Answers to reviewer 1:

We thank the reviewer for the extensive comments and suggestions to the paper. Based on the reviewer comments, we have significantly restructured and revised the paper. We will not provide a detailed summary of these structural revisions below, but hope to be allowed to submit a revised version of the paper.

A point-to-point reply to the reviewer's comments are provided below (in blue), along with a short description of the adjustments to the paper relating to these comments.

The manuscript is poorly written, too long with several repetition redundant, several contradictions between the same paragraph or other paragraphs, the data and the result are inaccurate present (see example tephra layers).

The manuscript has been thoroughly restructured and revised, and redundancies have been removed.

We do not agree with the reviewer that we contradict ourselves, or that the data and result have been inaccurately presented, and we believe that this rely on misunderstandings. We hope that the reviewer agrees upon reading the revised version of the manuscript.

The methods chapter are not well structured with several information reported two three times and not in the appropriate chapter as result and discussion. Most of the information about the methods is reported in the supplementary material, where are more clearly presented. The manuscript must be completely revised and shortening significantly.

The manuscript has been thoroughly restructured and revised, and its content has been rewritten more concisely. Since the reviewers requested several expansions of the manuscript, the final version of the paper is, however, about the same length as the original version.

In the revised version of the manuscript, we have e.g. moved the section on timescale validation by comparison to WAIS Divide from "Method" to "Results". We believe that with this structural change, it should also become clearer that the RICE17 timescale is NOT synchronized to WAIS Divide, and that our matching of the two cores is only a basis for comparing the two timescales.

Some references are uncorrected or mismatched.

We have gone through all references and corrected these.

Five accompanied papers of RICE core are submitted or in preparation, but their result are used to validate or as source of the result of the manuscript (ex. Lee et al., in preparation).

It is correct that the Lee et al paper on methane matching of the RICE and WAIS Divide ice cores has not yet been submitted. We hope that this paper will be submitted very soon and will then be accessible as a discussion paper in Climate of the Past Discussions. In the present manuscript, we have revised the wording of the section on methane matching to improve its readability as a stand-alone text.

All other papers have been submitted and/or published now.

To make this manuscript a significant contribution to the literature, the authors need to better justify their time scale and snow accumulation records.

We hope that the reviewer will approve of the revised version.

Clarify the use of the WAIS volcanic signal and methane with RICE17 chronology, in the text look like that is use as synchronisation (see 3.3.1.3), but several point is stressed that the accuracy is low and it is use only at posteriori as validation. All the process of comparison between RICE and WAIS must be clarify, it is repeated several time in different way.

The RICE17 timescale is NOT synchronized to WAIS Divide.

We believe that part of the misunderstanding may be due to us inappropriately using the word "volcanic synchronization", where it more correctly should have been called "volcanic matching". This has been corrected in the new version of the manuscript.

If the two records are synchronised by volcanic the age error must be the same closer the tie points, between one tie point to other can increase. The process must be revised.

Since the two ice cores have not been synchronized (although they have been matched), the ages of the volcanic markers are not expected to be identical in the RICE and WAIS Divide records.

The tephra layers where used to fix the chronology, but it is not reported the analysis of tephra particles (Raboul 1964 CE and Pleaide 1252 CE) and the analysis on WAIS ice record (up to now never published on my knowledge), that can be permit an unequivocal attribution.

Results from geochemical analysis of the Pleiades tephra layers have been made available from the AntT database (http://antt.tephrochronology.org/I.html?id=AntT-15, http://antt.tephrochronology.org/I.html?id=AntT-16), which are now referenced in the text. Geochemistry of the Pleiades tephra horizon in the RICE and WAIS Divide ice cores is reported and discussed in Kalteyer (2015) and Dunbar et al (2010), which have now been included in the references:

Kalteyer, D.A., 2015. *Tephra in Antarctic Ice Cores*. Master Thesis, University of Maine. Available at: http://digitalcommons.library.umaine.edu/etd/2381/.

Dunbar, N.W., Kurbatov, A.V., Koffman, B.G., Kreutz, K.J., 2010. Tephra Record of Local and Distal Volcanism in the WAIS Divide Ice Core. 2010 WAIS Divide Science Meeting September 30-October 1, La Jolla, CA.

The 1965CE (Raoul) tephra layer from RICE and WAIS Divide is available from the Interdisciplinary Earth Data Alliance (IEDA) database (e.g. https://app.geosamples.org/sample/igsn/IESDW0026 and https://app.geosamples.org/sample/igsn/IESDW0016) with data reference Kurbatov (2015).

Results from the tephra analysis will be reported on in forthcoming publications, which we now refer to (see also comments below).

The explanation because nssSO4 signal or acidity peak of major eruption reconnaissance in WAIS (Tambora, Unknow etc.) are not recorded in the RICE records is questionable, but Authors have attributed as unknown more than hundreds chemistry signal to volcanic eruption (123 event Table 2) and those are not observed in WAIS or others ice core in Ross Sea (Siple Dome, Taylor Dome, Talos Dome). Why RICE records is able to record 193 volcanic event, with all the problems pointed out in paragraph 3.3?

We observe many small acidity peaks in the RICE records, which we in the previous draft related to volcanic events, despite these not being correlated to volcanic events in other ice cores. As the reviewer correctly points out, there is, however, a risk that we in Table 2 included acidity peaks derived from other events, e.g. extreme biogenic emission events.

For the new version of the manuscript, we have gone through the volcanic signatures in RICE and their matching to WAIS Divide, and have made the following changes:

- We established a new conductivity-to-Ca excess depth profile, directly calculated from the two CFA records. Previously, we only compared the two records visually, and refrained from calculating their differences, due to issues related to e.g. slight differences in depth assignment of the two records. However, as it turned out, having a directly calculated record of the non-sea-salt conductivity greatly simplified the volcanic matching between the two cores.

- With help of the new depth profile of non-sea-salt conductivity, we were able to match most of the prominent acidity peaks in WAIS Divide to volcanic peaks in RICE. The majority of the signals that were found in both cores corresponds to bipolar volcanic signals. This strengthens our trust in the reliability of the volcanic matching, since these are expected to deposit acidic material over an extended period, and therefore be most easily recognizable from the RICE records.

- The RICE acidity record still contains a large number of acid peaks that do not have a counterpart in WAIS Divide, but we refrain from stating that all these originate from volcanic events. Table 2 now only includes the acidity peaks that could be matched to a corresponding sulfate peak in WAIS Divide (65 acidity match points).

