

Interactive comment on “Revisiting the absolute calibration of the Greenland ice-core age-scales” **by L. Skinner**

L. Skinner

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I am very grateful for the referees' thoughtful reviews of this manuscript, as well as the online comments that were provided. These have helped to improve the manuscript, in particular with regard to its clarity of purpose and scope. Below I attempt to address the referees' concerns in turn.

Response to Referee 1:

Although Michael Sarnthein's concerns are no doubt completely valid in the terms in which they are laid out, I fear that some of his comments may be based on a slight misunderstanding of my intentions in writing this manuscript, as well as the method applied (no doubt due to a lack of clarity on my behalf).

Contrary to his impressions, I am not a sceptic of the quality of the GICC05 layer

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counting; in the sense that I do not think it could have been done better. Merely I am suggesting a possible bias in the layer counting, which may well be within the maximum counting error attributed to the GICC05 age-scale (as pointed out by Referee 2). If it is true that there is a bias in GICC05 (even of say 500-800 years), then the term 'uncertainty' (or 2-sigma random error) is actually misleading as applied to the GICC05 age-scale error estimates. I think that this is something we should not be overly relaxed about (though we should not get overly excited about it either!), as it has repercussions for how we frame questions regarding carbon cycle dynamics for example, as well as claims that climate changes in Europe may have preceded/lagged (variously) changes in Greenland by hundreds or even thousands of years (see online comment for example). Perhaps the central issue (as I mention in the manuscript) is that no one should really use the word 'absolute' in relation to a chronology.

On a number of occasions I have tried to underline that GICC05 is indeed the best ice-core chronology available, and that much care has gone into its construction and uncertainty analysis (page 794, line 11; 795, lines 21-25). Further, contrary to Michael Sarnthein's assertion, I do not wish to promote the SFCP04 ice-core chronology as such, nor indeed do I wish to suggest an alternative 'definitive' chronology. I explicitly noted this on page 794, lines 5-8. My main intention (noted on page 795, lines 5-8 and again on page 799, lines 24-26) was to outline a method for testing the consistency of our chronologies, given a particular stratigraphic framework. One thing that I appear to have failed to communicate clearly enough to both referees is that we cannot simply invoke 'variable reservoir ages' (although I do not doubt that reservoir ages may have been larger and more variable in the past, again contrary to Michael Sarnthein's impression). Rather, one must declare what those reservoir age variations were, given a set of assumptions regarding chronology accuracy and stratigraphic linkage. We have to 'put our money where our mouth is', so to speak. As a slight digression: the same goes for claims of asynchrony between near-identical climate signals registered over the globe (see online comment). In this regard, one might ask: should numerical models attempt to reconstruct an 800-year lead of Iberian Margin temperature change

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with respect to Greenland? If one doesn't think so, then how much weight does the 'concern' over asynchrony (or variable phasing) carry? We do well to keep an open mind to such things of course, but we also have a responsibility to declare the theories behind our arguments so that they can be tested. The claim of a (variable) phase lag between Hulu or the Iberian Margin and Greenland would be highly significant indeed, and should be argued for or against - pointing out the mere possibility of either surely gets us nowhere.

The central working premise of the manuscript is that the 'event stratigraphy' represented in Greenland, Cariaco, the Iberian Margin and Hulu is identical - even if it is not identically represented in each archive as Michael Sarnthein points out. One could say that each regional record may be imperfect, yet they all strain at recording the same thing (this sort of argument is the basic premise of stratigraphic correlation: to argue against its possibility in principle is to argue against stratigraphy itself; to argue against its validity in specific circumstances requires specific claims to be made). From this simple premise (which of course can be questioned, although with particular repercussions that others may wish to address) follow very specific and explicit relationships between radiocarbon concentrations in the tropical Atlantic/Pacific (coral data), the Cariaco Basin and the Iberian Margin, as well as the calendar ages that are attributed to these archives. This, very simply, is the main thrust of the manuscript and applies regardless of any specific evaluation of GICC05 for example.

