Response to all three reviewers:

We thank Victor Brovkin and the two anonymous reviewers for their generally positive and thorough reviews. Please see our response to the comments below. We numbered the comments to enable referencing and we show the original comments in italics for completeness.

A) Response to Reviewer 1:

A.1) In the present form the model misses either some central parts necessary to describe marine carbonate chemistry or the model description is incomplete. So far, the model seems to exist only of fluxes of ocean circulation and biological export production as described in the set of equation on page 873. However, to calculate atmospheric CO2 concentrations at least temperature information is necessary, as via Henry's Law the C293 amount of CO2 uptake or release is calculated as a function of temperature. A carbon cycle box model normally calculates the concentrations of the three species (CO2, HCO−3 , CO2− 3) of the dissolved inorganic carbon (DIC), of alkalinity, and pH as functions of temperature T, salinity S, and pressure p. Apparently, this seems not to happen in the model, or was not described. If it is indeed not happening, then it needs to be explained how atmospheric pCO2 can be calculated, and how reliable this information then is. If the carbonate system is calculated, it does not need to be explained in detail, some references to the used published system, constants, or equations are enough, but it need to be mentioned what information on T, S, p are used. Furthermore, please be aware that changes in T alone is responsible for one of the three oceanic carbon pumps alone, which partially explains glacial CO2 uptake: the solubility pump (more CO2 dissolved in colder waters).

Our study pertains only to the biological soft tissue carbon pump and not the solubility or carbonate pump. We stated this in the introduction but we will also state this in the title and the abstract and we shall be clearer about this throughout the paper. Our title shall be

"A multi-variable box model approach to the soft tissue carbon pump."

We shall also summarize the other important mechanisms to glacial CO2 and refer the reader to appropriate review papers such as Kohfeld and Ridgwell (2008). Our paper is based on an elegant theory of Ito and Follows (2006) in which they described how one can relate the preformed nutrient concentration to the soft tissue pump and atmospheric pCO2 in a simple way. In a revised manuscript we will outline the theory to make the paper more self consistent. In short the theory goes as follows:

> Dissolved inorganic carbon in the ocean can be subdivided into preformed and regenerated carbon. The preformed concentration can be written as the sum of a saturated component C_{sat} and a preindustrial air-sea disequilibrium component ΔC and the regenerated carbon concentration can be split into a soft tissue, C_{org}, and calcite component, $C_{calcite}$. Assuming a constant reservoir of carbon in the ocean and atmosphere we can write a small change in the pCO_2 of the atmosphere in terms of the changes in the oceanic carbon components, i.e.,
>
> $$M\delta \mathrm{pCO}_2^{\mathrm{atm}} + V\{\delta\overline{C_{sat}} + \delta\overline{\Delta C} + \delta\overline{C_{org}} + \delta\overline{C_{calcite}}\} = 0 \qquad (1)$$
>
> where M is the total moles of gas in the atmosphere and V is the volume of the ocean. For simplicity it is assumed that changes in the soft tissue pump are independent of changes in

the carbonate pump or saturation so $\delta\overline{\Delta C} = \delta\overline{C_{calcite}} = 0$. Using the Buffer factor (Bolin and Erikson, 1959), B, one can approximate variation in $\delta\overline{C_{sat}}$ by

$$\delta \ln \mathrm{pCO_2^{atm}} = \mathrm{B}\ \delta \ln \overline{C_{sat}} \tag{2}$$

Eliminating $\overline{C_{sat}}$ from (1) and (2) gives an expression for the sensitivity of $\mathrm{pCO_2^{atm}}$ to $\overline{C_{org}}$

$$\frac{\delta \mathrm{pCO_2^{atm}}}{\delta\overline{C_{org}}} = -\frac{V}{M\gamma} \tag{3}$$

$$\gamma \equiv 1 + \frac{V\overline{C_{sat}}}{BM\mathrm{pCO_2^{atm}}} \tag{4}$$

We now define the efficiency of the biological pump,

$$\overline{P^*} \equiv \frac{\overline{P_{reg}}}{P_0} \tag{5}$$

where $\overline{P_{reg}}$ is the mean regenerated phosphate concentration and P_0 is the global mean phosphate concentration. For a 100% efficient pump $\overline{P^*}$ will be 1. A perturbation in the organic carbon concentration can now be written in terms of an equivalent change in the biological pump using a fixed Redfield ratio,

$$\delta\overline{C_{org}} = R_{C:P}\ \delta\overline{P_{reg}} = R_{C:P}P_0\delta\overline{P_*} \tag{6}$$

