Response to the comments of the reviewers

We thank the reviewers for their review of our manuscript. We have done our best to take their remarks into account and hope that the proposed changes have contributed to improve our study. In summary, we have tried in the new version to:

- improve the figure quality;
- correct language weaknesses;
- and re-write the conclusion section following the reviewer 1 suggestions.

We have also re-performed our spectral analysis. We previously thought that the detrended signals may be tested against a white noise considering that the multi-millennial periodicities had been removed form the signals. But, as noted by the reviewer 1, our analysis was finally not judicious and still resulted in an overestimation of millennial scale periodicities. Our new results are now in agreement with the reviewer 1 supplementary study and the interest of measuring high resolution δD data has been more clearly highlighted. Our main point about an observable increasing variability at the end of the MIS 11 warming phase has been reinforced.

We also agree with the reviewer 2 main point regarding dating errors. We do not have abilities at present time to go further in the exploration of such statistical noise impacts on our variability analysis. We think that this point requires a dedicated work with the development of a specific methodology including sensitivity tests using the glaciological dating tools (Lemieux-Dudon et al, 2010) and should be addressed in a separate paper. Nevertheless, we are aware of these limitations due to possible age–scale errors. They are discussed in our study and we have been especially careful in our conclusion regarding the periodicity values revealed by MIS 11 spectral analysis. The highlighted changes of variance in regard of trends are however still robust.

Detailed responses to each comment of the reviewers are listed below.

Reviewer #1 C. Lemmen

1 Summary

The authors discuss a new high-resolution deuterium data set obtained from the EPICA Dome C ice core. On this record, they analyse long-term trends, non-stationary variability, and spectral features in the marine isotopic stage 11 interglacial, a 20-30 ka period roughly 400 ka before present. In their analysis they take into account the poorly know length of MIS 11 and uncertainties introduced by molecular diffusion. Consistently, the findings for MIS 11 are compared to MIS 1, the current 12 ka interglacial. Their main finding is the apparent coupling of long-term (multi-millenial) to short-term variance at the onset of the cooling in both interglacials; this coupling is robust when tested against known uncertainties.

2 Evaluation

The content of the paper is a suitable contribution to the topics covered by Climate of the Past. The scientific question, i.e., the coupling of centennial to millennial climate modes is highly relevant for our understanding of the internal versus external dynamics of the climate system. The analysis presents well-established methods and an innovative approach to using the variability analysis. The new data set has not been published elsewhere. The analysis is presented clearly and straightforwardly, the conclusions are substantiated by the methodology and results presented. Reproducibility is given, once the dataset is published (see general comment below). References are appropriate and properly used. The structure of the paper is clear, with a very good abstract. The language is fluent, but could be improved, one issue with symbols remains (see technical details below). The length of the paper is appropriate, the figures are appropriate, but could possibly be reduced (see below). One weakness of the current manuscript is the conclusion section, also the title could be adjusted when the conclusion has been rewritten. Another weakness is the technical quality of the figures which must be improved substantially. Based on my evaluation, I recommend that the paper is accepted for publication in Climate of the Past with major revisions according to my comments below.

A particular attention has been brought to improve both language and figure quality. The conclusion section has also been modified according to the reviewer's suggestions. Please see the details below.

3 General remarks

3.1 Dataset publication

I recommend to follow the lower-resolution data set (Jouzel 2008) publication method, where the data was archived on PANGAEA (http://www.pangaea.de/) with assignment of the digital object identifier:
http://doi.pangaea.de/10.1594/PANGAEA.683655.
Once the DOI is assigned, it should be referenced in this manuscript.

The new version of the manuscript now includes an attached excel file displaying the raw data with respect to depth. The correspondence between depth and age was performed using the EDC3 age-scale [Parrenin et al., 2007a] (data available online as a supplement to their paper).

3.2 Time axis

To facilitate accessibility of your conclusions to interdisciplinary readers, you should consider reversing the time scale with old age on the left increasing towards younger age to the right (descending ka BP). Although this is not the standard display for scientist working on palaeodata, it will make your results better understandable to the modelling community which Clim. Past addresses, too. Moreover, throughout the text,

your discussion is time-forward, starting with older ages, again pointing to a time-forward x-axis as a more natural approach.

This particular time-axis is commonly used in the paleoclimate community and was previously used for the other EPICA Dome C stable isotope publications. We have consequently decided to keep it and hope that the editor and reviewers will agree with this choice.

