

Anonymous Referee #1

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 #2 as there is some overlap with the issues. Page (P) and line (L) numbers refer to the revised manuscript.

Major Comment:

As I understand it, the three main objectives of the paper are (i) to evaluate the ECHAM5-wiso simulation over Siberia, (ii) to assess the representativeness of the newly established observation site Kourouka, and (iii) to investigate if stable water isotopes can be used to monitor climate change in Siberia. Regarding point (i), I think that this is a prerequisite for the application of the model for process-oriented studies, but alone does not justify the publication of a new manuscript. The model has already been evaluated on global spatial scales in previous studies.

Werner et al. (2011) have validated the model on the global scale, and Langebroeck et al. (2011) discuss the model performance for Central and Western Europe. The model has not been validated for Russia at that level of detail. As a by-product, validation sheds light on the poor observational quality of some Russian GNIP stations. In the revision we explain the motivation for the validation as follows (P 5, L 19 - 22): “In particular, we present a thorough comparison of simulated and observed precipitation $\delta^{18}\text{O}$ in Russia, going beyond the previous global model assessment by Werner et al. (2011). This validation is a prerequisite for the following discussion of isotopic interannual variability and mechanisms.”

Furthermore, there is an accompanying paper evaluating the same model with data from Kourouka on (sub-)daily time scales (Gribanov et al., 2013), and there seems to be another publication in preparation dealing with the evaluation of a different model with the same data (Gryazin et al., 2013). If it is only about model evaluation, I think that this could easily be achieved in a single publication dealing with two models and different time scales.

Gribanov et al. (2013) discuss the isotopic short-term variability in Kourouka in 2012. Gryazin et al. will present analyses of the same data. We do not think that this is a good idea to merge their paper our paper with our manuscript. Here, we focus on the long-term variability during the last five decades, which is a completely different scope. This is now explained in the introduction (P 5, L 7 – 9).

With respect to point (ii), the most important innovation of the station Kourouka is the availability also of vapour data with high temporal resolution, is it not? This does not become very clear in the manuscript. Nevertheless, the representativeness of the station is only evaluated regarding monthly precipitation data, which I do not consider to be particularly interesting. There have been GNIP

stations relatively close by that provided the same kind of monthly data, at least for some periods between 1980 and 2000. Are these stations not in operation any more?

While we agree with Referee #1 that the Kourovka vapour data are exiting, we cannot draw any conclusion regarding the representativeness of this station for Western Siberia from these data. Our approach is to assess the representativeness of Kourovka by using a model, but regarding the isotopic composition of water vapour the observational data base for the necessary model validation is insufficient in this region (one station operating since 2012). For this reason, we do not agree with the statement that precipitation data are not particularly interesting. At present, precipitation isotope data provide more and longer time series. GNIP stations in this region are not operating any more which is now explicitly mentioned on P 7, L 25 – 27. This is one of the reasons why precipitation isotope Kourovka monitoring at Kourovka has started in October 2012.

Point (iii) is, in my eyes, the most promising scientific goal of the present study. Nevertheless, the results presented do not convince me that isotopes really provide novel, complementary information. (...) In terms of climate monitoring, it would be essential to demonstrate that the isotopes provide information that cannot easily be obtained otherwise (e.g., information on continental moisture recycling in summer, which is mentioned several times, but not explored further). In summary, in my opinion the scientific content of the manuscript has to be extended, since points (i) and (ii) alone do not justify the publication of a stand-alone manuscript, and point (iii) is not adequately explored in the present paper.

Point (iii) has been addressed by additional investigations. We show that in summer, interannual variations of precipitation $\delta^{18}\text{O}$ can be attributed to interannual changes of regional soil moisture, evaporation and convective precipitation. This indicates that the summer signal of precipitation $\delta^{18}\text{O}$ integrates climatic processes other than surface warming or cooling, and that $\delta^{18}\text{O}$ has the potential to reveal hydrometeorological regime shifts in the future which are otherwise difficult to identify. See P 17, L 9 – P 18, L 15; P 18, L 31 – P 19, L 11, P 30 (i.e. Table 3), and Figure 12a–c.

Minor Comments:

The abstract should be written more concisely and clearly, focusing on the main scientific objectives.

Done, see P 2.

P 29264, L 2: “a large increase” or “large increases”

Done, see abstract P 2, L 2.

P 29264, L 16 (and other elsewhere): The root mean square deviation is defined to be positive; omit plus-minus sign.

Done.

P 29265, L 7: “amounts to”

Done, see P 3, L 5.

P 29265, L 16: “it is so far less determined”: awkward wording

Changed to “it is uncertain”, see P 3, L 13.

P 29266, L 7: Mention that this is a west to east gradient.

Changed to “eastward isotopic depletion”, see P 3, L31.

P 29268, L 16-17: Why is cloud ice mentioned with respect to the isotopes, but not to the normal water? I do not like the terms “active” and “passive tracers” too much (though they might be technically correct).

