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

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We would like to thank the anonymous referee for the thorough consideration of our work and the long list of comments and recommendations for improvement. Very importantly, we thank the referee for pointing us to many relevant papers which did indeed consider the same or similar problems, and so should be credited.

Despite the duplication (or "multiplication") of some results or methodological developments from these references, we think that our paper still features original contributions. One of the two main new contributions is that we predict/calculate the _required_ solar forcing needed for cancelling or modulating the total response in an arbitrary desired fashion. For e.g. the GeoMIP experiment G2 the solar forcing was "simply" chosen

to have the exact same ramp shape as the "nominal"/theoretical radiative forcing due to CO2 concentration forcing. Presumably this is so because the people behind the project had in mind a desire of cancelling the global average surface temperature and observed that the relevant response characteristics of models to CO2 and solar forcing is very similar. In contrast with this, we outline the _general_ approach to geoengineering when 1. the choice of observable to control is arbitrary, and 2. the response characteristics to a given forcing and a geoengineering forcing are dissimilar. We mark this contribution by (I) now.

We acknowledge that our work regarding the typical side effects with respect to the spatial patterns of surface air temperature and precipitation is not original. Therefore, we remove the particulars about it from the Abstract. (The label (II) is now reserved for our other main contribution, to be detailed below.)

It appears to us that the paper doi:10.1098/rsta.2014.0134 outlines a feedback control for determining the solar forcing on the fly, similarly to our reference (MacMartin et al., 2014), by the same author. Certainly, it is the relevant approach to actually practicing geoengineering, because one doesn't need to know the pertinent Green's functions of the system. However, for an _efficient scenario analysis_, feedback control is of little use, according to our claim in the submitted manuscript: "Note that under feedback control, in a scenario analysis setting, a new simulation needs to be run for each emission scenario." (Of course, there still remains the issue that the assessment of geoengineering by an "emulator" is done based on a model, whose response characteristics are not necessarily (or rather likely not – given that different models differ from one another) the same as those of Earth's, and so the practice of geoengineering would entail further risk.)

Our choice of the Planet Simulator, PlaSim, for a model to analyse is for convenience only. 1. We had at our disposal preexisting simulation data. 2. To "keep to the word of response theory" we wanted to work with the forced response, excluding – as much as possible – internal variability (IV), because IV is out of the control of geoengineering. The correct approximation (unbiased estimate – see our reference (Drotos et al. PRE 2016)) of the forced response is a finite ensemble approximation. To our knowledge there is no freely available large ENSEMBLE simulation data for CMIP5 models forced by step functions, but up to three realisations only, as in the GeoMIP experiments (http://climate.envsci.rutgers.edu/GeoMIP/ http://climate.envsci.rutgers.edu/GeoMIP/doc/specificationsG1_G4_v1.0.pdf). Furthermore, for our analysis of linearity, we needed/wished to run other types of simulations, e.g. SXX, CX1, BR1, BR2, CQ2, CS2I, SS2I, BR2C. We did not have the resources and in-house skill to run these simulations on a CMIP5 model. We would like to point out, in particular, that even the SSX-type experiments do not seem to exist for CMIP5 models up to now; but even if there was such data, and so we could predict the required solar forcing, there is not a BRX-type data set for CMIP5 models for which the _predicted_ solar forcing is used – not a forcing that takes the same signal shape as that of the CO2 forcing (as in the G2 GeoMIP experiment).

We omitted the seasonality in the model so that the discrete-time theory, the convolution sum (11), could apply exactly (provided an infinite ensemble size, a staircase forcing and linearity, of course). Our conclusions about non/linearity rely on this. We do not know if it applies also when beside a staircase forcing component there is also a periodic one (with a time period equal to the stair length). We think that the forced response to seasonal forcing alone is strongly nonlinear, and it's not clear to us how the system responds to a combined strong periodic and weak (say) ramp forcing: whether the response of annual averages or at certain phases of the year is linear (see for reference (Drotos et al. J Clim 2015)).

Due to the lack of seasonality, e.g. to CO2-doubling in the model the response of the global average surface air temperature is more than 2x that of the average for the CMIP models. At high latitudes locally the response is "noticeably" nonlinear. And there is a significant nonlinearity of the precipitation response, too, at Equatorial regions. The nonlinearity of the temperature response should be due to albedo saturation (as the

referee suggests) and/or a nonlinear characteristics of radiation physics. These effects show up also in CMIP models (Winton, 2013; Good et al., 2015), even if the response is more moderate, likely because they are very basic effects, and so the otherwise very high complexity of the CIMP models compared to PlaSim does not set them apart in this respect. Therefore, we think that if we find that under combined forcing/geoengineering the regional response, even if very small, is nonlinear in PlaSim, there is a "good chance" that that carries over to CMIP models.

