Clim. Past Discuss., 9, C650–C695, 2013
www.clim-past-discuss.net/9/C650/2013/

Climate of the Past Discussions

Interactive comment on "Peak glacial ^{14}C ventilation ages suggest major draw-down of carbon into the abyssal ocean" *by* M. Sarnthein et al.

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Received and published: 7 May 2013

RESPONSE TO ANONYMOUS REFEREE #1

«Comments of Reviewers in brackets»

Executive Summary

In our response we tackle and refute four major reservations listed by Ref.#1, that concern (1) the impact of our postulated major deglacial deep-ocean degassing and atmospheric D14C drop on the atmospheric d13C budget, (2) the role of the different carbonate species in the ocean, (3) the role of changes in alkalinity for the carbon-

ate system, and (4) the role of potential changes in atmospheric 14C production for the deglacial D14C gradient. Furthermore, we acknowledge with thanks a series of valuable suggestions to improve the transparency and presentation of our evidence.

Response to specific comments:

«The most important aspect is the question if the paper can really address the question it was made for and is the paper supporting the final conclusions it is drawing. Here I have to say, that especially on the explanation of the Mystery Interval I am not convinced by the paper.

As this is utterly important I try to clarify where I see deficits: On page 945 it is claimed that by outgassing of 730 or 980 GtC from the deep ocean with a rather low D14C values a drop in atmospheric D14C of 210-230‰ can be reached. For that effort the authors have to eject the carbon until the end of the Heinrich 1 stadial into the atmosphere. They correctly state later-on, that this is at odds with the CO2 data, because at the end of HS-1 or at the beginning of the Boelling/Alleroed around 14.6 kyr BP atmospheric CO2 was only 240 ppmv, and therefore 85% of the carbon that was injected from the deep ocean into the atmosphere needs to taken up again by the intermediate and surface waters. I believe if such a huge transition of C had indeed have happened (from deep ocean via atmosphere to surface and intermediate waters) – and this huge transition is necessary to explain the drop in atmospheric D14C by 190‰ – then we would see signals of it in other records, at least in atmospheric CO2 and d13CO2 records from the ice cores (Schmitt et al., 2012). At least the overshooting in CO2 should be visible in the record. »

The perceived deficits, outlined above, are connected with the long-going discussion of the so-called Mistery Interval, which, as Skinner states, "has become too narrowly focussed on the existence of an extremely radiocarbon-depleted reservoir somewhere in the ocean". Seen in the context of the Global Carbon cycle, the "huge" transition of 730 to 980 GtC from ocean to atmosphere corresponds to the carbon exchanged between atmosphere and ocean every ten years (90 Gt C/year). Already a 1% perturbation/disequilibrium of this flux would effect the transfer in 1000 years (duration of HS-1 ca. 2800 yr). Due to the large exchange fluxes, the 14C and 13C concentrations of the atmosphere can change without a change in atmospheric CO2 content, as long as their concentration in the outgassing CO2 differs from that of the CO2 in the atmosphere and taken up by the ocean. The ocean-atmosphere carbon cycle thus offers enough freedom to allow our working hypothesis.

We thank Ref.1 for his valuable suggestion to look for CO2 and δ13C signals in other records. Indeed, we see a clear signal of major CO2 outgassing in the d13Catm records published by Schmitt et al. (2012), which show a negative 0.3-permil shift in d13Catm , that started around 17 ka, right near the onset of HS-1 and continued over HS-1 until $\sim$14.5 ka. In our view, it may indicate the continuous outgassing of 'old' CO2. Some records even suggest a 0.7-permil shift, including a slight but distinct overshoot signal in the beginning, which document a major oceanic outgassing of 13C depleted, that is 'old' CO2 at high latitudes, both in the Southern Ocean and subpolar North Pacific. Furthermore, what is missing in the calculations how atmospheric CO2 might vary is the fact that it is not only about moving carbon from the deep ocean to the atmosphere, because C can exchange between both pools only as CO2. But in the ocean the dissolved inorganic carbon pool DIC consists by 90% of HCO3, by 9% of CO2 -3 and only by 1% of CO2. This is also the reason, why – if such a large peak would have happened – atmospheric C anomalies are not very fast brought down to background levels again, because the gas exchange via CO2 is the bottleneck of the whole system.

The generally accepted ocean-atmosphere exchange of 90 Gt C/yr shows that CO2 is not the bottleneck perceived. This is due to the speed of the reactions in the chemical equilibrium between CO2, bicarbonate and carbonate. The hydration rate of CO2 is the limiting factor, but still indicates average lifetimes of CO2 from only 27 sec at 25 C to 500 sec at 0 C (Vogel et al., 1970; Z. Physik, 230).

«For the whole story it is also necessary to consider changes in alkalinity. Because the marine carbonate system has two degrees of freedom, it is not enough to calculate only changes in DIC, one has to make assumptions on alkalinity, even if stated that it is constant. Furthermore, the assumptions / changes in DIC and alkaninity at the surface ocean are relevant for atmospheric CO2.»

In our discussion paper (p.933, line 19-29) we did already consider minor changes in POTALK (potential alkalinity, showing the net effect of calcite formation/dissolution). Based on our regression of POTALK vs ventilation age (Fig. 4) we concluded that an average aging by 60 ‰ D14C may have resulted in an LGM rise of POTALK by $\sim$60 μmol/kg below 2000 m water depth. Given an ocean volume approximately equal above and below 2000 m (ETOPO-20) this rise in POTALK will induce an equal POTALK reduction in shallow and intermediate waters. Moreover, CaCO3 dissolution of sea floor sediments has further increased during LGM. Accordingly, we need to assume a further POTALK increase that cannot be quantified as yet (see p. 934, line 5-7).

«Nevertheless, the question is, what can be learnt from the study:»

«1. I believe for their main interpretation on the Mystery Interval, the authors need to use available data to restrict WHEN and HOW large such a flux from deep ocean C to the atmosphere at maximum might have been, e.g. using d13C and along the line of Schmitt et al. (2012) where it is discussed that a drop in d13C by 0.3‰ and a rise in CO2 by 30 ppmv at the beginning of Termination I might be connected with a drop in atmospheric D14C of 20‰Maybe these kinds of calculations can be streched to its limits and thus claim how much of the Mystery Interval can be explained with it.»

As stated above, the C transfer from deep-ocean to atmosphere is the result of a relatively minor disequilibrium between oceanic outgassing and uptake, combined with differences in 13C and 14C concentration between deep-ocean and atmosphere. The atmospheric changes resulting from this disequilibrium, documented by Schmitt et al.

are insufficient for a reliable reconstruction of the outgassing, due to the fact that two more, largely undefined carbon reservoirs, namely the shallow and intermediate ocean and the terrestrial biosphere and soil, are actively exchanging recipients of the outgassed CO2. Trying to explain, or not explain, the Mystery Interval from these data alone would, indeed, stretch these calculations beyond their limits.

Based on our D14C data (compare new Fig. S1) and in harmony with Schmitt et al. (2012) we believe that the massive CO2 flux from the deep ocean to the deglacial atmosphere of 400 Gt C (ca. 200 ppm CO2) continued over ~2500 yr during HS-1, coeval with the drop in atmospheric 14C, starting around 18–17.5 ka. Thus we prefer to interpret the salient 0.3-‰ shift in atmospheric d13C near 17 ka not as result of a single 30-ppm pulse of carbon from the deep ocean to the atmosphere near 17 ka but as ongoing small perturbation of the large ocean-atmosphere exchange (90 Gt/yr today). Increased input of 13C depleted deep-ocean CO2 initially lowered the 13C of atmosphere and surface ocean until balanced by isotope exchange between surface and intermediate waters, which led to a transient isotopic shift of a dynamic equilibrium of atmospheric d13C. This state had persisted over the preceding LGM and later on returned to the initial level over the Holocene. In our opinion this steady state was interrupted over ~2500 y until the B/A, likewise over the YD and Early Holocene, as long as a net outgassing from the deep ocean continued. As soon as the unusual outgassing of CO2 had ceased, the antecedent d13C level returned. The simple mass balance estimates above indicate that our suggested explanations are well within the possibilities of the system.

«2. I also believe that part of the Mystery Interval can be explained by changes in 14C production rates, but I agree that uncertainties are high here and the drop in atmospheric D14C is too fast be be completely explained by them, but see Köhler et al. (2006) for some model-based scenarios.»

We do not see any geomagnetic evidence in support of a significant change in atmospheric 14C production rates over HS-1 (Laj et al., 2004; Muscheler et al., 2005; Köhler

et al., 2006). On the contrary, ice-core 10Be fluxes may rather suggest a rise in 14C production by up to 100 ‰ D14C from 22–15 ka.

«3. Although not yet published and therefore difficult to finally judge, my understanding is, that the gradient in atmospheric _14C will be a lot smaller if data are based on the upcoming INTCAL13 _14C compilation. Maybe it is worth to wait for its release in RADIOCARBON later this year to redefine the target again. The amplitude might be the same but the drop has some more centuries / millennia time to take place.»

The IntCal13 compilation will be a Markov-Chain-Monte-Carlo modeling of the most probable atmospheric 14C record, based on oceanic data, speleothems, and Suigetsu. By virtue of its mathematical tasking it produces a strong smoothing. A direct comparison with the atmospheric Suigetsu record, although this still will need to be validated by additional atmospheric records, is therefore the best one can do.

Our study is based on the atmospheric D14C record of Lake Suigetsu, the chronology is superior because of varve counts (Ramsey et al., 2012; Science 338) and certainly will form a backbone of the upcoming IntCal13 record. – Also, we see an urgent need to publish our working hypothesis as soon as possible to keep priority on our concept.

Some more details:

«1. This paper is difficult to read. The authors switch between various ways how they describe the rather lengthly units of some of their calculations, e.g. “umol DIC kg-1 per ‰ 14C”, sometimes found as “umol DIC kg-1 seawater ‰1 14C”. This example is given just for illustration. Please unify.»

