Response to Editor

We would like to thank Editor Dr. Francesca Pellicciotti for reviewing our revised manuscript as well as the responses. In the following, we address all comments point-by-point according to editor’s comments.

Here are some suggestions of additional papers to consider for your Introduction and discussion:

Li et al., ERL, 2020, Does elevation dependent warming exist in high mountain Asia; You et al., Earth-Science Reviews, 2020, Elevation dependent warming over the Tibetan Plateau: patterns, mechanisms and perspectives; Guo et al., 2021, Sci Bull, Local changes in snow depth dominate the evolving pattern of elevation-dependent warming on the Tibetan Plateau.

Reviewer 1 suggests to cite the reference "Mountain Research Initiative EDW Working Group: Elevation-dependent warming in mountain regions of the world, Nature Climate Change, 5, 424-430, 2015." using "Pepin et al., 2015" rather than the present "Mountain Research Initiative EDW Working Group, 2015".

-Answer: Thanks a lot for the references. We added these publications in the context and also revised the Pepin et al. (2015) citation.

Response to Referee 2

We would like to thank anonymous Referee 2 for reviewing our revised manuscript as well as the responses. In the following, we address all comments point-by-point according to referee's comments.

The paper is improved by the addition of more detail of the datasets used, and I commend the authors on their careful and considered conclusions, which rightly highlight the complexity of this phenomena. While I still have some reservations about the dataset due to the fundamental importance of the lapse rates used to create the dataset in determining EDW, I understand the difficulties in validating such a dataset, and the limitations are well discussed in the paper now.

Some comments are discussed below.

I am clearer now about the purpose of looking at regional warming amplification, however I think the distinction between regional warming amplification and altitude warming amplification could still be made clearer. Unless the terminology of 'regional warming amplification' and 'altitude warming amplification' are used elsewhere in the literature, it might be clearer to stick to the terminology used in Rangwala and Miller (2012), and only use 'elevation dependent warming' to describe altitude warming amplification. It could be made clearer that section 3.1 is considering whether the Chinese Tianshan Mountains are warming faster than the surrounding lowland areas as a whole, and then that 3.2 and 3.3 are looking at elevation dependence within these mountains.

-Answer: Thanks a lot for the comment. This is a very important issue. In Rangwala and Miller (2012), they clearly pointed out that "*…we have explored available literature to address two important questions related to climate change in the mountain regions: (1) are mountain regions warming faster than* *low lying regions, and (2) is there an elevation-dependent climate response within mountain regions? From the available studies, it remains difficult to sufficiently assess whether mountains have warmed at a higher rate than the rest of the global land surface primarily because we lack adequate observations to resolve it conclusively.*" According to their conclusion, we summarized the "fundamental questions" of EDW into two aspects: regional warming amplification and altitude warming amplification. The former one focuses on the climate warming comparison with external regions (also include global), and the latter one focuses on the warming comparison within the mountain. Sometimes the altitude warming amplification is treated EDW in a narrow sense. In our understanding, these two "fundamental questions" must be answered separately.

As the Referee 2 pointed out, we investigated the "regional warming amplification" in Section 3.1 and detected the "altitude warming amplification" in Section 3.2. How does the temperature warming and what are the spatial characteristics within the CTM? To answer this question, we analyzed the spatial patterns of the CTM in Section 3.3. Meanwhile, we discussed the potential EDW mechanism in Section 4. At present, the structure of our paper is clear and complete, and it is consistent with the understanding of Referee 2.

Table 3 and 4: There still seem to be some instances where the warming trends are larger in both Tmin and Tmax than in Tmean. This may be what the data show, but I think it needs some discussion. It suggests either a fundamental change to the diurnal cycle, or that the results may be overly dependent on the hours chosen.

-Answer: Thanks a lot for the comment. We added more discussion with respect to Table 3 and 4 in this section. This phenomenon exists regardless of CTMD or CMA05 in the CTM, WCC and LCC. Therefore, it cannot be simply judged whether it is the reason from the changed diurnal cycle or the data source. We believe both have impacts on the warming trend. In the up to date publication, You et al (2020) also found this phenomenon over the Tibetan Plateau based on multiple data products.

Table 5: It is very useful to have all these put numbers in one table and makes a good addition to the paper. However, the method used to determine the trends is suggesting startling differences between the trends, which are being exasperated by elevation bands used to determine the trend.