3.3 Figure quality

Although this is "just" a technical issue, the figures are not suitable for publication as is. Bitmaps are inappropriate for this type of display, please choose a vector format for figures 1 and 2. Figure 3 could be shown as a bitmap, but the quality is vary poor due to the use of a spectral compression method (wavelet jpeg?) which leads to artifacts; incidentally, these artifacts are exactly those, that you tried to avoid in your scientific analysis by padding your data for the wavelet analysis . . . .

I would appreciate if you could adjust all texts (figure titles, axis labels, and tickmarks, labels on figure) to the font used by CP (Times 11pt), and increased the tickmark font size somewhat.

The font size for figure legends and annotations has been modified to fit with the CP one: Times New Roman. We have also increased the tickmark size and changed the Figure 1 and 2 formats. We hope that the quality is now suitable for publication.

Figure 1a: please use grey (and adjust in caption and text) instead of red to enable b/w printing. Add y axis label.

Done.

Figure 1b: I don't find this figure necessary, no added value, the findings can be stated without this figure.

The new age scales have not been obtained with a simple linear compression of MIS 11, but with an orbital synchronisation supported by O2/N2 data (Landais et al. revised). The MIS 11 duration is thus reduced in a distorted way (the text has been clarified to underline that point). Figure 2 illustrates how the MIS11 δD profile is being modified by application of these two dating tests and we therefore prefer to keep this figure in our revised manuscript.

Figure 2: it is hard to see that panels a,b,c,d refer to columns; please think of a better way to group the panels. Add information like 'detrended' within the figure (panel b), visually separate time axis (panels a,b,c) from variability axis (panel d)

We have tried to make easier the readability of Figure 2 with added annotations and separated time and trend axes.

Figure 3: I would like to see panels for EDC on the same x-axis or on an x-axis aligned by date. Here, it seems that TEST2 is longer than TEST1, but it is, actually, shorter.

Done.

3.4 Holocene climate variability

Regarding the Holocene, I recommend you include a reference to our recently published manuscript Wirtz et al. (2010) on changing Holocene climate variability. Below, I point to information contained in this paper, which would be relevant for your discussion. In Wirtz et al (2010) we identify 5.5 kyr BP as the change point in Holocene climate variability (fig 2), this information substantiates your analysis. Among the 124

globally distributed proxies, two are deuterium measurements on Antarctica (Komsomolkaia, Masson 2000, and Vostok, Petit 1999). We detected cyclic periodicites in both records, both in the upper and in the lower Holocene (Fig 4). Variability increased from the lower to the upper Holocene, especially in the sub-millennial band (200-850 a), no increase in the millennial band (860-1800 a) could be detected.

This finding tends to support your result that the 800 a cyclicity becomes dominant in the late Holocene. But note that also non-cyclic anomalies significantly increased from the lower to the upper Holocene in Antarctci records (see Fig 8). In line with your discussion and with Debret et al (2007) we found cyclic variability in the lower but not in the upper Holocene in the solar proxies, thus a decrease in solar variability, especially in the submillennial band. This could have lead to an increase of internal variabilities instead of the solar-driven earlier part of the Holocene. To substantiate these conclusions, however, I believe we need to assess more finely which frequencies we are talking about and what we mean by millennial (Bond uses 1500 +/- 500), you use 960, I used (860-1800), and submillennial (you see 300-500, I analysed 200-850). Especially in the frequency domain 600 to 950 a, there seems to be a lack of studies, you could point to this lack in your outlook.

We thank the reviewer for suggesting this very interesting and appropriate reference. It brings a real added value to our study and has been included in the discussion section (lines 460-468).

3.5 Reanalysis of your results

Referring to section 5.3, the sensitivity of the wavelet analysis to the uncertain duration of MIS 11, I did a quick calculation (see table below).