Following Ref.#1 we standardize with care the descriptive units used in our paper.

«2. Throughout: If talking about “‰’ changes in radiocarbon, it should be “D14C”, not “14C”.»

Yes, we are taking care of this problem.

«3. Throughout the MS: when writing about ventilation ages, does this mean "calendar years" or "14C year". This should be very clear every time an age is given.»

Yes, we now specify all ventilation ages as 14C years'.

«4. Abstract line 2: 530 GtC: should be transfered from WHERE to WHERE?»

We mean the transfer "from the ocean to produce the deglacial rise of carbon in the atmosphere and terrestrial biosphere."

«5. Abstract line 12: One strong assumption is that the gradient DIC versus D14C did not change over time. Is this reasonably when we know that 14C production rates varied and were for a long time in the glacial period more than 30% higher than today? See reconstructions based on 10Be and the geomagnetic field Köhler et al. (e.g. 2006). My understanding is that a lot of the paper is based on that assumption so some more support is necessary here, e.g. estimate what a rise in 14C production rate by 30% for 30 kyr would imply for deep ocean D14C values.»

As already mentioned at Point 2, above, we do not see any geomagnetic evidence in support of a significant change in atmospheric 14C production rates over HS-1 (Laj et al., 2004; Muscheler et al., 2005; Köhler et al., 2006). On the contrary, ice-core 10Be fluxes may rather suggest a rise in 14C production by up to 100 ‰ D14C from 22–15 ka.

«6. Page 927, line 25, Fig 1: You nicely use Matsumoto (2007) to argue about ventilation ages of water masses. However, the dominant work of Matsumoto (2007) is to redefine ventilation ages based on the fact that southern-sourced waters change all this calculations. I think this is later-on taken up once, but I think it need to be addressed right here and maybe also with some arguments.»

At p. 927 any more extended discussion will disturb and unnecessarily lengthen the 'Red Thread' of the introductory text. Hence we prefer to address this question in Ch. 2.1, p. 329, paragraph starting from line 20, and extensively in Ch. 2.2, p.930-

932. Moreover, we now address the question of southern-source waters in Ch. 4.1 discussing Fig. 3.

«Does your data / approach implies, that the revised ventilation ages of Matsumoto (2007) (which are about a factor of two smaller than the conventional ages plotted in Fig 1) are wrong?»

No, we do not regard the ventilation ages plotted by Matsumoto (2007) as 'wrong'. However, the ages were modified by gridding which implies a number of potential artifacts.

«7. page 928, line 9: when referring to other sections, please use section number.»

O.K.

«8. page 929, line 25: POTALK not explained.»

In our text we now supplement an explanation for 'POTALK' as 'potential alkalinity' (= alkalinity + nitrate) used to quantify changes induced by calcite formation and dissolution. In contrast to the original definition by Berner et al. (EPSL, 1975) we do not normalize POTALK by salinity, since salinity variations are negligible in the deep ocean.

«9. page 932, line 26: Ratios "0.48-1.43" have no units, maybe okay, but not clear, because I do not know what quantities are divided in detail.»

We now supplement the units 'mol/mol'.

«10. page 933, line 11: "the modern abyssal ocean is picking up almost 1.1 GtC per year". This is not clear to me. Today the C content should be about in steady state, which should be deep ocean accumulated C? I can also not follow, where the numbers come from, please clarify.»

Following Ref.#1, we now clarify at line 12 that "the regression slope in Fig. 2a implies that the biological pump contributes almost 1.1. Gt C per year to the modern abyssal ocean". The subsequent text is modified accordingly.

«11. Fig 3 and Fig 4. If remembering correctly, Fig 4 is cited before Fig 3, thus please change both figures.»

Fig. 3 is properly cited at p. 928 prior to Fig. 4 at p. 933.

«12. page 936, line 10: Please change “cm ky-1” to “cm kyr-1”»

O.K.

«13. page 937 lines 9-10: Please state all the time windows chosen here: What is the selected LGM and HS-1 time windows? Why is the Bolling/Allerod so short? For my understanding it should be 14700 to about 12700 yr BP (until the beginning of the Younger Dryas), you chose to stop at 14.0 kyr BP. Furthermore, here it is called “Bolling”, in most other places it is called “Bolling/Allerod”. Please clarify and unify throughout the MS.»

At p. 937 we now add to the text the age ranges defined for the LGM (23–19 ka) and HS-11 (17.5—14.7 ka) time slices. Following Ref.#1, we now pick the full Bølling/Allerød (14.7–12.9 ka) instead of only the Bølling. Moreover, we add a new Fig. S1 showing unpublished 14C and D14C time series for six sites, where the outlined three time slices are labeled.

«14. Fig 3: is not labeled as Fig 3a, 3b, 3c, but as such referred to in the text.»

Letters to label a, b, and c are now supplemented in Fig. 3.

«15. Section 4.3 and Fig 6: The whole discussion on oxygen is difficult to follow. From Fig 6 is can not follow how water masses might get suboxic or anoxic (but maybe I missed it). »

Apart from local trends linked to local ocean features, Fig. 6 indicates the large-scale overall trend of gradual oxygen depletion with increasing ventilation ages of modern deep waters. In addition, we added to Fig. 6 a line indicating that deep waters become suboxic near 10 μmol/kg (anoxic would be zero). Rough first estimates of past deep-

water oxygenation (Table 1a and p.942, last paragraph) were calculated from the overall D14C trends and regression slope values listed in the context of our response to item 18, together with alkalinity slopes.

«Fig 6: labels of both axis need revision.»

O.K, we see the point.

«16. Table 1: I do not understand the table: In columns 2, 3, 6 (DIC, alkalinity, PO4) what is the meaning of the “x-”? Maybe I have missed it.»

Following Ref.#1 we now largely modified Table 1 to reach a better intelligibility.

«17. Fig 1a: Do you have copyrights for reproduction from Matsumoto (2007).»

We are requesting the copyright as soon as our study will we be accepted for publication.

«18. Fig 4: Slopes of the regressions are not mentioned, please insert either in the figure or in the caption.»

The regression slopes for POTALK are now added to the caption of Fig. 4. Likewise we add the slopes for O2 vs D14C to the caption of Fig. 6.

Below we list the POTALK and O2 slopes vs D14C for six different ocean regions specified in this study: 1.03 / 1.16 global 1.15 / 0.71 (1) 1.06 / 1.26 (2) 1.71 / 1.9 (3) 1.18 / 1.7 (4) 0.83 / 1.13 (5) 0.96 / 1.68 (6)

«19. Fig 5: Maybe you shift this figure to become as subfigure “c” part of Fig 2.»

With thanks we considered the helpful suggestion of Ref.#1 and tried to add Fig. 6 as Fig. 2c to Fig. 2. However, it now competes with a new cartoon Fig. 2c added to show the differential budgets of preformed nutrients and the biological pump and their impact on D14C and DIC.

RESPONSE TO REFEREE #2 (Luke Skinner)

Executive Summary

We acknowledge with thanks the numerous, particularly constructive comments of Luke Skinner. In particular, (1) we now add a new Fig. S1 that displays all plateau-based D14C time series and the technique of their derivation; (2) we add a listing of key assumptions for our working hypothesis; (3) we specify more clearly the role of AABW both in terms of d13C signals and DIC-D14C slopes, which is spreading below North Atlantic source deep waters in the Southern Ocean both today and during the LGM; (4) we add citations on an increased ventilation and carbonate preservation of LGM intermediate waters; and (5), we improve the intelligibility of Fig. 2b and supplement it by a new cartoon Fig. 2c showing the DIC/D14C system of the deep ocean. However, (6) we won't follow the suggestion to supplement our study by model tests. In view of our evidence we regard simple box models as inadequate to meet the complex scenarios of the LGM ocean, hence possibly misleading. Likewise, two separate quick tests with GCM models proved unsuccessful because their design still turned out as immature to adequately reproduce the patterns revealed by our empirical evidence. In turn, we agree to regard model tests as a highly promising mid-term target for future 'community efforts'. (2) We arrived at the opinion that volumetric estimates of DIC-D14C on the basis of 'objective mapping' / 'gridding' are problematic and possibly misleading.

Reponse to specific questions:

«p. C229, top: .... I refer to a 'calibration', even though there are very clear physical mechanisms that underlie the observed modern relationships between radiocarbon, DIC, POTALK and O2 concentrations (which the authors describe), because in extrapolating from the modern relationships the authors must assume invariance in a range of unconstrained processes. Specifically, one must assume a constant relationship between export productivity and mass advection rates (mean transit times) in the ocean interior. »

The extrapolation had to be made because of a lack of data and, because of this, can only be defended by probability arguments, as given in the paper and discussed below. In support of this: Yes, we do find increased mean LGM transit times in the ocean interior. In contrast we assume that export productivity was decoupled from the reduced overturning rate and did not change on a global scale. We now added a new cartoon Fig. 2c (copy attached) to better explain the relationship we have in mind. This implies that the "Redfield Ratio" is not perfectly constant, since the nitrogen turnover is shallower and faster than the carbon cycling (Schneider et al., 2003; GBC 17, 2).

«This in turn would imply a past ocean circulation 'geometry' and nutrient cycle that was broadly similar to (or at least not drastically different from) today's. The broad similarity of the 'calibrations' that are found to apply to different ocean basins today is presented as an empirical argument that the line of reasoning adopted in this manuscript should be broadly robust to changes in ocean circulation/ export productivity that are analogous to the spatial variability in the modern global ocean; i.e. the modern 'spatial calibration' is taken as a viable 'temporal calibration' too.»

This presents a perfect summary of our approach indeed.

p. C230, top: «I think that the approach taken in this manuscript is not perfect and cannot be defended as being totally unequivocal, but it is nonetheless very useful and thought provoking, specifically as a working hypothesis that is premised on key assumptions (that are not proven, but might be tested). »

We fully agree to adopt the term "working hypothesis" which we already had used several times in our text and now also inserted at p. 328, line 13.