We consider the new volcanic matching to be very robust, and the main difference from the previous matching is that we have removed matches that we do not consider to be completely certain. However, the new depth profile of the conductivity-to-Ca excess allowed us to also identify a few new match points between RICE and WAIS Divide.

The text regarding the volcanic matching of the two cores has been revised accordingly.

Black Carbon, on the base of figure 4 does not appear the best proxies of seasonal signal, H+ appear more conservative and less misleading of BC

During development of the RICE17 timescale, we used all available proxies with annual signal, including black carbon and H+ (P. 9, line 26-27). Using multiple proxies will give us the most accurate timescale, as both records includes non-annual features that make annual layer interpretation based on a single record questionable.

Authors report strong gradient in snow accumulation spatially ranged from 0.09 to 0.30 m we/yr and migration of the dome from 500 to 900 m. Can the Authors exclude any impact on the snow accumulation history due to migration of the dome ? and/or on thinning function?

Over the last 2700 years, the Roosevelt Island ice divide has migrated only 500m from its present position (P13, L41), not 900m. We account for the migration of the ice divide in our derivation of the thinning function for the ice core, in that we vary the vertical velocity profile through time (P14, L11-20).

As suggested, we have added to the manuscript a brief discussion on how the migration of the ice divide could influence the obtained accumulation rate history:

The recent period (~1500-1750 CE) of divide migration at Roosevelt Island may impact interpretation of the climate records from the RICE core. Present accumulation rates across Roosevelt Island show a distinct decrease on the downwind (western) side of the ice divide, with a gradient of ~0.5 cm/km yr^{-1}, although the trend is muted around the summit area. Ice recovered in the deeper part of the RICE core, deposited before divide migration, have originated west of the ice divide. Assuming a stable snowfall pattern through time relative to the divide, its migration would have caused reduced accumulation rates to be observed during the early part (until 1500 CE) of the RICE accumulation history. With an origin of the ice recovered in RICE of up to 500m west of the divide at time of deposition, our estimates of Roosevelt Island accumulation rates during this early period would therefore have a small negative bias of up to 0.25 cm/yr.

Correcting for the influence of ice divide migration, the main impact on the Roosevelt Island accumulation history is an earlier onset of the period with more rapid decrease in accumulation rates. The differences are small, however, and the overall pattern of trends in accumulation rate through time remains the same. In particular, ice divide migration has no impact on accumulation rate trends observed before and after the migration period.

As mentioned, the impact on the accumulation history is small, and ice divide migration would have no impact on e.g. recent trends in accumulation rates.

Paragraph 5.3 "Current mass balance. . .. "does not report any new valuable information for the mass balance of the RIS

We have deleted this section from the manuscript.

P. 3, line 44-47: How could explained stable ice divide flow with a migration of the ice divide position of around 500-900m?

First of all, the ice divide has migrated only 500m, and this migration took place over a period of a few hundred years (roughly 1750-1500CE). During periods before and after, i.e. during the majority of the period, the ice divide flow was stable. We have accounted for the change in ice divide location over time in our modelling of the thinning function (P. 14, line 11-20).

We have revised the text to clarify this:

To account for the changes in vertical strain rates at the drill site over time, we assumed the following divide-migration history, informed by the architecture of the Raymond stack (Fig. 6b): Until 500 years before ice core drilling (1512 CE), the divide was located 500 m east of the present position, as indicated by the position of the deeper Raymond arches. Since 500 years ago, the divide migrated westward, reaching its current position approximately 250 years ago (1762 CE), where it has since been stable.

P.4, line 33-34: RICE would be representative of East Ross Sea, not of Victoria Land, see accompanied RICE paper (Bertler et al, submitted)

We have removed this sentence from the paper.

Chapter methods: This part is too long and inappropriate as method chapter and most of the text must be moved to result chapter.

As suggested by the reviewer, we have significantly restructured the paper.

The content of the methodology chapter is now split up into three parts: “Ice core processing and impurity analysis”, “Constructing the Roosevelt Island Ice Core Chronology, RICE17, for the last 2700 years”, and “Reconstructing past accumulation rates”. We further have removed the section on comparison of the RICE17 timescale to WD2014 to the Results. We hope that this restructuring has made the paper more easily accessible.

The does not provide information about the sample resolution along the core and the analysis performed and at which resolution ice (cm) and sample per year.

Sample resolution of the various records is provided in Table 1. Further, as the CFA records are measured continuously, a discussion of their effective depth resolution is made on P.5, line 47 – P.6, line 5. We have made a slight addition to this paragraph to clarify the distinction between the two, so that it now reads:

The CFA chemistry records are very densely sampled (1 data point per mm). Mixing in the tubing, however, as the meltwater sample travels from melthead to the analytical systems caused individual measurements to be correlated, and hence the effective depth resolution of the system is significantly less than the sampling resolution. This was especially the case for the RICE CFA set-up due to the relatively small fraction of total meltwater directed to the continuous measurement systems. Following the technique used in Bigler et al. (2011), we estimate the effective depth resolution for the CFA measurements to range from 0.8 cm (for conductivity) to 2.4 cm (for calcium) (see supplementary Table S1).

The layer thickness is decreasing with depth, causing the number of independent samples per year to also change with depth (and varying between the individual chemical species). We therefore prefer to refrain from providing a general value for the number of samples per year in the manuscript.

Instead, in the section “Overview of the annual-layer counting strategy”, we now mention the number of independent data points per year in the best resolved record in the very deepest part of the layer-counted timescale:

At this depth, the annual layers are too thin (<6 cm, i.e. less than 8 independent data points/year in the best resolved records) for reliable layer identification in data produced by the RICE CFA set-up.

Percentage of missing record of CFA example must be reported and show.

We have added the following paragraph to the paper (P. 5, line 46):

Core breaks and/or contamination in the system caused some sections of missing data. The percentage of affected core varied between chemistry species, ranging from <1% (BC) to 15% (H^+), the majority being small sections of missing data that did not severely impact annual layer interpretation of the records.

Most of the information about methods is relegate in the supplementary info.

We are not sure whether the reviewer would like us to move more of the material in these three paragraphs into the supplementary, or if he/she would like us to include some of the supplementary material into the paper itself.

