We would like to express our sincere thanks to both referees for their reviews, and their helpful suggestions. Below are our detailed, point-by-point replies to both referees.

Reply to referee 1

We thank Referee 1 for his/her helpful comments. Below is our detailed reply to the reviewer's suggestions. (Reviewer's comments in green italics.)

The paper tests a few very specific uncertainties: the vertical distribution of particulate remineralization, and the half-saturation constants of O2 and NO3 in remineralization. The sensitivity of the model to the remineralization half-saturation constants is particularly useful. However, these targets of investigation are somewhat buried in the article - they should be clearly stated in the abstract and introduction, and perhaps even the title.

We will do our best to rewrite the revised manuscript in a clearer, more accessible style. In particular, we will better emphasize the sensitivity of remineralization constants .

A couple of other complexities - such as anammox - are discussed in the paper, which is helpful, but are not a focus since there are no experiments to explore them.

Indeed there are no experiments dedicated specifically to the distinction between anammox and denitrification. We have left this out, because there are only few in situ observation to constrain these processes; further, many of them were carried out on or near the continental shelves, for which the model shows a poor resolution; finally, we had hoped to show via the stoichiometric analysis in the appendix that these are exchangable (in the light of current model resolution). We will try to make this clearer in a revised version of the manuscript. We prefer to keep the discussion of anammox, also motivated by the comments made by reviewer 2 on this topic.

At the same time, some other processes are left out entirely, such as the iron cycle, benthic denitrification, and variability in organic matter stoichiometry. That's fine, but in view of the specificity of the uncertainties tested, the title and opening of the paper seem unnecessarily vague about what the paper is actually going to address ('marine biogeochemical processes').
Along with this, it would be great to include some reasoning for why these specific uncertainties were chosen for this study, and to better highlight the results.
It would also be helpful to have some motivating statements about the model. Why was MOPS made? How does it compare to similar models, and what niche does it fill? Who should be rushing to download and test this model?

MOPS-1.0 is meant to serve as a first step of a model that resolves different elemental cycles, together with explicit consideration of oxidant affinity, a consistent stoichiometry, and a broad scan of the parameter space (i.e., some knowledge about the specific model sensitivities). We have chosen the exploration of remineralization half-saturation constants for this first step, because these - due the the very few observational studies available (see paper) - to our opinion are the most weakly defined parameterizations, in current models. More elements and processes - in particular, the iron cycle and a more detailed benthic-pelagic exchange including benthic denitrification - will be added and evaluated in follow-on versions of the model, as will be explained better in the revised manuscript. This increase in structural complexity will be done in a stepwise manner, examining the - possibly changed - sensitivities at every stage. By investigating explicitly remineralization,

sinking speed, and oxidant-dependency of remineralization in a global setting, and by calibrating the model against the background of many different data-sets, we think that MOPS and the current study is unique among global biogeochemical models that operate on time scales long enough to reach steady state.
We agree, however, that the current title may be misleading in this respect, and propose to change it to **"MOPS-1.0: Towards a model for the regulation of the global nitrogen budget by marine biogeochemical processes"**. We will also try to express our motivation, reasoning and explain our future directions and aims, and the current model restrictions, more clearly.

Benthic denitrification is not included in the model, even though it probably accounts for at least half of the total fixed nitrogen lost. I didn't see a scientific reason for not including it, it seems to be more because it simply hasn't been coded - the reason should be clarified on page 1949. I don't think its absence is a big problem, but it should be pointed out as a caveat in a couple of places where it will certainly impact the results. For example, the global distribution of nitrate concentrations would definitely be altered by the inclusion of benthic denitrification, given its different horizontal and vertical distribution relative to pelagic denitrification, and significant contribution to overall N loss. I would expect the nitrate concentrations to decrease in most of the ocean if this missing process were included, shifting most of the volume frequencies to lower concentrations. The conclusions based on nitrate concentration comparison with data should be reworded slightly in order to reflect this fact. The use of constant N:P stoichiometry should also be mentioned as a caveat, given that it has been shown to be important globally (e.g. Weber and Deutsch, 2012).

We will add some more in depth discussion about the restrictions of this study in a revised version of the manuscript.

The paper talks a lot about 'particle sinking speeds'. However, the parameter b re- flects both sinking speed and remineralization rate. Thus, the results can be viewed equivalently as sensitivity tests of remineralization rate, just as much as sinking rate.

We agree, and - in addition to our references to Kriest and Oschlies, 2009, where we have investigated this in more detail - will discuss this in more detail in a revised version. We note, however, that e.g. in the presence of strong currents, remineralization and sinking speed are disentangled, and the effects of changing either of these may not be exactly the same.

– Specific comments –

Abstract The abstract uses a lot of vague language. It would be more helpful to make it more specifically focused on the methods used and the results.

We will change the abstract in the revised version based on sugggestions made by Reviewer 1 above, in particular focus more explictly on the detailed results of our study.

p. 1950: '... thereby parameterizing some form of "implicit denitrification" without explicitly accounting for other oxidants beside oxygen...' I don't think it's fair to say it's implicit denitrification, since this would imply a change in nitrate limitation. Better to say 'implicit non-oxygen oxidants'.

