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Climate of the Past Discussions

Interactive comment on "Assessing the Statistical Uniqueness of the Younger Dryas: A Robust Multivariate Analysis" *by* Henry Nye and Alan Condron

Anonymous Referee #1

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General Comments

In this study, Nye and Condron consider the Bølling-Allerød (BA) and Younger Dryas (YD) in the broader context of abrupt climate change over the last glacial cycle. They apply an outlier detection algorithm to a number of paleoclimate records in order to test whether the BA/YD is statistically unique from DO events of the last glacial. From the results of this outlier detection method, they suggest that the BA/YD is statistically indistinguishable from other DO events (in Greenland ice core records), raising the question of whether its triggering mechanism is unique.

This paper raises important questions regarding our understanding of the mechanisms

of abrupt climate change and applies a novel technique to compare DO events. However, there are several aspects of the paper where more detail/analysis is required. The main components of the paper that I found insufficiently addressed were 1) the use of outlier detection in distinguishing mechanisms of abrupt change, and 2) a more quantitative discussion/demonstration of the (non)uniqueness of the BA/YD, relative to the other 24 considered DO events.

1) Use of outlier detection in distinguishing mechanisms of abrupt change

In this study, Nye and Condron use outlier identification (or non-identification) to 1) argue that the BA/YD should be included in the list of DO events, and 2) suggest that it may not have a unique triggering mechanism (when compared to other DO events). However, the study did not address how outlier detection may be used for this second argument. It is unclear if/how a statistical difference (or more accurately, a similarity) in the selected proxy records would indicate a different (or common) triggering mechanism for these events. As noted by the authors, AMOC variability is often invoked to explain the global signature of DO events and the BA/YD, alike. Modeling studies that compare the global imprint of freshwater forced versus spontaneous AMOC variations (see Brown and Galbraith, 2016, https://doi.org/10.5194/cp-12-1663-2016) suggest that forced and unforced AMOC variations have very similar signatures. This would suggest that similarities between climate proxy records during DO events (and the BA/YD) may not necessarily imply that they were triggered by the same mechanism. Please address the suitability (or limitations) of applying this outlier technique in differentiating between the triggering mechanisms for abrupt climate change.

2) Quantitative discussion/demonstration of non-uniqueness of the BA/YD

The main conclusions of the study are drawn from the results presented in Table 4, which shows the outlier detection results for a given set of climate proxies and metrics. However, from this table it is not obvious that the results support the conclusion that the BA/YD transition is non-unique, given that the BA/YD was identified as an outlier

in most of the tests. It is unclear whether these results are related to the algorithm's relatively high rate of 'false positives' (as the authors mention), or if the BA/YD is actually a statistically unique interval (as defined by the outlier detection algorithm). A fairly simple test of the relative 'non-uniqueness' of the BA/YD would be to perform the same outlier analysis for each of the other 24 events considered in the record.

In Table 4, please include a summary of the results for the other 24 considered DO intervals compared against the 25 DO events. For example, add three columns (and three rows) to the end of the table and include 1) the rate of outlier detections for the BA/YD (for instance, 12/15 or 0.80 for the first column), 2) the average rate of outlier detections for the other 24 events, and 3) the standard deviation of the outlier detections for the other 24 events for each column or row. Including these metrics for how 'unique' the individual DO events are from one another (and the BA/YD) provides a much more direct comparison of the BA/YD to the rest of the DO events. This eliminates the requirement for the reader to have an in-depth knowledge of the nuances of the applied statistical technique to interpret the results for themselves. Without this, it is difficult to assess the 'non-uniqueness' of the BA/YD, and thus the conclusions of the study.

Overall, I think that major revisions are required to provide a convincing argument of how outlier detection may be used to differentiate between mechanisms of abrupt change, and to quantitatively demonstrate the 'non-uniqueness' of the BA/YD. Other aspects of the manuscript that need to be addressed (such as the choice in paleoclimate proxies, and a quantitative assessment of uncertainties) are included in the specific comments. Technical corrections are included in a supplementary document.

Specific Comments

Lines 41-43: The authors do not discuss their choice in which paleoclimate proxies to include in this analysis. Please explain the choice in which proxy records were (and were not) included, and why they are well suited for this analysis. For instance, how are the chosen records better suited in this analysis than other available ice core records for

this interval (such as Greenland/Antarctic aerosol records)? It is also unclear why proxy records from Greenland ice cores are emphasized in this analysis (see comments on lines 178, 189-191). A more thorough discussion of how the chosen records provide insight into the mechanisms/expressions of abrupt climate change would enrich the manuscript.

Line 79: What is the age scale for the EDML d18O? Related to the above questions, why EDML d18O? EDML is often considered unique from other Antarctic ice core records because of its close proximity to the Atlantic basin, but this is not mentioned in the manuscript.

Lines 80-81: How were these three metrics selected? Why were the slopes and medians within the stadial (but not the interstadial) considered?

Lines 85-87: The goal of objectively selecting time windows to compare stadial and interstadial conditions for peak to trough analysis is a worthy one. However, the interval (as defined with water isotopes) may not be appropriate to apply to other variables. For the BA/YD, the selected interval for the stadial (shown in Figure 3, lower left panel) includes the abrupt decrease in CH4, so the amplitude of the peak to trough change appears to be underestimated. This technique assumes that there is no age uncertainty between the selected climate records. Please consider the influence of age (and delta age) uncertainties in selecting stadial and interstadial intervals for peak to trough analysis.

Lines 88-89: Why use a narrower (not wider) filter for CO2 if the data are sparser?

Lines 90-94: Again, error in the alignment of ice core records may influence the median and slope metrics for the selected stadial intervals. Please consider/discuss how age (and delta age) uncertainties may influence these results. It may also be informative to consider how analytical (measurement) uncertainties may affect these metrics (as well as the peak-to-trough metric), and their comparison between DO events.

Lines 95-136: The explanation of the PCOut algorithm is quite detailed, but also important. Please consider shifting some of the details/equations to an appendix.

Lines 146-148: I would caution in generalizing atmospheric CO2 as a Southern Hemisphere proxy, or at least explain the reasoning (also see comments on line 178).

Line 152: It's not totally clear what chemical makeup means here. Does this mean the choice in which proxies are included in the analysis? Please clarify.

Line 178: Why are Greenland proxy records prioritized? See also comments for Lines 189-191. I would also caution in referring to the NGRIP CH4 record as a Greenland proxy. It's true that the record comes from a Greenland ice core, but it is not a proxy for Greenland climate (and is also available from Antarctic ice core records).

Line 183-184: I could be mistaken, but I thought that assessment of leads/lags between CH4 and Greenland temperature came from Baumgartner, 2014, which used d15N-N2 (not d18O) for temperature (so there is no delta age uncertainty).

Lines 189-191: I'm not sure I understand the logic of this argument. Please explain why the NGRIP CH4 and d18O records are particularly well suited to evaluate the (non)uniqueness of the BA/YD in the context of their climatological significance.

Line 201-202: It is unclear how this degree of similarity (86-93%) is quantified. Please specify how the results (with 25 DO cycles versus 28-30) are compared.

Figure 5: It is unclear the direction in which time is moving in this figure.

Table 2: Please check the signs of the metrics. I would expect that the sign for peak-to-trough changes in d18O and CH4 during DO1 (BA/YD) would be the same.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2020-43/cp-2020-43-RC1-supplement.pdf

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