Combining Eqs. (3) and (6), we now find a linear relation between $\mathrm{pCO_2^{atm}}$ and the efficiency of the biological pump as defined by Eq. (5),

$$\frac{\delta \mathrm{pCO_2^{atm}}}{\delta\overline{P_*}} = \frac{VR_{C:P}P_0}{M\gamma} \sim 312\ ppmv \tag{7}$$

For more details of the derivation please refer to Ito and Follows (2005). Given that this study is not designed for large perturbations one should interpret the results qualitatively when considering large glacial-interglacial changes. We shall state this clearly in a revised version.

A.2) If this major issue above can be solved satisfactory the study seems then to be worth publishing and I have only some small (although a lot) concerns listed below. The most important one is the question of how their model behaves in terms of sensitivity to other box models and the authors might think about some sensitivity experiments as already performed with other models to set their model into a wider context.
And related minor comment: Model performance: What about the general model performance / sensitivity with respect to other box models or GCMs. It would help the reader to set you model into context to others, see for example "model evaluation and sensitivities in Köhler et al. (2005).

It will hopefully be clear after our response to comment A.1 that neither of the two model tests performed in *Köhler et al. (2005)* are applicable to this study because they involve the carbonate and solubility pump. Even the circulation and export production sensitivity tests are not comparable as they include also effects on the carbonate pump. We could compare the change in pCO2 due to a change in vertical mixing in the Southern Ocean and Northern regions in our model (Fig 5, top left, and bottom) to a similar test in *Köhler et al (2005, Fig 3a).* However, this could be more misleading than illuminating and we would only do this is the reviewer still thinks it would informative. It should be noted that we have done thousands of sensitivity tests (300 for each or 10 variable for the glacial, interglacial, and random states) and the results are given in table 2.

A. 3) I am also not sure if the representation of the global ocean as only one row of ocean boxes going from high southern to high northern latitude (thus combing all three ocean basins in similar boxes) is not too simple. The authors make in the final outlook the perspective that Atlantic and Pacific and Indic will be divided in the future, but maybe also some more careful rephrasing about the significance of the resent results might be necessary here.

We shall do this. We believe that our main conclusions hold in both the Indo-Pacific and Atlantic basins. In both basins the surface equatorial regions have high nutrient concentrations and the sub-tropical gyres low concentrations. That may not be so surprising, but also in the northern regions where we find deep ventilation only in the Atlantic, there are high concentrations of nutrients in both basins. There is mixing of sub-surface nutrients to the surface in both regions and in the North Pacific there is also subduction of surface water, even if it does not go as deep as in the North Atlantic. There is therefore good reason to believe that basic circulation or nutrient utilization changes may affect the basins similarly in this simple setup. The separation of the ocean into Indo-Pacific and Atlantic ocean basins would be a greater improvement in a box model that had more vertical levels than in the current model because the vertical structure is different in the two basins.

A. 4) Finally, atmospheric CO2 of a reference simulation should be stated somewhere, as this would be the point from which all the plotted differences in CO2 are calculated from. I understand, that this reference state is the average of the 300 best performing (in terms of comparison with World Ocean Atlas PO4 data) interglacial states. If this is not correct, than it should be explained how a reference state is defined here.

The theory only derives the change in $\mathrm{pCO}_2^{\mathrm{atm}}$ and not the absolute value. It is therefore that we assume our average $\mathrm{pCO}_2^{\mathrm{atm}}$ in the interglacial states as a zero reference and define our changes in terms of this reference state. Because the theory is linear the reference level is not important.

