Final response to the discussion of Koeve et al., 14C-age tracers in global ocean circulation models. Geosci. Model Dev. Discuss. 7, 7033-7074.

Dear Editor, dear Referees,

we thank the five referees and the editor for their comments on our manuscript. We provide a point-by-point response to the comments below.

Colour coding of our 'final response to the discussion': Referee comments are in black, our answers are in blue. At the end of each answer we specify the changes to the manuscript already made or planned. When presented, these are printed in bold type. Cited references are either from the original manuscript or provided in the text.

W. Koeve, on behalf of all authors

Anonymous Referee #1

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Geosci. Model Dev. Discuss., 7, C2285–C2287, 2014 www.geosci-model-dev-discuss.net/7/C2285/2014/

This submission uses three ocean models (two based on transport matrices of MITgcm and one UVic) to simulate a number of decomposed tracers of DIC-14 to investigate the distributions of preformed properties and ages (time elapsed since losing contact with the atmosphere) in the ocean interior. I do not ordinarily recommend an outright rejection of a submission, but I would make that recommendation here on two counts.

First, I do not believe this paper is appropriate for GMD. Even though there is a veneer of model assessment, this paper is not fundamentally about model assessment (or about development/evaluation of a new model or experiment protocol that GMD cares about). The only part that comes remotely close to assessment is the conclusion that the bulk 14C age may not be a good metric to assess the interior ventilation in ocean models. …

Answer 1.1:
We have submitted this manuscript under the GMD category of 'Model Assessment Methods paper'. GMD describes this category as follows: 'Model Assessment Methods include work on developing new benchmarks for assessing model performance, or novel ways of comparing model results with observational data. **Also included are discussions of** novel methods for data analysis or visualisation with relevance to geoscientific modelling, or the application of **existing techniques to this field**. These papers may be theoretical, in which case an example implementation should be provided as a supplement. …' (highlighting by the authors).

Our manuscript provides an in-depth analysis of the contribution of the preformed ^{14}C-age on the bulk (conventional) ^{14}C-age. The distribution of natural ^{14}C has frequently been used as an integrative measure of interior ocean circulation (Matsumoto et al. 2004).

Please see also the editorial comment (#6) by D.M. Roche on this subject.

Referee: …The paper describes a number of decomposed tracers of DIC-14 (section 2.2), but the more useful ones such as ideal age have been around for a long time. Most other tracers are not informative, and some can be diagnosed (e.g., DIC-14_decay=bulk DIC-14 – preformed DIC-14) without the need for explicit simulation.

Answer 1.2: We introduce the tracers ^{14}C-DIC^{decay} and ^{14}C-DIC^{pre} in our paper in order to quantify the effect of tracer mixing on age estimates based on ^{14}C-DIC^{bulk} (see results in section 3.2). It is only the comparison of ages derived from these 'decomposed' tracers together with those from tracers age^{pre}, age^{bulk}, and age^{ideal} which can provide this quantification. This quantification allows to compare the magnitudes of the 'age effect' of mixing and that of ^{14}C-age^{pre} on ^{14}C-age^{bulk}. With these tracers and the respective model runs we provide a detailed description of the contribution of either to global ocean models for the first time.

Further, to diagnose ^{14}C-DIC^{decay} at any point in a model from ^{14}C-DIC^{bulk} assumes that ^{14}C-DIC^{pre} is known or can easily be accessed. However, this is not the case! ^{14}C-DIC^{pre} is only known in the surface water. In the interior ocean the actual values of ^{14}C-DIC^{pre} depends on the mixing of the various relevant end members. While the ^{14}C-DIC end member values itself are known, the mixing ratios are not.

Referee: The second reason for recommending rejection, and this would be the overriding reason, is that I did not find anything new scientifically. There is too much textbook stuff that are discussed as if they are novel: about the longer timescale of isotopic equilibration vis-à-vis timescale of chemical equilibration, the relative importance of residence time versus equilibration timescale in determining the preformed 14C, age bias due to the nonlinearity of aging coupled with mixing (classic example is CFC age), the outsized importance of the small area of deep/bottom water ventilation sites that determine the preformed properties of interior waters, the importance of gas exchange kinetics over solubility in slow-equilibrating tracers like 14C. . .on and on. Most of the submission's figures and text are devoted to making these trite textbook points.

Answer 1.3: We do not claim to be the first to recognize the points the referee lists, but give references to the respective literature. Although the existence of a preformed (or reservoir age) component of ^{14}C-age^{bulk} has long been known, it is often ignored like in studies using ^{14}C-age^{bulk} for data-based evaluation of deep ocean circulation, or treated with rather poor diagnostics, like the top-to-bottom age correction. In the latter, trite textbook knowledge like the role of small areas of deep water ventilation to determine the preformed properties of the interior ocean is being ignored when the local surface ^{14}C-age reading from a planktonic foraminifer is used to estimate the reservoir age or preformed component of a deep ocean ^{14}C-age reading from a benthic foraminifer. In most of the oceans (excluding the small areas of deep/bottom water formation), this 'pelagic ^{14}C-age reading' has nothing in common with the correct preformed age of the respective deep-water reading. Still this is a frequently used approach.

In our study we provide a detailed analysis of the preformed ^{14}C-age in three global ocean carbon cycle models and discuss the pitfalls of ignoring (or improperly correcting for) this component when ^{14}C-DIC is used for the evaluation of deep ocean circulation. There are only very few studies which provide similar quantitative material (e.g. Matsumoto, 2007 for one depth layer of the ocean; see other references in our paper), but to our knowledge this has not been done in the context of data-based model evaluation of circulation. The figures in our paper are presented to make this point.

In the revised version, we will delete Fig. 5 and replace it by a discussion of the respective literature.

Referee: I was actually looking forward to reading this work, but I did not learn anything new from this submission and was disappointed. In fact, there are statements that are either incorrect or quite careless. For example, on page 7035 line 13: "14C is naturally produced in the upper atmosphere to reach rather constant atmospheric levels and enters the ocean via gas exchange." There is nothing about the nature of 14C production that leads to steady state budget of 14C in the atmosphere; rather it is the balance between the production by cosmic bombardment and loss by decay and exchange with the oceans and terrestrial biosphere.

Answer 1.4: Please note that from early on (first sentences of introduction) we emphasise the context of our study: The role of the ocean under a changing future climate. Where the (preindustrial or current) circulation of models used for this purpose has been evaluated by means of ^{14}C data, modellers assume homogeneous and constant atmospheric ^{14}C boundary conditions. Such models are typically spun up with constant preindustrial forcing and boundary conditions (including atmospheric ^{14}C) for a couple of thousand years. There are a number of reasons why this pragmatic approach is commonly chosen by the modelling community. These in particular include the rather moderate variations in late Holocene atmospheric ^{14}C concentrations and uncertainties in estimates of the preindustrial three-dimensional distribution of ^{14}C-DIC in the ocean related to the necessary but not error-free correction of the massive bomb ^{14}C invasion into the ocean which happened prior to most of the oceanic ^{14}C-DIC observations.

We understand that from a paleo-climatological view point our phrase may be considered 'careless'. We are of course aware that the atmospheric ^{14}C concentration derives from the balance of a net atmospheric source and the sinks on land and in the oceans. Fluctuations in source and sinks can yield fluctuations in the atmospheric ^{14}C boundary conditions on multi-millenial time scales. In fact, this is already mentioned in the introduction (original ms, p 7035, 23) as one of the issues complicating the use of ^{14}C for evaluation of a model's physics.

Following a **suggestion of reviewer 5 we will delete the phrase 'to reach rather constant atmospheric levels'.**

Referee: Or, on page 7036 line 18: "14C- ages in the interior ocean are not real," which is a very careless statement, because the geochemically measured 14C activity that gives the conventional age is definitely "real." It is contains useful information about the reservoir age (or preformed DIC-14 as the authors would like to say) and time elapsed since losing contact with the atmosphere.

Answer 1.5: Again, please, respect the context of what you call a careless statement! We wrote: 'Hence ^{14}C-ages in the interior ocean **are not real**, are not reflecting the passage time in the interior of the ocean, **but are apparent ages** only.' The fact that ages computed from bulk ^{14}C-data do not present real circulation ages but are apparent ages only is known since long (Broecker, 1979).