We agree, and will change this in the revised version of the manuscript.

p. 1952: I don't understand how a relaxing of NO3 towards an N:P of 16:1 is 'based on' Breitbarth & Laroche. Also, the authors in this reference are listed backwards throughout.

We are sorry that we expressed this parameterization in a misleading way. What we meant to say is that the regulation of nitrogen fixation by temperature has been parameterized acc. to Breitbarth and LaRoche. We will rephrase this accordingly in the revised version.

p.1967: The idea that the final state could depend on initial conditions has not been previously introduced in the paper. Why would anyone expect multiple equilibria in this model? There must be some literature on the factors that produce multiple equilibria that could be cited somewhere?

By adding a nitrogen cycle to the prior phosphorus-based model, non-linear switches in oxidants are introduced. Though it has, to our knowledge, never been shown that marine biogeochemical models can exhibit multiple steady state, other components of the Earth system, such as ocean circulation, atmospheric circulation, land ice and terrestrial vegetation all have been found capable of displaying situations with multiple steady states. We thus wanted to test whether multiple steady states may also exist for this relatively simple marine biogeochemical model that, despite its structural simplicity, contains non-linear processes that to our knowledge cannot be ruled out to lead to hysteresis and multiple steady states.

Section 4.4 doesn't seem to add much to the paper - I think it could be removed.

We would prefer to keep this section, because recently there has been some discussion about the representation of physics in that region, and the impact it may have on biogeochemical tracers. We would like to indicate, that we are aware of this discussion, and that some mismatches in the model may be related to insufficient physics.

Appendix

- *How does the matrix deal with physical mixing within the mixed layer? A couple of sentences about this would be nice, given the importance of mixed layer dynamics for biogeochemistry.*

We will add a short paragraph on the TMM and the underlying assumptions and mechanisms in a revised version (see also comments by reviewer 2).

- Temporal discretization should be described. Is the circulation annual mean? What are the timesteps?

See above. We will add a short paragraph on the TMM and the underlying assumptions, including time stepping in a revised version (see also comments by reviewer 2). In short, we are using monthly mean transport matrices, with linear interpolation between these. The physical transport is then time stepped with 2 time steps per day. Within each ocean time step, we time step the biogeochemical model with 8 time steps, resulting in 16 biogeochemical time steps per day (i.e., Delta T = 5400 sec or 90 min).

Reply to referee 2

We thank Referee 2 for his/her helpful and encouraging comments. Below is our detailed reply to the reviewer's suggestions. (Reviewer's comments in green italics.)

Summary:

The manuscript presents the extension of an existing marine biogeochemistry model grounded on the phosphorus cycle, to one that includes the nitrogen cycle and pro- cesses such as denitrification and nitrogen fixation. The resulting model is examined within the framework of the so-called Transport Matrix Method, a computationally- efficient offline mode that readily permits the simulation of time periods appropriate for examining equilibrium states (and the role of different processes in reaching these states). As part of the manuscript's sensitivity analysis, assumptions going into the revised model – including stoichiometry, substrate affinities, temperature dependence and detrital sinking – are examined to determine their role for realism and performance.

Among other conclusions, the model lends support to lower observed estimates of denitrification, and its investigation favours a classic remineralisation profile of sinking material.

Overview:

Overall, the paper is a solid piece of work that builds well on previous modelling work by the same authors. It is thorough in its exploration of the various assumptions that frame its new nitrogen cycle, giving the reader confidence in the resulting understanding provided by the model. I do not have any show-stopping concerns or criticisms of the manuscript. I do, however, have a short list of specific additions / changes that I think would strengthen the manuscript or give it a broader appeal, plus a longer list of more minor comments or queries. The former are included below, the latter are pre- sented as in-text modifications to the GMDD draft. I do not see any of these as critical, but would ask that the authors consider them. My recommendation is publication after minor revision.

Specific comments:

For a GMD manuscript, I find the expulsion of model description to an appendix a strange decision. It would make more sense for the model equations, etc., to move from the appendix to the model description section in the main body. Not least because it would obviate the need to keep skipping forwards to the appendix to properly understand the context of the text currently in the description section.

We will change the manuscript structure accordingly, and move the model description to the main part of the paper.

A paragraph summarising the basic concepts involved in the TMM would be extremely helpful. Nothing much, just an outline so that readers aren't obliged to consult other manuscripts to get a basic idea of what's going on.

We will do so in the revised version of the manuscript.

I would like to see a short section describing the alternative approaches taken by other contemporary models for dealing with the aspects of the nitrogen cycle considered by the manuscript (e.g. denitrification and nitrogen fixation). This is already done ina piecemeal way through the manuscript, but it would be better (to my mind) if there were a specific subsection devoted to it. In the GMD framework, it's important that a new model is *contextualised in this way so that readers can better judge what it brings to the table. This could include, for instance, noting where (why?) other models neglect such processes.*

We will try to do so in a more focused manner in the revised manuscript, and present this work in the context of previous models.

Please also note the supplement to this comment: http://www.geosci-model-dev-discuss.net/8/C936/2015/gmdd-8-C936-2015- supplement.pdf

Please see our supplement (PDF), in which we have added our replies to to reviewer comments (denoted by IK-xx in the corresponding boxes).