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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

**Anonymous Referee #2**

Received and published: 9 May 2020

Review of cp-2020-43

General comments:

The paper by Nye and Condon applies statistical analysis to metrics derived from ice core data ($\delta$18O, CH4, CO2) spanning a large group of abrupt climate shifts between 120-11 thousand years ago. The procedure PCOut, essentially a form of principal component analysis that is particularly suited for outlier identification, is relatively novel for paleoclimatology studies. The results are interpreted to mean the Bølling-Allerød/ Younger Dryas is not statistically different from 24 other preceding Dansgaard-Oeschger events during the last glacial period in terms of the specific metrics they



derived from the ice core data (specifically, stadial slope and magnitude of peak-to-trough change). The authors conclude that future work should not focus on identifying a unique cause for the YD cold event, suggesting that similar mechanism(s) may have controlled all of the interstadial-to-stadial transitions in the past.

The manuscript is well written, and the analyses and results are very clearly presented. Researchers will be able to repeat this work or apply the techniques to other datasets, thanks to the clear presentation of the methods. I have no issues with the analysis itself, save for a few minor comments listed below. I do, however, think the underlying motivation for the study – the notion that the paleoclimate community considers the cause of the YD event unique from other D-O/ stadial transitions - is overstated throughout the manuscript. Yes, there are studies proposing "one-off" causal mechanisms; for example, there is a large body of literature debating the bolide impact hypothesis. But many researchers think the reason the YD is unique is not because of its cause necessarily, but because it occurred during the last deglaciation, a sort of "failed" transition back to glacial conditions. Or, by the same token, D-O events were "failed" deglaciations that for some reason reverted to glacial conditions. The YD is also unique because similar "reversals" toward glacial conditions did not occur during other terminations of the last 800,000 years. The authors' analysis does not consider these aspects, which to me are more fundamental qualities that make the YD unique, more so than the shapes and slopes of the ice core data themselves. If the authors disagree with this assessment, they should make a stronger case in the introduction for why it should be proven the YD is or is not statistically different from other D-O cycles. More specifically, the authors should specify why their outlier test on only three specific metrics – i.e., stadial slope, median, and peak-to-trough magnitude – is a sufficient test of whether the YD is or is not unique. Is there reason to believe these metrics should look statistically different if the YD was in fact caused by something different than other stadial transitions?

Furthermore, I am not fully convinced that the results support the conclusions. The PCOut results in Table 4 show (at face value, at least) that the YD is statistically significantly different from other D-O events. That is, there are 64 instances of yes while only 41 instances of no, and the first column of Table 4 (the all metrics evaluation) points overwhelmingly to outlier status for the BA/YD. The authors' explanation for the median results is that there is significant offset from the rest of the glacial period, but this is precisely why I find the YD is unique and interesting in the first place - not because it looks different from other events in terms of the data, but because it occurred during a deglaciation. That being said, the authors do provide a very clear and thorough discussion of the most interesting aspects of the statistics, which convinced me that the BA/YD is not unique in terms of its expression in NGRIP d18O and CH4. It is useful to point out the statistical likenesses between the BA/YD and other D-O events, as they have done, and I think this paper should be published for this reason, as well as for the reason that the statistical technique is potentially useful for other studies.

I think the manuscript would greatly benefit from revisions such that the paper emphasizes the statistical method rather than the (to me) unsurprising result that the YD is not unique from other stadial events in terms of a few certain patterns resolved in the ice core data. For example, the paper might present the YD/BA is an interesting application of the method, but not an absolute test of YD uniqueness. The description of the method could be bolstered by describing how PCOut might be used for other paleoclimate work, or how it could be adapted to deal with age and measurement uncertainties that are inherent to most paleo datasets. In addition, please see specific comments and technical corrections listed below.

Specific comments:

Line 6-7: Freshwater forcing of circulation due to meltwater from ice sheets or iceberg discharge has been proposed as the cause of stadials during the last glacial period as well, not just for the YD.

Lines 58-59: Can you provide any objective reason for choosing these durations? What makes them conservative?



Line 65: Can you show or say what happens to your final results if you do the extreme scenarios – (1) merge all sub-events into single events, (2) discard all sub-events and only look at the main events, and (3) include all sub-events as their own individual events? This could be in a supplementary section.

Lines 74-77: What exactly do "normalize" and "centered" mean in this context?