We will rewrite to 'Hence ^{14}C-ages in the interior ocean are not real circulation ages. They are not reflecting the passage time in the interior ocean ….'.

Referee: Then there is their final, punch line in the abstract: “if model evaluation would be based on bulk 14C-age it could easily impair the evaluation and tuning of a models circulation on global and regional scales. Based on the results of this study, we propose that considering preformed 14C-age is critical for a correct assessment of circulation in ocean models.” The authors fail to understand or acknowledge that the reason why bulk 14C is traditionally used in model-data comparison is because that is what is both directly measured and simulated. Preformed 14C age or activity may be diagnosed in water column data or reconstructed from archived surface samples for past times, but significant uncertainties are introduced when trying to compare them to model-simulated reservoir ages. So their main model assessment proposal seems completely unrealistic to me.

Answer 1.6: We acknowledge that a one-to-one comparison of model output with measured data of the same property has its strengths: uncertainty of the data is mainly from the measurement error, which is often small. If one just wants to know whether a model is good or bad with respect to representing certain observations this is the best approach. If, however, we also want to understand why a model diverges from the real ocean or whether it appears to represent a certain property well for reasons right or wrong (!), a straightforward comparison may not be sufficient.

Importantly, often there is no ideal match of a process, which we like to understand with the properties, which can be measured with small error. In this case one needs to go through some more specific interpretation and analysis. Such an analysis in turn introduces additional uncertainty, no doubt.

Concerning ocean ventilation, there simply is no ideal tracer of the ventilation age in the real ocean. All tracers like ^{14}C-DIC, CFC (etc.) which may be applied in order to understand the distribution of ventilation age have their specific problems. Concerning the use of ^{14}C-DIC we briefly mention three (introduction 7035-6) such issues (which we are not treating in detail) and focus on a fourth one, the impact of preformed ^{14}C ages which contributes about 50% to bulk apparent ages, and this with a high regional variability.

We agree that particularly for paleo-circulation questions it might be quite difficult to derive correct estimates of the preformed ^{14}C-age (see also our response to referee 2, Answer 2.2 and Fig. R2.1). Concerning the present ocean, however, the available data allow to estimate preformed ^{14}C-age with reasonable confidence (Matsumoto, 2007; Holzer et al. 2010 (JGR; doi:10.1029/2009JC005750); Khatiwala et al., 2012). Note, that in the first paragraph of the introduction we explicitly introduced and restricted the domain to which our manuscript is to contribute: Evaluation of circulation in biogeochemical models used in studies of future climate change.

In the revised version of the paper we will make this focus of study more explicit.

Anonymous Referee #2

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Using different 14C and “age” tracers, the study highlights limitations in using natural 14C to determine the age of a water parcel. The results suggest that 14C age of a water parcel is dependent on the preformed 14C and the aging of the water. They conclude that using bulk 14C age of water masses to test/tune the circulation of a global ocean model might lead to errors. Instead preformed 14C ages should also be included. Please find below a list of comments regarding the manuscript.

This paper tries to understand the bulk 14C age distribution in the ocean and its relationship with "ideal age". I think some interesting conclusions could be obtained but in its present form, the "interesting" conclusions are difficult to grasp. The main conclusion is that using bulk 14C age of water masses to test/tune the circulation of a global ocean model might lead to errors. However as far as I know, the evaluation/tuning of the oceanic circulation in most global ocean models is first based on physical parameters (e.g. T,S). Indeed many global ocean models do not include biogeochemistry and thus 14C.

The authors point to the importance of the air-sea gas exchange parameter in determining the preformed 14C and thus the bulk 14C age. However, as stated above not all biogeochemistry models include 14C and thus the tuning is oftentimes not based on 14C. Finally, even if 14C distribution is used to tune the models, other tracers are also used. While the authors are most likely right in their analysis, they should clearly state that it is relevant only for the models that use 14C as a validation/tuning of the model.

Answer 2.1: We think that we carefully introduced and restricted the domain to which our manuscript shall contribute: evaluation of the circulation in biogeochemical models used in climate change studies. To this end we assume that the respective models include biogeochemistry, in particular CO_2 and related tracers. Adding ^{14}C-DIC and an ideal age tracer is then straightforward. However, considering ocean-atmosphere models without biogeochemistry an implementation sometimes referred to as the normaliszed radiocarbon ratio method (Toggweiler et al. 1989), in which only one additional tracer needs to be implemented, may be combined with the ideal age tracer approach to allow for a circulation model assessment using ^{14}C and ideal age.

We agree, that such a model evaluation is never based on one set of tracers alone. Physical data, namely T and S, are of course to be used. We think, however, that using these alone cannot provide a sufficient evaluation of the circulation in such models. To better understand, for example, whether a model's phosphate distribution matches data for right or for wrong reasons, requires a more indepth analysis. This involves separation into preformed and remineralised P (e.g. Duteil et al., 2012). It also involves an understanding of the intensity of the circulation in the interior ocean which may for example be described by the patterns of the time elapsed since last contact with the atmosphere (ventilation / ideal age). Depending on the domain of interest (e.g. near-surface or deep ocean), a variety of tracers may be used to quantify this 'ventilation' age. Each of them has advantages and disadvantages with respect to measuring the time elapsed since last contact with the atmosphere. ^{14}C is one of them. T and S, however, do not allow to derive the ventilation age.

The natural abundance of ^{14}C-DIC has extensively been used in data-based model evaluation (e.g. OCMIP-2, see Matsumoto et al., 2004). Concerning current climate models ^{14}C is, to our knowledge, at least implemented in the following models: PISCES/IPSL-CM5, HAMOCC/MPI-ESM), CESM1, POP2/CCSM3, CM2Mc-ESM, MoBidiC, NCOM1.4 (Jahn et al., GMDD, 7, 7461-7503, 2014). The natural abundance of ^{14}C-DIC has also been used in to derive rates of $CaCO_3$ dissolution (Feely et al., 2002) from observational data, assuming that it scales directly with the ventilation age.

In our attempt to identify problems of this approach by quantifying the distribution of the preformed age in three different models we intend to improve the respective methods and approaches.

In the revised version of the manuscript we will clarify our intention.

D14C is also measured in benthic and planktic foraminifera, and the difference between the two is used to estimate the ventilation age at a certain location, mainly in relation to changes in ocean circulation. The authors briefly state the paleoclimate use in the conclusion but I think this is an important point that the authors can make. Is this method robust given their results? Would that work in any part of the ocean? Could we compare these measurements with modeling studies? Additional figures might be needed to really answer these questions.

Answer 2.2: We dealt with this issue after discussing our work with paleo-climatologists from Kiel University. In fact, there are marked differences between ideal age (ventilation age) and the top-to-bottom age (see example in Figure R1). We decided not to discuss this further in this paper mainly since we are not experts on paleo-climatology or the potential problems related to the use of ^{14}C-data derived from benthic and planktic foraminifera.

I would suggest to remove section 4 and the associated figures. I don't see the point of the first example (oxygen minimum zone of the Pacific Ocean) and I don't think a good point is made out of the second example: changing Kv alters the oceanic circulation, which thus changes all the age tracers. For the second example to be relevant much more information should be added.

Answer 2.3: In Section 4, Case Studies, we intend to 'demonstrate the adverse effects of neglecting the preformed component of ^{14}C-age' by discussing two examples. The first example is dealing with the correct estimation of the sensitivity of OMZ-water circulation age with respect to certain physical model parameters and the second example is dealing with the correct interpretation of Atlantic Ocean age gradients.

Considering our first example, ^{14}C-age^{bulk} appears to indicate the sensitivity of age to vertical diffusivity (Fig. 11a), with consequences for the interpretation with respect to the intensity of the OMZ (Fig. 11d). However, much of the pattern apparent in ^{14}C-age^{bulk} is NOT related to the ventilation age and the respective interpretation is hence misguided.

We think that to present these examples is important since they emphasize the practical relevance of our claim that 'to consider preformed ^{14}C-age is critical for a correct assessment of circulation in ocean models' (cited from the abstract).

However, following also comments of referee 5, we will improve the presentation in Section 4, which includes modifications of the figures.

