General Comments:

First, I would like to commend the authors for their detailed response to all the reviewers' comments. I recognize that the clarity and flow of the manuscript have improved somewhat in this revised version. However, as outlined in my observations, several key concerns remain unaddressed. Both the writing and the figures still require substantial improvement. For these reasons, I maintain my initial recommendation against publication of the manuscript in its current form in the NHESS journal. I apologize for any inconvenience that my comments may have caused, and I wish the authors the best of luck with their next submission.

Regarding the term "forest damage": Once again, I advise against using the term "forest damage", as the authors cannot demonstrate—based solely on NDVI data—that the observed low greenness events result in measurable negative biological impacts at the population or community level in broadleaf or coniferous forests (see my previous comments in the first round review). Therefore, I strongly recommend that the authors consistently use terms like "forest browning" or "low greenness events", as they already do in the paper's title.

Regarding the land use concerns: I appreciate the authors' response to my concerns regarding potential biases arising from land use change. While I recognize their efforts to address this issue, I remain concerned about the lack of clarity in describing the adopted procedures and the limitations of the underlying data. From their explanation, I understand that only forest pixels (at a 0.01° resolution) were used to calculate the average NDVI values, which were then aggregated to produce the 0.1° GP values, with non-forest pixels excluded from the calculation. I would appreciate confirmation of whether this interpretation is correct. If so, this approach would indeed help mitigate biases introduced by annual crop variation. However, it does not fully account for silvicultural forestry areas, such as those subjected to pruning, which can lead to abrupt changes in NDVI while remaining consistently classified as forest over time. Portugal, for instance, contains extensive regions of such managed forests. In this context, my earlier suggestion to include control areas—such as unmanaged protected areas—could help reduce these uncertainties. While I acknowledge the author's rationale, I believe it is important that the manuscript includes a more detailed description of these methodological steps, especially regarding the upscaling process from 0.01° to 0.1°, and the justification for including GPs where forest pixels represent as little as 10% of the area—a threshold that seems quite low, in my view.

Regarding the use of hydro-meteorological predictor variables: I respectfully disagree with the author's justification for including predictor variables that are likely to be autocorrelated (note

that this is also a concern raised by Reviewer 1). For instance, the authors state that they selected the dew point temperature because it provides "information about water content in the air". However, there are other variables—such as specific humidity or absolute humidity—that also quantify atmospheric moisture and are not directly correlated with air temperature. Combining temperature with a measure of air humidity can reduce the risk of multicollinearity while still providing an effective proxy for vapor pressure deficit (VPD). Similarly, the inclusion of three different soil moisture metrics raises questions. Is there a specific ecological rationale for this choice, particularly given that NDVI measurements in forested areas are likely to reflect the condition of mature trees with deep root systems? In this context, what added value do the authors expect to gain from surface soil moisture (0–7 cm) that is not already captured by deeper measurements (28–100 cm)?

Interestingly, the authors themselves acknowledge the challenges associated with soil moisture data (Section 5.2), yet they opted to include not just one but three soil moisture variables in their models. Moreover, the study they cite in support of the reliability of soil moisture data—Zheng et al. (2024)—explicitly discusses the limitations of these time-series datasets, particularly their tendency to underestimate values in dry regions, such as the Mediterranean, during summer. The reduction of the number of soil moisture variables would significantly reduce the number of predictors—from 70 to approximately 50—potentially improving model interpretability without sacrificing performance. In a future submission, the authors should consider adding more detail to Figure 1 or including a new table that provides information on each variable, such as the measurement method, units (which are currently missing), the ecological rationale for its inclusion, and supporting bibliographic references.

As I highlighted in a previous review round, Random Forest models are not entirely free from biases introduced by highly correlated variables. Similarly, Lasso regression also has limitations and does not solve all problems involving multicollinearity. See the example of grid point (A) in France (Section 4.2.2), where two different soil moisture layers measured in the same period were identified as the most important predictors for explaining low greenness events. Assuming a strong correlation between these two variables, how will the authors be able to detect or discard the influence of multicollinearity on the final model result, or identify which variable is influencing the low greenness event?

