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Interactive Comment

# *Interactive comment on* "An optimized multi-proxy, multi-site Antarctic ice and gas orbital chronology (AICC2012): 120–800 ka" *by* L. Bazin et al.

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This is an important synthesis of a large body of chronological information, and will be a valued addition to the ongoing effort to date ice cores ever more accurately. I particularly appreciate the introduction of formal methods to combine disparate types of information using a cost function approach. In this sense the Lemeiux et al work and the present work have broken new ground. The Datice methodology is an innovative and potentially transformative addition to the ice core researcher's toolkit.

No chronology assembled by a vast array of workers and data sets can satisfy everyone, and this one is no exception. There are very probably better constraints available, that the current authors chose not to incorporate. However, I feel I cannot really criticize their omission in the present work because so much depends upon practical factors such as timing of publication of other papers supporting these constraints. Perhaps the next iteration of this AICC chronology could incorporate these constraints, however. So I will mention them anyway, wishing to reassure the authors that this is not a criticism of the present work.

One new area of understanding that has emerged in the last few years is that the deviations of d18Oatm from the precession curve correlate extremely well with deviations of the Chinese cave d18O records from the precession curve, when the d18Oatm curve is deconvolved for the O2 residence time and corrected for seawater isotopic change (Severinghaus et al., 2009). This correlation in many cases approaches R = 0.9 or higher. These correlations are extremely unlikely to have occurred by chance, and although the exact causal mechanism is not yet well understood, it can be safely concluded that there is some kind of mechanistic link that can be trusted to assure synchroneity between the events observed in cave and air d18O records. Taking the next logical step, a better understanding of the relationship between d18Oatm and precession becomes available. Without relying on the cave U-Th chronology explicitly, information about the nature of the deviations in d18Oatm can be independently inferred.

Using this approach, it has become rather clear that one should not use a constant lag of d18Oatm behind precession, because indeed the cave deviations from precession are not at all constant over time. Note that this conclusion is not very sensitive to systematic errors in the cave chronologies, such as errors in the decay constants of U and Th, for example, because it derives from differences in duration and intensity of the d18O deviations, rather than their absolute ages per se. I believe it is possible to now falsify, with high confidence, the null hypothesis that the lag of d18Oatm behind precession is a constant. The underlying physics behind this conclusion are probably that large differences existed among precession maxima in the amount of meltwater input to the N. Atlantic (Cheng et al., 2010), depending on how much excess ice was being destroyed at the time. For example, strongly nonlinear responses of the cryosphere,



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known as Terminations, consistently seem to cause a longer lag of d18Oatm behind precession than non-Terminations.

There is perhaps a philosophical issue that could be useful to examine in this context. Many of our community have argued that it is premature to use information from the cave records in constructing ice core chronologies, because it has not yet been satisfactorily demonstrated that events seen in both types of archives are in fact synchronous. Yet it also would seem suboptimal to intentionally blind ourselves to the rich cornucopia of new information coming from speleothems. Other communities have made somewhat different choices than ours, for example, the radiocarbon community. They have decided to adopt the cave chronologies (Southon et al., 2010) in the new version of INTCAL for the time periods beyond the era of dendrochronology.

The question then becomes, how to optimally incorporate the new information from caves? One path forward might be to adopt approaches that are somewhat robust to systematic errors such as decay constant uncertainty, as outlined above. Another approach might be to collaborate closely with the leading individuals in the speleothem community, so that there develops a deep mutual understanding of the relative sizes of the true uncertainties in each respective tie point age or interval duration. Then it should be possible to combine the cave and ice information using a weighting scheme, in which each source of chronological information is weighted by the inverse square of its error. In principle, if we actually knew the true uncertainty in each piece of information, we should be able to incorporate ALL sources of information into a timescale. So the challenge becomes to know the true uncertainty.

