

Responses to reviews

The reviewers make many insightful comments (in regular type) and we respond to them here and indicate what we propose to do to the text in response in *italics*.

Anonymous Referee #1

General Comments

While the study addresses an important issue, namely the inclusion of interactive desert dust in a coupled climate-carbon cycle model including iron fertilization, the resulting change in dust mobilization and iron deposition in a climate change scenario is fairly small. The authors then artificially increase the variability of the dust module performing a 2x dust and a 0.5x dust experiment to increase the response of the climate system and the carbon cycle. Much of their conclusions then are based on these experiments rather than the interactive dust experiment. The additional sets of experiments (NONEQ, TRAJ, AEROSOL) I find more confusing than illuminating, and generally they decrease the readability of the figures.

The ms. could benefit from more careful phrasing and a better guidance through the figures - often it is more or less left to the reader to decipher the details of the - often too small - figures.

The figures must be improved with regard to readability and contents of the captions. In the state they are, it is difficult to evaluate the ms.

This reviewer makes many extremely helpful suggestions and we try to incorporate them into the manuscript. We try to improve the readability the figures, and the text references to the figures. Since this paper focuses on the global pictures, we think having small sizes, but global view is the correct choice for the figures. It is quite easy in the electronic journal age to increase the size of any individual figure and see more detail, so we think this represents the best compromise. However, some of the figures were needlessly reduced in size by the journal in the publishing effort, and so we will insist that the figures are printed larger.

This model has very little response to the inclusion of aerosols, which is quite different than the only previously published fully coupled-carbon cycle simulations from the Hadley center, so it is worthwhile to see this. The TRAJ and NONEQ simulations are really for the modelers at our and other centers, and analyzing the IPCC simulations, so we understand how the exact way that we formulate and set up the experiments biases our results. These are important to present somewhere, since not previously published, but will not be informative for the casual reader.

The coupling between desert dust and coupled-carbon-climate models has not yet been done, so this is a new coupling, and shows an important response for the ocean biogeochemistry. We try to make sure these new parts of the paper are highlighted.

Specific Comments

p. 6620 | 28 No, see Stier et al., Atmos. Chem. Phys., 6, 3059-3076, 2006. (in particular sec 2.2). But you are the first to use dynamical vegetation and N-limitation in this context.

Stier et al, 2006 has a partially-coupled carbon cycle: with ocean biogeochemistry, but the carbon cycle on the land is not included. There is almost no discussion of the response of the ocean carbon cycle to the aerosols in that paper. The model here does not include dynamic vegetation in it, but rather has a land carbon cycle, with N-limitation. We add a sentence and include a reference to the partially coupled carbon cycle simulation in Stier et al., 2006.

p 6621 | 12 'The estimates range from changes of +/- 60% ...'? do the estimates range from -

60% to +60%? rephrase.

We rephrase with more details.

p 6622 l 15 'iron deposition in dust' 'from' dust? deposition of iron in dust?

Either is good English, it depends on which one prefers.

p 6623 l 25ff The description of the model experiments is quite confusing: First it is stated that these are grouped into three types, then 'cases' are introduced, but case is simultaneously used for groups of experiments (like 'BASE') and individual experiments ('case names' in Tab.1) and control vs. transient (p 6624 l10/11). It would be helpful to add the case names (BASE, NONEQ, etc) after the section headings 2.2.1 to 2.2.4

This suggestion is very helpful to improve the readability: we distinguish sets of cases from individual cases to make this more clear. We add in the sets of case names in the section headings.

p 6624 l 25 correct 'land, atmosphere and atmosphere....'

We correct this typo.

p 6625 l19 a uniform global mean CO2 'distribution' → value which is set to the same as the BASE1 simulation → which is taken from the BASE1 simulation?

We replace "which is the same as the BASE1 simulation" with "which is set to the same as the calculated globally averaged value from the BASE1 simulation"

p 6628 l 15 if you add 'with units of ppm/AOD' after 'carbon dioxide', it becomes clearer why the epsilon is within the paranthesis in eq. 8.

We add in this.

p6629 l 15 net uptake of 'anthropogenic' CO2?