Michael Sarnthein notes that high (and variable) global reservoir ages are perfectly plausible. I concur. However, we must go further than this and say what they actually were (at least in a few places) given a proposed calendar age-scale for Greenland. We must do this because we can do this: this is what the manuscript argues. For example, given the GICC05 age-scale and a 400 year reservoir age for the tropical corals, then Iberian reservoir ages would have been up to 1,700 years and Cariaco reservoir ages up to 1,300 years (and the Hulu age-scale would be biased toward older ages). Another possibility (see online comment by M. Blaauw) is that the Iberian

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Margin led Greenland by up to 1000 years, but not all the time. I would argue that the principle of Occam's Razor should apply in this regard, at least while we await a cogent argument that supports this possibility and draws out its implications.

If, as Michael suggests is possible, the tropical reservoir ages were much larger than 400 years, then the Iberian Margin and Cariaco reservoir ages must have been even larger than 1,700 and 1,300 years respectively. This is indeed possible, though it has significant implications for past atmospheric D14C and carbon cycling during the last glacial. These issues are the subject of an ongoing study (as I'm sure Michael Sarnthein is himself engaged in), and I believe are beyond the scope of the 'methodological' paper that was intended here. Most importantly though, revising all reservoir ages upward does not address the apparent discrepancy between Huliaco and GICC05, noted on page 798, lines 3-10, of the original manuscript.

Indeed, if much larger reservoir ages are applied to Cariaco (1,300 years) in order to completely reconcile it with GICC05, then it must also be true that Hulu ages tend to be biased and too old. If this was not the case, GICC05 and Huliaco would both imply the same reservoir age correction be applied to Cariaco (given the premise that they include the same event stratigraphy). So, the point being made is one regarding a potential bias between age-scales, not the fact that one could 'fudge' here and there with variable reservoir age corrections (which one cannot). The only way to deny this conclusion is to affirm that the event stratigraphies are not the same, or they are too imprecise in each archive to be correlated at all, especially in the interval 22-30 ka BP. Although it is true that the event stratigraphies are not perfect (I note this for example with respect to the speleothem archives on page 798, lines 14-20), what matters is that the events between 22 and 30 ka BP match pretty well and appear to be older in the speleothem records than in GICC05. I believe that this is shown adequately in Figures 3 and 4 combined. Nevertheless I have tried to clarify this point further by re-writing the end of the discussion section, where previously I had noted on page 799, line 19, that "*as the speleothem chronostratigraphy improves in future it may be possible*

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(and necessary) to revise this proposed explanation of the discrepancy between the GICC05 and SFCP04 age-scales. The method outlined here indicates one way that this can be done". I believe that this last line demonstrates that I am not in fact arguing passionately against the GICC05 age-scale, as both referees seem to have come to believe. I hope it is clearer in the revised manuscript.

I wish to stress that the main intention of the manuscript is not to provide a definitive age-scale for Greenland (again, see page 794, lines 5-8). Rather, the goal is to show that we already have an integrated system that allows us to declare the repercussions of our age-scale choices, and to say where we think biases reside. It is no good gesticulating at 'variable reservoir ages' - we must declare them. Michael Sarnthein mentions the 'plateau tuning approach' as evidence for large and variable reservoir ages in the past. Indeed, these results seem convincing to me. However, as shown in the present manuscript one cannot simultaneously adopt the GICC05 age-scale and the 'Huliaco' radiocarbon reference curve. As a result, the findings of Sarnthein and co-authors (which I stress I do not question here) are 'formally' inconsistent with the adoption of the GICC05 age-scale (they make use of 'Huliaco' as a reference curve). The same would go for any radiocarbon calibration that makes use of speleothem ages (e.g. future INTCAL versions?). I must stress again, that I am not arguing in favour of one or the other of these age-scales with any sort of passion, merely I am trying to show that we must adopt age-scales that are at least consistent with our stratigraphy if we are to make any sense at all. Alternatively, we must come out and say that the stratigraphy is wrong, and that significant leads/lags exist in the system, or that particular coral/speleothem dates are biased. In this manuscript I basically adopt the tentative premise that the U-Th dating is probably more accurate than the layer counting. However I have been careful to note that evidence may emerge in future that would require us to drop this assumption (see end of revised discussion section).

