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Interactive comment on "Critical Assessment of Geoengineering Strategies using Response Theory" *by* Tamás Bódai et al.

Anonymous Referee #1

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The authors need to do a better job of reading the existing literature on the subject and articulating what new contribution they have; in particular see the first (and second) comment below – it looks like this paper is simply repeating previously published work but in a less clear manner and using a worse climate model. The wording also needs to be significantly improved for clarity; in particular, the paper tends to use way more mathematical notation and terminology than is necessary to describe fairly easy concepts. Finally, the paper does not use a state-of-the-art climate model, so that at best the paper could be useful for illustrating methodology (were it not for the fact that others have already done so), and not for actually useful results regarding whether linearity is or is not a useful approximation, or the degree of "cancellation" of spatial patterns of temperature or precipitation (since many other papers have already explored that over

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Discussion paper

the last 20 years in far better climate models).

1. My very first thought on the very first line of the abstract was, hasn't this already been done? Go look at MacMartin and Kravitz, in APD in 2016 doi: 10.5194/acp-16-15789-2016. The same authors (among others) have shown linearity of the response to solar geoengineering in a whole range of papers.

2. The remainder of the first paragraph sounds exactly like one element of the approach used in this paper: doi:10.1098/rsta.2014.0134.

3. Second paragraph, line 9, this seems not very well worded. The bigger comment here is that no real system is actually linear, the question is simply whether a linear approximation is good enough to make useful predictions. So a residual error doesn't mean that a linear approximation is not correct – it is never correct, just maybe useful if that error is sufficiently small. The other comment here is that the sentence is extremely hard to understand; one has to guess that you made some linear approximation to predict something that should have been zero to interpret the first half of the sentence (in general, some arbitrary combination of greenhouse and solar forcing can give you any residual you want, including zero if you want it to), and I have no clue what you mean by "linear susceptibility". (The susceptibility of what output to what input?)

4. L12-13, again, "under geoengineering" is too vague. One can pick any level of geoengineering one wants and (subject to saturation limits) get any level of global mean temperature you want.

5. L13-17, how is this different from the conclusions reached in many many dozens of previous papers on the subject? (I can't even think of a single paper to point to as it is quite well known, perhaps Kravitz et al 2013 in JGR, or even going back to Govindasamy and Caldeira in 2000 in GRL; really one could find this observation in every paper that has ever been written on the subject.) This isn't new, and as such, this isn't a contribution of your work, and thus does not belong in the abstract.

6. L15, strike the word “ideal”. There’s no way to justify that word here, nor is it necessary.

7. L21-22, I’ve never heard someone talk about “great risk” as an “enormous gain”. Suspect you didn’t mean that, reword.

8. L24, this is not true. See comment #1 above for an example (and presumably any paper that has cited it). See comment #2 for another example.

9. Ditto P2 L1-3.

10. Section 1.1 is poorly motivated:

(a) First, is the goal of this paper to learn something useful about geoengineering, or is the goal of this paper to show off particular mathematical tools? If it is the former, then surely some of the problem description in section 1.2 should precede this, and be used to motivate this. Furthermore, if it is the former, and the math is a means to an end, then surely one shouldn’t include more mathematical notation or concepts than are actually required to solve the problem at hand, yet there is zero motivation in this section to say why these concepts are needed. If you want this paper to be read by climate scientists (or really, anyone at all other than the authors), you need more motivation. For example, if the only thing one is interested in is “the response” of system (1) to some forcing f(t), that doesn’t need any of the subsequent paragraph or concepts. . . that’s much more simply given by the solution to the differential equation.

(b) Eq. (1) as written contains nothing stochastic, so as written pretty much the rest of this entire page is superfluous to solving eq (1). (And similarly, whenever you talk about ensemble-mean quantities, which only make sense if there is some stochastic component to the problem.) If you are intending later on to introduce some stochastic component, then you should both say so before you introduce the math, and furthermore justify why that math is actually needed – that is, why you think it is insufficient to simply include additive stochastic forcing. To my mind, the forced-response (i.e., ensemble mean) is the only thing we are trying to predict, and while for a nonlinear system the forced response will itself depend on the statistical properties of the stochastic forcing, I would expect this to be such a trivial effect that it would be perfectly reasonable to use eq (1) in a deterministic sense – in which case, much of the math introduced on this page is irrelevant. If you disagree, you should explain that, rather than just regurgitating math onto the page.

(c) Also, as written, there is no indication of why you have chosen to separate epsilon from g, since obviously that doesn't actually change anything. (And I didn't find anything later in the paper that explained it either...)