In conclusion, I have a strong suspicion that this probable multicollinearity present in the predictor variables may have biased the Random Forest results. To help avoid this issue in future work, I strongly recommend that the authors conduct exploratory analyses (e.g., VIF- Variance Inflation Factor) to identify and address autocorrelated variables prior to model development. Alternatively, the use of Empirical Orthogonal Functions (EOFs) could be used to remove

correlations in the spatial and spatiotemporal data. Another option would be to use the elastic net approach. That said, I recognize that implementing such changes is no longer feasible for the current submission.

Regarding the section 4.2.1: I still have some reservations regarding Section 4.2.1. The authors should clearly explain the criteria that guided their selection of these two specific grid points. Moreover, the wording in this section is unclear, and the absence of a theoretical foundation undermines the credibility of the results. Notably, this section lacks any bibliographic references—a shortcoming that also applies to Section 4.2.2. The authors are encouraged to engage more thoroughly with the physiological and ecological literature, which would help readers better understand how variation in the predictors relates to photosynthetic activity. While I acknowledge that some of this supporting literature is addressed in the discussion, I would like to reiterate a point raised in my previous review: the current structure of the article hampers the overall reading flow (see my next comment).

Lines 250-255: This paragraph would be more appropriately placed in the study area section of the methodology, as it primarily serves as a methodological justification. Its current placement disrupts the flow of the results section. I would also like to note that these concerns about section structure were already raised in the first round of review.

Regarding the quality of the figures: Figure 5 remains difficult to interpret and requires improvement. I recommend increasing the size of each plotting panel to prevent the legend boxes from overlapping with the lines representing different latitudes. Additionally, the y-axis scaling appears inconsistent—some lines abruptly disappear from the plotting area, making it hard to follow their trends. The plotting area in map-based figures (e.g., Figure 2) could also be expanded to utilize the full page width available on an A4 sheet.

Regarding the figure captions: In my previous review, I highlighted the need for more detailed figure captions. Although some improvements have been made, several issues persist. For instance, in Figure 4, the meaning of the variable “Max Td2m” is unclear. Using the search tool, I found no explanation or definition of it in the text. Based on the first sentence of the discussion section, I infer that it refers to dew-point temperature. However, this acronym is inconsistent with the nomenclature used in Figure 1, where the variable is labeled as “Dewpoint temp.” The authors should ensure consistency in variable naming and include clearer, more informative figure captions to aid reader comprehension. The same comment applies to Figure 3, where the

reader must deduce the meaning of each variable acronym presented on the x- and y-axes. Another example: the authors could provide a brief explanation of the AUC and CSI values in Figure 2 to aid the reader's understanding.

Regarding the discussion and conclusion sections: I agree with Reviewer 1's comment that the "results are not informative, and the discussion section is lacking". Throughout the manuscript, the authors were limited in establishing comparisons with the work of Hermann et al. 2023, and the discussion lacks a strong theoretical foundation and engagement with relevant literature, particularly in the areas of forest physiology and ecology. Another notable concern is that the authors devote considerable space to discussing the limitations of their input data and modeling approach, while giving relatively little attention to the potential strengths and innovations of their methodology. Finally, the manuscript would benefit from a more detailed discussion on the practical implications of the results for forest management in Europe. Specifically, the authors should elaborate on how their findings could inform actionable strategies to mitigate the impacts of low-greenness events in forested ecosystems.

Specific Comments:

Lines 88–93: I appreciate the authors for including this excerpt in response to my suggestion. I believe these added sentences will greatly enhance the accessibility of the methods and results for readers who may not be deeply familiar with the subject.

Lines 291-292: The authors open the discussion section by suggesting that dew point temperature was important in the models for identifying dry and hot conditions as adverse for European broad-leaved forests. However, this variable's importance is not highlighted in Sections 4.2.1 or 4.2.2, nor is it clearly reflected in the main figures. The same comment applies to coniferous forests (line 302).