For example, April in table 5, there is a suggestion of increased warming with elevation in Tmin and Tmax, but decreased in Tmean. This discrepancy seems to be due to the authors taking the gradient of the slope for minimum and mean temperature from all the elevation bands, but the gradient of the slope for the mean temperature only from 2500 m upward (Fig S4). Could you explain why you chose a different method for Tmax and Tmean? I think the values in table 5 should compare similar slopes, otherwise they are somewhat confusing. Fig S6 is also somewhat surprising, in that in the highest elevation band, the trends for minimum and maximum daily temperature are both smaller than the trend for mean daily temperature.

-Answer: Thanks a lot for the comment. That is true we used different elevation bands in Table 5 and Figures S1 to S12 in supplementary material. Our purpose is to discover the EDW at different altitudes. For example, if we take the all elevation bands for maximum temperature in April (Fig. S4), there is not any warming trend; even there is a cooling trend below 2500 m. However, it is obvious that there is an obvious warming trend above 2500m altitude. Due to length limitation, the results presented by the figures are only the final findings we found. In fact, in our analysis process, we did a lot of data analysis. Especially we attempted to fit the warming trend with different altitudes. Most of the fits did not reach statistical significance. Thus, we did not show these fitting results. However, this is still an open topic. For example, Li et al (2020) found a significant EDW in the altitude of 2500–5000m from 1980-2012 in the high mountain Asia. You et al (2020) concluded a clear EDW would be found above 2000 m in the Tibetan Plateau in 1961-1990. We share all the dataset released at https://doi.org/10.1594/PANGAEA.887700. We welcome other scholars to further explore EDW at different altitudes in the CTM. In the cation of Table 5, we also emphasize the bold and underlined value indicates a warming trend at higher altitudes, rather than the whole elevation band. We added more discussion in the revision.

Figure 5: While the subplots are added are striking, I am not wholly convinced that they are representative of the whole subregion being examined. For example, figure 5 b, in zone 2, if you took a similar transect at the very northern region of zone 2, would you see the opposite results? These subplots would be better based on average temperatures with elevation within each zone, rather than unique transects.

-Answer: Thanks a lot for the comment. We agree that the presented transect is just an example. However, if we plot the subplots based on the average temperatures and elevations for each zone, the consistent trend between temperature and trend would be nonexistent, because the altitude differences are averaged. As below Figure 1 shown that, the relationship between December minimum temperature and trend in Zone 2 is very complicated. We cannot arbitrarily judge that there is EDW or not. However, we could found that there are some very good liner relationships between temperature and elevation, which represent the EDW (within the red rectangle). Thus, the subplot (the example transect) we showed in the presented Figure 5 is just one of the "good relationships". We believe there are some other transects with significant EDW.

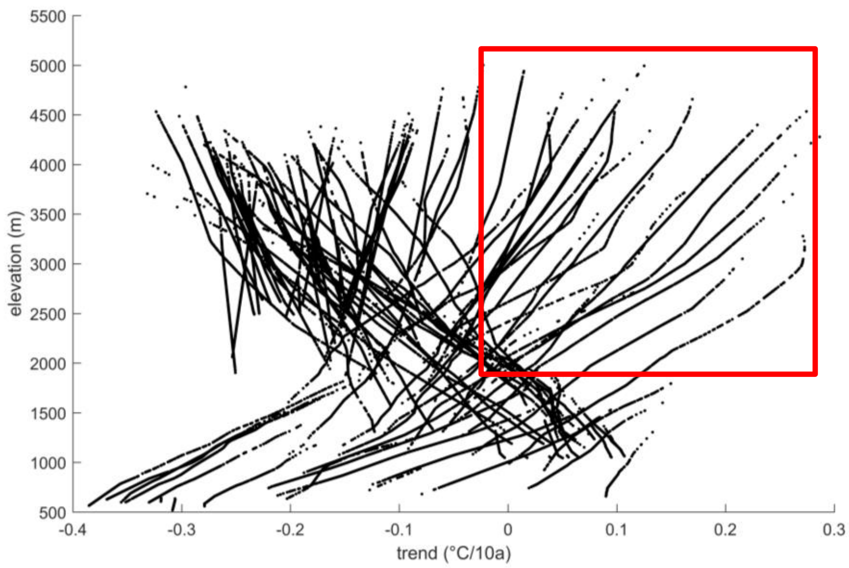

Figure 1: Scatter of December minimum temperature and elevation in Zone 2.

