Clim. Past Discuss., 8, C802–C806, 2012
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Climate of the Past Discussions

Interactive comment on "Automated ice-core layer-counting with strong univariate signals" *by* J. J. Wheatley et al.

Anonymous Referee #1

Received and published: 11 July 2012

General comments:

Annual layer detection and counting is a key tool for ice core research and similarly varve counts, tree ring counts etc. There is a need for automated method for annual layer counting and evaluation of the uncertainty of the counting procedure, and as such, the study is appropriate for publication in CP.

The manuscript presents a clever way to automate annual layer counting by splitting the signal in question into "easy" and "hard" sections. In the "easy" sections, annual layer boundaries can be assigned in an unambiguous way without much difficulty, while the "hard" sections are highlighted for manual evaluation or handled by assigning probabilities based on annual layer thickness statistics. The automated assignment of annual layer boundaries in the "easy" section is convenient, but will probably in the case of

long data series only represent a modest improvement and/or work saver, as it not a difficult or laborious task to assign annual layer makings to a single data series with a well-characterized and strong annual signal. The real value of the methods thus lies in its ability to perform well when applied to the "hard" sections, or "issues" in the nomenclature of the manuscript, and especially its performance in difficult case, e.g. when either the annual signal is unclear or when the data has problems with marginal resolution, frequent missing data sections etc. Unfortunately, the results presented do not allow a full assessment of whether the method performs well under these more difficult conditions.

Specific comments:

The method performs well when applied to the test data presented, but that is not very surprising given the unusually fine data quality - the sampling rate is very high and the noise/non-annual part is weak – and the exceptional simple, regular and well-expressed annual signal.

As I state in the general comments, I think the method is clever and has potential, and I think that it should be published, but I think the current manuscript is almost too pretty and too much based on low-hanging fruit to allow the reader to evaluate the potential strength of the method: Given the quality of the data and the limited length of the test data section (153 years), any reasonably successful method should be able to produce a count close to the target. Thus, the true potential of the method remains to be demonstrated. In short, it would be interesting to test the method's performance on data with less favourable sampling rate and on data with a less clear-cut annual signal.

With regard to the sampling rate, a data series with $\sim$8 samples per average year would be a good test. If the authors have no other suitable data for a test along these lines, a down-sampled version of the data used in the study could be used.

With regard to the complexity of the annual signal, there are several possible tests that could substantiate the results: - Using data with a more complex annual signal, for example ECM data, water isotopes, or the (challenging) Visual Stratigraphy data of Winstrup 2011). - Addressing the method's sensitivity to annual layer thickness variability, which seems unusually small in the data set used, potentially because the data are from a high accumulation site which has many annual precipitation events. Annual layer statistics could be used to evaluate if the test data used in the manuscript have unusually low layer thickness variability, i.e. when compared to data from records with thinner annual layers (both Rasmussen et al. 2006 and Andersen et al. 2008 already referenced presents relevant Greenland statistics), and a comparison to e.g. WAIS layer statistics would also be relevant if available.

Another comment relates to the issue reconstruction probability assignment on and around p. 2487/14: The method as described and Fig. 5 indicate that some of the data that actually are available are disregarded as the probabilities are based solely on layer thickness statistics. Maybe the authors could think of a clever way to utilize the (dis)similarity of the different reconstructions and the available data across the "issue" (i.e. the black curve bits on Fig. 5) to refine the reconstruction probability assignment.

Also, the way issues is handled statistically in section 4 implies that the true curve shape (disregarding sampling problems and missing data) is a sine. I fear that this way to interpret sections of difficult data is too simplistic, especially in the case where the difficulties could also be cause by a less well-behaved signal. This calls for tests like the one indicated above, and/or a discussion of how the method or pre-processing steps could/should be adapted to different data characteristics.

Finally, I miss a discussion of the potential (if any) to extend the method to multiparameter data.

The authors mention in the very last lines of the conclusion that work is under way to address some (if not all) of the above-mentioned issues, but I really miss some of these results and discussions in the current manuscript.

The manuscript text is not very long, but still describes the details of the rather simple

method in at least sufficient detail. The abstract is fine and the language as well as the artwork and technical quality is good. Relevant annual layer detection / counting work is referenced, and the briefness of the reference list mainly reflects that automated annual layering methodology is an emerging field. Maybe the authors would like to elaborate a bit on how their method assumptions compare to those applied by some of the referenced works and to varve data as part of the BMPix tools of Weber et al.. The number of figures is on the high side, but many can be reproduced in small size if the legends etc. are sized appropriately. Fig. 4 carries little information in itself, as almost everything is reproduced in Fig. 5. Also, Fig. 6 is not essential. Figs. 10 and 11 could be integrated as the captions are almost identical.

In conclusion, I think that the current version presents a nice and clear description of 1. a few elegant and well-chosen data pre-processing steps, 2. a simple and efficient way to characterize the annual signal in the "easy" parts of the record, 3. a simple way to assign probabilities to different number of annual layers across difficult / reconstructed parts of the record based on layer thickness statistics 4. the results themselves and comparison to the results of the manual count. 5. sufficient test of parameter sensitivity etc. However, the manuscript fails to sufficiently demonstrate that the method represents a significant advance because the test data chosen are not sufficiently challenging. With very few news among the elements of the presented method (possibly with the exception of the assumption 2486/3) and no demonstrated performance on difficult data, I think the calorie count is on the low side.

To add some weight, I therefore suggest that - the method in its current form is applied to a more challenging data set in order to test whether the method represents a significant improvement compared to much simpler methods (e.g. by addressing some of the comments and suggestions above), or - the manuscript is seen as the first part of a manuscript that in its second part will elaborate on at least some of the material which is mentioned as ongoing work in the outlook In the first case, the manuscript's score on "Scientific Significance" will likely increase to 1 or 2 and would be recommendable for publication with minor technical/revisions.

Technical Corrections / Minor issues: 2478/22: While it is clear that noone has yet presented a statistically rigorous treatment of uncertainty of annual layer counting, it's not quite fair to say that "Little consideration" has been given to the issue. 2479/5: Winstrup now has a CPD reference that could be added. 2479/26: Why a sine wave? Even though the initial UV forcing may be close to sinusoidal, the log transform should change this, and unless the mean is thought to vary within each annual cycle (in which case any signal with a quasi-periodic behavior can be thought of as a sine wave on a non-linear time-scale with varying amplitude and mean), there is no basis for assuming that the signal resembles a sine. The statement needs clarification (e.g. what is meant by varying amplitude and mean) or can be removed. 2480/25: As above. No convincing arguments or evidence for the signal being sinusoidal is presented. 2484/3: . . . or sections where the annual signal simply isn't sufficiently clear cut. 2485/20: Does this sentence end like it should? 2486/18: This symmetry may not hold for other data sets, esp. after normalisation and log transformation. The authors could address what would happen in this case. 2488/5: model's

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Interactive comment on Clim. Past Discuss., 8, 2477, 2012.