In our endeavor to shorten the paper, we have decided to keep the division between the two more or less as is. We have transferred some of the technicalities on the StratiCounter set-up to the supplementary.

However, we have included into the main paper the discussion of the annual layer signals observed in the various CFA records, as we consider these to be of general interest.

P. 6, line 21-24: This paragraph present contraindiction in several points, along the entire text, correlation between RICE and WAIS "volcanic event" are used or not to tune the RICE scale? Ex. See line 29-30 of pag 6

Apart from the top 42m (where the timescale can be constrained by historical events), the RICE17 timescale is a fully independent layer-counted timescale, i.e. it has not been tuned to the WAIS Divide timescale. The correlation to WAIS volcanic events is only used for subsequent comparison of the timescale to WD2014. We recognize that this essential aspect was not sufficiently clear from the manuscript, and, as previously mentioned, we have made several revisions to the paper for clarification, including the following:

- We have moved the comparison of the RICE17 timescale to WD2014 to the results section.
- A few instances of incorrect use of the word "synchronizing" has been removed, and the word "tiepoints" has been replaced with "matchpoints".

We have further revised the section "Overview of the annual-layer counting strategy" on page 6, so that it now reads as follows:

The uppermost section (0-42.34 m) of the core was dated by manual identification of annual layers in records of water isotopes and ice impurities from the RICE main core as well as the RICE-12/13B shallow core. For this most recent period, several distinct marker horizons from well-known historical events were used to constrain the chronology.

Below 42.34m (1885 CE), the timescale was augmented using the StratiCounter layer-counting algorithm (Winstrup et al. 2012) applied to multiple CFA impurity records from the RICE main core. A previously-dated tephra layer at 165 m (Pleiades; 1251.6±2 CE according to WD2014) was used to optimize the algorithm settings, but other than that, RICE17 is a fully independent layer-counted ice-core chronology.

Page 6, line 24: The Raoul tephra is a unequivocal volcanic event or not?

The Raoul tephra is an unequivocal tephra horizon, but as it is located above 42m it is not mentioned here. In the revised version of this paragraph (see above), we have removed the reference to unequivocal volcanic events.

Page 6, line 31-32: The layer counting stops at 343.72, because the annual layer is to fine (<6cm), to identify seasonal signal needs at least 10-12 sample per year, a graph showing the number of sample analysed per year must be show, from the surface to the 344m

As the CFA measurements are made continuously on the melt water stream, the notion of the number of samples per year is not a straight-forward measure for such data sets. Further, due to different amount of mixing in the various chemistry melt-water lines in the CFA set-up, the effective depth resolution differs between data sets. Instead of showing these in a graph, we have elected to mention the number of data points per year in the best resolved record in the very deepest part of the layer-counted timescale (P.6, line 31-32), as also mentioned above.

In the bottom part, we have less than 8 independent data points/year in the best resolved records. We note that this number is less than ~10-12 independent samples per year, mentioned by the reviewer to be required for annual layer identification. This lower number is somewhat counteracted by the near-continuity of the CFA records, which was employed on a depth scale with ~1mm resolution (i.e. resulting in 60 correlated samples/year in the deepest part of the timescale).

Page 6, line 44-46: The record of overlap section must be shown to see the ratio noise/signal in the two cores

As the reviewer also requests a significant shortening of the paper, we have elected not to extend the manuscript with such figure.

Page 7, line 10: Several other records also displayed annual variability, but much less reliability" Why do use BC instead of H+ or both

This is a misunderstanding. For all depth intervals, annual layer interpretation in the RICE17 chronology is based on the complete set of available chemistry and isotope records, as also mentioned in the section "Overview of the annual-layer counting strategy".

Our intention with the remark on page 7, line 10, was simply to note that some data series displayed a more reliable annual signal than others. In the deeper part, BC is the record with the most reliable annual signal, partly because of the high resolution of the record. Yet, also here annual layer counting is based on all CFA records, including H+.

Page 7, line 11-16: The peak of proxies seasonality are quite different in time (isotope versus sea ice proxies, or photolysis), and most depend from the occurrence of snow fall. The use of ERA model does not look appropriate and the reference is still not published

We observe from the data records that water isotopes and acidity signals peak simultaneously in most years (Figure 4), which we interpret as the maximum annual temperature and lowest sea ice extent taking place approximately at the same time. We have assigned the depths of these peaks to correspond to January 1st. Seasonal variations in snowfall will influence the precise depth location of the peaks in the various records, but it will do so similarly for all records.

In the text, we have shortened and reorganized the section, and removed the reference to ERA-interim data, so that the paragraph now reads:

Summers could be identified as periods with high stable isotope ratios, high concentrations of nss-SO_4^{2-} *and associated acidity [originating from phytoplankton activity in the surrounding ocean during summer (Legrand et al. 1991; Udisti et al. 1998)], and low iodine concentrations [due to summertime photolysis of iodine in the snowpack (Frieß et al. 2009; Spolaor et al. 2014)]. Layer marks were placed according to the depths of concurrent summer peaks in water isotope ratios, nss-*SO_4^{2-} *concentrations, and acidity levels, and assigned a nominal date of January 1st.*

Page 8, line 17-21: Geochemical composition of the tephra at RICE-WAIS and source must be show before any attribution of a tephra never reported in Antarctica before (Raoul 1964)

Results from the geochemical analysis of the Raoul tephra from RICE and WAIS Divide is available from the IEDA database, with data reference Kurbatov et al 2015 (which unfortunately had dropped out of the previous version of the manuscript):

Kurbatov, A. V. et al., 2015. Major element analyses of visible tephra layers in the Roosevelt Island Climate Evolution Project ice core (Antarctica). *Interdisciplinary Earth Data Alliance (IEDA)*. Available at: https://app.geosamples.org/sample/igsn/IESDW0025

The attribution to Raoul will be reported in a paper currently in preparation.

We have added these references to the text:

A couple of volcanic horizons in RICE during this most recent part could be unambiguously related to well-known volcanic eruptions. Rhyolitic tephra located between 18.1-18.2m (Kurbatov et al. 2015) was found to have a similar geochemical composition as a tephra layer found in the WAIS Divide core, with a depth corresponding to late 1964 CE. The tephra likely originates from Raoul Island, New Zealand (Wheatley et al, in prep), which erupted from November 1964 to April 1965. This is consistent with the RICE17 chronology, according to which the tephra is located in early 1965 CE (Table 2).

