Review:
The authors have appropriately addressed most of my comments, but there are still questions and concerns about the paper that require substantial revision of the text.

First point: It seems like there is still confusion about injecting aerosols vs aerosol precursors and there needs more clarification and discussion throughout the text. The text often refers to aerosol injections, which is however not applied in most model studies and potentially for applications. Injecting aerosol precursors (not aerosols) will result in a different evolution and growth of aerosols, than aerosols. This is because the time it takes for nucleation of sulfur (SO_2), as for example discussed in Mills et al. (2017). The lifetime of SO_2 is usually about 30 days, but it depends on the availability of OH. After a large volcanic eruption, or large SO_2 injections, OH depletion can lead to a prolonged lifetime of SO_2 (47 days after MT Pinatubo). The application of the dispersion model using a tracer or aerosol is therefore different than injecting SO_2. To outline the complexity, it has been shown that SO_2 injections at a point location results in smaller aerosols than injections at a longitudinal band. This is because, the zonal wind is already dispersing the gas quickly and therefore reduce the amount at the injection location. More nucleation is induced instead of condensation on existing participles. Furthermore, after enhanced aerosol burden has been established in the stratosphere, sulfur injections will condensate on existing particles independent on the dispersion efforts.

Second point: From what is shown in the paper, I am not convinced that this method leads to significant improvements. As shown in the paper, after a one-year simulation of the fully coupled model, it seems that there are no significant differences in coverage between the fixed injection method and the dynamically derived injection method in terms of efficiency (Fig 9). Fig 9 is showing a strong reduction in coverage in the first 30 days, which may have to do with the lifetime of SO_2, and how long it takes to build up a larger sulfate coverage. It would be helpful to also show the absolute values of sulfate here, which are likely to be very small initially. Furthermore, Table 2 indicates, that after 30 and 365 days, in 2 out of 4 cases, the radiative forcing is more strongly reduced in the fixed injection case and the third case shows almost no difference between the DI method. I agree that this method may be useful to consider for the onset of sulfur injections and should be explored in more detail. However, I don't think the authors can support that there are "long-term gains in sulfate burden and radiation" as stated in the conclusions. I would therefore at this point advise against recommending this method as "a benchmark improvement in injection protocol".

Finally, as stated in my earlier review, two co-authors of this study work on a feedback controller to improve climate impacts. It is important to address the question whether this method is suitable for applying a feedback-controller. Injection locations and amount have been chosen to improve the climate outcomes in particular of surface temperature. How could this method be integrated?