Anonymous Referee #2

Thanks for reviewing our manuscript and for your recommendations. Please find our answers below highlighted in blue. You may also wish to check our reply to Referee #1 as there is some overlap with the issues. Page (P) and line (L) numbers refer to the revised manuscript.

General Comments

This paper uses the ECHAM5-iso model to look at variations of oxygen-18 in Western Siberian precipitation during the last 50 years, and they aim to "assess the potential of a recently opened monitoring station in Kourovka to successfully track large-scale water cycle and climate change in this area." While this has potential I think that the main message of this work has got lost in the detail. The paper provides much discussion of interannual/decadal variability in temperature, precipitation and $\delta^{18}O$ – across western Siberia, and at the Kourovka station – although it is not well tied together and the reader is left to disentangle much of the message themselves. I would also like to see much more discussion about how these results could be utilized to take the science forwards. For example although the paper shows that Kourovka $\delta^{18}O$ is related to the temperature of the region – how could this information be used in future studies.

The introduction now includes an improved outline in which we expose our objectives and explain the organisation of the manuscript (see P 5, L 7 – 24). From this it should become more clear why considerable parts of the manuscript deal with model validation and the discussion of interannual variability, before we are able to assess the potential of $\delta^{18}O$ as a tracer for climate change in Western Siberia and the potential of Kourovka as a representative monitoring site in this region. Regarding the scientific advancement, we discuss additional investigations indicating that the interannual isotopic variability in summer can be attributed to changes of regional soil moisture, evaporation and convective precipitation. From this we are now able to conclude that the summer signal of precipitation $\delta^{18}O$ integrates climatic processes other than surface warming or cooling, and that therefore, $\delta^{18}O$ has the potential to reveal hydrometeorological or hydroclimatological changes in this region which are difficult to identify by conventional measurements. See P 17, L 9 – P 18, L 15; P 18, L 31 – P 19, L 11; P 30 (i.e. Table 3), and Figure 12a–c.

The text on all of the figures is too small. The titles on the figures seem to be meaningful for the authors of this paper, rather than for the readers of the paper.

Done.

Specific Comments

Statistics could be used a lot better in this paper. They seem to be only used to provide evidence of obvious statements, while statistics are not used to back up statements which need some

justification.

The use of statistics has been improved in Section 3. We now discuss uncertainty ranges and consider the statistical significance of our results. Numbers are listed in the new table 2 (see P 29). Uncertainty ranges have been also added to Figures 3, 4 and 6.

Examples of this are: 1. Abstract: Line 15. and P29273 lines 19 and 28. “Annual mean model results and measurements are highly correlated (r=-0.95). This is not a good use of correlation/r values; as it is quite meaningless whether the two are correlated. It is whether the two are close to the 1-1 line that is meaningful in this case.

The order of discussion has been reorganised, see P 11, L 6 – P 12, L1.

2. P29275 line 15. You mentioned that global modelled precipitation may have decreased – this is not clear from the plot and would be a good use of statistics to show this.

Done, see P 13, L 15 – 19, P 29, Table 2, and the revised Fig. 6b. Please note that the global curves shown in Fig. 6 have been recalculated because ocean areas were not correctly eliminated in previous calculations.

3. P29275 line 25 – P29276 line 14. Because the data is very noisy it is difficult to be certain about some of these trends. This would be a very useful place to add some statistics to back up what you are saying.

Done, see P 13, L 2 – P 14, L 1, and P 29, Table 2 for a summary of numerical results.

4. P29279 line 3 - “correlation decreases in Northern Siberia” could you say the original and new values of correlation.

Done, see P 16, L 14: “the correlation decreases in Northern Siberia from r ~ 0.6 to values less than about 0.4”.

5. P29279 lines 11-13. Would be a very good place for some r values, to highlight the relative importance of the NAO on $\delta^{18}O$ and temperature.

Done, see P 16, L 24 – 27.

The abstract contains a lot of detail but it is very difficult to extract the main message from this. See general comments. I would suggest that the abstract is fully rewritten to include less detail but that the main message and its importance for the future science is highlighted.

Done, see P 2.

P 29266: line 1-5. Why would it be useful to see Arctic warming in $\delta^{18}O$? We know the Arctic is warming from temperature observations? Is the point of this for calibrating the paleothermometer

for paleo studies? If so this should be mentioned.

Done, see P 3, L 25 – 27: “The magnitude of this isotopic response is of interest when it comes to reconstruct past regional climate changes by isotope data retrieved from various paleoclimate archives (e.g., Sidorova et al., 2010).”

P29268: line 1-3: “How well can large-scale West Siberian climate and water cycle variations be observed in the isotopic composition of precipitation at Kourovka Observatory?” I would like to see this question better addressed in the conclusions along with discussion for its utility. (See general comments).

Done, the conclusions have been reformulated. In particular, we conclude that “$\delta^{18}O$ has the potential to reveal hydrometeorological regime shifts in future summers which are otherwise difficult to identify”, see P 18, L 31 – P 19, L 11.

Section 2: Did the model include vegetation? If so how was that treated/initialised/spun up?

Vegetation in the model is prescribed by a time-invariant set of land surface data (vegetation ratio, leaf area index, forest ratio, background albedo). This is now mentioned on P 7, L 11 – 13.

P29270 line 18. Stations where monthly mean temperatures disagree by more than 10degC were not included. The 10degC appears quite arbitrary. What about stations where the disagreement was 9degC?

At all other stations, WMO, GNIP and reanalysis temperatures agreed within the range of about ±2°C. The data quality problem has been stated more precisely as follows (P 7, L 32 – P 8, L 4): “We excluded six stations (Kandalaksa, Khanty-Mansiysk, Kursk, Olenek, Salekhard, and Terney) where reported monthly mean temperatures are clearly unrealistic (i.e. showing winter values in summer or vice versa) and systematically disagree from WMO measurements and/or ECWMF reanalysis data, indicating issues with data quality control within the GNIP database.”

