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Interactive comment on “The radiative forcing potential of different climate geoengineering options” by T. M. Lenton and N. E. Vaughan

T. M. Lenton and N. E. Vaughan

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We thank the referee for their supportive remarks 1-5.

Responses to requests for moderately important modifications:

6. We agree with the need to briefly discuss the implications of both (a) seasonal latitudinal variation of insolation and (b) the contrast of surface albedo between land and sea (see detailed responses to referee 2). Briefly, in response to (a), we adopt a solar zenith angle of 60° in our global average approach, but propose to include discussion of the effects of changing solar zenith angle, in particular on the relationship between surface albedo and planetary albedo, due to changing the amount of atmospheric absorption as well as changing the surface albedo itself. Factor (b) is important in estimating the effects of changes in the amount of back reflection from the Earth's

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surface when changing cloud albedo or stratospheric aerosols above. We will correct for the lower surface albedo of the ocean underlying marine stratocumulus clouds in our estimate of the effect of changing their albedo (see response to referee 2). (However, we note that when estimating the effects of changes in land surface albedo, only the change in albedo, not its absolute value, matters for radiative forcing.)

7. We apologise for using the same symbol ‘C’ for quantities in different units. We will correct this by using ‘CO₂’ for atmospheric CO₂ (in ppm) and clarify its units in the text.

8. Section 3.2.4: Phosphorus addition: we agree that the original discussion of phosphate fertilisation was somewhat inconsistently uncritical. The assumptions of 100% conversion to organic carbon with a Redfield ratio of C:P = 106 were both questionable, although in opposite directions. The estimated sink of 0.34 PgC yr⁻¹ at present is similar to other estimates (Mackenzie et al., 2002). However, the statement that it would be “mostly in coastal and shelf sea sediments” we now think is questionable based on reading the literature, especially for the North Sea region. The phosphorus budget of the North Sea is dominated by exchange with the open North Atlantic, with recent estimates giving a net export of P to the open ocean (Brion et al., 2004). There is an estimated sediment burial flux of P that is around 20% of the estimated input from estuaries plus direct (mostly sewage) additions (Brion et al., 2004). Many other coastal regions also have rapid exchange with the open ocean. This suggests 100% conversion to organic carbon in coastal and shelf sea sediments is unlikely. The referee is also right to point out that organic matter in shallow waters is extensively and rapidly remineralised, including from sediments. Interestingly, however, phosphorus is preferentially recycled from shallow sediments (particularly under anoxic conditions) and this can support more new production and lead to higher C:P burial ratios. This preferential recycling is particularly important within relatively isolated basins such as the Mediterranean, e.g. the Adriatic Sea (Ogrinc and Faganeli, 2006). A minimum C:P burial ratio of 250 (Lenton and Watson, 2000a) is more appropriate than our originally assumed Redfield ratio of 106, and values can range up into the 1000s (Ingall and

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Jahnke, 1994). Assuming 20% of anthropogenic P inputs to coastal regions are converted to organic carbon in sediments with a C:P burial ratio of 250 we obtain a revised global estimate of 0.16 PgC yr⁻¹ being sequestered at present.

What happens to the remaining 80% of anthropogenic P inputs? Assuming they end up in the open ocean, the question becomes how much can they increase the global sequestration flux below 500 m (upper limit 5.5 PgC yr⁻¹)? If we assume all this P goes to produce export production and (following our rationale for nitrogen fertilisation) we assume that the long term sequestration flux is half the export production flux then we obtain a carbon sink of 0.14 PgC yr⁻¹ at present.

Thus, our revised estimate for maximum total C sequestration due to P addition is 0.30 PgC yr⁻¹ at present, which is not much below the 0.34 PgC yr⁻¹ originally estimated. If instead we assumed all anthropogenic P loading ends up in the open ocean (none is buried in coastal and shelf sea sediments) then the estimate becomes 0.17 PgC yr⁻¹ at present. In revising the paper we will give this range and revise the future estimates downwards, according to the new upper limit.

9. Sections 3.2.4-3.2.6: We are aware that carbon fixed in the ocean comes from DIC in the water and only drives an atmospheric sink insofar as the DIC deficit drives an air-sea flux of CO₂. This was one reason why we focused on changes in carbon export below the depth of winter mixing as anything remineralising above this will not create a DIC deficit on the annual or longer timescale and hence not drive a CO₂ sink. We propose to add a short subsection before 3.2.4-3.2.6 discussing this and grouping together other generic issues pertaining to all the approaches to ocean nutrient fertilisation.

10. We agree that relieving a deficit of one nutrient is likely to lead to limitation by another and that silicate limitation is a possibility, especially for diatoms. We propose to add discussion of this in the new subsection prior to 3.2.4.

11. We acknowledge the referee's concerns about the ordering statements on p. 2589 and p. 2591. We found one significant error in Table 2 (see separate Author Comment)

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and are redoing Table 1 estimates in response to referee 2. We also agree that there are significant error bars on all the estimates - typically of order a factor of 2 in each direction - and we will include some indication of those in revising the paper. We like the suggestion of a figure to replace the ordering statements and have drafted versions on log and linear scales. We propose to replace the ordering statements with a figure using a log scale for radiative forcing.

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Responses to requests for minor modifications:

12. We agree and will adjust to “current and potential future radiative imbalance” throughout.
13. We agree that with hindsight our description of “significant errors in existing calculations” was a bit harsh, especially given the simplicity of our own approach. We propose to remove “Already it reveals some significant errors in existing calculations” from the Abstract and to generally tone down or remove comments elsewhere (see response to referee 2) and let the reader decide.
14. We agree with changing “favoured” to “widely discussed”.
15. We agree that Klaus Lackner should be cited in relation to air capture and storage and will cite his original paper on this (Lackner et al., 1995).
16. We agree with changing “poorly” to “not precisely”.
17. We agree and will add a sentence saying “This is a modest amount when compared with anthropogenic emissions of 55–68 TgS yr⁻¹ (in 2000) from burning fossil fuels (Smith et al., 2001; Stern, 2005)”.
18. We have been in correspondence with Andy Ridgwell and our statement that the actual canopy albedo change in their model runs is less than the prescribed maximum 0.08 is correct and helps explain why our estimated RF is higher than the one derived from their study (Ridgwell et al., 2009). In addition we have learnt that snow cover in their model effectively damps out imposed changes in canopy albedo in some key

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regions (because the canopy is covered in snow for a significant part of the year). Also, changes in cloud cover in their model tend, on average, to counteract imposed changes in surface albedo. In revising the paper we will rewrite the unclear sentence and compare RF estimates for increasing cropland albedo to the estimated RF due to historical land use change of -0.24 W m⁻² (Betts et al., 2007).

19. Apologies for this journalistic language. We will replace “whopping” with “massive” and replace “knocked out” with “eliminated” throughout.

20. Section 3.2.7: We will add a sentence explaining the problem of upward mixing of high DIC water and reference the suggested letter (Shepherd et al., 2007).

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