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Interactive comment on "Radiative and climate effects of stratospheric sulfur geoengineering using seasonally varying injection areas" *by* Anton Laakso et al.

Anonymous Referee #2

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The broad objective of this paper is exactly the type of research that geoengineering needs. However, there are a number of clarifications needed. Furthermore, the primary motivation for the specific seasonally-dependent scenarios considered is based on tracking the latitude at which the insolation is strongest, but the actual situation is somewhat more complicated and not as well described in the paper as it could be. Because of the stratospheric circulation, the peak aerosol concentrations will not occur at the latitude of injection (other than for the equatorial case). Thus, if the only thing you cared about was being "efficient" in the sense of trying to best align the peak aerosol concentration with the peak of insolation, you'd have to do some complicated estimation of where to inject as a function of time of year, taking account of the seasonally-

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varying Brewer-Dobson Circulation. So what you picked is a reasonable first guess just to see whether the seasonal-variation idea has any merit at all, but should simply be described as an initial step towards coming up with better strategies, acknowledging that much more work would be needed to understand the options. Figure 4a,b should be given somewhat more prominence in the discussion, that is, the aerosols are widely dispersed relative to the insolation even with the seasonal strategies.

Also, I'm unclear whether the objective is to be more efficient by aligning aerosols with peak of solar radiation, or whether the objective is to do a better job of compensating for the spatial pattern of warming due to CO2 (as described in a few papers using patterns of solar reduction). These are different objectives, and the "right" strategy for each will be different (this is why I raise questions with your use of terms like "optimal" and "efficacy" below). You mix these objectives in your motivation; the introduction talks more about the latter objective, but the choice of seasonally varying injection is motivated by the former. In principle its ok to say that both of these are issues with the usual equatorial injection and that you're exploring how alternate strategies affect things, but you should be clear that you simply picked something that was somewhat physically motivated to see how it would affect the climate, and that there's no attempt to optimally solve either of these two problems.

1. L11-13, the actual issues here are a bit more subtle. Equatorial injection is often picked because the aerosols will disperse globally, so to know that the radiative forcing from equatorial injection is highest at the equator, one needs to also know that the aerosol concentrations from equatorial injection are at best uniformly distributed spatially (and in fact they'll be concentrated equatorially, as shown in your Figure 4). Not sure how to convey this concisely, but the second sentence isn't quite right.

2. L12, "optimal" in what sense?

3. L15, what do you mean by "efficacy"?

4. L23, I would be careful using the word "significant" unless you mean it in the narrow

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sense of “statistically significant” (which will of course depend on the magnitude of forcing). More to the point, the last sentence of the abstract does seem like a rather important result, and quite “significant” in the non-narrow sense of the word.

5. L31, not sure what “efficiently” means in this context. Ditto page 2 line 2.

6. P2, L17, I’ve cited that paper; I think the year is 2013 not 2012. (The same authors also have a more recent study from 2016 in ESD that would be appropriate to also cite.)

7. P2, L24, “this kind” means which kind? Specifically studies looking at how injection at different latitudes affects the climate differently? (Didn’t Tilmes do a study on that in the last few years too?)

8. P2, L30, what do you mean by “target area”?

9. P2, last two sentences, I know why you use two models, but you might want to say that explicitly. (And be explicit about what you’re giving up by not having a single model that includes everything.)

10. P3, L4, note that keeping the height constant while varying latitude might matter. . . because the tropopause height varies with latitude, at higher latitudes you’re putting material higher into the upper branch of the circulation.

11. Section 2.2.1, I haven’t read all of the references, but is there any validation against, for example Pinatubo observations, to suggest that the aerosol processes are correctly captured? Might want to mention that explicitly. Did aerosol simulations involved stratospheric chemistry also? (Interactions with ozone concentrations could matter.) How does your aerosol spatial distribution and amplitude compare with previous simulations for the equatorial case?