«I think that a great deal of the utility of this approach is obscured by an overly complex and (apparently overly) forceful description of the methods as being completely robust. My impression is that the methods are indeed robust, but ONLY relative to a number of key assumptions. These could probably be stated more clearly and especially more simply in the manuscript.»

At p. 935, top paragraph, we now add the required short listing of key assumptions, in addition to the key assumptions we already listed for calculating the early deglacial D14C drop at p. 944, line 25, to p. 945, line 5. The assumptions are:

– The modern variability range of the regression slope DIC vs. D14C constrains the range of slope variability during the LGM.

– The regional slopes of DIC vs. D14C are regarded constant as long as overall carbon fluxes were constant.

– Most deep-water ventilation ages were not estimated as projection ages.

– The LGM circulation patterns were basically similar to today, but slower. – Global export production is assumed constant, thus decoupled from the transit time of ocean overturning circulation, because of non-constant carbon-to-nutrient element ratios.

«Furthermore, I think that tests of some of these key assumptions, using simple model (box- or EMIC) experiments, or using auxiliary proxy data (e.g. for oxygenation, carbonate ion or nutrient concentrations), are possible and might be included in a revised manuscript.»

As shortly outlined at p. 935, paragraph 2, we seriously considered model tests of our key assumptions. Moreover, we tried to test them in cooperation with the kind support of colleagues in Hawaii and Australia by means of two different General Circulation Models (MIROC and Bern-3d), tests that were unsuccessful for the reasons outlined in our text. In turn, in our opinion box models are inadequate to meet the complex and interactive scenario of the past ocean and global carbon cycle, as already discussed in our answer to Ref #1. Thus, a simple two-box model calculation can demonstrate that an explanation is physically possible, but cannot provide a quantitative verification. Thus we regard these tests as possibly misleading, since they won't provide robust results but stretch the paper significantly. – However, we'd feel pleased to start a future cooperation project with Ref.#1 and/or #2, where they may test our data sets by means

of models they have in mind, since none of us co-authors is a box modeller.

p. C230, last paragraph. «Indeed, I think that one way to improve the communication of the approach advanced in this manuscript and to defray over-zealous and non-specific criticism of it as 'too ambitious' or subject to 'too many unknowns' would be to simplify its description greatly, and to recast it as a 'thought experiment' or 'working hypothesis' based on clearly stated assumptions, as opposed to an unequivocal derivation from fundamental/necessary chemical principles. If the arguments presented in the manuscript are intended as robust derivations, they will be critiqued as such, and are likely not going to be able to stand up to such criticism. However, if presented as a working hypothesis that deserves some consideration and that is based on clear unproven assumptions that have yet to be tested, then the manuscript becomes less prone to criticism and becomes more useful for the palaeoceanographic community. »

We agree with the referee and try to recast the wording of the text accordingly.

«Again, some simple numerical model tests might go a long way in helping to demonstrate what this method can tell us and what it cannot (or what needs to be tested before we can infer much more).»

We fully agree that our hypothesis needs to be tested by box- and more sophisticated models. However, a number of issues first need to be solved, e.g. the question of POTALK distributions as outlined above. To our knowledge, at least two GCM models (bern-3d and MIROC, unpublished) were able to reproduce the observed DIC-age relationship, though showing essentially the same regression slope for the LGM. However, due to the experimental setup of using a prescribed atmospheric CO2 concentration for the LGM, which in fact reduces the total amount of carbon in the system under consideration, MIROC was unable to show an increase in the deep-ocean carbon inventory. For these reasons, we'd like to constrain the present paper to our data-based evidence and the hypotheses arising from it.

Comments on p. C231, pp. SPECIFIC COMMENTS:

«p.927, line 19: .... If the authors mean that Skinner (2009) presented a 'ceteris paribus' thought experiment to explore the impacts on marine carbon cycling of water-mass volume changes, while the present manuscript presents a ceteris paribus thought experiment to explore the impacts of changing transit times in the ocean interior, then this is a useful statement that might be made more clearly.»

O.K. We modify our text accordingly, presenting a ceteris paribus experiment.

«p.931, line 24: is it worth noting here that the equilibration time for radiocarbon across the various carbon species in seawater is much longer? What implications might this difference have (order year vs. order decade)?»

The chemical reaction time between various carbon species in seawater is on the order of seconds and minutes, thus irrelevant to the processes discussed in this study (Vogel et al, 1970; Z. Physik, 270).

«p.932, line 3: It would be helpful for the reader if this paragraph stated something like "Below/In section 2.3 we assess the storage capacity: : :"» «p.933, line 10: please state exactly what volume is considered here (is it >2,000m water depth; i.e. what % of the total ocean volume?).» «p.933, line 12: This paragraph is not extremely clear. It might be better to use a word like 'input', rather than 'picking up', since (although vague) the latter implies to me a net change, rather than just a positive flux, that is augmented by other positive fluxes and balanced by other negative fluxes. Also, the last line of this paragraph is not clear at all to me; please clarify.» «p.933, line 29: please add depth range after "deep ocean".. is it >2,000m? »

We thank the referee for these suggestions and clarify our text accordingly.

«p.934, from line 3: The whole discussion of the 'probability argument' regarding the 'variability test' is not at all clear to me, in the sense that I cannot see that it provides anything beyond an ad hoc argument that the modern spatial variability in radiocarbon-DIC calibrations is likely broader than the temporal variability since the LGM. This argu-

ment is backed up with estimates of the likely sense and magnitude of changes in some of the parameters, such as dust flux and export productivity, that might have occurred since the LGM; but no further test of the argument can be advanced, and it is noted that GCM simulations might eventually be able to provide such a test. This section reads as a survey of the modern spatial variability in the radiocarbon-DOC scaling, where the most divergent regional relationships are discounted as being outliers. This comes across as a weakly convincing argument that the modern spatial variability provides a conservative estimate of the temporal variability (driven by spatially heterogeneous changes in circulation times/routes and export productivity) since the LGM. My view is that this argument might be more convincing if it was presented not as a test, but as a hypothesis that can be defended but that also needs to be tested fully with clearly stated measurements/ experiments.»

With thanks to Ref.#2 we adopt these suggestions for text revision. We now replace the term 'variability test' by 'Evaluation of the modern DIC / D14C relationship'.

«p.935, line 21: for the 'projection [age] method', it is probably most appropriate to cite its originator: Adkins, J.F., and Boyle, E.A., 1997, Changing atmospheric Delta-14C and the record of deep water paleoventilation ages: Paleoceanography, v. 12, p. 337-344.»

We agree.

«p.936, line 23: The point is well made .... But I also have a question regarding the 'plateau tuning' technique:»

Following the referee's suggestion we add a new Fig. S1a-f to the AUXILIARY MATERIALS, that displays all planktic and benthic 14C time series based on the 14C plateau technique.

«if this technique demonstrates in a sediment core large changes in surface reservoir ages, should the technique not be re-applied to the sediment core with those reservoir ages subtracted from the original radiocarbon dates, at least as a test to see if the apparent position of the plateaus does not change, and therefore that zero reservoir ages can be inferred for the 'corrected dates' as a result? »

In some way, that is precisely what we display in our new Fig. S1 in the Auxiliary Materials, where the suite of plateaus is compared with the time series of atmospheric plateaus in the varved Suigetsu record (with zero reservoir ages).

«In my experience, plateaus in radiocarbon dates in sediment cores can represent a large change in surface reservoir age that need not correspond to a radiocarbon plateau in the atmospheric record. How does the plateau tuning technique hold up when there are large changes in surface reservoir age? In extremis, it seems that the method should work best if there is zero reservoir age variability and not at all if there is enormous reservoir age variability.»

All 14C plateaus are defined by comparison with particular structures that mark both the whole plateau suite 13–23 ka and single plateaus (e.g., long vs short plateaus; two-step plateaus; etc.) in the varve counted Suigetsu atmospheric 14C record (Ramsey et al., 2012, Science 338). Thus we can specify irregularities within a plateau suite and single plateaus. For example, a short-term drop in reservoir ages results in fragmented 14C plateaus and enlarged 14C jumps as found in various cores near the end of HS-1, whereas a rise in reservoir ages leads to reduced 14C jumps (further details in Sarnthein et al., 2007, AGU Monogr. 173, 175; and 2011; EPSL 302). This approach rests on two assumptions: (1) Reservoir age changes of surface waters take 10–100 years (per analogy to shifts from DO stadials to interstadials), whereas 14C plateaus last over 400 – 1000 yr. (2) Reservoir age regimes are conservative, lasting over several hundred to several thousand years per analogy to the duration of Dansgaard-Oeschger and Heinrich stadials and interstadials, hence may cover time slices of several successive 14C plateaus.

«p.937, line 16: To ignore intermediate water records on the basis of their 'complexity'

might be more defensible it if was shown that they all yield conflicting results. If we ignore a large volume of the ocean, it becomes difficult to infer changes in 'average ocean chemistry'. Can the authors provide an estimate of the extent to which changes in intermediate ocean chemistry could counteract the changes that are inferred in this manuscript in the deeper ocean?»

Yes, we now cite various papers (Bryan et al., 2010; Burke and Robinson, 2012; Marchitto et al., 2007; Sarnthein et al., 1994; Curry and Oppo, 2005; and others) that display temporal d13C and D14C-based trends of intermediate-water chemistry, which were reversed to those in the deeper ocean.

«p.938, line 1: Is it hard to get good 'average' estimates for HS1 because the records conflict, or because they are all changing in different ways? This starts to sound like the case of intermediate waters described by the authors previously; is it therefore just as difficult to say anything robust about HS1, across the whole ocean? »

On the basis of our fragmentary evidence of D14C records it turns out almost as difficult to specify the gross circulation mode for HS-1 deep waters as stated for past intermediate waters, except for a minimum statement: The HS-1 North Atlantic appears as a dead-end road for extremely old deep waters advected from the Southern Ocean, whereas the North Pacific was marked by overturning convection similar to that of the modern North Atlantic (e.g., Gebhardt et al., 2008). The circulation system of the Southern Ocean remains largely unknown.