Minor comments

Line 75: please provide some references relating to the Alps, Andes and Rockies.

-Answer: Thanks a lot for the comment. We added the references in the revision.

Line 80: is this trend in minimum and maximum temperature differences a worldwide phenomena?

-Answer: Thanks a lot for the comment. We made it clear in the revision.

Line 137: some words missing in this sentence 'for example, the lapse rates of ERA_Interim are greater than those from September to December'.

-Answer: Thanks a lot for the comment. We revised this language mistakes in the revision.

Response to Referee 3

We would like to thank anonymous Referee 3 for reviewing our revised manuscript as well as the responses. In the following, we address all comments point-by-point according to referee's comments.

The manuscript revision by Lu Gao and co-authors has improved significantly based upon the detailed comments of the editor and all reviewers. The authors have done a lot of additional work to incorporate the reviewer's points and this reflects in a more robust article that better explains the methodology and limitations of EDW exploration in the Tianshan mountain range. While I am generally happy with the changes made to the manuscript, a few comments remain, as well as some small minor text changes. With these changes made, I would recommend the manuscript be accepted for publication.

General comments:

1) The results section is nicely divided into a regional, altitudinal and sub-domain focus. However, each section is a little too descriptive and the authors should attempt to shorten each section, focusing upon the main features and utilizing the tables and figures to explain all of the individual values of warming and significance etc.

-Answer: Thanks a lot for the comment. At present, the length of the article is indeed longer than the previous version, since we answered all referees' comments point-by-point and added some more analysis. We revised some parts in the revision.

2) I believe that the figures have improved slightly, though I think small changes could still help the reader to navigate the information more easily. See specific comments on figures below.

-Answer: Thanks a lot for the comment. We revised the figures according to referee's comments in the revision.

3) The authors have incorporated information regarding snow following my initial review. I think this is a good additional to the manuscript, though a few small pieces of information are still missing in my opinion. For example, the snow cover rate (or rather fraction) and average (?) depth is provided for the whole CTM for a given month in each year and compared to the mean (all pixel) warming rate for each month? I think the authors could show the elevation of snow cover in those years vs. the EDW without too much additional effort, but adding extra value to the study. I think the authors should in fact add this as an additional (but succinct) results section rather than just in the discussion. Especially as the data are presented earlier in the manuscript.

-Answer: Thanks a lot for the comment. The referee is right that "snow cover fraction" is more appropriate. We changed the "snow cover rate" to "snow cover fraction" in the revision. This fraction is for the entire CTM. We added more information about the snow data set in the revision. The snow cover fraction is at annual scale. We have no elevation information of snow cover, only two values (maximum fraction and minimum fraction) per year. Here, we added two more tables to show the relationship between maximum/minimum snow cover fraction and monthly temperatures (Table 1 and 2). The monthly snow depth calculated from daily depth was applied for the relationship of snow depth and temperature.

Table 1. Relationship (R^2) of maximum snow cover fraction (%) and monthly Tmin, Tmean and Tmax from 2002 to 2013.

	Tmin	Tmean	Tmax
January	0.086	0.024	0.117
February	0.302 *	0.038	0.009
March	0.005	0.073	0.102
April	0.075	0.089	0.060
May	0.162	0.000	0.012
June	0.025	0.096	0.012
July	0.144	0.158	0.161

August	0.033	0.036	0.001
September	0.019	0.186	0.003
October	0.003	0.001	0.001
November	0.060	0.097	0.017
December	0.002	0.017	0.003

Note: * denotes the significance level $p < 0.1$.

Table 2. Relationship (R^2) of minimum snow cover fraction (%) and monthly Tmin, Tmean and Tmax from 2002 to 2013.

	Tmin	Tmean	Tmax
January	0.181	0.092	0.093
February	0.198	0.320	0.073
March	0.171	0.153	0.068
April	0.106	0.118	0.006
May	0.031	0.296 *	0.043
June	0.085	0.244	0.020
July	0.246	0.006	0.019
August	0.000	0.156	0.256 *
September	0.004	0.081	0.043
October	0.056	0.026	0.022
November	0.001	0.024	0.009
December	0.001	0.011	0.003

Note: * denotes the significance level $p < 0.1$.

Specific comments:

L24: "..typical high mountain regions…"

-Answer: Thanks a lot for the comment. We revised it in the revision.

