

Interactive comment on “Ocean biogeochemistry exhibits contrasting responses to a large scale reduction in dust deposition” by A. Tagliabue et al.

A. Tagliabue et al.

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RESPONSE TO REVIEWER.

Review of Ocean biogeochemistry exhibits contrasting responses to a large scale reduction in dust deposition by Tagliabue, Bopp, and Aumont. By J. Keith Moore

>>We thank J. Keith Moore for his considered appraisal of our manuscript. Our responses are in italics (preceeded by **) following each specific point/commentary.

This paper examines the response of an ocean biogeochemical model to a ~60% decline in dust deposition over a 240 year transient simulation. As iron is a source of the key micronutrient iron, this dust decrease has the potential to modify global biogeochemical rates and air-sea CO₂ exchange though its influence on the biological pump. The main finding of this paper is a very weak response to the large decrease

in atmospheric iron inputs from mineral dust. This is very different than several previous model results (Moore et al., 2006; Parekh et al., 2006; Moore and Braucher, 2007). The authors attribute their weak response to the inclusion of a sedimentary iron source (largely missing from previous modeling efforts). I think the sedimentary source certainly accounts in part for the weak response to dust, but there are other relevant factors (see comments below). The paper is interesting and the topic is important. I certainly think the paper is suitable for publication, but I have a number of questions and concerns that should be addressed in a revised manuscript.

General Comments The paper results are critically dependent on the parameterizations of nitrogen fixation, denitrification, iron scavenging, and the various iron sources. The paper could benefit from additional description and analysis of each of these processes (see more specific comments below). Secondly, the paper could benefit from some comparison with the paper published this year in *Biogeosciences Discussions* by Moore and Braucher (Sedimentary and mineral dust sources of dissolved iron to the World Ocean, J. K. Moore and O. Braucher, *Biogeosciences Discuss.*, 4, 1279-1327, 2007, a revised version for *Biogeosciences* is available at www.ess.uci.edu/~jkmoores). Dr. Aumont was a reviewer for this article. Several aspects of this work are relevant for the current paper including the relative influences of sedimentary and mineral dust iron sources, and one experiment with no iron from mineral dust (a more extreme dust reduction than that examined here). They also suggested that models that do not include the sedimentary iron source would be overly sensitive to variations in dust deposition.

Specific Comments Two aspects of the iron cycle are important for accessing this manuscript. The first is that the authors assume a low solubility of 0.5% for the iron in mineral dust. Most other studies have assumed 1 or 2%, with some as high as 10% solubility (Fung et al., 2000; Moore et al., 2004; Parekh et al., 2006). Worse still a number of recent studies suggest that the solubility in many of the HNLC regions (far from dust source areas) are much higher, from 10-30%, even higher (see Mahowald et al., 2005;

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Fan et al., 2006 and references therein). A low assumed solubility of 0.5% will tend to minimize the importance of the mineral dust source relative to other sources, and give the model a weaker response to variations in dust deposition. This idea needs to be acknowledged and the authors should discuss how a solubility of 10-20%, outside the large dust plumes near source regions, might alter their results and conclusions.

****Reconciling the observed wide range in dust solubility with the smaller values that need to be used in global ocean models is a challenge. As the reviewer is no doubt aware, including a 10 or 20% solubility for Fe in PISCES results in unrealistic distributions of Fe (and other tracers), especially in locations that experience high rates of dust deposition. Our choice of solubility value was driven by these concerns. We would also note that, in effect, the model is considering Fe that is $\delta^{216}\text{bioavailable}\delta^{217}$; (bFe), whereas solubility studies are measuring a dissolved Fe concentration (dFe). These two quantities need not necessarily be the same and if we assume bFe makes up some fraction of the generic dFe pool, then it is not entirely unrealistic to utilize a lower value. Nevertheless, in reality solubility is likely related to a multitude of processes, e.g. source region, atmospheric transit time, mode of deposition, mass flux and particle size. We likely underestimate solubility in regions of low dust deposition and vice-versa. One way forward might be to inversely tie the solubility to the deposition flux, but this is beyond the scope of this paper.**

****As suggested by the reviewer, the manuscript has been amended to include a comment on how our solubility choice might impact model results. In brief, we would not anticipate a large effect, since regions where solubility would be higher should be those that experience low rates of dust deposition already. In addition, such regions do not show the large changes found in the high deposition regions by 2100.**

**** Finally, the weak response of ocean biogeochemistry to a reduction in dust deposition mostly results from the compensatory biogeochemical processes that arise in adjacent ocean regions rather than an underestimation of the direct dust effect. Indeed, we find the local biogeochemical response can be large in those regions directly**

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impacted by the lesser dust deposition (previous Figures 3 and 4).

The authors need to better quantify the iron cycle in their simulations. For the preindustrial, and year 2000, what are the inputs of dissolved iron in the upper 300m (or 500m) from mineral dust, sediments, and the riverine source? No reference or details of any kind are given concerning the river source for iron. A three panel map that showed the spatial distributions of the three source terms would be helpful. Maps of surface dFe and nitrate should be added as additional panels to Figure 2.

