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Earth System Dynamics
Discussions

Interactive comment on "Climate model emulation in an integrated assessment framework: a case study for mitigation policies in the electricity sector" *by* A. M. Foley et al.

Anonymous Referee #2

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This is an interesting and generally well-written paper on developing climate model emulation although there is a mismatch between the model development and the application. Since, at least as implemented in this paper, mitigation by the electricity industry is dominated by CO2 changes, other approaches (simpler MAGIC-like models plus pattern scaling) would have produced near-identical results. An application using shorter-lived climate forcing agents may have allowed the power of the emulation methodology to have been explored in a more challenging application. And hence I feel the paper somewhat overstates what is gained from the emulation approach for this application.

Comments – where preceded by an M, this indicates major comment

1278:17. "in response to" – perhaps "associated with" is better. The global mean temperature change is an average of the diverse temperature patterns, not the driver of them.

M1278:16-20. I do not feel the paper ultimately establishes this for the case that is studied. Indeed, I was rather interested in this statement in the abstract (which implies that different scenarios lead to quite diverse precipitation responses) and then disappointed to find patterns that one might expect from CO2 dominated changes. So this sentence needs to be re-cast so as to reflect the reality of the results.

1279:10. GCM not defined (nor is AOGCM later – a systematic checking of acronyms would be useful)

1282:2. ESM?

M1282:5 non-CO2 radiative forcing is very ambiguous especially as we are told very late in the paper (1295:2) that the model lacks aerosol forcing (but then 1284:22 says aerosols are included). There is a need for greater clarity. If non-CO2 radiative forcing just means the non-CO2 well-mixed greenhouse gas forcings, then this should be said clearly, but it also needs to be spelt out how the emissions from the electricity industry influence aerosol and other short-lived climate forcings agents and hence what is potentially missing from this study. This is an example of the potential mismatch I noted in the introduction, as it is not clear that the model explicitly includes all the processes that are needed for a detailed examination, and this needs to be spelt out rather clearly but in the body of the paper and in the conclusions.

M1282:11. For clarity, is the full GENIE-1 simulator here the same as the GENIE-1 ESM referred to on line 1 of this page. A consistent terminology would help the reader.

M1284:21-22. I don't understand this on two levels, and suspect that it is the writing (rather than the model implementation) which is at fault. If not, then the authors need to discuss the major caveat in how the forcing is applied. First if all climate forcings (even

aerosols) are represented as perturbers of long-wave radiation, then important characteristics of the effect of, for example, aerosols (especially for precipitation changes) are lost (see 1295:2), and any spatial influence of the forcing on the response is lost, if a globally-uniform modification is applied (again see 1295:2), as this is not even the case for CO2 alone. This would be a potentially significant limitation to the model, especially for the application here, and this needs to be spelt out as a caveat more clearly. Second, methodologically I do not understand the apparent permanence of the modification of the outgoing longwave radiation. Radiative forcing changes the top of the atmosphere radiative budget in only a transient perturbation – the climate system responds (via warming) to eradicate the perturbation in radiative budget (and so globally it returns to zero in an equilibrium situation). If the forcing is applied as a permanent modification of the LW budget, then how does the longwave budget re-adjust following a warming? I could understand this more if the emissivities (which appear to be used in the Fanning and Weaver model) were instead modified.

M1285:1:16. I got very confused in this section, and the focus of my comments is on Equation 3. I apologise if the misunderstanding is all mine. If I apply Equation (3) with the stated parameters I generate some very strange time profiles of forcing; as R2 and R3 are zero at 2105, the forcing is dominated by R1 at this time leading to strange temporal variations, where the sign of the non-CO2 forcing suddenly changes in late century. I also easily generate mid-century peaks. In addition, the magnitudes seem odd, compared to the RCPs, as I easily generate forcings of magnitude 30 W m-2, which seem well out of a sensible range. So are the units of the R parameters wrong? Is Equation (3) wrong (I note in Holden and Edwards that the 0.5 embraces R1, R2 and R3 rather than just R1 here)? Not being a Chebyshev expert, I was also confused by the R3 parameter; lists I see in text books etc have 4x**3 - 3x, but perhaps this is what is meant by "modified" here?

1285:17 and 1285:21. I didnt understand what "were reproduced three times" and what "successfully" means. Could you clarify?

1286: 11. Is this time-series of concentrations or emissions?

1291:2 and 1291:11. I wasn't clear whether Figure 3 was emissions just from the power sector, or the different electricity scenarios on the total CO2 emissions. I guess the latter, as I could not see a 90% reduction on Figure 3.

1291:25-28:. These sentences seem contradictory – one says the appropriate non-CO2 RCP is chosen, but it then says that RCP8.5 non-CO2 is used for all scenarios. Which is it?

M1292:5-15. The implication here is that non-CO2 here means just methane and nitrous oxide? Is that correct? If so, what is the implication of just considering these non-CO2 gases rather than the wider mix including the short-lived pollutants?

1292:7. In principle, the correct application of equation (7) to obtain equivalent CO2 is to sum the forcings before calculating the equivalent CO2. There is a hint in the next sentence that the CO2 seen by the model is the sum of equivalent CO2's calculated individually for actual CO2 and non-CO2 forcings. Perhaps the difference is negligible, but it would be worth clarifying.

1294:5. "due to the effect of non-CO2 forcing" – does this mean via the carbon-cycle feedbacks in the model? I was unsure.

1295:12-14. Indeed, but this is not what is implied in the abstract, which is altogether more tantalising.

1295:8. This is a minor quibble in the context of this paper, but I do not think a strengthening of the Hadley Circulation is needed to generate this pattern. They emerge from the differences in water vapour amount in the atmosphere that follows (assuming fixed relative humidity) from the warming – in the absence of a circulation change, you still amplify the precipitation fields as more water is available in the convergence zones to condense. See e.g. http://dx.doi.org/10.1175/JCLI3990.1

1295:21. "demonstrates" – I think "suggests" is safer. I suspect that in the CMIP5

simulations it is the short-lived forcings that are important in modulating the precipitation pattern in the scenarios which are not CO2 dominated, but the model here cannot represent this.

1295:25. I am not familiar with the literature on the climate effects of mitigation in the electricity sector, but I would be surprised if there were not several studies using simpler model frameworks already. I might have expected some discussion in the conclusions about what has been learnt here which goes beyond these studies. If no such studies exist, it may be worth stressing this, as it would render this paper more original.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1277, 2015.