Changed, the sentence now reads (P 6, L 6 – 8): “Therefore, the transport scheme for all water-related variables is the flux-form semi-Lagrangian transport scheme for positive definite variables implemented in ECHAM5.”

P 29271, L 18 – P 29272, L 2: It is not clear to me which of the results from this paragraph (comparison with station data from Yekaterinburg) are taken from Gribanov et al. and which are new.

All results are new, because in this paper the model is validated with observations for a much longer period. This has been clarified at P 9, L 11 – 15: “Gribanov et al. (2013) showed that the ECHAM5-wiso simulation results agree well with observations from Kourouka Observatory and the surrounding area 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.”

“Simulated mean monthly surface temperatures ...in the annual mean” is unclear (monthly or annual?).

Changed to “Simulated surface temperatures show a small cold bias of less than 1°C in the annual mean, ...” see P 9, L 18 – 19.

Add geographical coordinates for Yekaterinburg.

Done, see P 9, L16.

P 29272, L 27: It confuses me that GNIP stations are mentioned already here, but analysed only after the comparison with the satellite data.

At this position, Figure 1c is intended to illustrate the general $\delta^{18}\text{O}$ pattern and the large

observational gaps in Russia. The passage now reads (P 10, L 15 –16): “Figure 1c illustrates the few locations in Russia where $\delta^{18}\text{O}$ time series data are available. A rigorous model – data comparison of $\delta^{18}\text{O}$ will be presented further below.”

P 29273, L 5: I cannot see such a “patch of more depleted δD values”; there is just a zonal δD gradient.

Changed. The sentence now reads (P 10, L 22 – 23): “Even some details such as the regional δD gradient southwest of Kourouka Observatory are resolved.” (The gradient is not strictly zonal.)

P 29275, L 5: Which records are you referring to? Give a reference?

We refer to the West Siberian GNIP data shown in Fig. 5. The sentence now reads (P 12, L 26 – 28): “The sampling period of precipitation $\delta^{18}\text{O}$ in Russia is too short for a thorough investigation of the long-term variability seen in the West Siberian isotope and climate records shown in Fig. 5.”

P 29275, L 14: “atmospheric moisture content”

Done, see P 13, L 7.

P 29275, L 15: “maybe” is awkward; either it decreases or it does not

Changed to “precipitation over land has slightly decreased” (P 13, L 7 – 8). Please note that the global curves shown in Fig. 6 have been recalculated because ocean areas were not correctly eliminated in previous calculations.

P 29276, L 7: Is the trend in $\delta^{18}\text{O}$ statistically significant?

Yes. This is now mentioned in the manuscript (P 13, L 32 – P14, L 2): “The changes are small and at the detection limit ($< 1\text{‰}$ per 50 years) but everywhere statistically significant at the annual time scale.” as well as on P 29, Table 2.

P 29276, L 26: “temporal length scale” does not make sense

Changed to “precipitation is known to strongly vary at short spatial and temporal scales” (P 14, L 19).

P 29278, L 23: It is not really possible to infer about “moisture source regions” from monthly mean moisture flux vectors; I’d omit this term.

Done, see P 16, L 6 – 7: “... winter precipitation is also associated with enhanced moisture transport from the subtropical North Atlantic.”

P 29279, L 9: I do not understand how the nudging strategy could influence the $\delta^{18}\text{O}$ -NAO correlation, since the water isotopes are not directly affected by the nudging. Does one of the models have a weaker NAO than observed? Differences in timing of the NAO cycles should not

influence the correlation.

According to Casado's figures for temperatures (affected by nudging) unweighted by precipitation, the range of the winter NAO in LMDZiso is shorter than in ECHAM5-wiso and reanalysis data (CRU-NCEP, ERA-Interim). For this reason we have not changed our statement: “Regarding the results for unweighted temperatures as well as for $\delta^{18}\text{O}$ which Casado et al. (2013) obtained using the LMDZiso model, LMDZiso shows a weaker response to the winter NAO than ECHAM5-wiso.” (P 16, L 18 – 20).

P 29281, L 16: Could you give exemplary references on such investigations using d-excess and moisture tagging? In particular moisture tagging is a rather technical term.

Done (P 18, L 8 – 12).

P 29281, L 17: What is the difference between diurnal and daily?

Diurnal means sub-daily (in the course of a day), daily means from day to day.

Caption of Fig. 2: Mention the white squares.

Done (P 31, L11), we added “white squares are grid cells with low-quality observations.”

Caption of Fig. 11: What is meant by “average strong”?

This is now explained in the caption (P 32, L 27 – 28): “... anomalies of $\delta^{18}\text{O}$ in precipitation (colours) for (a) strong NAO+ (mean over all years with NAO index ≥ 2), and (b) weak NAO-conditions (mean over all years with NAO index ≤ -2).”