Clim. Past Discuss., 7, C2609–C2615, 2012
www.clim-past-discuss.net/7/C2609/2012/

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.

M. G. G. De Jong et al.

mdejong@susures.nl

Received and published: 10 February 2012

We offer the following comments on the criticism of Anonymous Referee #2 (AR2).

What is our study about? Using the time-equivalent, predominantly volcanic, events of Rasmussen et al. (2008) as guidance, De Jong et al. (2009) have identified match points and trends in the ice core data and used these to generate a stratigraphic division of the interval 23-15 ka. Building on this work, we present a newly extended analysis of the interval 32-15 ka. The boundaries of our stratigraphic scheme delimit packages which we interpret, based on their 'content', as climatic oscillations. The packaging shows a multiscale pattern. We compare and correlate the discontinuous

terrestrial record of the Alpine Rhine glacier, which shows a distinct multiscale pattern, with the Greenland ice-core record, and conclude that the "correlation between the two areas is sufficiently good to propose that the pattern of climate change was synchronous (within the available time resolution) between Greenland and the Rhine glacier area." A major point of our work is that our understanding of the each of the records benefits from the comparison and correlation with the other.

Below are our responses to specific points of criticism of AR2.

Ad.1 Statement of AR2: "...the interpretation of the generated INPEFA curves is a purely time-series-analysis-based exercise with no direct or obvious climatological interpretation".

Referring to Section 2 of the manuscript, we would like to emphasize that we analyze and interpret the patterns in the unprocessed δ18O (and [Ca2+]) data. The INPEFA curves are an important tool for detecting patterns in the unprocessed data. δ18O and [Ca2+] data being proxies for climate change (refer to Section 2), our interpretations of the unprocessed data are climatological (with focus on stratigraphy). See also our published comments on the report of Anonymous Referee #1.

Statement of AR2: "This contrasts with the conventional view in which the Greenland Interstadials (GI) are recognized as warm excursions from a glacial background state.......".

Our findings, indeed, are not in line with the conventional view. We observe that the δ18O data in our study interval between 23.4 and 14.7 ka are overall higher than those before 23.4 ka. This change is clearly expressed in the INPEFA curve. See tracks 3 and 4 in our Figure 2. A similar pattern is present in the GRIP and GISP2 data (see Figure 1 of De Jong et al. 2009). We interpret this pattern to be significant in climatological terms: a period of predominantly cool conditions followed by a period during which the conditions overall were slightly warmer (without quantifying it). Corroborative evidence for this interpretation we find in the correlation of the terrestrial proxy from the northern

Alps with the Greenland proxy. Although discontinuous, the terrestrial proxy shows beyond doubt an overall glacier advance followed by an overall glacier retreat, with a timing of events remarkably close to the observed major pattern in the Greenland proxy.

Statement of AR2: "The first stratigraphical division basically covers the whole period where they have data from all three cores and thus carry little significance".

We have data (GICC05 time scale) older than 32.4 ka only for NGRIP. We should have stated explicitly in the text that we consider the data of NGRIP for this period to be representative (we will do so in the next version of the manuscript). The period 42 to 32 ka in the NGRIP ice core (see Figure 2) shows a larger number of the so-called warm excursions (GI-10 through GI-5) than the subsequent period. The Upper Würmian, as opposed to the Middle Würmian, is characterized by large-scale glaciation of the Alpine area and foreland. The exact time boundary between Middle and Upper Würmian being subject of debate, it is commonly placed around 30 ka. Combining the information from the two proxies, we feel that our Last Major Weichselian Oscillation (LMWO) is 'significantly' defined at the lower boundary of 32 ka. The upper boundary of LMWO is above the study interval, at the well-defined, significant, Weichselian-Holocene boundary (Section 4.2).

Statement of AR2: "The second order division is essentially as the GS/GI division of Rasmussen et al. 2008 . . .. . .".

The division of Rasmussen et al. 2008 and our scheme are based on the same data. It is therefore no surprise that there are similarities. Our proposed division is based on the concept of climate oscillations. The level 2 division is part of a multiscale stratigraphy. In other words, the differences between the division of Rasmussen et al. 2008, which is based on the concept of warm excursions from a glacial background state, and our division are substantial.

Statement of AR2: "The third and fourth level division relies on small wriggles in the

data that are not always present in all cores. . .. . ..".

