Disclaimer: *This review was written by MSc students Erik van Schaik and Lars van Galen as part of their course work on "scientific reviewing", under supervision of Prof Wouter Peters from Wageningen University. The comments were submitted because they can contribute to the scientific process, and because they contain helpful questions and suggestions for the authors. Although the structure of this review follows the formal conventions, it is thus not a solicited peer-review from the editor of ACPD.*

Injection of sulphate aerosols in the stratosphere can reduce incoming global radiation, but increase the diffuse fraction of solar radiation at the surface. Higher levels of incoming diffuse solar radiation at the surface and lower surface temperatures caused by aerosol injection are associated with increased plant productivity, especially in the tropics. Using the Community Atmospheric Model (CAM4-chem) coupled with the Community Land Model (CLM) it is calculated that the global gross primary productivity would increase with 3.8±1.1 Gt C yr^{-1}, under the assumption of no nutrient limitation. This increase is mainly due to the increased fraction of incoming diffuse solar radiation rather than decreases in surface temperature.

The paper by Xia et al. gives a novel insight in how the terrestrial carbon sink could change under different radiative conditions, introduced by the injection of sulphate aerosols into the stratosphere. The addressed topic is a logical step from the current state-of-the-art in anthropogenic-induced diffuse radiation perturbations, and has not yet been investigated as comprehensible before. The writing style is clear and the authors make good use of the available literature. The paper does include some assumptions and choices that should be addressed prior to publication, such as the different baseline scenarios for the two experiments and the limited number of ensembles. In addition, the title presents the final conclusion as a fact, whilst the large number of uncertainties associated within this study do not justify such a claim. The abstract is well-written and does provide a more nuanced overview of the study and its findings.

In short, this paper provides new insight in a topic relevant for publication in Atmospheric Chemistry and Physics. We suggest a number of revisions to help make this paper suitable for publication.

1. In the paper two different scenarios are tested (G4SSA and G3S). The two scenarios use different baselines for both scenarios (based on RCP6.0 and RCP4.5). As stated on p25634 l4-8 these different reference runs do not allow for direct comparison of the fractional impact of diffuse radiation on the increasing photosynthesis rate. In the paper itself we can not find arguments that support the use of two different baseline scenarios. But it looks like the G3S scenario comes from an earlier phase of the GeoMIP project when the reference run was RCP4.5, and it was easier to re-use this than to recreate the G3S scenario based on RCP6.0.

   But it would be very interesting to quantify the effect of both processes (increased diffuse radiation and decreased surface temperatures) on the gross primary production. Direct comparison of the scenarios would allow for additional analysis of the results, in both space and time. Such an analysis can help give insight in the response of ecosystems on a diffuse perturbation event and determine spatial and temporal variability in a more direct manner.

   Our suggestion would be to redo the G3S solar reduction experiment with the RCP6.0 as a baseline, and use the results from this experiment to separate the changes caused by diffuse radiation and surface cooling.

Alternatively, we noted that in the earlier GeoMIP phase, there was also a G4 scenario that includes 5Tg of SO_2 emissions into the stratosphere. So for a fair comparison of the cooling vs diffuse radiation effect the authors could try to include those runs in this analysis as well.

2. The influence of changes in ultraviolet (UV) radiation reaching the surface due to SSG on photosynthesis activity has not been investigated in the paper, as is mentioned in the article as well (page 25633, line 26-29). However, it is known that UV radiation has considerable impact on photosynthesis rates and thereby gross primary productivity (GPP).

   Increasing amounts of UV radiation reaching the surface have profound negative impacts on photosynthetic activity of plants (Stapleton, 1992). One potential means for increasing amounts of UV radiation (especially UV-B) reaching the surface is by decreasing ozone concentrations in the stratosphere (Madronich et al., 1998).

   While there is agreement on the fact that ozone concentrations near the poles will decrease as a consequence of SSG, contradicting findings exist with respect to the effect of SSG on tropical ozone concentrations (Tilmes et al., 2009), (Heckendorn et al., 2009), and thereby its effect on photosynthesis rates in the tropics. As photosynthesis rates in the tropics are higher than near the poles, it is uncertain whether photosynthesis rates are forecast to increase or decrease based on the SSG-ozone-UV radiation link.

