To the Reviewer,

Thank you very warmly for your thorough and well worded review of this manuscript. We found your comments and suggestions both insightful and extremely helpful. When writing this manuscript our intention was not to propose controversial mechanisms or feedbacks, but to provide a more indepth and mathematically robust treatment of the periodicities of observed proxy records. The mechanisms presented in this paper are not original, our own, or new – and it was our goal to link, through the causal synchronicity of the periodicities, the many seemingly disparate mechanisms presented in other literature. As an aside, we found that a more rigorous treatment of the time-series data, presented arguments for why the solar-forcing mechanisms may be fundamentally flawed over this time period.

As such, we had purposefully limited our discussion of the methodology and development of the many proxies and feedbacks (such as how the volcanic sulfate signal is generated, and how MOC is affected by freshwater influx) to a more cursory treatment.

While a strict presentation of the frequency analysis, with limited mechanistic interpretation, remains our goal, your feedback has made it clear that certain aspects of the discussion need to be expanded. We hope that with these additions the future reader is able to question less the source of the data, and focus more on the correlation of the observed periodicities.

We have also tried to "tone down" our emphasis on linking the specific glacial-atmosphere-volcanic feedback mechanism.

Thank you once again for your time,

A. Flinders.

The reviewers general comments are a summary of specific issues, later commented on line by line. We have addressed all of the reviewers specific comments, and where applicable have listed which specific comments addresses the general comments listed below.

General Comments

Comment #1:

"You deduce enhanced global volcanism preceding regional warming from a Greenland ice core. This site is strongly biased to record Icelandic and Northern Hemisphere eruptions, many of which are not stratospheric, and have limited global climate effects. A 45,000 yr record from Dome C (Castellano, 2004) indicates no increase of activity from tropical eruptions (which are supposed to have strong climate effects). Dating uncertainties of the radiocarbon dated volcanoes from the database are large, and independent from the GISP2 timescale with its one dating uncertainties (although never discussed in this manuscript), hampering any comparison between those series relative to the onset of the short lived DO events."

Response:

I disagree with your statement that "***Castellano, 2004) indicates no increase of activity from tropical eruptions"***. Castellano (2004), "Volcanic eruption frequency over the last 45 ky as recorded in Epica-Dome C ice core (East Antarctica) and its relationship with climatic changes" makes NO reference to the locality of the eruptions. They do state that they believe the sulphate glacio-chemical profiles provide a reliable proxy for volcanic activity. While they conclude that they see no "clear link between number of [volcanic] events recorder and climactic changes", they go onto state their study did show, in some case "the presence of intense volcanic signatures during times of rapid climactic change" (Pg196, 2nd Column Paragraph 1). We see a discrepancy and mistreatment in their assumed relation between forced climactic change and the number of volcanic events, opposed to the strength of the event, given for example by the Volcanic Explosivity Index, VEI, or the total anomalous sulphate concentration over a event wise time period (several years). Most importantly, they do not provide a rigid methodology or treatment of the frequency analysis – only calculating mean number of events over millenial scale periods. There repeated use of the term "frequency" is a complete misnomer.

For a discussion on the relation to tropical influence and dating uncertainties please refer to **Response 7, 13, 17**.

Comment #2:

"Extracting volcanic sulfate fluxes, frequencies and concentrations from a Greenland ice core is not straightforward given the changes in past accumulation rates, terrestrial dust load and the high concentrations of Ca2SO4 inherent in atmospheric dust at Greenland. All these parameters vary strongly during stadials and interstadials. Therefore the intensity of laser light scattering, 10Be, and volcanic sulfate show a lot of co-variability. According to the references, the volcano sulfate record is derived by methods of dimension reductions from total particle mass or volume determined with a particle counter. I would assume that even in the EOF representing the volcano variability a bias towards higher values during the cold interval might exist. Anyway, details about the analytical measurement and volcanic sulfate calculation should be investigated by the author and provided to the reader as this parameter is essential to the entire study and micro-particulate volcanic sulfate in Greenland is for sure not a straightforward parameter like d18O."

Response:

We thank you for your input, and see now that it is imperative to give a more thorough background of the derived sulfate signal, and we have included this in the updated M.S. For a discussion on this specific topic please refer to **Response 5**.

Comment #3:

"There are dating uncertainties in all time series. Unless you compare d18O and sulfate from the same ice core, these dating uncertainties need to be addressed when you discuss temporal variations. For gases you need to account additional for uncertainties of the gas-age / ice-age difference."

Response:

The strength of a frequency analysis approach is that issues arising with dating uncertainties are mitigated. There is a low relative uncertainty between the dating uncertainties and the periodicities calculated, allowing for a high degree in confidence of the observed frequencies and hence described correlations. For a discussion on this specific topic please refer to **Response 9, 12, 16**.

Comment #4:

"There are entire sections without citation, where the reader cannot know if you are describing scientific knowledge or your own interpretation, especially the links to MOC and NADW formation."

Response:

I would disagree that "there are entire sections without citation", there are sentences where citations have been left out due to a continued flow of thought extending from a leading sentence—with the appropriate citation—through to the ending sentence, with the same citation. We address this issue in **Response 2, 11, 18** and have updated the references where appropriate.

Comment #5:

"Recent scientific findings from other Greenland ice cores (GRIP, NorthGRIP, NEEM), the role of the Southern Hemisphere for CO2 variability and solar forcing are widely missing."

Response:

We have updated the manuscript to present alternative hypotheses for increase CO2 during the specified time period (Bereitier et al, 2000; etc), and present these alternatives as well as our own.

Comment #6:

"Although you often refer to the volcano-deglaciation feedback, as described by Huybers & Langmuir (2009), you never address potential areas of deglaciation during the DO events that would provide these increased emissions for this feedback and the proposed CO2 increase. The feedback theory relies on ice sheets that retreat from volcanic source areas. No spatial information where that could happen during or before the DO events is provided in the manuscript."

