Interactive comment on "MEDUSA-2.0: an intermediate complexity biogeochemical model of the marine carbon cycle for climate change and ocean acidification studies" *by* A. Yool et al.

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The following is a preliminary response to the comments and criticisms made by our referees. As a number of the points raised require additional analysis that will require time and effort, the aim here is to respond to our referees and to outline the manuscript changes that we intend to make to address its current omissions. Upon completion of a revised draft, a further response will be submitted that fully details the actual changes made. That said, we would encourage our referees to review our planned responses to help ensure that our interpretation of their criticisms is correct and that our plans for the revised manuscript fills the gaps that they have identified. It is anticipated that the

revised manuscript will be completed by the end of June 2013.

Before addressing the points raised in their reviews, we would like to thank all three referees for their thorough scrutiny of our manuscript. Particularly so because of its length. We are also extremely grateful for their support of the GMD "model" of publication. While the detailed description and baseline analysis of models is somewhat mundane, it provides a critical underpinning upon which incisive and targeted research can take place.

In the following text, referee comments are italicised while our responses appear in normal font.

REFEREE 1

This model development manuscript describes the second version of the MEDUSA model. The modifications made to the model, which include the addition of state variables for DIC, alkalinity, oxygen, detritus C, and benthic pools of C, N, Si, and CaCO3, do indeed require a descriptive publication such as this. Overall, the manuscript is well written and tends to follow the general format of the first MEDUSA description. While some of the new biogeochemical properties require a better description and analyzes, there are no major issues with the manuscript that would prevent its publication after a moderate revision.

General comments:

1) Oxygen needs a better explanation and/or diagram. It's a new tracer but not well described or justified aside from the short paragraph on pg. 1289. The volume and spatial location of the oxygen minimum zones is also not shown in the results section. Can the model get the oxygen minimum zones in the right place? How would a plot of oxygen at 300m compare to World Ocean Atlas data? Most models have a problem getting oxygen right, especially in the Indian Ocean, is MEDUSA any better?

Similar to its current treatment for DIN and silicic acid, the revised manuscript will include a comparison of the spatio-temporal characteristics of MEDUS's oxygen distributions with those of the WOA, together with some commentary on the model's reproduction of the location and extent of oxygen minimum zones.

2) Why do none of the results show simulated pH? The title mentions ocean acidification and the model can certainly calculate pH. Can the model reproduce the surface ocean pH decline from its preindustrial value?

The revised manuscript will present inter-comparisons of simulated with observationally-estimated (GLODAP) pH for both pre-industrial and "present-day" conditions (= 1990s).

3) Why are no results shown for the 2-D benthic tracers that have been added to the model? The authors state that these variables were initialized at a value of zero. What happens during the model runs? Perhaps supplemental figures could be shown that indicate what is happening to the simulated benthic pools of C, N, Si, and CaCO3.

These were not originally added just as a space-saving consideration. The revised manuscript will include an expanded treatment of the benthic reservoirs.

4) Simulating the export of carbon to the deep ocean is one of the major reasons for the improvements made to the model, yet no serious attempts have been made to compare the model results to any observations. I suggest that the authors make some comparisons (both graphically and statistically) to the deep sediment trap data compiled by Honjo et al. (2008). Note that this data set also contains biogenic opal data that can be compared as well. Reference: Honjo, S., Manganini, S.J., Krishfield, R.A., Francois, R., 2008. Particulate organic carbon fluxes to the ocean interior and factors controlling the biological pump: A synthesis of global sediment trap programs since 1983. Progress in Oceanography 76, 217-285.

This is a fair suggestion and is actually something that we have explored. We will revise the manuscript to include comparisons like those suggested by the referee.

5) Why aren't more DIC and alkalinity comparisons made to the GLODAP database? It would be nice to see figures like Fig. 8, 9, and 10 comparing DIC and alkalinity. Annually averaged surface DIC and alkalinity comparisons (maps and statistics) could also be included.

As per our earlier remarks about oxygen, we will revise our manuscript to include a comparison between MEDUSA's output and GLODAP.