L40, add semi-colon after "characteristics".

-Answer: Thanks a lot for the comment. We revised it in the revision.

L43: "..Outside of these mountain ranges."

-Answer: Thanks a lot for the comment. We revised it in the revision.

L45: as L24

-Answer: Thanks a lot for the comment. We revised it in the revision.

L45: add "..BOTH" before " observations and models"

-Answer: Thanks a lot for the comment. We revised it in the revision.

L50: I think a more recent reference regarding water towers (Immerzeel et al. 2020) would be suitable here. Immerzeel, W. W. et al. (2020) 'Importance and vulnerability of the world's water towers', Nature, 577(7790), pp. 364–369. doi: 10.1038/s41586-019-1822-y.

-Answer: Thanks a lot for the comment. We added this reference in the revision.

L60-65: There are a lot of short sentences that could be merged and improved for flow.

-Answer: Thanks a lot for the comment. We revised it in the revision.

L97: The Immerzeel reference would also be appropriate here.

-Answer: Thanks a lot for the comment. We added this reference in the revision.

L99: How do the authors quantify "water resources" here? Is this a water equivalent of ice volume? It is not clear to me and should be revised.

-Answer: Thanks a lot for the comment. Yes, it means ice volume. We revised it in the revision.

L101: warming at what elevation? A mean of the entire CTM? Perhaps clarify that here.

-Answer: Thanks a lot for the comment. We revised it in the revision.

L115: change "system" to "systematic"

-Answer: Thanks a lot for the comment. We revised it in the revision.

L145: change to “low elevation terrain”

-Answer: Thanks a lot for the comment. We revised it in the revision.

L160: Reference needed here to support the ‘reliability’ of CMA05.

-Answer: Thanks a lot for the comment. We added some references in the revision.

L162: first step? I do not understand what the authors refer to here. Please revise this to clarify.

-Answer: Thanks a lot for the comment. The terrain of China can be roughly divided into three steps according to altitude range. The Qinghai-Tibet Plateau has the highest average altitude. Thus, it is called the first step. To avoid ambiguity, we revised it in the revision.

L167: snow cover rate? The authors describe a snow cover fraction here. This terminology should be ideally used throughout the manuscript. It is not explicit whether this fraction is just for the entire CTM or another area. The authors need to briefly clarify this. As mentioned, I think the authors could better leverage this information, if only simply, in order to show the snow cover fraction by elevation bands. I believe that this would more appropriately indicate the relationship to altitudinally resolved EDW.

-Answer: Thanks a lot for the comment. Yes, “snow cover fraction” is more appropriate, and we revised it in the revision. This fraction is for the entire CTM. The snow cover fraction is at annual scale. It means that we have no elevation information of snow cover, only two values (maximum fraction and minimum fraction) per year. Here, we added two more tables to show the relationship between maximum/minimum snow cover fraction and monthly temperatures (Table 1 and 2). The monthly snow depth calculated from daily depth was applied for the relationship of snow depth and temperature.

Table 1. Relationship (R^2) of maximum snow cover fraction (%) and monthly Tmin, Tmean and Tmax from 2002 to 2013.

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October	0.003	0.001	0.001
November	0.060	0.097	0.017
December	0.002	0.017	0.003

Note: * denotes the significance level $p < 0.1$.

Table 2. Relationship (R^2) of minimum snow cover fraction (%) and monthly Tmin, Tmean and Tmax from 2002 to 2013.

	Tmin	Tmean	Tmax
January	0.181	0.092	0.093
February	0.198	0.320	0.073
March	0.171	0.153	0.068
April	0.106	0.118	0.006
May	0.031	0.296 *	0.043
June	0.085	0.244	0.020
July	0.246	0.006	0.019
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November	0.001	0.024	0.009
December	0.001	0.011	0.003

Note: * denotes the significance level $p < 0.1$.

L189: but y was just given as variable estimate from equation 2

-Answer: Thanks a lot for the comment. We revised it in the revision.

L203-204: Not clear, please re-write this more clearly. The authors mean that although some pixels did not have significant change, all pixels in CTM were averaged and compared to WCC and LCC?

-Answer: Thanks a lot for the comment. Yes, the referee is right. This comparison is to detect the regional warming amplification. Although, the trend in some grids did not reach a statistically significant level, it can still reflect climate warming on a regional scale. Thus, we used the all grids in the CTM for comparison.