A more obvious example of the same sort of problem described above would be to take the old Cariaco radiocarbon calibration (on the GISP2 age-scale), use it to cali-

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brate a marine core, and then compare this marine core with any Greenland ice-core archive placed on the GRIP or GICC05 age-scale (especially the former). This would be formally (i.e. logically) incoherent: you can't adopt one age-scale for Greenland and then compare it with records that make use of another Greenland chronology! The manuscript here tries to show that the same sort of reasoning also applies to GICC05, Huliaco etc... under the premise of a globally coherent event stratigraphy. It turns out that if, as Michael Sarnthein argues, we should revise the Cariaco reservoir age up to 1,300 years (in order to reconcile it with GICC05), then the reservoir ages that he has inferred from plateau tuning are in fact minimum estimates: the (upper) glacial ocean would have been very depleted in radiocarbon indeed. This has quite intriguing ramifications (many of which can be cross-checked). If so, I would argue that the present paper would be quite helpful in 'setting up' the investigation of such ramifications, which include an implied bias in the Hulu chronology.

Michael Sarnthein makes two other points that must be responded to at this stage. Firstly, he argues that the apparent age-discrepancy in GICC05, with respect to coral, Huliaco and Iberian Margin radiocarbon, does not accumulate with greater depth in the core, as one might expect for an accruing counting error. The second referee makes the same observation, adding that all of the Greenland age-sales seem to converge towards agreement near where the Laschamp event is known to be (independently dated at 38- 44 ka BP). This is indeed curious, and I do not have an explanation for it either. Nevertheless, it is notable that the number of uncertain years counted in NGRIP varies with depth, so that uncertainty accumulates unevenly. There are also 4,000 years of scope for placement of the Laschamp event, given its actual quoted dating uncertainty. Probably the point made by Referee 2 is the most pertinent: agreement between all of the age-scales might be achieved if the maximum GICC05 counting uncertainty envelope is considered. Andersen et al. (2007 , CP) have already shown that many of our best chronostratigraphic age-control estimates may indeed fit within the GICC05 maximum counting error envelope. However, if one overwhelmingly considers just one side of this envelope in order to reconcile all of the records (e.g. only age under-estimation,

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or too many uncertain years), would this not tend to suggest a 'bias' in the counting, rather than a random uncertainty? This is the possibility that I wanted us to consider in the manuscript (see page 799, lines 10-12, of the original manuscript).

Secondly, Michael Sarnthein suggests that a significant pitfall for the manuscript is the fact that the correlation between Cariaco and the Iberian Margin is not perfect between 27 ka and 37.5 ka BP, such that the uncertainty in the SFCP04 age-scale could reach 500- 1000 years in this interval (this would arise mainly due to the grey-scale signature recorded in Cariaco deviating significantly from a Greenland-like signal). The first thing to point out is that the SFCP04 age-scale is not based on the correlation with Cariaco, and therefore has nothing to do with the correlation quality with Cariaco. The correlation between these records was undertaken in order to assess the degree to which radiocarbon dates (and therefore reservoir ages) tended to be older/younger at one site relative to the other for a given stratigraphic event. This in turn was used to argue that the reservoir ages on the Iberian Margin probably had been underestimated by Shackleton et al (2004), as surmised by both referees. However, I go further to suggest that the reservoir ages probably cannot be revised upward by enough to completely reconcile SFCP04 with GICC05 (I believe this is what Michael Sarnthein takes issue with). What was shown in Figure 3 was that the SFCP04 - GICC05 age discrepancy could entirely be explained by reservoir age underestimation on the Iberian Margin except in the interval of GIS 2 to GIS 7: precisely where the speleothem ages also disagree slightly (on the basis of 'Huliaco', and see Fig. 4 in Andersen et al. 2007, CP).

It is crucial to understand that the main issue here is how much time is included in the interval between GIS 1 and GIS 4. If the SFCP04 age-scale puts GIS 4 at 30 ka BP (and GIS 1 is pretty firmly placed at 15 ka BP), then it is clear that SFCP04 requires a longer duration for the GIS 1 - GIS 4 interval than GICC05. Michael Sarnthein's suggestion that the interval *"26-29 ka cannot be used for comparison because it is not covered by ice-core ages from both chronologies"* may therefore be set aside. The fact remains: we have to decide how many years there were between GIS 1 and GIS 4.

