

Interactive comment on “An automated approach for annual layer counting in ice cores” by M. Winstrup et al.

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This paper describes a new approach to automated layer counting in ice cores. This is a really important topic: layer counting is used (in conjunction with volcanic matching) to date numerous shallow ice cores, and has been used to date much longer ice cores. Most notable of these long counting age scales is the GICC05 scale used back to 60 ka for Greenland ice cores. This age scale is used as a reference in numerous palaeoclimate studies, but it involved many person years of effort. Furthermore, despite the best intentions of those involved, decisions about when a year is counted are somewhat subjective, and the uncertainty placed on the age scale even more so. An automated method, if it can be established, would offer much faster counting, the opportunity to codify the subjective decisions and test their importance, and a clear way

of setting an objective uncertainty on the age scale. However, it is a hard mathematical problem – a number of simple approaches have been tried but found to be of limited use.

This study is a technically challenging attempt to create such an automated system. It uses strong mathematical techniques (that I hope other reviewers will be more qualified than me to comment on), and represents a very good first attempt and first step. A particular strength is that it tries to really analyse what it is that manual counters do, in order to try to mimic that. This is perhaps its weakness as well, in that manual counters may not always be following consistent and justifiable rules, so that it could be dangerous to use the fit to manual counting as the test of the system's success. However this may be an issue for a later iteration of the method to worry about. Considering the complexity of the techniques, the work is explained in a very clear and logical progression. This is not a finished method – the method cannot yet use multivariate data, and seems to require some subjective decisions at the start (whereas it would be good to include the subjectivity into the uncertainty estimates). However, I believe it marks a significant step towards a possible “universal” method that, taken together with other parallel work, could lead to a really good method in the near future. I do have some minor points, but overall I think the paper is in good shape and deserves to be published in CP. Some of my comments are not really criticisms of the paper, but are more in the style of philosophical musings on the method, that might provoke some interesting discussion, or an added sentence.

Introduction: I think more should be made of the importance of getting an objective error estimate. For me, this is the crux of the improvement automated methods allow – if I make a range of reasonable decisions about what constitutes a layer, what is the range of ages I will reach? This is what manual counting simply cannot do, because it would require each sequence to be counted many times. Could you add more about that?

Page 2521, line 15. I think you should clarify that while the method allows for multi-

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parameter counting, it has not yet been attempted.

Page 2521, line 16. While we know from modern measurements that dust input to Greenland is seasonal (with one season showing highest values), we would have to admit that we cannot be absolutely certain this was the case in the last glacial. This assumption (that the same applies to the glacial) should be stated (and is an argument for multi-parameter).

Page 2522, line 15-18. I don't think this paper requires such a long list of HMM applications.

Page 2523, line 23: t should be inside the bracket, "(e.g. time, t) is considered"

Page 2525, line 11-13. This is crucial, because you are asserting that this is the basis of manual counting and therefore the basis for the automated method. I think it is well-stated here, and should be made quite prominent. What is not so clear is how the two issues are played against each other, which may be the basis of subjectivity – how thick will I allow a layer to be before I decide a layer must be present even though the shape and amplitude are wrong? Is it worth saying something about this?

Page 2532, line 20 "deduced" not deducted.

Page 2536. This whole idea of the sensitivity tests on artificial data is a good one, but is not described in enough detail here for the reader to judge. This section reads like a précis not a paper. I think you need to show a figure or else give a longer description. I don't think the reader would understand what you have done here. Page 2537, lines 1-10 is also hard to follow. Please re-write this section.

Page 2539. I don't think you should see it as the right test that you agree with the manual method. Rather I think this test shows that you have understood the manual method well enough to codify its assumptions. That means that now you are in a position to test how robust and important those assumptions are. With apologies to some of your co-authors, manual counters could be wrong! This thought actually leads

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on to the top of page 2540 when the optimistic uncertainty arises because you have not yet fully explored the range of parameters that might reasonable be altered.

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