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Geoscientific Model Development Discussions

Interactive comment on "HIRM v1.0: A hybrid impulse response model for climate modeling and uncertainty analyses" *by* Kalyn Dorheim et al.

Kuno Strassmann (Referee)

kuno.strassmann@alumni.ethz.ch

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General comments

1. The manuscript describes the emulation of the temperature response to radiative forcing of a simple climate model with an impulse-response function. This is not a novel concept, and corresponds to what is typically one out of a set of many equations in existing simple climate models. The wording used by the authors suggests a more sophisticated, original approach, and is in my opinion a bit misleading. My main objection, however, is that the limitations of the model, which are quite strong, are not sufficiently addressed. The main limitation is that the (climate) model is not really coupled to a carbon cycle component and cannot therefore represent the carbon-cycle-climate-

feedback. Another important but never mentioned limitation is that the model presented has a fixed climate sensitivity. The approach could be extended by extracting IRFs from models (or model setups) with different sensitivities, but this is not straightforward.

2. Furthermore, I find the validation exercises presented (section 2.3) not very informative, and I feel that the case studies (section 4) are only useful to demonstrate model application, and do not yield significant scientific insight. Several of the conclusions offered based on the latter appear unfounded.

3. Finally, the model description is incomplete, as no details on implementation are given, short of the model code itself. This said, the model as such seems to be correct, and my comments only concern its presentation, applications, and interpretation. I leave it to the editor to decide whether the limited material presented here warrants a publication in gmd.

Specific comments

1. p1/25 Earth system models of intermediate complexity should be mentioned here, especially as they are referred to later in the paper.

2. p2/31 "SCMs can be characterized as either process-based or idealized climate models." All models, especially SCMs are idealized - a better word would be "abstract" as opposed to "process-based". However, it would be better to speak of IRF-based models, as the authors use the term "idealized SCM" synonymously, although other types of non-process-based models may exist.

3. p2/40 Another key nonlinearity that should be mentioned here is the chemistry of CO2 uptake at the ocean surface.

4. p2/44 Are all idealized SCMs based on IRFs? It would be better (and avoid repetition) to say "IRFs used in SCMs are defined as. . .".

5. p2/48 AR5 mentions several types of IRF-model, but the main model used to calculate GWP represents the relationship between emissions and CO2, not temperature.

6. p2/53 “Idealized SCMs may exhibit biased results, however, due to their lack of nonlinear dynamics.” It is not in general true that idealized SCMs, meaning SCMs that are not (fully) process-based and use IRF functions, lack nonlinear dynamics. There are SCMs that apply IRFs only to the quasi-linear parts of the system, linking these with equations that capture essential nonlinearities (e.g. Joos and Bruno, 1996; Strassmann and Joos, 2018).

7. p2/56 Using the RF simulated by a model with nonlinearities as input hardly counts as “incorporating nonlinear dynamics”.

8. p3/65 (whole paragraph) I find it rather misleading to call the use of an existing model to provide input a “framework”, given that there does not seem to be any real coupling, i.e. exchange of information at intermediate timesteps. If this is the characteristic that distinguishes this “hybrid approach” from other IRF-based models, it does so in a negative way. There are SCMs that represent the climate response with IRFs and allow for coupling with a carbon cycle component at each timestep, for example, the BernSCM model (Strassmann and Joos, 2018). BernSCM combines IRF-based components describing linear systems with nonlinear parametrisations to capture the essential nonlinearities of the carbon cycle-climate system, and expresses the IRF-components as a system of ordinary differential equations to allow for efficient integration in coupled mode.

9. p3/69 “incorporate the nonlinear dynamics. . . if the majority of the nonlinear dynamics of SCMs occur between the emissions to radiative forcing calculation” -it would be more adequate here to say that the IRF-model, which really constitutes the contribution of the authors, does NOT incorporate any nonlinearities, because there aren’t any.

10. p4/96 (whole paragraph) It is true that the carbon-cycle-climate feedback could be included in the IRF. However, the resulting model would still have strong limitations. It is likely that such a model, being liearized, would give accurate results only for a limited range of forcing scenarios or time scales.

11. p4/114 “The end of the IRF was extrapolated with an exponential decay function” Please mention the decay timescale.

12. p4/120 “underlying assumptions about where the majority of the nonlinearities occur are true” - This simply means the climate component of Hector is linear, which is to be expected of an SCM and could be inferred by looking at the design of that model.

13. p4/122 I don’t see what chemical processes could affect the relationship between RF and temperature, at least in an SCM.

14. p5/126 “For each RCP. . .” this sentence is not very informative and could be dropped.

15. p5/149 As mentioned above, this finding is not surprising; it merely characterizes the Hector SCM and holds no scientific information on the physical climate as such.

16. p5/155 “difference of 0.0%” There are no significant digits in this number.

17. p6/156 As for the additivity of temperature changes, the lack of nonlinearities in the Hector climate component is not a scientifically relevant finding.

18. p6/164 “In this analysis, however, the black carbon (BC), organic carbon (OC), indirect SO2 effects (SO2i), and direct SO2 effects (SO2d) RF input time series were varied.” It is not correct to only vary these components. The uncertainty of other forcings should also be considered. The uncertainty of CO2 RF, for example, though small in relative terms, is important due to the dominant contribution of that component. Leaving out these uncertainties will result in an overconstrained temperature range.

19. p6/169 “sampled at intervals of 0.04 W/m2” It should perhaps be mentioned that this sampling does not produce a plausible probabilistic distribution of the results, since the RF uncertainties cannot be assumed to be uniformly distributed. Since the authors do not make a probabilistic interpretation, this is not a major issue, however.

20. p7/189 “This shows. . .” due to the overconstraining mentioned, this result is not

valid in my opinion. Consequently the excercise described is only useful as an illustration of using the model framework, as stated in the following sentence.

21. p7/193 (section 4.1) It is possible to use an IRF for a specific component from another model, as the authors do, but I am not sure how meaningful this is, since this mixes the climate responses of two different models. To get a consistent model emulation the IRFs for the other RF components should, in principle, be taken from the same model (i.e., NorESM-1).

22. p8/239 “it demonstrates nonlinear dynamics” I find this claim unfounded since the nonlinearities in question concern the dynamics of a previously existing model, while the model component contributed by the authors cannot represent the relevant nonlinearity, i.e., that of the climate-carbon cycle feedback.

23. p9/253 (whole paragraph) “most of the linearities” - there is no finding about any specific nonlinearities, and the fact that the Hector SCM has a linear RF-temperature response is no basis for a recommendation for further model development. The near-linear relationship between RF and temperature is well known and has been demonstrated and exploited in SCMs for a long time (e.g., Joos and Bruno, 1996).

24. p9/271 “we demonstrate that the use of a forcing-based impulse response function overcomes most of these limitations.” I don’t see that any limitations are overcome by this approach.

25. p9/273 “These findings imply. . .” Again, there is no basis for such a recommendation.

Technical corrections

- p5/132 “In this experiment HIRM was configured” - The word “was” seems to be superfluous here. - In Table 1, the unit should be given.

References

- Joos and Bruno, 1996. Pulse response functions are cost-efficient tools to model the link between carbon emissions, atmospheric CO2 and global warming, Phys. Chem. Earth, 21, 471–476.

- Strassmann, K. M. and Joos, F.: The Bern Simple Climate Model (BernSCM) v1.0: an extensible and fully documented open-source re-implementation of the Bern reduced-form model for global carbon cycle–climate simulations, Geosci. Model Dev., 11, 1887–1908, https://doi.org/10.5194/gmd-11-1887-2018, 2018.

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