This is the 4th review of the present study by Kozachek et al.: ***Large-scale drivers of Caucasus climate variability in meteorological records and Mt Elbrus ice cores***. I also reviewed the 2nd and 3rd version. The manuscript has constantly improved with each version. I appreciate the efforts by the authors to address the issues raised in the previous reviews. By now, almost all of these points are convincingly considered. Still, the splitting of the data into seasons seems not quite settled yet, a concern also raised by another reviewer. Further, there are a few explanations provided in the authors' response which did not find their way into the manuscript text although they seem important for clarity. Unfortunately, again the wrong dataset is plotted in one of the figure panels. Although such repeated, basic oversights do not help, I take the authors by their word, trusting that no comparable mistakes happened when evaluating the datasets. The language has improved significantly between the previous and this version, but nevertheless I encourage the authors to take advantage of the language editing offered by the journal. In conclusion, if the (generally) minor points in the comments below can be addressed, I think the manuscript could be accepted for publication.

Detailed comments:

L178-181 This is still unclear. If the maximum values are always assigned to July and the minimum values are always assigned to January, how can in some occasions minimum values suddenly become assigned to summer and maximum values to winter? This makes no sense. The approach described above is similar to the one in Vinther et al. (2010) and the underlying assumption for such an approach (max=summer, min=winter) is 50% winter and summer accumulation. The boundary between summer and winter is then defined by the middle between these two extreme (depth scale in m w.e.). This approach to split the record into seasonal data (cold, warm) allows comparison with the meteorological data separated into the seasons in the way described in the manuscript.
However, the way I understand the approach described in the present version it seems the minima and maxima are always assigned to the middle of the respective season (cold and warm, respectively). But still I cannot imagine why minima/maxima suddenly should become assigned to winter/summer. In any case the describe approach (or my interpretation of its description) contains two additional assumptions: (1) the month of lowest (highest) T is always in the mid-season and (2) this has not changed over the investigated period. Both points and their potential consequences for the analysis due to the fact that both introduce additional uncertainty should in this case be discussed. With the station T data at hand it is easily possible to investigate these two assumptions. Therefore, e.g. plot the months with lowest/highest T against time. The results should show the months of most extreme temperatures to (1) lie in the middle of the respective season and (2) the months when these minima/maxima were observed did not change over time. Actually (1) has been shown to be valid by the data presented in manuscript Fig. 4.
Please explain your approach accordingly or adjust your methodology following the description in Vinther et al. (2010). The subsequent evaluation of the data (correlation analysis etc.) and its interpretation should then also be revised.

L168-170 With the additional information about the two absolute time markers (1963 and 1912) now provided in L163-164, a lot about the dating was clarified including the reasoning behind the selection of the 100 year period. Clearly, this period was not selected because of the beauty of the number 100 as

suggested in the authors' response, but rather because at that depth the age scale is well defined by the 1912 time horizon. I thus suggest, adding this relevant information to the manuscript along the lines: "This period has been chosen because at this depth, the age scale is well defined by the time horizon found slightly below (Katmai 1912) resulting in a relatively small dating uncertainty of ±2 years, and because of the availability of other records such as local meteorological observations."

L279-280 and Fig. 8 Is the uncertainty of the defined lapse rate (not indicated in Fig. S3) propagated? In any case, the upper panel (annual means) looks very much the same as in the previous version 4. Please check if the correct dataset is plotted and include the lines indicating the standard deviation across the individual records.

L280-284 (and Fig. S4) As suggested, Fig. S4 has now been updated so that it is now consistent with the manuscript text. When now also adding the information about the station altitude to the figure legend, it becomes visually obvious that precipitation variability and particularly for the cold season precipitation amount has a strong altitude effect. With this, the choice to only use the two high altitude stations for precipitation data is clarified. The according text should be added to the manuscript. Therefore, please add altitudes to the legends in Fig. S4 and adjust the text along the lines: "All the precipitation data available for this region since 1966 is shown in fig. S4. Because of the obvious altitude dependence of both precipitation variability and precipitation amount (particularly for the cold season) only the data from the two high altitude stations Klukhorskiy Pereval (???? m asl.) and Mineralnye Vody (???? m asl.) were used for the calculations here. The two stations are further representative for stations with and without a prominent seasonal cycle (Mineralnye Vody and Klukhorskiy Pereval, respectively)."

L437-438 It is unclear for what season NAO is correlated with regional temperature. I suggest changing to: "For the cold season, the ice core d18O record shows…"

Fig. 7 There are only 11 increments for the 12 months which is confusing. To be consistent within the manuscript and with the commonly used way for display, please adjust the x-axis scale similar to manuscript Fig. 4.

Fig. 9 Even though the temperature record in this figure (and in Fig. 10) is now at least plotted on the correct age scale, it is still not correct. In the lowermost panel, not d18O is plotted but normalized temperature (most obvious by the y-axis scale). Please correct.

Table 4 (and according sections in the text) Although in this version, the normalized temperatures are again used for the correlation analysis as it was suggested, none of the correlation coefficients changed. I do not expect extreme changes but at least some considering the fact that in the previous version only the station data from Klukhorskiy Pereval and Mineralnye Vody was used.
Further, for the field in the upper left corner, annual means - T vs d18O – the value of 0.16 (n=100) is not significant and should not be bold.

<u>Language:</u>

When accepted for publication, please take advantage of the language editing service offered by the journal. One easy fix: in many cases δ18O should be replaced with $\delta^{18}O$.