L231: better to write as “regional warming”

-Answer: Thanks a lot for the comment. We revised it in the revision.

L297: hilltop? The authors refer to Mountain peaks?

-Answer: Thanks a lot for the comment. We revised it to Mountain peak in the revision.

L324: not types – metrics or indicators (as previously written)

-Answer: Thanks a lot for the comment. We revised it in the revision.

L326: “terrain” not “terrains”

-Answer: Thanks a lot for the comment. We revised it in the revision.

L335: “for” Tmin

-Answer: Thanks a lot for the comment. We revised it in the revision.

L358: A reference is required for this statement.

-Answer: Thanks a lot for the comment. We revised it in the revision.

L370: These are very small snow depth values and likely within the uncertainty of the microwave measurements? Perhaps the changes in depth are therefore not significant? This is another example where elevation bands of depth could be more informative than the average for the whole CTM.

-Answer: Thanks a lot for the comment. We admit that there may be certain errors in the snow depth data, especially the snow depth information was extracted from different remote sensing data. However, the data provider claimed that the data accuracy is above 90% via validation. Thus, this is the best data we could obtain. We believe that the referee's suggestion on the elevation bands of depth is fantastic. We found that Li et al (2021) just published a similar work on the Tibetan Plateau. Currently, the elevation information of snow depth needs more time to collect and process. Thus, this suggestion is a topic of on-going and future research in the CTM.

L377: the reported value is the significance (p) value, not the "remarkable" correlation value.

-Answer: Thanks a lot for the comment. We revised it in the revision.

L396: How can the authors state a higher accuracy of monthly EDW here? There is no evidence that the monthly values are more accurate, rather that they allow the exploration of sub-seasonal trends that are obscured when averaging over several months/the whole year.

-Answer: Thanks a lot for the comment. Yes, the referee is right. This statement is indeed not very rigorous. Previous studies on EDW were mostly on a seasonal scale, and it tends to overlook the potential EDW. According to our experience, the detection of EDW on a monthly scale is more effective and reasonable. We revised it in the revision.

Figures:

The figures have improved a little, though I still find figures 2-4 could be improved. I think that each subplot could have a title that specifies the month, rather than having to look to the caption, especially as the group of months changes for each figure. I think a righthand axes with a shaded area or bar could be used to indicate the percentage of the pixels in each elevation band as a product of the total area (total pixels). As this does not change between each panel, it could also be added to figure 1.

-Answer: Thanks a lot for the comment. The referee is right. We added the month for all subplots for Figure 2 to 4 for a better readability.