Interactive comment on Geosci. Model Dev. Discuss., 7, 7033, 2014.

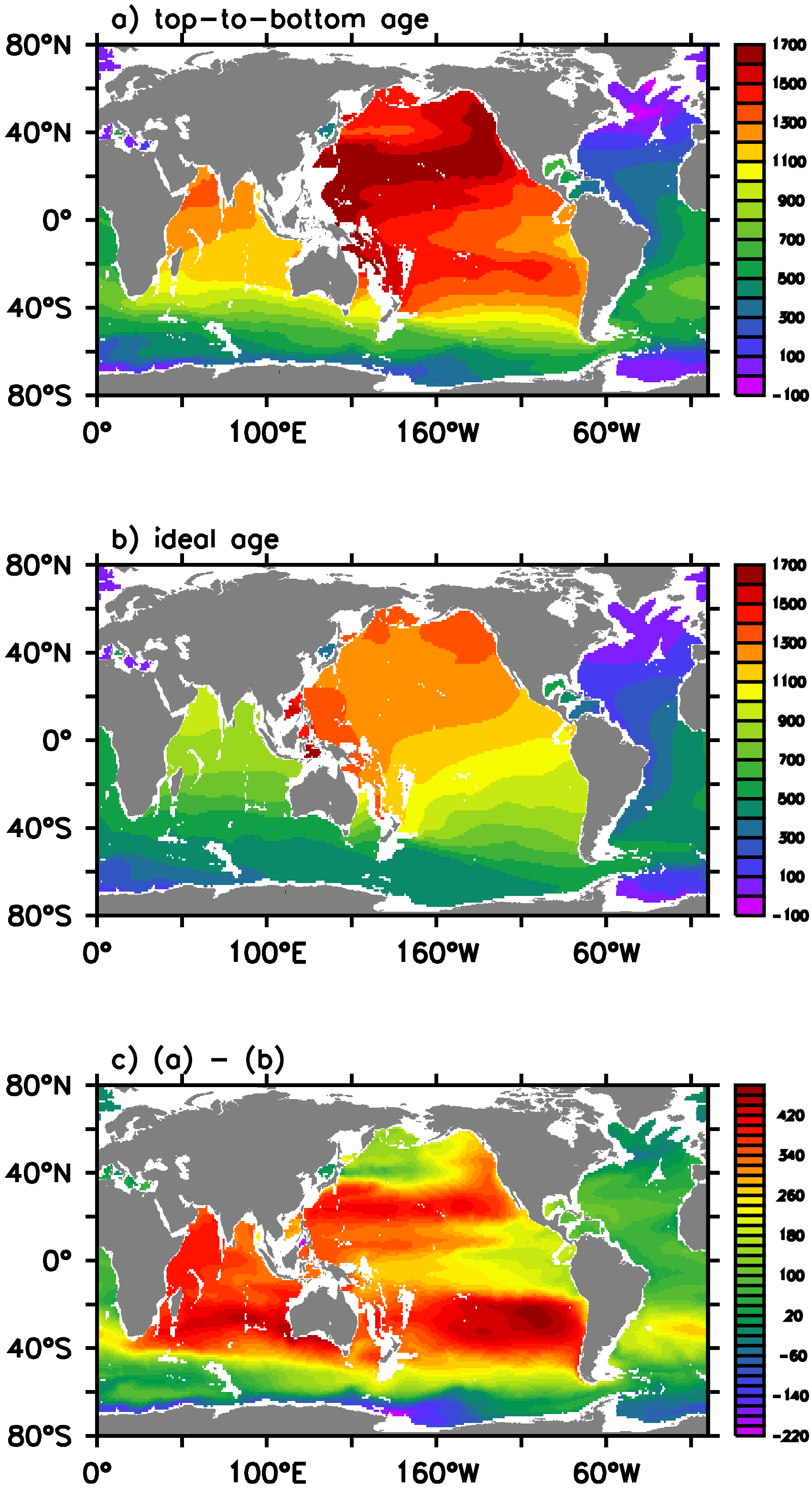

Fig. R1: Comparison of top-to-bottom age and ideal age. Data (z=2000m) are from the TMM-ECCO reference run used in the original ms. (a) ^{14}C-age$^{bulk}_{z=2000}$ – ^{14}C-age$^{bulk}_{z=0}$. (b) age$^{ideal}_{z=2000}$. (c) difference (a) – (b)

A. Schmittner (Referee #3)
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Review Koeve et al.
Scientific Significance: Excellent
Scientific Quality: Excellent
Scientific Reproducibility: Good
Presentation Quality: Excellent

This paper examines the relation between radiocarbon (D14C) and age in a new, interesting, and useful way by introducing the concept of preformed 14C-age. Scientists have long known that the c14 age and the real age of a subsurface water mass are not the same due to finite air-sea gas exchange. But I haven't seen such a clear presentation of the issue before.

The paper is useful for climate modelers and paleoceanographers working with radiocarbon. I recommend publication as is, or, if a revision is undertaken, with consideration of my minor comments listed below.

Answer: We thank the referee for his encouraging words!

Page 7035: Observationalists could object to calling models "the method of choice".

Answer 3.1: **We will rephrase to '… models are often-used tools for studying …'.**

Line 18: I think capital Delta^14C is usually referred to after correction for d13C.

Answer 3.2: We follow the notation used in OCMIP-2, the largest experiment of model intercomparison and data-based model evaluation using ^{14}C carried out so far, which we here consider as a respective reference (Matsumoto and 31 others, 2004). **But we fully agree with the referee that some of the assumptions and procedures of the OCMIP-2 protocol need to be spelled out explicitly in our paper, to avoid any potential misunderstandings. We will improve the methods section accordingly.**

Page 7040: Definitions of 14C-age^pre and 14C-age^decay: Line 18: I don't under- stand why DIC^pre is in the denominator. Using the decay function C(t)=C(t=0)*exp(- t/tau), where tau is 8033 years and solving for t=-tau*ln(C/C(t=0)) it seems to me that C(t=0) should be in the denominator, which, in this case should be 14C-DIC^pre at the surface. Do I miss something? Line 23: Those ratios must have completely different orders of magnitude. 14C-DIC^pre/DIC^pre ~ Rstd~10^-12, while (DIC + 14C-DIC^decay)/DIC ~ 1. Am I missing something ?

Answer 3.3: Concerning line 18, definition of the $^{14}C\text{-age}^{pre}$ tracer. The preformed tracers are designed in analogy to e.g. model tracers of preformed phosphate or oxygen used elsewhere (Najjar et al., 2007; Duteil et al., 2013). In the surface ocean (k=1) and at every time step during model integration, a preformed PO_4 tracer, for example, is assigned the value of the bulk PO_4 tracer in surface waters. Everywhere else in the ocean the preformed PO_4 tracer behaves like a conservative tracer, i.e. is subject to mixing only. With introducing such artificial tracers it is intended to separate the impact of the tracer component transported from the surface ocean into the interior and modified by mixing therein (i.e. the preformed) from the component subject to sinks and sources in the interior ocean (in the case of PO_4, the remineralisation of PO_4 from organic matter decay).

In order to compute the ^{14}C-age^{pre} one needs two preformed tracers: a tracer of preformed ^{14}C-DIC and a tracer of preformed total-DIC. Again, in the surface, both tracers are assigned their values from the bulk ^{14}C-DIC and the bulk total-DIC tracers, respectively. In the interior ocean both preformed tracers are ONLY subject to mixing, i.e. they behave conservatively. The artificial tracer ^{14}C-DIC^{pre} is not subject to decay in the interior ocean, and the artificial tracer DIC^{pre} is not subject to organic matter remineralisation or $CaCO_3$ dissolution!

