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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Impact of snow deposition on major and trace element concentrations and fluxes in surface waters of Western Siberian Lowland" *by* Vladimir P. Shevchenko et al.

Anonymous Referee #1

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General Comments

Shevchenko et al. proposes to show the impact of the snow chemical composition during snowmelt in the lake and river water of the western Siberia, a region of lowlands, which is still poorly studied. They acquired a very representative and complete dataset of chemical element concentrations on the snow of this region. By assessing the chemistry of both dissolved (+colloidal) and particulate fractions of snow, they address the effect of latitude and anthropogenic sources on major and trace element concentrations in snow as well as the interaction between dissolved (+colloidal) and

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particulate snow fractions that are released in lake and river water of this region.

This manuscript corresponds to the scope of the journal, especially in presenting such a novel dataset in a permafrost area where geochemical and hydrological processes are still poorly studied. However, the submitted draft still needs significant improvement before to be published.

The presentation of the data should be done in a more synthetic way and not as a list of chemical elements, which change in each paragraph, making the reader completely lost after the first section of the result part. This comment should also be applied to the figures, which always present different chemical elements without any clear link between the different figures and also between one figure and its relative text. Keeping the reader interesting during all the manuscript when presenting such a large dataset is a real challenge and in my opinion this objective is not reached in this manuscript. Covering the large spectra of the ICP-MS analysis provides a more complete information on the snow, lake and river water composition but increases the difficulty to valorize the results with clarity, especially to an audience from various environmental disciplines. As the chemical elements do not behave equally in the different water type and in different locations this gives a difficulty to easily follow the description of the result, as they are presented, and the following discussions.

Proposing a working hypothesis could help to improve this presentation by given a clearer story-line to the manuscript. Then, the authors could choose a reduce number of elements to illustrate the defined hypothesis. Some parts of the discussions should be thoroughly addressed by improving the use of this amazing dataset. I see that the potential of the data is not fully exploited in regard to the discussions that are proposed in the manuscript.

Specific comments

Abstract

The abstract should be revised in order to clearly state the objective in regard to the existing knowledge on snow chemical composition at these latitudes. You should clearly explain the added value of this work to the international community.

Introduction

The introduction presents a clear state-of-the-Art on the knowledge of the chemical composition of snow under these latitudes but is lacking of a working hypothesis, which would be the directed line for the entire manuscript.

Study site, material and methods

This part is well structured and presents the important technical information to check the quality of this important dataset. You could nevertheless improve this section of the manuscript by removing some redundancies and merging some parts (citation of filtering in several parts of the methodology. . .). Calculation of EF could be added and explained according to recent literature in this part of the manuscript (see comment below).

Results

The results part is, in my opinion, the part that needs the main improvement. Using multivariate statistical analysis is a very good idea but is not fully exploited. Why not using the synthesis of such mathematical results to identify group of elements that behave in the same way and building the discussions on the elemental groups and not on the individual chemical element? But doing such an approach also requires to clearly stating the criteria that are used to identify the group of elements. I do not see the interest to propose such a classification in the present form of the manuscript and I do not understand the criteria that were used to define the different groups using this PCA analysis. For instance, how could the author justify that Cr and Zn are part of the same group that the other “lithogenic elements” (Fig. 3) and same question regarding elements of the second group. I think that a complementary hierarchical cluster

analysis should be done in order to clearly state the significance of a given element to be part of a given group that is shown in the PCA analysis of Fig. 3 and S5. (Kaufman and Rousseeuw, 2005. Finding Groups in Data. John Wiley and Sons Inc., NY). Such statistical technique has been widely used for drawing meaningful information from geochemical data (Bini et al., 2011 J. Geochemical Explor.; Levitan et al. 2015 J. Geochemical Explor.; Schot and van der Wal 1992 . Hydrol.; Gourdol et al, 2015, Appl. Geochem ; Moragues-Quiroga et al. 2017 Catena. . .). An interesting issue should be to compare the distribution of the elements in both snow fractions with the distribution of the same elements in the lake and river waters to observe some similarity/dissimilarity that could be explained by contrasted behaviour of elements after they are released from snow to river/lake water. Once the groups are significantly identified, you should replace the list of elements by the group number in order to clarify the results and discussion parts.

Some literature should be added to strengthen the similar behavior of elements that are present in a given group and then should behave in the same manner in the environmental fractions that are analyzed (snow, lake and river water, particles).

Using EF is always a debate, especially regarding the choice of the reference that has to be used for the normalization. How representative of the area geological characteristics is the chosen reference? How could this impact the results? Some publication should be considered to explain your choice like Reimann and de Caritat (2005 – Environ Sci Technol) and N'guessan et al. (2009 – Sci Tot Environ). This could be added to the methods by describing the EF calculation. As you used in the discussion soil/peat profiles collected in the WSL should you not relate EF to a more local geological/pedological information rather than an average Earth Crust?

Discussions

By filtering the snow and the lake/river water to 0.45μm, the dissolved fraction includes a colloidal load, which can play a crucial role in the concentration of trace elements.

Then, the <0.45μm dissolved fraction of the snow water (which may not include colloids, see lines 364-370) is compared to the <0.45μm dissolved fraction of river water (in which the trace element concentrations can be heavily controlled by colloids). If the colloids are not present in the dissolved fraction of snow, it is known that their contribution can be important in river, especially during flooding periods. Then the environmental compartment, which releases the colloids to the freshwater during the smelting period is not considered in the fluxes calculation of the present study and could overestimate the contribution of the dissolved snow fraction to freshwater fluxes. This point needs to be clarified in the discussions.

The part that discussed the anthropogenic impact is like a list of potential industrial and urban sources but no real proof regarding the exact origin of the analyzed snow particles is given. The use of elemental ratios could be interesting to use especially with such a dataset to relate with known and existing anthropogenic/natural sources in the investigated region.

Detailed comments

Line 39: ". . . of properties. . ."

Line 68: as the separation between dissolved and particulate fraction is <0.45μm, the dissolved one actually corresponds to a truly dissolved fraction associated to colloids. This colloidal load may be of importance in freshwater according to hydrological conditions and should be indicated in this manuscript when the dissolved fraction is described, especially if snow water is compared to lake and river water compositions. I propose to use dissolved + colloidal fraction

Line 76: correction "unprecedently"

Line 91: in contradiction to the discussion part on industrial impact on the snow particles

Line 137: indicate Rare Earth Element (REE)

Line 146: dissolved and colloidal

Line 286: what about Ga, Co and Cl, which present similar values in Fig. S4 than the element cited in the text?

Line 298: Mg and Ca are not plotted in Fig. 6.

Line 356: replace “lieu” by “origin or source. . .”

Line 362: please define LREE

L365: I agree regarding the marine aerosols but what about the atmospheric deposition that come from anthropogenic or lithogenic origins? Could they be enriched in Fe- or organic-colloids?

Line 377-379: “soil column” and “peat column” might not be the appropriate term, you could use “soil profile” and “peat profile” or “soil” and “peat”.

Line 379: remove the symbol between “mosses” and “collected”

Lines 398-399: Is this not an important conclusion of this study? This should appears in the abstract and the conclusions. Figures A harmonization of the chemical elements presented in the different figures should be done for clarity improvement in the manuscript. Check the correspondence between the figures and the text: Ca and Mg do not appear in Figure 6. . . Figure 1: the different permafrost zones should be indicated Figure 3: caption should include more information on the parameterization of the PCA that was used. Figure S4: same scale should be used for the Y-axis of the three graphs.

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