Specific comments:

1) Why do zooplankton of the same size class not prey on each other (i.e.,selfpredation)? How are these losses and biogeochemical cycling accounted for?

Is it implicit in their mortality term?

Intra-trophic level "cannibalism" of the modelled actors is certainly a possibility, and it can be argued that the use of a quadratic closure term represents this (Edwards and Yool, 2000). While not explored in this manuscript, Yool et al. (2011) examined this closure term in MEDUSA-1, and while the model performed differently, its behaviour was still relatively close to that with the model's default hyperbolic closure term. The manuscript will be revised to draw the reader's attention to both cannibalism as a possibility and Yool et al. (2011)'s examination of such functional forms.

2) Zooplankton grazing does not appear to be temperature dependant. Without temperature dependence there may be some instances in colder waters where strong top-down control occurs because the fixed zooplankton growth/grazing rate exceeds the temperature-dependent phytoplankton growth rate. Could the authors comment on this?

Properly investigating the impact of this for MEDUSA would be a significant effort. However, we will revise the manuscript to draw attention to the issue that the referee raises, and to models in which this sort of temperature dependence is included.

3) In equations 22 and 29 the "I" looks somewhat like a slashed division sign. In both equations this becomes confusing because the denominator is raised to the 1/2 which looks very similar to the "I" just before it. Can the font of the "I" be changed?

The appearance of equations in discussion manuscripts in GMDD differs significantly from that of published manuscripts in GMD. We will ensure that this formatting issue is dealt with should this manuscript proceed to publication.

4) In equations 22 and 29 how is irradiance calculated? What kind of light attenuation occurs? Is it dependant on phytoplankton biomass (i.e., self-shading)? If this is described in another paper could the authors please indicate which one?

MEDUSA uses a light attenuation submodel derived from the simpler LOBSTER model

(Levy et al., 2001). This splits PAR into two wavebands ("red" and "green-blue") that are attenuated separately by seawater and phytoplankton chlorophyll (i.e. not biomass). As such, the model includes self-shading by phytoplankton. The text will be amended to describe this more clearly.

5) Alkalinity paragraph on pg. 1289 (lines 12 − 17) is there any justification for this simplistic model and parameterization?

This simplicity is justified by the omission from consideration of processes that would require a more complex treatment. For instance, bulk DIN is considered rather than nitrate and ammonium, differing use of which impacts proton consumption / production. Similarly, the inclusion of explicit calcifiers would require a more sophisticated alkalinity cycle. However, given the actors and processes included in MEDUSA, such embellishments cannot be supported.

6) Pg. 1299 line 10 "localized observations" are mentioned. What regions or where are these localized observations from?

They are from studies in the Sargasso, the Equatorial Pacific and the central North Pacific.

7) Pg. 1299 lines 12- 22 referring to Fig. 25. Do deep Chl/productivity maxima occur? I suspect that they do. This should be mentioned.

Yes, they do occur in the model at various locations and times of the year. The text will be revised to mention this.

8) Pg 1304 line 9. The reference to Riebesell et al. is not clear. What did Riebesell et al. show?

We agree, this is a rather opaque reference, and will revise our text accordingly. Riebesell et al. showed that elevated availability of CO_2 can increase primary production. This phenomenon is absent in MEDUSA-2, despite it being one that could potentially be included.

9) Pg. 1308 lines 7 – 14. Nitrogen fixation and denitrification are discussed as potential future model improvements. This could be very important if the model is used in climate change studies. Right now the model assumes that the nitrogen budget is balanced. If future changes occur to throw off this balance, then the model won't work well since it's a nitrogen-based model and the ocean losing or gaining nitrogen would change the biogeochemical dynamics considerably. Perhaps the authors should mention this caveat somewhere.

We would again agree with the referee – the manuscript does not currently mention either model's current fixed nitrogen budget or the implications for it of the suggested nitrogen cycle changes. The revised manuscript will draw attention to these points.