«p.939, line 28: 'evaluate' might be better than 'value'.» We agree.

«p.940, paragraph from line 12: I am not convinced that changes in AABW formation rates or air-sea exchange would have no impact on the calibrations that are proposed. It is suggested that this will have no effect because the modern calibration is the same for NADW and AABW, but why does this follow? Surely the potential problem is precisely to occurrence of a non-analogue situation in the past.»

In this case our poor wording of line 15 has caused some confusion. Based on GLODAP data the regression slope of the DIC–age ratio of AABW indeed matches that of NADW. However, the intercept of this relationship has slightly shifted toward higher DIC values ("bump" in Figure 2a; p. 930, line 23), since the newly ventilated source waters of AABW (originating from upwelled NADW) are more nutrient enriched than waters downwelled in the source regions of NADW. Following this rationale, any rise in the flow rate of AABW in the past will further increase the DIC estimates of past ocean deep waters. We now correct our text accordingly.

«Again, my understanding is that the modern spatial calibration is taken here to apply as a temporal calibration, regardless of possible changes in circulation geometry/rates, and the question is now can this be defended as plausible and what tests might exist for verifying this? Indeed, the reference to the study of McCave et al. (2008) is problematic, rather than helpful I think, because this study found more positive stable carbon isotope values in the deepest (abyssal) south Pacific and inferred that is represented the input of newly formed deep-water from around Antarctica, underneath more 'aged' water that included an Atlantic component. How does this support the authors' claims?»

In harmony with Ref.#2 we need a more detailed wording for lines 17-19, where we refer to the study of McCave et al. (2008). In particular, these authors show a major 13C depleted deepwater mass at $\sim$2000–3800 m w.d., assigned to NPDW and UCDW, where the LGM ventilation has dropped by Δ1.25 ‰ d13C as compared to the Holocene. Further below, however, the generally better ventilated LCDW that stems from circum-Antarctic downwelling was likewise 13C depleted by 1.25 ‰ during the LGM, starting from a higher 13C reference level. Accordingly, both water masses were subject to the same degree to a negative 13C shift. This match of trends implies that the downwelling of less juvenile and more carbon-enriched surface waters around Antarctica continued from the Holocene to the LGM. Taking d13C as rough proxy of D14C estimates, the distinct stratification of circum-Antarctic deep waters may also add to a better understanding of the two highly divergent D14C values found in the Atlantic sec-

tor of the late LGM Southern Ocean (Fig. 3). Whereas apparent ventilation ages reach 3500–3800 yr in Core MD07-3076 at 3770 m w.d., those of Core TNO57-21 near 5000 m depth do not exceed 1000-1300 yr and possibly record a near-bottom incursion of juvenile AABW.

«p.940, last paragraph: In my view, there is an absolutely fundamental point that needs to be made with regard to the method presented in this manuscript and with regard to Figure 3. This is that changes in average ocean chemistry can only be inferred from point estimates from around the ocean if the volumetric 'representativity' of each point estimate is known (or guessed). An average that is calculated, for example from data in Figure 3, must represent a volume weighted average (and must take into account assumptions regarding the ocean <2,000m for example). This problem is why many workers in this field have struggled to make strong statements regarding past deep ocean radiocarbon concentrations and their implications for the global carbon cycle. The authors really need to address this limitation in their study I think.»

After an in-depth discussion of this suggestion, we arrived at the opinion that volumetric estimates on the basis of 'objective mapping' / 'gridding' are problematic (also see our text p.929, paragraph from line 20). In a recent correspondence with R. Key, where we pointed out our problems with discrepancies between GLODAP raw data and gridded data, he himself referred in his response to the large, only "guesstimated" inventory errors of the gridding technique and largely corroborated us in our conclusion to stick to the use of raw data.

«p.941, line 1: There is a further study using the radiocarbon data of Skinner et al. (2010), combined with new Nd isotope data measured in parallel that argues for an increase in the southern end-member ventilation state between HS1 and the pre-Boreal. If this paper emerges in Geology soon enough, it might be useful for the authors here.»

We just discovered this useful paper in Geology and enjoyed reading about millennial-scale pulses in deep-ocean ventilation during deglacial times.

«p.941, line 11: I think it is important to stress that it is "modern [spatial] variability" that is being referred to.»

«p.941, line 15: instead of "turned out", it might be better to write "appears to have been" or similar.»

With thanks we accept these suggestions for text revision.

«p.941, line 20: I do not understand the meaning of the last sentence of this paragraph, please clarify (especially with regard to the suggestion of McCave et al. (2008) that the deepest southern Pacific was less depleted in 13-C than shallower depths). Is the argument that the more positive d13C values in the abyssal ocean were due to reduced air-sea exchange under sea ice? This would not work, as it would drive values more negative surely?»

McCave's vertical d13C transects across the Southern Ocean document for abyssal AABW at >3500 m w.d., which are marked by generally more positive d13C values, a 13C depletion by 1.0–1.3 ‰ for the LGM relative to the Holocene. As already lined out in our response to comments to p. 940, this d13C shift precisely matches the 13C depletion of the more negative d13C values characteristic of deep waters at 2000-3500 m w.d., ascribed to NADW sources. Accordingly we feel entitled to conclude that the LGM AABW originated from upwelled NADW and Circumpolar waters that already were strongly 13C depleted and enriched in DIC when downwelled. The short-term exposure to the atmosphere led to a relative rise in d13C and D14C, but was insufficient to delete the inherited preformed-carbon signature of upwelled NADW, similar to today. – We now clarify the wording of our text accordingly.

«p.941, line 25 (cont'd p.942): This seems problematic; through their method, the authors infer a significant increase in the carbon storage of the deep ocean during the LGM that is much more than is required to account for the atmospheric change CO2 (and the glacial terrestrial carbon release). So where is this extra carbon coming from? The line of discussion seems to explore the possibility that the intermediate ocean may

have provided this 'extra' carbon to the deep ocean, but it is decided that the available data is too sparse to decide if this is correct. Nevertheless, surely we can say more than that, or at the very least we can say what observations we would expect to be able to make IF the inferences of the authors are correct. For example, would we expect reduced ventilation ages in the intermediate ocean during the LGM, increased oxygenation, increased preservation of carbonate etc: : : and by how much, if the modern spatial calibrations are inferred to apply to the past? This point seems to be absolutely crucial; if the method applied by the authors to infer the change in the deep ocean's carbon stock leads to a further mystery of where all the carbon came from, then that needs some discussion. I suspect that carbonate compensation and changes in alkalinity may come into this discussion, but it is not clear to me that this is resolved later in the manuscript when such things are discussed.»

As pointed out in our response to the Ref.'s comment on p.937, line 16, we now add citations of various papers (Bryan et al., 2010; Burke and Robinson, 2012; Marchitto et al., 2007; Sarnthein et al., 1994; Curry and Oppo, 2005; and others). They clearly display temporal d13C and D14C -based trends of intermediate-water chemistry, which were just reversed to those found in the deep ocean. On the basis of these trends the ocean intermediate waters form the most suitable reservoir to provide the 'extra' carbon to the LGM deep ocean.

«p.943, line 6: This paragraph is not that easy to understand. My understanding is that it is saying that we can infer a decrease in ocean carbonate ion concentration from the authors' interpretations, and that this would be compensated for (on the multimillennial timescale of glaciation/deglaciation) by carbonate dissolution. But what does this mean? Does it mean that the modern spatial calibrations applied by the authors would in fact not apply anymore, since these are based on constant carbonate chemistry of the ocean? »

Yes.

«Does it mean that the authors' inference that the deep ocean took up way more carbon than 'needed' to account for atmospheric/terrestrial carbon inventory changes is made even more problematic, since carbonate compensation would lead to even more CO2 dissolving in the ocean?»

Yes. It implies that the LGM deep ocean may have absorbed even more DIC than suggested by our fairly conservative estimates. Moreover, the newly added Fig. 7 displays a widespread depletion of carbonate ion concentrations in the deep ocean in support of enhanced carbonate dissolution.

«p.944, line 8: do the authors mean: "does not yet allow even a rudimentary: : :"?

Yes.

«p.944, line 26: The suggestion that atmospheric radiocarbon production remained nearly constant (more accurately we might say that it cannot account for the observed atmospheric radiocarbon changes on its own) across HS1 should properly be attributed to a study of Be-10 fluxes or palaeomagnetic field strength (e.g. Muscheler et al., 2005; Laj et al., 2002).»

In harmony with our response to item 2 of Ref.#1, we assume a nearly constant atmospheric radiocarbon production.

«p.945, first paragraph: I think it is important to make clear that these calculations are based on assumptions regarding the volumetric weighting of the data used in this study (see my comment above) and that they are also based on assumptions regarding the invariance of intermediate water ventilation ages and carbon storage: : : despite the fact that the inferred magnitude of the deep ocean carbon inventory change would suggest that some other reservoir, perhaps within the ocean lost carbon at the LGM.»

In harmony with our response to the Ref.'s comment on p.940 we arrived at the opinion that volumetric estimates on the basis of 'objective mapping' / 'gridding' are problematic. Moreover, they won't lead to final results that were essentially different from those

established on the basis of our arithmetic averaging.

«p.945, line 8-19: the use of ratios (e.g. 1105/1355 GtC) is confusing: : : what does it refer to; the mixing ratio of radiocarbon/carbon in the atmosphere? Please clarify.»

We do not mean 'ratios' but 'ranges' of DIC, etc. stored in the ocean. The text is clarified accordingly.

«p.945, line 18: replace 'were' with 'was'.»

We agree.

