We thank both reviewers for their support of our manuscript and for their considered comments. We have amended our manuscript to take account of the proposed suggestions/modifications and here we take the opportunity to respond explicitly to each point. Our responses are in bold. We thank the reviewers again for their time and effort.

Best regards,
Alessandro Tagliabue (on behalf of all authors)

Anonymous Referee #1
Received and published: 14 July 2009
The authors describe a set of modeling experiments designed to investigate the impact of changes in ocean circulation and biogeochemistry on oceanic carbon isotopes and atmospheric CO2 between Pre-Industrial and Last Glacial Maximum periods. The model description relies on previous publications, and the implementation of the carbon-13 in the complex biogeochemical model refers to Tagliabue and Bopp (2008). The experimental strategy has a sufficient number of simulations to separate the various mechanisms. The section 3.1 "LGM ocean carbon-13" identifies the mechanisms responsible for the carbon isotopes distribution and points to a reduction of both North Atlantic and Southern Ocean ventilations. The section 3.2 "LGM atmospheric CO2" describes more carefully the changes in the carbon cycle. The discussion considers all necessary aspects. Nevertheless, regional changes in biogeochemical properties could be more detailed. Changes in export production among the various simulations are describedin section 3.2 and Table 3. Earlier comments on this aspect are needed in section 3.1 already (see specific comments below). Tables and Figures are sufficient to support the discussion. Nevertheless, Figures could be enlarged to facilitate the model-data comparison.

Scientific Significance: Good(2): the modeling of carbon isotopes in a state-of-the-art ocean general circulation and biogeochemical model for paleo studies is substantially new and contributes to important discussions on the topics addressed in this work.
Scientific Quality: Good (2). The scientific approach and applied methods are valid.
Presentation Quality: Good (2). A few sentences need re-phrasing.
Therefore, I recommend for publication after minor corrections.

Specific Comments

3 Results and discussion
3.1 LGM ocean carbon-13
p1470, line 1. "While increasing dust iron supply..."
C: Figure 1a shows a reduction of d13C-DIC in the Southern Ocean and a slight increase in the rest of the ocean, not a reduced d13C in deep-water. Are the primary production and the export production significantly modified by the additional iron supply in this dust only simulation ? Please already refer to your Table 3, and that you will comment the changes in export production in section 3.2.

Acknowledged and addressed

p1470, line 7. "Increased LGM overturning..."
C: Please rephrase.

Acknowledged and addressed

p1470, line 11. "... an excellent regional agreement with NA delta-d13C-DIC..."
C: Table 2 is for the entire ocean. Is the correlation coefficient better than 0.6 for the NA ?

Yes. The R for the northern Atlantic is 0.73 (relative to 0.6 globally) and the slope also increases to 0.42. All of this reflects a better agreement with the data collected in the NA.

p1471, line 7. “...since dust is insufficient...”
C: Once again, what is the response of the ecosystem to the dust supply in terms of export production ? Your simulation “Circa+PI_dust” mentioned in table 3 should help to separate this effect. Your interpretation in section 3.2 is satisfying, but comments on this aspect are missing at this stage of the paper to discard the biological effect.

Acknowledged and addressed. We will add some discussion of the biotic response here (and refer to a more extensive discussion in section 3.2).

p1472, lines 11-14. “In fact, colder temperatures,... elevate deep oxygen in some regions”.
C: Is this new deep oxygen distribution only due to the circulation ? What about biology reorganization and remineralization ? Seesaw effects are visible on the export production: the PI+LGM_dust simulation has +15% of Cex in the SO, and -2.6% in the global ocean. Remineralization and oxygen consumption are linked to the biology reorganization and not only circulation.

The reviewer is correct to note that biology is, of course, impacting the changes in oxygen. To permit this to be better clarified, we have added a new panel to the oxygen figure, that shows the impact of only LGM dust on bottom water oxygen. This shows how a reduction in deep oxygen in the Southern Ocean (due to greater export production) is present and permits us to show that this is then exacerbated by a more stratified deep ocean – since mixing of high oxygen water is reduced. This is except for regions typified by greater ventilation, wherein oxygen increases. The reorganization of ventilation sites (despite an overall reduction in SO ventilation) is driven by the climate model (see other responses).

3.2 LGM atmospheric CO2
p1472, line 23 “...reduce pCO2atm by _15ppm...”
C: To which simulation do you refer ?

We quote a common result of changing temp and salinity – for example Brovkin et al. (2007). The MS has been amended to make this more explicit.

p1473, line 6 to 9. “That we measure...”
C: Having changes in preformed nutrients and export production does not mean that the system is at the equilibrium. This will be visible on time series.

Acknowledged and addressed

p1473, lines 25-26. “This results from...”
C: Please re-phrase.

Acknowledged and addressed

p1475, line 10. What is “the Antarctic sector of the Southern Ocean” ?