10) Fig. 26 right panels can the color bar be changed? There are really only 3 colors (limiting factors) so a range probably isn't needed and is confusing.

We will revise the figure as suggested by the referee.

11) Fig. 29 and pg. 1301 why is only N detritus shown don't we have C detritus now. How does it compare especially give the varible C:N ratio detritus can now have. It's hard to figure this out even if looking at both Fig. 29 and 25.

The choice of N detritus for these figures is simply based on habit – MEDUSA-2 now both contains carbon tracers and permits a variable C:N ratio in detritus. We will revise the manuscript to include a treatment of the C:N ratio of sinking detrital material.

REFEREE 2

Yool, Popova and Anderson describe and present an updated version of the MEDUSA model. The predecessor, MEDUSA-1.0 has been extended by oxygen and components of the carbon cycle, including a variable stoichiometry in some of the pools, a simple formulation for pelagic-benthic exchanges and extended phytoplankton parameterisations. Model results are compared to those of its predecessor, and to observations of nutrients, oxygen, Chl and production, pCO2 and CO2 flux at the sea surface.

General comments:

The modified model presents important additions to the MEDUSA model and is a more appropriate tool to investigate recent questions such as ocean acidification. The paper is very well written, concise and mostly comprehensive. I really appreciate the effort the authors have put into the model documentation, even if - having had a look at the MEDUSA-1.0 documentation - the paper in some parts repeats the previous description. However, this is stated clearly at the beginning, and I agree that a "stand alone" version of model description is really helpful. I would even suggest to extend this a bit more with respect to the large detritus description (see below).

The few concerns I have are related to the representation of oxygen, alkalinity, and the associated stoichiometry (see below). I would further appreciate a few sentences on two discrepancies between models and observations that have not been addressed in the - otherwise very open and thorough - discussion (see below). I recommend publication after this, and a few minor points, have been addressed.

Specific comments:

1) As already noted by Anonymous Referee 1, oxygen requires a more detailed description. I also suggest to comment more on the stoichiometric relations used by the model. In particular, my suggestions are as follows:

1.1 Eq. 14 and Table 4: As far as I understand, nit = 2 describes the oxygen demand

for complete nitrification, i.e. it stems from the bulk equation NH3 + 2O2 ! HNO3 + H2O. If this is true, I suggest to mention it explicitly.

We would agree with the referee about this omission of detail concerning the oxygen cycle. The revised manuscript will include a separate subsection on MEDUSA-2's treatment of oxygen, including its stoichiometry relative to the other modelled elemental cycles. The current text (in the "Miscellaneous" section) is too terse.

1.2 For remineralisation including nitrification the model seems to use the stoichiometry by Anderson (1995), with -O2:P=150, or, alternatively, -O2:N = 9.375 = nit+ rem Pn. If this is the case, I suggest to mention it explicitly.

Yes, this is the case and should not have been omitted. The revised manuscript will identify this source.

1.3 Table 4 notes a value of Phy = 9:4375 for "phytoplankton O2:N ratio", which I find quite confusing. If this is meant to be the oxygen content of phytoplankton organic matter (i.e. about 19 oxygen atoms per nitrogen atom, or more that 300 oxygen atoms per phosphorous atom), this would imply an extreme aqueous phytoplankton (see e.g., Paulmier et al., 2009 for the implications of different organic matter constituents, and their relation to oxygen demand of remineralisation). I therefore suggest to either omit this parameter (which does not seem to be used anyway) from the description, or to be more explicit about the oxygen cycle and associated stoichiometry. The same holds for the zooplankton parameter, zoo

The referee is correct. These parameters crept into a previous version of the manuscript and have not been removed despite not being used in the model description or code. The revised section on oxygen mentioned earlier will properly expand on this element's stoichiometry.

1.4 There are Taylor plots for all sorts of model-observation comparisons - why not for oxygen?

Per our remarks to Referee 1, material showing the model's oxygen performance will be included. It was originally omitted because surface oxygen is more strongly constrained by temperature than ocean biology so was not felt to strongly inform assessment of MEDUSA-2's performance.