Lines 298-301: Just a note here. I respect authors' writing styles in my reviews. That said, this paragraph consists of only two sentences and directly complements the preceding one. Would it be possible to merge them for improved flow and coherence? The same comment applies to lines 367-368, a single-sentence paragraph.

Lines 302–309: The explanation of the results related to coniferous forests appears overly speculative (e.g., bark beetle infestations), which echoes my general comment from the previous round of review.

Section 5.2: I commend the authors for their transparency in Section 5.2. However, several parts of this section read more like methodological descriptions and would be more appropriately placed in the methods section rather than in the discussion.

Before resubmitting this paper, I recommend that the authors carry out a careful review of the manuscript's grammar and punctuation (particularly regarding the use of the comma and articles).

Technical Corrections:

Line 20: Replace “occurences” by **“occurrences”**

Line 32: "for example" must be enclosed in commas or be deleted (my suggestion).

Line 57: “*on-parametric algorithm, based*” remove the comma.

Line 58: Include "to" before "pinpoint".

Lines 67-69: The authors must include at least one reference to support this statement presented at the end of this phrase.

Line 77: I believe the correct name for EUMETSAT is "European Organisation for the Exploitation of Meteorological Satellites", not "European operational satellite agency", as stated by the authors.

Line 78: Add a comma between “MetOp2” and “and MetOp3”.

Line 80: Add a comma between “instrument” and “as described”.

Line 95-96: Replace “*extreme*” by **“extremely**”. Perhaps the end of the sentence could be changed to "…Hermann et al. (2023), which binarized NDVI to focus specifically on explaining extremely low NDVI events".

Lines 110 and 111: Add a comma between “2012” and “and 2018” (in both lines).

Line 113: Delete the dot after "step 3".

Line 115: Exclude “in” before 1988.

Line 119: Add the article "a" before "longer time series".

Dew point: Throughout the manuscript, the term "dew point" is spelled in three different ways: dewpoint, dew-point, and dew point. Authors should review the entire document (including Figure 1) to use only "dew point." I also noticed that in most of the text, the authors use "dew point temperature", and in others just "dew point" (e.g., line 302).

Line 121: Consider adding the word "hydro-meteorological" between "adverse" and "conditions" (i.e., *To capture potential adverse* ***hydro-meteorological*** *conditions for forests*).

Line 124: This phrase is incomplete (*We extract these seven variables*…) and repeats much of the text provided in the previous sentence.

Line 128: Replace the preposition ‘’to” by “with” (…*was associated to the same*).

Line 142: Consider changing from *"...large amount of data available spatially*" to "...large amount of available spatial data”.

Line 145: Remove the article “a”**:** (…*with a sufficient data*)

Line 190: Add a comma in “1,059” and consider changing from “GPs over Europe” to “GPs across Europe”.

Line 200: Consider changing from "... (forest damage), and xiC are all the other predictors in the training data and n is the length of the training data." to "...(forest damage), xiC is all the other predictors in the training data, and n is the length of the training data".

Line 225: Replace the preposition ‘’4” by “four” (*The four most important*…)

Line 230: Add a comma between “before” and “increase”

Line 234: Replace the preposition ‘’3” by “three” (*The three most important*…)

Line 241: Add a comma between “small” and “and” (*are relatively small, and several*…)

Lines 243-249: Review/include the articles before the variables (e.g., *predictor is* ***the*** *maximum 2-m temperature…*).

Line 291: Add a comma between “i.e.” and “high temperature”

Line 295: Consider changing from *“…,which increased consistently with temperature over these last decades in many regions*” to “… “*which* ***has*** *increased consistently with temperature over* ***the*** *last decades in many regions*”.

Lines 296-297: Consider changing from "*and once soil is dry, plants have to close their stomates*" to "*and once **the** soil is dry, plants have to close their **stomata***"

Lines 313-314: Consider changing from "*…at European scale are set on flat areas with important water reserve or available groundwater table*." to "*…at **the** European scale are set on flat areas with important water reserves or **an accessible** groundwater table*."

Line 375: Replace "measurement" by **"measurements**"

Line 388: Add a comma between "that" and "given"