It is the region south of the polar front. We have added a parenthetical statement to clarify this.

p1476, lines 1-2. “due to reduced vertical nutrient supply”
C: We are talking here about the SO. So which vertical nutrient supply are we talking about ? Is it a deep nutrient feedback ?

The upwelling of nutrients in to the SO surface layer

p1476, lines 8 to 11. Interestingly, increasing the C/N ratio by 12% increases the SO Cex, but also decreases the global Cex (Table 3). In this experiment CircA+LGM_dust+CN, the further 9 ppm draw down is occurring while the global biological Cexport has decreased by 2%. By favoring the biological pump with the C/N ratio, you end up with a less efficient biological pump globally, and a more efficient physical pump. This is counter intuitive to me. Is this a robust result of you model ? Did you modify the C/N planktonic ratio only in the southern ocean ?

Changes (increase or reduction) in export production are not a good metric of the efficiency of the biological pump as already demonstrated several times (more recently by Marinov et al. 2008). Indeed, budget of preformed nutrients have been suggested to represent a more accurate measure of the efficiency of the biological pump (Ito and Follows, 2006). This metric is underpinned by assuming constant organic matter stoichiometry.

In the C/N experiment however, our stoichiometry (of course) changes, which increases the efficiency of the biological pump, which is not reflected in the changes in preformed nutrients (as stated in the original manuscript).

Minor corrections:

p1471, line 21. "... is not the forcing which we have used..." or "..which has been used..."

Acknowledged and addressed

p1473, line 7. I guess you are referring to Table 3, not 2.

Acknowledged and addressed

Anonymous Referee #2

Received and published: 11 August 2009

Alessandro Tagliabue and co-workers preset a well-rounded model-data synthesis of glacial-interglacial changes in ocean d13C, as a means of helping constrain the mechanisms involved in the observed variability in atmospheric CO2. I have a few moderate sized points, and some minor ones, in keeping with the overall quality of the study.

I am somewhat nervous that CO2 changes are discussed and plotted in 'carbonate compensated space' in Figure 7. The reference for carbonate compensation contributing 30-40 ppm is Brovkin et al. [2007], yet this presumably assumes the same glacial ocean state that the authors dismiss in their discussions(?) You cannot have you cake and eat it! ;) Since the glacial CO2 problem is meant to still remain unsolved, and indeed, apparently even less well understood than we thought according to the abstract, it seems rather unsafe to assume that the contribute of carbonate compensation (30-40 ppm) is accurately known. Please re-plot Figure 7 in normal delta-pCO2 space. I believe that the paper will be all the more clearer and less subject to confusion and hence have greater impact this way.

Acknowledged and addressed

By all means then speculate how much of the CO2 change not explained to the model might be attributed to carbonate compensation (with references). Similarly, in the abstract, I do not think that you can state: 'we can attribute over 90% of the change in atmospheric CO2' on the basis of the work presented here, i.e., lacking an explicit estimate of carbonate compensation.

Another area that might be much better clarified concerns the initial state of the model. Most of the plots (Figures 1, 2, 5, and 6) are given as anomalies (deviations from a baseline state). It would be really helpful to better interpret the impacts presented if the initial states were shown alongside (I recognise that these states may well be fully described elsewhere, but it is a real hassle to have to go dig them up simply to fully interpret the results presented here).

We understand the reviewers point. To address this point, we will add the plots for the pre industrial ^{13}C and GLODAP ^{14}C (see below point). But we will make these supplementary figures. This way, those who do not have the original manuscript can access the plots, without adding an unwieldy number of figures.

Associated with this I have some queries
regarding some of the more prominent features in the model behaviour. For instance, the 'plume' of highly oxygenated water emanating from the Ross Sea and filling the SE Pacific basin at depth (Figure 5) is highly unexpected. It is also picked out in d13C (Figure 2). The feature spills across the Drake Passage and into the SW Atlantic Ocean, yet I thought that the glacial radiocarbon ages in the Drake Passage area were meant to be pretty old (do not have ref immediately to have . . .). Do the authors believe that this feature could be a genuine facet of the glacial ocean or might it be a model artefact? What role does an apparently elevated rate of ventilation of (at least part of) the deep glacial Southern Ocean play in the atmospheric CO2 predictions – could the CO2 drawdown be underestimated here because of this? Also, how can CircA represent 'decreased ventilation' w.r.t. d13C (Figure 2 caption) while at the same time causing substantially elevated dissolved oxygen concentrations (Figure 5)? Or is the 'decreased ventilation' an overall (mean) assessment, which is actually highly heterogeneous in practice, with regions of both decreased and increased ventilation? Further explanation/discussion is required on this point.