Response:

It is not our intention to reproduce Huybers and Langmuir (2009), where they show this exact information;

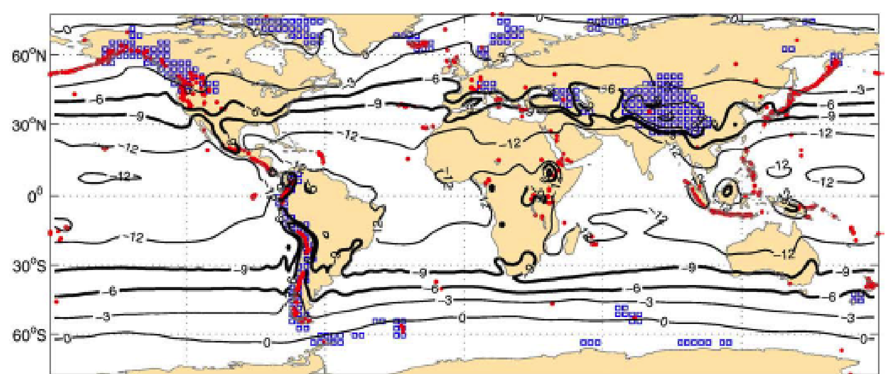


Fig. 4. Ice volume balance for the modern climate system. Ice volume balance (contours) is calculated as the number of meters of precipitation per year minus the meters of ice ablation, both estimated from the NCEP/NCAR reanalysis of meteorological data (Kalnay et al., 1996) (see text for details). Also indicated are the locations of volcanoes (dots) and regions containing modern day glaciers (squares) (Cogely, 2003). The bold contours at −6 and −9 m/yr indicate the two divisions used to distinguish glaciated and unglaciated regions in subsequent calculations. Note that the orography associated with volcanoes tends to lead to higher volume balances.

But we agree that some discussion of the locations of potential volcanic-deglaciation is warranted and we have made appropriate additions to the text.

Reviewers' specific comments;

(1) PG4942 LN16: ***"You are using 400 yr resolution data to investigate climate fluctuations acting in decades and centuries. Is there no higher resolved data (d18O, SO42-) available for Greenland, where they claim annual dating over most of the last glacial? Is the data set you are using the best suitable data for the analysis or the only assessable data?"***

Response:

The "approximate 400 year resolution record" of d18O in the manuscript, is a bit misleading if you are not familiar with some of the fundamentals of signal processing. The GISP2 d18O ice-record, has on average 80 years between sample points (average 2 m intervals). This temporal sample spacing gets longer (> 200 years) when you extend further back to > 80kya. With an average 80 year sampling rate and using a fairly stringent Nyquist sampling criteria, of at least 5 measurements to resolve one wavelength, we arrive at 5*8 = 400 year approximate resolution.

There is a higher resolution record for d18O available via the **Niehls Bohr Institite, Centre for Ice and Climate** (http://www.iceandclimate.nbi.ku.dk/data/), with a temporally-evenly sampled 20 yr resolution. We have repeated the analysis for this record, also addressed in our response to ***Reviewer 1***), and repeat it below. Additionally, while the Castellano et al. (2004) calculation of volcanic flux from EPICA Dome C provides a higher resolution data set for volcanic sulfate (they state < 10 year resolvability for much of the last 45 ky), they unfortunately do not provide online access to their analyzed sulfate record. While we have more confidence in the methodology performed by Zielinski et al. (EOF analysis), we do think it would be interesting to compare the periodicities between the two methods at the two sites. Unfortunately until Castellano et al. provide access to this data, via the **NOAA NCDC Epica Dome C Ice-Core Data Repository** for example, I do not see this comparision taking place. (http://www.ncdc.noaa.gov/paleo/icecore/antarctica/domec/domec_epica_data.html)

As repeated to our response to ***Reviewer 1 Comment #6***, in regards to a higher resolution record for d18O with a lower uncertainty time scale;

> While the NGRIP/GICC05 may offer a more accurate time-scale, this accuracy is first (1) limited to chronologies extending to only 60,292 b2k, not the entirety of the time period we are investigating. Secondly (2) while the correlation between NGRIP/GISP2/GRIP is highly resolved through the GICC05 tie-in's, this correlation is primarily limited up to the LGM – which is the lower boundary for our time series investigation. Specifically, (a) "The dating of the period from 14.7 to 42 ka b2k is based on the visual stratigraphy and measurements of the ice conductivity." (b) general agreement was seen between the two timescales for most of the record, although you are correct they do see a change in the stadial duration;
>
> "As seen in Fig. 1 , in general there is a very good agreement between the GISP2 and GICC05 time scales. GICC05 agrees with GISP2 to within 250 yr over the entire period back to 30 ka, and the two chronologies determine the onset of interstadials within 300 yr (Fig. 2). One will notice, however, that the duration of the interstadials/ stadials generally appears longer/shorter for the GISP2 time scale than for GICC05 respectively"

(Svensson et al., 2006)