Yes, we clarify this.

l 22/23 clarify this sentence ('response of the CCCM to the sensitivity studies and aerosols included in the different cases' is unclear)

We add in 'temperature, precipitation and carbon ' to make it more clear what we will concentrate on.

p 6630 l l2 make clear that 'This' refers to the model trajectory of (atmospheric) CO2 on

p6629 l 24 (and not to the surface temperature from the previous sentence)

We replace "this" with "The modeled co2 trajectory"

l 27 l presume the ref to Fig2. refers to the sentence starting at line 25, not to the cooling of the atmosphere. (But 2x Dust is not shown in Fig. 2, so maybe the ref to Fig.2 just should not be there.

Yes, the reference to figure 2 is wrong in this sentence, so we remove it.

p 6631 l 9 'Denitrification tends to decrease in the future due to climate change,....' If oxygen is depleted in a warmer, less well mixed ocean I would expect denitrification to increase, unless there is a significant decrease in export production. Can you give a reference for your statement?

This is from our model simulations: we make this more clear.

l 13 If adding interactive dust results in an increase in dust deposition (that would likely be the yellow line in Fig 3b, but it is impossible to see if that's above or below zero, or the yellow bar in Fig4a, slightly positive) then I do not understand why an increase in N-fixation would cause a stronger decrease in marine productivity than without dust. Likewise I do not understand how you derive the sentence starting at line 15 (Thus, for nitrogen fixation and productivity changes including aerosols is more important than simulating increasing CO2)

The changes in dust when dust is interactive is very small, which is why the reviewer can't see the changes. We try to note this.

The change in Nfixation and related changes in productivity are discussed in more detail in the latter section: this has to do with regional budgets. We make this point more clear here.

Finally we expand the statement about including aerosols and resulting changes in productivity, since the second reviewer also did not see this relationship clearly.

I 21 'The temporal evolution of ... illustrate(s) that these increase at first with higher dust deposition, but then productivity starts going down (Fig.5)' - from fig 5, productivity reaches its peak around 1900, when the dust source factor of the 2x DUST experiment is only around 1.17. I have difficulties to see a dust-triggered signal here, it looks more like random variability. Also, the control experiments seem to have a similar temporal behaviour as the transient experiments - so what are we looking at?(again, the figure is barely readable) *It is unfortunate that ACPD chose to make this figure so small: we will try to get them to enlarge it in future versions. As discussed later (Section 3.6), this is because dust fertilizes some areas with iron, and draws down the P, so that upwelling in other regions has less P: limiting their productivity. In addition, the aerosols change the ocean mixed layer depths and cause a decrease in nutrient upwelling. We hope this discussion is more clear in the regional discussion (3.6), and refer to this more clearly in the text.*

p 6632 I 13 Does 'aerosol cases' here mean DUST and AEROSOL experiments? Or only the DUST experiment?

It means the prognostic aerosol cases (DUST,AEROSOL): we make this more clear by adding in this information.

I 28 Fig. 7a shows the radiative forcing of non CO2 GHGases, not the climate sensitivity discussed in the preceding sentence

This is correct: we clarify that our estimates of the forcings are shown in figure 7a (which were used to back out the resulting changes in climate sensitivity).

p 6634 I 6 In this para it is only mentioned that 'there are' statistically significant changes in predictions of the surface temperature, without any hint on the significance criteria or the size of the change. This should be added or the para dropped (and Fig. 8)

The significance criteria were indicated in the figure captions: we add them into the text in this sentence.

I 10 As for para above and Fig. 9

The statistical significant criteria are included in the figure 8 figure caption, and we indicate that figure 9 is as in figure 8: we included them once in the text, in response to the above statement: this should be sufficient.

I 14 The sentences starting here are unclear (and hard to relate to the again too small figure 10). what you show is flux at the end of the run minus flux at the beginning of the run. Does a negative value here mean that the flux is reduced or increased? It also seems that also much of the high latitudes are 'blue', i.e., like in the tropics.

We make the figures larger, and will insist that the journal makes the figure larger. We add information into the figure caption, indicating that carbon fluxes are positive upward. So a negative value here means that 2080-2099 has lower fluxes of carbon into the atmosphere than in preindustrial. We include this also in text.

I 22 What is the ref to Moore et al., 2006 implying here? Aren't the 2xDUST and 0.5xDUST experiments made for this study?