Age scale	*Wavelet cycle (a)*	*duration (a)*	*compression*	*corrected cycle (a)*
EDC	*495*	*24*	*1*	*495*
Test1	*470*	*22.4*	*22.4/24.0*	*460*
Test2	*320*	*16.4*	*16.4/24*	*336*
EDC	*1395*	*24*	*1*	*1395*
Test1	*1330*	*22.4*	*22.4/24.0*	*1295*
Test2	*960*	*16.4*	*16.4/24*	*947*
EDC	*3360*	*24*	*1*	*3360*
Test1	*3220*	*22.4*	*22.4/24.0*	*3122*
Test2	*n/s*	*16.4*	*16.4/24*	*2282*

This table shows in the last column the expected cyclicity with the assumption that the timeseries was compressed equally from the EDC age scale. All comparisons between your result and this expected value agree. In the light of this, there is no qualitative information gain from the wavelet analysis on the Test1 and Test2 age scales beyond corroborating the robustness of the approach. Therefore, the figures are not necessary for reaching your conclusions if you would include my analysis above.

Again, we thank the reviewer for his supplementary work, showing his interest in our study. We agree that such table could have made the Figure 3 lighter. But again, we underline the fact that: first the new MIS 11 durations were not derived from a simple compression of the EDC3 chronology; and second, such dating-tests are not just impacting our spectral analysis by reducing periodicity values but also by affecting their significance.

I did a further analysis using the methodology from Wirtz et al. (2010) on the dataset by Jouzel (2008, this is the 55cm sampling of the same ice core EDC). For the Holocene focus period, at p=.95, I identify significant frequencies at 49, 85, 770, and 3500, but not at 100-300a as you do in the fine sampling data.

This method analysis presents the advantage of a non-necessary resampling step that is a real added value for the study of MIS11. But it has to be noticed here that Holocene is described at a

theoretical resolution better than 20 years in EDC ice. This resolution is not realistic for sites located in East Antarctica plateau, considering post-deposition effects such as wind scouring or isotopic diffusion occurring in the firn, that result in a homogenisation of highest frequency variations (<20a). Hence, our 20a re-sampling helps to filter from our record raw data noise that cannot be objectively considered as part of climatic signal (as demonstrated by detailed signal to noise studies conducted for another low accumulation site in East Antarctica, Vostok, where long meteorological records are available, by Ekaykin et al, (2002). Thus, if this noise is not removed from the signal, it may induce the significance of non climate related high frequencies (as your 49a periodicity) and hide some ones else (as 100-300y ones).

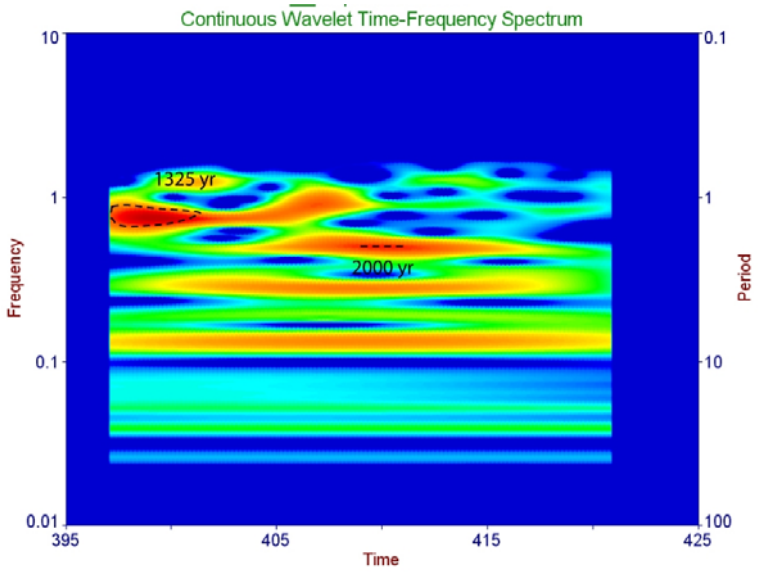
Also note, that these periodicities are not significant at the critical p-level of p=.99 (according to Thomson 1990). For MIS11 and the EDC time scale, I do find significant (at p=.99) frequencies at 330 and 490 (corresponding to your 495). For your convenience I attached figures of my analysis to this review.

We recognize our mistake in our first spectral analysis, tested against a white noise at a p-level of p=.95. We have corrected it and re-performed our analysis by testing our signals against the appropriate red noise and p-level using the statistical test of Torrence and Compo (http://paos.colorado.edu/research/wavelets/). The significant periodicities are now in agreement with the analysis performed by reviewer 1; the new findings are still consistent with our conclusion.