1.5 A side note: What about denitrification? Currently denitrification seems to be built implicitly into that model (p. 1289, lines 9-10). On the other hand, the model already simulates - in contrast to phosphorous based models, e.g. by Najjar and Orr - another potential oxidant, namely nitrate. With the given parameterisation, however, simulated nitrate to me seems to be closer to phosphate times 16, than to "real" nitrate. The issue is discussed briefly at the end of the paper, but I suggest to perhaps comment on this also in the model description. I also would not consider this as an optional task for a "specialist" model (as stated in the discussion), but, given that the model is supposed to be "an efficiently-sized tool for realistically simulating the oceans major biogeochemical cycles" (p. 1309, lines 9-11) a more necessary future step than some other complications.

The description of aerobic remineralisation is insufficiently clear on MEDUSA-2's behaviour. As Referee 1 has noted, the nitrogen cycle of the model is conservative, and DIN is not consumed through denitrification when oxygen becomes limiting. The rationale – which will be explained in the revised manuscript – is one of simplicity, since it spares MEDUSA-2 from the need to include a submodel of nitrogen fixation (which, as we note in the manuscript, is still not well understood). Also, given the time-scales over which MEDUSA-2 is currently run (decades to centuries; insufficient to fully ventilate the deep ocean), and their relative magnitudes compared to primary production, we judge that the omission of denitrification and nitrogen fixation will not significantly affect our simulations. That said – and as we discuss in the manuscript – the inclusion of these processes will be an obvious future extension of MEDUSA-2, and will permit it to explore the impacts of changes in the balance of these processes under likely future climate change.

1.6 The only effects on alkalinity considered in the model seem to be those related to CaCO3. However, this representation is not exhaustive, as production and remineralisation, and associated changes in nitrate (and phosphate) will also affect alkalinity. See e.g. Paulmier et al. (2009) for a brief overview on how the different processes affect this tracer, or Wolf-Gladrow et al. (2007) and citations therein for more details. Also, the effects of denitrification (i.e. the removal of nitrate) might have a large effect, if these were considered.

As mentioned in our response to Referee 1, the alkalinity cycle in MEDUSA-2 is simple because it ignores a number of processes that are known to play a role in the cycle in the real ocean. For instance, the consumption and production of protons driven by the nitrogen cycle.

2. (p. 1282-1284) I found the description of fast detritus production (and remineralisation) a bit confusing. The "T"-terms are not explained, and suddenly there seems to be some reference to the vertical box index k (correct?). Does, for example, eqn. 88 mean that the gain in detritus-N in box k + 1 comes from detritus in the layer above, plus the local production from zooplankton grazing etc? In other words, does detritus sink one vertical box per day? If so, sinking speed is is coupled to the vertical discretization, which could be mentioned, in case other users want to apply this model to different grids.

Because the fast-sinking detritus submodel largely overlaps that of MEDUSA-1, the description here is somewhat curtailed. However, as the referee's points clearly indicate, too much detail has been removed. The manuscript will be revised to explain the source of the T terms as well as to expand upon the relationship between the vertical grid boxes. On this latter point, fast-sinking detritus is assumed to sink sufficiently quickly that its vertical profile of remineralisation can be modelled implicitly, without direct consideration of sinking speed. The transfer of material between boxes that the referee mentions occurs within a single time-step, and not between time-steps. This is a common approach across models where the particle flux to the deep ocean is not

explicitly treated, and originates in early analyses of ocean data such as Martin et al. (1987).

3. Many of the model features in the equatorial Pacific and the upwelling off Peru and Chile do not appear in the observations, e.g. elevated DIN or production. Could this be due to an insufficient physical model? On the other hand, at least along the equator the model's pCO2 seems match the observations quite well (but not in the coastal upwelling). A few sentences on this would be nice.

In part these deficiencies stem from modelled physics, but they also relate to the availability of iron in this region (which has changed between MEDUSA-1 and MEDUSA-2). The manuscript already briefly mentions this latter explanation, but some remarks about the role of physics will be added in the revised manuscript.