(d) Note that while page 2 is, as written, nearly impenetrable to your intended audience, the first paragraph on page 3 seems written at too low a level.

11. Sec 1.2, first line, choose a word other than "mitigated" which (unfortunately) has a specific narrow connotation in climate change (referring only to reduction in greenhouse gas emissions)

12. First par of Sec 1.2 should be rewritten for clarity, e.g., start by saying "here are the approaches that could do this" and then "we will only consider a solar reduction". Note that SRM refers generally to all of these methods, it does not refer specifically to a solar reduction.

13. P4, L14, again, this is not true, see point #2 above.

14. L15, even aside from the observation that other studies have indeed formulated the required input as the solution to an inverse problem, it is also not true that other studies have only considered predetermined inputs as implied by this sentence, there are now quite a few papers that have used a feedback algorithm to adjust the forcing level; one should at least insert the word "many" (i.e., "In many previous studies. . .").

15. L30, I think the first half of this sentence is unnecessary.

16. P5, L9-12, this seems like a deliberate and unnecessary introduction of jargon

to describe an incredibly simple concept; as implied in earlier comments, who is your intended audience? This does not seem written in a way intended to be read by any climate scientist (indeed, strikes me as deliberately written in such a way so that no climate scientist will read it).

17. L13-16, again, this is very well known to any reader of ESD, and while it is important, it is unclear to me how using a different set of terminology is helpful.

18. Regarding the footnote, the first three sentences seem completely redundant with the text before the footnote, while the last sentence is normative.

19. L20-23, a trivial editorial comment, but can you use bullets rather than hyphens that could be confused for a minus sign, and then perhaps a colon before the clarification for the same reason.

20. L27, can you translate this sentence into English? (I think you're saying something trivial.)

21. P6, L2, no, that is not an open question. Read virtually ANY paper that has ever been written about geoengineering.

22. P6, L4-5, what do you mean that "response theory can predict spatial patterns"? I think that the spatial patterns of response to forcing are a result of climate physics, and are predicted by climate models.

23. P6, L10, minor wording, but it is well known that you can't "cancel" the effects of greenhouse forcing but rather you can offset some effects or reduce the effects, or some such wording…

24. P7… given availability of simulation output from more realistic models (e.g. GeoMIP), why use this one?

25. P7, first paragraph… see also comment #1; this has been done before specifically for geoengineering.

26. P7, L13, I don't know what basis you could conceivably have for asserting that reality would "most likely" be worse.

27. P8, L5, what distinction are you emphasizing with the \hat notation?

28. P8, L26, why are you claiming that a single experiment is insufficient? I agree if one is trying to diagnose whether the dynamic system is linear or not, but if the system is assumed to be LTI and noiseless, then the Green's function can indeed be perfectly computed from a single experiment

29. P9, L3-10, the answer as to which is better depends on what range of frequencies one is interested in as well, and thus I think your comparison of two possible choices (out of an infinite number of possibilities) is a bit simplified (in contrast to most of the paper that seems to take an overly complicated approach to everything). I think Ben Kravitz has a paper in the last couple of years on system identification in the context of climate science.

30. P9, L13, note you missed the year (2005) in Hansen et al, though I don't recall that paper dealing with the dynamics at all (I didn't go back and look it up though). Caldeira and Myhrvold (in ERL, 2013) for CO2 and MacMynowski et al (in GRL, 2011) are the ones I might cite to say that the dynamics themselves are similar for both forcing mechanisms (in those papers, both satisfying a semi-infinite diffusion model).

31. L14-16, I'm guessing this point is also made in Ben Kravitz' paper, but you should check (looked up the reference I was thinking of, it was 2017 in ACP, but I didn't go back and re-read it to refresh my memory).

32. L18... yet again, this seems like you are planning on precisely duplicating the methodology of MacMartin and Kravitz, 2016!

33. Figure 2 caption, Caldeira and Myhrvold also did the fit with a 2-box model... as did Isaac Held a few years earlier. (The former of these papers notes that the most appropriate functional form depends on the specific climate model, though my

guess would be that it is difficult to distinguish from step-response type inputs. There's another paper in the last couple of months, also by MacMartin et al, that also uses a semi-infinite diffusion model in fitting the step response, in Phil. Trans. I think.)