The level three and, even more so, the level four units are based on sometimes rather subtle changes in the δ18O and [Ca2+] data, highlighted by the INPEFA curves. It goes without saying: the degree of uncertainty in the interpretation increases with increasing order of stratigraphy. Our method of assessing the relevance of these "small wriggles" – and generating a core-to-core correlation framework - is pattern analysis & graphical correlation: we look for patterns in the (original, unprocessed) data, facilitated by graphical enhancements and the spectral trend curves. Patterns are compared between the ice cores. We do not rely on the data of one ice core, we use three. Essential in this approach is that each time we find more or less the same succession of features in data series, we consider that we are reducing the risk that they are random (although we do not try to quantify this). We look for correlatable temporal variations, taking into account that the patterns between ice cores may be different due to spatial variations (which may have many causes). In addition, rather than using one curve (i.e. δ18O) we use two (δ18O and [Ca2+]) – which should further reduce error and/or uncertainty. We consider the green lines in our Figures 3 and 4 to mark trend changes in the (unprocessed) data sufficiently distinct in a temporal sense and sufficiently laterally persistent to be stratigraphically (and, thus, climatologically) significant. Units bounded by green lines, at the same stratigraphic level, we interpret to be climate oscillations; refer to Section 3.1 of our manuscript for a definition. With regard to the suggestion of AR2 to "compare their depth-to-depth relationship . . ..to that of the (mainly) volcanic horizon match of Rasmussen et al. 2008. . ..." we refer to Figures 2 and 3 of De Jong et al. 2009. Our stratigraphic work very much honors the work of Rasmussen et al.

In a reaction to the conclusions of AR2, we see our work as a new look at an old problem, with results that are sufficiently solid and presented with enough attention for uncertainty to be published. As stated throughout our manuscript, the integrated analysis reveals the multiscale nature of the ice-core and terrestrial proxies over the study interval. This element appears to have been largely overlooked so far in the analysis of the ice-core data – and this must be considered an important contribution to the ongoing research in this realm. The high-order division of the ice cores is also a new. It is based on a data-driven approach. It has physical meaning in terms of the input data. We interpret the high-order information in terms of stratigraphy. We do not present an in-depth explanation in terms of dynamics. For a discussion of this issue we refer to our Interactive comment on the report of AR1.

Ad 2. The second major point of criticism of AR2 revolves around the issue of uncertainty. We offer the following comments.

Under Ad 1. we have already commented on the issue of uncertainty in the identification of meaningful correlatable boundaries in the ice-core data, at all levels of the stratigraphy. For additional detail, please refer also to our Interactive comment on the report of AR1. In summary, the way we address uncertainty is not purely statistical, it is integral part of our method of pattern analysis & graphical correlation – we consider it systematic, solid and properly documented in the manuscript.

Comparing continuous with discontinuous records is always challenging and exposed to uncertainty. In our case, we do not rely on age datings only (linking 14C analyses, with sometimes large ranges, to the GICC05 time scale), we also compare the proxies through pattern analysis. We feel that we have clearly presented how we compare the main patterns of the proxies, what type of boundary we correlate, whether boundaries are distinct or less certain, and the available 14C age-dating control – thus, addressed uncertainty. We have also outlined the context within which we propose the correlation (Section 6.1), arguing that the proxies have enough in common to justify proposing a correlation. What we have not discussed explicitly – a point that is implicitly raised by AR2 – is a potential revision of the stratigraphic scheme of the terrestrial proxy based on the correlation with the ice-core record: the Higher-order Events & Recessional Complexes (RC) of our scheme (Table 2) may not all be of the same order. We will add a discussion of this topic in the next version of the manuscript.

Our study essentially is a stratigraphic study. The underlying mechanisms of the records, the climate dynamics, are not the main topic of our study (refer also to our Interactive comment on the report of AR1). We acknowledge the importance of understanding the mechanisms that generate the stratigraphic record. A full understanding of all processes, in their interactions, is, however, not critical in making a core-to-core correlation framework the way we do it, and in erecting a corresponding stratigraphic scheme. Conversely, stratigraphic studies may reveal patterns that may guide or constrain studies of dynamics. We feel that our study demonstrates the value of such an approach. On a similar token, the records which are subject of a comparison all become better understood, and the study of the underlying mechanisms may benefit from this. In this context, it is worthwhile to take into consideration that many, if not all, patterns in the ice cores were observed before understanding of the underlying mechanisms developed.

With regard to his/her concluding comments, we disagree with AR2 that we "have pushed the spectral method too much without sufficiently considering what the physical background of the results are." With the help of the INPEFA curve we have detected patterns in the unprocessed data, patterns that have been largely overlooked so far, patterns (though subtle they may be) that are correlatable between the ice cores. We feel that this information cannot be ignored or be rejected as being random. We also disagree with AR2 that our results are the "product of a clever mathematical time series analysis tool. . ." The INPEFA curve reveals and/or highlights trends and trend changes in the input data (see Figures 2, 3 and 4 of our manuscript), it does not create patterns that cannot be seen in the input data.

Our empirical data-driven approach is founded on sound scientific principles, our results are reproducible, our discussion of uncertainty is in line with common practice in stratigraphic correlation, and our results are new. Clearly, AR2 advocates a different approach – which we respect. We are of the opinion that both approaches are valuable (indispensable) to improve the understanding of climate change (stratigraphy, dynamics). We hope that AR2 is willing to reconsider his/her recommendation for rejecting the manuscript on the basis of the comments we have offered on his/her criticism.

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