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The SFCP04 and GICC05 age-scales make different predictions in this regard, which can be assessed. Indeed, if the two age-scales differ primarily in their prediction of the age of GIS 4, then it is precisely the interval 20-30 ka BP that must be looked into (helpfully, there are also many coral dates 20-26 ka BP to compare with). It therefore does not matter that SFCP04 originally only covered 30-60 ka BP if its main prediction is a longer MIS 2.

Perhaps with a mix of temerity and conviction, I would argue that the method outlined in the paper is basically sound. Either: 1) GICC05 is perfect, the reservoir ages of Cariaco and the Iberian Margin were 1,300 and 1,700 years respectively (between GIS 2 and 7) and the Hulu age-scale is slightly wrong (and the carbon cycle during the last glacial needs urgently to be revisited!); or 2) GICC05 is slightly biased to younger ages between GIS 2 and 7, the reservoir ages on the Iberian Margin should be revised upward by about 400 years, and the Hulu age-scale is taken to be broadly correct. Both of these rely on the coral dates being correct. I have tried to make it more clear in the revised manuscript that slightly different conclusions might be drawn, although with different implications (in particular at the end of the discussion section). Again, it is primarily the method that I wish to highlight at this stage. I hope that I have struck a better balance in the revised text.

Finally, Michael Sarnthein argues that it is impossible for annual chemical signature to disappear due to stratigraphic 'compression' of some sort in the ice-core. While it must be true that chemical species will not disappear from the firm (unless the firm itself is eroded), it seems entirely possible that weakly expressed annual peaks could become 'shoulders' on neighbouring peaks, and eventually become an 'uncertain year' (see Fig. 3 in Andersen et al. 2006, QSR). The issue at hand is whether too many or too few uncertain years have been counted over a given interval of ice. Based on the maximum counting error in the GICC05 age-scale, if every 'uncertain year' was in fact a real year leading up to GIS 4, the age of GIS 4 would already be more than 900 years older (Andersen et al., 2006, QSR, Fig 6).

Michael Sarnthein also notes that the *"present age uncertainty [for the GICC05 chronology] still requires further reduction"*. This is essentially what is argued in the present manuscript, except that I make use of an explicit method in order to make the same proposition, and I go on to suggest why one might believe that some of the uncertainty in GICC05 could turn out to be a bias, which is entirely different from random uncertainty. The comments of Referee 2 take this up.

Response to Referee 2:

Referee 2 has suggested that I make the GICC05 age-scale 'mostly' responsible for any discrepancies with regard to SFCP04. This is not true. As originally noted on page 799, lines 12-15, I estimate that probably about half of the discrepancy could be due to reservoir age underestimation, and half due to missing years (i.e. too many 'uncertain years' counted) in GICC05. Furthermore, Figure 3 also shows that this equitable contribution is only true for GIS 2 to GIS 7; for the rest of the record it is nearly all down to reservoir age variability.

Referee 2 makes three broad charges: 1) that correlation uncertainties should be addressed more fully/quantitatively; 2) that more consideration should be given to reservoir age variability as an explanation for SFCP04 - GICC05 age discrepancies; and 3) that Huliaco, SFCP04 and paired 14C - U/Th dates in coral are not actually independently derived.

I have dealt with the second of these (that reservoir ages might have been large and variable) in my response to Referee 1 above. To reiterate: it is a question of deciding which set of dates is biased: Hulu (U/Th too old), coral dates (U/Th too old also; or else radiocarbon too young), marine radiocarbon (too old, both on the I. Margin and in the Cariaco Basin), or GICC05 (too young). The Cariaco and Iberian Margin radiocarbon dates may well be biased to older ages as a result of reservoir age underestimation (as proposed by Referee 2), but this means that Hulu ages are biased too (i.e. too old). Alternatively, if coral radiocarbon dates are biased to younger ages, then they can only

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be made much older by reducing the reservoir age of the tropics below zero - which can probably be ruled out. Coral U/Th dates could also be biased (too old), but I specifically state that the accuracy of the coral U/Th dates is a premise of the manuscript (page 796, line 19). One cannot therefore simply invoke variable radiocarbon ages 'here and there' as a fix: the stratigraphy integrates the records in a way that precludes this. My point is that we must make explicit choices, and I try to show why. Again, if evidence emerges to suggest a younger (average) speleothem chronology in future, then the same method outlined in the paper could be reapplied to see if it reconciled all the available records. The same goes for any reassessment of the speleothem chronologies based on, for example, new half-life estimates.

