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Nonlinear Processes in Geophysics
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Discussions

Interactive comment on "A sequential Bayesian approach for the estimation of the age–depth relationship of Dome Fuji ice core" *by* S. Nakano et al.

Anonymous Referee #2

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The authors develop a method for estimating the age-depth relationship for an ice core, which lessens the assumptions about the error structure for deviations from expected values of thinning and accumulation rates. They apply the method to obtain an age-scale for the Dome Fuji core.

General comments:

The method is new and very interesting, and the paper is reasonably well-written, although it could be written more to-the-point. The description of the mathematical model is sometimes difficult to follow, and would benefit from adding more explanations. I think the described method has a lot of potential, although for several reasons I am not

convinced about the quality of the age model derived here. In this review, I will mainly focus on the glaciological context and implications.

Very importantly, I am missing a description of the glaciological context for development of the age model, as well as an interpretation of the results. This include: 1) A discussion about the input assumptions for the age model, and whether these are good assumptions. 2) A comparison to previously published ice core age models for Dome Fuji (their assumptions as well as corresponding age models and modelled uncertainties). Such comparisons will also help to evaluate the performance of the technique as the authors promise in the abstract. It is unclear to me whether the age model developed here is an improvement to previous age models, which may have less strict assumptions regarding, for instance, steady-state of the ice sheet.

The developed technique rests upon two major glaciological assumptions, on both of which I would like to see an expanded discussion:

When developing the equation for the thinning factor (eq. 9), the authors assume a steady-state and hence no change in accumulation or ice sheet elevation over time. Yet, the age model spans several ice ages and interglacial periods, during which the ice sheet thicknesses and accumulation rates must be expected to vary. Indeed, according to Parrenin (2007), past elevation changes up to 120m are likely at the Dome Fuji site, and the effect on the thinning function are bumps corresponding to these variations in ice thickness over time (see figure 5b, Parrenin 2007). While the authors of this paper do mention the steady-state assumption, it lacks a thorough discussion, and if possible an investigation, of how this impacts the resulting age scale. I will even suggest the authors to consider to include the changes in elevation over time as another hidden variable to be estimated using the PMCMC technique.

The thinning factor is thus calculated based on a steady-state assumption, assuming e.g. a constant accumulation over time. Yet, the authors subsequently assume the accumulation rate to be related to past temperature, and thereby oxygen isotope values from the Dome Fuji ice core. The authors should make this inconsistency clear to the reader. It should also be mentioned that recent research has shown that using such relationship is oftentimes a poor assumption (e.g. WAIS Divide Members, 2013). While accumulation rates are indeed very affected by climate, there are sudden periods during which the relationship between accumulation rates and isotopic values does not hold. Please discuss this aspect.

Indeed, to some extent the model does allow deviations from the steady-state thinning function and expected accumulation rates based on the isotope profile, and exactly this is one of the major forces of the described technique. However, two things are not accounted for or discussed by the authors: a) In both cases, the difference between the estimated and true values will be very strongly correlated with depth. In the paper, the error estimates are described as white noise, i.e. independent with depth (P. 495). b) Further, given that 1 m of ice contains substantially more years in the deeper part of the core, is it reasonable to expect that the discrepancy from the expected values as accumulated over 1 m are the same in top and bottom part of the core? (I would suspect these to be significantly larger in the bottom part). As this method is developed to allow more flexibility in the error structure, it is unfortunate that the assumptions of the underlying errors are sub-optimally chosen. Does the model have sufficiently flexibility that these error structures can be changed into more appropriate ones?

Further, the paper should include a description of the age markers used in the model, and their associated uncertainties in terms of depth as well as age. There are many kinds of age markers, with very different properties in terms of their uncertainty. It appears that the authors use O2N2 markers, which have age uncertainties of maybe 2000 years. Which values and how are these uncertainties accounted for here? How many tie points are used? How are they spaced? Could other age markers (such as volcanic horizons) be used in addition to these? It would also be an idea to select a subset of these age-markers, repeat the analysis, and compare the resulting ages at depths corresponding to the age markers omitted for age-scale construction. This

would allow another estimate for the validity of the corresponding timescale.

Finally, the technique relies on prior distributions for the involved parameters. But nowhere in the text is it described how these are obtained, or which values are used.

Specific comments:

It would be very helpful for the reader if the authors provide a table with definitions of the many variables employed.

P: 940, line 17, P. 941 line 14-20: Without assuming linearity or Gaussianity - of what? Please make sure that this is clear throughout the text. The technique assumes Gaussianity of age markers etc.

P. 941, line 3-7: Please expand on these earlier approaches where Bayesian and MCMC methods are used for estimating the depth-age relationship.