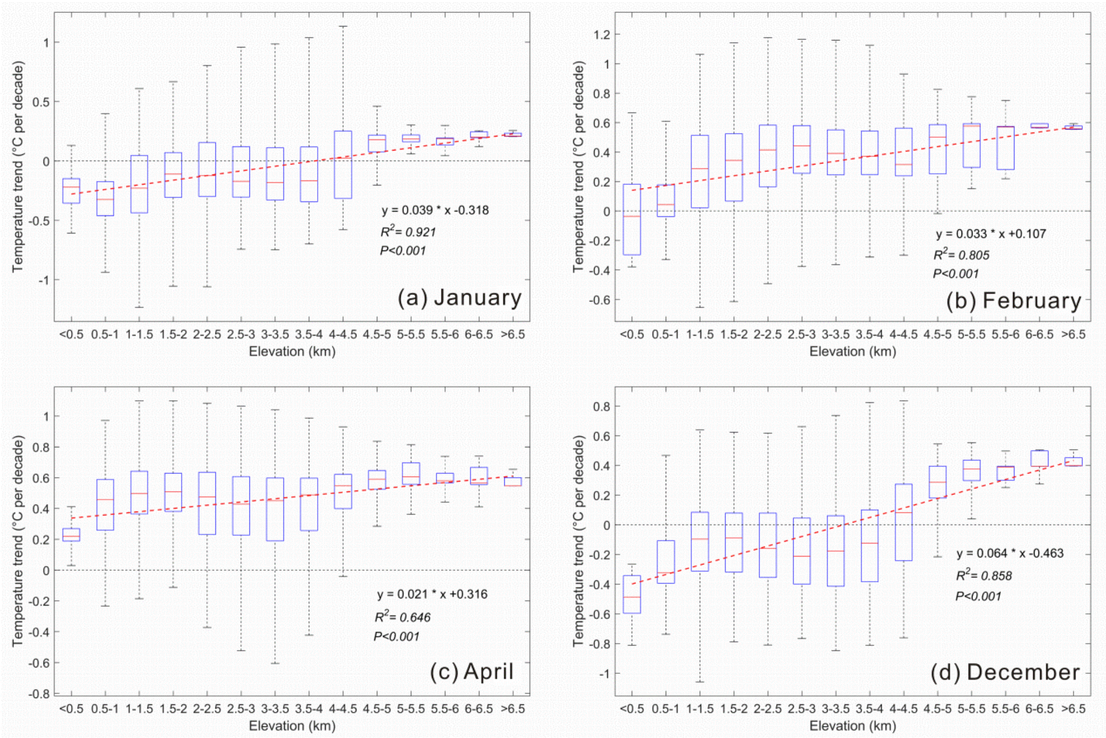

Figure 2: Box plots of monthly minimum temperature trends at different elevations from 1979 to 2016. (a) January, (b) February, (c) April, and (d) December. Thick horizontal lines in boxes show the median values. Boxes indicate the inner-quantile range (25% to 75%) and the whiskers show the full range of the values. The red dashed lines represent the significance of EDW.

About the percentage of the pixels in each elevation band, actually we have provided this information in Table 1. We added one more column to show the percentage information in the revision.

Table 1. Grid number and percentage for each altitude group over the CTMD.

	Altitude range (m)	Grid number	Percentage (%)
1	<500	3139	0.881
2	500–1000	30810	8.651
3	1000-1500	83018	23.311
4	1500-2000	70229	19.720
5	2000-2500	46545	13.069
6	2500-3000	43400	12.186
7	3000-3500	39579	11.114
8	3500-4000	28256	7.934
9	4000-4500	8789	2.468
10	4500-5000	1666	0.468
11	5000-5500	496	0.139
12	5500-6000	150	0.042
13	6000-6500	52	0.015
14	>6500	4	0.001

Figure 1 should also be referred to in the text, as it is not currently.

-Answer: Thanks a lot for the comment. We revised it in the revision.

Figures 5-7: I understand the reviewers point regarding the colour bar scaling being different in each figure. However, I think the authors should still consider setting 0°C 10a-1 to yellow in all figures, so the divergent colour scale (blue negative, red positive) is always equal and the intensity of blues and reds can still be compared for different figures, even though the scale limits are different.

-Answer: Thanks a lot for the comment. We revised all the figures S14 to S30 in the Supplementary material using the same colour bar for a better comparison. As we expected, sometimes the spatial pattern is not significant. The following is an example:

