General comments:

The author is analyzing a volcanic sulfate record from GISP2 between 10-110,000 krs BP and its relationship to regional temperatures, global greenhouse gas concentrations and records of ice raft debris in the North Atlantic using wavelet analysis. From common periodicities he is deducing causal links to explain the observed variability in the underlying time series. This leads to the interpretation that the DO events are a product of periodic increase of global volcanism, increasing the concentrations of CO_2 and CH_4.

The paper is short and in general clearly written. My major concerns with this manuscript are:

a) The ability of the underlying **data** to perform the analysis;
b) The **simplistic approach** of deducing mechanism from observed co-variability;
c) The absence of properly addressing the effects of sampling resolution and **dating uncertainties** for the analysis;
d) The lack of a detailed discussion of up-to-date alternative explanations for glacial climate variability from **available literature**. One could get the impression in this manuscript that DO events have never been studied in other ice cores before.
e) Very strong statements about the "mistaken" attribution of ocean dynamic, solar variability and the role of terrestrial biosphere in explaining observed climate variability.

Given this major concerns I cannot recommend the publication of this manuscript in the present form in "Climate of the Past" nor do I see the possibility to address this concerns within short time. A detailed summary of the major concern follows:

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.
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 Ca_2SO_4 inherent in atmospheric dust at Greenland. All these parameters vary strongly during stadials and interstadials. Therefore the intensity of laser light scattering, ^{10}Be, 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.

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.
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.
5. Recent scientific findings from other Greenland ice cores (GRIP, NorthGRIP, NEEM), the role of the Southern Hemisphere for CO_2 variability and solar forcing are widely missing.
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 CO_2 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.

Specific comments:

4942:

16: You are using 400 yr resolution data to investigate climate fluctuations acting in decades and centuries. Is there no higher resolved data (d18O, SO_4^{2-}) 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?

4943:

8-26: 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.

29: Strong statement! How would you know their attribution is mistaken?

4944:

6: 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.

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. Ca_2SO_4, c) changes in accumulation rates, relative changes of wet vs. dry deposition. You carefully investigate the ^{10}Be 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.

10: 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?

11-13: 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).

17: 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 SO_4^{2-} relative to the timing of the warming for visualization.

21-22: You ignore the fact that parameters like d18O and SO_4^{2-} 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.

24: Only stratospheric sulfate injections from tropical eruptions cause global atmospheric cooling!

25-27: Please provide references!

4945

5-9: In their first order model estimate Huybers & Langmuir (2009) attribute a 10ppm increase of CO_2 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 CO_2 in Antarctica during inter-stadials within decades are therefore unlikely attributable to increased volcanism.

19: 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.

21-29: 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.

4946

3-4: Which increase are you describing? Which time period? Relative to what background? Why do use ppbV?

26: 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.

4947

9-10: Speculation! There is no evidence for reduced atmospheric oxidation presented in the manuscript.

10-14: As before: Are there any references?

4948

1-23: Many of the problems you describe in using ^{10}Be 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 ^{10}Be and independent series of ^{14}C from precisely dated speleothems or tree-ring chronologies, giving confidence in the use of these ^{10}Be 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.

25-26: 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.

4948

1-3: I completely missed your attribution of 1-5 degree warming in the main text!

3: I read the first time in the manuscript from this feedback.

6-10: 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.

Technical corrections:

4942:

2: ~~Measurements~~ The record of d18O in …

8: Sulfate = SO_4^{2-}

10: a database can't show periodicity!

15: as above (l.2)

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

26: delete "Fig. 3"

4954:

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.