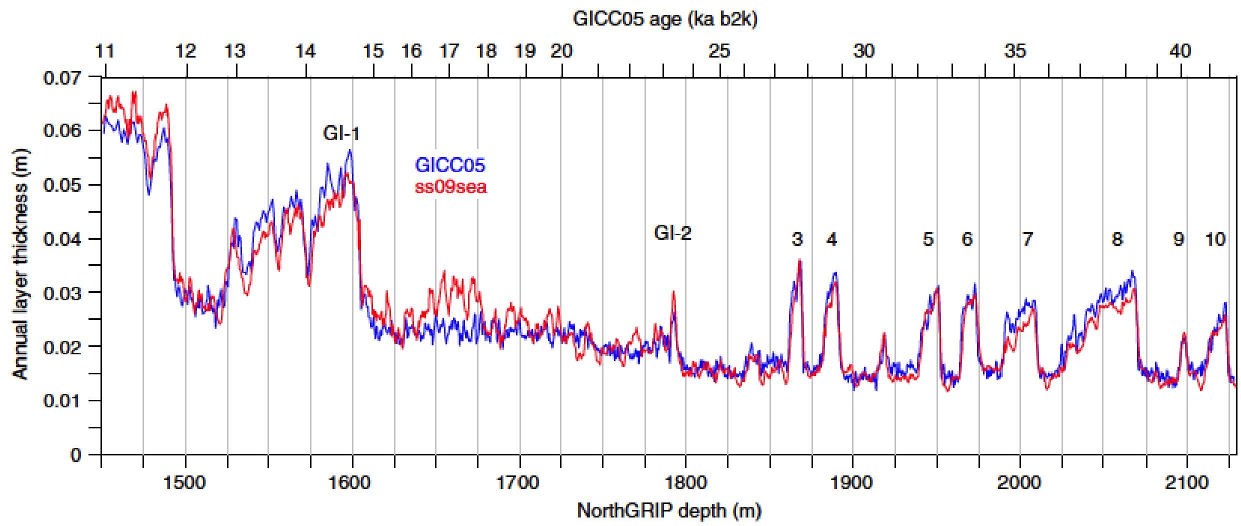
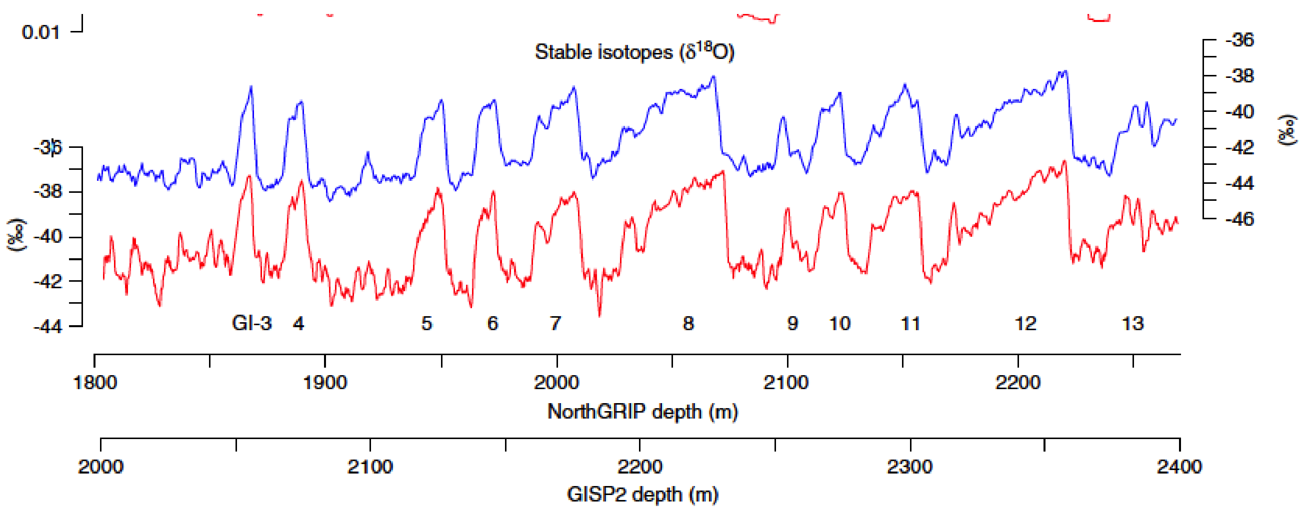


Fig. 3. Comparison of NorthGRIP annual layer thicknesses between the 'ss09sea' model and the GICC05 time scales.

But note, small change in their length will not primarily affect their periodicity. We are also not directly concerned with the accumulation rates, only as they affect the stable isotope record. Most importantly the delta 18O periodicities and durations are not .



While Svennson et al. (2006) proposes that;

"many others discuss the existence of a 1470 yr climatic cycle in the stable isotope profiles of the last glacial period. The conclusions of these studies may very well be justified, but we would like to emphasize that the finding of such periodicities obviously relies profoundly on the applied time scale. For example, the existence of the proposed 1470 yr cycle depends on the exact timing and phasing of the onset of D–O events, and, as discussed above, this is exactly where we believe that the GISP2 time scale is inaccurate."

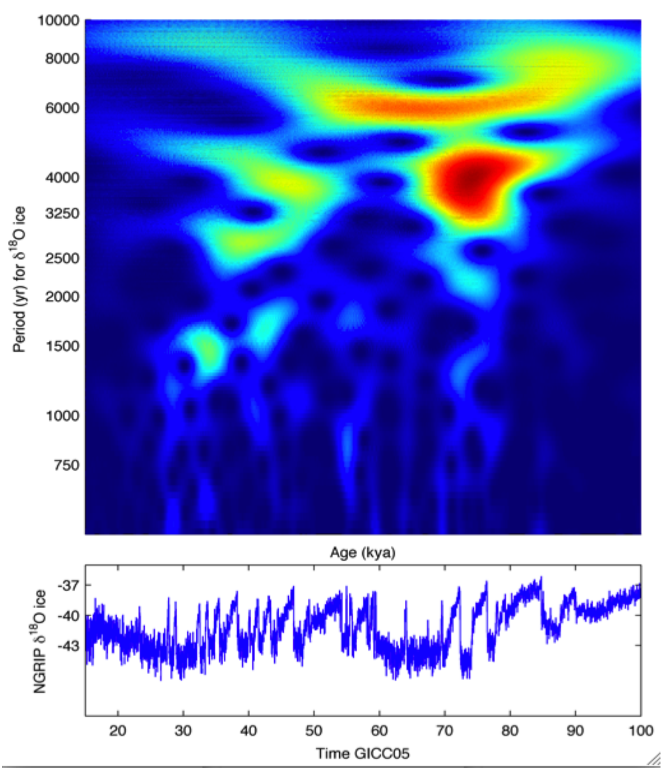
They do not offer and discussion on this proposed in accuracy – making their comments supposition at best.
We should also note, that GIPS2 d18O data on the GICC05 time-scale is only available for 11.6-32.55 ka b2k, once again only a small portion of our total time-series. (http://www.iceandclimate.nbi.ku.dk/data/).