Page 8, line 21-36: It is not clear why some sulphate deposition is attributed to eruption and others no, and correlated to WAIS

For this most recent part of the timescale, for which historical volcanic eruptions constrained the timescale, we used all available unambiguous volcanic horizons. Apart from the Raoul tephra, however, we were only able to identify two volcanic horizons during this time interval: Santa Maria and Krakatau. We note that the ages of these volcanic horizons are constrained from historical records, and not because of synchronization to the WAIS Divide core, but agree with the reviewer that this was not clear from the previous version of the manuscript.

We have shortened this section, removed the references to WAIS Divide ages, and revised the wording to clarify these aspects. It now reads:

Only two volcanic eruptions could be unambiguously identified in the acidity records over this period, namely the historical eruptions of Santa Maria (1902 CE; 37.45m) and Krakatau (1883 CE; 42.34m). These two horizons were used to constrain the deeper part of the manually-counted interval of RICE17, which terminates at the Krakatau acidity peak (Table 2). Deposition age of volcanic material for these events was assumed identical to observed in the WAIS Divide ice core (Sigl et al. 2013). Imprints from other large volcanic eruptions taking place during recent historical time, such as Agung and Pinatubo, did not manifest themselves sufficiently in the RICE records to be confidently identified.

Page 8: Analysis of comparison between manual and automated annual counting must be performed and show

The manual counting performed was only a rough preliminary counting, with the sole purpose of producing a set of templates for the annual layers in the chemistry records, as required for initialization of StratiCounter. We therefore refrain from performing an analysis between the manual counts and the automated layer counts in the paper.

We have revised the wording in this section to ensure that the preliminary nature of the manual layer counts is better conveyed to the reader:

StratiCounter was initialized based on a rough set of manual layer annotations in a short section of the data (40-150m). The manual annotations were used to produce a template for an annual layer in the various impurity records. To increase the independence of the StratiCounter timescale from the preliminary manual interpretation, in a final step the entire timescale was reevaluated using an improved set of layer templates derived from the output of the algorithm itself.

In order to shorten the paper, these and other details regarding how the StratiCounter software was run has been moved to the Supplementary.

Page 9, line 14-15: BC is used to date the 90% of the core analyzed, but on base of fig 4, is not the best proxies of seasonality, also as reported by Author pag 7 line 10. At line 26-27 is reported different use of the proxies the Authors contradict themselves

This is a misunderstanding. We always use all available proxies for the dating (see e.g. P9, L25-27), see also our answer to the question reg. P7, L10.

We do not have IC or ICP-MS measurements (of e.g. S, as shown in figure 4) in sufficiently high resolution that these can be used for identifying layers below 40m. Fortunately, in the deeper part of the core, the annual signal in BC is better than for the top part, and, due to its high effective resolution, it maintains a good annual signal with depth.

We have now included a discussion on the annual layer signal in the various chemistry series in the main paper, along with figure S2 (now: figure 7), which shows the evolution in annual signal with depth of the various CFA species. See an updated version of the figure below.

The reason for the misunderstanding may lie in the paragraph on P9, L15-16, where we mention that we use the peak in BC for the annual markers. However, this is simply a matter of where the layer marks are placed, and it does not imply that BC is the only chemistry series used to identify the layers. To avoid misunderstandings, we have removed this sentence from the revised version of the paper.

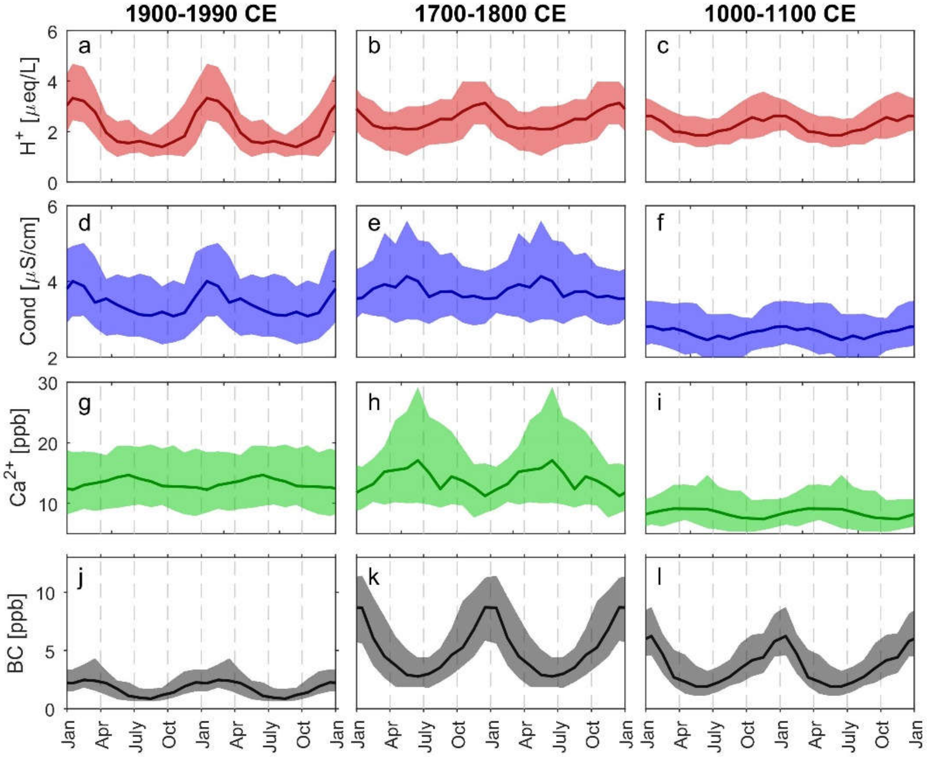

*Figure 7: Average annual signals of 2 successive years in **a-c)** RICE acidity (H^+), **d-f)** conductivity (Cond), **g-i)** calcium (Ca^{2+}), and **j-l)** black carbon (BC) over three 100-year periods, calculated under the assumption of constant snowfall through the year. The line shows monthly-averaged median value of measured concentrations, and colored area signifies the 50% quantile envelope of the value distribution.*

Page 9, line 39-40: The geochemistry is not show; Pleiades volcano is not West Antarctica, but in Northern Victoria Land; Kurbatov et al, 2015 is not reported in reference, and it is not present in any database as reference for tephra layer reported; the 1252 tephra attributed to The Pleiade was iscovered the first time at Talos Dome and dated at 1254+-2 by Narcisi et al, 2001; on my knowledge the analysis of the tephra at WAIS is still not published

We have changed “West Antarctica” to “Northern Victoria Land”.

