Author Response, MS No. essd-2012-1

I am grateful for the detailed feedback from the 3 reviewers.

Reviewer 1, J.C. Yde

This paper presents supraglacial snow and ice chemistry data from four glaciers in different Alpine settings. The dataset is valuable for a number of reasons and worthy of publication in ESSD. For instance, for me as a glacial hydrochemist there is a need for supraglacial snow and ice chemistry data to interpret the chemical signal in glacial bulk meltwater. The documentation, presentation and focus could be improved at places, and I have a few suggestions that might be helpful to the author. Overall, I enjoyed reading the paper and found the dataset interesting.

Specific comments:

1. Page 109, lines 6-13: This part of the Introduction is a bit unclear. I suggest that you rewrite and expand this section to give the reader a more thorough introduction to the topic. First, it should be emphasized that the focus here is the glacier surface, not the 'glacier geochemical composition' in all ice facies (e.g., basal debris-rich ice is likely to have a different composition than supraglacial ice). It will also be appropriate to give a short overview of supraglacial snow-ice dynamics, such as the firnification process, preferential ion leaching from the snowpack and formation of superimposed ice. Then, an explanation of supraglacial dust and debris entrainment processes would be informative. In particular, it would be relevant to mention mass movement processes such as avalanches and rock-falls and glacial processes such as medial moraine appearance, marginal debris transport along shear planes (thrusting) and the potential role of ice crystallography in trapping wind-blown dust particles (see references in Goodsell et al., 2002, page 288). I guess this organization will make it easier for the reader to follow the discussions of geochemistry in the Background section and of dust and debris provenance in the remaining part of the paper.

I am thankful for this advice. The introduction and background was restructured, the above points were included in rewriting this section.

2. Page 113, lines 10-24: More documentation on sample collection is needed. Were the snow samples collected at surface or at intervals in a snow pit profile? Was it fresh snow or an old surface? What about ice lenses? Were the ice samples collected at the glacier surface or was the top layer removed before sample collection? Were the ice samples snow-derived or formed by refrozen meltwater (superimposed ice; basal ice)? Did the samples contain visible cryoconite?

More details were provided in the sample collection section. Snow, ice and debris samples were all collected at the surface. Further details on surface processes were included as known.

3. Page 115, lines 10-16: A significant limitation of this dataset is the lack of Cl measurements in determining the importance of sea-salt derived solutes. In Svalbard the snow chemistry composition in five pits on nearby Longyearbreen showed that sea-salt deposition was the most important process, but dust deposition was detected due to relatively high Ca and SO4 concentrations (Yde et al., 2008). At Grønfjordbreen the basal ice contains a high Cl concentration, supporting the hypothesis of a significant input of sea-salt deposits (Yde et al., 2012). Both at Longyearbreen and Grønfjordbreen the SO4 concentrations in glacier meltwater are very high compared to global standards, indicating that S is derived from a reactive source, which most likely is the local S-rich coal (Yde et al., 2008, 2012). Also, at both Grønfjordbreen and Aldegondabreen cryogenic calcite precipitates are formed in the proglacial and subglacial environments (Yde et al., 2012), and it could be interesting to examine whether a similar process occurs in the supraglacial environment.

I am thankful for this expert local knowledge of the Svalbard environment. Further studies would certainly aim to measure Cl and SO4 as stated, and supraglacial processes. The focus of this paper is to publish the first-order comparative data set of the four distinct regions.

Minor comments:

4. P. 108, l. 19-20: This reference to data access should also be given in the main text (e.g., at the beginning of section 4). *Agree, done.*

5. P. 109, l. 11-13: The influence of anthropogenic processes is often related to elements such as C and N. *Thank you for this comment.*

6. P. 111, l. 17: Grønfjordbreen and Aldegondabreen are valley glaciers, not cirque glaciers. *Thank you for this manuscript oversight! Corrected.*

7. P. 111, l. 24: Insert a sentence on the local geology (see references in Yde et al., 2012). *Agree, done.*

8. P. 112, l. 6: Insert a sentence on the local geology (see Matthews et al., 1979). *Agree, done.*

9. P. 115, l. 8-14: Explain in more detail how these seasonal patterns are demonstrated. Could you make a figure that visualizes seasonal changes in composition? *Agree that the study does not provide enough data to start making seasonal analysis. Therefore, seasonal comments were removed from the manuscript.*

