Earth Syst. Dynam. Discuss., referee comment RC2
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Review on esd-2022-17

Anonymous Referee #2

Referee comment on "The deployment length of solar radiation modification: an interplay of mitigation, net-negative emissions and climate uncertainty" by Susanne Baur et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2022-17-RC2, 2022

Review of Bauer et al. (2022)

The authors of this paper note in multiple places that little attention has been posed to the question of deployment timescales of SRM. Considering they cite other studies that have done similar analyses (and arrived to similar conclusions, but with far more deatil) one might wonder how many papers are enough before something has received more than "little attention". Nevertheless, I agree that many times the words "temporary measures" or "stopgap measures" may give the impression that we're talking about decadal deployments overall, whereas it is more likely that (were SRM ever implemented) the commitment would be longer than that, and definitely span more than one generation. And this has been discussed before.

However, when defining "how long" this "long" would be, one must rely on very long term assumptions about not just SRM, but climate policy and humanity in general. My fundamental issue with this paper is that the authors replicate partly what has been done in the extended RCPs and SSPs but without any of the carefulness employed by Meinhausen et al.: in the work around the extended scenarios, it is made clear multiple times that the goal is to look at the long-term Earth System response, and not to pretend to forecast how emissions (or CDR) will look like in 2250. It is a subtle distinction but an important one, especially if, as the authors do here, the simplistic assumptions around long term policies (i.e. "we extend the last 20 years of the century for 400 more") are used to come up with rather precise numbers over the timescales of SRM deployment where the uncertainty is only related to climate sensitivity. But the work of, for instance, Lehner et al. (2020) clearly indicate that by the end of the century the main source of

uncertainties in CMIP6 projection is the one related to scenarios - and indeed this is why the IPCC spans multiple ones. Selecting one arbitrary scenario, pretending it is valid for 500 years (I appreciate the validity of scenario-building, even on the very long-term, but imagine extrapolating the last 2 decades of the sixteenth century to find out how humanity would have fared in the year 2000) and then pretending that can give us a hard, numeric idea of SRM timescales of deployment just because MAGICC has been used to derive it seems incredibly weak to me.

The authors admit the same at the beginning of Section 4: different assumptions over GHG emissions and CDR would alter the outcome considerably. But what the authors dismiss as obvious, it is not. I don't see the merit of "determining" an outcome with a +/- 35 years precision that far into the future without including the context of other scenarios that span multiple sets of assumptions. Is a middle-of-the-road scenario that is currently tracking pledges the same scenario that would better track emissions at the end of the century? We can't know, and therefore the only way we have is exploring multiple future pathways - being extremely clear that they are idealizations most likely to be wrong. But what the authors do is try to pretend their assumptions are the only "reasonable" ones, and fail to highlight enough how arbitrary their results are.

There are multiple parts of the manuscript where the authors make pretty arbitrary assumptions but try to pass them off as "neutral'. For instance:

"Specifically, we assume that the availability of SRM affects mitigation ambition and that after SRM initialization there is no incentive to increase ambition beyond the currently pledged targets. It is of course impossible to know how emissions would evolve under SRM"

If it's impossible (and I agree) than how can one make that assumption so light-heartedly? Also, the authors say that unlike MacMartin et al. (2018) they consider mitigation, CDR and SRM in conjunction, but if you take an emission pathway that already exists and don't modify it based on the presence of other components (SRM), you are indeed considering them as independent additive components. I would suggest you look

into Drake et al. (2021) for an example of an exercise trying to consider these aspects in conjunction. The authors can also discuss some recent results relating to economic games where the concept of SRM is introduced, which generally point towards opposite results from what the authors assume (there are two that just recently came out, Talbot et al., 2022; Todd et al., 2022 and more in the past). If the authors chose to ignore these results, they need to explain why.

As another comment, in the abstract, the authors define their derived long times of deployment for SRM as a "risk". But a risk compared to what? If it's an abstract risk of just "doing" SRM (i.e. an almost ethical one: SRM is wrong and therefore the longer you do it, the more you are sinning) then it's the authors personal view. If it's a risk of negative effects (that the authors mention) and these risks trump those from climate change (which the authors don't mention, plenty of literature around comparing risk of deployment versus risks of *not deploying*) then one might imagine that SRM wouldn't hinder mitigation ambition but strengthen them.

In conclusion, I don't think this manuscript is suitable for publication on ESD in its current form. The authors analyze their own scenario, based on their own assumptions, come up with a number and then consider and explain that number as an inevitable conclusion of any kind of SRM deployment under any possible scenario. What novel insight does that shed on anything? The analyses related to the carbon cycle are also to my eyes unremarkable, especially when other analyses based on more comprehensive models are already available.

I agree with Reviewer 1 that this work would be far more robust if multiple scenarios where analyzed: if stronger mitigation and more CDR was available, as in other IPCC scenarios, how would these results look like? If the stabilization target was 2 instead of 1.5? This could probably be a way in which this work could become suitable for publication, together with a much more in depth discussion of methods used and a broader overview of past literature on the subject.

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