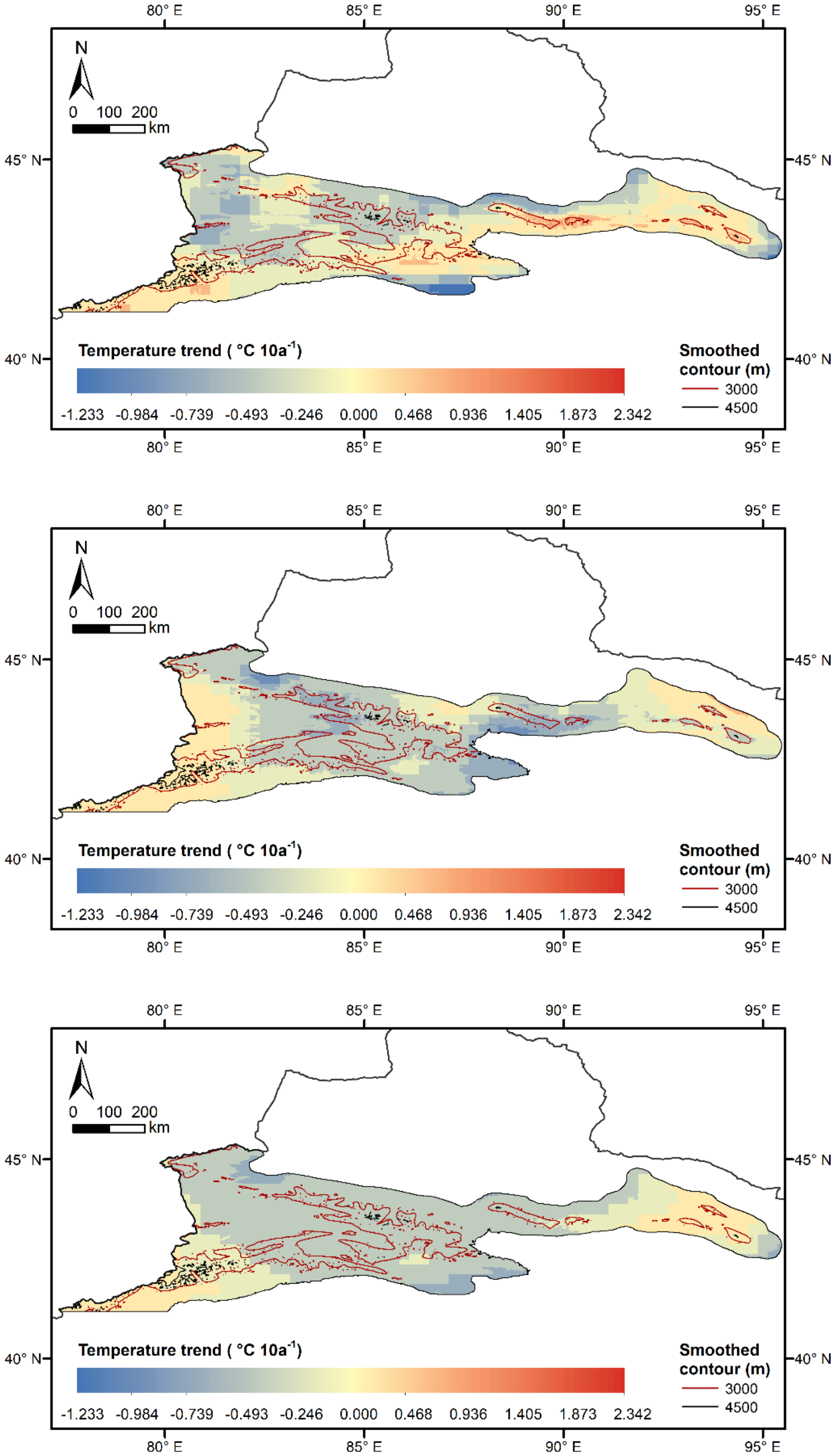

Fig. S14 Monthly temperature trends in January for the entire CTM from 1979–2016, minimum temperature (up), maximum temperature (middle) and mean temperature (down).

Thus, we keep different colour bars in figure 5 to 7. Actually, in the last revision, we have set the yellow (RGB: 255, 255, 0) for zero, blue for negative trend and red for positive trend according to referee's comment. The gradient divergent color (blue and red) represents the changed value. In the software (ArcMap 10.0©), the value of zero is not labeled. We could compare the old version and the revised version for maximum temperature in March (Figure 3).

