General Comments:

This study explores a relevant topic that requires further attention from the scientific community. I acknowledge the considerable effort made by the authors in organizing and analyzing the data. However, I have significant concerns and questions regarding various aspects of the study, as outlined in my comments below.

The authors do not provide a clear biological definition of "forest damage." From an ecological perspective, considering that this study operates at the interface between population and community scales, forest damage should ideally be assessed by indicators such as a reduction in the reproduction or survival of tree populations or a phytosociological shift within the community (note that changes in species composition over time can also alter the NDVI response of a forest). However, none of these ecological proprieties can be inferred solely from NDVI. NDVI indirectly measures photosynthetic activity, which can be temporarily reduced by many factors unrelated to hydro-meteorological variations, such as changes in nutrient absorption or pathogen infestations affecting aerial or root systems. At the ecosystem scale, NDVI reduction could reflect changes in productivity by capturing decreases in the photosynthetic biomass of forests. If this photosynthetic biomass reduction were detected in NDVI long-term trends and supported by direct field measurements, there could be evidence of "forest damage", but the authors cannot demonstrate this in this work.

The manuscript's structure does not adhere to the conventional format (Introduction, Materials and Methods, Results, and Discussion), compromising the overall coherence and readability. For instance, Section 5.2, which follows the discussion, introduces methodological aspects and essential criteria that should be included in the description of the methods. Therefore, I strongly recommend a comprehensive reorganization of the text to enhance its clarity and logical flow.

The information regarding forest data represents a fundamental layer of this work, but it is scattered in different sections of the text (e.g., lines 103-105, 238-242, and 297-309). The authors should gather all the forest information in a single opening subsection of the materials and methods section, providing more details for the readers. For example:
- If the land cover product generated by CORINE is separated into three classes, why did the authors analyze only two?
- What is the criterion for considering 2018 as the reference year for vegetation? Wouldn't it be better to identify the areas (pixels) where there was no change in land cover over the period available in CORINE products?
- Based on the literature, the authors could briefly describe the seasonal patterns of NDVI in European forests so that readers have a better understanding of the inherent fluctuations of this variable.
- In their maps, the authors should also provide a line (or another feature) clearly showing what they consider southern and northern Europe.

One of the crucial issues of this work is the inclusion criterion of pixels with only 10% forest cover and the lack of control over the heterogeneity of land cover within the pixel. Some cases shown in Figures 1 and D1 include some of the large metropolitan areas of Europe (e.g., Lisbon and Naples) that can hardly be considered as forests. Additionally, many of these pixels also include agricultural or forestry areas, where managing these sites can significantly affect the NDVI result independently of climatic factors. In other words, the assumptions listed in section 5.2 (lines 297-309) do not seem appropriate to me, and the authors cannot guarantee that the variation in the NDVI of the analyzed pixels is only affected by meteorological fluctuations.
This concern could be minimized in different ways, including:
- Utilizing NDVI products with higher spatial resolution (e.g., Landsat).
- Selecting only pure forest pixels to enhance accuracy.

- Throughout the analyzed period, the authors could use control areas to mitigate potential biases arising from land use and land cover changes or stochastic events (e.g., wildfires, windstorms, pest outbreaks).

I have some concerns regarding the hydro-meteorological predictive variables used in this study. The authors include a large number of variables but do not mention any tests to assess autocorrelation among them. Given the similarity of certain variables (e.g., temperature and dew point temperature or the three soil moisture measurements), there is a high likelihood of significant correlation. While the Random Forest method is robust, it is not entirely immune to biases introduced by highly correlated variables. Therefore, I recommend conducting an exploratory analysis to evaluate and account for potential multicollinearity before selecting the final set of variables.

Concerning rainfall data, did the authors consider using datasets with higher spatial resolution, such as CHIRPS (Rainfall Estimates from Rain Gauge and Satellite Observations)? It is well established that regions with rugged topography or predominant convective rainfall during the summer can exhibit significant spatial variability in precipitation volume. Therefore, caution is necessary when extrapolating these data to neighboring areas. This may have influenced the results in some way, as rainfall volume was not identified as a primary variable influencing NDVI variations. This finding is unexpected, given that water plays a fundamental role in stomatal opening and photosynthesis, which are essential for regulating metabolism and maintaining plant homeostasis. Wind is also a variable that may have influenced the results (e.g., removal of leaves in extreme events or by microclimatic alteration of the leaf boundary layer). However, I understand the reasons that led the authors not to consider the use of this variable.

