

## Reply document

### Meteorological history of low forest greenness events in Europe in 2002-2022

*Reply to minor revisions of both reviewers in February 2023 by Mauro Hermann, Matthias Röthlisberger, Arthur Gessler, Andreas Rigling, Cornelius Senf, Thomas Wohlgemuth, and Heini Wernli*

We acknowledge the reviewers again for the second round of constructive feedback regarding our manuscript. We are thankful that our revisions based on their comments could improve the manuscript to the point where only very minor revisions were necessary. We addressed most of them in the presented final version of the manuscript. Below you can find the final comments of both reviewers, which mainly addressed the formulation of the null hypothesis tested in our study, including our replies. [Again, the comments of the reviewers are shown in black and our replies in blue. We number reviewer comments for referencing purposes throughout the document \(comment 1 = C1, etc.\).](#)

Prior to that, we list a few minor changes made to the manuscript during the final read that go beyond typos:

- L. 103: We use CLC land cover from the year 2012 (in the center of the study period), not 2018 as stated in the previous version.
- Sect. 2.3 & Appendix A: We use an improved formulation for the count of negative 16-daily *NDVI*' values in year  $n$  (new  $c_{n,ev}$  instead of  $n_{t,ev}$ ), as well as its minimum threshold value (new  $c_{ev}^{min}$  instead of  $n_{t,ev}^{min}$ ). This is to avoid confusion with further abbreviations used (e.g.,  $n_{ev}$ ) and is more accurate regarding the use of the indices  $n$  and  $t$ . The new terms are introduced in L. 137 and L. 142.
- L. 219: Removal of unit IQR, as the normalized NDVI anomalies are in fact unitless.

Finally, note that the low NDVI dataset will be uploaded under the doi <https://doi.org/20.500.11850/505559> once the published study can be cited in the data manual. The doi will be reserved up to that point.

## Reviewer 1

Altogether, the authors have done a good job in revising the manuscript and I highly appreciate the efforts they've taken to refine the paper. In my opinion, all the possible technical flaws from previous submissions have been overcome with the extensive revisions provided, wherefore I deem the manuscript publishable pending minor corrections. Of somewhat greater concern is however the formulation of the null-hypothesis of the bootstrapping test, which I don't agree with (see specifications below). Once these minor adjustments have been taken into consideration, the manuscript in my opinion deserves publication.

**Authors:** Many thanks for this very supportive feedback including one final suggestion for correction.

**1.** Lines 69-81: In this paragraph I was missing the context provided in Kornhuber et al., 2019: <https://doi.org/10.1088%2F1748-9326%2Fab13bf> which seems to be highly relevant in context of meteorological histories.

**Authors:** We have significant reservations regarding the theoretical underpinning of the arguments put forward in Kornhuber et al. (2019). These reservations include (but are not limited to) arguments made in Wirth & Polster (2021) and thus prefer not to refer to Kornhuber et al. (2019).

Wirth, V., & Polster, C. (2021). The Problem of Diagnosing Jet Waveguidability in the Presence of Large-Amplitude Eddies. *Journal of the Atmospheric Sciences*, 78(10), 3137–3151. <https://doi.org/10.1175/JAS-D-20-0292.1>

**2.** Line 73: I guess you mean insolation not insulation

**Authors:** Yes of course, we changed insulation to insolation.

**3.** Section 2.2: Here I was wondering, whether the authors only considered forest-pixels (as per Corine land-cover) for further analyses. Reading further I got that it is mentioned in 2.3 but I think this is relevant information for this section, so that readers immediately become aware that only forest pixels were considered and thus noise from other land-cover types can be more or less ruled out (this you can even mention specifically to make your approach more robust). Also, I recommend providing a URL for the AppEEARS website.

**Authors:** This is a great suggestion. We included the following sentence at the beginning of Sect. 2.2 “As mentioned in Sect. 2.1, we only use NDVI at forest pixels according to CLC in order to minimize noise from other land cover types. The NDVI is based on...” (L. 113).

We include a link to the AppEEARS website in L. 119.

Furthermore, we again highlight the masking of non-forest land cover types in the second paragraph of this section in L. 120: “In addition to masking non-forest land cover, we mask NDVI values that are of poor quality...”

**4.** Line 171: Why are leap-days discarded? I don't really see a reason but I'm sure you had a good reason for doing so. Please elaborate.

**Authors:** We included the two reasons for our decision in the same sentence, now in L. 172 : “Leap days are discarded from the analysis to maintain consistency in each calendar day's climatology and the length of the meteorological histories.”

For example, the use of only 5 days for calculating the climatologies on 29 February would result in standardized meteorological anomalies that are not readily comparable to those at other calendar days.