Kurbatov (2015) has been added as reference, see also previous comments.

We have elected to shorten the discussion involving tephra in the manuscript, as this will be the focus of forthcoming publications. We have further moved the discussion about the Pleiades tephra layer to the Results, as it was primarily used during the matching of RICE to the WAIS Divide ice core. We have revised the text, and added additional references for the geochemistry analyses of the tephra.

Results from geochemical analysis of the Pleiades tephra layers have been made available from the AntT database (http://antt.tephrochronology.org/I.html?id=AntT-15, http://antt.tephrochronology.org/I.html?id=AntT-16), which are now referenced in the text. Geochemistry of the Pleiades tephra horizon in the RICE and WAIS Divide ice cores is reported and discussed in Kalteyer (2015) and Dunbar et al (2010), which have now been included in the references.

We also mention that tephra of similar composition has been found in Talos Dome (Narcisi et al, 2001), and TALDICE (Narcisi et al, 2012).

The section now reads:

A visible tephra layer was found in RICE at 165m depth, with a RICE17 age of 1251.5±13 CE. Geochemistry of the tephra particles is consistent with an eruption from the Pleiades (Kalteyer 2015), a volcanic group located in Northern Victoria Land, Antarctica (Fig. 1). Tephra of similar geochemistry has been found in several other Antarctic cores dated to approximately the same time, including WAIS Divide (1251.6±2 CE; Dunbar et al. 2010) and Talos Dome/TALDICE (1254±2 CE; Narcisi, Proposito, and Frezzotti 2001; Narcisi et al. 2012). The Pleiades tephra horizon allowed a firm volcanic matching of the RICE and WAIS Divide ice cores at this depth (Fig. 8).

Page 10, line 5-18: RICE site is farer than other cores (WAIS-Byrd-Siple Dome and Talos Dome) from "many active volcanoes", it is very difficult to understand why RICE record is able to identify volcanic eruptions those are not identified in the ice cores of the region with much lower ratio in noise/signature due to marine biogenic sulphate emissions

This is a good point. In the previous version of the manuscript, we had included several minor acidity peaks. There is, however, a risk that we had included acidity peaks of non-volcanic origin, e.g. extreme biogenic emission events. In the new version of the manuscript, we only include RICE acidity peaks that could be matched to the WAIS Divide volcanic record.

We note that one reason why we might observe more regional volcanoes in the RICE core is our use of acidity records to identify volcanic eruptions. By using acidity rather than sulfate, we might better observe the signals from regional eruptions, as discussed on P. 11, L1-9. This section has been moved to the discussion.

Page 10, line 19-25: Methane gas synchronization less precise than volcano matching, after 4 line "but methane it is better than volcanic matching", clarify

As described on P10, L5-31, the two methods are complementary: Methane matching is less precise (i.e. relative age differences is much better resolved using volcanic matching), but it provides better absolute age control, since there is less risk of misalignment of the records. The text has been revised to clarify this aspect.

Paragraph 3.3.1.1: The use of acidity and ECM to detect volcanic signal is not a new tools. Hammer have used H+ in 1980 as proxies of volcanic signal

Agreed. However, the volcanic record here is based on direct measurements of H+ in the ice core, whereas H+ used by Hammer (1980) was estimated from the ECM signal. Further, the volcanic proxy developed based on non-sea-salt conductivity is new. We have changed the title of the paragraph to "New and traditional ice-core tracers for volcanic activity" to reflect that it is not new to use ECM to detect volcanic signals.

Page 10, line 43: A resolution of 9.5 cm (about 4 sample per yr) is very low to observe the seasonal variation, but enough to detect the important volcanic signal like Tambora, Kauve etc present a signal for 2-3 yr in Antarctic cores (from 8 to 12 sample)

We agree with the reviewer that with this resolution, we would expect to be able to see some signal in discretely-sampled S record from large bipolar volcanoes that deposit material over several years. However, we observe from the retrieved sulfate records that it is usually very hard to distinguish volcanic eruptions in these. This is likely due to the large inter-annual variability in biogenic sulfate influx, shading the volcanic signatures. The volcanic eruptions are easier to identify from higher-resolution records, such as acidity.

We have added the following to the paper:

Resolution of the discretely-sampled sulfur record was too low (below 67m: 5 cm, i.e. less than 4 samples/year), and even large volcanoes only left a vague imprint in form of slightly increased sulfur levels over a multi-year period (Fig. 8a). Detection of volcanic horizons in the RICE core therefore primarily relied on two new high-resolution tracers for volcanic activity; direct measurements of total acidity and estimated non-sea-salt liquid conductivity.

To illustrate our point, we have added the sulfur records to figure 7 (now: figure 8), see below, so that the reader can verify our statements. We note that exactly this section is actually where the sulfur record displays the most distinct volcanic signal.