P29271 line 14-15. "Explain what is meant by convolved with averaging kernels", so that the paper can be accessed by those not familiar with this technique, who don’t have the Risi paper to hand.

Done. We have added two new paragraphs describing in more detail the model-data comparison procedure. See P 8, L 19 – P 9, L7.

P29271 lines 18-22. It appears that Gribanov 2013 does much data-model comparison with the same model against the same data – and there is a lot of data-model comparison in this paper. It would be useful here to state (as introduction) the additional data-model comparisons that will be performed in this paper and how this takes the Gribanov study forward.

Done. See P 9, L 11 – 15: “Gribanov et al. (2013) showed that the ECHAM5-wiso simulation

results agree well (...) for the year 2012. As we are going to analyse model results for 1960 – 2010, the period of model validation with meteorological observations has been extended accordingly. This section summarises the results of the updated validation."

P29273. Lines 2-3. Are you saying there is an offset? If so the offset should be added onto the figure so that the reader can understand and compare how the patterns agree more thoroughly.

Yes, there is a systematic offset due to the absence of calibration. In Fig. 2, we take this offset into account so that the reader can better compare the spatial patterns. In the caption of figure 2 it has been stated: "In (a) and (b) the global average of δD has been subtracted to highlight spatial patterns". Now we clarify this in the main text as well : "As there is no absolute calibration for column-integrated δD of GOSAT (Risi et al., 2013), we subtract the global average of δD for both GOSAT and ECHAM to enable an improved comparison focussing on the spatial distributions" (P 10, L 18 – 20).

P29273. line 10. "underestimate the eastward depletion...." by how much?

We added: "From 20°E to 120°E, δD decreases by about 80‰ in GOSAT observations and by only about 40 permil in ECHAM" (P 10, L 27 – 28).

P29273. Line 20. Change the sentence "A linear fit indicates that ECHAM5-wiso tends to underestimate the observed temperatures by 0.6degC". You should not need to do a linear fit to show this – you can simply average the model results and the observations and subtract them.

We agree. However, this section now also discusses the uncertainty range of estimated mean values and the conclusion is not robust any more. Therefore, the statement has been reformulated as follows (P 11, L 8 – 14): "A linear fit, applying an algorithm which accounts for the uncertainties in both coordinates (Krystek and Anton, 2007), yields an optimum slope of 1.05±0.23 and an optimum intercept of (-0.75±1.45)°C. This may suggest that the model tends to underestimate the observed temperatures. (…) However, given the uncertainty range of the fit, the conclusion of an overall cold bias of ECHAM5-wiso is not robust."

P29274. line4. Label these stations on the figure to help with clarity

Done.

P29278 line 24. The shift towards the arctic ocean is very difficult to see in the figure. Perhaps a schematic would be better.

Figure 11 has been enlarged.

P29281 line8 Quantify the importance of temperature on $\delta^{18}O$. What percentage of $\delta^{18}O$ variation can be attributed directly to temperature?

Done, see P 18, L 28 –32: “According to our model results, temperature is the predominant factor controlling up to 80% of the variability of annual-mean and winter precipitation $\delta^{18}O$ in Russia on interannual to decadal time scales. During summer, local temperature has only a minor impact (about 20%) on the isotopic composition of West Siberian precipitation.”

P29281 line 12. You say that “Our analyses support the importance of moisture recycling, involving the delayed reevaporation of isotopically depleted winter precipitation....” However it appears that the only justification for this is that DJF results are better correlated than JJA results. Perhaps you could do some further tests on this using a multiple correlation analysis of upstream DJF temperature and local JJA temperature to determine $\delta^{18}O$.

This point has been addressed by additional investigations. We show that in summer, interannual variations of precipitation $\delta^{18}O$ can be attributed to interannual changes of regional soil moisture, evaporation and convective precipitation. This indicates that the summer signal of precipitation $\delta^{18}O$ integrates climatic processes other than surface warming or cooling, and that $\delta^{18}O$ has the potential to reveal hydrometeorological regime shifts in the future which are otherwise difficult to identify. See P 17, L 9 – P 18, L15.

Figure 7. Rescale since everything is red!

Done.

Figure 8. I think the caption is wrong as the correlation between “global sea-level pressure and $\delta^{18}O$ in precipitation at Kourovka” would give a single value – there would be no need for a map. Do you mean “local sea level pressure”. Same comment for figure 12.

Done. The caption now reads (P 32, L 16 – 17): “One-point correlation map showing the correlation between $\delta^{18}O$ in precipitation at Kourovka and local sea-level pressure during winter”

Technical Comments:

Abstract: line 5 - do you mean "underlying mechanisms causing this variability"

Yes, but the phrase has been dropped from the abstract.

P29265: line 20 - do you mean "until the end of this century"? I think this should be "may have increased by ∼ 25% at the end of the century"

Changed, see P 3, L 16.

P29266 line 7: “negative zonal isotope gradient”. Could you simplify this by showing the exact direction of change (i.e. more depleted further East). Also this has been known for some time and some references are needed.

Done. The passage now reads “Data (...) depict eastward isotopic depletion over Russia (…). This continental effect has been known for some time (e.g. Araguas-Araguas et al., 2000, and further references therein).” See P 3, L 30 – P 4, L 2.

P29274. line 24-25. This is misleading as it implies that the seasonal cycle is sometimes 25permil and sometimes 5permil. This is not what you mean.

Changed (P 12, L 17 – 18): “The data exhibit seasonal variations ranging from -25‰ in winter to -5‰ in summer, closely following the seasonal cycle of temperature.”