In this context it might be worth noting that different communities treat uncertainty in different ways. I was surprised to learn, talking with Larry Edwards, that he usually quotes the 2 sigma error, whereas our community usually quotes one sigma errors, just to name one example. Another remarkable fact, not well appreciated in our ice community, is that the independent U-Th dates measured on clean, well-preserved, U-rich samples, all fall in stratigraphic order (ages taken from higher in the stalagmite



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are younger). Even though the interval between the dates is often 30 years or less, 40,000 years ago, they fall in stratigraphic order! The stated uncertainty is typically 100 years or more. A two sigma error of 100 years should produce age inversions in a 30-yr resolution record about 34% of the time, in a Gaussian distribution. Clearly the true relative accuracy of these independent dates is far better, perhaps around 15-20 yr in 2 sigma, based on the observed absence of inversions in data sets with hundreds of points. The accuracy of the decay constants, in turn, has recently been checked using astronomical calculations in very old stalagmites containing a precession signal in d18O, and in even older stalagmites that are in U-series decay secular equilibrium, placing an upper limit on the decay constant error that is even smaller.

My reaction from the vantage point of ice cores is, to throw up my hands. Are we really going to do better than this, counting layers, for ages older than 30 ka or so?

A second philosophical question is, at what point will we be prepared to accept the cave chronologies and the hypothesis of abrupt-event-synchroneity at face value, as we have done for so many years with astronomical calculations (which, by the way, are not free of error, but instead have errors that are demonstrably much smaller than any others at present)? Many of us would answer, when the avalanche of circumstantial evidence becomes overwhelming and impossible to ignore any longer. The question is whether or not we have reached that point. For many of our community, the answer seems to be no.

Another important source of information that could assist the authors in their AICC2012 quest is the Dome F ice core. I found it surprising that they did not strive to incorporate more of the excellent constraints from this ice core. My subjective judgment is that the true uncertainties of many of these tie points are smaller than those of the constraints used in AICC2012. A cynic might be forgiven for calling this the European Ice Core Chronology rather than the Antarctic Ice Core Chronology, as the latter has a kind of global implication to it.

Another potential source of tie points is radiometrically dated tephra. There is one at 135 ka +- 0.9 ka in the Mt Moulton "horizontal ice core" record that lies at the inflection point in dD, which marks the onset of the Termination in many Antarctic records. Unlike the Chinese cave records, these markers are all within-continent and so the synchroneity assumption is more robust. I would urge the authors to use this one and others like it. Nelia Dunbar and Bill MacIntosh at New Mexico Tech, and Trevor Popp at CIC, are the contact people.

A broader concern I have with the approach of using background scenarios is transparency. It is difficult as a casual reader to truly assess the strength of each pinning point and how much the final solution relies upon it. I don't mean to suggest that communicating this information is an easy task: as our data sets get richer and the ice flow physics becomes more realistic, it is a necessary evil that complexity increases and transparency suffers. However I wonder if there are some ways that transparency could be increased, for some partial subset of critical points. Perhaps sensitivity tests, with removal of a critical point, could illuminate for the reader just how much is resting on one particular tie point. This was done for the 131 ka speleothem point, for example, and was very helpful. More of these kinds of sensitivity tests would be beneficial, I think, especially for the problems around MIS 12.

It would also be useful to perform some sensitivity tests to show the reader how much influence ice physics has on the solutions. How are fundamental uncertainties in ice rheology incorporated into the cost function? This is not clear from the present text.

It is well known that ice rheology depends on fabric, and fabric depends on the integrated history of deformation as well as impurity content.

These issues become increasingly severe in the older half of the 800,000 year record, and I would welcome some analysis of them along with sensitivity tests.

I hope I have not sounded too much like a grumpy elder statesman, and I do hope that my comments are useful to the authors. Overall, this is a good contribution to the com-



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munity and the authors should be congratulated for their hard work, and the manuscript should be published with minor modifications (and some grammatical edits).

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