Minor issues: Thank you for pointing out errors and phrases which are not clear. We shall correct, elucidate, or rephrase as appropriate. The following three minor comments contain questions or require explanation.

A.5) *Model description: Why do you choose to have as fifth flux "northern upwelling flux", and not one describing deep water formation in the north, which is to my understanding more one of the principle causes of the global ocean circulation: dense surface waters (cold and saline) which sink down in both the SO and the N Atlantic. You have the related flux in the SO in your setup, but why not in the north?*

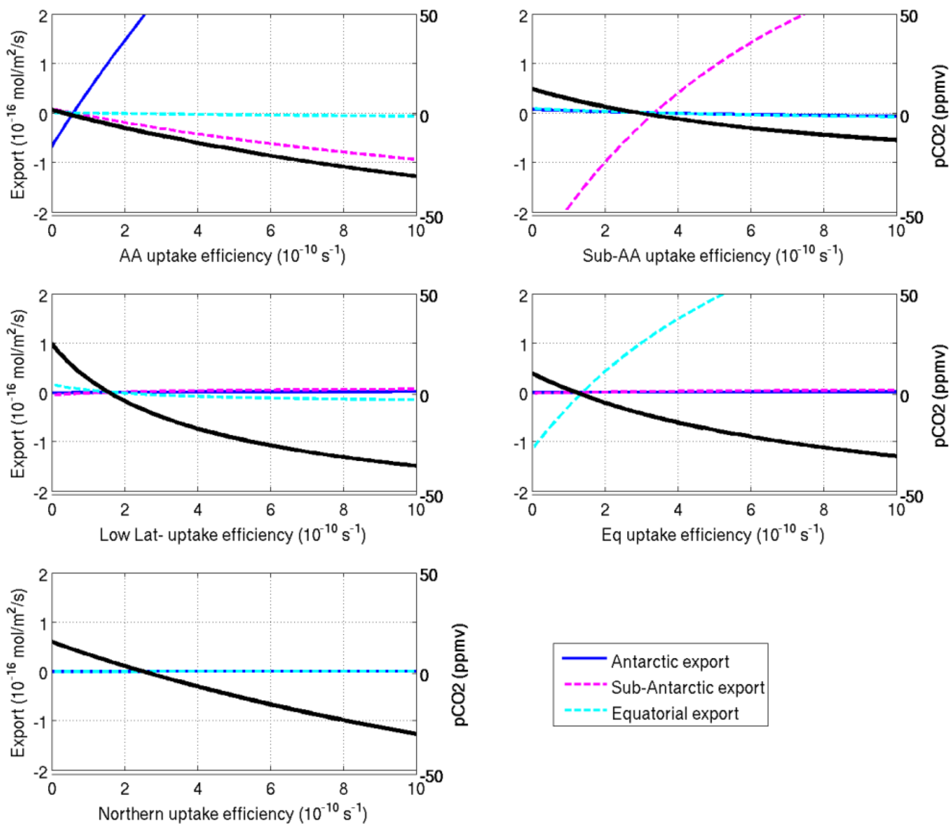
Our rationale was as follows. The northern sinking flux in our model is a combination of the traditional meridional overturning branch driven by mechanical forces (Southern ocean winds and mixing driven upwelling) and a local mixing flux with the deep ocean which is commonly used in box models in the Southern Ocean. The latter is to allow deep nutrients to be mixed to the surface (through convection for instance). Without it it is impossible to obtain the observations of high nutrients in the north. We could have made a northern sinking flux the constrained parameter such as in the SO but this would lead to two problems. First, the sinking in the North Atlantic is often considered to be a function of remote fluxes such as wind-driven Southern Ocean upwelling rather than other way round (although in practice the system is strongly coupled). Second, the northern regions describe an average deep water formation (or sub-500m) in the North Atlantic and North Pacific. The sinking flux therefore does not correspond to a well known 'single' water mass such as Antarctic Bottom Water. The total sinking in the north can still be derived from the other fluxes. In the current setup we can actually see the difference between the sensitivity of pCO2 to the mechanisms that change NADW such as Southern Ocean northward Ekman transport or low latitude upwelling.