4. (Fig. 25 and p. 1299 ff) Astonishingly, the model shows a high rain rate of organic matter to the sediment south of 40 S. I am not aware of this pattern in any observation or compiled data set (e.g., by Seiter et al., 2005, or Jahnke, 1996). Is it possible to comment on the reasons of this mismatch?

As Figure 13 hints, excessive Southern Ocean productivity in MEDUSA is responsible for this excessive seafloor flux. The manuscript will be revised to draw attention to these mismatches.

Technical/minor corrections and comments:

p. 1272, eqns. 23 and 24: I suppose +N and +F shouldn't be subscripts?

Well spotted. This is a formatting error and will be corrected in the revised manuscript.

p. 1274, eq. 36: What is U1? Could it be explained below?

As described in Table 1, U is the "hypothetical growth ratio at infinite Si:N ratio". The manuscript will be revised to mention this within the text.

p. 1274, lines 18/19: brackets of reference

This will be corrected.

p. 1274, eqn. 38: If I am not mistaken, then units of RSi:N are mol Si/mol N? How can the inequality relates this term to its inverse?

We would agree with the referee that this presentation of the nitrogen-silicon dynamics of the diatom phytoplankton is somewhat confusing. When the submodel was originally described in Mongin et al. (2003), numerical values were used instead of functions of parameters used here (whose actual values are identical to those in the original description). And in Yool et al. (2011) we augmented a very similar description of the submodel with a diagram (Figure 2) that illustrated how the uptake rates of N and Si are impacted by the diatom biomass Si:N ratio. We will revise the manuscript to clarify this confusion.

p. 1277, eq. 50 and 53: These equations, i.e. respiration, seem to be the same, regardless of the food composition - so why write them two times? (Likewise for eqns. 63 and 66.)

We agree, this redundant repetition escaped our attention – even the model code omits it. It will be removed in the revised manuscript.

p. 1280, eq. 79: I assume there is an index missing? (MDc?)

Yes, there is. This will be amended.

p. 1281, line 4: deposition

This will be amended.

p. 1295, line 11 and elsewhere: what exactly is meant with "surface" - first layer only?

Yes, this is correct. The text will be amended so that this is clear to the reader.

p. 1305, line 17: sensitivity –> seasonality?

Yes, this will be amended.

p. 1307, line 5: foraminiferans

This will be amended.

p. 1307, line 16: "influence OF ambient marine chemistry"?

This omission will be corrected.

p. 1309, line 6: hierarchy

This will be corrected.

REFEREE 3

General Comments:

The manuscript describes the MEDUSA-2.0 global biogeochemical model, which is an upgrade to MEDUSA-1.0 that seeks to explore perturbations of the marine carbon cycle due to climate change and oceanic carbon addition. This model, as compared to its earlier version, includes several new tracers that represent the marine inorganic and organic carbon cycles, dissolved oxygen, and benthic nutrient cycling. The paper describes 1) core features of the previous version of the model as well as modifications, 2) the addition of new state variables and parameters, and 3) the steady-state results of a spin-up of the model from 1860 to 2005. The model description is good and its subject is very appropriate for publication in a specialized model description journal such as Geoscientific Model Development. The impact of carbon addition to the ocean and atmosphere on the marine carbon cycle, and its role in ocean acidification, is an incredibly important subject and a variety of models and experimental work will be required to gain a better grasp of the magnitude of this complicated problem. This model has the potential to do just that. However, as it has been presented, it is difficult to identify what MEDUSA-2.0 will add to this scientific question that other comparable models could not. This issue was not sufficiently argued by the authors and should be included in the abstract. Furthermore, essential figures and results describing the state of the MEDUSA-2.0 marine carbon cycle (DIC and alkalinity) and comparison to available data, were lacking. I expect that the model will be publishable; however, I view the problems mentioned above as important omissions and recommend that the manuscript be returned to the authors for major revisions.

Specific Comments:

I have read and agree with the general and specific comments of Reviewer 1 and will not repeat them here unnecessarily.