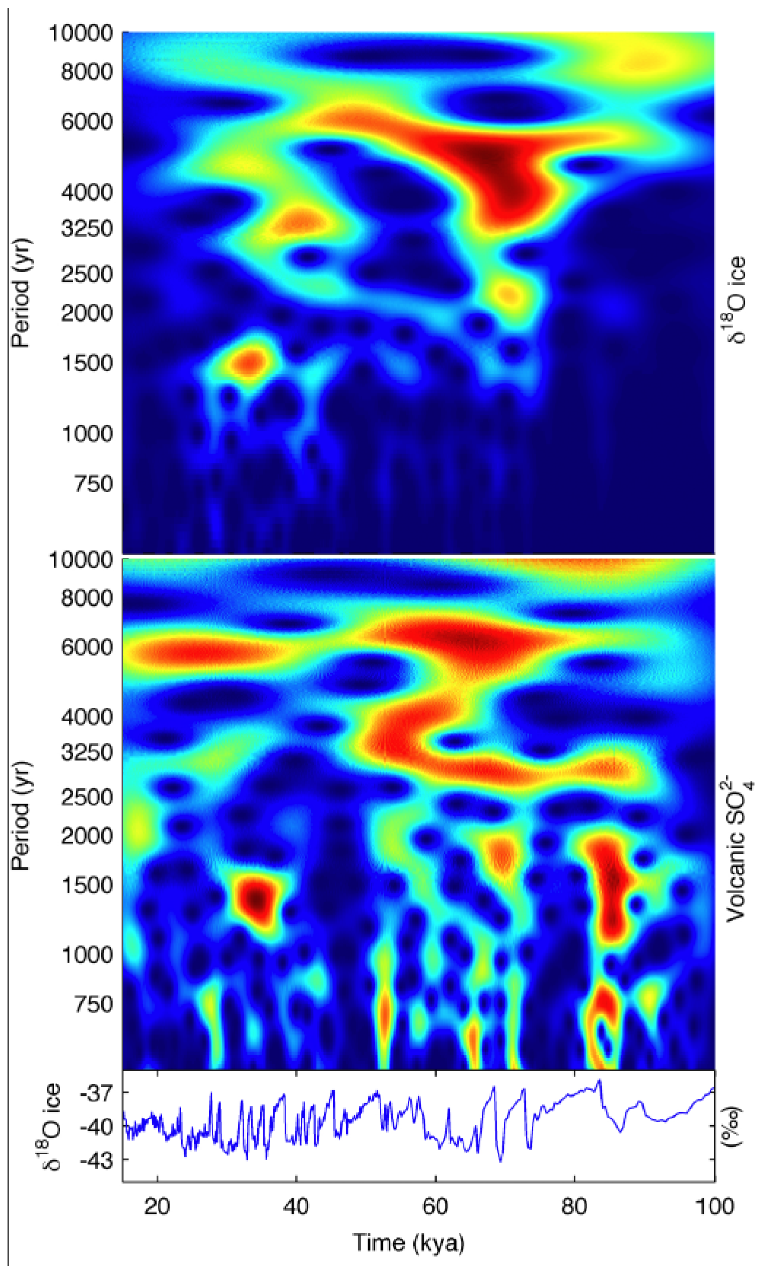
It is unreasonable for the reviewer to question why we did not use the GICC05 time-scale, as the correlation for GISP2, for the range we are investigating, does not exist.

To address those that may be still hesitant, and still insistent that the Svensson et al. (2006) proposition that the continually observed 1475 periodicity observed in the GISP2 d18O record is an artifact of the GISP2 time-scale, we redo the frequency analysis for the NGRIP d18O record (with its GICC05 time-scale). We observe similar periodicities between this analysis and our original GISP2 analysis. We are fully confident in our original processing.

GICC05modelext

GISP2





(2) PG4943 LN8-26: ***"The entire section describing causal links between IRD, NADW and MOC has no references!!! It is not clear if you present common knowledge or already your own interpretation."***

Response:

We have updated the section with the appropriate references.

(3) PG4943 LN29: ***"Strong statement! How would you know their attribution is mistaken?"***

Response:

This is one of the underlying hypothesis of the manuscript. The mis-attribution for the solar forcing hypothesis is extensively discussed in section **2.2**. The mis-attribution to internal/external ice-sheet dynamics is unclearly presented in the manuscript. What we mean is that fully attributing the variability to ice-sheet / ocean circulation dynamics is incorrect. They do play a role, but are part of the overall feedback discussed in the MS. This is shown through the frequency analysis and co-variability of the volcanic signals. We have clarified this in the MS.

(4) PG 4944 LN6: ***"The deglaciation-volcano feedback hypotheses as proposed by Huybers & Langmuir (2009) requires large changes in the ice sheets which you didn't have (or at least you don't provide evidence) during the short lasting DO events."***

Response:

Our purpose on this manuscript is not to provide a detailed, pathway-specific model for how the deglaciation-volcano feedback, presented by numerous other authors (Huybers & Langmuir one of them), works. This would involve a much more thorough treatment of time-varying ice-sheet extent, mantle decompression, and crustal-rebound models; which is beyond the scope of this paper. The purpose of this manuscript is to test whether these hypotheses proposed by others is supported by the time-series data and what the dominant periodicites are as calculated through a more rigorous treatment of spectral analysis. We have tried to clarify the intent of this manuscript in the introduction.

(5) PG4944 LN 10: ***"How was the insoluble micro-particulate sulfate determined? Which method was used in the reference you are citing (Zielinski 1997)? It would not hurt to provide this information to the reader who will probably not want to track his way through all the references and the references therein. If you are using particle counter data, is there potential in that the volcanic sulfate record is biased by a) large dust flux, which also peaks during cold stadials, b) input of non-volcanic sulfate e.g. Ca2SO4, c) changes in accumulation rates, relative changes of wet vs. dry deposition. You carefully investigate the 10Be records for potential biases later, but you should test the datasets that you use to develop your hypotheses equally careful. A dust record should be added also to Figure 1."***

Response:

We have expanded this paragraph to fully present how the Zielinski et al 1997 record was produced;