**Such a figure has been included. This we feel permits an evaluation as to the relative contributions of atmospheric and sedimentary Fe sources. In addition, maps of surface Fe and NO₃ have been added to Figure 2.

The paper also needs some evaluation of how well the model is reproducing the observed iron distributions for the present time. Moore and Braucher (2007) suggested that models which overestimate the sub-euphotic zone iron concentrations would display a weakened sensitivity to variations in dust deposition. They also noted that biological uptake and scavenging would tend keep surface iron values more reasonable if even if sub-surface concentrations were too high. I am wondering if part of the weak response to dust variations in the current work is because sub-euphotic zone iron is too high (due to insufficient scavenging). Aumont and Bopp (2006) compared model results with observed iron at the surface and at 1000m. Something similar is needed here but for sub-euphotic zone waters (100-300m). The figure 2-type plot from Aumont and Bopp (2006) is one option, but some more statistical detail would be preferable. What is the mean bias and correlations over this depth range, of the model compared to observations?

**We include a statistical analysis of model performance in the manuscript. We note that subsurface (i.e. 100 to 300m) concentrations are not systematically over or under estimated in regions impacted by dust (mean bias is +0.05nM). Specifically, in the Pacific sector of the Southern Ocean there is a positive model minus data bias, but the

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opposite is true in the Atlantic sector (where the dust deposition declines). Elsewhere, both the tropical Atlantic and North Pacific Oceans also show no systematic over (or under) estimation (the bias ranges between -0.4 to $+0.4$ nM). Furthermore, even with our low solubility parameter, dFe is still overestimated in regions typified by large fluxes of dust (e.g. Tropical Atlantic and eastern Indian Oceans). This is probably due to the higher dust deposition estimate in the Mahowald et al. [2006] dataset versus previous deposition estimates. Further highlighting the need for a variable Fe solubility in dust, outlined during our response to the reviewer's comments on our choice of solubility parameters. We also report correlation coefficient R and the root mean squared error for the above noted depth ranges. Overall, these values compare well to those recently published by Moore and Braucher (2007). The text has been amended to include these points.

The full description of the nitrogen fixation parameterization from Aumont and Bopp (2006) needs to be included in the methods section. In that work, the total nitrogen fixation is scaled by the total denitrification to maintain a relatively balanced system (is this the same here?). If so, it is not surprising that the two were linked in the present simulation. This was not due only to realistic feedback processes as the current ms implies, but also to this explicit (unrealistic) linkage through the N fixation equation.

**In this study (unlike the previous study mentioned by the reviewer) N_2 fixation and denitrification are uncoupled. Therefore, denitrification is indeed responding to the feedback processes we outline, rather than as an artificial linkage to N_2 fixation. We have amended the text to make this clearer.

The authors should also address why phosphorus was not included as a limiting factor on N fixation. It should also be made clearer that the approach assumes that the photosynthetic efficiency, the nutrient uptake efficiencies and stoichiometry (Fe/C, P/C, etc..) for diazotrophs is the same as for the model small phytoplankton group. Is phosphorus the limiting nutrient anywhere in the simulation?

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****Acknowledged and addressed.**

On page 2535, lines 5-12 the authors state diatom Fe limitation increases by between 10 and 60% between 2000 and 2100 What are these statements referring to, the degree of Fe-stress in particular regions, the % of total ocean area?

****This refers to the change in the degree of Fe limitation of growth. The text has been amended to make this clearer.**

A nice additional figure would show how the spatial patterns of nutrient limitation for each phytoplankton group shift between preindustrial and 2100.

The requested figure has been included (new figure 5).

In terms of the experimental design, if I understand, only the dust forcings and atmospheric CO₂ are changing in the transient run. All other meteorological forcings are held constant. This needs to be spelled out explicitly in the methods section.

****Acknowledged and addressed.**

On page 2534 lines 10-12, the authors state that ...the continental shelf is the principal source of Fe to the mixed layer in the large majority of HNLC waters (Tyrrell et al., 2005; Blain et al., 2007; Aumont et al., 2007)... This statement is too broad and not supported by evidence. The first two references refer to HNLC waters directly adjacent to shelf sources, and the third reference is not available.

****The text has been amended appropriately.**

On page 2535, the authors discuss a potential shift towards Fe-limitation for the North Atlantic. More details are needed here, what areas of the North Atlantic? The figure seems to have surface nitrogen in the subtropical gyre, has this region actually shifted to Fe-limitation? What about the high latitude NA? Some field and model studies have suggested Fe-limitation at high latitudes at present. Again, maps of nutrient limitation would be helpful.

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**See our previous comment regarding nutrient limitation maps. Our point relates to a potential role for dust derived Fe in controlling nutrient utilization. Since when dust deposition declines, NO₃ stocks increase. This is suggestive of some degree of Fe regulation in this region (as previously proposed). For reference, Fe limits diatoms and nanophytoplankton for 5 to 6 and 1 to 4 months of the year, respectively, in the North Atlantic (at around 60°N). By 2100, the months where Fe was the most limiting nutrient increased by 2 to 5 and 2 to 3 months for diatoms and nanophytoplankton, respectively (represented in new Figure 5).