1) Important variables were not presented, such as DIC and alkalinity, along with com-

parison to available data (GLODAP, for example). Given that the goal of the manuscript is to describe a model that will be used to study the marine carbon cycle, these are absolutely necessary and will go a long way to justifying the value and novelty of MEDUSA-2.0.

As remarked to Referee 1, the revised manuscript will include intercomparisons of MEDUSA-2 and carbon / alkalinity observations.

2) Discussion of many of the results was not more than a simple description of the most evident features of the associated figure. The authors must provide (for pCO2, DIC, and alkalinity, among others) more interpretation and insight, and in particular a more profound discussion of the sources of model bias.

Per our remarks to the other referees, we will add material intercomparing MEDUSA-2 with observations relating to the carbon, oxygen and alkalinity cycles.

3) The authors do not sufficiently discuss how this model compares to others of similar structure and complexity. More importantly, the authors do not describe what are the most positive features of the model and why one should use it over other models. This should be a prominent feature of the abstract.

The Referee raises a fair point concerning MEDUSA-2's relative performance. To address this, we propose to extract relevant biogeochemical fields (nutrients, chlorophyll) from comparable models submitted to the CMIP5 archive, and to quantify relative goodness-of-fit. Part of this intercomparison will examine the relationship between model accuracy and complexity, since a guiding force in the development of MEDUSA has been its relative simplicity compared to contemporary phytoplankton functional type (PFT) models.

Page 1261 Line 6-11: A core result presented in Behrenfeld (2006) was that net primary production had been decreasing from 1999 to 2006. However, this was following a strong increase in net primary production during the 1997 to 1999 transition from

El Nino to La Nina. To say that the conclusion of Behrenfeld (2006) is that primary production is decreasing is a much stronger conclusion than that drawn and detailed in the article, and is moreover an incorrect one. Please rewrite this sentence to better reflect the limitations of Behrenfeld (2006). Moreover, since it is by no means clear how carbon dioxide emissions will impact net primary production in the future, please phrase this as something that is being investigated and not something that is certain (see, for example, Taucher, J., and A. Oschlies (2011), Can we predict the direction of marine primary production change under global warming? Geophys. Res. Lett., 38, L02603).

The Referee is entirely correct to dispute our manuscript's presentation of Behrenfeld et al. (2006). The work cited is tangential at best to the point being made. We actually meant to cite the study of Boyce et al. (2010) as an example of phytoplankton (and, presumably, productivity) decline. We will amend the manuscript on this point, and will include reference to other studies such as that of Taucher and Oschlies (2011) that examine change in the ocean.

P 1262 L 22-23: I do not agree that a multi-decadal hindcast simulation for the years 1860-2005 was performed, since no data throughout that time period was used to validate the model. Instead, I would term this simulation as a spin-up starting from a pre-industrial state for certain variables (DIC and alkalinity, results not shown) and a present-day state for others (DIN, silicic acid, and oxygen).

We will reword this to reflect the Referee's concerns. In passing, only the DIC initial condition is technically "pre-industrial", and even then it is an estimate based on near-present day observations and a deconvolution technique to back-calculate anthropogenic CO2.

P 1277 L 17 (Eq. 53): On L 6-8 the authors note that excess carbon is respired and excess nitrogen excreted. Therefore, in the case where cabon is limiting, there should be no excess carbon, and therefore no excess carbon respired. I wonder therefore why

equation 53, describing R Z,mu, is not simply zero, as equation 49? Another way of asking this question is, why are the respiration equations (53) and (50) identical to one another under the different regimes of nitrogen and carbon limitation?