«p.945, last paragraph: The inferences presented here are pretty crucial, but they seem to slip away from much discussion. Are the authors predicting that intermediate waters should show a net increase in their carbon content across deglaciation, »

Yes, we do.

«while the deep released carbon, and therefore that the intermediate ocean should exhibit attendant changes in carbonate preservation (i.e. a reduction) etc: : :? Can the authors state all this in terms of clear test criteria for their inferences, which might be sought out in the geological record?»

As pointed out in our response to comments on p.937, line 16, and p. 941, line 25, we agree and now cite various, though still fragmentary lines of evidence that the intermediate ocean exhibits the postulated net increase in carbon content across deglaciation.

«Line 9 might instead read "conclude tentatively a potential rise: : :" Line 13 should state the water depths that define these terms: ": : :relocation from ocean intermediate waters [to >2,000m..?]".»

We agree

«Line 17 might better read: "The peak glacial deep ocean [appears to have been a] "cold acidic ocean..".»

We agree.

«Finally, this section does not present any of the emergent issues that will need to be addressed in order to assess the full (as opposed to the conditional) validity of the paper's claims. I think that this is the place for a short sentence or two on what tests might be performed in future, e.g. with regard to intermediate water ventilation or regional carbon preservation etc: : : Basically, as presented, the conclusions seem over-strident and without any possible contingencies, and therefore as a result seem to lack credibility. Can the authors redress the balance somewhat here while keeping the conclusions short and snappy? If this is not possible, then one might argue that the conclusions are far too contingent on things we do not know or cannot even identify.»

We fully adopt this suggestion and now list at this place in short some most important emergent issues that need to be addressed by tests in the future. In particular, we consider well focused model tests and the need for a better reconstruction of LGM intermediate-water and deep- and abyssal-water ventilation in the Southern Ocean, changing ventilation and circulation patterns in the deep ocean over HS-1, and the patterns of regional carbonate dissolution.

Figures/Tables:

«A really important issue for me concerns the transparency of the data compilation and averaging. I think it is really important to show the data plotted on their respective timescales, with bars indicating the LGM, HS1, B-A and 'modern' windows from which data are collated. I also think that it is important to tabulate these data individually. If anything went into a supplement it would be a table of the compiled data (each core, its location, water depth, citation and the data points for each time-slice). Furthermore, I think it is not really acceptable to include data that are unpublished or obtained via personal communication, certainly without plotting and tabulating these data here. I would propose that the authors make a time-series plot for each region considered in Table 1a, showing the data that are used and the time windows from which data are

excised for the regional averages.»

We adopt this suggestion and present for most sites a new Fig. S1 (copy attached) with time-series in the Suppl. Materials. Fig. S1 also shows how our benthic 14C ventilation ages were derived from the sum of benthic-planktic age diffences and planktic 14C reservoir ages that were reconstructed by means of the 14C plateau method, moreover, where our past time slices were picked. As outlined at p. 930, line 1-2, the 'modern' 14C reference values were picked from the gridded-data map of Matsumoto (2007), except for a modern value measured directly at site MD01-2378. By contrast, we do not include published D14C records that were reconstructed by various colleagues by means of different techniques (except for records listed in Table 1b under items (b) and (d)). All (apparent) ventilation ages under discussion and their variability ranges had already been tabulated in Table 1a.

«Furthermore, as discussed above, Table 1a should at least include volumetric weightings for each region, so that the global average can be estimated properly (these volumetric weightings might also be shown as different symbol sizes in Figure 2 for example).»

For reasons already outlined above (concerns about unrealistic spatial patterns that resulted from the 'objective mapping' method), we do not enrich our reconstructed D14C data in Table 1a by rough volumetric weightings that refer to the various major ocean and depth regions the data represent. However, we now add a scale to Fig. 2a, that relates different data colors shown to specific ocean longitudes, where the data come from, and reflects a fairly homogenous coverage of the modern ocean by GLODAP data. Thus, our simple approach continues to use unweighted arithmetic average values to be compared with the arithmetic averages of modern D14C and DIC in Fig. 2a, where the arithmetic averages below 2000 m may correspond by and large to volumetric averages. The caption of Fig. 2 is now supplemented accordingly.

«Figure 2 is absolutely crucial for the manuscript. I would suggest that it might be rendered much more clearly. The Figure contains a lot of information that can eventually be drawn out, but it is not very clear, and it is very hard to see what the plots intends to show at a glance. I can think of a few different ways of showing the same information that might be less confusing. But this might require more direct discussion of the figure in the text (e.g. "Figure 2 shows: : :. it can be seen from... that: : :"); this currently isn't really the case as far as I can tell.»

We follow this suggestion and enlarge the discussion of Fig. 2a in text section 2.2 (p. 930, 2nd paragraph). Fig. 2b (copy attached) is explained with more detail in the first paragraph of section 4.2 (p. 940). Also, we now insert small letters in Fig. 2b, that relate each reconstructed ratio to the details specified in Tables 1a and b.

«One suggestion is to include a 'cartoon' figure before Figure 2 that demonstrates the impacts on radiocarbon/DIC of the biological pump, solubility pump etc: : : so that fewer labels and arrows need to be included in Figure 2. I would also note again, that the averages shown in Figure 2 should be volume weighted averages and they might be best included in the figure as symbols with estimated error bars.»

Thanks to this suggestion of Ref.#2 we developed a small cartoon Fig. 2c (copy attached) that demonstrates the differential impacts of preformed carbon, the solubility pump and the biological pump on the regression slope of Figs. 2a and 2b. – Error bars are now added to the averages shown in Fig. 2a. In Fig. 2b the average ranges already now display a broad variability range that considers the range of differential DIC/D14C regression slopes characteristic of the different ocean basins. In our opinion, any further error bars would simulate a precision that cannot be supported by robust data.

«Figure 3 is a sort of hybrid of a 'cartoon' and a data plot. I think it is important to stress in the caption that the arrows are not really data, but inferences (and highly simplified ones) regarding the mass transport in the ocean interior. It cannot be said that the arrows show waters with a certain ventilation age if there are no data where the arrows are, surely. They are extrapolations based on a hypothesis of the ocean's circulation

geometry – this is acceptable in my view but the distinction between inferences and data needs to be made clear.»

We agree and supplement the caption and the text accordingly.

«Figure 4: again, averages need to be volume weighted surely; how is this done for the LGM?»

As discussed in the context of Table 1a, the modern arithmetic averages in Figs. 2a and 4 may roughly correspond to volumetric averages, since the ocean-wide distribution pattern of GLODAP data is fairly homogenous. The LGM average for D14C was transferred from Fig. 2b, which indeed needs to be explained in the figure caption. A volumetric average would be highly desirable but cannot be established at present.

Supplementary material:

«I am not sure that this is necessary. I do think that there are key supporting arguments/illustrations missing from the manuscript (see comments above regarding figures/tables), but they are not the ones included in the supplement and I would prefer to see such important things in the main text, especially in a journal that can accommodate this such as CPD.»

In our opinion and based on preceding discussions with various colleagues we need to make these statements in the Supplementary Material. If we shift them into the main text, they would interrupt the red thread and disturb the focus of the text.

On the other hand, as outlined above, we now supplement the Suppl. Material by Fig. S1 with key supporting illustrations of time series, Ref.#2 is still missing.

RESPONSE TO ANONYMOUS REFEREE #3

Executive Summary

Five major objections of Ref.#3 are concerning (1) the enhanced LGM dust fluxes in the Pacific sector of the Southern Ocean, (2) large reservoir ages of surface waters identified in subpolar oceans and elsewhere, (3) the role of 'inherited/ preformed' D14C signals of AABW, (4) whole-ocean changes in alkalinity for the interpretation of the D14C–DIC relationship, and (5) the North Pacific ventilation pulse during HS-1 in contrast to records of Jaccard and Galbraith (2012, 2013). The objections to an enhanced South Pacific dust flux, based on Chase et al. (2003), and to the North Pacific ventilation pulse, based on Jaccard and Galbraith (2007, 2012, 2013), are shown to stem from a misinterpretation of the data in those papers. Our evidence for large reservoir ages of surface waters is shown in the new supplementary Fig. S1, whereÂň the assumÂňption of constant surface water ages turns out indefensible. To reconstruct the impact of inherited D14C signals is turning out as problematic in view of the strong centennial-scale variability of past atmospheric D14C levels. Past alkalinity changes are constrained in a new Fig. 7 showing the variations of carbonate ion concentrations.

Reponse to specific questions: «Some problems noted in my 2012 review have been corrected; e.g., a misconception concerning the solubility pump has been clarified. Others have not; e.g., an error in comparing LGM dust fluxes and paleoproductivity of the Pacific and Atlantic sectors of the Southern Ocean remains unchanged (p. 934, last paragraph), so statements in the manuscript are inconsistent with observations (Z. Chase et al., DSR-II, 2003).»

Unfortunately the study of Chase et al. missed almost completely the LGM dust signal from Australia for various reasons. (1) Chase et al. did not know of the main LGM dust trajectory reconstructed by Thiede (1979), that lay to the north of 55°–45°S and spread toward east south east. (2) Accordingly, Chase et al. erroneously focused on flux records from AESOPS cores that were retrieved south of 55°S, southeast of Tasmania, that is far aside from the LGM Australian dust track. (3) Only four supplementary sites they studied, E33-22, E11-2, E14-6, and E20-10, cover the southern margin of the LGM dust belt. And indeed, these sites show a significant LGM rise in the flux of

lithogenic dust (Chase et al., Fig. 5). In our text we now allude to the misunderstanding of these authors.

«In my 2012 review I also requested a clearer description of age control for the 14C records as well as a more precise definition of terminology. For example, how is "ventilation age" defined, and does its meaning differ from that of "apparent ventilation age"? »

Following Ref.#2, a new Fig. S1 in the Suppl. Materials (copy attached) is now giving a clearer description of both the age control and D14C. It displays time series plots of all D14C records based on the 14C-plateau technique. Following Ref.#3, we now define the term "ventilation age" more formally in the Introduction chapter as simple abbreviation of "apparent ventilation age".

