution of hazard classes across the study area is provided. Such an analysis could, however, be interesting and would help support this interpretation more robustly.