3.6 Your conclusion and title

I do not see how you demonstrate the relevance of this new dataset for the comparison between MIS11 and MIS1. Was this type of analysis not possible with the older 55 cm sampling? If it was simply not done, you could redo your analysis with the Jouzel 2008 dataset and show how the new sampling improves the comparison between MIS11 and MIS1. From my quick analyis (see section above) it seems that you could have gotten these results also from the coarsely sampled core.

The spectral analysis performed on the high resolution signal reveals periodicities at centennial scale that are not accessible with the previous bag data (please see figure below displaying the wavelet analysis of the 300a resampled signal). The reviewer 1's method highlights more periodicities with the same bag data signal but fail to capture values under ~300 years. Moreover, our variance analysis method based on the calculation of a running standard deviation over 3ka was not possible with the previous bag data because of the lack of a sufficient number of points (10 points for the time interval chosen).



In your conclusion, I miss that the (at least to me) apparent and very interesting coupling between the different frequency scales is not highlighted enough, despite the uncertainty in the frequency analysis. You do get this result also from the variability analysis. In this light, I would like to see the title focus more on the coupling between the frequencies or on the onset of the cooling phase.

Title and conclusion have been modified in that suggested way.

Also, change isotopic to deuterium in title.

Done.

4 Specific remarks

P1783 L1: please use a different symbol for standard deviation (_) here or use a different symbol for diffusion length (also _ in L14). One of these has to be renamed, otherwise readers are confused.

Done.

P1784 L 8ff: please motivate why you do not in general use a common time step of 50 years for both MIS 1 and 11

The text has been completed in that way (lines 171-174).

P1784 L 9: can you quantify the effect of the resampling on the signal? how regular/variable is the age vs depth model in the period of MIS 11, please state a number.

The text has been re-written to give more details about MIS 1 and 11 available temporal resolutions as given by the EDC3 chronology (lines 116-121). Moreover, a reference to Pol et al., 2010 has been added for the visual correspondence between depth and age over the full EDC ice core and the evolution of Bag temporal resolution.

P1785 L20: explain why you do not test against a red noise background as I would expect for this type of record.

Corrected.

P1785 L22: what is the motivation to take p=.95? why not .99? According to Thomson (1990), a good p level would be 1 1=(1 n), where n _ 700 the number of samples in your focus period, i.e., n = 0:998. Are your spectral results significant at this level?

Corrected.

P1786 L1: explain Antarctic Isotope Maximum and give reference

We have removed from the text this wording. There is not yet a clear consensus about the definition of Antarctic Isotopic Maxima. They were defined in EPICA, 2006 for the last glacial period, based on the correlation with the DO events recorded in Greenland isotope records. This wording was then generalised in Jouzel et al., 2007, supp. mat for all glacial periods, but without clearly highlighting the abrupt event of early MIS 11 as a possible AIM. Even though Early Holocene and Last Interglacial maxima have also been recently labelled as AIM events (Stenni et al, 2011; Masson-Delmotte et al, PNAS, 2010), our text has been modified and now refers to "MIS11 abrupt event" (line 226).

P1786 L3: I see no plateau but a small decrease

Corrected (line 228).

P1786 L6: what do you mean by "calculated"?, a reference to figure 1b is missing here

The sentence has been re-written (lines 230-231).

P1786 L10f: depths are wrong, please correct by 100 m (i.e. 2640 to 2740)

Done (line 235).

P1786 L13: not published? please give pers. comm. or indicate whether you are doing this analysis yourselves.

Done (line 238).

P1787 L2ff: indicate focus period also in figure 1, possibly still show (but don't analyse)the period outside the focus in figure 2.

Done.

P1788 L3f: referring to P1787 L12, could the warmer temperatures in the Holocene be responsible for overall increased variability? I would like to see this discussed more.

This is not the main point of our study. We understand the request of the reviewer but have chosen to discuss this point in an other dedicated paper in preparation.

P1788 L6: see comment on P1784 L 8ff

Done.

P1788 L7: add (not shown)

Done (line 287).

P1789 L1: change optimum (valued statement) to maximum (objective)

Done (line 309).

P1790 L7: The 2500 a cyclicity is outside the cone of influence. Is your interpretation valid despite this? Please add a statement.

The significance of periodicities outside the cone of influence may be underestimated not overestimated as stated in the text. Consequently, significant periodicities revealed outside the cone are clearly relevant.

P1791 L17: add reference: DF chronology (Kawamura 2007), expand DF to Dome Fuji

Done (line 379).

