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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Variations of oxygen-18 in West Siberian precipitation during the last 50 yr" *by* M. Butzin et al.

Anonymous Referee #1

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In this paper, Butzin and co-authors explore a 51 year climate simulation with the isotope-enabled model ECHAM5-wiso, focusing on climate and water isotope variability in Siberia. After evaluating the model performance by comparing the results to different observational data sets, the authors use the simulation to assess the interannual to decadal variability of the oxygen-18 content of precipitation. In particular, they investigate the representativeness of a newly established measurement site for larger-scale variations in West Siberia as well as simulated correlations of precipitation d18O with near-surface temperature and the North Atlantic Oscillation. While the presentation of the results and the writing is mostly okay, in my opinion the scientific content of the manuscript is somewhat weak. Further details regarding this point are given in my major comment below. I think that before the paper can be accepted for publication in

ACP, major revisions are necessary.

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 asses the representativeness of the newly established observation site Kourovka, 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. Furthermore, there is an accompanying paper evaluating the same model with data from Kourovka 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. With respect to point (ii), the most important innovation of the station Kourovka 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? 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. The authors mainly focus on the correlation of d18O with local temperature. However, this correlation is not very useful in terms of climate change monitoring, since temperature can be observed directly (measuring temperature is much easier than measuring d18O). Also the correlation of d18O with the NAO is neither surprising nor very helpful in terms of monitoring: The NAO is known to influence temperature in western Siberia, and since temperature is correlated with d18O, there is also a correlation between d18O and the NAO. Of course, such correlations can be very useful in paleoclimatology, but this point is only briefly mentioned and not discussed in detail in the manuscript. 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.

Minor Comments:

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

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

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

P 29265, L 7: “amounts to”

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

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

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).

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. “Simulated mean monthly surface temperatures . . . in the annual mean” is unclear (monthly or annual?). Add geographical coordinates for Yekaterin-

burg.

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

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

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

P 29275, L 14: “atmospheric moisture content”

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

P 29276, L 7: Is the trend in d18O statistically significant?

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

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.

P 29279, L 9: I do not understand how the nudging strategy could influence the d18O-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.

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.

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

Caption of Fig. 2: Mention the white squares.

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

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29263, 2013.