34. Section 2.3, this happens to be a case where doing the analysis in the time-domain instead of the frequency domain would be utterly trivial. While the paper noted in my comment #2 used a particular functional form for the Green's function (aka impulse response), in general all one needs to do is uniquely solve for the required forcing at time 1, then given that, uniquely solve for the forcing required at time 2, and so forth. I'm not sure why you're choosing to make this complicated when there's such an obvious, easy, exact solution method available (that also, in a real situation, has the benefit of not needing to know future values for the greenhouse forcing). Seems like this section could be replaced by a sentence. . . (or, indeed, by a reference). (If the time-domain process is susceptible to noise in the estimated Green's function, seems like the solution might be some very slight smoothing – my presumption being that you are implicitly doing that with the frequency domain approach.)

35. Re Fig 3 caption, penultimate sentence, no, I wouldn't think that. . . the atmospheric response is very fast, and the time-constants that you see in annually-averaged data result (mostly) from ocean memory; there might still be differences in the time-constants due to different latitudinal patterns of forcing though.

36. P13, L6, I would be cautious about using the word "linear" here given that much of the paper involves concept of a linear dynamic system, which isn't quite the same meaning as a signal with a constant slope.

37. P14, L14, this sounds likely for geoengineering, but I don't know that it is actually clear. . . I could write down a dynamic system where nonlinearity resulted in errors during the transient but no error at all at large time. (Might point this out at eq 16 too.)

38. P14, L15, nonlinearity is only one source of error in estimating susceptibility, the other being the climate variability (and finite ensemble size)

39. L23, again, I again agree this is plausible, but it is not rigorously justifiable.

40. Section 3.1.1, I'm confused. If you take a single forcing scenario, your "predicted" and "truth" should by definition be identical, and only deviate if you estimate the Green's function from one forcing scenario and then apply it to predict the response to another; this section needs to be much clearer about what you are doing.

41. Also note that I would suggest avoiding the word "truth" here. . . just to be clear that we are talking about models and not the real world.

42. Figure 5, it is not theoretically possible to assert based on this plot alone that the response is nonlinear. One could certainly use this plot to uniquely determine a Green's function for a linear system that would perfectly capture the response; one can only identify nonlinearity when using two different time histories of forcing. . .

43. Note re any nonlinearity found here, it is hard to assess the relevance – since, of course, all you are doing is diagnosing whether a particular climate model is or is not nonlinear. There've been tons of studies looking at linearity of the response to forcing, including many specifically for geoengineered forcing (none of which you cite), showing that in many other ESMs, the response to a solar reduction is relatively linear. (See for example the reference in my first comment.)

44. P16, L15, why "unsurprising". . . whether or not the response is well-approximated with a linear one has to do with the physics, not the magnitude of the change. (In this case I might presume saturation of ice-albedo feedback as has been noted elsewhere.)

45. Fig 6, same comment as earlier; I presume you are estimating the Green's function from one simulation and using it to predict another (otherwise the error would be zero), but you don't say so.

46. P18, L13, there's a ton of papers on this. . . starting with Govindasamy and Caldeira in 2000, through to at least Kravitz et al in 2013 (looking at GeoMIP results).

47. P18, L17, you appear to really like the word "unsurprisingly", but generally speaking

every time you use it I don't think the usage is appropriate. The difference between solar and greenhouse forcing is a result of climate physics – different spatial patterns of radiative forcing in particular; it happens to be true that those patterns of radiative forcing differ most at high latitudes (where there is little sunlight), and that that is also where there is the highest response to either forcing, but the physical reason for these two is different – the residual being largest there is NOT because the response itself is largest there – so your lack of surprise is based on an incorrect assumption! I would suggest you think more carefully before choosing to not be surprised. . .

48. Table 2, might note that this is inconsistent with every single climate model simulation (in decent climate models, that is) and basic physics; a solar reduction with the same effect on global mean temperature as CO2 will have a larger effect on precipitation (due to the "fast" response to precipitation, see Andrews et al 2009, Bala et al in PNAS, numerous other papers).

49. Figure 11 (and also Figure 9), there's something really weird about this climate model; I've never seen a precipitation response that looks remotely like that. . .

50. Footnote 8 is weird. When talking about equilibria and climate sensitivity to a steady forcing, one is referring to the forced response alone (which under mild assumptions one could estimate through a sufficiently long simulation or sufficiently many ensemble member average); the resulting quantity is not itself chaotic, only the natural climate variability that is superimposed on top of it is.

51. P25, L25-28, this is a deeply disturbing "foundational" claim on which to base this paper, since it is so central to the entire study of geoengineering, and so deeply explored in every single paper that has ever been written on the subject; surely the authors are not unaware of that?

52. Section 5, first few sentences – as noted earlier, none of this is new.

53. P27, L21-22, worth noting that this is model-specific. Other models (e.g., the

GCM's used in GeoMIP) don't exhibit as much nonlinearity as yours does.

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