Lines 74-77: It is unclear if the "narrowing down" of D-O events applies to the statistical analysis or not. Did you effectively screen the number of events this way? Can you either describe or provide a figure to demonstrate how you narrowed them down, given that this step was done subjectively by eye. Please also state how many events you kept/excluded based on the criteria of visually resembling the BA/YD, assuming you did screen them.

Lines 74-77: Another comment here, and I am assuming that the visual selection of events that you described was used as a screen for the statistical analysis (if not, then disregard the following but please simply reword so it is clear in the text). If so, however, this step would strike me as a major weakness of your analysis. The overall conclusions is that the YD/BA is not statistically distinct from other D-O events in terms of shape, structure, and the other metrics described, but in this selection step you intentionally chose to only look at D-O events that visually resemble the YD/BA in the first place? Please address to what degree the selection criteria influence the final statistical result.

Lines 91-93: Do you have to identify D-O behavior by these criteria for the analysis? I thought you already identified them using the algorithm and the visual resemblance to the BA/YD. If you are just describing the characteristics of D-O behavior, you might change the wording to reflect this so readers are not confused.

Lines 138-140: Again, I don't follow why. Is this just for ease of visualization, or is this related somehow to the statistical analysis? Please state so if that is the case.

Figure 5 – Is the "BA/YD exclusive mean" the mean of all 24 other D-O events, or

just the 7 shown? Please clarify. Additionally, are you sure that the "BA/YD exclusive mean" in the fourth panel (second column, second row) is the mean of EDML d18O? My understanding is this is supposed to be the EDML mean of the 7 events (or 24?), but excluding the BA/YD... It doesn't look like the mean of the other colored lines in that panel, and it looks more sawtooth shaped than the normal phasing of Antarctic temperature with respect to the onset of D-O events. Perhaps there is a mistake.

General technical corrections:

There are numerous places in the text where the term "D-O event" is used somewhat loosely to describe a warming event in the NGRIP ice core record, a cold stadial that follows the warming, or the combination of both warm and cold intervals. Technically the D-O events are just the warming events as they are expressed in the Greenland isotope records.

Specific technical corrections:

Figure 1: The transitions to stadial conditions are marked, but not the transitions to interstadials (i.e. the onset of the D-O events). It is a little confusing when you refer to a specific D-O event. You might consider distinguishing whole interstadial periods versus stadial periods with shading, as in Figure 1 of Rasmussen {Rasmussen, 2014 #751}.

Lines 17-19: Needs a reference.

Line 39: Suggest changing "paleoclimate research" to "future work."

Line 48: Do you mean, "labeled by lowercase letters" in Rasmussen 2014? Please clarify. Line 67: "of a well-defined and complete record for all four of our chosen proxies, as we restrict our analysis of the last glacial cycle to..."

Line 76: "NGRIP d18O," rather than "NGRIP d18"

Line 106: I am being picky here, but two of the four records – NGRIP CH4 and com-



posite CO2 - are not proxies, they are direct measurements.

Line 109: The word "remaining" is confusing to me here? Should it say "resulting," since the components are the result of the principle component analysis?

Equation 1: In reading Filzmoser 2008, I noticed that equation (1) in this manuscript is different from equation (11) in Filzmoser, which has a fourth power in the denominator. However, I also notice the Filzmoser equation (11) is missing a parenthesis in the numerator. Perhaps the fault is in Filzmoser 2008 and not in this manuscript? Please clarify.

Lines 117-120: Consider dividing this long sentence into two sentences for readability.

Equation 4: Would be helpful to define M and c immediately after equation 4, not after describing calculation of bi.

Line 138: I think it should be "BA/YD," not BA/YA. There are other instances of this typo throughout.

Line 145: I disagree with calling the data a CH4 "proxy." A proxy is when you measure one thing, and the data mean something else – like d18O and temperature. In the case of CH4, it's a true measurement of the CH4 concentration in the atmosphere in the past, rather than a proxy for it.

Figure 3: There are some typos in caption and legend.

Figure 4: Consider numbering the panels, or make them look more distinct. I originally thought the top right panel was panel 2. The arrows are not helpful as they are currently displayed because it took me too long to realize they point to the next step in the procedure.

Table 1 caption: "different" rather than "difference." By "point of departure" do you mean "preferred parameter choices used in the statistical analysis?"

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