The submodel is configured in accordance with well-known stoichiometric theory (Anderson and Hessen, J. Plankton Res., 1995; Anderson and Pondaven, 2003). Excretion of N is associated with the respiration of compounds that contain N. If sufficient C-rich compounds are available (i.e., high C:N ratio in food) to meet respiration costs, N-containing compounds are spared for growth and N excretion is zero. In contrast, respiration always entails the use of C-containing compounds and can never be zero. In the event that N is limiting, excess C is respired in addition to the obligatory metabolic requirements. Although it has been removed here to save space, Yool et al. (2011)'s description of MEDUSA-1 (which shares this submodel) includes a diagram (Figure 3) that illustrates the shifting balance of growth, excretion, respiration and egestion across a range of food C:N. All that said, and as noted by another referee, the current presentation of the equations confuses things by repeating the respiration term. Revising the manuscript to remove this repetition will hopefully make the submodel's treatment of respiration more obvious.

P 1278 L 7-8: Since there are no exact equivalents in the microzooplankton equations, equations 55 and 56 should be explained in more detail.

Equation 55 serves simply to make Equation 54 fit on a single line, while Equation 56 simply accounts for the silicon ingested by mesozooplankton grazing on diatoms. The text will be amended so that this is clear.

P 1279 L 2: Same comment as for equation 53. Why is this not zero?

See previous remark.

P 1279 L 12-13: I do not understand the sentence describing equations 67 to 71 as density-independent terms. These appear to all be linear density-dependent terms.

The *absolute* rate of loss is, of course, dependent on density, but the *specific* rate of loss is not – that is, the same fraction of standing biomass is lost per unit time regardless of density. This differs from equations 72 to 76, where the specific loss increases (to saturation) with increasing density – hence density-dependent. The text will be amended so that this standard terminology is clearer.

P 1280 L 1: Based on the response to the previous comment, this should be corrected so that it contrasts with the linear density dependent terms of equations 67-71.

See previous remark.

P 1287 L 6-14: Since a core addition of this model is the inorganic carbon cycle, I think it would be useful to discuss the sources of the differences in Fig. 3 in more detail.

Differences between the modelled and observationally-estimated CCD fields are driven by changes in the deep concentrations of DIC and alkalinity. We will investigate the source of these changes and will amend the manuscript to discuss our findings.

P 1283 L 5: The order of presentation in this section is different from the sections before it. Here, the authors describe equations and then present the equation, whereas earlier the authors presented equations and then described them. Please change this so that the entire text is consistent.

We will endeavour to address this suggested change. The change in presentation order here is not accidental but reflects the nature of this submodel and our intention to describe it clearly.

P 1295 L 10-21: The authors did not describe a possible cause of the most noticeable discrepancies between observational and simulated results for DIN and silicic acid.

Some sources of these discrepancies are currently addressed in the Discussion of the manuscript (e.g. over-diffusive physics; excessive ventilation; changes in iron availability since MEDUSA-1). We will make this clearer in the manuscript.

C589

P 1296 L 17-end: A few lines discussing how chlorophyll is better or worse represented than in MEDUSA-1.0 would be instructive.

The manuscript will be expanded on this point. Since MEDUSA-2's chlorophyll sub-model is the same as that of MEDUSA-1, the differences between them are a matter of degree. There are some locations – e.g. the equatorial Pacific – where differences are more systematic, and this is related to aspects such as iron availability that are discussed elsewhere in the manuscript.

P 1297 L 5-17: The authors should describe or suggest reasons why the global primary production has decreased relative to MEDUSA-1.0 and whether this is a positive or negative feature of MEDUSA-2.0.

An important reason for this decrease is the change in the field of iron deposition between model versions. This is already discussed on P 1304 L 21-25 though the text at this point will be amended to direct the reader to this.

P 1298 L 3-6 and Fig. 18: To be consistent with the observations as presented in Figures 13 and 14, I suggest that the observational data used for the Taylor diagram be the average of the three models (VGPM, Eppley-VGPM, and CbPM) instead of just those for the VGPM estimate.

While the Referee's suggestion is a sensible one, it was suggested to us at a recent presentation of these results that the simple average of different estimates is frowned upon in certain sections of the community. However, we will nevertheless revise this Figure to present a broader intercomparison with estimates of primary production.

P 1298: As mentioned for several other variables, it would be instructive to discuss a potential source (or sources) of the discrepancies between the model and observations.

