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Atmospheric Chemistry and Physics Discussions



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

**Anonymous Referee #1**

Received and published: 31 March 2009

Referee’s report: Lenton & Vaughan, ACPD (2009)

Summary Recommendation

Publish with fairly minor amendments

General Comments

1. This is a very useful and quite ambitious contribution to the literature on this very important (and quite controversial) subject. It explicitly aims to give only a broad-brush and first-order approximate assessment of the various geo-engineering options considered, and does so using rather simplified methods. This is (in my opinion) helpful rather than otherwise, as it makes the process usefully transparent. There are in any case major uncertainties in the scope and magnitude of most of the methods proposed,



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and an order of magnitude assessment at this stage is highly appropriate.

2. The paper is ambitious since it attempts a rather difficult task, i.e. to assess both albedo management and CO2 capture schemes using a common metric (net radiative forcing). This involves use of a simple (but well established) model of the retention of CO2 in the atmosphere, and some assumptions about the future trajectory of emissions and hence CO2 concentrations. The assumptions made are reasonable, and the authors briefly discuss the sensitivity of their results to possible alternatives.

3. The analysis discovers an unexpected and rather useful cancellation of effects (pp 2573/4) in that the non-linear effects of CO2 additions/removals on radiative forcing, and on the residual fraction of CO2 left in the atmosphere actually cancel to a rather good approximation, so that a simple rule-of-thumb can be deduced (1000 PgC added or removed corresponds to about 1.5 W/m2 of radiative forcing on a centennial timescale).

4. The analysis also treats in a simple fashion the non-additivity of albedo changes (especially at different altitudes) and thereby exposes and clarifies some discrepancies in previous estimates.

5. The discussion and conclusions are (except as noted below) sound.

Moderately important modifications needed

6. Although reference is made to spatial/geographical effects, the analysis is fundamentally for the globally averaged situation. This should be stressed and the implications discussed briefly (e.g. the effects of the non-uniform and seasonal latitudinal variation of insolation, and of the contrast of surface albedo between land and sea, e.g. on p2567 et seq)

7. On p2569 the symbol C is used for atmospheric CO2 concentration (in ppmv), and on p2570 the same symbol is used for Carbon inventory (in GtC). There is (as stated) a simple proportionality between these, and the constant (k) is given, but the use of



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the same symbol makes the discussion unnecessarily confusing, since C and C(t) are referred to in the text without qualification.

8. On p2583 (et seq) the discussion of phosphate fertilisation seems to me to be unhelpfully (and inconsistently) uncritical, insofar as it assumes that all of the (Redfield-equivalent) carbon fixed is sequestered, presumably in shelf-sea sediments. In practice (and as discussed for nitrogen and iron fertilisation) only the fraction exported is properly sequestered, and since there is also extensive and rapid remineralisation of organic matter in shallow waters, this should be allowed for. This will reduce the efficacy of P fertilisation considerably.

9. Throughout sections 3.2.4 to 3.2.6 it seems to be assumed that any carbon fixed comes directly from the atmosphere. In fact, in the first place it will come from the pool of DIC in ocean waters (where DIC is present in circa ten-fold excess relative to P & N), and will then come from the atmosphere only insofar as the increased deficit of DIC in surface waters creates an extra air-sea flux leading to a new equilibrium at lower sea surface/atmospheric pCO2, with partial replenishment of the surface DIC. For example, even if ocean surface winter macronutrient concentrations could be enhanced (and maintained) globally by 50%, this would only lead to a drawdown of about 100 ppmv of CO2, i.e about 200 PgC, eventually, and once only. Some discussion of this issue would be in order.

10. A clear statement that relieving a deficit of one nutrient is likely to lead to limitation by another would also be appropriate, together with some mention of potential for silicate limitation, especially if diatoms are the target phytoplankton group to be enhanced.

11. I have not checked Tables 1 & 2 in detail but the results appear to be consistent with the discussion in the text. However, I do not find the summary presentation of the results on pp 2589 and 2592 to be very helpful, as the $>$ sign may mean either much, or not a lot. Some sort of plot of blobs (on a log scale ?) with the centennial



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and millennial results side by side would give a better and more quantitative display of the results, and also allow some representation of the potential errors in the estimates, which are not explicitly discussed, but are doubtless substantial (multiply or divide by a factor of 2 or 3, in most cases ?).

Minor modifications needed

12. Abstract, line 1 (and elsewhere, e.g. p2576): it would be better and more accurate to say “current and potential future radiative imbalance”

13. Abstract, line 10 (and elsewhere, e.g. pp2566, ): the reference to “significant errors”, which elsewhere takes a more pejorative form is a bit hard, since these are all essentially order-of-magnitude estimates, anyway. I suggest that the discussion of “errors” in previous work be toned down throughout the paper.

14. P 2563, line 10: replace “favoured” by “widely discussed” ?

15. P 2563, line 18: add references to publications by K Lackner et al.

16. P 2564, line 15: replace “poorly” by “not precisely” (it’s better known than some of the estimates elsewhere in the paper !!)

17. P 2576: point out that 1 TgS is only 1 MtS, which is not a lot c.f. current anthropogenic emissions of S from burning of coal & oil

18. P 2579: the explanation at the bottom of the page was not clear to me.

19. P 2582: line 21: the reference here to “whopping”, and elsewhere (e.g. pp ) to “knocked out” (p 2591 etc) are inappropriate language and should be replaced.

20. P 2586: after the reference to Lovelock and Rapley it would be appropriate to also explain the fundamental problem (upward mixing of high DIC along with the nutrients)



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and cite the subsequent comment to that effect by Shepherd et al.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2559, 2009.



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