Yes, this reference is not well explained. We add in 'similar to'.

p 6635 I 20 '...shows a similar to the BASE case...' makes no sense

We rewrite this statement to make sense.

p 6636 I 27 '...the model predicts a large increase in precipitation, precipitation minus evaporation, and a' but Fig. 14b shows a decrease of P-E, please clarify

This is true: we clarify the text.

p 6637 l 5 the source strength is not responding ... (not are)

We fix.

l 24 what is meant by (similar to what was seen in Fig. 8)? Fig 8 shows SST

We fix this reference (should be to Figure 10).

p 6638 l 1 I cannot follow the reasoning here. if 2xDUST and 0.5xDUST are compared (a factor of 4, not 2) Fig.14d implies that the dust mobilization in South America is little changed in 2xDUST relative to PI (~-20%), but strongly changed for 0.5xDUST (70%). Deposition in SA +10% and -60%, respectively (Fig.14e). Surely not constant dust? Then, what do the relatively small changes in Chl in Fig. 15d imply? Did you mean to compare 2xDUST and PI?

This sentence is just too confusing: we eliminate it.

l 12 correct sentence ...have a slightly lower the mixed layer depth... l 15 Regions that are thought to be iron limited have an increase in productivity... l 17 Some regions that are thought to be iron limited show less decline with....

Maybe change l15 to 'Some' regions in productivity 'even if dust deposition is reduced (red line in Fig. 16. f-h) while some of those regions show a decline (e.g., Fig. 16c) which is reduced with higher dust input. [this seems to imply that other reasons than dust input trigger the changes - check l 24/25]

We try to rewrite these sentences to make them clearer. The other reviewer also wanted an expansion of this area, so we add more details and analysis.

l 24 'changes in ocean productivity which are as large as changes in climate' makes no sense

We clarify

p 6640 l 5 'The sensitivity studies conducted in this model' make no sense

We clarify

l 9 carbon 'cycle'? 'budget'?

We clarify: we are talking about the co2 value seen by the land and ocean surface and whether it is a globally average value or that advected by the atmosphere (which can contain gradients).

l 11 see ref to Stier et al above

Again, Stier et al is not a fully coupled-carbon simulation (no land carbon), so it does not count: there are many partially coupled simulations we do not cite here.

l 17 correct 'has a slightly negatively sensitive to climate'

We correct

p 6641 l 7 Inclusion of realistic desert dust does significantly impact....

if one compares the black and yellow bars in Fig4, the delta is smaller than the black bars, so I have difficulties to see

l11 why 'changes in desert dust are as important or more important as changes in (atmospheric) carbon dioxide'

Here we are talking about productivity and nitrogen fixation, not co2 uptake: we clarify this and the expansion of the calculations in section 3.1 should support our statements.

p 6649 Tab.1 the use of 'case' is confusing p 6652 Fig.1 (b) is missing in caption, labels too small, cases -> experiments

We try to use 'sets of cases' and case more consistently: case is an individual run.

p 6653 Fig.2 minus sign is missing for -10, -15, change e.g. to i.e.

We fix the minus signs. We do not understand the e.g. to i.e. comment.

p 6654 Fig.3 figure and labels too small, Fig. not readable

It would be difficult to rescale the figure and still get the information across. We think that the problem is in production: we will insist that the figure is published larger. If full paper size, this figure is fine.

p 6656 Fig.5 labels too small

We increase the size on the labels.

p 6657 Fig.6 labels too small, units missing

We revise the labels to be larger.

p 6658 Fig.7 labels too small, (c) missing in caption

We fix the figure

p 6659 Fig.8 base in capitals helpful, 95% 'level', labels on color bar have funny values (also other figs.)

We write the BASE in capitals, and revise the color bar.

p 6661 Fig10 does <0 mean reduced or increased uptake?

We clarify in text and figure caption.

p 6662 Fig11 95% 'level', Notice that cases(!) (b),(c),(e), and (f) -> note that panels (b-d) show differences.... (also Fig A5) units?