See our previous remark about model discrepancies. Again, we will try to make our explanations more obvious in the text.

C590

P 1301 L 3-9: It would be a very useful validation to include a brief discussion comparing these results to databases of zooplankton (see COPEPOD, http://www.st.nmfs.noaa.gov/copepod/, for example).

This is a helpful suggestion. We have downloaded the most relevant COPEPOD dataset (carbon biomass) and will include an intercomparison with MEDUSA-2 in the revised manuscript. However, there are still a number of problems when intercomparing such a dataset with model output (e.g. incomplete seasonal sampling; relevant depth of observational sampling; sizes of zooplankton sampled), but these will be discussed in accompanying text.

Technical Corrections:

I suggest more clearly identifying the state variables by bolding or with an overbar, for example. The model consists of many equations and identifying the state variables clearly would greatly improve their readability.

While we would like to oblige, GMD has a house style for formatting that may preclude this. After the manuscript was originally submitted, a number of changes were made by editorial staff to conform to this style. However, we can try.

P 1262 L 1: Archer reference not in parentheses

Yes. This will be corrected.

P 1262 L 4: I think "shadowing" should be "shallowing"

Yes, it should be. This will be corrected.

P 1268 L 9: The equation for time rate of change of DIC does not have a number.

It should have. This will be corrected.

P 1270-1271: Equations 15, 16, 17, 18 are missing the denominator d in the d/dt

They should have. This will be corrected.

P 1272: Equations 23, 24. The + N and + F terms in the denominator of the right-handside of the equations should not be subscripts.

Well spotted. This formatting is incorrect and will be amended.

P 1274 L 18: Reference to Martin-Jezequel et al. (2000) should be in parentheses

Yes. This will be corrected.

P 1275 L 14: "these" instead of "this"

Yes. This will be corrected.

P 1277 L10 and 14: The ellipses are not appropriate here

Yes. These will be removed.

P 1277 L 23: As noted earlier, the equation for time rate of change of DIC is not numbered, and this causes an incorrect reference to the DIC equation, which is cited as equation 13 (which is the equation for time rate of change of alkalinity on page 1269).

This formatting glitch will be corrected.

P 1278 L 1: Although it is included in the equations, I think it would be instructive to mention in words the four prey items of the macrozooplankton.

A list of these will be added to the text.

P 1279 L 13 and 17: The ellipses are not appropriate here

Yes. These will be removed.

P 1279 L 5: Should be "losses due to grazing"

Yes. This will be amended.

P 1279 L 6: Should be "losses due to other processes"

Yes. This will be amended.

P 1280 L 5: The comma at the end of this line should be a period.

Yes. This will be corrected.

P 1280 L 16: The S in the left hand side of equation 77 appears to be a different font (or italicized) than the rest of the text.

Yes. This will be corrected.

P 1284 L 6: in the fc(lat) term, please correct the fonts

Yes. This will be corrected.

P 1286 L 10: silic should be silica

Yes. This will be corrected.

P 1292 L 15: The reference to Jones et al. (2011) should be in parentheses.

Yes. This will be corrected.

P 1295 L 17-18: Correct "show very the same patterns of bias"

Yes. This will be corrected.

P 1297 L 6: "Simpled" should be "simple"

Yes. This will be corrected.

P 1298 L 2: "that" should be "than"

Yes. This will be corrected.

P 1298 L 12: Replace +ve with positive

Yes. This will be amended.

P 1303 L 17: "size of supercomputers" should be "processing power of supercomput-

ers"

Yes, this is a more accurate description. The text will be amended.

P 1307 L 5: "foraminiferrns" should be "foraminiferans"

Yes. This will be corrected.

P 1307 L 16: "influence ambient" should be "influence of ambient"

Yes. This will be corrected.

Figure 1: There is an errant line to the right of the Benthic CaCO3 box.

This is meant to be a connection between the benthic CaCO3 box at the right of the diagram and the pelagic DIC box at the left of the diagram. Amending this with an arrowhead may make this clearer.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1259, 2013.