10. P. 116, l. 6-7: Why do Al and Ti suggest sublimation and leaching? *Al and Ti are relatively insoluble elements (see Fig 1) which are not released by melt waters, and tend to stay at the surface during melt processes (see Kreutz and Sholkovitz, 2000).*

11. P. 118, l. 15: Why was Ti chosen as the reference element? *Ti was chosen due to the ability to measure this element precisely, and the relative insolubility of the element. Therefore, Ti is a wise choice in this study for use as a reference element.*

12. P. 121, l. 1-16: This part on total elemental abundance is significantly hampered by the lack of determinations of important elements such as C, N and Cl. I suggest that section 5.5 is deleted. *Agree, Section 5.5 was deleted as suggested.*

13. P. 121, l. 17-P. 122, l. 8: The section on air trajectories is superfluous and does not lead anywhere. I recommend that section 5.6 and Fig. 6 are deleted. *Agree. Section 5.6, Fig 6 deleted as suggested.*

14. P. 124, l. 17-20: Delete these two superfluous sentences. *Agree, deleted.*

15. Table 1. This table should only show the physical characteristics. Therefore, delete all notes on influences in column 2. *Column 2 notes deleted.*

16. Figure 2. Delete the superfluous pictures. *Pictures kept to demonstrate environment of sample collection areas.*

To sum, all above minor comments were addressed as noted.

Reviewer 2, Anonymous

In general this manuscript presents a new set of data that has not been published based on my knowledge. The data set consists of 70 samples from 4 locations, targeting at 45 elements or components and 10 oxide compound abundances. The author aims to report geochemical compositions based on supraglacial dust debris results on glaciers, which are encouraged to publish.

Broad range abundance comparison of trace elements provides a wide scope of difference of those elements in different regions in both S and N hemispheres. However, the manuscript is poorly organized and some necessary information is missing while many sections throughout the manuscript are repeating, including tables and figures as well as text expression. Some of the descriptions or explanations are not pertinent. Wording is quite informal, impertinent to journal's format. Not enough information to evaluate the data quality based on the current version as there is no information on in-situ sampling and tools used for generating the data set. This manuscript is not ready for publishing. A major revision is recommended.

I am thankful for Reviewer 2's detailed comments. I have expended significant energy in restructuring the manuscript and improving readability. I am thankful that the data set is acknowledged for its value in representing supraglacial geochemical compositions at the four diverse glacier study sites.

Major concerns (not limited):

Redundant, not well organized/presented. For example, Table 1 and Chapter 3, instrument and reagent information were not provided precisely. Notations are complicated, which makes readers hard to follow. *The structure of the manuscript was overhauled to improve study readability. Attention was paid to improving clarity of tables and figures, to simplify notations as suggested.*

Not enough details how those samples were collected even though there is a section on Sample Collection (4.1): for example, how did the sampling personnel take the samples? What tools were used especially for ice samples? If the field personnel wore a clean jacket? *The Sample Collection section was rewritten with significantly more details on the heritage of the procedures followed when acquiring surface ice, snow and debris samples. Tools were outlined, and in situ sampling techniques noted.*

Quotations from literatures are not precise. In some cases, literature conclusions sound from this study. Some of the explanations in Results and discussion are not convincible. For example, L10 to 16 on page 115, results from two locations (Gronfjordbreen and Aldegondabreen) and different seasons are compared. My questions here are: 1. When soluble elements are washed away during melting, why not K? If this is due to imbedding particles, explain it. Also, why does not the similar pattern happen to S. Norway samples? 2. If washing effect is suitable to your data set, why does it conflict of this explanation between Svalbard (washed away when melting) and New Zealand (concentrated in ice samples when melting)? 3. Concentrations of insoluble elements in Ce samples are often much higher than their counterparts in snow samples, up to 100x times. If this is due to melting, it suggests 100x times concentrated. How does this happen? 4. 10x times higher Pb concentrations were with the Nepal snow samples while 10x lower in the Norway snow samples compared to the same site ice samples. How do you explain the washing effect? *Seasonal conclusions were removed from the manuscript as I came to realize that not enough data was collected to speculate on such processes. The concentration differences in Nepal and New Zealand were further detailed. The late dry non-monsoon season in Nepal is speculated to be the reason for the high particulate concentrations in snow - where as ice samples were derived in some cases from refrozen meltwater (explaining this concentration difference). Similarly, New Zealand surface snow also experiences high particulate loads from continual Crater Lake volcanic aerosol emissions, as well as dust from surrounding dry terrain.*