The first suggestion made by Referee 2, that an assessment of the correlation uncertainties should be undertaken, is certainly valid. In fact, a statistical analysis of the correlations and inter-relationships discussed in this paper is the subject of ongoing work. The goal of this work (which will take some time to complete adequately) will be to assess more objectively what biases are present, where they might apply, and how large they 'probably' are. I have added a note to this effect in the revised manuscript (line 235). The purpose of the present paper, as noted repeatedly in the manuscript, is primarily to present a method, not to critique the GICC05 age-scale. I mention on page 799, lines 19-22, that a lot hinges on the accuracy of the speleothem age-scale, the improvement or alteration of which could cause the specific conclusions of the manuscript to be changed (also see above). It should also be obvious that significant changes to the coral dates (specifically to younger calendar ages and/or older radiocarbon ages) would also alter the conclusions. The method presented in the paper on the other hand will not be affected; indeed it might be used to reach different conclusions as new information comes to light (I state this on page 799, line 19). Nevertheless, in order to address Referee 2's concerns regarding the stratigraphic correlation uncertainties between the Iberian Margin and Greenland I have redrawn Figure 1 to show the implications of adopting a correlation between the two that basically reconciles Iberian Margin radiocarbon dates with GICC05 calendar ages. I have also changed the way

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that IRD is presented in Figure 1, in order to stress the importance of the placement of Heinrich 2 and 3 relative to Greenland stadial - interstadial transitions. Added discussion of these constraints has been added to the revised manuscript, from line 91.

The third main point made by Referee 2 was that Huliaco, SFCP04 and paired 14C - U/Th dates in coral are not independently derived. Referee 2 argues this by suggesting that Konrad Hughen chose the Hulu age-scale on the basis of Richard Fairbanks coral dates, just as Nick Shackleton chose Richard Fairbanks' dates on the basis of his own radiocarbon dates, and just as I have chosen Huliaco and Fairbanks' corals based on their superficial agreement. All of this is supposedly a case of 'forcing agreement'. However, Referee 2 makes an error of logic here. Agreement is not the same thing as tautology. In order for Referee 2 to be correct, it would have to be shown that the records, their radiocarbon dates and/or their chronologies had been derived from the same basis. In fact, they were not. The Hulu chronology was created independently of any radiocarbon dating or ice-core layer counting. The fact that Cariaco radiocarbon dates agree better with independently dated coral radiocarbon measurements when placed on this Hulu age- scale is just that: agreement, not tautology. If anything, applying reservoir corrections precisely in order to support the precision of GICC05, regardless of what this implies for the Hulu chronology, would be disingenuous and tautological. Nevertheless, this is precisely what both Referees seem to be suggesting we do: we decide the GICC05 age- scale has no biases; we correlate our radiocarbon-dated marine records to Greenland; we then ascribe any inconsistencies entirely to 'reservoir age variability' (or else throw out the coral data); and all this without concern for whether or not it has implications for the speleothem chronology. I think the latter approach is somehow unsatisfactory, and this is why I wish to propose a method that is flexible with respect to evolving premises regarding stratigraphic correlation or speleothem chronology, yet which nonetheless can be explicit about these premises.

Finally, Referee 2 has suggested that "the various dating methods [and stratigraphic correlations] could agree with the GICC05 time scale when the maximum counting er-

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ror is taken into account". In effect, this is precisely my point (stated on page 799, lines 10- 12), except that I suggest that overwhelmingly negative errors should be considered. This would imply a bias, not a random uncertainty, as inferred by Referee 2: "uncertainties in the ice-core dating may not be statistical errors but they could be systematical errors". In the revised manuscript I try to make it more clear that the identified chronological discrepancies can almost all be accommodated within the maximum counting error of the GICC05 age-scale, albeit with the suggestion of a negative bias in MIS 2 (see lines 213-224 in the revised manuscript).