A.6) *page 877, line 2-3: I can not reveal the numbers you are giving here out of Fig3 left. If I am in a glacial state, Fig 3 says to rely on the left side, there I find change of pCO2 of -160 to -200 ppmv (not 100 ppmv (which is without a sign and therefore presumably positive (+)), but preformed nutrients is always below 0.5 μmol/kg (and not 1.6 μmol/kg as started in the text). Something seems to be wrong here.*

Yes, the 1.6 μmol/kg is a typo that shall be corrected. We shall be more careful with the sign in front of the CO2 changes.

A.7) *Fig 6: It says "Same as Fig 5", but this should probably mean "Same as Fig 3"? What is the difference between Fig 3 and 6?*

Apologies. There was an error with the copy-editing. The wrong figure was put here. The correct figure is the one below. It should make more sense now.



B) Response to reviewer 2: Your positive endorsement is much appreciated. Thank you for the constructive comments which we shall address as follows.

B.1) My main concern is the lack of precision and remarks about the processes that are (or not) included in the model. My understanding is that some significant contributions to the glacial CO2 drawdown, ocean cooling and carbonate compensation for example, are not included. These processes are rather well-quantified and should be discussed in the manuscript because the targeted CO2 is not the same if the model does represent these processes. Also, the effect of iron fertilization has been quantified by a series of more "realistic" ocean carbon models (see Kohfeld and Ridgwell, 2009 in the SOLAS AGU Book for a discussion) : the outcome of the model output(change in nutrient utilization rate) could be compare to these previous estimations. In summary, I feel that the description of the models needs more details and that the results of the work need to be included into a wider context

We will describe the model and theory on which our results are based in more detail. Please see our response to A1 in which we give more detail about the theory on which the model interpretation is based. We shall also discuss the results in the framework of previous work (please see response to A2) and what is known about the mechanisms that we neglect in this study such as the solubility pump and carbonate pump.

B.2.) I have also a concern about the conclusions made about the role of the equatorial regions. As stated by the authors themselves, the design of the box model preclude them to conclude on any *importance of the EEP upwelling & biology in governing atm. CO2. I believe box models are not the appropriate tools to deal with the complex equatorial circulation.*

We agree. The reason we included an equatorial box is that we were concerned that if the Sub-Antarctic was only divided from the Northern box by a single box that had no upwelling it would affect our conclusions about the importance of the Sub-Antarctic region and the residual circulation. We therefore did our study in a model with and without an equatorial region and warned the reader that the results in the equatorial region itself should be interpreted with caution. We shall stress this point.

B.3) I would also mention that the biology is seen in its simpler expression here. For example, even if nutrient utilization efficiency can change regionally, carbon and phosphorus are intrinsically linked in this model representation. One mechanism that could contribute to the glacial CO2 drawdown is a decoupling of C:P or C:N in the organic matter. This is worth being mentioned (see Tagliabue et al. 2009 CP)

Agreed. We shall mention it.

B.4) Minor corrections:

We appreciated the comments and shall correct these as suggested

C) Response to reviewer 3:

C.1) The paper structure is often confusing. For example, the key assumption of the model that atmospheric CO2 depends only on preformed nutrients and not on pCO2 of different boxes (which in turn depends on alkalinity, temperature, etc.) should be clearly stated in the model description. Instead of this, it is now presented in the results section. This is very confusing: is the relationship between CO2 and preformed nutrients (shown on the figure 2) the model result or is it a part of the model itself?

We do not suggest CO2 depends only on the preformed nutrients but assume only that the preformed nutrients give a good indication of changes in the *soft tissue biological pump*. We shall change the title and abstract to reflect this. The title shall be

"A multi-variable box model approach to the soft tissue carbon pump."