**5.** Line 192: I really like the bootstrapping approach but I disagree with the formulation of the null-hypothesis. In my opinion, your bootstrapping does not allow for assessing whether a meteorological history is related to the low NDVI event, since there is no direct link (like a correlation score or similar) incorporated in the test. To me, the H0 should be something like:

The meteorological history at  $t_{ev} - dt$  is not different from a randomly (arbitrarily) sampled meteorological history.

The way your H0 is formulated suggests a direct link (which might be the case, but this you do not test with the bootstrapping) whereas my suggestion emphasizes on a potential difference of meteorological histories between events and ‘normal’ periods. Please elaborate this throughout the manuscript and the appendices.

**Authors:** We want to highlight that no matter the formulation of H0, failure to reject an H0 in a statistical test does not imply that the H0 is true. Nevertheless, the reviewer raises an important point, which we would like to accommodate in our manuscript. We, therefore, reformulated H0 in L. 194 as follows:

“The meteorological history at  $t_{ev} - dt$  is equal to a randomly sampled meteorological history”

This change was also incorporated in Appendix B.

**6.** Fig. 2: it is quite difficult to digest panel a. I think it is okay like this, but for deeply interested readers it might be a good idea to have a multi-panel figure in the Appendix depicting the events for each year in a single map.

**Authors:** We agree on this point which is why we had Appendix D included (exactly the plot suggested by the reviewer). We now refer to this Appendix earlier in Sect. 3.1 (L. 208) and additionally in the caption of Fig. 2, which is why it now is Appendix C, i.e., the earlier reference has changed the order of Appendices.

**7.** Line 264-266: This sentence is really hard to read. I guess I get its meaning (which by the way supports my claim to reformulate H0, see above) but I believe it is possible to reformulate to make it easier readable.

**Authors:** We rephrased the sentence in L. 266 to make it more comprehensive to “Again recall that these precursors are features of the low NDVI events' meteorological histories that were significantly more frequent than during any random meteorological history in the climatology.”

**8.** Line 333-334: see my comment on H0 and please revise accordingly. It is rather a difference to climatology and not a relation with low NDVI events that you're testing.

**Authors:** We rephrased H0 in L. 334 accordingly: “...under the null hypothesis  $H_{0,EV}$  that the fraction of dry/warm periods preceding the low NDVI events (i.e., during  $dt$ ) was not different from a randomly sampled meteorological history (grey shading...)”

**9.** Line 348-349: The formulation used here again suggests the reformulation of H0: it is a comparison to normal conditions and not a relation with low NDVI events.

**Authors:** Yes, see answer to C5.

**10.** Line 405: To be very clear, please specify which approach you refer to here. I assume the low NDVI-approach and not the meteorological history bootstrapping approach.

**Authors:** We changed “Our approach” to “Our approach to identify low NDVI events”.

**11.** Line 490: I propose to reformulate this sentence: on the one hand the anticyclone favors low NDVI in northern Europe but on the other hand it is unfavorable for low NDVI grid cells in western Europe. It is not clear how the anticyclone should be unfavorable for low NDVI. Please reword.

**Authors:** We assume that the reviewer refers to the sentence in L. 493: “So while in JJA a European-centered anticyclone can favor low NDVI grid cells in northern Europe, it might be unfavorable for low NDVI grid cells in western Europe.”. As we explain in the two previous sentences, there are regions in western Europe that receive a large portion of summer precipitation during the presence of an anticyclone (see L.491-493 and references, or also L. 547). Our results show that the JJA anticyclone frequency is reduced in these regions at the time of low NDVI events. Consequently, we conclude with the above-cited sentence that in these regions, more frequent anticyclones (i.e., more JJA precipitation) are not favorable for the occurrence of a low NDVI event. A possible explanation could, we think, be that weak cyclonic activity with no or little precipitation instead of heavy convective precipitation at the western inflow of the anticyclone sustains drought conditions better, and, hence, is favorable for low NDVI events (in the given spatio-temporal context). As this is an interesting point to make, which is shown in our results and relates to precipitation patterns found in previous studies, we did not change this part of the manuscript.

## Reviewer 2

The authors have made an excellent job in addressing the comments and the manuscript is now clearer, and the methods, analysis and results more robust. The manuscript is still heavy on acronyms, but it is probably unavoidable

The approach proposed to study low NDVI events is still very relevant for the community, as I mentioned in the first revision. The more didactic explanation of atmospheric processes and the introduction of Sections 3.2 and 3.4 are very good addition. The extension of NDVI until August 2022 results in very timely new results (summer 2022 was again hot and dry).

In summary, I strongly recommend the publication of this study.

**Authors:** Many thanks for these very positive comments on our revised manuscript.

I have only a minor comment:

**12.** Line 90 and Line 162 should read "90-day \*moving\* average", right?

**Authors:** Thank you for highlighting this aspect, we included the reviewer’s suggestion in both passages.