Referee 2 and Referee 1 both pointed out that we do not have 5 completely independent Greenland ice-core chronologies. I have therefore noted 5 different chronologies instead.

Response to online comment by M. Blaauw:

Maarten Blaauw has commented on the dangers of making correlations between disparate records, as this may obliterate phase relations between them, or at worst entrench links that do not actually exist. He has suggested instead that we adopt radiometric chronologies wherever possible. The basis of this argument must be that radiometric dates and calibrations are necessarily more accurate than stratigraphic correlations. I have made some initial reflections on this argument in my response to Referee 1 (3rd paragraph).

The statement that stratigraphic correlations can be dubious is true, but it is also without content. The same goes for the statement that radiometric dates can be biased, miscalibrated or otherwise compromised. These are diligent but ineffectual statements that should surprise no one. I would suggest that if we wish to construct an argument, then we must say that a specific correlation or radiometric date is compromised, of limited accuracy, or even misconstrued or inappropriate... or else we must propose alternatives. The perennial sceptic bears no fruit; the scientist learns from her mistakes!

Carl Wunsch (QSR, 2006) showed how the relationship between various records (some

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of which are employed in the present manuscript) was often difficult to support statistically. However, what Carl Wunsch ignored was that other pieces of information than the 'kinematics' of the signals might inform our assessment of such correlations. The integrated system that I present in the manuscript is a case in point: the correlations may be subject to uncertainty, but when they are made they provide us with information that can be tested. In other words there is a host of additional 'priors' available for assessing the proposed correlations. We might believe a correlation more (or less) if its application can be sustained (or becomes frustrated) in the face of other pieces of information, or if it makes predictions that are verified (or refuted). This sort of approach is necessarily an iterative procedure, whereby climate theory is also tested and modified as a result (this being the point of the whole exercise). When uncertainties are large, a lot depends on expectations.

A famous illustration of this is Nick Shackleton's demonstration of the relative accuracy of orbital tuning (a stratigraphic method) versus radiometric dating, in determining the timing of geomagnetic reversals. Orbital tuning proved more accurate than K/Ar dating, and this was finally confirmed by Ar/Ar dating (Shackleton, et al. 1990, Transactions of the Royal Society of Edinburgh, Earth Sciences 81, 251-261). While Maarten Blaauw might see this as proof that better radiometric dating is the keystone of chronostratigraphy, it in fact demonstrated that orbital tuning could be used to great effect when Ar/Ar dating is not possible (i.e. for great sections of the geological record!). By lending credence to the premises of orbital tuning, this vindication also influenced climate theory (i.e. Milankovitch theory) in a way that Ar/Ar dating of some marine sediments would never have done.

The latter remark is important as it underlines the fact that stratigraphy itself has 'content'. As I explicitly state in the introduction (page 792, lines 25-26) any given correlation embodies a theory or hypothesis, which can (and must) eventually be tested. For example, to apply the INTCAL04 radiocarbon calibration beyond 12 ka BP is to hypothesize a correlation between rainfall in the Cariaco Basin and changing Green-

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land temperature (amongst other things). Even radiometric dates can therefore carry stratigraphic (and climatological) assumptions. When they do not, it can actually be more difficult to test their accuracy without recourse to extensive replication (which is very rarely done, due to cost or lack of material rather than laziness of course).

In summary, to argue against the utility of stratigraphy would be foolish, as I'm sure Maarten would agree. On the other hand, to completely mistrust radiometric/calendar dating would also be foolish, so in a sense I concur with Maarten too! In this manuscript I have tried to illustrate an approach that combines both stratigraphy and radiometric-or calendar dating (in multiple archives) in order to test their consistency under various assumptions. A proper statistical analysis of these relations is indeed necessary, and will be forthcoming. In the mean time, I would counter Maarten's comment by saying that stratigraphy may be risky, but 'chronological absolutism' is downright dangerous.

Interactive comment on Clim. Past Discuss., 4, 791, 2008.

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