The majority of warming events (increasing #18O) are preceded in the GISP2 ice-core record by an increased concentration of volcanic sulfate, indicative of increases in global volcanic activity (Fig. 1; Zielinski et al., 1997; Zielinski and Mershon., 1997). The GISP2 volcanic sulfate record spans a (approx) 110,000 year period of explosive, high sulfur-producing volcanism, derived from the SO42- time series (Zielinski et al., 1996). The record is comprised of direct deposition of volcanically-generated aerosol sulfuric acid (H2SO4; Hammer et al, 1980), originating from both equatorial, mid-to-high latitude eruptions (Palais et al, 1992; Zielinski e al., 1994), and possibly Southern Hemisphere eruptions (Taupu, New Zealand, 22,000 kya; Froggatt and Lowe, 1990). The record developed by Zielinski e al. (1996) is based on multivariate principle component analysis of the entire suite of ion species in the GISP2 ice-core through empirical orthogonal function (EOF) decomposition (Mayewski et al, 1994). The temporal variance of the ion species time series was decomposed into orthogonal spatial patterns--empirical eigenvectors--with each successive eigenvector explaining a maximum amount of variance (Zielinski e al., 1996). Two different EOF's explained variance in the SO42- time-series, one correlating directly with continental salt deposition (CaSo4) the other independent of both continental/marine input sources (CaSo4/DMS emissions from marine plankton). The EOF independent of continental and marine SO42- thereby proves a reliable record of past volcanism. The location and identification of tephra in the ice-core raises the significance of the volcanic record, and allows for determination of the source volcano through matching glass chemistrys (Palais et al., 1991). The sampling interval is between 10-15 years from 18-51 kya, with a gradual increase to approx 50 years at 90 kya. Due to the limited sampling resolution this record is considered a minimum estimate of the number of distinct volcanic eruptions. Further, the record may be incomplete due to the location of the source volcano, variability in circulation patterns, and post-depositional modification of the signal. Icelandic eruptions and those from the Northern Pacific Rim, could also produce an enhanced signal from tropospheric transport to Greenland. The increases in volcanic sulfate are also observed in the other paleo-volcanic proxies, notably the magnetic susceptibility record of North-Atlantic marine sediments (Stoner et al., 2000), and for between 20–40 kya in a calibrated radiocarbon database of late Quaternary volcanic eruptions (Bryson et al., 2006) (Fig. 1).

(6) PG4944 LN10: ***"Do you mean increased volcanic sulfate concentration or increased sulfate concentration? The latter is more reflecting changes in accumulation rates. How do you define increased concentrations? Which averaging period? Can you give some numbers? What about frequencies? Did also the frequency of volcanic events change or only the concentrations? What about volcanic sulfate fluxes?"***

Response:

We are referring to increased concentrations of sulfate, in the volcanic sulfate record. Accumulation rates have been taken into account, as referenced in the previous comment. We observe increases in both the standard, non-averaged, record, as well as the 400-yr smoothed volcanic sulfate record. This is given in the caption of Figure 1, **but we have also added it to the text**. The dominant periodicity is 5-6 ky, as discussed in the wavelet analysis section (**2.1**).

(7) PG4944 LN11-13: ***"The record of sulfate from Greenland alone is no indication of increased global volcano activity, but heavily biased to capture Icelandic and other eruptions from Northern Hemispheric source volcanos. The same is true for records of magnetic susceptibility from the North Atlantic. Sulfate records from Antarctica, Dome C over the last 40kyrs, which are also less influenced by dust impurities, indicates little change in volcanism (Castellano et al. 2004)."***

Response:

No ice core will be a perfect record of global volcanic activity. Northern hemisphere ice-cores will of course preferentially sample volcanic eruptions from Northern Hemisphere volcanoes. I do no think this is a hinderance. As the majority of currently outline feedback theories involve deglaciation and ice-calving to promote shutdown of NADW, it would stand to make sense that Northern Hemisphere volcanoes will be more susceptible to ice-loss and mantle decompression.

I would not argue that *Castellano et al.* (2004) indicates liltte change in volcanism.

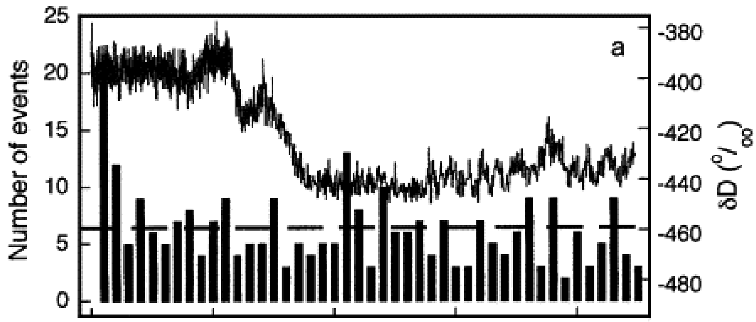


Figure 5. from *Castellano et al.* (2004), above, shows the frequencies per millennium of volcanic events (black bars) recorded in the EDC96 ice core against the δD profile (grey curve) used as a proxy for climatic variations. The black dotted line indicates the mean number of volcanic events per millennium. There are at least nine events that nine events that are > 50% of the average.

(8) PG4944 LN17: ***"What does the mean lag time of 1060 yr look like for the individual inter-stadials? Is the d18O lagging always? Is sometimes d18O leading? Is the variability of the lag ±100 yrs or ±1000 yrs? You could overlay d18O and SO42- relative to the timing of the warming for visualization."***

Response:

The d18O record is always lagging, hence why we attribute the increased volcanic activity as the precursor/initiation mechanism to the warm inter-stadials. The mean lag, is for a best fit cross-correlation of the entire time-series. We have not treated individual DO events separately, due to the subjective difficulty in aligning individual d18O spikes with what we think would be the corresponding smoothed SO4 record. Cross-correlation of the two time-series in their entirety allows us to get a mean estimate of the lag-time, but not an estimate of its uncertainty.

(9) PG4944 LN21-22: ***"You ignore the fact that parameters like d18O and SO42- are on an ice age timescale and CO2 and CH4 are on gas age timescales and cannot be directly "associated" without knowing delta age. Further, there is additional uncertainty in using a record from Byrd in Antarctica which is on an independent timescale, if not synchronized to GISP2. As you try to directly deduce mechanism from these time series it is essential to properly treat ages of ice and gas and underlying uncertainties."***

Response:

Yes, we do ignore the Δage difference between the CO2 and CH4 signals with the d18O and SO42- signals. We believe we can do this for two primary reasons; first (1) the periodicity of the CO2 and CH4 signals, which shows correlation with the d18O and SO42- signals, is independent of Δage, and we do not use the **timing** of the CO2 and CH4 signals to directly interpret mechanisms. We do state that the CO2 and CH4 concentrations increases initiate during the increases in global volcanic activity (high SO42-);

> Cross-correlation of the 18O ice-core and a 400 yr-smoothed volcanic sulfate record yields a mean lag time of 1060 yr – the time by which, on average, increases in global volcanic activity precede the transition from cold stadial to warm interstadial. Over these time periods the atmospheric concentrations of CO2 (Byrd-Antarctica, ice-core; Ahn and Brook, 2008) and CH4 (GISP2; Brook et al., 1996) increased by up to 32 ppm and 290 ppbV, respectively (Fig. 1).