Several features of the simulation seem similar to the results from Moore and Doney (2007). Nitrogen fixation is most sensitive to the change in dust in the Pacific basin. There is a clear spatial separation between areas of nitrogen fixation and denitrification in this basin. That is there is little nitrogen fixation above the main zone of denitrification in the ETP. This should give the model the muted linkage discussed by Moore and Doney (2007), as nitrogen fixation has little direct influence on the amount of organic matter falling into the OMZ. The authors seem to acknowledge this in several places, noting that dust also affects the (Fe-limited) production above the OMZ (Page 2540 lines 1-14). This seems to support the findings of Moore and Doney (2007) for the Pacific region, and are counter to the arguments put forward recently by Deutsch et al., (2007). Could some discussion of these issues be added?

**The reviewer makes a very good point and the text has been amended to note the points raised. Our results would indeed suggest that the primary control on denitrification is organic matter export over the OMZ, rather than N₂ fixation and have noted the accord with Moore and Doney (2007). We feel that the Pacific system is coupled (from the concomitant changes in N₂ fixation and denitrification that the model suggests), but that this arises (primarily) from NPP in the ETP, which controls organic matter flux to the OMZ (and thus denitrification rates), as well as the degree of NO₃ utilization in surface waters and at depth (both of which feed back onto N₂ fixation). However, in contrast to Moore and Doney (2007) (who noted that stabilizing feedbacks

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were muted’ in the Pacific), we found strong feedbacks between N₂ fixation and denitrification in Pacific (Table 2). We had already noted the lack of N₂ fixation in the ETP upwelling zone (as stated by Moore and Doney, 2007) and that our overall estimates of N₂ fixation are closer to Moore and Doney (2007) than Deutsch et al. (2007) (section 3.4.1 of our manuscript). More discussion has been included on this topic. Further field and modeling studies will be necessary, with longer model integration times, to more adequately assess the impact of changes in DIN utilization (that arise from both surface NPP in the ETP, as well as subsurface processes) on N₂ fixation and the hypothesis of long term stability in the marine DIN inventory put forward (most recently) by Deutsch et al. (2007).

In the following section, they imply that nitrogen fixation and denitrification are strongly linked and that iron cannot significantly unbalance the N cycle. In figure 3A, it actually appears that over much of the denitrification zone along Central America, NPP actually increases, which should drive up denitrification. It might be preferable to put export production in this plot in place of NPP. It is not really clear to me why there is the, rather small decrease, in denitrification. Could the authors expand on this topic somewhat? Does the sedimentary source dominate Fe inputs in the ETP? I would not expect much of a dust response along the continental margin in this region. Again, a map showing the Fe inputs might shed some light.

****While there is a (very) slight increase in NPP above the Central American denitrification zone, the reviewer is correct in his inference, as export production does indeed decline over the ETP region - thereby retarding denitrification rates. Reduced NPP over the central ETP results in lesser rates of carbon export and oxygen consumption during remineralization declines accordingly. Greater concentrations of oxygen therefore persist and reduce denitrification rates. Indeed, in the southern ETP, there are slight increases in denitrification underneath the zone of increased NPP (Figure 3A and D). The text has been amended to make this clearer.**

On Page 2539, the authors note that at the end of the preindustrial spin-up there was a

positive N imbalance (N fixation > denitrification) of 42.3 TgN/yr. Was such an imbalance maintained over the 3000 year spin-up? How much did total N inventory increase during the simulation (initialized presumably with WOA nitrate)? Excess fixed N in the system would also tend to mute the biogeochemical response to a decline in N fixation.

**As already shown in Table 2, the imbalance declines to 39.6 and 33.4 Tg N/yr by 2000 and 2100 respectively. This reflects a decline in the imbalance of 8.9 Tg N over the 240 years of the study, or a rate of change of 0.038 Tg N yr⁻¹.

Is the near balance in the N cycle the result of a homeostatic DIN inventory or has it been somewhat prescribed by the N fixation parameterization and the explicit link between N fixation and denitrification?

**See above comment that N₂ fixation and denitrification have no explicit linkage in this study.

Moore and Braucher (2007) conducted a simulation with no dust inputs to the oceans, only the sedimentary iron source (implemented in a manner similar to this paper). They found a reduction of export production by 18% and N fixation by 48% after 200 years. This is a more drastic dust reduction, but the results imply a stronger dust sensitivity in that model. Could the authors compare their results, and comment on model differences that might govern the varying sensitivity to dust variations?

**This appears to have been an instant and large reduction in dust deposition, rather than the transient decline simulated during this study. It is therefore not surprising that a higher sensitivity was found. On the other hand, the lesser impact on export production than on N₂ fixation is in accord with our findings (we also found the effect on N₂ fixation to be approximately 2 times the effect on export). A discussion of these points has been included in the text.

In several places the authors refer to changes in oxygen or NPP as driving changes in denitrification. It is more accurate to attribute changes in denitrification to changes in

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export production.

****We acknowledge the reviewers point. We note that the role of npp in dictating export and export production in controlling deep oxygen concentration was included in our summary schematic (new Figure 6).**

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