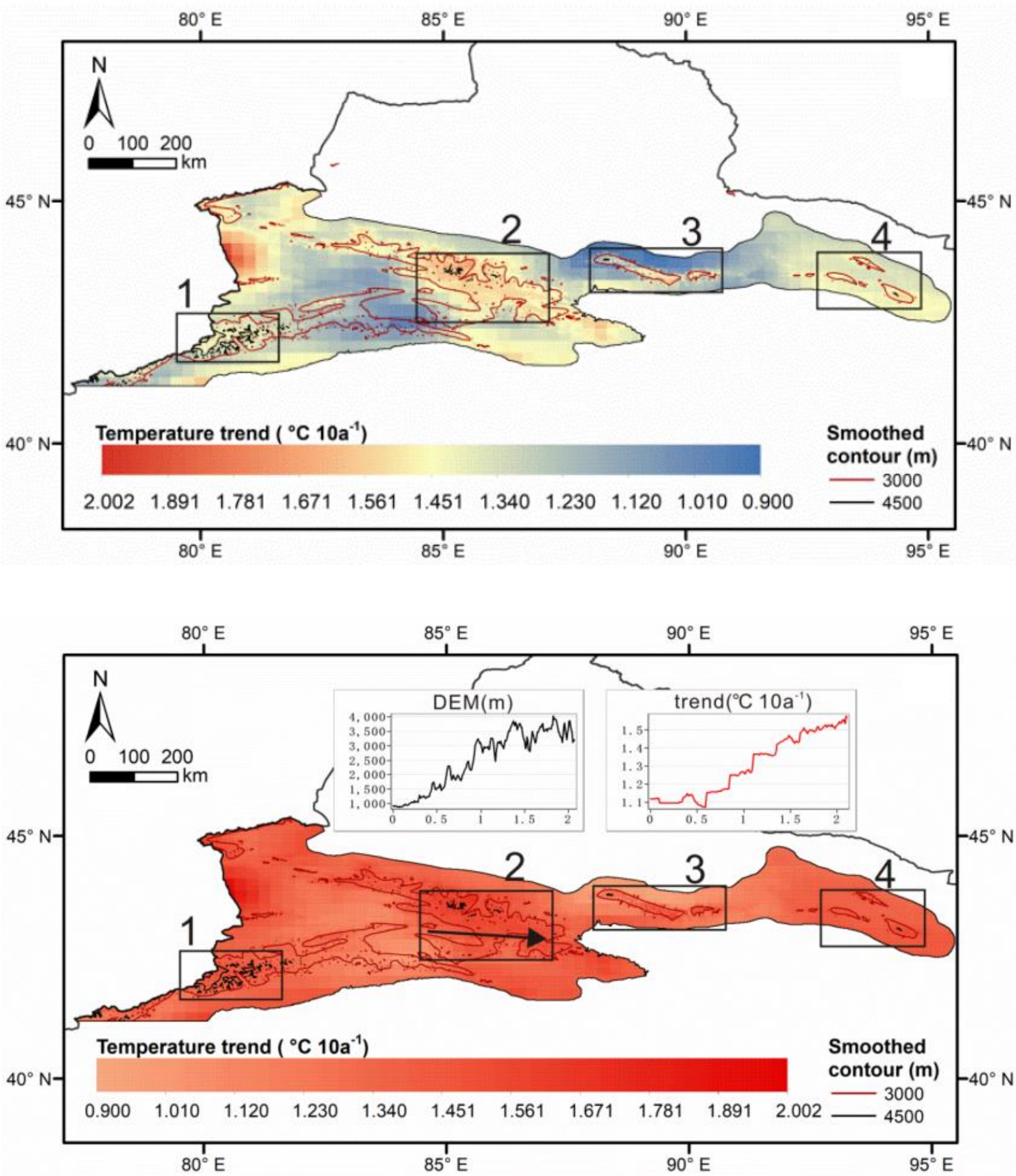

Figure 3: Monthly maximum temperature trends in March in old version (up) and revised version (down).

References:

Guo, D., Pepin, N., Yang, K., Sun, J., and Li, D.: Local changes in snow depth dominate the evolving pattern of elevation-dependent warming on the Tibetan Plateau, Science Bulletin, 66(11), 1146-1150, 2021.

Immerzeel, W.W., Lutz, A.F., Andrade, M., Bahl, A., Biemans, H., Bolch, T., Hyde, S., Brumby, S., Davies, B.J., Elmore, A.C., Emmer, A., Feng, M., Fernández, A., Haritashya, U., Kargel, J.S., Koppes, M., Kraaijenbrink, P.D.A., Kulkarni, A.V., Mayewski, P.A., Nepal, S., Pacheco, P., Painter, T.H., Pellicciotti, F., Rajaram, H., Rupper, S., Sinisalo, A., Shrestha, A.B., Viviroli, D., Wada, Y., Xiao, C., Yao, T., and Baillie, J.E.M.: Importance and vulnerability of the world's water towers, Nature, 577(7790), 364-369, 2020.

Li, B., Chen, Y., and Shi, X.: Does elevation dependent warming exist in high mountain Asia? Environmental Research Letters, 15, 024012, 2020.

Pepin, N., Bradley, R.S., Diaz, H.F., Baraer, M., Caceres, E.B., Forsythe, N., Fowler, H., Greenwood, G., Hashmi, M.Z., Liu, X.D., Miller, J.R., Ning, L., Ohmura, A., Palazzi, E., Rangwala, I., Schöner, W., Severskiy, I., Shahgedanova, M., Wang, M.B., Williamson, S.N., and Yang, D.Q.: Elevation-dependent warming in mountain regions of the world, Nature Climate Change, 5, 424-430, 2015.

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You, Q., Chen, D., Wu, F., Pepin, N., Cai, Z., Ahrens, B., Jiang, Z., Wu, Z., Kang, S., and AghaKouchak, A.: Elevation dependent warming over the Tibetan Plateau: Patterns, mechanisms and perspectives, Earth-Science Reviews, 210, 103349, 2020.