Sections 5.5 and 5.6 are weak. *Agree. These sections were removed from the manuscript.*

Both chapters 5 and 6 contain discussion. They should be combined. *Agree. I am thankful for this oversight. The repetitive discussion was removed in the restructuring of the manuscript.*

Conclusion contains significant explanation and description that should be avoided. *Agree. The conclusion was rewritten, repetitive explanation and descriptions were avoided.*

Minor (a few examples only):

Table 1: no explanations for your notations such as 1a, 1b, 2a: : : Move those in Table 2 to Table 1 and then note it in Table 2. Also, based on the row alignment of this table, 8 snow samples were taken in April 2009 while 2 ice samples, in July 2009 for Svalbard sites. This is understandable. However, Bodalsbreen site, there is no sampling date for the 4 ice samples. If they were taken also on June, say so. Reading the original data set suggests me that Table 1 was not organized in order but arbitrarily. Duplications for same information in Table 1 and Chapter 3 (Geochemical influences to the study areas). *Agree. Date of southern Norway ice samples clarified - 24-27 June 2009. Duplications in Table 1 were deleted (namely, column 2, sample id's in column 5).*

Line 3 to 5, P111: the wording reads vague. It sounds to me that the author did the meteoric solubility of the elements measurements. *Agree, reworded.*

Line 1, P114: Thermal Finnigan Element 2 or XR? Give the instrument detail. Line 4, P114: Romil SA or SpA or UpA? Give what you really used? Line 12, P114: What is the blank value subtracted? And what is the blank range? Table 2. Why are there data missing for 4b and 4c samples? Were you analyzing the samples in a different batch? Explain why? *All instrument and solution and analysis details were given in as complete detail as the author was aware.*

Table 5. Site separation line (vertical line) for New Zealand is misplaced. *Thank you for pointing this out, corrected.*

Fig. 1. Not accomplished in this study and can be removed. *Figure left in as it was created for this study to depicts the solubility of the elements chosen for use in this study. The figure is referenced several times in manuscript descriptions, and improves understanding of the audience with regard to why elemental abundances were observed.*

Fig. 2. photos can be removed. *Pictures kept to demonstrate environment of sample collection areas.*

Fig. 5. Inconsistent presentation in the figures. Also, for S. Norway, you have 2a as well. Table 7 and Fig. 5 are actually equal as the author stated. Repeating presentation is not necessary. *Agreed. Figure 5 was removed.*

5.1 Line 8, P115 and Table 2: seasonal patterns and comparison between the two sites in Svalbard: You need demonstrate how you conclude seasonal variations. From my reading, you seem do not have enough data supporting this conclusion. *Agreed that there is not enough data within this study to derive seasonal conclusions. Therefore, seasonal conclusion statements were removed from the manuscript.*

Reviewer 3, Anonymous

The manuscript by K.A. Casey describes a new data set about measurements of geochemical components in dust and debris taken at four different glaciers. These glaciers are located in

Svalbard,southern Norway, Nepal, and New Zealand and should therefore represent four different glacier types with the following characteristics.

• The Svalbard glaciers are located in a mild polar climate with high rates precipitation in close proximity to the ocean, where marine aerosols are emitted.

• The Norway temperate and marine glaciers also receive high accumulations rates but are located “in” the downdraft path from industrialized regions of the northern European Union.

• The Nepal glaciers are the highest elevated glaciers in this study and are subject to a continental climate with enhanced summer accumulation. In contrast to the others these glaciers receive also a considerable amount of dust and debris load.

• The New Zealand glaciers at the outer flanks of the stratovolcano Mt. Ruapehu are subject to high precipitation rates in a temperate marine climate and to aerosols from the adjacent volcano and acidic crater lake.

The manuscripts informs after the introduction about the characteristics of the four glaciers and its environment. Afterward the utilized data collection protocol and the analysis technique are represented. The main part of the manuscripts are the descriptions of the abundances of the numerous analyzed major, trace, and rare earth elements in the ice and snow probes and the debris samples. Some of the measured concentrations are related to signatures of the surrounding environmental conditions, such as the aerosol contributions from the neighboring ocean, trace abundances of the upper continental crust, or anthropogenic pollutants transported by atmospheric circulations to the glaciers. The discussion and conclusions close the manuscript. The manuscript's structure needs to be improved. In addition it would immensely benefit from some professional language editing.