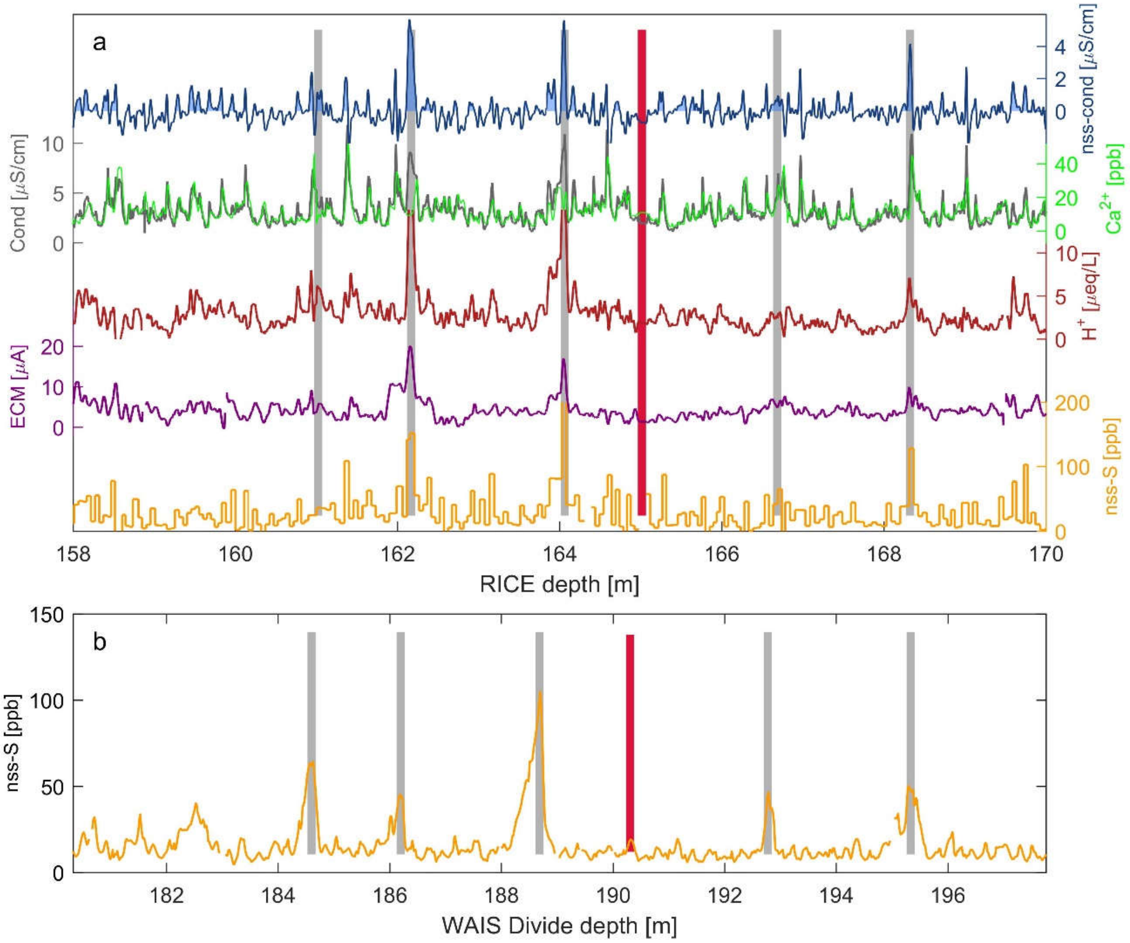

*Figure 8: **a)** The RICE volcanic proxy records: non-sea-salt-sulfur (nss-S; orange), ECM (purple), acidity (H^+; red), and non-sea-salt conductivity (nss-cond; blue) based on the conductivity-to-calcium excess (grey and green). **b)** Matching of the RICE records to the WAIS Divide non-sea-salt sulfur record (Sigl et al. 2015). Vertical bars indicate volcanic match points (Table 2), with the red bar being the Pleiades tephra horizon (1251 CE).*

Page 10, line 25-32: On the base of figure 7a and Table 2 the volcanic events identified in RICE at 158.15m and 160.77m do not present any clear evidence in ECM, H+, due to the high background of ssSO4 at RICE. It is very difficult to understand why a site with this high noise/signal ratio could record local eruption sulphate does not observed at other site core.

We agree that these two volcanic eruptions are hard to identify from the figure. During the process of re-evaluating the volcanic matching between RICE and WAIS Divide, we have removed the volcanic event at 158.15m, which was not matched up to a volcanic horizon in WAIS Divide. In addition, we removed the volcanic event at 159.5m, as we did not consider this horizon to be a sufficiently clear-cut match to WAIS Divide, when considering all available volcanic proxies.

We have added to figure 7 (now: figure 8, see previous comment) the newly developed non-sea-salt conductivity record, which facilitates identification of volcanic eruptions from the difference between the conductivity and calcium records.

At 160.77m, the non-sea-salt conductivity record has predominantly positive values over a significant section, indicative of a volcanic event. This is backed up by significant, albeit small, peaks in the other volcanic proxies. We note that the age differences between this and surrounding volcanic events are identical to those obtained from matching this peak to the significant non-sea-salt sulfur peak in WAIS Divide at 184.5m. We hence consider this peak to be well-qualified as match point between the two cores.

We note that we do not believe that RICE records volcanic sulfate peaks not observed at other sites (such as WAIS Divide). However, by identifying volcanic eruptions based on the acidity rather than sulfate, we may be able to better identify regional eruptions, which also deposit other acids than sulfuric acids.

Paragraph 3.3.1.2 The Pleiades horizon is discussed in several part of the manuscript (parag 3.2.1, 3.2.4, and 3.3.1.2), but without provide any evidence on the base of geochemistry analysis. This tephra layer was reported for the first time in Talos Dome 1996 ice core and dated by Narcisi et al, 2001, at 1254+-2 and attributed as source to Melbourne Volcanic Providence, probably "The Pleiades", located about 250km from Talos Dome. This tephra was than identified in Siple Dome and Taylor Dome by Dunbar et al, 2003. Moreover, Narcisi et al, 2012 pointed out that at TALDICE (a core drilled from 2004 to 2007) is present the tephra of 1254 as TD87a (86.2m depth) close in composition to the previous found in the ice core on 1996. However a subordinate set of glass shards (TD87b) is also trachytic but with a chemical signature inconsistent with The Pleiades products. Mount Berlin could thus be a suitable source of ash (Narcisi et al, 2012). Moreover an other tephra layer TD85 at 84.37m depth younger than 25 yr has been reported by Narcisi et al, 2012 and the suggest source is Mt Melbourne volcanic province. Without any geochemical analysis is impossible attribute unequivocally the tephra found in RICE"

We now discuss the tephra horizon attributed to the Pleiades only in a single paragraph in the manuscript. In this paragraph, we now also mention that it has previously been found in other ice cores in the region, and provide additional references, see our reply to previous comments.

We prefer to not go into the tephra geochemistry in details here, as this will be the topic of forthcoming publications. As previously mentioned, geochemical analysis of this tephra layer is now published in the AntT database.

Page 11, line 41-42: If the tephra layer identified is 1252+-2 yr, why use 1252+-13 for this horizon in RIC17?

RICE17 is an independent timescale, and thus the uncertainty on the age of this tephra horizon reflects the uncertainty in layer counting in the RICE records.

Page 12, line 9-18: See above, more than 170 volcanic event most of them never see in closer core

This number has been reduced, see our previous comments.

Page 12, line 19-42: Volcanic event and Methane records are used for Synchronization or validation?

They are only used for validation, see our previous comments.