The final point of the reviewer is spot on. Yes, there is an overall reduction in 'mean' Southern Ocean ventilation that is highly heterogeneous in space. Some regions show greater ventilation, despite an overall reduction. Unfortunately, characterizing overall mean circulation in the Southern Ocean in one or two statistics is not as straightforward as for a smaller closed basin like the North Atlantic. However, we understand that this can lead to potential confusion, so we have added some text to point out that while overall ventilation declines, there are some places that show increased ventilation at the LGM in our OAGCM.

Model integration time – quoted as 3000 years for the pre-industrial baseline simulation, and 500 years for the glacial experiments – is this sufficient? 3000 years seems surprisingly short to establish the D14C distribution in the global ocean (especially compared to the lifetime of 14C!), and 500 years relatively short for establishing a new (perturbed) steady-state in deep ocean d13C. Do the authors know how far off of steady state they might be? Could some of the 'unusual' features, such as the high O2 anomaly in the SE Pacific (Figure 5), potentially be a transitory flushing event induced by the change in glacial boundary conditions? 500 years is well within the range of lifetimes of transitory states induced in models assessing Heinrich and D-O events (via freshwater perturbations) for instance. Would benthic properties have differed at all significantly if the model had been run for a further 500 (or even 5000) years? I appreciate the likely numerical expense ('heaviness') of ORCA2-PISCES, but running even a single glacial state rather further would give a good indication of how quickly the system re-equilibrates and hence how reliable all the experiments are.

A few responses are necessary here:

Firstly, it should be noted that the suite of ocean circulations that we test with ORCA2-PISCES are constant. They arise from different runs of the IPSL OAGCM under a variety of different forcing regimes. The suite of different ocean circulations are then used to run the biogeochemical model 'offline" for 500 years (after PISCES has been spun up for 3000 years under pre industrial circulation from the same OAGCM). As such, as far as we are concerned for this study, circulation features are constant and not transient for the duration of our biogeochemical experiments.

Secondly, in a similar fashion, the carbon-14 simulations were run 'offline' with the same glacial circulations for 3000 years. This was mentioned in the methods, but evidently it could be clearer. We will reiterate this when we introduce the carbon-14 results.

Thirdly, the reviewer is correct that the timescale of the runs might be increased. However, as the reviewer notes, this is extremely computationally expensive for ORCA2-PISCES. Already 500-year simulations (repeated many times for different scenarios) are very numerically expensive. However, we agree with the reviewers point and did extend one simulation (and the control) for 1000-years (CircA) and found that the change in atmospheric CO2 after 500 years was 85% of the change after 1000-years. We have added this to the text. Unfortunately this is the trade off between complex (both spatially and

mechanistically) OGCBMS and simpler box or intermediate complexity models. Our OGCBM has the advantage that we can statistically evaluate the distribution of carbon-13 against observations (which simpler models cannot due to poor resolution), but OGCBMs are not numerically 'light' enough to be run fully to equilibrium (i.e., for 10,000+ years). We would note that we did attempt to address part of the issue by running parallel simulations with the EMIC climber-2. These tests showed us that our conclusions regarding the glacial circulation necessary to reproduce observed carbon-13 changes were relatively robust. Unfortunately, CLIMBER-2 is not complex enough to compare the biogeochemical impacts (examine the difference between prognostic LGM dust and the arbitrary changes in macronutrient utilization from previous CLIMBER-2 simulations).

On the subject of figures – some of them tend to the small side and it is difficult to make out some of the features, especially Figures 1 and 2 (whose dark background colours make the observations difficult to make out – perhaps plot the observations with a white border rather than black?).
other comments

We will try to improve the size of the figures. However, I am not sure if this is a typesetting issue as most of them take a whole page of A4 in my graphics program? We could also remove panels from some of the figures and increase the number of figures to permit them to be larger. But I am concerned that this will result in an unwieldy number of figures.

o Abstract: The statement: 'we can attribute over 90% of the change in atmospheric CO_2 to such factors' does not seem consistent with the following: 'over half of the necessary CO_2 change remains to be explained'. Either you 'know' what the 'answer' is or you do not . . . ?

Acknowledged and addressed

o Page 1464 / line 23: Please check the year of 'Sigman and Boyle' (and in the reference list and cited elsewhere) – I thought it was 2000, not 2002.
C565

Apologies, the reviewer is correct, its 2000.

o Page 1464 lines 22-24: There are better references for glacial-interglacial changes in terrestrial biosphere carbon storage, and it would be helpful to add the estimated amounts of carbon.

Acknowledged and addressed

o Page 1465 / lines 2-4: Clarify whether the increase in d13C was global or restricted to a particular basin(s), e.g. Atlantic (it matters to model-data inter-comparison).

The increase we talk about here in the introduction is a measured increase in the upper – deep water gradient. Data in Duplessy and Curry & Oppo is from the Atlantic.

o Page 1465 / lines 15-16: I know what you mean, but perhaps qualify that you are talking about greater carbon production per mol of (macro) nutrients.