The theory is derived by Ito and Follows whom we cite. We shall state this now more clearly and we shall describe the model and how the atmospheric CO2 is derived in more detail. Please see our detailed response to (A.1.)

C.2.) In the later case, the section 3.1 should move into the section 2 which could be called Methods for a better orientation of the reader.

We shall move the section as suggested.

C.3.) The section 4 could be called Discussion and it should be always made clear in this section whether the authors have in mind the real ocean processes or the model results. Using of terminology in the paper is confusing as well …. The paper should be re-written carefully in the terms of the model setup to avoid the reader confusion.

We apologize for frustration caused by unclear terminology and shall clarify and rewrite as suggested.

C.4.) A serious drawback of the approach is that the box model configuration is fixed to a setup which is not fully appropriate for studying biological pump. In the real ocean, nutrient utilization is taking place in the upper 100 meters or less, while the box model assumes the surface waters to be as deep as 300 - 500 m. This chosen box geometry could influence the results stronger than the processes they consider.

We acknowledge this drawback. In this model the nutrient utilization indicates the ability of the organic matter to escape from the top 500m. We have tried a few other setups (shallower upper layer and no equatorial box) and found that our main conclusions hold in all these setups. However, unlike most box model studies in the literature (which all suffer from geometric problems that could influence results for first order), we are careful to keep our conclusions qualitative.

Minor comments. Thank you for the comments. We shall address all these according to the suggestions. The comments that contain questions or require clarification is discusses below.

C.5.) The title "A comprehensive, multi-process box-model approach" does not reflect the paper content. Is it a comprehensive box model (I do not think so – for example, the BYCICLE model is much more comprehensive) or a comprehensive approach? What are multiple processes of the box model? There are only two processes studied – the circulation and the nutrient utilization. Why not to mention them explicitly in the title?

With comprehensive we mean that we cover a much larger variable space than is traditional in box models. We think of a process as a specific mechanism that changes the circulation or nutrient utilization in a specific way or place (e.g., sub-Antarctic iron fertilization). Most box model studies look only at a few processes, such as Antarctic mixing or a reduction in Antarctic surface nutrients, and thus only vary a few parameters in their model. In contrast, we investigate the whole of our parameter space and this represents many more mechanisms for CO2 uptake than is traditional. Perhaps more importantly, we investigate all combinations of our variables (and not just vary each variable separately from a best guess glacial or interglacial state). This is a novel. However, we do not want a confusing title and have therefore taken out the words comprehensivefrom the title and replaced process with variable. We know that other models have more variables than ours, but they do not use them all as variables but rather as (tuned) constants.

C.6) Introduction, 1st para: "The pCO2-temperature correlation is much stronger in the Antarctic (AA) than in the Northern hemisphere records which suggest that the Southern Ocean (SO) played a dominant role in the glacial carbon cycle." I disagree. The air temperature over Antarctica depends on the atmospheric CO2 content much stronger than the temperature over Greenland, and this is a good reason for the correlation as well. Besides, CO2 is well-mixed gas in the atmosphere and it is not clear why southern sources of CO2 are more important for Antarctica than the northern ones.

We shall rephrase this. It is indeed not that surprising that the correlation in the northern hemisphere is not as good and because of the atmospheric circulation and ice sheets it would not be good even if there was an important mechanism for CO_2 change in the Northern hemisphere. What we would like to say is that because the correlation is so strong in the AA record it points to a positive feedback mechanisms that is at work in the SO, especially at the deglaciation when temperature leads pCO2. If no Southern Hemisphere mechanism existed it is unlikely that there would be such a close relationship between AA temperature and CO2 in which temperature leads.

C.7) Figure 3: labels are unclear (what does "eff" mean for the plot at the bottom)?

This should be nutrient utilization. We used the words 'uptake efficiency' in an earlier version and have not corrected it yet. Apologies.

C.8) Figures 6 and 7: these scatter plots are almost identical. Do you need them at all?

There was an error in copy-editing. The wrong figure 6 is not right. It should be the one in comment A.7