12. Page 6, what do you mean by saying they were based on GeoMIP G4?

13. Section 3.1 has lots of good insights and observations, but one thing missing is

any discussion of aerosol size distribution – is it the same for the different injection scenarios? This could have a big impact on the radiative forcing.

14. P10, L17&18, what is the standard error in the temperature changes due to natural variability? (Are the differences between scenarios statistically significant?) Ditto for the rates of warming on L28, and for precipitation changes on next page.

15. P10, L32, I'm not sure what you mean. . . I assume you mean that nonlinearities in climate feedbacks could change the rate of warming (since if the feedbacks were linear, there would be no effect beyond the dynamic one you already mentioned regarding ocean equilibration timescales). It is certainly true that the ice albedo feedback will have some nonlinearity in it, but I would expect that to behave with opposite sign – that is, in the warmer world, there is less sea ice left to be melted, less change in sea ice per unit increase in warming, and thus that positive feedback that amplifies warming would start to saturate. Re first line of page 10, why do you say that ice area is "clearly higher"? What figure shows this? It isn't obvious to me why it should be higher (aside from global temperatures being slightly lower, but since I know that at least with EQ you overcool tropics more than the poles, the poles are probably warmer in 2070 compared to 2010, so ice area could easily be lower, not higher).

16. P11, L17-19, slightly confusingly written (being generous; it is unequivocally false as written). A uniform reduction in SW does not lead to warming in high latitudes, indeed in every GeoMIP model, the high latitudes cool *more* than low latitudes in response to solar reduction, this is due to the spatial pattern of climate feedbacks. However, the polar amplification is even stronger for CO2 warming, so that the net effect is that the solar reduction overcools the tropics and undercools high latitudes relative to CO2.

17. And following from that, it is quite surprising that your equatorial case cools the Arctic more than the mid-latitudes; if I look at GeoMIP G1, there is not a single model that does that, and I would expect equatorial SO2 injection to have an even stronger

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tropical cooling relative to arctic cooling than G1. The sentences on P11, L20,21, does not really explain why this model should behave differently from the models in G1 (including MPI). Regarding the one ensemble member that is significantly warmer in 2010-2020 in this region, you can look at what pattern you get if you exclude that member, and then state whether or not that explains the result, rather than simply commenting that it might explain the result; this isn't hard to test.

18. Fig 6e is repeated twice and 6f is missing.

19. So NH case has slightly lower SO4 burden, slightly lower globally averaged radiative forcing, but preferentially loaded in the North. Not surprising that it is more effective at cooling the Arctic than EQ in the summer, nor that EQ is more effective in boreal winter, but the idea that it is actually LESS effective at cooling high northern latitudes than equatorial injection when averaged over the year does seem remarkable; this is also inconsistent with other model results that I have been shown but that have not yet been published. From Fig 7, the NH does indeed cool the Arctic more in the boreal summer than EQ does, as expected, and supports the idea that if the only thing you cared about was Arctic ice cover (in September), then the NH case ought to be better, in contrast to your unsupported claim. If you are going to make a claim, even for just this model, that EQ prevents melting of arctic ice better than NH, you should show a plot of it, because Fig 7 doesn't actually support that claim and looks contradictory.

20. P12 L19, chapter should be section

21. P13, L19, I don't recall seeing any optimization in this paper. . . what do you mean by optimize?

22. Bottom of P13, can you be more consistent? You use one set of metrics to compare EQ and p2w, and a different set of metrics to compare p2. . .

23. P14, L6, what do you mean be efficiency here?

24. P14, L11-13, meant to comment on this earlier, but was this effect seen in previous

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G4 simulations? If it was, not really something to highlight in conclusions here, since it is rather tangential to the purpose of this paper. If it wasn't, why not? (Obviously wouldn't show up in models without a real ocean, but at least some of those did?)

25. P14, L20, just to reiterate, you haven't shown this. (It may be true in your simulations, but you haven't shown any simulation results to back that up.)

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