The authors opted to present the NDVI time series as a secondary outcome of the study, including only an appendix figure (Figure B1) and omitting an in-depth analysis in the results and discussion sections. This approach creates a disconnection between the introduction, where climate change is a central theme, and the remainder of the paper. Therefore, I recommend incorporating Figure B1 as a primary figure within the main text and that the authors highlight the annual variations and long-term trends of NDVI in the manuscript.

Specific Comments:

Lines 22-23: The phrase "*In particular, dry and hot conditions can be highly detrimental to forests (Brodribb et al., 2020)*" should be revised for greater clarity and accuracy. It is important to note that hot and dry weather (and climate) constitutes the natural conditions of numerous forest formations worldwide, such as the tropical seasonal dry forests of northeastern Brazil, the eucalyptus forests of Australia, and various forests and woodlands of the Iberian Peninsula, for example. While Brodribb et al. (2020) highlight the potential dangers posed by rising temperatures and reduced water availability to forest ecosystems, this crucial context is not conveyed effectively in the original sentence.

Lines 28-30: In both phases, while these statements may be appropriate within the original context, they appear somewhat generic in this instance. It is important to note that some forest ecoregions, such as the northwestern Amazon and the La Plata Basin, are experiencing increased rainfall due to climate change. In the same way, the conclusion of Meier et al. (2021) seems restricted to the case of broad-leaved trees in Switzerland. Therefore, I recommend that the authors specify the region they are referring to. The same observation applies to other parts of the introduction section.

Line 44: As this is the first occurrence of the acronym ECMWF in the text, the authors should provide its full designation. Other similar cases are scattered throughout the different sections of the manuscript, such as EUMETSAT and MODIS.

Lines 44-56: At the end of the introduction, the authors describe the advantages of using ECMWF forecast models. However, this approach disrupts the logical flow of the text and does not effectively communicate the study's primary objectives. To enhance clarity and coherence, I recommend that the authors revise the final paragraph of the introduction to outline their research objectives and hypotheses explicitly. This revision should establish a clear connection between climate change and vegetation responses, ensuring a more structured and engaging introduction.

Lines 85-87: The authors need to provide more details on the reason for adopting this approach (Maximum NDVI) and its potential consequences for the dataset.

Section 2.2: The authors should provide a more detailed justification for their decision to binarize the NDVI data rather than treating it as a continuous variable. Additionally, they should clarify the criteria used to determine the 80% threshold.

Lines 110-115: The wording of this paragraph seems more appropriate for a discussion section than a methodological description.

Section 2.3 (lines 122-124): How were these layers with varying spatial resolutions harmonized for subsequent analyses?

Section 2.4: While I understand the rationale behind this methodological step, I question whether 41 data points can support a comprehensive assessment of an area as vast as Europe.

Section 4.2.1: The criteria for choosing the two grid points and the objective of this type of approach/analysis are unclear. The description of all analyses and their objectives and predictions must be detailed in the methods section.

Figures 2 and 4: Both figures are important results but challenging to interpret, particularly Figure 4. The authors should provide a more detailed explanation in the text to clarify the meaning of these results and enhance readability.

Figure B1: Why are the analyzed pixels omitted (i.e., not colored green or orange) on some maps, as in much of Scandinavia in 1986?

Figure legends and resolution: The authors should provide more detailed figure captions, avoiding including acronyms without a complete meaning description. Additionally, some figures have low resolution or could be better formatted and expanded to enhance clarity and facilitate reader interpretation

<u>Technical Corrections</u>:

Line 15: "*202 <u>millions</u> hectares*" Should be singular "202 million hectares".

Line 15: "*32 %*" Remove the space between the number and the percent sign.

Line 17: "*for a high biodiversity*" remove the article "for high biodiversity".

Line 18: Add a comma between "*spiritual*" and "*and*".

Line 20: "have significant consequences on the economy and society" The preposition "on" should be replaced with "for".

Line 21: The repetition of "*and*" is redundant and disrupts the sentence flow.

Lines 25-26: I suggest changing "*photosynthesis capacity*" to "photosynthetic capacity".

Lines 26-27: Avoid repeating terms in the same phrase. Here, "*dry year*" could be replaced by "one single event" (or another term) without changing the meaning of the sentence. The comma separating "dry year" and "due to" could also be removed.

Line 32: I think "*advance*" can be replaced by "advanced".

Line 39: "*forest-fire*" I think no hyphen needed "forest fire".

Line 61: "*vegetation greenness, widely used*" remove the comma "vegetation greenness widely used" or "vegetation greenness that is widely used".

Line 136: "*We therefore artificially*" change to "*Therefore, we artificially…*". The same observation is valid for other parts of the text, such as in **lines 187** and **292.**

Line 187: "*wheat yield, in their study*" remove the comma.