«From the description on p. 936 of the paper now under review, it appears that "ventilation age" refers to the difference between DELTA14C of deepwater DIC and DELTA14C of atmospheric CO2. Is this correct? »

Yes, as shown in Fig. S1, this is correct.

«That is, are projection ages (i.e., with reference to DELTA14C of atmospheric CO2 at the time of deepwater formation) never used in this analysis?»

Projection age records are rarely employed in this study for three reasons. (1) Except for Thornalley et al., (2011), no projection-based ages were published for sites at >2000 m w.d. (2) The short-term variability of atmospheric 14C introduces an uncertainty in the inherited age calculation, larger than our plateau-based uncertainty. (3) The new varve-counted atmospheric 14C record of Suigetsu shows for the LGM a first D14C plateau from 24–21 ka, separated by a ~70-‰ drop (600 yr) near 21 ka from a second plateau from ~20.5–17.5 ka, marked by a gradual drop by <10 ‰İn view of the overall order of magnitude of the benthic D14C shifts under discussion and further uncertainties that apply to the derivation of deep-water D14C (e.g., short-term variability), we neglected these shifts for our LGM average estimates of benthic D14C. For subsequent HS-1, deep waters with apparent 14C ages of 1000–5000 yr may contain a D14C "heritage" of LGM waters reaching up to 120 ‰ (equal to ∼1000 14yr) from an earlier 14C-rich atmosphere, making them artificially "young". In particular, this factor concerns the extremely old waters of the deep northern North Atlantic (first identified by Thornalley et al., 2011). The magnitude of the short-lasting ventilation pulse of 'young' northeast Pacific deep waters (Fig. S1) far exceeds what could be expected from an inherited D14C signal (as seen in the Suigetsu record). The B/A provides a further example for a short-term 14C heritage of 'fossil' deep waters, that may reach 100–220 ‰ (equal to 850–2000 14yr), when benthic ventilation age estimates are fairly low for all ocean basins. In part, these may result from rejuvenation by inherited high D14C, a factor so far neglected.

«It appears that the SOM appended to the CPD submission was written in response to some of my comments in the 2012 review rather than incorporating new information into the main text. Specifically, I noted that many paleoceanographers are skeptical of the very large surface-water 14C reservoir ages (sometimes in excess of 2000 years) reported in some studies of the LGM. The assertions in the SOM (Auxiliary Text #1) remain unconvincing.»

To document the origin of the large reservoir ages of surface waters, that we use in this study, and following a suggestion of Ref.#2, we now add to the SOM a new Fig. S1 (copy attached) that shows published and unpublished time series of all D14C records based on the 14C plateau technique. Further and in part very large planktic reservoir ages that were derived by other techniques are justified in the publications cited in the text. Large and variable reservoir ages are also indicated by the Suigetsu record (Fig. 4 in Ramsey et al., 2012, Science 338).

«The vague remarks in Auxiliary Text #2 refer to my comment that one specific challenge to the inferred large surface 14C reservoir ages is the inconsistency between the large surface reservoir age reported by Skinner et al. (2010) for South Atlantic core

MD07-3076 and the much smaller surface reservoir age reported for South Atlantic core TN057-21 by Steve Barker. Sarnthein dismisses the results from TN057-21 (without identifying the core by name) by invoking problematic age control due to CaCO3 dissolution. To the contrary, the extensive work on TN057-21 by Barker and colleagues has produced an extremely robust age model. If an inconsistency between surface reservoir ages of TN057-21 and MD07-3076 remains, then I contend that this raises questions about the reliability of the age model for MD07-3076.»

As outlined in our response to Ref.#2 (concerning p.940, paragraph from line 12) we now realize that the discrepancy between the ventilation age models for cores MD07-3076 (3770 m w.d.) and TN057-21 (4981 m w.d.) may be linked to various reasons. (1) Barker et al. (2009) prescribed a constant planktic reservoir age of 600 yr for LGM to Early Holocene times to derive their age model, an assumption today indefensible (see our text p. 936, paragraph from line 19, and Auxiliary Text #1). In reality, LGM planktic 14C reservoir ages at southern subpolar latitudes may have reached 1500–2500 yr, as estimated by Skinner et al., 2010, which implies that benthic ventilation ages at TN 057-21 were up to 1900 yr larger than indicated. Thus we do not regard the age model of Barker et al. (2009 and 2010) as 'robust'. (2) A foraminiferal fragmentation rate of 40–60 % during LGM and 60–70 % during HS-1 may have biased significantly the age model by a preferential dissolution of 'younger' foraminifera specimens near the sediment surface of the late LGM. (3) Finally, slightly lower ages near 5000 m than at 3770 m may reflect somewhat rejuvenated AABW spreading below NADW, as reflected by the d13C transect of McCave et al. (2008) near New Zealand. – We now include these reasonings into our text at p. 938, last paragraph.

«Nevertheless, accepting for the moment that the large (>2000 years) surface 14C reservoir ages may be correct for some subpolar regions, »

(Certainly large 14C reservoir ages also apply to upwelling regions in low latitudes)

«one must then ask how this impacts the estimation of deep-sea DIC concentrations in the past using the modern 14C-DIC relationship shown in Figure 2a. Sarnthein and coworkers argue that the slope of the 14C-DIC relationship would not have changed significantly between the LGM and today. However, they do not address the possibility that the "intercept" of the 14C-DIC relationship could also have changed. That is, could the older apparent ventilation ages of deepwaters during the LGM simply reflect an inherited signal, that is, the incorporation of subpolar surface waters bearing a large 14C reservoir age at the time of deepwater formation, rather than an increase in the residence time of the water in deep basins? Stated another way, is there any control on the preformed 14C age of newly ventilated deep water during the LGM, and how might variability of the preformed 14C age affect the interpretation of the 14C-DIC relationship? »

Indeed, this presents an important question. Modern AABW formation (text p. 930, line 23, and p. 940, line 12) provides a fine analogue of inherited signals, since AABW stems from 'old', that is already nutrient enriched upwelled NADW. Fig. 2a shows that the intercept of the 14C-DIC relationship for AABW indeed is slightly shifted toward higher DIC values ("bump" in Figure 2a), since the newly ventilated and modestly rejuvenated Antarctic Bottom Waters ab initio are more nutrient enriched than waters downwelled in the source regions of NADW. Based on this example we feel entitled to assume that LGM deepwaters that inherited high ventilation ages of 'recycled' deepwaters necessarily also inherited most of their DIC content. Accordingly the DIC estimates of ancient deep waters presented in our study may present the most conservative DIC ranges conceivable, since each successive event of inheritance will further enlarge the DIC-14C ratio in high latitudes, where sea ice is hindering the outgassing of CO2. Moreover, this conceptional model is supported by the fairly uniform distribution of the modern 14C-DIC ratio over six basins of the ocean, except for the nutrient-depleted Pacific sector of the Southern Ocean, where the biological pump today is dampened (Fig. 5 and discussion at p. 934, last paragraph).

«The problem identified in the paragraph above is the single greatest conceptual prob-

lem with the analysis reported by Sarnthein et al. If the preformed 14C age of deep water masses changed significantly over time, then this would have shifted the intercept for all of the relationships discussed in this paper (14C-DIC, 14C-ALK, 14C-PO4, 14C-O2).»

As outlined above, we see good evidence in support of our assumption that per analogy to DIC-14C the intercept shifts of ALK-14C, PO4-14C, and O2-14C were modest. If inheritance of old water masses induced shifts, they necessarily resulted in enhanced DIC, ALK, and PO4, and a further reduction of O2. However, future experiments with more adequate GCM runs are encouraged to constrain more closely the intercept question for an LGM scenario, by now only constrained by conceptual reasoning.

«Other aspects of this paper require clarification before publication as well. For example, what is meant by "paleo modern carbon - 1.0"? (see Section 4.2 and Table 1). Readers should not be obliged to make assumptions about the intended meaning of critical terms such as this. »

We propose the term fraction of 'paleo-Modern' Carbon per analogy to the modern atmosphere, where the fraction of Modern Carbon (fMC) equals 100 %. Hence, the atmospheric 14C composition of any past time slice is defined as 100 % for our age calculations of that particular time span. Based on the Suigetsu record the D14C plateau of the LGM was 3000 to 6000 yr long, depending on the variability range of D14C accepted (as discussed above in the context of 'projection ages'). Following Ref.#3, we now add this short definition of 'paleo-Modern' to our text.

«Similarly, the presentation of information in Table 1 is extremely confusing. If this paper is revised for resubmission, then I recommend that information about variables other than DIC (i.e., ALK, PO4, O2) be removed and that the information pertaining to DIC be spread out so that each column provides information about only one variable. »

In our opinion the present scheme of Table 1 provides a fine overview of all variables under discussion, hence is helpful and in principle should be kept. However, in harmony with the view of Ref.#3 we improved and/or specified more precisely various aspects of Table 1 to avoid confusion.

«Indeed, since this paper ignores whole-ocean changes in ALK due to CaCO3 compensation, the inferences about changes in ALK (p. 943) are likely incorrect and should be eliminated in any case.»

We agree that we don't employ whole-ocean changes in POTALK due to CaCO3 compensation in our calculations. Instead, we estimate the differential changes of POTALK for the different oceans basins as listed in Table 1a and discussed on p. 943. To reduce further confusion, we now add an new Fig. 7 that exhibits our new fragmentary evidence for glacial-to-deglacial shifts in CO32- (i.e., Alkalinity - DIC), that we derived for all three time slices under discussion on the basis of our D14C estimates and may provide pertinent new insights into the spatial and temporal changes of CaCO3 dissolution and compensation depth.