We clarify and make changes suggested.

p 6663 Fig12 perhaps add pos= release of stress, neg= increasing stress

We add this clarification to the figure caption.

p 6664 Fig13 change colors to colored lines

we make this change to the figure caption.

p 6665 Fig14 figure and labels too small, caption not consistent with figure (a,b) vs. a-e

We fix the labels: we can only insist that the journal publish as large as possible. These fit fine on a normal page.

p 6667 Fig16 Fig and labels too small, regions as in Fig 14e, note different scaling for different panels

We clarify the caption and make the labels larger.

p 6668 Fig17 from the simulations presented here (colored and solid black line) or simply 'solid lines'mean: dashed black, various models: dotted black (I think this should read)

We fix following suggestions.

p 6669 Fig A1 labels much too small

We fix the labels in A1 and A2.

p 6671 Fig A3 correct caption to 1980-1999, base->BASE, 95% level, DUST2080-2099 -> DUST2030-2049

we correct.

BASE2080-2099 -> BASE2030-2049

p 6673 Fig A5 check 'first 20 years' against 1870-1899 (30 years)

we fix.

Technical errors spell greenhouse gases consistently (greenhouse, green house both used)

p 6620 l 5 & l 23 correct to: Friedlingstein et al p 6627 l 16 Friedlingstein et al. (2006)

show(s) [delete s] p 6629 l 21 correct to: Globally averaged response p 6630 l 7 simulation

...BASE1 has (just one) p 6634 l 7 change 'of' regional l22 show(s) (add s, the comparison shows) p 6635 l 12 base -> BASE l24Fig. 8->Fig. 9 p 6650 Tab. 1 , DUST experiments, aerosols pre(s)cribed [add s] p 6670 Fig A2 correct 'base- don'

We fix these technical errors and thank the reviewer for finding them.

Anonymous Referee #2

General comments

This study addresses the impact of changing dust and anthropogenic aerosols on the climate-carbon retroactions in the 21st century. The authors make use of simulations with a coupled carbon cycle – climate model in which the dust cycle is fully interactive. The main result is that the inclusion of aerosols does not impact significantly the global average carbon cycle and the magnitude of the carbon-climate feedback. The authors attribute this weak response to the weak carbon- climate feedback of the model used. They show however

significant responses of the ocean biogeochemistry to dust deposition changes.

This is, to my knowledge, the first study in which both the dust cycle and the carbon cycle are fully included in a coupled climate model. The results are interesting but would benefit from a more careful and detailed description and analysis. I detail hereafter a few major and specific comments.

We thank the reviewer for their detailed comments and incorporate their suggestions into the text.

1. On the main conclusion. The authors conclude that “changes in desert dust significantly impact productivity and the nitrogen cycle, and are as important or more important as changes in CO₂ and the resulting climate changes.” This is fairly strong, but should be put into the context of the simulations that have been realized. In the “dust case”, where dust cycle is fully interactive, the changes to productivity due to dust are fairly small. It is only in the sensitivity experiments (dust *2, and dust*0.5) that dust impact seems to overcome, in some regions, the direct impact of climate change. This conclusion has to be changed (although in the abstract).

The inclusion of interactive aerosols and dust is just as important as co₂ to influencing ocean nitrogen cycle and productivity, although both reviewers had trouble seeing this. So we extract more details into the text to support our statement: adding in an additional paragraph of discussion here.

2. On the review of literature. The manuscript clearly lacks references to key publications for some of the points that are developed. On the inclusion of aerosols in coupled carbon cycle models, Cadule et al. (2009, GRL) also analyse coupled climate-carbon cycle simulations with and without aerosols and compare their results to Jones et al. (2003). On the impact of changes in dust on marine biogeochemistry, many mechanisms that are present in these simulations have already been discussed elsewhere, and in more details (Parekh et al. 2006 GRL, Tagliabue et al. 2008 BG). The authors need to put their results into that context.

We add references to these additional papers and thank the reviewer for pointing these out. The Cadule et al., GRL paper does not actually show an aerosol-coupled-carbon-climate simulation, but rather cite a paper submitted to PNAS. The lead author contacted the authors of the GRL paper and found out that this paper is still under review at PNAS, and was not able to get a copy of the PNAS paper. So all that is available is the ‘gains’ from the GRL paper, which we include in our paper. We highlight the differences between our approach and the Cadule et al paper.