Now concerning the calculation of the preformed ^{14}C-age based on these tracers, consider the experimental and boundary conditions. We implemented the explicit ^{14}C-age^{pre} tracers only in the TMM models for which the experiments carried out are so-called ABIOTIC runs, as described in detail in the respective OCMIP-2 protocol (Orr et al., 1999). In ABIOTIC runs there is no production or decomposition of organic matter and no $CaCO_3$ cycle. The ocean's DIC is governed by gas exchange with the atmosphere (pCO_2=280 uatm) only. This is affected by the surface ocean distribution of T, S and alkalinity (prescribed in a fixed ratio to S) and ocean circulation. DIC in an ABIOTIC run behaves conservatively in the interior of the ocean, hence DIC^{pre} is equivalent to DIC in these experiments. Following the OCMIP-2 protocol, and for reasons of avoiding potential artefacts in dealing with very small numbers in the model integration, ^{14}C-DIC is used as a normalized concentration, i.e. divided by the $^{14}C/^{12}C$ standard ratio of 1.176e-12. Consequently, the atmospheric boundary condition consistent with $\Delta^{14}C = 0$ in these experiments is: $R_a = (^{14}C/^{12}C)_{atm} = 1$. The standard equation to compute 14C-age from 14C-DIC and DIC tracers, i.e.
^{14}C-age = - tau . ln (1 + $\Delta^{14}C$/1000), with $\Delta^{14}C = (R_o - 1) * 1000$, with R_o = ^{14}C-DIC / DIC
can then be rewritten as
^{14}C-age = - tau . ln (1 + (^{14}C-DIC / DIC – 1) * 1000/1000), or
^{14}C-age = - tau . ln (^{14}C-DIC / DIC)

For the preformed tracer this directly translates into
^{14}C-age^{pre} = - tau . ln (^{14}C-DIC^{pre} / DIC^{pre})

Concerning line 23, the computation of ^{14}C-age^{decay}, please note additionally that the respective ^{14}C-tracer is set to zero at the surface, i.e. all values are negative. Alternatively we could have set this tracer to the value of the DIC tracer, given the underlying assumption of a pure decay tracer under the OCMIP-2 protocol. At the surface, such a tracer is assumed to be in perfect equilibrium with the atmosphere, i.e. with R_a =1, $R_{o(surface)}$ = 1 and hence ^{14}C-DIC = DIC. We assume that there is no difference between our actual implementation and this one. For the latter, however, the age equation would have been different: ^{14}C-age$^{decay-alt}$ = - tau . ln (^{14}C-DIC$^{decay-alt}$ / DIC)

We acknowledge that we assumed familiarity with the OCMIP-2 protocol throughout our original manuscript. This may not be justified, given also that Orr et al., 1999 is grey literature and the publishing website has recently changed. **In the revised version of the manuscript (Material and Methods section) we will provide more details concerning the OCMIP-2 protocol and its underlying assumptions.**

Page 7042: line 24: "background mixing coefficients" are these in addition to a tidal mixing component? Please specify. If so, I suggest to refer to k_bg rather than k_v.

Answer 3.4: Yes, these are in addition to the tidal mixing component.
We will modify the text to reflect this and use the abbreviation K_v(bg) instead of K_v.

Page 7045: line 18 “two water masses of different age”: Which values were used in Fig. 5? 0 and 2000 years?

Answer 3.5:
Following a suggestion of referee 5, Fig. 5 will not be shown in the revised version and the respective text will be changed accordingly.

Page 7047: lines 15-17: “for CO2 the equilibration time is governed by the product of the time scale of gas exchange (order of one month) and the ratio CO2−3 /COaq2 (10–15 in the surface ocean)” Why?

Answer 3.6: To first order, the net reaction of CO_2 invading or leaving the ocean, can be summarized as an acid-base neutralization (e.g. Orr, 2011, in Gattuso and Hansson (ed.), Ocean Acidification, Oxford Press) , i.e.

$$H_2O + CO_2 + CO_3^{2-} \Leftrightarrow 2\ HCO_3^-$$

To arrive at isotope equilibrium, it is hence not the CO_2^{aqu} nor the total DIC pool which matters, but the CO_3^{2-} pool. For a detailed discussion see Broecker and Peng, 1974.

Page 7048 line 12: I don’t see negative ages in Fig. 1b

Answer 3.7: **We rephrase to: “In combination, both effects give rise to moderately negative surface $\Delta^{14}C$ and moderate ^{14}C-ages^{bulk} in the surface”**

Page 7049 lines 23-25: I suggest to add “except at the surface.”

Answer 3.8. **We will rephrase this sentence.**

Page 7053 lines 10-15: I suggest to discuss Schmittner (2003, EPSL 6702 1-10) who examines sea ice effects on bottom water radiocarbon.

Answer 3.9: **We suggest to rephrase to:**
‘Model experiments (Campin et al., 1999; Schmittner, 2003) showed that during the last glacial maximum waters in the deep Southern Ocean and South Atlantic appeared older (older ^{14}C-age^{bulk}) than in the late Holocene. An increase in Southern Ocean ice cover, which inhibited ^{14}C-gas exchange, was thought to explain much of the apparent age increase (Schmittner, 2003). The actual circulation age as measured by an age^{ideal} tracer, however, was younger in the South Atlantic pointing to a more vigorous circulation (Campin et al., 1999). The shift to older ^{14}C-age^{bulk} in that region was at least partly related to the increased invasion of Antarctic Bottom Water with a large preformed ^{14}C-age compared to that of North Atlantic origin . The relative contribution to high preformed ^{14}C-ages from (a) the ice-cover related inhibition of ^{14}C-gas exchange (Campin et al., 1999; Schmittner, 2003) and (b) intensified upwelling of old, ^{14}C depleted, water in the formation region of Antarctic Bottom Water has not been analysed for the last glacial maximum. In the present-day simulations of our study where the impact of ice cover on ^{14}C-gas exchange was switched off, leaving circulation, however, unchanged, this impact was found to be relatively small (Fig. 9 (of old ms version)).

See also our response, 5.11.

The above review was not influenced by reading reviewer #1 and #2's comments. With regard to reviewer #1's comments I don't agree with him/her that there is nothing new here. To my knowledge the concept of preformed c14-age has not been proposed nor used before. This is therefore, in my opinion, a new contribution, even if much of the text sounds like "trite textbook points". I also think that the paper is appropriate for GMD since it proposes a new way to analyze model output.

Andreas Schmittner

Interactive comment on Geosci. Model Dev. Discuss., 7, 7033, 2014.

Anonymous Referee #4

Received and published: 25 November 2014

This paper describes a suite of numerical model experiments to identify the various factors - circulation, reservoir age, gas transfer parameterization, ice cover, etc - that control the distribution of 14C in the ocean. 14C is an important tracer in studies of both the modern and paleo oceans and is frequently used to evaluate model performance by comparing simulated 14C with observations. Understanding what controls its distribution in the ocean is therefore critical. The main conclusions of this papers are that (1) bulk 14C (the directly simulated tracer and what is "observed") is influenced by both circulation (time elapsed since the parcel was at the surface of the ocean and mixing in between) and the "preformed 14C" of the water parcel (the concentration or age - usually called reservoir age - the parcel had when it was at the surface), and (2) the gas transfer parameterization.

Neither conclusions will come as a surprise to those studying ocean radiocarbon. In fact, much of this paper could be regarded as "textbook-ish" in nature and describes results that are generally considered "known". But the literature is full of papers which will mention in passing these caveats and then blithely go on to ignore them and use bulk 14C - simulated or measured as the case might be - to make/jump to conclusions. The present work shows very concisely and clearly the dangers of using bulk 14C in a naive manner. So despite the lack of novelty and that this isn't a 'model development' paper per se I'm entirely in favor of publishing it.

Answer 4.1: We thank the referee for his/her supporting words.
Concerning the interpretation that "this isn't a 'model development' paper per se" please see our answer 1.1 and the editors comment from 20. Dec. 2014

I only have a few comments/suggestions. While I liked the novel use of multiple idealized tracers (Sec. 2.2) to tease apart the different controlling factors, at the end of the day their relevance was somewhat lost to me. For example, for the purposes of this paper it might be quite acceptable to begin with the assumption that nonlinear mixing has a relatively small effect on 14C, such that the bulk 14C age is to a good approximation a sum of ideal age and reservoir/preformed age. That this should be the case under plausible ocean conditions was theoretically demonstrated by Holzer et al. (2010) - which really should be cited - using a Green function approach. Khatiwala et al. (2012) then applied this to ocean Green functions inverted from tracer observations to show that this is indeed the case. Here the authors have demonstrated it to also hold for 14C simulated in ocean models. So while I'm not suggesting the authors get rid of all this, I would suggest they simplify/shorten the presentation, and also try to put their results within the theoretical analysis of the two studies mentioned above.