The selected journal “Earth System Science Data” aims to foster the publication of reference data of benefit to the earths system science community. Since no data set of such quality is to my knowledge freely available, *I recommend the publications of the manuscript after minor revisions*.

I am thankful for the careful review and feedback from the 3rd Reviewer. The paper was restructured with a great effort toward improving the focus, clarity, readability and presentation of the study.

Minor comments about the text

In an enumeration within the text, I personally prefer a comma in front of the final “and”. But I guess there are other views out there about this particular point. I have not collect all problems in language and grammar. Therefore the following list might not be complete. I strongly recommend to double-check grammar and phrasing. Also the structure need to be streamlined. *Agreed. I am thankful for*

this feedback. A thorough restructuring was performed on the manuscript. Rewriting focused on proper grammar and phrasing.

Since reference data might not only be of interest for a selected group dealing with the specific problems related to undertake the measurements, I would strongly suggest to introduce even common abbreviations in the field like LHREE. *Agreed. Abbreviations were defined (e.g. Fig 1, REE, section 5.4 LREE, HREE).*

Specific comments

In the following text, the given page and line numbers refer to the printer-friendly version of the manuscript.

Page 108, Line 21-23: In the abstract ablation add satellite relevance is from my point of view quite vague. Would you consider to delete these lines? Please comment this point. *Agreed, lines deleted.*

Page 108, Line 25-26: I disagree about the strong statement, that the factor of increase glacier melt is primarily driven by the lowered albedo. I admit, it might be a strong contributor in some regions, but I think that some glaciers in polar regions, where due to polar amplification the temperature rises stronger than the global average, the related higher longwave radiation might by of high importance. Therefore I would suggest at least a more cautious phrasing like: "There is indication that one of the primarily factor …. " *Agreed, such powerful statements were reworded for increased accuracy, less speculation and more caution.*

Page 109, Line 22-24: What is the meaning of this sentence "In this study were collected from … glaciers speculated to maximize … composition diversity."? *This awkward phrasing was removed. Simply meant to state the samples are expected to provide surface glacier geochemical composition data which reflects the inherent geochemical diversity of the chosen glacier study regions.*

Page 111, Line 23: " … to the ocean as well as a local … source." Would you like to replace it against " … to the ocean that act as a local emission source." ? *Agreed, done.*

Page 112, Line 22: "... upper outer flanks and in the summit". Do you need the "and"? *No, deleted.*

Page 113, Line 2: "such" do you need this word here? *No, deleted.*

Page 113, Line 11, 12, 13: I miss some verbs in the list describing the handling of the bottles. *Okay, section rewritten.*

Page 114, Line 4: measurements instead of measurement. *Corrected.*

Page 114, Line 9-10: Please clarify for what the standard spike was utilized, for example by phrasing :"An internal standard spike of … was utilized to … ." *Corrected.*

Page 114, Line 24/28: Are the devices fully described by “Philips XPERT diffractometer” and “Philips PW2400 XRF”? Might you be able to cite references about these particular devises describing their abilities and limitations. *Further instrument details and references provided.*

Page 115 and Page 116: I would like to see separate paragraphs for each for the described glacier and its elements distributions, such as Page 115, Line 14: … element glaciochemistry. (New paragraph) Jostedalsbreen … Page 115, Line 21: … as well as slight marine influence (Pacyna and Pacyna, 2001). (New paragraph) Khumbu Himalayan … Page 116, Line 2: … concentrations (e.g. Kang et al., 2007). (New paragraph) Mt. Ruapehu winter … *Thank you for this helpful comment. Agreed, implemented.*

Page 116, Line 22- 24: Might you consider to replace the brackets against a different type of brackets or dashes to guide more clearly the reader, since your might get lost to find immediately the begin and end of your thought placed in the brackets. *Agreed, implemented.*

Page 117, Line 15-17: What would be needed to confirm/falsify the assumed relation between negative Zr anomalies and incomplete nitric acid digestion? Would you please clarify this point. *When acidic digestion does not run to completion, elements such as Si and Zr can indicate the lack of dissolution. References are given for further reading toward incomplete nitric acid digestion in silica ICMPS studies (e.g. Kollensperger, G., S. Hann,G. Stingeder Determination of Rh, Pd and Pt in environmental silica containing matrices: capabilities and limitations of ICP-SFMS, J. Anal. At. Spect. 2000, 15, 1553-1557.)*

Page 118, Line 23: I do not completely understand what is meant by “ for all previous elements”. Do your might mean “for all remaining elements”? *Sentence reworded to improve clarity.*