P1791 L27: the 1.5 permille drop is very difficult to see, can you better visualize this?

In order to keep a homogenous axis for all panels of Fig. 2, we have prefered not to change the axis units.

P1792 L14: In the light of the EDC multimillenial shift outside the cone of influence, and TEST1 no multimillenial shift, and TEST2 insignificant multimillenial frequency, you should qualify the robustness as applying only to the millenial and submillenial dynamics.

Remark confirmed by the new spectral analysis results: the revised text has been modified to take it into account (lines 339-342).

P1792 L19: system response to orbital forcing, add reference (Milankovitch 1941, or any other of the many)

Done (line 410).

P1797 L15: interest = relevance?

The conclusion section has totally been revised.

P1797 L19: what is "unexpected" about this. I would not expect any relationship between trend and variability pattern a priori.

The term has been removed form the text.

P1800 L12: check status of submitted paper

The paper has been revised.

P1801 L32: add information how to access this conference paper

Done.

P1809 figure caption Fig 2: remove The from The remarkable.

Done.

Also it would help to point to this interesting additional information from the manuscript main text. I only found much of the relevant information I was looking for

in these figure captions, and not in the main text: when you first mention figure 2, say, e.g., Fig 2, including details on significance testing.

This information has been included in the main text (lines 181-187).

P1810 Fig 3 caption. Which confidence levels are marked? You only talk about p=.95 in the text

There is only one p-level taken into account. It is now of 0.99.

5 Technical corrections

5.1 General corrections

I would recommend to reduce the number of hyphened words, such as de-trended (say detrended), pre-industrial (instead: preindustrial), re-sample to resample, possibly submillennial and multimillennial. The spell checker will disagree, but the words form an established terminology in palaeoscience already. Please be aware, that the word 'however' cannot be used at the beginning of a sentence in the sense of 'on the other hand'; put it after the noun or verb and enclose it with commas. There are about 10-20 occurrences of this throughout the text. Use correct prepositions, i.e., consist of, point to, on average, Replace actual (meaning real) by recent (meaning current). Be careful with the use of 'the', especially in conjunction with the Holocene. Use a comma in constructions starting with: 'Nevertheless,' 'Thereafter,' Possibly around 20 occurrences of this in your rtext. You make generous use of the construct 'thanks to' which I do not find appropriate for scientific writing. Due to, because of, derived from, based on... are possible alternatives and should be used instead.

We have carefully corrected the text from such language weaknesses and paid attention to other added remarks.

In the specific corrections below, I did not indicate every occurrence of the above general remarks.

5.2 Specific corrections

P1779 L23f: 'consists' replace with exists

Changed into "relies on" (line 44).

P1779 L25f: 'thousands of' replace with thousand

Done (line 46).

P1780 L9: actual=recent?

Changed (line 56).

P1780 L11: move position of however

Done (line 59)

P1780 L18: Nevertheless followed by comma

Done (line 64).

P1780 L19: add 'MIS, which is'

Done (line 65).

P1780 L20: points to

Changed into “appoints” (line 67).

P1780 L22: end of line add , however,

Done (line 69).

P1781 L2: along = during/within?

Changed (line 76).

P1781 L9: remove obviously

Done (line 86).

P1782 L1f: reverse order: describes past interglacials at a lower temporal resolution

Done (line 106).

P1783 L27: delete ‘As already mentionened’

Done (line 159).

P1784 L27: ‘in the next’ where is the context?

The sentence has been re-written (lines 195-197)

P1785 L12: move however

Changed into “But” (line 207).

P1785 L14: requested=necessary?

Changed (line 208).

P1785 L19: modelling=simulation?

Changed (line 213).

P1786 L9: on average

Corrected (line 234).

P1786 L18: Thereafter,

Done (line 242).

P1786 L26: Thanks to = because of

Changed (line 251).

P1787 L21ff: change 2 permille to 7 permille

Done (line 272).

P1787 L27: subtract from
P1787 L27: one can access to = one gains access to

Corrected (line 279).

P1788 L17: area=period, region?

Changed (line 297).

P1789 L13: delete have

Done (line 322).

P1789 L14: focussed = focus?

Corrected (line 323).

P1789 L15: delete method

Done (line 323).

P1791 L20 :ranged=ranging

Corrected (line 382).

P1791 L 25: delete logically

Done (line 386).