Page 13, line 9-10: Which is the gas-age at RICE? And compared to closer site as Siple and WAIS?

For the period discussed in the paper, Δage values for RICE is 145-171 years. This is slightly less than Δage values for the WAIS Divide ice core (174-206 years), and somewhat smaller than those for Siple Dome (233-255 years). TALDICE has significantly larger values of Δage.

Page 13, line 24-29: What is the source of surface temperature of -22oC? Why is used this instead of 27.4oC, this value is also proposed in accompanied paper of Bertler et al, submitted. Why do you use a warmer temperature of 5.4oC? Which implication on density and thinning model using 5.4oC warmer?

We thank the reviewer for pointing out this inconsistency regarding the current surface temperature at RICE. The -27.4^{o}C temperature is derived from ERA-interim data, obtained for a location slightly south of the Roosevelt Island. It seems that there is a large temperature difference between this location and the RICE drill site, with borehole temperatures and AWS data from the RICE site showing an average temperature of -23.5^{o}C. This difference in mean temperature based on the two methods is also discussed in Bertler et al. 2018 (published version). In the new version of the manuscript, we refrain from mentioning the too-cold ERA-interim temperatures.

As the surface temperature of -23.5^{o}C was always used for the density and Δage modelling, these are correctly calculated in the manuscript.

Page 13, line 30-40: Kingslake et al, 2014, instead of Raymond

We have moved the Raymond 1983 reference to immediately after the phrase “Raymond arches”.

Page 13, line 40-41: The recent migration of ice divide, could be attributed to change in snow accumulation variability at ice divide? Due to the snow accumulation variability between the flank of the ice divide, which is the influence have on snow accumulation record and thinning function of RICE core? Kingslake et al., 2014 report that near-surface strain rates are compressive at ice divide than in the flanks 90% higher at RICE. The migration of the ice divide respect to Raymond Bump position indicate a role of temporally changing in spatial snow accumulation distribution, as well as the role of along-ridge flow is un-clear and hampers a solid interpretation about thinning function and snow accumulation records

Neither the high frequency (shallow, <100m) or low frequency (deep, >50m) profiles suggest a significant change in accumulation pattern over time that could have driven the divide migration. Therefore, we believe the divide migration is likely caused by ice dynamics, potentially caused by changes in buttressing by the surrounding Ross Ice Shelf. The relatively small changes in divide position over time, however, suggest that neither has changed significantly over the past 2700 years. Since the reviewer has recommended a significant shortening of the manuscript, we have elected not to expand the discussion of the thinning function.

The applied thinning function takes account for the divide migration (see previous comments).

It is correct that the large gradient in accumulation rates across the ice divide could influence the obtained accumulation record from the ice core, and as previously mentioned, we now estimate this in the paper. Since the divide has only migrated a short distance, this does not significantly impact the derived trends in the accumulation rates.

Fig 8: The pRES measurement (Kingslake et al., 2014) was at ice divide and does not follow the Raymond bump features as reported in figure 8b

Correct. That is why we vary the vertical velocity profile through time (P14, L11-20).

Page 14, line 11-20: On the base of which data the Authors construct a vertical velocity profile along the Raymond Bump?

As stated, we are assuming that the ice in the core was not directly beneath the divide from 700BCE to 1450CE. The vertical velocity profile is allowed to change over time, based on a linear combination of the measured vertical velocity profiles from the flank and the topographic divide.

In the revised paper, this section has been clarified.

Page 15, line 14: "Control point… of atmospheric oxygen isotope" at page 12 "Given the stability of the d18Oatm record over the last millennia, the synchronization was solely constrained by the observed variability in the methane records" as in several other part of the text none coherence exist between the paragraphs and some times also in the same paragraph"

We used both $\delta^{18}O_{atm}$ and CH_4 for matching the two cores. However, given that the levels of $\delta^{18}O_{atm}$ were very stable over the last 3000 years, using this record did not provide many constraints to the synchronization. Essentially, therefore, the matching was based on the methane records. We have added the following to the text to make this clearer:

The feature matching routine employed discretely-measured records of methane as well as isotopic composition of molecular oxygen ($\delta^{18}O_{atm}$). Over recent millennia, however, the $\delta^{18}O_{atm}$ concentrations are stable, and hence provided minimal matching constraints.

Page 15: Along all the paragraph it is not clear the process of adjustments of the counting layer respect to matching between RICE and WAIS

RICE17 is an independent timescale, and hence we do not adjust the number of counted layers between match-points to fit the WD2014 timescale.

Page 16, line 13-16: High internal-annual variability in snow accumulation is normal issue (see eg Eisen et al, 2008 and reference within), 1.3% is very low value

We have removed this section from the manuscript.

Page 16, line 40-45: The three accumulation record of snow accumulation must be shown in the overlap time, the correlation coefficient of 0.85 and 0.87 indicate that the RICE annual are representative, but at pluriannual scale (see Eisen et al., 2008; Frezzotti et al., 2007). The comparison of the three cores can confirm only the stability of snow in the overlap time, not at secular or millennium scale

Agreed. However, while we can only say for certain that the accumulation is spatially consistent within the time period covered by these three cores, we still believe that the consistency between these records in the overlap period provides a basis for our general statement that the strong correlation between these indicates spatial consistency in accumulation rates.

We have added to the paper a figure comparing the accumulation records obtained from the three cores during their overlap period, see below. The three records show very similar inter-annual variability.

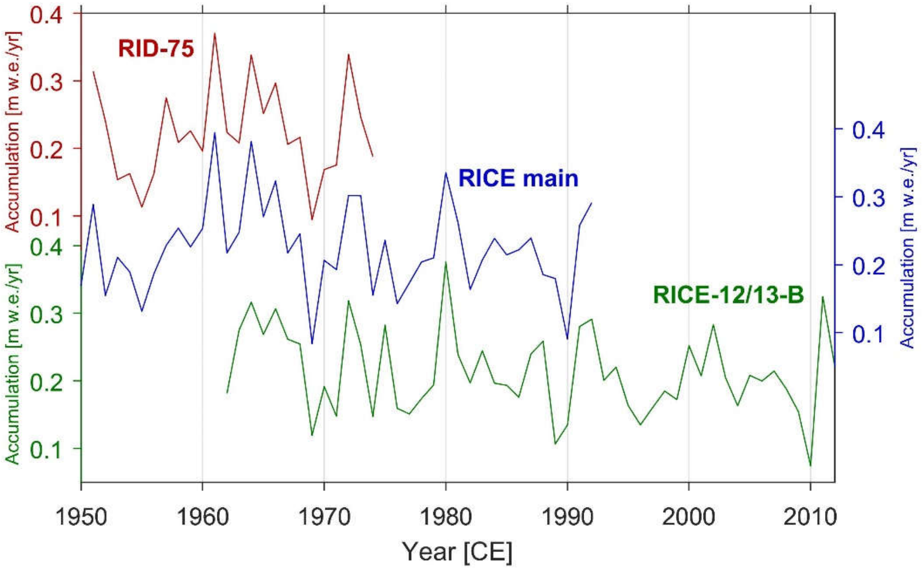

Figure 12: *Accumulation reconstructions for the three Roosevelt Island ice cores.*

