Clim. Past Discuss., 7, C2519–C2522, 2012
www.clim-past-discuss.net/7/C2519/2012/

Climate of the Past Discussions

Interactive comment on "Correlation of Greenland ice-core isotope profiles and the terrestrial record of the Alpine Rhine glacier for the period 32–15 ka" *by* M. G. G. De Jong et al.

Anonymous Referee #2

Received and published: 30 January 2012

The manuscript deals with two topics: 1) stratigraphic division of a period in the late glacial (roughly MIS2) and synchronization of the deep Greenland ice cores using a technique called spectral trend analysis applied to previously published d18O and Calcium profiles 2) a proposed link between the Greenland ice cores records and records of ice advance/retreat in the Rhine glacier area

Ad.1 The spectral trend analysis has previously been applied to Greenland data by some of the same authors. The method is well described in a technical sense, but the interpretation of the generated INPEFA curves is a purely time-series-analysis-based exercise with no direct or obvious climatological interpretation. The authors subdivide

the records into a large number of units, ordered hierarchically in 4 levels, each unit roughly corresponding to a cooling and a subsequent warming. This contrasts with the conventional view in which the Greenland Interstadials (GI) are recognized as warm excursions from a glacial background state, a view that is supported by large amounts of published work that discuss the physical mechanisms behind the GIs. The spectral trend method instead splits the sequence into a number of units that may (or may not) have significance from a time series analysis point of view, but bears no direct physical interpretation. The limitations of d18O as a temperature proxy are largely ignored and large emphasis is put on very small details in the curves that are not always represented in all 3 cores, and thus may be of local origin. The authors describe their approach as a way to emphasize the multiscale nature of the climate variations, but I do not find this argument convincing: i) The first stratigrapical division basically covers the whole period where they have data from all three cores and thus carry little significance. From fig, 2 it is quite unclear why the lower boundary could not be placed roughly at GI-6 ot GI-7 instead of roughly at GI-5 (expect from the fact that the published data on which the analysis relies do not reach that far back in time). ii) The second order division is essentially the as the GS/GI division of Rasmussen et al. 2008 except from that the boundaries chosen from visual inspection of the INPEFA curves tend correspond roughly to the midpoints of GIs due to the different convention in event definition. iii) The third and fourth level division relies on small wriggles in the data that are not always present in all cores. The division is presented on the figures but the criteria for what constitutes an event are not clear, and even if they were, I seriously doubt that the features that are used for the division are significant from a climatological point of view. The features are apparently found independently in all 3 ice core records, and comparison of the location of the divisions derived from the three cores (tentatively illustrated by the green lines that connect the parts of fig. 4) represents a depth-to-depth match of the Greenland records that will differ from that of Rasmussen et al. 2008. In order for the manuscript to make any additions to this field of work, the authors should compare their depth-to-depth relationship derived from INPEFA-based synchronization to that of the (mainly) volcanic horizon match of Rasmussen et al. 2008 and make a convincing case that their division is more meaningful and makes glaciological sense. In summary, as it stands now, the proposed synchronization and stratigraphical division add very little compared to existing work: level 1 seems insignificant, level 2 seems to be essentially a copy of the existing stratigraphical framework (but in my eyes with a less physically meaningful boundary definition), while the third and fourth levels seems based on unclear and subjective criteria applied to the small details in the INPEFA curve whose physical relevance is largely uncorroborated.

Ad. 2 There seems to be little dating control between the Rhine glacier data and the Greenland ice core records. The "equating" of glacial advances/retreats to the stratigraphy framework described above seems mainly based on uncorroborated assignment of significant physical/climatological meaning to sometimes small details in the INPEFA curves. The errorbars are not discussed, or discussed very briefly, and are not represented in graphs. Within the combined errorbars of the ice core dating and the 14C dating and calibration, many other ways of "equating" level 3 and 4 stratigraphical units with the Rhine glacier events are possible, and no argument is made why the proposed assignment is superior. The correlation can thus not be underpinned by the dating of the individual records alone, so an understanding of the underlying mechanisms is essential, and this is absent. The reason why small-scale variations in a linear filter prediction error derived from Greenland ice cap proxies should be related to the main and direct control of glacial dynamics in Germany/Austria is not discussed, and even if this connection was established, the existence of lags in the climate system is ignored. Furthermore, even if one accepts the statement on page 4354-5 that "It is generally accepted that the lag time between climate (temperature) change and mass balance change . . . is a short one", assuming fast reaction in the margin position due to mass balance changes is a stretch, and the synchroneity of local temperature change (the cause of the mass balance changes) with Greenland d18O is uncorroborated. The link between parts 1 and 2 is weak: After setting up such a comprehensive stratigraphical framework, I would have expected a rigorous correlation procedure between the

Greenland and Rhine glacier records rather than one based on "equating" events with little basis in data or dating.

It is quite clear that the authors have put in a lot of effort into this work, and the manuscript as such is well written, but I simply think that the authors push the spectral trend method too much without sufficiently considering what the physical background of the results are. In my view, a meaningful stratigraphical subdivision of a climate record should start with a discussion of the physical or climatological hypotheses that lead to the criteria applied to make the division, and not be the product of a clever mathematical time series analysis tool, no matter how useful that tool may be for other applications (in this case: wireline log analysis). In summary, I do not think this manuscript represents a significant advance, and that the shortcomings mentioned above are of such fundamental nature that the manuscript should be rejected.

Interactive comment on Clim. Past Discuss., 7, 4335, 2011.