Page 119, Line 6: Might you add to the name Ngozumpa also its region, so that the sentence is “ … was found in Ngozumpa debris (Himalaja)”, because the jumping between the different regions, glacier names makes it sometimes hard to follow the text. *Sure, regions included in addition to glacier names to ease understanding for readers.*

Page 120, Line 21: I might have missed it, but please introduce both acronyms LREE and HREE. *Revised as suggested.*

Page 121, Line 14-17: “The more silica-rich … absorbs less solar radiation than the basaltic, silicapoor debris … .” What point do you want to make here? Please clarify. *This section was deleted, as suggested by the previous two reviewers.*

Page 121. Line 24-26: “In each region, 5-day transport paths were mapped … meteorological inputdata.” Which period does these estimates represent? Please clarify. In addition the related figure 6 suggests, that only the last month of the probe collection period has been used to determine the trajectories. Since glaciers also accumulate during the remaining period, I suggest to determine a probability density function (PDF) of trajectories that covers the climatological state in the year (years) before the measurements have been collected. This would give a more reliable picture of the different source contributions. Since you seem to be mostly interested in the ablation season you might weight differently the various seasons that go into the PDF. *This section was deleted, as suggested by the*

previous two reviewers. However, I am very thankful for your comments as to how to strengthen these results. This will be considered for further work in this area.

Page 121-122, Paragraph 5.6: Supraglacial nutrient observations on Svalbard [Hodson et al., 2009] suggest that sporadic inflow of pollutant-rich air masses might contribute significantly to observed pollutant concentrations. Do you account for this sporadic events that would also influence your reported concentrations? Are their also indications of such events for the other regions? *Sporadic events are acknowledged (e.g. volcanic events) in influencing glacier surface geochemical compositions. This first-order data set is limited temporally. Sporadic events could be covered in much more detail in future studies with greater temporal sampling capabilities.*

Page 122, Line 12-15. "Increased atmospheric deposition of dust ….. increase snow and ice mass loss, and ultimately contribution (better: contribute) to sea level rise." It sounds like a clearly proven chain of processes that have a high significance. I agree in the assumptions but phrase them more cautious, such as "Increased atmospheric … might amplify snow and ice melt rates, which increases snow and ice loss, and ultimately contributes to a faster rising sea level" *Agreed. Reworded to reduce speculation and provide more cautious, accurate representation of dust contribution to ice mass loss.*

Page 122, Line 26-27: The sentence about ongoing improvements is an obvious statement. I do not think that it is necessary? Please consider to delete these lines with the exception you complete you thought about the consequences that goes beyond this general statement. *Agreed. Deleted as suggested.*

Page 123, Line 7-20 (last paragraph of discussion): I find this last paragraph a little bit vague. I will certainly gain power if you please the final conclusion (first sentence) at the end. *Thank you for this helpful comment, implemented as suggested.*

Figures

Fig. 1, Caption: Please decipher REE printed in the figure legend in the figure caption. *Agreed, REE defined in figure caption.*

Fig. 2, I personally like to see in photos how these areas look like. So I have a more clear picture about their characteristics. This is in particular important for people you might use your data and perform purely numerical model studies and have never seen such an environment. *Thank you for this comment. Agreed that the pictures add descriptive proof of sample sites. Figure 2 pictures kept in.*

Fig. 2-4, Caption: I would prefer to either place for example "a)", "b)", and "c)" in the figure panels and/or describe their figure position more clearly like "upper left" /"top left", "upper right", and "lower". *Figure labeling altered as recommended.*

Fig. 3: I've not seen the reason for the data gaps neither in the text nor the caption. Please clarify this point. At least you might phrase that data gaps are depicted as gaps in the lines or refer to the corresponding table. Fig. 4: Gaps are related to .. Please clarify or mention at least the existence of the gaps. *Stated non-measurement and below detection limit reason for data gaps again in Figure captions.*

Fig. 5: In the three figure panels, the slightly different color for identical symbols per plot make it hard to identify the groups immediately. Would you consider to use either more distinct colors, different symbols for the four glacier groups or place vertical "bold" lines that separate these groups in the plots. *Duplicate Fig 5 (to Table 7) was removed as recommended by previous 2 reviewers for manuscript streamlining.*

Tables

Tab. 5-7: Might you be able to use a different symbol for below detection limit and missing data, such for example "0" and "-", respectively. *Thank you for the suggestion, the two data types are now differentiated.*