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

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All text is copied from RC1, and the authors' comments are preceded by "Response:".

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.

Response: We thank the reviewer for their constructive feedback, and provide some additional details as follows: In our study, we have indeed shown that many proxy-based qualities of the BA/YD are statistically indistinguishable from the other DO events. The fact that the climate response to freshwater forced or spontaneous variations in AMOC is similar is one of key and overarching motivations for our work that a single freshwater forcing mechanism should not be the only triggering mechanism for the BA/YD. Indeed, our main goal with this study is to demonstrate that no assumptions should be made regarding the trigger of the BA/YD, precisely because it bears strong resemblance to other DO events. In so doing, our revision will make clear that the outlier detection technique is not aimed to assess the qualities of DO events as they result from specific triggers, but rather to provide a general framework for situating the BA/YD within a broader context of many other DO events, each of which may (or may not) have the same underlying trigger. In this sense, our outlier technique is limited to that simple conclusion, and is meant to negate any strong favorability for the BA/YD as triggered by a unique mechanism.

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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.

Response: Thank you for this comment. While we believe that Table 4 supports our conclusions, we also agree that the way the results are currently presented could be misleading. In our revised manuscript we will make two additional points to clarify the validity of our conclusions from Table 4. Firstly, we will state explicitly that we are specifically looking for subsets of proxies that PCOut identifies as non-outliers, rather than to compare the results of such subsets to one another. This is motivated by sentiments in the paleoclimate community that treat the BA/YD as outlying from other DO events. Secondly, we will make clear that since we are performing tests on all subsets of our data, tallying the total number of subsets registering as outliers is not a statistically sound way of determine outlier behavior on a large scale. Instead, we are asking if the shape of the BA/YD's Greenland proxies exhibits outlier behavior, for which the answer remains to be no.

We believe that the additional analysis you've proposed is a sound way to add context to our argument, but caution against taking means and standard deviations of the total number of outliers in rows and columns of this data. This is because our goal is not to compare the number of proxy subsets for which the BA/YD is an outlier to other DO events. Rather, our results in Table 4 serve as grounds to support pointed claims about each subset (for example, that the shape of the BA/YD's Greenland proxy data is not an outlier compared to all other DO events). In our revised manuscript, we will certainly perform the PCOut analysis on other DO events as a grounds to discuss the extent to which the general picture of outlier behavior in the BA/YD differs from the other DO events in our study.

We also propose to rework the results section and Table 4 such that the results of PCOut regarding the measurements on proxies of interest (namely, measurements of the shape of Greenland proxies) are highlighted as entirely separate from the other results that indicate the BA/YD as an outlier. This will aid the reader in understanding that the BA/YD may well be an exceptional event in the context of southern hemispheric proxies, but is certainly not when viewed through the Greenland lens. The PCOut results for proxy subsets that are of less interest will be added in a supplementary section.

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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.

Response: Our choice of proxies is based on those with the highest spatial resolution and tradition in the field of paleoclimatology of using these to study climate variability

during both DO events and the BA/YD. The high temporal resolution of the ice cores during the last glacial period makes them idea for use in our work. Furthermore, we use both d18O records (NGRIP and EDML) to provide local approximations of climate, whereas CH4 and CO2 are more indicative of global climate fluctuations.

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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.

Response: The age scale of the EDML record spans from 150kya to the present. It was chosen because it has a spatial resolution comparable with the Greenland ice core records. Indeed, the snow accumulation at EDML is two to three times higher than at other deep drilling sites on the East Antarctic plateau, so higher-resolution atmosphere and climate records can be obtained for the last glacial period, making the EDML core especially suitable for studying decadal-to-millennial climate variations in Antarctica. Including EDML d18O allows us to observe changes in NGRIP d18O as distinct in location but similar in meaning. This allows us to make conclusions about how the BA/YD may not have been a unique event in Greenland, but perhaps was so in the southern Atlantic. We will certainly make note of EDML's special status as close to the Atlantic basin in our revised manuscript.

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

Response: The slope and peak-to-trough metrics are meant to give an idea of the shape of each DO cycle, whereas the median gives us a sense of how cold each stadial became. This latter feature allows us to discuss how larger timescale glacial-to-interglacial changes have the potential to render the BA/YD's average temperature exceptional or not, while the former two (slope and peak-to-trough) allow us to isolate the shape of each DO as an independent feature. We focus solely on the slope and median of the stadial periods because of their known volatility, but will perform an analysis of slope during the interstadials as well in our upcoming revision.

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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.

Response: Thank you for this comment. In our revised manuscript, we will be sure to explore questions of age uncertainty.

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

Response: Using a narrower filter for the sparser CO2 data ensures that we are not taking too broad of an average, which, when data is sparse, has the potential to erase important trends in the data. In our revision, we will certainly perform this analysis using a wider filter to quantify this.

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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.

Response: We use robust techniques in this paper primarily because of the uncertainties you mention. For example, we take the median of each stadial interval rather than the mean because the mean is far more sensitive to the uncertainties that present themselves when working with proxy data. Further, we take the mean of the top 10% extrema for both interstadials and stadials rather than simply the highest individual value to protect against uncertainty and observation-based variations. In our revised manuscript, we will perform an age uncertainty analysis to see if slight differences in age alter our results significantly.

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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.

Response: Thank you for your comment. Although equations (1)-(6) regarding PCOut are detailed, we prefer to leave them in the body of the manuscript.

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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).

Response: Noted. In our revised manuscript we will be more specific about the meaning of the CO2 proxy data.

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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.

Response: Chemical makeup is incorrect. We mean "proxy subset", and will amend this in the revised version.

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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).

Response: The Greenland proxy records are prioritized in part for their historical significance in terms of looking at DO events, as well as to address the hypothesis that the climatic signature of the BA/YD and DO events centered predominantly on the North Atlantic due to changes in AMOC. In our revised manuscript, we will be more explicit in discussing the NGRIP CH4 proxy.

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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).

Response: Thank you for pointing this out. This citation will be fixed in our revised manuscript.

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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.

Response: The logic of this argument relies on the fact that the pair of these proxies evaluated together are non-outlying behavior across the board. They are both indicators of temperature, yet d18O is generally indicative of local temperature, while CH4 is more global in nature. Thus, the fact that the pair of behaviors of d18O and CH4 during

the BA/YD is not unique compared to the pair of behavior for these records in other DO events is a stronger conclusion than if we were to restrict this analysis to only one proxy record from Greenland.

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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.

Response: For both the 28 and 30 cycle versions of the data, we create a table in the same format as Table 4, and then count which cells display the same result as our Table 4. By this metric, 93% of the cells in the 28 cycle version are the same as the 25 cycle version, and 86% of the cells in the 30 cycle version are the same as the 25 cycle version. We will include text along these lines to our revised manuscript to clarify this analysis

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

Response: Noted. It moves right to left, where more recent times are toward the left. This will also be clarified in our revised manuscript. —-

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.

Response: That's correct, they are the same. We used absolute value for the d18O peak-to-trough measurement because they are all negative, but for the sake of clarity in the revised version we will include the sign. —-

Technical Corrections:

Response: Thank you for your thoughtful technical corrections. All such corrections will be implemented in our revised manuscript. Citations for lines 17-18 and lines 23-24 are Li and Born (2019), and citation for lines 103-104 will be Filzmoser et al. (2008).

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