Answer 4.2: Our original presumption was similar, when we started to work on this problem. However, from the literature we could not derive any good idea on how important the nonlinear mixing effect might be quantitatively. An initial attempt (not shown in the paper) to diagnose the ventilation age offline from the difference of $^{14}C\text{-age}^{bulk}$ and an estimate of preformed ^{14}C-age derived from tracer-based water mass fractions (similar to Khatiwala et al. 2012, but for our model) combined with regional means of surface ^{14}C-ages failed to yield non-zero residuals. It became obvious that the preformed part was dominant, but we also concluded that the mixing effect is detectable. This led us to design the experiments described in Section 3.2, which allow for an explicit quantification of the mixing effect on $^{14}C\text{-age}^{bulk}$ and its components.

We understand that the complex design of the respective experiments and their specific tracers impede on the readability of the paper. **In the revised version we will follow the advice of Refereee 5 to improve the readability of this section. We will further mention and discuss the respective conclusions drawn from Holzer's and Khatiwala's works.**

Incidentally, the authors use the term "preformed age" where I believe the more common term is "reservoir age". Ideally the authors could switch to the latter rather than introducing yet another term.

Answer 4.3: The concept of preformed properties is widely used in oceanography. Preformed phosphate, for example, describes that part of observed PO_4 in the interior of ocean that is not related to PO_4 remineralisation in the interior but due to conservative mixing of PO_4 endmembers. In a similar way preformed oxygen is mandatory to the concept of AOU in the ocean. The earliest reference to the concept of preformed properties dates back to at least the 60ies (Redfield et al., 1963). Hence we are not introducing a new concept here, but apply an existing concept and terminology to ^{14}C-DIC. In fact, we are not the first to do so, but follow Emerson and Hedges (2008) and their textbook on Chemical Oceanography (Cambridge Univ. Press). Actually, the concept of a non-zero preformed component, applies to many tracers one may study in oceanography, including carbon isotopes.

We are aware that the term 'reservoir age' is used widely in the radiocarbon community. However, having been derived from the radiocarbon dating of objects and organisms, we have the impression that it is used in a somewhat variable way (see e.g. Bowman, Radiocarbon dating, Univ. Cal. Press, 1990; Taylor, Radiocarbon dating, Academic Press, 1987). For trees fixing carbon on land, for example, the reservoir age is the age-equivalent of the ^{14}C-content of the atmosphere at the time of carbon fixation (the local atmosphere is the reservoir). For foraminifera living at the surface of the ocean it is the age equivalent of the ^{14}C-content of its habitat (surface seawater is the reservoir). For the $CaCO_3$ shell of foraminifera dwelling in the sediment of some deep-sea location the respective reservoir age is either related to the ^{14}C-content of the seawater overlying the sediment or the sediment porewater. In any case, a correct dating of the above organisms requires the correction for the respective reservoir ages.

Now, considering the attempt to estimate the ventilation age of the same deep-sea location from ^{14}C-DIC, the reservoir age is something very different, i.e. the age-equivalent of the preformed ^{14}C-DIC, i.e. of mixing states of its remote endmembers at the surface of the ocean. It is this ambiguity of the term 'reservoir' age together with the general applicability of the preformed property concept in oceanography that led to our choice in terminology.

In the revised version of the manuscript we will stick to the terminology 'preformed age'. However, we will mention in the introduction that the term 'reservoir age' has sometimes been used synonymously in the literature.

Apart from preformed/reservoir age the other conclusion concerning gas exchange is also interesting and deserves to be better highlighted as it seems from the comparison of ECCO and uVic that even a smallish change in the gas transfer coefficient can easily lead to the opposite conclusion as to which model is closer to observations. In this context I suggest the authors cite Graven et al. (JGR 2012) who looked at the impact of gas exchange on 14C.

Answer 4.4: **We will highlight this aspect better in the conclusion section of the revised manuscript. We will also refer to Graven et al. 2012**.

In summary, I recommend publication of this clean and interesting study that should be quite useful to many oceanographers who exploit 14C for a variety of problems.

Anonymous Referee #5
Received and published: 26 November 2014
The comment was uploaded in the form of a supplement: http://www.geosci-model-dev-discuss.net/7/C2462/2014/gmdd-7-C2462-2014- supplement.pdf
Interactive comment on Geosci. Model Dev. Discuss., 7, 7033, 2014.

The approach and methodology in this paper are interesting. The use of specific tracers for tracking reservoir ages is new in modeling studies of radiocarbon. With these new tracers the authors are able to illustrate and quantify the respective roles of circulation, mixing and boundary conditions in setting apparent ages (or radiocarbon ages) of water masses. The most important outcome of this study is that the preformed component of radiocarbon age in seawater may simply be obtained from the difference between the actual 14C-age and the ideal or ventilation age.

This is not too surprising a result since it is known that mixing only weakly affects radiocarbon ages when compared to ventilation ages (e.g., Deleersnijder et al, 2001; Delhez et al., 2003; Khatiwala et al., 2012). Up to now, however, nobody had demonstrated that the preformed or reservoir age in models may easily be obtained. The new tracers suggested by this work will be beneficial in model assessment and inter-comparison studies.

Answer: We thank the referee for his supporting words.

However before accepting it for publication there are several points which should be improved or corrected.

Main issues

1) The general presentation is rather confused. The text should be reworked in order to gain structure and fluency. I would suggest that the authors systematically make use of the dedicated name (as given in Table 1) when referring to tracers. Some tracers experience change of name or notation within the same paragraph and among sections. It doesn't help the reader.

Answer 5.1: **We will replace our original Table 1 by one, which largely follows the referee's suggestions. We also understand that switching for example between 'bulk ^{14}C-age' and the dedicated name '^{14}C-age^{bulk}', possibly confuses a reader. We will make consequent use of the dedicated tracer/age names in the revised manuscript.**

Another point would be to describe the experimental set-up when presenting the experiments. Section 2.3 should be shortened to the reference model runs and the other material moved to the relevant sections.

Answer 5.2: **We agree with the referee and will consider this for the revised version. Moving these details out of the methods sections is also necessary since we see the need to add more details concerning the OCMIP-2 experimental protocol and the ^{14}C-implementation in UVic to the Material and Methods Section (see several other responses).**

2) Line 10, page 7042, line 20 on page 7047 to line 1 on page 7048: Age and residence time are different concepts. The quantity computed here is the time elapsed since water left the deep sea. Conceptually it is an age, not a residence time. The latter is the time for water leaving the surface reservoir (Bolin and Rohde, 1973; Takeoka, 1984).

Answer 5.3: The referee is of course correct. **We will change the wording accordingly. (See also the next response.)**

Further this diagnostic does not seem appropriate to evaluate the capacity of waters to equilibrate with the atmosphere. Indeed water parcels may remain close to the boundary and meander in and out of the surface layer. Their total exposure to the atmosphere would actually be much larger than indicated by the age with respect to depth. Indeed this age is reset to zero any time the parcel reenters the surface layer. A diagnostic like exposure time (Monsen et al, 2002; de Brouwere et al., 2011) would be more appropriate.