«Lastly, referring again to Section 4.2, the statements about complete ventilation of the deep ocean during HS1 are unsubstantiated. For example, Sarnthein et al. claim that the North Pacific was well ventilated to depths >3600m during HS1, dismissing evidence to the contrary presented by Jaccard and Galbraith (2012). Now, newer findings from Jaccard and Galbraith (GEOPHYSICALRESEARCH LETTERS, VOL. 40, 1–5, doi:10.1029/2012GL054118, 2013) cannot be so easily pushed aside. It is quite clear from the geochemical evidence that deepwater oxygen levels in the North Pacific, the most reliable indicator of ventilation, did not increase significantly until the Bolling (Jaccard and Galbraith, 2013).»

In our opinion, the answer is easy: 'Yes, we can' push aside the objections of Jaccard and Galbraith. For this purpose our new Fig. S1 now displays the raw planktic and benthic 14C records of all North Pacific cores, the derivation of planktic reservoir

ages by means of the 14C plateau technique, the benthic-planktic age offset, and the resulting benthic reservoir ages. By comparison with our high-resolution 14C records of MD2489, Galbraith et al. (2007) unfortunately have undersampled early termination 1 in their 14C record of S. 887, a paper that forms the basis of all subsequent papers. Thus these authors just missed the massive and robust, >1200-yr lasting signal of North Pacific deep-water formation and continuously were led to wrong interpretations. – Moreover, our detailed 14C reconstructions have shown that upper deep waters were much 'older' than deeper Pacific deep waters during LGM and HS-1, such as today (Matsumoto 2007). Accordingly, one needs to be careful not to mix records of shallower and deeper North Pacific deep waters.

Furthermore, Galbraith's chronology was erroneously based on the assumption of constant planktic water reservoir ages of 950 yr in contrast to an actual variability range of 450–1700 yr. Finally, Jaccard and Galbraith's (2013) 'silver bullet' of authigenic U as new constraint on the oxygenation state of waters has failed its target. As shown in a manuscript we submitted elsewhere, both model results and sediment data imply a deglacial North Pacific overturning that led to a complex long-standing downwelling of low-oxygen intermediate waters down to the deepwater sites we studied. At the onset, the downwelling indeed induced a few-100-yr long benthic 13C minimum at MD02-2489, followed by a short-term distinct positive excursion.

«In fact, it is to the advantage of Sarnthein et al. to accept the conclusions of Jaccard and Galbraith. Otherwise, if the entire deep ocean were ventilated by the end of HS1, as claimed on p. 944, then Sarnthein et al. would have a mass balance problem involving the transfer of too much carbon from the deep sea to the atmosphere. By contrast, if the deep North Pacific remained unventilated during HS1, then the mass balance problem is reduced.»

Following the evidence listed above, we cannot accept the conclusions of Jaccard and Galbraith but arrive at the following conclusions: (1) The North Pacific experienced a major downwelling event down to more than 3600 m w.d. during early HS-1. (2) Based on independent model results (Menviel et al.) the juvenile North Pacific deep waters did not spread far beyond the equator because the time span of downwelling was short, thus did not create a mass balance problem. (3) The downwelling incorporated strongly oxygen-depleted intermediate waters, thus just led to a rise of alkalinity in the deep North Pacific at the onset of this event. (4) The overturning event in the North Pacific led to CO2 outgassing and contributed significantly to the rise in atmospheric CO2.

«In summary, Sarnthein et al. present interesting ideas that are worthy of discussion in the paleoceanographic community, but several problems (including clarifications) need to be addressed before I can recommend this paper for publication.»

We thank Ref.#3 for his valuable comments and objections. Several of them certainly meet the point of our conclusions. However, we see no problem to clarify unclear statements and to refute the objections by plenty of robust evidence. Thus we feel confident that Ref.#3 will now recommend our paper for publication.

ATTACHED FIGURES

Fig. 2b. Variability of modern DIC - age ratio over six major ocean regions (green numbers at upper left) defined in Fig. 5. Regressions slopes are -1.49 μmol/kg (1), -1.43 μmol/kg (2), -1.87 μmol/kg (3), -1.27 μmol/kg (4), -0.79 μmol/kg (5), -1.44 μmol/kg (6). Red oval dots depict LGM regional estimates and variability ranges of DIC – ventilation age ratio, calculated using the assumption of stable regional regression slopes. Following Fig. 2a, Δ14C values for intra-LGM age differences between atmosphere and deep-water (= apparent benthic ventilÂňation ages) and related DIC values are calculated for 'paleo' fMC=1 (Table 1). LGM ventilation ages show end members of <500 yr in the Icelandic Sea (IS) and 3600 in the Southern Ocean (SO) and Northern Pacific, resulting in an average of 2100 yr equal to -230 ‰ of fMC (fraction of Modern Carbon). 4300 yr occur in the shallower and deeper South China Sea (SCS-S and SCS-D), segregated from the open ocean, 4800 yr in the Northwest Pacific (NWP).

NEA = Northeast Atlantic, EIO = Eastern indian Ocean, NEP = Northeast Pacific. Blue numbers (μmol/kg) show mean DIC stored in the LGM ocean, estimated for a mean ventilation age of 2100 yr. Gt C scale at upper right labels DIC mass in the LGM ocean, stored in addition to the modern 38,100 Gt DIC, with 1 μmol DIC/kg correspondÂňing to 8.5 Gt C in the total ocean at >2000 m w.d. (deep-sea morphology of Amante and Eakins, 2009). Biol. Pump = 'Biological Pump', Solub. Pump = 'Solubility Pump'.

Fig. 2c. Raw scheme of global MOC in the deep ocean at >2000 m water depth with bulk D14C values measured for today and concentrations of both spreformed and gradually accumulating biogenic carbon after 1000, 2000, and 4000 yr for today and the LGM.

Fig. S1. Suite of raw 14C plateaus (horizontal boxes) in six marine sediment cores (top panel; core locations listed in Table 1), plotted vs core depth and tuned to the varve-counted atmospheric 14C reference record of Lake Suigetsu (lower panel) (Ramsey et al., 2012; Science 338). B/A = Bølling-Allerød; H1 = Heinrich 1; LGM = Last Glacial Maximum. Planktic reservoir ages result from the difference between the average uncorrected 14C age of planktic 14C plateaus measured in the cores and the 14C age of equivalent atmospheric 14C plateaus numbered 1 – 7. Green broken lines mark uncorrected 14C ages of paired benthic foram samples. Light green record depicts temporal evolution of apparent benthic ventilation ages that sum up the planktic reservoir age and the coeval benthic-planktic age difference. Red horizontal arrow marks apparent modern ventilation age.

Interactive comment on Clim. Past Discuss., 9, 925, 2013.