P1791 L28: on average

Corrected (line 390).

P1793 L4: parallel=analogy?

Changed (line 421).

P1793 L6: others=other

Corrected (line 423).

P1793 L6: 'for the MIS 11 period' or 'for MIS 11'

Corrected (line 424).

P1793 L11: the Holocene

"The" removed (line 428).

P1794 L18: better: solar forcing of

Changed (line 430).

P1794 L18: has been = was

Corrected (line 460).

P1794 L20: remove indeed

Done (line 479).

P1794 L26: North = northern?

Done (line 481).

P1794 L26: remove actually
P1794 L26: highlights=highlight

Done (line 445)

P1795 L17: move however

Changed into “Nevertheless” (line 493).

P1796 L9: add apostrophe others’

Done (line 514).

P1795 L15: replace ellipses with e.g.

Done (line 520).

P1796 L2: add space after CO2

Done (line 527).

P1796 L4: given = within
P1796 L10: move however

Corrected (line 536).

P1796 L15: interest = relevance?

The conclusion section has been re-written

P1796 L20: repeated clarification of optimum/maximum, don’t need reference here

Changed (line 556).

6 References

• Jouzel, Jean; Masson-Delmotte, Valerie (2008): EPICA Dome C Ice Core 800KYr deuterium data and temperature estimates. doi:10.1594/PANGAEA.683655
• Masson, V., Vimeux, F., Jouzel, J., Morgan, V., Delmotte, M., et al., 2000. Holocene climate variability in Antarctica based on 11 ice- core isotopic records. Quat. Res. 54, 348–358.
• Milankovitch, Milutin (1998) [1941]. Canon of Insolation and the Ice Age Problem. Belgrade: Zavod za UdzÌE˛benike i Nastavna Sredstva.
• Petit, J. R., Jouzel, J., Raynaud, D., Barkov, N. I., Barnola, J.-M., et al., 1999. Climate and atmospheric history of the past 420, 000 years from the Vostok ice core, Antarctica. Nature 399, 429–436.
• Thomson, D., 1990. Time series analysis of Holocene climate data. Phil. Trans. R. Soc. London B 330 (1615), 601–616.
• Wirtz, K.W., G. Lohmann, K. Bernhardt and C. Lemmen (2010): Mid-Holocene regional reorganization of climate variability: Analyses of proxy data in the frequency domain, Palaeogeography, Palaeoclimatology, Palaeoecology Volume

298, Issues 3-4, Pages 189-200 http://dx.doi.org/10.1016/j.palaeo.2010.09.019

Reference added.

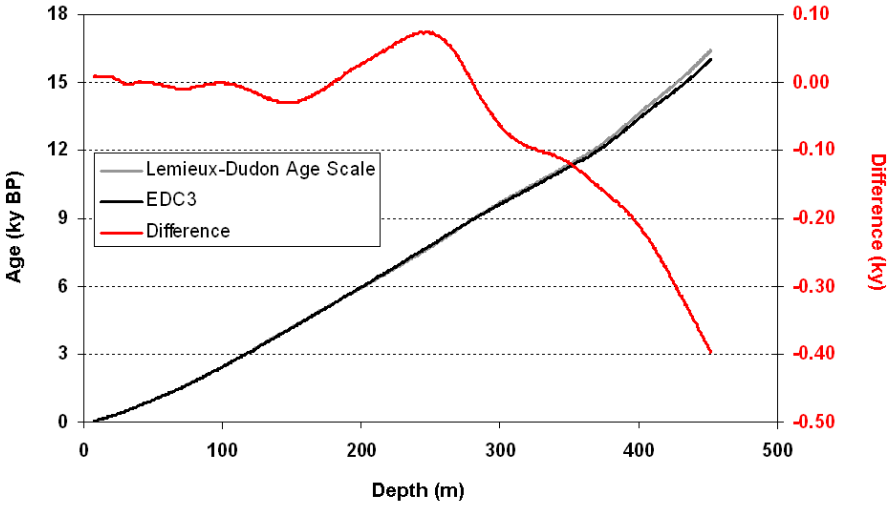
Reviewer #2 M. Mudelsee

Very interesting manuscript: new EDC dD data enabling a comparison of centennial-to millennial-scale variability during MIS 11 with that during MIS 1 (Holocene), generally very well written. But: timescale errors not sufficiently taken into account.