Answer 5.4: Thanks for pointing us to these very interesting publications out to us. Having read these and related publications (e.g. Gourgue et al., 2007) we considered to replace the presentations of 'surface water age' (as shown in the original manuscript) by diagnostics of residence or exposure times. Considering the fundamentally different approaches discussed in the literature to do this, there are the Lagrangian approach, which requires a large numbers of particles or drifters to be followed individually, and the Eulerian approach, which uses idealised tracers. The former is not possible for our global model (TMM) given the related computational and coding effort. The Eulerian approach is feasible in our modelling environment. We see, however, some conceptual problems since the Eulerian approach only provides large-scale average estimates. Using for example a setting where we have a tracer which has initial values of 1 everywhere in the ocean's surface and values of 0 in the interior, and which during model integration is transported like a conservative tracer with no sinks or sources, allows to compute only the global mean of the surface ocean residence time. Now, designing a suite of tracers each having initial values of 1 in a well defined part of the surface ocean (e.g. the North Atlantic subtropical gyre or the northern North Atlantic) and values of 0 everywhere else at the ocean's surface and in the interior of the ocean, would allow to compute average residence times for these specific regions. However, these would be residence or exposure times with respect to the exchange with the interior ocean AND the rest of the surface ocean. We think that neither of these two possible estimates would be really helpful to understand why certain regions show larger or smaller surface ocean ^{14}C-ages. Particularly, for the latter approach, we do not see a straightforward solution to exclude the impact of water exchange within the surface on the estimate of the exposure time for a selected part of the surface ocean. In fact, the successful examples of residence time computation we have seen in the literature overcome these problems only by restricting themselves in terms of the dimensions studied and provide either residence and exposure times for depth-averaged 2D-models (de Brauwere et al, 2011) or average values for the whole surface domain of their model (Gourgue et al., 2007). In the first example the authors study depth-averaged residence (and exposure) times in an estuary with respect to horizontal transports while in the second examples the mean residence time of the epilimnion of a lake with respect to water renewal from below is studied. Finally, a recent approach to derive exposure times at any time and location by an adjoint method (e.g. Delhez et al., 2004), i.e. be integrating the model backward in time, is beyond our technical possibilities.

In the manuscript we will highlight the limitations of the chosen approach (age of water since last time below the surface layer).

3) Lines 12-20, page 7043: I expected the apparently opposite behavior of 14C-age and ventilation age among models to be further investigated or discussed later on in the paper.

Answer 5.5: We agree. **We will discuss this in more detail in the final version of the manuscript.**

4) Lines 9-10, page 7045: I have concerns about the duration length of experiments; 2500 yr is rather short with respect to the 14C lifetime. I doubt equilibrium with the new boundary conditions was achieved. OCMIP2 suggested objective criteria for achieving equilibrium: 98% of the ocean volume should have a drift of less than 0.001 permil/year (http://ocmip5.ipsl.jussieu.fr/OCMIP/, OCMIP2 HowTo Docs). Did the various experiments meet that criteria? The models used in the present work are efficient enough so that several thousand year simulations may be achieved within a reasonable time.

Answer 5.6: 2500yr was not the standard experiment length, but the original length of that particular experiment. Note, however, that for this experiment the ^{14}C-DIC and the DIC tracer had been pre-equilibrated in the respective reference run (see original manuscript, page 7041, line 20-21; p 7045, line 7ff). In detail, the run the referee is referring to had been initialized with results of the reference run after 4000 yr and thereafter been integrated for another 6000 yrs. Total integration time of ^{14}C-DIC^{bulk} was hence 10000 yrs. There was, however, no significant difference between the results at +2500 yrs and at +6000yrs. The respective results shown in Fig. 4 and Fig. 6 (of the original ms) in fact are from the +6000yr time slice.

Similarly, the length of other runs where also sufficiently long. For example, the respective ECCO reference run had been integrated for a total of 10000 model yrs.

We will replace 2500 by 6000 (in line 10 of p 7045, orig. ms) and be more specific about

the runtimes also of the different sensitivity runs.

5) Section 3.2, pages 7045-7046: The discussion of the impact of mixing on apparent ages should be shortened since this is not really new material. The bias toward smaller ages when in presence of mixing has been established in many previous works: e.g., Jenkins (1987), Delhez et al. (2003) and references therein. The results should be discussed in light of these previous works. Figure 5 is clearly not needed.

Answer 5.7: **We agree and will leave out Fig. 5 as suggested, and refer to published work instead.** Yet, we consider the presentation and discussion of the results shown in Fig. 6 as an important quantitative contribution from this work.

6) Lines 21-23, page 7046: "*The largest difference is found between ageideal and 14C - agedecay. ... Over much of the Pacific Ocean it is equivalent to about 15% of ideal age.*" This difference is much larger than reported in previous studies (5% in Deleersnijder et al., 2001; less than 50 yr in Khatiwala et al. 2012).

Answer 5.8: Thanks for pointing out this difference. **We will compare and discuss our results with those of the publications mentioned.**

Is this a consequence of not-well equilibrated deep water masses? Is this difference model-dependent?

Answer 5.9: See our response 5.6: Since model run times were sufficiently long, there is no issue of not-well equilibrated deep-water masses. **We did the same type of experiment also with the MIT28-TMM. We will briefly report these results in the revised version to give some insight into potential model-dependent differences.**

7) Lines 4-29, page 7049 and lines 1-4, page 7050: there are several issues to be cleared in this discussion of the impact of the gas exchange coefficient.

First, the data-based bomb radiocarbon inventory has been shown to be underestimated (Naegler, 2009; Mouchet, 2013). The reasons are inherent to the available measurements. The drastic decrease in the exchange coefficient suggested here seems unjustified in light of these works. To assess the value to be used proper simulations with CFC and bomb-radiocarbon should be performed.

Second, the conditions at the sea surface differ among the 3 models used in this study: UVIC is constrained with different wind fields than MIT2.8 and ECCO; each model has its own sea-ice climatology. I would also compare models with respect to gross air-sea fluxes.
Third, a good correspondence between model and data 14C may be obtained either by adjusting the level of vertical diffusivity or the gas exchange or both. This is an aspect which needs to be more carefully addressed than it is done in the present text. There are many previous works on this topic (e.g., England and Rahmstorf, 1999; Cao and Jain, 2005; Müller et al., 2006; Müller et al., 2008).

Eventually, the sentence "*In fact this is occasionally seen in the literature (e.g. Cao and Jain, 2005; their Fig. 8d).*" is inappropriate.

Answer 5.10:
The experiments discussed in this section are sensitivity experiments quantitatively exploring

the sensitivity of ^{14}C-age^{pre} to the choice of the gas transfer constant while leaving everything else unchanged. In fact, we were surprised to see that several models simply adopted the coefficient from OCMIP2, although they use different wind fields, ice masks, or ice models. We fully agree with the referee that any model study, which uses the natural abundance of ^{14}C-DIC to validate a model's circulation has first to tune the gas exchange constant via a proper bomb ^{14}C experiment. Related difficulties have been studied elsewhere; see the references mentioned by the referee as well as Graven et al., 2012 (JGR). The most significant problem we see is that early after the bomb ^{14}C invasion pulse there are too few observations to well constrain the ocean's ^{14}C-bomb inventory and thereby the gas transfer constant while later, during the WOCE-CLIVAR period, there are sufficient data in the ocean, but differences in ocean circulation between models and the real ocean start to matter limiting the constraint on the gas transfer constant. Since the atmospheric CFC transient lacks the pulse-like nature of the bomb ^{14}C invasion into the ocean, we do not see how CFC simulations constrain the uncertainty of this constant.

However, it is not our intention to tune any of the models used here! Hence we need not deal with this problem. We don not want to select the model, which fits any available observations best. Our paper is a 'method evaluation study' (see the editor's letter at the end of this rebuttal and our responses 6), and sensitivity experiments are a characteristic element of such studies. By changing just this constant independently in the different models we want to demonstrate how sensitive the ^{14}C-age^{pre} is to the uncertainties of the bomb inventory and the related choice of the gas transfer constant. Choosing 0.24 may be considered an extreme choice, but reflects the largest reduction suggested elsewhere.

In the revised version we will carefully revise that paragraph. We suggest to rephrase that section as follows:
"… In particular **the standard configurations of all models** apply … the OCMIP2 gas transfer constant of 0.337. This value is based on tuning one model of the OCMIP2 family **together with its given wind and sea ice fields** against the bomb ^{14}C ocean inventory …. Evidence has since accumulated suggesting that the bomb 14C ocean inventory may in fact be smaller **by up to 25%** (Sweeney et al., 2007). As a consequence, the gas transfer constant **may need an equivalent reduction**. …. Assuming, **for example**, a value of 0.24 …".
The paragraph (on page 7050, line 3) ends with a brief mentioning of the need of bomb ^{14}C calibration experiments. **In the final version we will extend this by a few sentences discussing the uncertainties in the bomb ^{14}C ocean inventories in more detail as well as methodological aspects of the ^{14}C implementation in that respect (Mouchet, 2013). We will also be more specific about our interpretation of Cao and Jain (their Fig. 8d).**

8) Lines 4-10, page 7050: the result that ice cover does not impact 14C preformed ages seems in contradiction with previous works (e.g., Campin et al, 1999). Does present-day ice cover sufficiently affect areas of the Southern Ocean where large preformed 14C ages are observed? Wouldn't it be more sensible to test this point by extending the northern extent of sea-ice in the Southern Ocean?

