

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

### **Anonymous Referee #2**

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#### General Comments:

This paper describes a useful method for identifying annual layers in ice cores. Hidden Markov Models provide an appropriate approach for characterizing the strength of a signal and the probability of individual peaks being present based on neighbors. The ability of HMM to provide an uncertainty estimate is very valuable. One of the constraints on the described approach is the assumption of stationary parameters within a model run. However, this assumption is addressed in part by implementing the algorithm as a set of sequential runs on short stretches of data. A wavelet or other frequency-based analysis of the dataset may be helpful for determining an appropriate segmentation of the dataset prior to dating with this method.

If the PCA was calculated using the entire width of the image, as seen in Figure 1, then

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it is possible that selecting a narrower band of the image may provide better results. Variability in layer position across the scan that appears as wiggles in fig. 1 (or inclined layers elsewhere) would result in aliasing of these features in PCA axes 2 and higher. In some cases this might act as a type of edge detector that helps to define a layer, but that may not always be true. It would be interesting to know if a relatively narrow strip of values averaged by depth with less aliasing (but wide enough to average out bubbles, scratches, etc.) would perform as well as a PCA calculated from the full image width.

#### Specific Recommendations:

It is unfortunate that the analysis did not also include a shallow dataset with known volcanic tiepoints so that the accuracy could be assessed in more quantitative terms. I believe that some of the statements regarding the correspondence between the HMM method and the manual GICC05 chronology are overly optimistic (pages 2539-2540). The authors take an overlap in uncertainty bounds between the automated and manual counts as indicating correspondence. For example, it is stated that there are only two regions where the counts are outside the confidence intervals in figure 5c. Given the lack of statistical significance tests, it would be prudent to point out that the estimated thicknesses from each method are outside the others confidence interval more often than not. In the later discussion of GI-12 and table 1, the very slight overlap of tails between the automated and manual counts is mentioned as suggesting that the two are not entirely dissimilar. Given that the GICC05 chronology is not independent of the NGRIP dataset, a more stringent interpretation that is more in line with a pair-wise test of variance might be appropriate. Table 1 suggests there would be an extremely high degree of significance in the difference. This does not definitely imply that one result is more correct than the other, but especially in the case of figure 5c, the fact that often just the extreme tails of the distributions are overlapping is important and needs to be acknowledged.

This method shows very good promise for future development, and the authors have indicated useful directions to explore. Among these, a method of passing informa-

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tion from adjacent segments of the piecewise processing seems a high priority given their interpretation of the excursions in figure 5c. While comparisons between different studies are difficult, I note that in the conclusion the authors describe this approach as having “high skill,” while their discussion in the background section was entirely dismissive of prior studies that had equal or better statistical results. It would be useful to put the current results more in context to prior work.

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Interactive comment on Clim. Past Discuss., 8, 2519, 2012.

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