   Given the importance and the uncertainty of (the global distribution of) changes in UV radiation reaching the surface due to SSG, we advise to implement UV-radiation and the effects of SSG thereon in the model used in the analysis.

3. The two experiments (G4SSA and G3S) use a limited number of ensemble members: three for G4SSA and one for G3S. The consensus in current literature is that a minimum of ten ensemble members is desired to capture the uncertainty within climate models (Buizza and Palmer, 1998; Bonavatia et al., 2011; Kumar et al., 2001).

   A higher number of ensemble members would help to better define uncertainties. Opting to increase the number of observations for the G3S scenario would allow statistics to be calculated for that scenario, such as standard errors and confidence intervals. The large uncertainty found in climate model predictions due to the complexity of the system (Murphy et al., 2004) means that proper statistical descriptions of the errors are essential to put the results into perspective.

   Within the article we could not find arguments for the decision to use three / one ensembles per scenario. We would invite the authors to carefully think about the consequences of their decision to use a limited number of ensemble members and assess the impact of this on their final results. In addition, we would like to see the authors include arguments for their decision on the number of ensembles used per experiment. In addition, the paper can be improved by including a jackknife analysis on the ensemble members to determine bias and variance per member (Berger and Skinner, 2005; Buishand and Beersma, 1993). This can help to give insight in the influence of different members on the spread and determine the direction to which new ensemble members can be explored.

4. The photosynthesis rate is calculated under the assumption of no nutrient limitation. This is debatable, as nutrient-limitation is shown to have a significant impact on the terrestrial gross primary production (Elser et al., 2007). In addition, nutrient availability is shown to be dependent on soil temperatures, with lower N-mineralization rates associated with lower temperatures (Rustad et al., 2001; Davidson and Janssens, 2006). This is especially important as sulphate aerosols tend to have a cooling effect on the surface. Neglecting to include nutrient limitation can lead to wrong conclusions on the gross primary productivity. Within the article it remains unclear why the authors have opted to turn off the C-N cycle.

   Lawrence et al. (2011) states the the C-N module in CLM4 is biased (specifically in overestimation the leaf area) and therefore potentially unreliable, which is supported by Bonan et al. (2011). Such claims could be a valid reason not to include the C-N module. However, the real arguments why the authors decided not use this module remain unclear in the paper.

5. In addition, we would like to see a more in-depth analysis of the effects of assuming no nutrient limitation on the results. A good starting point would be to compare the areas that show the largest increase in photosynthesis rates (e.g. the Amazon rainforest) and compare this to the nutrient status of these locations (Davidson et al., 2004). Such an analysis can help to put the estimate into perspective and help answer the question: how valid is the result of a 3.8±1.1 Gt C yr^{-1} increase in global gross primary productivity under the assumption of no nutrient limitation?

6. Please consider changes the title to something less definitive to account for the uncertainties and limitations within this study. Stating that stratospheric sulphate injection enhances gross primary productivity removes all debate, which is in my opinion not justified. An example for a more neutral title: “The impact of stratospheric sulphate geoengineering on terrestrial gross primary productivity: A model analysis”.

7. Figure 1d shows that global low cloud coverage reduces due to SSG. In the results on page 25631, line 20-22, the authors state that the low cloud cover decrease observed in the G4SSA model caused by SSG is consistent with literature. However, this is not mentioned in the referred article (Jones et al., 2013). Furthermore, the article of (Jones et al., 2013) is about solar constant reduction, and not about SSG. An article which also finds the decreased low cloud cover as a consequence of SSG is (Kalidindi et al., 2015). Therefore, it is recommended to replace the reference to (Jones et al., 2013) to (Kalidindi et al., 2015).