Major criticism.

(1) *Holocene timescale errors are not taken into account. Although these should be clearly smaller than MIS 11 timescale errors, they are certainly larger than zero and should be analysed.*

The maximum estimated error for Holocene is of ~100a. According to the new Lemieux-Duduon age-scale that synchronizes simultaneously 3 Antarctic records (EDML, Vostok and EDC) and the NorthGRIP one, the differences between EDC3 and Lemieux-Dudon age-scale are very small (please see the figure below showing the two age scales as a function of depth and their difference) and have no impact on the spectral analysis as it had been tested (not shown because the result is exactly the same as the Holocene one in Fig. 3). This agreement between both chronologies make us very confident in the Holocene dating and the spectral analysis results.



(2) *MIS 11 (and Holocene) timescale errors not sufficiently taken into account. Analysing just two alternative scales (called test 1 and test 2) is not enough. You should construct a statistical age-depth model, and then perform n_sim simulations: for each simulation, (i) draw a random timescale and (ii) draw random AR(1) values, (iii) combine timescale and AR(1) values, (iv) calculate wavelet spectrum on combined simulated AR(1) series. Then, after the simulations, you take an upper percentile of the simulated wavelet power at each point in the "wavelet domain"; you should take test multiplicity into account by selecting a high enough percentile (see Lemmen's citation of Thomson 1990 or consult Mudelsee 2010: Chapter 5 therein). (If a test is performed multiple times, it becomes more likely to find a significant single result.) Usage of higher percentiles requires higher n_sim; a typical value may be n_sim = 10000. A paper on spectrum estimation (Lomb-Scargle for unevenly spaced series) with timescale errors taken into account is Mudelsee et al. (2009). The wavelet peaks (calculated on the EDC data) above that upper percentile may then be a reliable result and worth of climatic interpretation.*

The correct p-level of p=.99 has now been applied. Moreover we agree with this point and are aware of difficulties about discussing spectral analysis results when taking into account such

dating uncertainties. This is why we have limited our discussion in dealing with the changes in variance and the general dynamic patterns that are not affected by such age scale errors. A supplementary work including statistical sensitivity tests using the glaciological dating tools (Lemieux-Dudon et al, 2010) would bring a real added value to our study, but deserve the development of a specific methodology that could be addressed in a separate paper.

Minor criticism.

The number of minor errors is indeed less than what I normally see in other papers I have to review, but it is still large enough to let me think: "These authors wish the reviewer to do that tedious job" or "Who guarantees that their measurements, data processing or software development are not corrupted by a similar amount of errors?"

Let me give a few examples.

Example 1: Affiliation 4 should be "Niels Bohr Institute" and not "Niels Bohr Institue".

Corrected (line 15).

Example 2: "Siegenthaler" is the first author of the EPICA CO2 (late interval) paper and not "Siengenthaler".

Corrected (lines 40, 52 and 858).

Example 3: You define "MIS" on page 1779, line 3-4 as "Marine Isotopic Stage" and on the same page, line 25, as "Marine Isotope Stage".

Corrected (lines 25 and 47)

Example 4: You cite (page 1785, line 4-5) a QSR paper for detailing the "mathematical formalization" of wavelet analysis, instead of consulting original mathematical sources (for such, see e.g. Mudelsee 2010: page 217-218 therein).

Reference added.

A short comment on Lemmen's review comment:

Section 3.1 (dataset publication): agreement.

An excel file including data has been attached to the manuscript.

Section 3.2 (time axis): no agreement, it is paleoclimatic convention to plot time from right to left.

We have decided to keep the time axis following the common use in the paleoclimate community.

Section 3.3 (figure quality): agreement.

The figure quality has been improved. We hope that the revised figures are now suitable for publication.

Section 3.5 (reanalysis): this is not sufficient because timescale errors have not been taken adequately into account (see major criticism 2 above).

References:

Mudelsee M, Scholz D, Röthlisberger R, Fleitmann D, Mangini A, Wolff EW (2009) Climate spectrum estimation in the presence of timescale errors. Nonlinear Processes in Geophysics 16:43–56.
Mudelsee M (2010) Climate Time Series Analysis: Classical Statistical and Bootstrap Methods. Springer, Dordrecht, xxxiv + 474 pp [www.manfredmudelsee.com/book] Interactive comment on Clim. Past Discuss., 6, 1777, 2010.