Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2018-17-RC2, 2018

Earth System Dynamics Discussions

Interactive comment on “Risk and the Point of No Return for Climate Action” *by* Matthias Aengenheyster et al.

Anonymous Referee #2

Received and published: 26 April 2018

The authors present a very simple climate-carbon cycle emulator that is tuned to CMIP5 ensemble simulations. They address the question of timing of the "point of no return" (PNR), i.e. the time when a certain mitigation policy will not longer guarantee that the global mean temperature increase relative to preindustrial stays below a given temperature target, e.g. 1.5 or 2°C with a certain probability. The simplicity of the model enables the authors to carry out large ensembles from which they can determine the PNR under different probabilities and targets. The authors venture to give precise years for PNR. This may actually be misleading given the host of uncertainties that are associated with such approaches, and mitigation projections in general. In principle, this is a valuable study that could eventually be published. However, I raise a few concerns and questions below, that the authors need to address, before I can

recommend publication.

Comments:

1. The new approach is essentially twofold: first a very simple deterministic model is developed that reproduces global characteristics of CMIP5, and second, emission pathways are given as an exponential increase at rate g (information not found in the paper: g=??) multiplied by a linearly decreasing factor (mitigation effect). In addition, negative emissions due to carbon capture and storage can be considered in this model framework. It would be useful to quantify the difference of the considered paths (11c) to an even more basic choice of just a simple exponential decrease of emissions at a constant rate from t_s onwards, as used by Stocker (2013). Obviously, the discontinuity of emission rates at t_s (increasing exponentially before, and then decreasing) are avoided here, but how would that matter for the PNR? Incidentally, for a given mitigation rate PNR can be read off Fig. 2A of Stocker (2013): it is the required starting time of emission reductions. Therefore, much of the information, which is the focus of the present paper, has been available already from an even simpler framework. This should be mentioned in the introduction.

2. Uncertainty is only substantively addressed in the text of the appendix. As this is a short text, I suggest to incorporate the appendix into the main text and amplify it. Regarding uncertainty, a general caveat would be useful in the abstract and the conclusion. Otherwise, the stated years of PNR are somewhat misleading.

3. A constant factor A in the forcing (8b) is used to optimize the agreement with CMIP5. The size of this factor is quite large (1.48). For \alpha_CO2 (in 8b) the correct value is taken (see Tab 3 - however inconsistent parameter notation - only \alpha there!). The authors justify the factor A with the existence of non-CO2 GHG drivers in the CMIP5 results (RCP scenarios), but the effects of these drivers have a time evolution and characteristic time scales that are very different from the primary driver CO2. So I don’t quite understand how is it possible to achieve a better match with CMIP5 by using

a simple scaling of (8b).

4. It is not clear, why in (11) both mitigation and abatement are used. Also, there is a conflict of parameters (a_0 in 6 and 11). Is 11b, i.e. a(t), really needed and relevant in this paper? I see no discussion in the text or the figures relating to the difference of m(t) and a(t) pathways. In fact, inspecting (11c) I can see no benefit why one would consider both mitigation and abatement. Both have the same linear time dependence, even the same rate. Therefore, the difference seems to lien in the quadratic (positive) contribution a(t)*m(t) to the emission factor, essentially (m1ˆ2)(t-t_s)ˆ2, presumably a rather small contribution. Therefore, for simplicity, I suggest that you would eliminate a(t) altogether, which would also remove the parameter conflict of a_0.

5. Further to the emission pathway described in 11c, I note that E_neg is included. However, it is not clear from the text, how Fig. 3 is constructed. From the rather short caption I surmise that this is taken from Rogelj et al., and then just prescribed here. This must be stated in section 2.3 more clearly.

6. You seem to consider only the strong negative emission of Fig. 3 for the calculation of PNR in Tab. 6. As this strong case appears nearly exponential in nature, I would suggest that you simply approximate the Rogelj negative emissions by an exponential and a starting time, and give it explicitly in eq 11 with its associated rate. This would eliminate Fig. 3, be more transparent for the reader and actually more consistent with the simple scenario approach that you chose in eq 11.

7. In order to construct ensembles, the mitigation rate m_1 is drawn from a Beta distribution. It would be helpful for the reader to have an explanation why this distribution is chosen and what difference a simple uniform or normal distribution would make.

8. Some noise is added to the model as stated on line 167ff. It seems of only minor relevance for the results (see Tab 5 and 6 - PNR changes only by about 1 year compared to the 50%-probability case). I wonder then why the addition of noise should be necessary at all. I cannot see any new insight from this. If you retain the noise, a more

detailed description would be necessary. In particular, the noise should be evident in eqs 10a and 10b as additional terms.

9. Table 5, 6, and 7 could be presented in a more effective way. Table 7 is trivial (just the difference Tab6 - Tab5) and could therefore be omitted. I further suggest to combine Tables 5 and 6 into one table. Each probability column should then contain two subcolumns, one without E_neg the other one with E_neg. The small difference caused by E_neg makes would then be directly visible.

10. In the appendix and in Tab. 8 some parameters (\gamma_0, r_\gamma) are listed without explanation. Where do they come from? Are they needed in this paper?

11. Line 374: please spell IPCC correctly. It is an edited document and that information is missing, as well as the total page number.

12. Figure 2: Put the 10ˆ3 factor into the label unit (1000 ppm).

13. Figure 2 and line 147. The discrepancy with the CMIP5 CO2 concentrations for RCP8.5 is quite worrying. This would imply that cumulative emissions will be way off, as well. The discrepancy for the forcing is removed by introducing the factor A, but what about CO2(t) and cumE(t)?? This must be addressed in a more convincing way.

14. Figure 6: Caption should be amplified by elaborating on the "different policies". You could add, e.g.: "... as described by m in eq 11, the rate of mitigation increase per year."

15. Figure 7: y-axis labels not complete.

Interactive comment on Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2018-17, 2018.