8. The authors are not consistent in their use of the terms ‘visible diffuse radiation’ and ‘broadband diffuse radiation’. It would be helpful for the readability of the paper to be consistent in the naming. For instance, on page 25631, line 22 the authors state “Diffuse radiation over land”, which should be elaborated to “Visible diffuse radiation over land”. On the other hand, on line 27 on the same page, the authors state that “diffuse radiation increased from 40 to 140 W/m^2…”, which should be changed into “broadband diffuse

radiation increased from 40 to 140 W/m^2", as this value refers to broadband diffuse radiation (Robock, 2005).

9. In the abstract on page 25628, line 9 the authors mention an increase in plant photosynthesis of 2.4% as a consequence of enhanced diffuse radiation and cooling of the atmosphere caused by SSG. This value is not mentioned the article itself, and it can only be inferred from Figure 2a. In the results themselves the number is mentioned, though, as a fixed increase in photosynthesis rate (on line 14 page 25634). Concerning the increase in plant photosynthesis mentioned in the abstract, it is misleading to state that there is a fixed *relative* increase in photosynthesis when the overall photosynthetic activity is increasing over time as well. At the start of the geoengineering period (in 2020), the relative increase in photosynthesis is larger than at the end of the geoengineering period (in 2070) given that the actual increase in photosynthesis activity due to SSG does not change (which can be concluded judging from Figure 2a). Therefore, we advise to change the number concerning the increase in photosynthesis rate in the abstract from 2.4% to 0.07 ± 0.02 µmol C m^{-2} s^{-1}.

10. The discussion (p25636 l5-8) mentions the potential impact of stratospheric sulphate injection on the ocean carbon cycle. Whilst this is a valid statement it feels out of place within an article that does not assess ocean carbon biogeochemistry or use a model which includes it. The authors might want to consider removing this claim.

11. In the abstract (p25628 l14-16) the authors mention the potential risk of stratospheric sulphate injection geoengineering. This raises the expectation that this topic is described in further detail within the paper. However, as this is not the case we suggest to remove this from the abstract.

12. In general, the paper makes good use of available literature and gives citations where required. On p25628 l19-20 two references are made (Crutzen, 2006; Wigley, 2006) which are not included in the references.

13. Figure 2 mentions a photosynthesis rate in micromoles per m^2 and second. However, neither the caption, nor from the body text it becomes clear in terms of what molecule this photosynthesis rate refers to. It took some time for us to understand that the photosynthesis rate is in micromoles carbon per m^2 and second. It is advisable to clarify this in both Figure 2 and in the body text on page 25633. The same applies for the calculation in the Results section on page 12-18, page 25634.

14. The results section contains some parts that could fit better in an introduction or discussion section. For example, one part of the results on page 25632 between line 13 and 25 starts with introducing why diffuse radiation is important for plant productivity. Thereafter, the link between stratospheric aerosols and photosynthesis rates via the partitioning between

diffuse and total radiation is introduced. The real findings are presented at the end of this section. It would be advisable to critically review the results section and move parts to the introduction or discussion section where necessary to improve the readability of the paper.

15. In addition, the results section might benefit from division into subsections. For example:
    a. Radiation balance (p25631 l4)
    b. Vegetation (p25632 l13)
    c. Terrestrial carbon sink (p25634 l12)

16. P25632 l5-7: No reference to the estimate sulphate release of Pinatubo is given (see Bluth et al., 1992). In addition, the authors should reflect on the large uncertainty of this number by stating that the G4SSA scenario 'is roughly equivalent to one Pinatubo eruption every 2.5 years'.

17. P25633 l23: 4.2±5.9% is not statistically significant (assuming the mean and standard error are given). Please replace this with 'could potentially' or something similar.

18. P25643: Figure 1 has reversed line colours: G4SSA should be blue (not red), and vice versa for RCP6.0.

19. P25643-25644: Figures 1 and 2 are not suitable for black / white printing. This can be improved by using dashed lines for the ensembles related to the baseline, or vice versa.

20. p25644: In Figure 2 the y-axis should state that it is a photosynthesis rate, and not just photosynthesis.

21. p25631 l20, the authors state "the global cloud coverage, mainly low clouds is less". Figure 1d only takes low clouds into consideration, and therefore this sentence should become "the global low cloud coverage is less".

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