In fact this is just what our latest analysis suggests. We had a closer look of our results and what it implies, and have come to reverse our conclusion about the (approximate) linearity of the local response. This is something that the referee claimed to have been shown by many authors, by MacMartin & Kravitz (2016) among others, and so our (original) claim is a duplication. In fact (M&K 2016) demonstrates in Figs. 3,4 the linearity under geoengineering only for global average temperature and precipitation, and for a weaker forcing. Regarding local responses, their Fig. 7 is actually inconclusive – contrary to the referee's claim (which inconclusivity is consistent with our suggestion of possible local nonlinearity also in CMIP models). That diagram shows results averaged over nine models. (Note that in an assessment of geoengineering it is not the average of all possibilities that we are interested in but the range of possibilities and how extreme some possibilities may be.) Furthermore there is an issue that the comparison of a linear prediction and the "truth" (e.g. our Figs. 14 (a,c)) can still be inconclusive. To compliment this comparison we also evaluated another measure of nonlinearity in our new Fig. 15. Thus, we propose this finding and conclusion as another main contribution of our work; and in the revised manuscript attached we label this contribution by (II) in the Abstract and elsewhere in the text. The reversed conclusion also prompted us to change the title.

A further original contribution of our paper spins out of the main proposal said above, i.e., that the required geoengineering forcing should be calculated as a solution of an inverse problem (I). In the generic situation we cannot adopt for the geoengineering forcing the shape of the GHG radiative forcing, determining the slope – if it's a ramp – by an iterative procedure considering the stationary climate (like our BR2C experiment or the GeoMIP G2 experiment). Instead, the inverse problem has to be fed by the Green's functions. We demonstrated the importance of determining the Green's functions accurately by a dramatic improvement of the linear prediction. (Although, the linear prediction might still not be very accurate for certain observables: local, rain.) Although the methodology that we employed was published elsewhere first (Gritsun, A. and Lucarini, V, 2017), that project evolved in parallel with ours. Furthermore, in our manuscript we pointed out that in the case of linear response under geoengineering "it is meaningful to strive to determine the linear susceptibility accurately, unlike in the case of having to predict large responses which have considerable nonlinear contributions".

We note that this method of determining the impulse response/susceptibility more accurately by eliminating even order nonlinearities in the XSX experiments can be very useful considering that it might allow for reducing the number of ensemble members to simulate. We could choose to raise the identification forcing magnitude to improve the signal-to-noise ratio, as done by (M&K 2016). The increased inaccuracy of the estimate due to possible nonlinearity can then be compensated by our technique.

Next we respond to some specific points of the referee. The numbers below correspond to the numbers of the respective points of the referee's comments. Any comment that we do not address explicitly, we accepted, and catered for in the attached revised version of the manuscript. This is not the final version of the manuscript intended potentially for resubmission but a working document for the purpose of discussion; we anticipate further streamlining and rewording. Changes of any significance are highlighted by boldface typeset.

10. (a) A paper on geoengineering would best begin with the problem description. However, our aim is to frame the geoengineering problem as an inverse problem, labelled by (I), and that cannot be done in Sec. 1.2. without some introduction of the

mathematics. As we propose point (I) as a main contribution of our work, we think that it is important to explain it carefully. A crude problem description is already given in the Abstract. Some additional text now before Sec. 1.1 hopefully gives sufficient motivation. We refer to the work of MacMartin and Kravitz (2016) whose motivation was also to create an emulator based on response theory, which emulator can make predictions, or consider what-if scenarios, for an efficient assessment of geoengineering.

10. (b) We are puzzled by the referee's statements here, it does not resonate with any of our understanding of the problem at hand. Eq. (1) is a very high dimensional nonlinear system of equations. It describes the motions of a turbulent fluid. Clearly, its solution is chaotic, with a large variability on many time scales. That is, the internal climate variability is not represented by stochasticity, but deterministic processes. (For a discussion on the forced response and internal variability, see (Drotos et al. J. Clim. 2015).) We never introduce stochasticity in this work. Yet, the ensemble average can behave very simply, even linearly under weak forcing. This is a very nontrivial result of response theory. It appears that the referee thinks that Eq. (1) is linear, representing the forced response of the climate system, and the internal variability can be somehow added as random (not deterministic) noise, i.e., stochasticity. We guess that the referee has in mind LTI theory (https://en.wikipedia.org/wiki/Linear_time-invariant_theory), but response theory (see e.g. the book by Risken, or Ruelle's work) is generic in its scope, applying to any Axiom A (nice) NONlinear system.

10. (c) The concept of _linear_ response arises via a perturbation theory approach, when the full response is sought in the form of Eq. (2). The diminishing contribution of higher order terms can be represented by the powers of a small number epsilon. When one knows or assumes that the response is approximately linear, one can indeed just retain g(x). In what is now Sec. 4.1 it makes it easier – considering Eq. (2) – to see how the even-order nonlinear terms can be eliminated given that even powers of eps=-1 are the same as that of eps=1.