Answer 5.11: The initial hypothesis of Campin et al. is in fact that differences in ice cover during LGM compared to the present-day situation, yield a reduced ^{14}C-gas exchange and thereby increases what we refer to as the preformed ^{14}C-age of Antarctic Bottom Water. However, Campin et al. do not provide explicit evidence concerning the specific role of ice cover on preformed ^{14}C-age. Rather, they demonstrate (by means of their ideal age tracer) an intensification of the circulation in the Southern Ocean. They conclude that the LGM-present day difference of (^{14}C-age^{Campin} - age^{ideal}) may be due to (a) an intensified upwelling of old,

^{14}C-depleted, Circumpolar Deep Water and/or (b) an ice induced reduction in 14C-gas exchange (see also Schmittner, 2003). What is missing in the study of Campin et al. is a quantitative separation of both potential impacts by dedicated experiments in which the effect of ice cover on ^{14}C-gas exchange is switched off while keeping the circulation unchanged. To our knowledge our respective experiments carried out for the present-day situation (see Fig. 9 of original ms) provide the first direct evidence that the ice-cover effect on ^{14}C gas exchange is not dominant but rather small. Unfortunately, we currently do not have a LGM circulation for the TMM and hence cannot infer how large the ice cover effect is on the change of ^{14}C-age^{pre} between LGM and present day. Just increasing the area over which ice cover affects ^{14}C-gas exchange for a fixed present day circulation is not suitable to understand LGM-to-present difference.

In the revised version of the ms, we will provide a more detailed discussion of the work of Campin and Schmittner by extending the paragraph lines 4-10 on page 7050 of the original ms. Please note also changes in the conclusion (p 7053 of original ms) as drafted in our response R 3.9.

9) Section 4: the declared aim of this section is "... *to demonstrate the adverse effects of neglecting the preformed component of 14C-age* ..." (lines 18-19, page 7050). The material is available for such a purpose but the results are not fully exploited and the discussion is a bit confused.

In my opinion Figs 13 and 14 are not needed. Global vertical profiles such as in Fig. 8 for the various Kv (bulk and preformed 14C-ages) would be more illustrative. The impact of both Kv and gas exchange coefficient on these profiles should also be put into perspective.

Answer 5.12 Fig. 13 and Fig. 14 were introduced to the original ms in order to better understand why the gradient in ^{14}C-age^{pre} in the Atlantic (Fig. 12c) is so dependent on the choice of Kv. Fig. 13 presents ^{14}C-age^{bulk} at the surface, which is equivalent to the endmembers of ^{14C}-age^{pre} in the interior. Comparing these endmembers for low and high Kv (Fig. 13a, c) reveals how the interplay of Kv and ^{14}C-gas exchange affect these endmembers in particular in the Southern Ocean (up to a factor of two in important regions of deep water formation there). Presenting global mean profiles like in Fig. 8 would not suit this purpose. **We will rephrase the caption of Fig 13 to make this clear. We agree with the reviewer that the results are not fully exploited and will carefully revise that section.**

10) Lines 1-2, page 7039: "*For UVIC the 14C-simulations are made alongside a normal, biotic model run.*" Does biology in UVIC affect 14C? In which ways?

Answer 5.13: **As expressed elsewhere (see response R 3.3) we will provide a brief introduction to the principles of ^{14}C-experiments carried out following the OCMIP-2 protocol. Here we will also explain the specific differences of ^{14}C-simulations in UVic.**

Minor comments

Lines 20-21, page 7035: "*Surface water in equilibrium with the preindustrial atmosphere (1890 AD) would have a Δ14C = 0‰ and a 14C-age of 0 yr.*" This is incorrect; even if considering a constant atmospheric 14C-production and steady-state ocean and climate the Δ14C of ocean surface water and their age would not be 0. This is in contradiction with what is stated in the paper on page 7036, lines 7-19.

Answer 5.14: On page 7036 we discuss why the surface ocean does NOT reach equilibrium with the atmosphere, i.e. due to continuous or at least periodic input of ^{14}C-depleted waters from below the surface. Combined with the slow exchange with the atmosphere the surface ocean steady state (or quasi steady state, given seasonal to multidecadal fluctuations) of Δ^{14}C is a non-equilibrium. The sentence the referee refers to simplifies in an important aspect (fractionation) without mentioning it – and the OCMIP-2 protocol does not cover it either. However, in the introduction we wanted to point to the large difference between an ocean surface in equilibrium which would have a apparent ^{14}C-age close to 0 yrs, if considering fractionation, compared to the real ocean situation in which regions important for deep ocean ventilation have ages of about 1000yrs. **In the final version of the manuscript we will be more specific on these aspects.**

Line 12, page 7035: suppress "*to reach rather constant atmospheric levels*"; atmospheric 14C is not constant neither on the anthropogenic (Suess, bomb...) nor on millenial time scales. This affirmation is confusing and in contradiction with what is stated on page 7036, lines 24-26.

Answer 5.15: **We will delete this phrase in line 12** (page 7035 of the original ms) as it obviously and unnecessarily may confuse the reader. See also our response 1.1.

Line 16, page 7035: suppress "*(for equations see Sect. 2)*"; not needed here Line 19, page 7035: why the reference to 1890 AD here?

Answer 5.16. The reference to 1890 AD was added here just to be explicit. One of us felt that the term preindustrial may potentially be understood as a longer time period (the late Holocene), over which the atmospheric ^{14}C boundary condition was variable. This variability, however, is usually ignored when performing ^{14}C runs for evaluation of ocean circulation (OCMIP-2 protocol; Matsumoto et al., 2004)

Lines 5.16, page 7036: "*Thirdly, it is usually assumed that the transport of 14C/C from the surface to the deep sea via sinking organic particles can be neglected (Fiadeiro, 1982).*" The authors state that the neglect of the 14C transport to depth via POC is a problem. They nevertheless make the same assumption in their model (page 7038, line 28) without discussing this point.

Answer 5.17: In the introduction, we identify several issues that complicate the use of natural 14C for data-based evaluation of ocean-model circulation. For three of them we only mention them briefly: variability of atmospheric boundary conditions on long time scales, quality of bomb correction, and biotic transport of 14C. Some of them have been studied elsewhere, please see the references given in the text. Since we do not provide new evidence concerning these issues in our paper, we see no need for a more detailed discussion here. Concerning the biotic transport we will update the references and point also to the very recent work of Jahn et al., 2014 (GMDD).

Line 23, page 7036: is the use of the word "corrected" appropriate in this context?

Answer 5.18. **We will change the text to 'requires reliable age determinations…'.** See also our response 5.20.

Lines 20-21, page 7036: "*In the context of ocean biogeochemistry the time elapsed since the* *last contact of a water parcel with the atmosphere, i.e. water of age zero, is of particular interest.*" Reference to previous works on the ventilation age tracer (e.g., Thiele and Sarmiento, 1990; England, 1995) is missing.

Answer 5.19: We agree, that both studies (see their introductions) mention the need of unbiased (ideal) tracers of ventilation age for example when quantifying oxygen utililization rates. For the explicit formulation we chose here, we were hesitating to cite any of them as their studies focus on closely related but not literally identical aspects. We cited Thiele and Sarmiento as well as England on page 7040 of the original ms, where the ideal age tracer is introduced explicitly in our paper.

Lines 21-25, page 7036: "*For example, the estimation of rates of ocean respiration or CaCO3- dissolution from cumulative tracer changes requires corrected reliable age determinations (Jenkins, 1982; Sarmiento et al., 1990; Feely et al., 2002). 14C-ages of several hundred years for waters actually in contact with the atmosphere can thus pose a severe problem.*"

The authors of the quoted studies did not rely on 14C for their estimate of oxygen utilization or CaCO3 dissolution rates. Hence this paragraph should be reformulated.
Lines 20-27, page 7036: this paragraph should be reworked. I do agree with the idea behind it, but the topic is presented in a rather confused way.