Paragraph 4.3.2: "The inflection point in accumulation of fig 9 with a trend in decrease is closer to age of the hypothesis of the stabilization of the ice divide at present position 1450 EC (Pag 14). The uncertainty of change in accumulation must be taking in account also the spatial variability at ice divide. The topography position of ice divide is probably linked also to spatial variability of snow accumulation in a feedback mechanism (see Drews et al., 2013; King et al., 2004; Matsuoka et al., 2015; Lenaerts et al., 2014).

Firstly, we note that there was a small error in the previous version of the draft regarding our inferred timing of stabilization of the ice divide at its present location – we expect the migration to start 500 years ago, i.e. in 1512CE, not in 1450CE, and arrive to its present location 250 years ago (1762CE).

With these new ages, there is an even more significant difference in timing between the onset of a negative trend in the accumulation rates (1250CE) and the onset of divide migration (1512CE). As previously mentioned, the migration of the ice divide would give rise to a small negative bias in derived accumulation rates for the first part of the period. However, it does not significant impact the derived trends in the accumulation history.

Given the different timing as well as the small effect on the accumulation rates, we do not believe that migration of the ice divide has been the main cause behind the observed changes in accumulation rate over time.

The uncertainty due to age scale and thinning function and ice divide migration must be tacking in account when is analysed the trend, uncertainties is not small amounts

We note that the uncertainty associated with the change in ice divide migration (~0.25cm/yr) is small compared to the uncertainty in thinning function (~2 cm). Similarly, the small (correlated) uncertainties in the age scale do not significantly impact trends in the derived accumulation rates (P. 17, L10-15).

We have taken the approach to estimate uncertainty on the trend estimates based on a linear regression to the most likely accumulation rate history. We note that this is much more reasonable now that the uncertainties on the thinning function have been significantly reduced. Due to the high variability of the accumulation rate over time, we believe that this approach provides reasonable estimates for the uncertainty in observed trends.

Page 17, line 36-46: The interpretation of the reason of trend in accumulation differ from that hypothesis reported by Bertler et al., submitted paper

We now mention that the recent decline in accumulation at Roosevelt Island may be due to increased sea ice extent in the Eastern Ross Sea, as put forward by Bertler et al (2018).

Page 18, line 1-4: The decrease of 6.6 cm/yr per century is not agree since 1950 with the paragraph 5.3 "Clausen et al. (1979) estimated the current (1954-1975) accumulation rate at the summit of Roosevelt Island to be 0.20 m w.e yr-1, whereas we here find the current accumulation rate (average of the last 50 years) to be 0.22±0.06 m w.e yr-

The value of 6.6cm/century is derived from the RICE accumulation rate history, which shows a distinct decrease in recent accumulation rates. We agree that this may appear contradictory to the old cores finding slightly smaller accumulation rates than the present value obtained from RICE. The difference between accumulation rates in the two cores, however, is caused by the spatial variability in accumulation rates, with the RID75 core being drilled at a location with slightly lower accumulation rates. The two statements are therefore not contradictory.

Paragraph 5.1: Most of points reported are repetitions already pointed out in Methods and Result, see above for the comment, in particular for "we noted several strong volcanic imprints that seemingly have no counterpart in the WAIS Divide ice core data, and thus most likely originate from local West Antarctic volcanoes.

We have significantly reorganized the paper, so that we now in Discussion discuss the implications of using total acidity instead of sulfate, and how it impacts the volcanic record from RICE.

Page 18, Line 31-44: The dipole effect change during the time, see Bertler paper, are this occurs in correspondence with presence or absence of RICE-WAIS volcanic event synchronization?

It would be interesting to investigate the effect directly from a comparison of the two volcanic records, but it not possible to say from our matching between the two ice cores. Given the general challenge of establishing volcanic march-points in the cores, the identification of these partly depended on the density of reliable matches in the surrounding core sections. In other words, with a higher density of reliable match points, the more additional acidity peaks were sufficiently convincing to be annotated as a match-point. This positive feedback loop significantly influenced the frequency of identified match-points, overwhelming any effect caused by the dipole strength.

Page 18, line 38: "Absence of sulfate in RICE" with a higher background of 200 ng/g, exactly the opposite

We have changed this to:

Absence of volcanic signal in the RICE core

Page 19, line 1-7: The tephra number of RICE is not unusual as presence compared to TALDICE or west Antarctic core, as the Authors have written few line after. Moreover, RICE present "several strong volcanic imprints that seemingly have no counterpart in the WAIS Divide ice core data, and thus most likely originate from local West Antarctic volcanoes.", but not tephra, this is very unusual if the volcanic event reported in Table 2 are true

We have removed the sentence about the number of visible tephra layers in RICE.

Page 19, line 3: "Only one exists within the last 2700 yr", but on the base of manuscript the tephra are two: Raoul 1964 and Pleiade 1252

Correct. This paragraph has been removed.

Page 19, line 22: "longer-term trends are significantly different between the two locations" the text after describe similar trend with higher accumulation in the past respect to the present and change trend close at secular scale

We agree that this is not formulated very clearly. The text has been rewritten to clarify that while part of the long-term trends differ at the two locations (increasing at RICE until 1250CE, decreasing at WAIS Divide), the two records also show some similarities. For instance, the decline in WAIS accelerates around the same time that RICE accumulation rates starts to decline.

Paragraph 5.3: The result of RICE does not provide new information for the mass balance of RIS, taking in account the previous cores with similar SMB value and the high spatial variability of the rise and RIS

We have deleted this paragraph from the paper.