14. We are not aware of publications that frame geoengineering as an inverse problem.

Please kindly provide us reference. In the following paragraph we are referring to feedback control, which clarifies that we didn't mean that in all previous studies the SRM was prescribed. Nevertheless, we replaced "previous studies" by "e.g.". Also, we would not like to refer to "many previous studies" when we cite only one or two papers.

15. This is meant to be an emphasis on the fact that may be global warming can be cancelled by geoengineering but some climate change should still be expected.

16. Why would we not want climate scientists to read our paper? The term "isoline" should be familiar to every single climate scientist as it is a basic concept in theormodynamics, and climate models involve a great deal of thermodynamics. Furthermore, our exposition of geoengineering allows for a schematic seen in Fig. 1 that should aid rather than confuse understanding. We do not think this is a convoluted way of thinking, but acknowledge that it is not everyone's way.

17. We accept that this is not a new idea (although our original submission citing (Ferraro et al. 2014; Ricke et al. 2010, 2012) had already acknowledged it, even if we had not been aware of other references that the referee pointed us to), but we can certainly expect that it will be to some of our readers, and we choose to expose this in a way that is the most appealing to us, and we would like to believe that we won't be alone with this.

18. We do not think that there is a redundancy here. The footnote can be considered to fall under considerations of social sciences. No normative statement is made in the footnote. Normative statements express moral judgements or wishes how things should be. The last sentence expresses a factual statement, or a kind of "prediction".

19. The same latex syntax for the "itemize" environment using the plain article document class produces a bullet. The hyphen is likely produced by the esd document class. Hopefully this issue will be fixed in the copyediting process if our manuscript makes it.

20. The sentence is grammatically correct. We suppose that the meaning of "isoline" is the key to understanding the statement, and believe that our target audience will not be overly challenged. Also, Fig. 3 of the cited paper (Boschi et al. 2013) would help the readers who look it up.

22. We think that we use the word "prediction" correctly in this context. For example, as the referee suggests, a model can provide the prediction of a future state of the system which is not _explicitly_ represented in the model, but somehow coded by the model equations. Similarly, we can use the Green's function to predict the evolution of the system under some forcing, which evolution is not explicitly represented in the Green's function. The Green's function is determined either from the model equations by eq. (4) or from a forced experiment which is different from the situations that we want to have a prediction for.

23. As far as we know the word "cancel" is an exact synonym of the word "offset". Also, we wrote that the _effect_, i.e., the response, is cancelled not the cause, i.e., not the forcing. Actually, our framing of geoengineering as an inverse problem, our point (I), does imply this. This is what sets our approach apart from previous approaches, including the G2 experiment of the GeoMIP project.

27. The sentence that incorporates Eq. (11) expresses clearly, we think, that \hat{\Psi} = \Psi for staircase forcings only. For e.g. a continuous ramp forcing the realised \Psi is somewhat different from \hat{\Psi} given by the convolution sum (in which f[n] is the same for the continuos ramp and staircase functions).

28. This goes back to point 10. (b). (M&K 2016) appreciates the need for an ensemble in the bottom right of page 15791, under point 1.

29. We agree that any forcing can be used in principle for system identification, since it's just a matter of substituting that forcing and the response to it into an expression for the susceptibility, which can be obtained by rearranging our Eq. (5). We intended to "excite" the system at all frequencies, in order to have a "balanced" signal-to-noise

ratio (SNR) for the different frequencies. The spectrum of the delta function is flat, and so it is suitable for this purpose. Unless one has a specific requirement for the SNR depending on the frequency, we cannot say what the best forcing is. We would like to consider the problem of optimal identification forcing in the future – unless it is something that has already been solved by Kravitz, as the referee suggests. We would greatly appreciate if the referee could identify the paper by Kravitz that he/she referred to; unfortunately we haven't found it by ourselves. Apart from the issue of the ideal identification forcing, in the paper we compared two possibilities that have the same result in the ideal situation of having an infinite ensemble, but one is better when the ensemble is finite.

32. Taking the annual average is a minor methodological issue, and we wouldn't refer to adopting a methodology as "duplication" the same way as some results are redundant in the literature. However, we note that we actually explain that the convolution sum (11) applies exactly to the annual average, provided that the forcing is a staircase, with steps of the length of a year.

34. It wasn't obvious to us that there is a time-marching solution method to the inverse problem. However, the referee is right; we describe in the revised manuscript briefly this method too. If only we could acknowledge gratefully the referee by name! However, we note that although the referee is right also about not needing future values of the response to determine the forcing, we do need concurrent values, and so the solution technique is not applicable in practice. It is not a great issue, however, because one can employ feedback control to approximate well the solution of the inverse problem, as we discuss this now in more detail in the manuscript.