Which brings us to (2), the above statement is true regardless of the Δage differences. The Byrd CO2 record has an approximate Δage of < 1500 years (Blender et. Al, 2005), which if looking at Figure 1 in our paper, could not shift the CO2 signal to post increase in volcanic SO42-. More importantly this same behavior is seen for the GISP2 CH4 signal (Brook et al., 1996), **which does take into account** the Δage of the CH4 signal being trapped at a depth between 80-100 m.

We have clarified this uncertainty in the text.

(10) PG4944 LN24: ***"Only stratospheric sulfate injections from tropical eruptions cause global atmospheric cooling!"***

Response:

No. This statement is incorrect. Short-term cooling is generated from increased sulfate aerosols regardless of geographic location (polar to equatorial) and is effective both in the troposphere and the stratosphere. While stratospheric aerosols mainly contribute to this cooling through attenuation of incoming solar radiation, sulfate aerosols in the troposphere enhance backscatter of solar radiation (Charlson et al., 1990; Balling and dso, 1991; Ramaswamy et al., 2001). Further, stratospheric aerosol radiative forcing on tropospheric climate is almost wholly a function of the aerosol column optical depth, and not geographical distribution (Hansen et al, 1992; Lacis et al., 1992).

I assume this misunderstanding is likely because nearly all geoengineering related discussions of aerosol injection to offset global warming discuss stratospheric injections. Stratospheric injections are on the forefront of these discussions because of the difference in residence times. Sulfate aerosols have year-length residence times in the stratosphere, opposed to week-length residence times in the troposphere. Refer to Crutzen 2006 for a more thorough discussion.

(11) PG4944 LN25-27: ***"Please provide references!"***

Response:

References have been added; they were already included in the sentences following the references lines, but have been incorporated into lines 25-27 as well.

Referring to: "While volcanic eruptions are typically associated with global atmospheric cooling, increased volcanic activity can lead to elevated global temperatures through (1) direct addition of greenhouse gases into the atmosphere, (2) changing tropospheric circulation patterns and/or (3) affecting the atmospheric generation of hydroxyl radicals (OH)."

Changed to: "While volcanic eruptions are typically associated with global atmospheric cooling

(Hansen et al., 1992), increased volcanic activity can lead to elevated global temperatures through (1) direct addition of greenhouse gases into the atmosphere (Huybers and Langmuir, 2009), (2) changing tropospheric circulation patterns (Robock and Mao, 1992). and/or (3) affecting the atmospheric generation of hydroxyl radicals (OH) (Manning et al., 2005)."

(12) PG4945 LN5-9: ***"In their first order model estimate Huybers & Langmuir (2009) attribute a 10ppm increase of CO2 over 18-13ka to volcanic activity related to the deglaciation of volcanic areas in the Northern Hemisphere. He further shows that volcanic eruption frequencies were reduced in the glacial relative to the deglacial time period. The observed 10-30ppm rises of CO2 in Antarctica during inter-stadials within decades are therefore unlikely attributable to increased volcanism."***

Response:

I'm sorry, but I do not follow your train of thought. First, Huybers and Langmuir (2009) observe a decrease in atmospheric CO2 of 10 ppm from 40 – 18 Ka. They interpret this as a global trend towards lower atmospheric CO2, partly attributed to excess weathering, relative to the volcanogenic signal. During the first half of the deglaciation (18 – 13 Ka), the time frame you bring up in your comment, there is an average 10 ppm increase (5 to 40 ppm), in the purely volcanogenic CO2 signal. You are correct in that there is an increase in the number of volcanic events during the deglacial in the database of recorded events (Bryson et al., 2006). I believe you are miss-interpreting Huybers and Langmuir (2009) Fig. 6. In Huybers and Langmuir (2009) Fig. 6b, you see that during the deglacial the contribution to atmospheric CO2 from volcanic activity (red line) is much larger than during the glacial – which is in agreement with there be more volcanic events in the deglacial relative to the glacial. In terms of attributed CO2 during both periods, the 10 ppm in the deglacial compared to the seemingly larger 10-30 ppm in the interstadials is more difficult to interpret. The overall trend of lower total atmospheric CO2 during the deglaciation, from increased weathering, could artificially mask higher volcanogenic CO2 increases. The difficulty in comparing glacial and deglacial periods is specifically why we limited our analysis to pre-LGM.