3. On the analysis. I have to admit that I have been a little frustrated at the reading of the manuscript. Some of the more interesting and new results are not detailed enough: for instance, what is the relative contribution of dust-induced climate changes and dust- direct iron deposition changes on marine biogeochemistry? This is new and could have been more detailed.

We add more details on this: however determining exactly why there is a physical response that is so strong is not easy in a coupled simulations: that requires an offline ocean simulation, which we hope to do in the future. We add a figure showing changes in mixed layer depth for comparison, which we previously mentioned as not shown.

More specific comments.

P6620 Line10 : “much reduced terrestrial carbon uptake, and a reduced climate feedback onto carbon” . . . All the models results that are listed here do not show reduced feedback when the land N-cycle is taken into account, but even may show accelerated warming (for instance Zaehle et al. GRL 2010).

The Zaehle et al study shows a reduction in the climate impact onto the land carbon cycle

From that paper's abstract "Conversely, N dynamics reduce projected losses of land C due to increasing temperature by 16% (49 Pg C);" and is seen in the higher gamma factor (less negative). The higher C in the atmosphere in the Zaehle model is from reduced co2 fertilization: that is a separate issue mentioned at the first part of the sentence (reduced C uptake on land), which is also seen in our model.

P6620 Line 18 : "Sulfate and volcanic aerosols have been implemented in one coupled carbon cycle model. . . (Jones et al. 2003)". Please check Cadule et al. 2009 GRL, where a comparison to Jones et al. 2003 is already presented.

Again the Cadule et al., GRL paper does not show any coupled climate results: that is in the PNAS submitted paper. The Cadule paper develops a new 'simple' modeling approach different than our approach, so we evaluate it there.

P6622 Line 8 : "the model includes N-limitation of plant growth". Change to N- limitation of plant growth.

We change.

P6625 Desert dust experiments: It is not clear from one of previous paragraphs if the ocean biogeochemical model used in these experiments is different or not from the one used in the BASECASE scenario. Is sediment iron input taken into account in all simulations? If this is not the case, explain how it would interact / interfere with the results presented here.

We add more details to the description of the ocean biogeochemistry and how it would impact the results here.

P6626 Simple feedback analysis. I'm not convinced that all the equations have to be repeated here.

Yes it is quite boring, but because we extend the math, it does seem required. It is in the methods section, so it's less jarring to the reader than in a results section.

P6631. Impact of dust changes onto N-fixation / Denitrification. Please compare to previous results (Parekh et al., Tagliabue et al., . . .).

We add in more discussion of other previous studies of the impact of dust changes onto ocean biogeochemistry, such as the paper suggested by the reviewer.

P6631. "Thus for productivity changes, including aerosols is more important than simulating increasing CO2". This not clear and is contradicted by previous sentences.

We add in details in Section 3.2 which make this point more clear.

P6631. Direct Effect of rising CO2. Please cite Oschlies 2009, in which it seems to be first-order effect.

We will add a citation to Oschlies 2009, although Oschlies 2009 isn't directly relevant to the issue we are talking about, since it speaks to only ocean-only simulations, and suggests that having feedbacks onto the co2 from them changes the co2 cycle of the ocean: this is extremely important for ocean only experiments. Here, we are talking about the fact that several of the fully coupled simulations (e.g. IPSL, Hadley) do not keep the spatial distribution of co2 (but only a global average) and do not allow the spatial distribution to interact with the carbon cycle, while CCSM advects the CO2 and allows it to develop spatial distributions that interact with the carbon cycle. This is especially important for the land models, since that is where the global average deviates the most from the local value: for co2 fertilization issues this maybe important. In this simulation we show that this is not important for our simulations. Our co2 fertilization is on the low side, so it doesn't disprove that it wouldn't be important for their simulations, but does suggest it isn't the source of substantial bias. We add more text to clarify this point and add the citation. We put this text into the methods section, not the conclusions, to keep the flow of the conclusions. One would expect that keeping the spatial distribution would be of much smaller importance than changing the global average, so it seems clear that our answer could be quite different than Oeschlies 2009.

P6637. Aerosols feedback onto the ocean. Could you be clearer on the physical mechanisms?

We add more discussion on the issue of the physical forcing of the ocean biogeochemistry, as it is brought up by both reviewers. We do not understand the physical mechanism, but merely point to a possible additional feedback that should be considered in more detail in further studies.