Answer 5.20: In the sentence for which we give the references we only say that rate estimates require correct, reliable age estimates. The sentence the referee argues about is our sentence and we do not claim that Jenkins, Sarmiento or Feely used 14C for age determination. (Feely did, by the way.) Reliable estimates are also needed when using other tracers of age. The paper of Sarmiento makes the particular case that uncertainty in the age determination contributes much to the uncertainty of respiration rate estimates (see also the text book of Sarmiento and Gruber, 2006, their Fig. 5.2.4). One might, however argue that the word 'corrected' in line 23 of our original text may imply to some readers that the cited references used 14C based ages.

In the revised version we will move the citations such that the text reads '…from cumulative tracer changes ([Refs]) requires reliable age determinations….'. I.e. we will also delete the word 'corrected'. Note that we further will replace the Jenkins, 1982 reference by a reference to Broecker et al. 1991 (GBC 5, 87ff) on the estimation of respiration in the deep ocean.

Line 26, page 7038: "DIC and 14C-DIC are prognostic model tracers of total dissolved CO2 and 14CO2 respectively." In order not to confuse between dissolved CO2 (gazeous) and total carbon I would recommend to replace CO2 by carbon or C in the above sentence.

Answer 5.21: The term we use is 'total dissolved CO2' and not just 'dissolved CO2'. Our term is standard, see the abbreviation TCO_2. Using 'carbon' or 'C' instead may be misleading, since 'total dissolved carbon' would include DOC (dissolved organic carbon). **However, to avoid any possible confusion with e.g $CO_{2(aqua)}$ we will change to 'total dissolved inorganic carbon and its ^{14}C-isotope'.**

Line 12, page 7039: *dissolved* is misspelled

Answer 5.22: Line 12 (dissolved), **is corrected.**

Formula (3) page 7039: the mean life of 8033 yr does not agree with the half-life of 5730 yr given on line 14, page 7035. What is the decay rate of 14C in the model experiments?

Answer 5.23: It is correct that we assumed the formerly used mean half-life (Libby, 1955), we uncritically adopted equation 3 from Stuiver and Polach (1977) and Matsumoto (2007). The mean life consistent with our decay rate (1.2097e-04 yr-1; OCMIP2 protocol, Orr et al., 1999) is 8267 yrs. **We re-did all computations and plots accordingly. This increases bulk age and computed preformed ages (Equation 5) somewhat, but does not change any of our conclusions. We will correct text and equations accordingly.**

Lines 20-21, page 7040: "*... and adds up any 14C-decay of the 14C-DIC tracer in the interior*" This sentence is hardly intelligible. Do you mean 14C-DICdecay *undergoes radioactive decay as does* 14C-DICbulk?

Answer 5.24: We compute the local $^{14}C\text{-}DIC^{decay}$ rate at any time step and for any model grid cell based on the actual $^{14}C\text{-}DIC^{bulk}$ tracer concentration and the decay rate constant (see above). Technically, this local decay rate is added to the current value of the $^{14}C\text{-}DIC^{decay}$ tracer at each time step in the model. The $^{14}C\text{-}DIC^{decay}$ tracer, like any other tracer, is subject to mixing in the physical loop. This treatment is analog to that of a PO_4^{remin} tracer or a TOU-tracer (e.g. Ito et al., 2004; Duteil et al., 2013).

In the revised version we will provide a more detailed technical description.

Section 2.3: Only the reference runs and short description of the other experiments should be given here. Too many details are given here for the gas exchange, vertical diffusivity, and "residence" time; this should be moved to the relevant sections.

Answer 5.25: See response 5.2.

Lines 4-7, page 7045: there is some redundancy in these lines.

Answer 5.26: **We will simplify the text and use the dedicated tracer names (see Tab. 1) only.**

Line 12, page 7048: "*... to moderately negative surface Δ14C and 14C-ages...*" do you really mean negative ages?

Answer 5.27: Text has been corrected, see response 3.7.

Table 1, page 7060: Under its present form this table is of no real utility. I would suggest to transform it as illustrated below, it would greatly help the reader to follow the text.

<Table suggested by reviewer 5, screenshot from pdf of review>

below, it would greatly help the reader to follow the text.

Tracer name	Age name	Source/sink	Sea-surface B.C.	Component	Comments
^{14}C-DIC^{decay}	^{14}C-age^{decay}	radioactive decay	0	Circulation	^{14}C-ages: subject to nonlinear mixing effect.
^{14}C-DIC^{pre}	^{14}C-age^{pre}	-	^{14}C-DIC^{bulk}	Preformed	
^{14}C-DIC^{bulk}	^{14}C-age^{bulk}	radioactive decay	Eq(1b)	Total	
age^{ideal}		aging	0	Circulation	Ages: not subject to nonlinear mixing effect.
age^{pre}		-	^{14}C-age^{bulk}	Preformed	
age^{bulk}		aging	^{14}C-age^{bulk}	Total	

Answer 5.28: See response 5.1.

The affirmation "14C-*based tracers: are subject to non-linear tracer mixing effect*" is not correct. The 14C-ages are subject to nonlinear mixing effects.

Answer 5.29: **Yes, we will corrected this in the revised version of the ms.**

Figure 5: not needed

Answer 5.30: We agree and **will leave out Fig. 5 in the revised version. Instead we will refer to and discuss work by others related to this issue.**

Caption of Figure 12: *patterns* is misspelled

Answer 5.31: **Is corrected.**

Figures 13 & 14: suppress and replace with one similar to Fig. 8 but for the different Kv.

Answer 5.32: See our response 5.12.

Line 5, page 7056: *Fiadeiro* is misspelled

Answer 5.33: **Is corrected.**

References

This list is restricted to papers not referred to in the authors manuscript.

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Answer 5.: We thank referee #5 for a very thorough reading and for his/her extensive and very helpful review!

D.M. Roche (Editor)
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Received and published: 20 December 2014
Dear all,
I would like to first warmly thank all the reviewers for the time taken in this first assessment of "14C-age tracers in global ocean circulation models"
Given the unusual situation of strongly diverging comments on the submitted manuscript, I would like to provide a short comment on the relevance of the manuscript to the scope of Geoscientific Model Development.

In my reading of the comments of the reviewers, the apparent divergence on the novelty of the study and the usefulness of the proposed preformed 14C-age depends on the perspective in which it is to be used. Reviewers acknowledge its interest in model intercomparison studies but not in the calibration to present-day oceanographic measurements (since this is not actually measured).

While it is the role of the authors to resolve / respond to the numerous suggestions received in a revised version and point-to-point answer to the reviewers, I would like to state in this short comment that I do think that the manuscript falls into the scope of GMD in the "Model Assessment Methods Paper" article type as chosen by the authors. Indeed, GMD website (http://www.geoscientific-model- development.net/submission/manuscript_types.html) clearly state that "work on developing new benchmarks for assessing model performance" is the subject of this manuscript type together with "discussions of novel methods for data analysis or visualisation with relevance to geoscientific modelling, or the application of existing techniques to this field". Whatever the outcome of the next steps of the review process, the manuscript submitted does fall into the scope so stated.

I therefore look forward to reading a complete response to the reviews received and to a revised manuscript version.

D.M. Roche

Answer 6: We thank the editor for these clarifying words.

We have submitted this manuscript under the GMD category of 'Model Assessment Methods paper'. GMD describes this category as follows: 'Model Assessment Methods include work on developing new benchmarks for assessing model performance, or novel ways of comparing model results with observational data. Also included are discussions of novel methods for data analysis or visualisation with relevance to geoscientific modelling, or the application of existing techniques to this field. These papers may be theoretical, in which case an example implementation should be provided as a supplement. …').

It is unfortunate that obviously only the submitting authors, the editor and the editorial staff have knowledge about the submission category chosen by the authors. We suggest that GMD finds ways to communicate this at least to the referees.

In case of referee 1 in particular it is obviously from his/her review that s/he expected a contribution to one of the other GMD categories (e.g. Model description paper; Technical, development and evaluation paper, …) and his/her rating is based on this perception.