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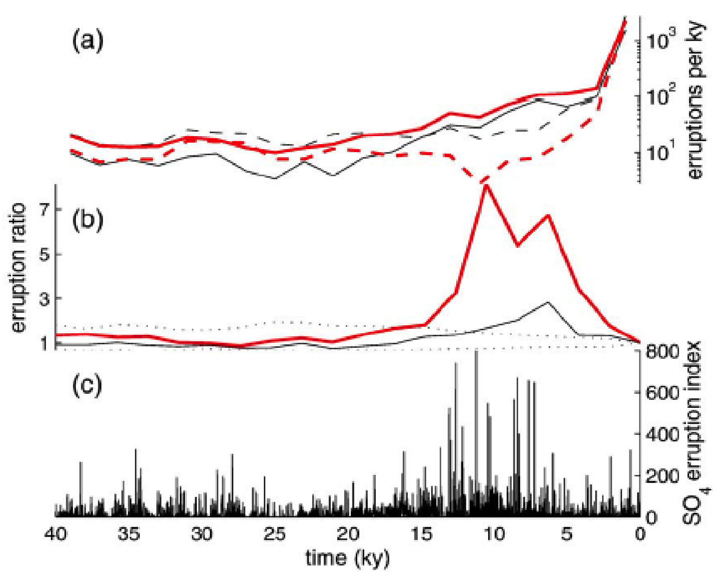
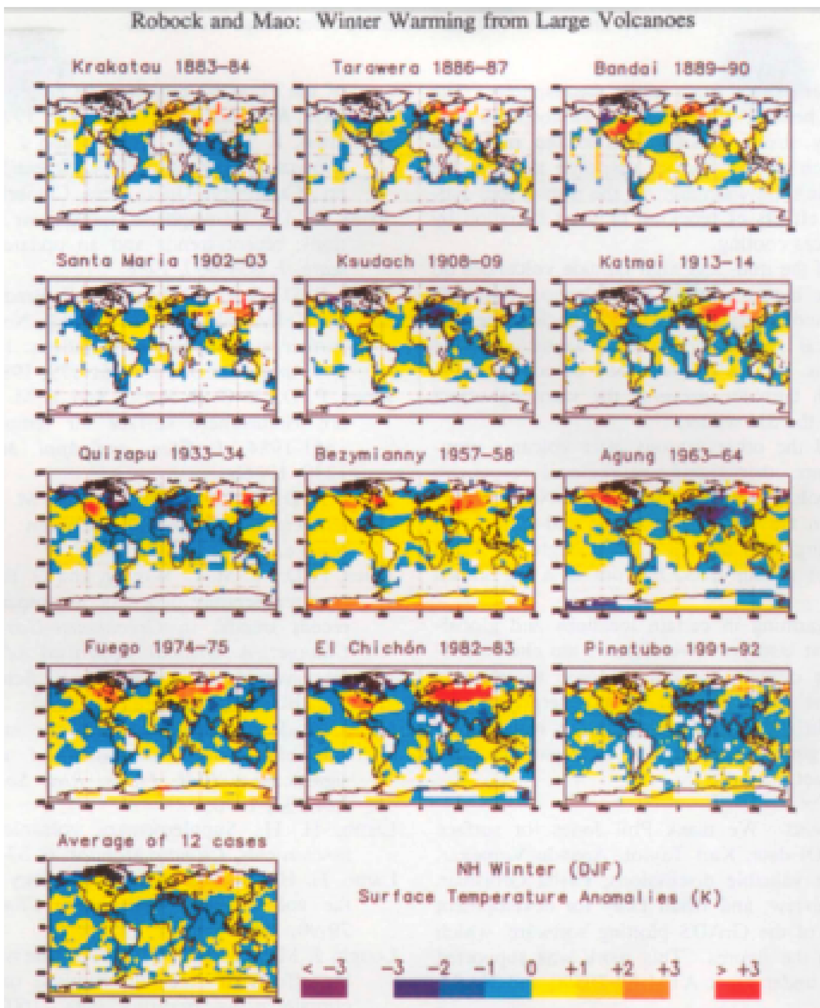


Fig. 6. Changes in volcanic activity over the last 40 ky. (a) The number of recorded volcanic eruptions per ky for glaciated (solid lines) and unglaciated (dashed) volcanoes. Glaciated volcanoes are defined either as those with a modern ice volume balance exceeding −9 m/yr (bold lines) and those exceeding −6 m/yr (thin lines). See Fig. 4 for a map indicating which volcanoes are in the glaciated and unglaciated groups. Note that the y-axis is logarithmic. (b) Estimates of the global frequency of volcanic eruptions using the −6 m/yr (thin line) and −9 m/yr (bold line) volcano groupings, where global frequency is normalized relative to the last 2 ky. The 99% confidence interval for the null-hypothesis of no systematic difference between glaciated and unglaciated events (dotted lines) indicates that the deglacial increase in eruptions is significant. (c) An eruption index based on volcanic SO_4 from a Greenland ice core (Zielinksi, 2000).

(13) PG4945 LN19: ***"Are the 3 degree winter warming global averages, continental averages or maximum values? How much warming does Greenland ice sheet experience? With a lifetime of the winter warming of 2 years and obviously no increase in global tropical eruptions evident from Antarctic ice cores this mechanism seems unlike to contribute to increased ice sheet rafting."***

Response:

The 3 degree's are maximum Northern Hemisphere winter surface temperature anomalies, following the winter of a major tropical eruption, the first or second winter following midlatitude eruption, and two winters following high-latitude eruptions. Greenland is unfortunately largely below their model bounds. More specifically (1) "*With a lifetime of the winter warming of 2 years*", we do not know this is limited to two years as their may be compounding feedbacks, and (2) "o*bviously no increase in global tropical eruptions evident from Antarctic ice cores",* I do not see where you are see that there is "obviously" no increase in global tropical eruptions evident in ice-cores. I do not think we can say either way, we do not have anyway to resolve that specificity.



(14) PG4945 LN21-29: ***"With no increase of stratospheric eruptions evident from the data the previously attributed changes of methane due to changes in the terrestrial biosphere might still be valid."***

Response:

How do you resolve whether an eruption is from a tropical location or a midlatitude location, or whether it is purely tropospheric or extends into the stratosphere, based on the ice-core data? You can not. I am perplexed by your repeated assumption of the data being able to resolve this specificity, as it can not. We can correlated volcaniclastic stratigraphic records with ice-core and marine records to get a basis of the eruption fallout extent (isopach) which tells us the possible atmospheric height the eruption reached, given an assumed eruption epicenter. We can not look at an ice-core, and based off of SO4 and microtephra determine where the volcano originated and the height of the eruptive plume.

(15) PG4946 LN3-4: ***"Which increase are you describing? Which time period? Relative to what background? Why do use ppbV?"***

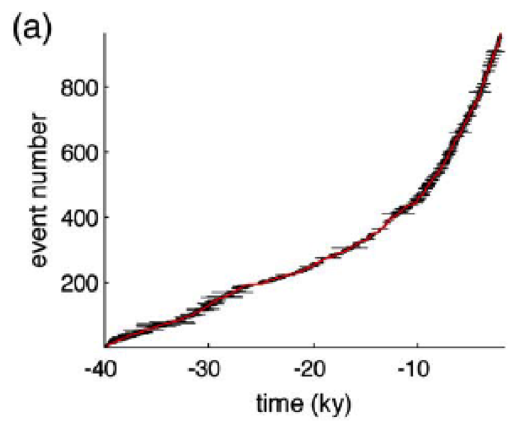
Response:

We are referring to the increase of volcanic SO4, seen in the 400 year-smooth volcanic SO4 signal, shown in Fig. 1. We are referring to increases seen throughout the record, above a baseline of 0 ppbV volcanic SO4 signal. We use ppbV because that is the unit of measure used in the volcanic SO4 GISP2 time-series.