Acknowledged and addressed

o Page 1467 / lines 7-9: You mention 'LGM' pCO_2 and salinity and alkalinity, but I thought that the 3000 year spin-up was for the pre-industrial (Page 1490 / lines 14-15) (?) If pre-industrial – please correct. If LGM is correct, then please state what the assumed salinity changes was, and more importantly, what the ALK change was, as surely we do not know LGM ALK, 'else we would have already solved the glacial CO_2 problem . . . ?

For the LGM simulations we use the salinity and temperature that arises from the coupled OAGCM under LGM forcing. To then account for the change in sea level we added 1psu from salinity at the beginning of our run. We also increased nutrient stocks (by 3%) in accord with this 'concentration' effect. However, it is important to note that we did not change alkalinity. In our model, alkalinity is computed explicitly and is not an empirical function of salinity. We have added text to clarify this point.

o Page 1469 / line 11: Please add reference for the prescribed -0.4‰ change in mean ocean d13C. (And adding how much carbon this is would be helpful.)

We lowered $\delta^{13}C$ by 0.4‰ globally, by reducing the ocean $DI^{13}C$ accordingly (only applied for the first timestep). $DI^{12}C$ was unchanged.

o Page 1471 / lines 4-7: You can only be talking about Atlantic + Indian sector ventilation changes in the SO? Please clarify.

Yes. This is correct. We will note that the increased ventilation in the Ross Sea sector explains the modeled $\delta^{13}C$ response in this region.

o Page 1472 / line 11 and Figure 5: Please clarify that the O2 anomaly is relative to the pre-industrial (or equivalent) simulation in Figure 5. Ideally add a second panel showing the pre-industrial simulation. We cannot possible verify or evaluate the statement in the main body of the text that the sluggish glacial circulation did not 'drive total anoxia at depth' (Page 1472, line 11) without seeing the baseline.

Acknowledged and addressed. We note in the original text that oxygen concentrations do indeed decline markedly due to the circulation/biogeochemistry changes, but wished to point out that while this might be true globally, there might be certain regions where (due to a reorganization of Southern ventilation sites) O_2 increases. I will reiterate that overall oxygen decreases at depth (suboxia increases 3-fold).

o Page 1472 / lines 21-26: I do not understand this. If you have less nutrient supply to the surface, then you presumably have lower nutrient concentrations at the surface. If the nutrients are not at the surface, they are at depth, along with CO2 . . . (?), and hence reducing atmospheric CO2? Looking at it another way – if you have glacial O2 depletion at depth compared to the interglacial baseline, normally one would expect more CO2 at depth associated with the O2 consumption . . . ? One might find ways of de-coupling CO2 and nutrients, but it is much harder to (anti) decouple CO2 and O2? You may be right, but this seems the wrong way around. Please check very carefully that this is really what is going on. Or did you include an addition of CO2 due to a reduced terrestrial biosphere carbon reservoir?

The canonical anti-correlation between O_2 and CO_2 noted by the reviewer does not occur everywhere. For example, the cooler LGM climate during our experiments will elevate the solubility of CO_2 and O_2 and would therefore work against any anti-correlation due to production/remineralization processes.

We note that we mentioned this cooling induced increase in O_2 (that can then be subducted) in our original manuscript when discussing the O_2 changes. In addition, there is an issue related to the time step of the model and its associated ability to resolve seasonal variability. Box models that have integration steps greater than one year will not include any seasonality. In real world, seasonal changes in 1) the solubility of CO2 and O_2 and 2) the timescales for gas exchange (O_2 equilibrates much faster than CO_2) can also modify the production/remineralization relationship between O_2 and CO_2.

o Page 1476 / lines 6-11: Can you really invoke an increase in C:N on top of all of this? You have already admitted to having widespread dysoxia, which is not observed . . . ? And further involved biological processes increasing export efficiency (Page 1477 / lines 6-10) would surely exacerbate your deep O2 problem?

It does, but we thought it would be worth including in order to examine its CO_2 impact.

o Figure 2: Missing colour bar/scale.

Acknowledged and addressed

o Figure 3: Why are the CLIMBER ‘change in NA ventilation’ not broken down into depth and strength changes as per ORCA2-PISCES? (It is not, in fact, entirely clear how the AMOC changes in CLIMBER are calculated.)

Calculated in the same fashion as for the OGCBM. The text has been amended to mention for this.

o Figure 4: Is this a zonally-averaged Atlantic profile or a specific line of longitude in the mode (please specify)? It would be helpful to see the GLODAP (estimated preindsutrial) D14C for comparison with panel (a). Also – please label the 3 panels (‘a’, ‘b’, ‘c’) if you are going to refer to them by panel letter identifier in the caption text.

This is a zonally average Atlantic profile. We have also amended the figures as requested.