37. We do not understand the premise of the referee's statement. We think that it might be that our basic aim was not clear to the referee, namely, the framing of geoengineering as an inverse problem (I). If the actual response is nonzero at any time, despite that we use the forcing that is the solution of the inverse problem, it can be a sign of two things only: 1. the actual response is nonlinear or 2. the susceptibility that feeds into

the inverse problem was not determined accurately.

41. We do not think there is a real danger of such a misunderstanding. Also, at the first use of the word "truth" we made it clear that we mean the outcome of a model simulation.

42. We expressed clearly that in this scenario the forcing is very slow. Therefore, what we see in Fig. 5 is the static response characteristics. If it is not a straight line, then the static characteristics is nonlinear. We also note that the Green's function cannot be extracted from this data with any precision, which goes back to the point 29: the time scales present in the Green's function are not excited by such a slow forcing. Note also that, as we already wrote in the original submission, the ratio (17) (in the revised manuscript) that expresses nonlinearity can be evaluated from the data in Fig. 5 and the knowledge of the forcing used.

43. As we wrote in our main response above, we think that our analysis of the non/linearity of _regional_ averages, motivated by the referee's comments, is an original contribution. Please kindly let us know of papers that consider the linearity of regional response and make claims about it, because we are not aware of any. It is only the linearity of the response of _global averages_ that have been looked at more closely.

45. Sec. 2.2 describes how we obtain the Green's function. We use this Green's function always to make a linear prediction of the response to any other forcing, such as the ramp in the XRX experiments. This is the premise of using response theory for making predictions in the sense of point 22. above on the first place, so it goes without saying, we believe.

47. In this instance it doesn't take a physical consideration where the superposition of the patterns results in the largest values; it is just mathematics that if the patterns to superimpose are similar but slightly misaligned, then the superposition results in the largest value where the separate items had the largest values. The referee suggests

that the misalignment is the worse where the response is largest. We cannot back this up; as far as our lack of surprise is concerned, we just assumed a generic misalignment of the two patterns.

48., 49. In PlaSim there is a fast response to precipitation of opposite sign. Please see attached the Green's function for precipitation (Fig. 1) and temperature (Fig. 2) as determined from the CS2 experiment. A source of difference from published results could also be the lack of seasonality in our simulations.

Please also note the supplement to this comment:
https://www.earth-syst-dynam-discuss.net/esd-2018-30/esd-2018-30-AC1-supplement.pdf

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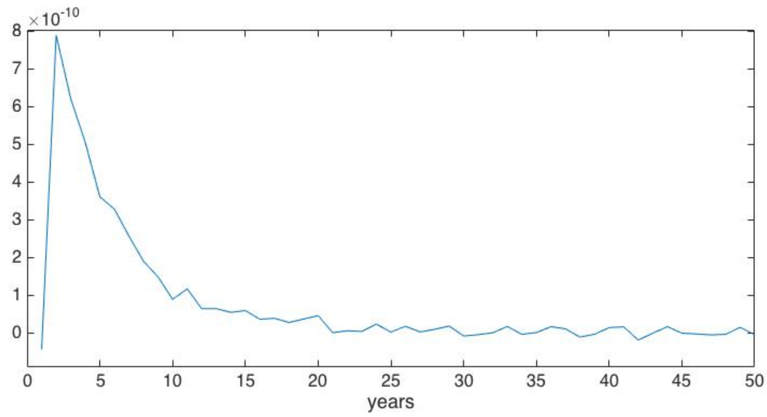

Fig. 1. Green's function for the global average annual precipitation.

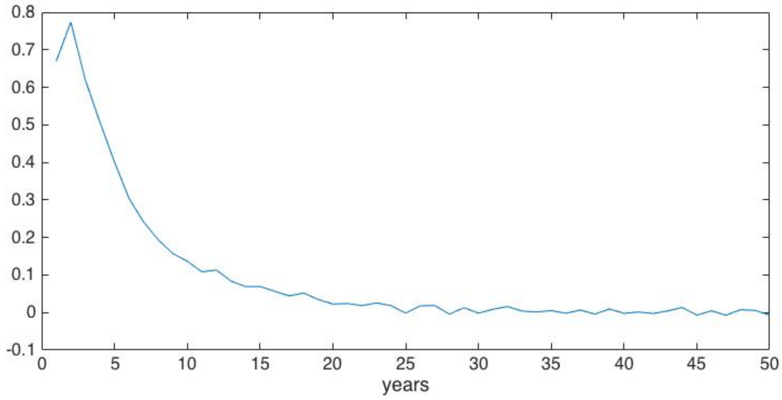

Fig. 2. Green's function for the global average surface air temperature.