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## ***Interactive comment on “Optimisation of glaciological parameters for ice core chronology by implementing counted layers between identified depth levels” by L. Bazin et al.***

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### General comments

It took quite some effort to understand just what this paper was doing. The title suggests something methodological; but the explanation is then tightly bound up with discussion of the specifics of GICC05.

Similarly, the comment about "the implementation in Datice of a new type of markers" I found a bit misleading. It wasn't clear what these markers really were until much later. In fact, they don't really seem to be NEW markers; it is simply that the dependence structure of layer-counting measurements is being appropriately taken into account, to

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give sensible uncertainties on age differences, "instead of only absolute ages with artificially small uncertainties" and, presumably, unrealistic assumptions of independence. The implementation is new; but that needs to be made clear.

Such proper allowance for dependence is important. But a lot of details of this are glossed over, even though it is the central methodological idea of the paper. The authors say that "[a] last parameter permits to correlate or not this uncertainty with other duration uncertainties at various depth levels." How is that correlation implemented? If there is no correlation, is that equivalent to R being diagonal for these observations? Given the topic, and the comment that the R "matrices play a key role", it is surprising that no detail is given on their construction.

Independence between these "new" markers and other measurements on the cores is a very strong assumption, and it is not clear what the justification is. Is there a clear scientific basis for it, or is it a simplifying assumptions in the analysis (or does that depend on the particular application?). Either MIGHT be acceptable, but much more clarity is needed.

The MCE approach to assigning errors to layer counting will interact in a complex way with the frequency of the measurements used; again, this is essentially about dependence, but the choice of spacing will affect the actual MCE substantially. I fear that the issues are more complex than are fully explored by the currently reported experiments.

Given that there is quite a bit of discussion of the MCE approach, at least it would be good to acknowledge that other approaches to assessing this kind of error are possible e.g. Wheatley et al (2012) [Wheatley, J. J., Blackwell, P. G., Abram, N. J., McConnell, J. R., Thomas, E. R., and Wolff, E. W.: Automated ice-core layer-counting with strong univariate signals, *Clim. Past*, 8, 1869-1879, doi:10.5194/cp-8-1869-2012], which lead to the prospect of a more coherent representation of their dependence structure.

I think that this kind of improvement is important, not least because of the artificiality of

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the tricks that were needed in some of the earlier analysis described in the Introduction. However, I think the nature of the measurements and the modelling of the size and dependence of the errors need to be defined and explained much more clearly, and separated from the details - however important - of GICC05 (and of the coding). Only then can the merits of the methodological change be properly judged.

### Specific comments

The terminology relating to estimation in Datice is unclear for those not already familiar with it. Is the "first guess" essentially a prior mean? If so, the terminology is rather misleading, to me; it may be established, for this particular case, but should nevertheless be explained a little. Since the method is specifically described as Bayesian, it seems important to relate the description of the method to standard terminology.

The same applies to the discussion of the cost function in 2.1. In a way this is even more confusing - or at least distracting - because the cost function suggests something different in a Bayesian context.

Equation 4 seems unnecessarily off-putting - or redundant for those readers less likely to be put off. Relating the function  $J(\cdot)$  to a set of independent multivariate normals would be clearer and would justify its form. In addition, the details of the R matrices are needed (as mentioned above) as well as a description in more standard statistical terms.

Some of the discussion of the fitting talks about curves meeting the constraints of the error bars. Again, that seems to be at odds with the Bayesian approach. Are the error bars part of the process, or is this description purely figurative?

In general, it would be much better to talk about the model structures and parameters rather than focus on features of the code.

### Technical corrections

There are some technical corrections, mostly minor, to follow. For now, I would just like

to say that, in the title, "implementing counted layers" doesn't sound right, and doesn't quite reflect the emphasis on age differences within the body of the paper.

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