(16) PG4946 LN26: ***"I am not fully familiar with spectral analysis but when I look at the error distribution of the radiocarbon dated events (Fig. 1, Huybers & Langmuir, 2009) in the dataset you are using, I notice that the average age uncertainty of these eruptions is close or even exceeding the frequencies you are using in your wavelet analysis."***

Response:

It is important to clarify, that we should be talking about Figure 1a, as Figure 1b in Huybers and Langmuir (2009) is for eruptions less than 2 kya .



While the uncertainties in the Bryson et al. (2006) data (plotted above in Huybers and Lammuir, 2009) is on average approximately 2-3 kya, this uncertainty is (1) smaller than the main controlling perodicities seen in the wavelet analysis; 5-6 ky for d18O and So42-, and 5-8 ky for Heinrich events. There could be a periodicity spreading/dilution due to the age uncertainty for any periodicites less than 2-3 ky, such as the 1500 year harmonic signal, but this would only be an issue for the frequency analysis of the Bryson et al. 2006 data, and not the ice-core time series data. Since we make no conclusions or interpretations based of the sub < 2-3 ky perioditicits for the Bryson et al. (2006), we do not see this as problematic. We have included a brief discussion of these uncertainties in the wavelet analysis section of the manuscript.

(17) PG4947 LN9-10: ***"Speculation! There is no evidence for reduced atmospheric oxidation presented in the manuscript."***

Response:

Please see PG4945, LN22 – PG2946LN6; the entire paragraph is a justification for why there might be an atmospheric oxidation component. The evidence for reduced atmospheric oxidation is the increase in atmospheric methane, that is correlated with the proxies for increased volcanism (SO4, etc). We also do no present the mechanism as a hard proven theory, but as a general hypotheses, to explain the periodicities seen in the spectral analysis (PG4947 LN6).

(18) PG4947 LN10-14: ***"As before: Are there any references?"***

Response:

No, this is our interpretation.

(19) PG4948 LN1-23: ***"Many of the problems you describe in using 10Be as a proxy for solar activity are valid also in using particle counter data as a proxy for volcanism. Therefore all data sets should be evaluated equally careful. Many of the references you are citing are quite old and I am aware of highly correlated time series of ice core based 10Be and independent series of 14C from precisely dated speleothems or tree-ring chronologies, giving confidence in the use of these 10Be series as a solar forcing proxy. I am missing the according citations right now, but you might have a closer look into the most recent published literature before revisiting solar forcing."***

Response:

It is important to note that we are NOT saying that 10Be can be used as a proxy for solar activity – far from it, it is a well established methodology particular in analysis for recent records of the Holocene (Bond et al., 2009). These analysis do not rely solely on 10Be and use 14C as well, allowing independent confirmation of the 10Be. We agree with your statement that "***all data sets should be evaluated equally carefully"*** and we believe we have presented a careful analysis of the 10Be over the time period from 20-110 kya, and show that it does not support the solar forcing model.

(20) PG4948 LN25-26: ***"I admit that the data you present show a lot of co-variability but missed how the exact mechanism worked and how it is supported by the presented data."***

Response:

We have explained the concluding text as well as added an additional figure to better explain the mechanism.

(21) PG4948 LN1-3: ***"I completely missed your attribution of 1-5 degree warming in the main text!"***

Response:

The 1-5 degree warming in the conclusion is based off of the IPCC equation for climate sensitivity for equilibrium surface temperature to radiative forcing – using maximal increases in CO2 and CH4 seen prior to a warm interstadial. We have expanded this in the text for clarification.

(22) PG4948 LN6-10: ***"Artificial end where you try to link your finding to recent global warming. Most of the volcanic regions in the world that could have provided a deglacial-volcano feedback in the past (e.g. Iceland, Alaska) are deglaciated by now and will not have much effect during future warming."***

Response:

I disagree. There are both large scale and small scale ice volumes than can affect this feedback – and we point you to the mantle decompression studies done in Iceland for further detail. Additionally, there are large glaciated stratovolcanoes through the Aleutians, Alaska, and Pacific Northwest. We have expanded on the text to clarify these issues.

Technical corrections:

PG4942 LN2: Measurements The record of d18O in …

Response:

Stylistic, not changed.

PG4942 LN8: Sulfate = SO42-

Response:

Changed.

PG4942 LN10: a database can't show periodicity!

Response:

We do not say a database shows periodicity, we state "Wavelet analysis of ….[]". We have clarified this to say;

"Wavelet analysis of ice-core and marine-sediment records show a repeated 5000–6000 yr periodicity in both volcanic SO4 and 18O ice records, as well as a 5000–8000 yr cycle in the lithic concentration of ice-rafted debris, atmospheric CO2 concentration, and an dated-event database of late Quaternary volcanic eruptions."

PG4942 LN15: as above (l.2)

Response:

Stylistic, not changed.

PG4942 LN25: You might want to move "(high d18O ice)" closer to the "warm inter stadials"

Response:

I see your motive here, although I believe that "high d18O" should be immediately after "increased global precipitation" since it is the increased rainfall that leads to the "high d18O" signal in the ice—through isotopic fractionation—which is then interpreted as a warm period.

PG4942 LN26: delete "Fig. 3"

Response:

This is a key figure in the spectral analysis, showing correlations in the periodicities between multiple signals, besides the d18O and SO4. Unfortunately, I think if you think this figure should be deleted, I really did not explain the spectral analysis well enough.

PG4954 Figure 1: “The time series should be labeled (a-g) in the figure and the according caption to make it easier and faster for the reader to identify the time series. By showing the overall record only, it is hard to see any detail. Maybe you could provide a detailed inlet for certain events or an overlay of time series relative to the start of the inter-stadial warming, in addition. The reference for the so called “documented” volcanic events is missing.”

Response:

Agreed, we have added labels, as well as an secondary figure showing a zoom into on specific inter-stadial/stadial transition. We have added the appropriate reference (Bryson et al., 2006) as well.