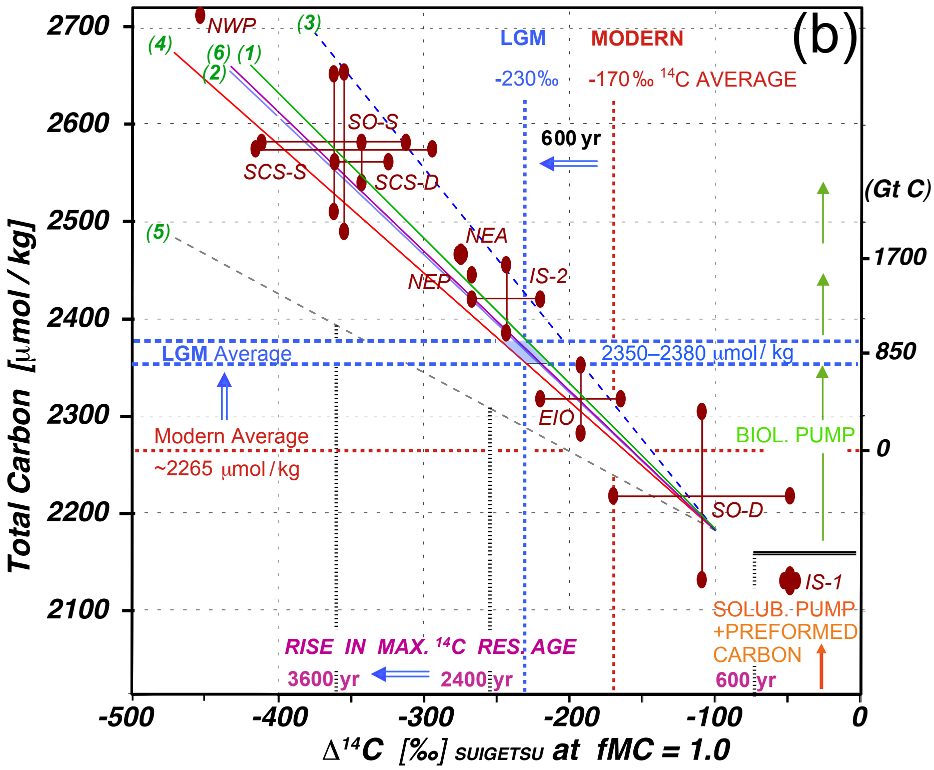


Fig. 1. new Fig. 2b

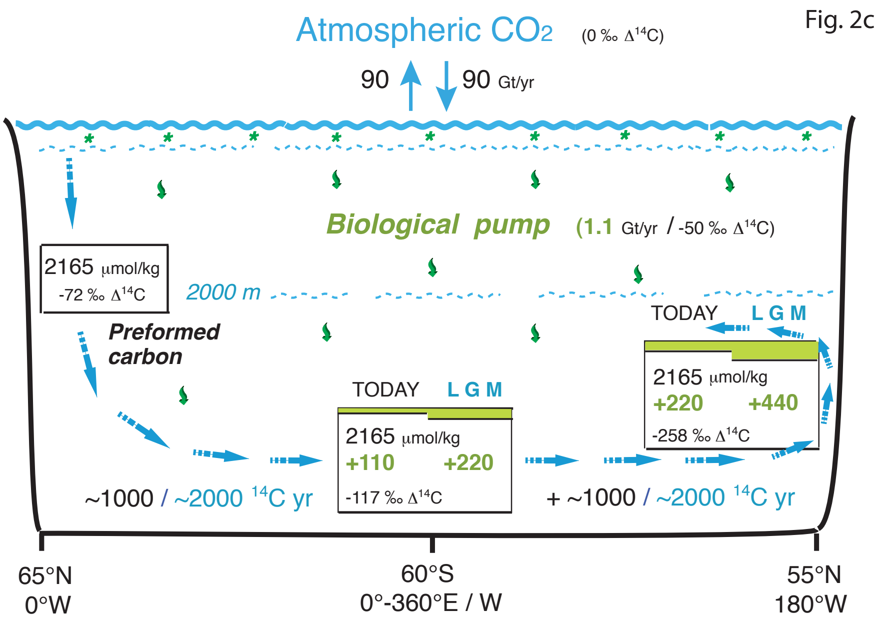


Fig. 2. new Fig. 2c

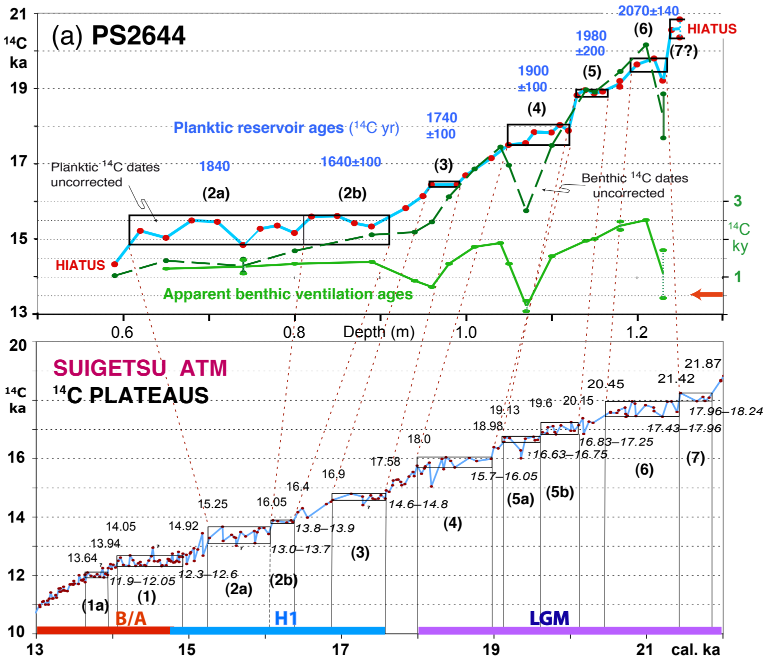
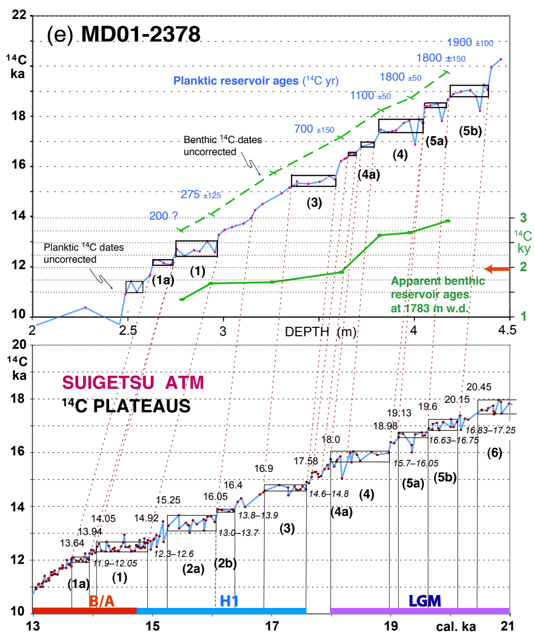


Fig. 3. new Fig. S1a



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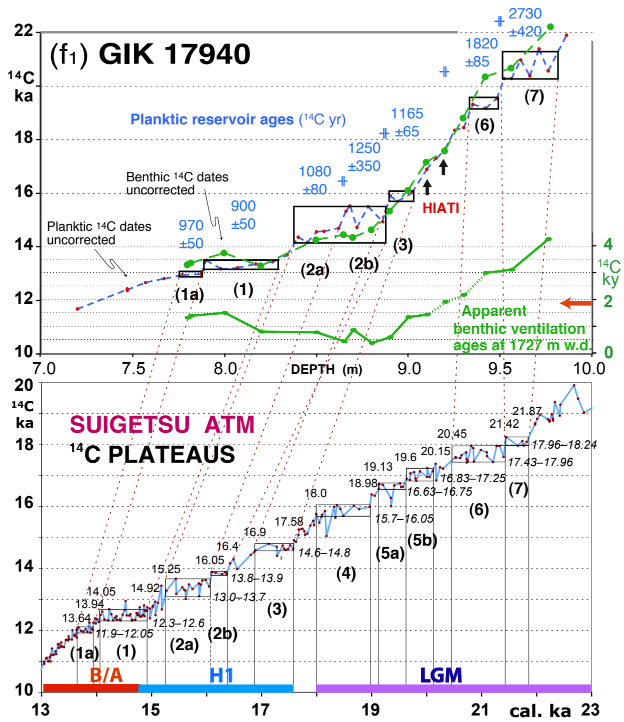
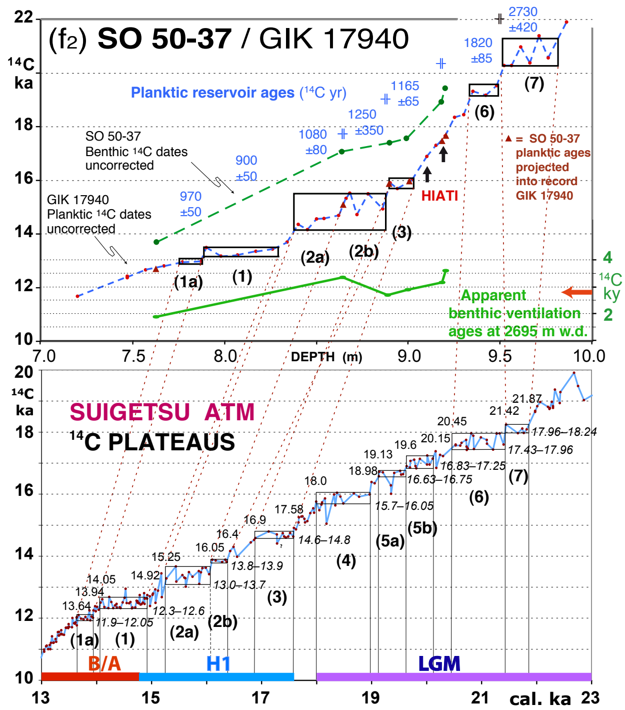


Fig. 5. new Fig. S1f1



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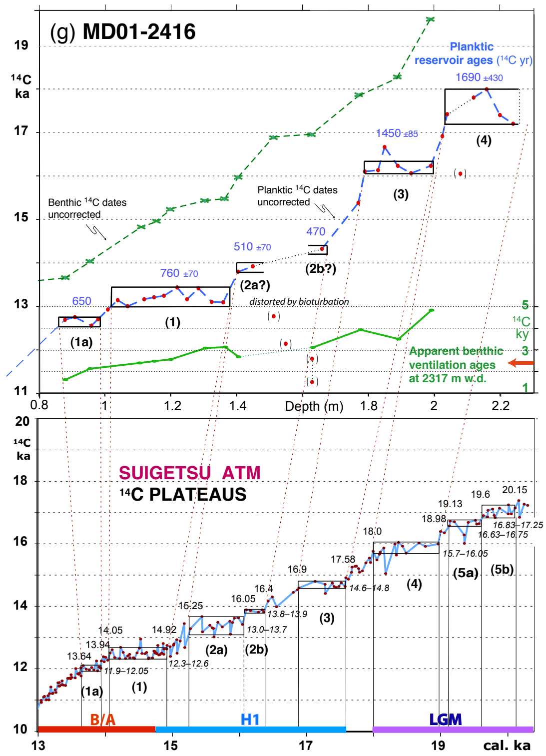

Fig. 7. new Fig. S1g

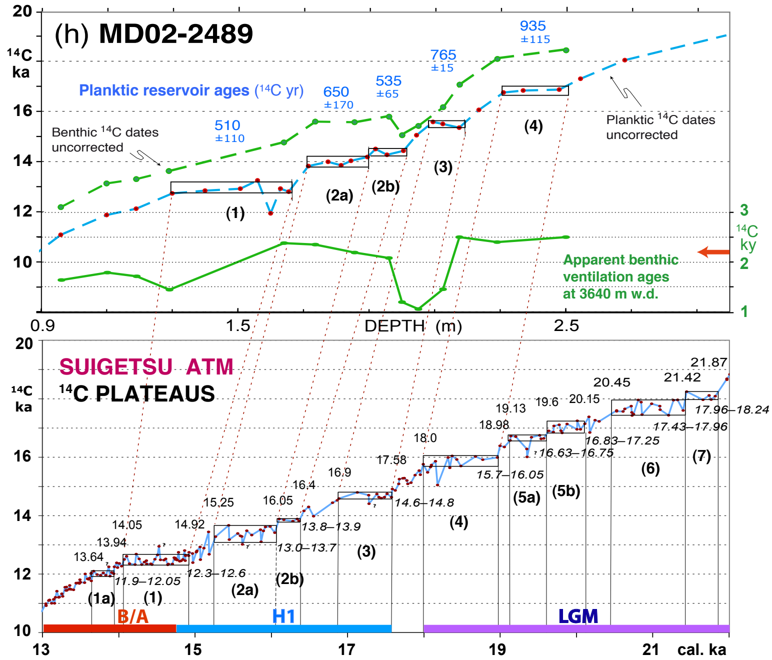

Fig. 8. new Fig. S1h