P. 944, line 18: It might be worth a mention that recent research (Freitag, 2013) has shown that thinning may also be affected by impurity content.

P. 945, line 21: Are the O2N2 tie-points assumed to have no depth uncertainty? If so, is this a reasonable assumption?

P. 946, line 8-13: Discuss why this type of equation is chosen to translate from isotope values to accumulation values. Provide reference(s) for previous usages of similar equations.

P. 946, line 18: How often does it happen that isotopic data is missing for a 1 m section? (My guess would be that it is very rare)

P. 948, line 8: Please explain how we would know that the uncertainty is “too large”

P. 948, line 10: It is not the uncertainty of the d18O data that gives rise to the deviations described by σ_w; it is the flaws in model used for predicting the accumulation rates based on the isotope values. Hence this parameter does not have any significance in terms of standard deviation of the isotope values.

P. 948, line 15-20: Describe the advantages of using the hybrid method.

P. 949, equations 25-30: I do not understand these equations. What is meant by the notation {X_o:z-1|z-1}? Define delta and N used in equations.

P. 950: It would facilitate understanding if the authors included a figure illustrating the method.

P. 954, line 3: Why is every 5th iteration retained? Is this number based on a correlation analysis of the MCMC samples?

P. 954, line 6: Which values were used as priors for θ? Surely, the result will be very dependent on what is used for priors?

P. 954, line 17-19: I suggest the authors to spend a little more time reflecting on the difference in the results obtained here relative to those in Parrenin 2007. If, as suggested by the authors, the obtained velocity profile in the ice sheet (reflected in parameters p and s) really depends so significantly on the isotope-modelled accumulation rate - which in best case is a rough approximation - this is not very encouraging for how well an age-model can be constructed away from age markers.

P. 954: How well does the resulting age scale match the age markers? Does it correspond to what was expected?

P. 954, line 6: 5 trials were performed starting from random seed; could the results from these be combined for the final results?

P. 954, line 20: How does the timescale compare to the one obtained by Parrenin, 2007? (this should also be added to figure 2)

P. 955, line 6: The thinning factor shown here is significantly different from the one obtained in Parrenin 2007. Why is that? What would be the thinning function simply based on the initial non-steady-state version with parameters as e.g. given by the mode

of the obtained posterior distributions?

P. 955, line 11: Similarly, how does the initial estimate for accumulation-rates look based on the simple isotope model that forms the basis for the analysis? And compared to the estimate from Parrenin 2007? It would be great, both in figure 4 and 5, to include the results from Parrenin 2007, so that it possible to evaluate the difference between the two.

P. 955, line 17-P. 956 line 11: These are not really results, but rather a sensitivity study.

Technical corrections:

P. 940, line 2, P. 942, line 7: Remove “mainly” and “primarily”: Below the uppermost zone, where snow is compacted into ice, a depth-age relationship can be calculated directly from the initial accumulation rates and the thinning rates; this is the definition of the thinning rate. Of course, we can only aim to estimate this function.

P. 940, line 5: Except for the uppermost zone where snow is turned into ice, ice is not compressed, since its density remains constant.

P. 942, line 12-14: This sentence is awkward. A is the accumulation at time corresponding to the age at z, i.e. it is actually a function of time, not depth. Thinning factor is not defined. Any reason not to use (the usual) t for time instead of ξ?

P. 942, line 21: Define H, and facilitate the reader’s understanding by describing these two equations in words

P. 943, line 16: Θ is a function of ζ, not z

P. 944, line 16: Z is not used.

P. 944, line 19: Add “in a steady state”

P. 945, line 10: Equation is missing “+ 1/AzΘz”

P. 945, line 20: “The tiepoints .. depths”: This should obvious; sentence can be removed.

P. 946, line 15: Same as above

P. 947, line 14: Define Z here.

P. 948, line 1 (and various times later): “accumulation at the surface”: A0 is present accumulation (accumulation always occurs at the surface).

P. 948, line 13: Provide value.

P. 954, line 8: What is the unit of A0? Clearly, it’s not kg/mˆ2/yr?

Figure 1: “Accum at surface” -> A0. It is clear from figure what shows the various parameters, and hence their names do not need to be repeated in figure text.

Figure 2: Missing solid line and red dotted lines.

Figure 2 and 3 can easily be combined to a single figure.

Figure 3: This figure shows the width of the 80% confidence interval. This should be stated somewhere.

Figure 4: it is shown as function of depth, not age.

Figure 5: There is no need to show accumulation as function of depth as well as age. Accumulation rates as function of time makes most sense.

Figure 7-9: The coloring makes it hard to compare the two distributions. I would suggest instead to e.g. only show the outlines of the distributions (i.e. without vertical lines) in different colors. This would allow to combine at least figure 7 and 8, and possibly also figure 9.

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