Earth System Dynamics Discussions

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Comment on esd-2022-39

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Referee comment on "Emit now, mitigate later? Earth system reversibility under overshoots of different magnitudes and durations" by Jörg Schwinger et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2022-39-RC1, 2022

The authors present results from a comprehensive set of 400-year long, idealized emission-driven climate-carbon simulations with the Norwegian Earth System Model to investigate the impacts of carbon dioxide removal (CDR) and a temporary exceedance of 2^{o}C warming for the global carbon budget, Earth system parameters such as global surface air-temperature, steric sea level rise, ocean heat content, Atlantic Meridional Overturning, ideal watermass age, ocean oxygen, pH, and productivity, permafrost extent and carbon stocks, and climate metrics such as TCRE.

In the reference simulations, CO2 emission increase and decrease and are phased out over the course of the simulation to a total of 1500 GtC. In overshoot scenarios, prescribed CO2 emissions are temporarily larger than in the reference simulation and these “excess” emissions are later compensated by negative emissions (net carbon dioxide removal (CDR)) to yield again cumulative emissions of 1500 GtC. The authors apply excess emissions/CDR of 250, 500, and 1000 GtC and two different timing for CDR in their setups.

The authors compare results between the reference simulation and the over-shot scenarios, all with equal total cumulative emissions, to investigate whether climate parameters and the carbon budget (atm, ocean, land biosphere C stocks) of the overshoot simulations approach those simulated in the reference towards the end of the 400-yr simulations. The authors denote a parameter change (e.g., Delta-SAT) reversible if the parameter value in the overshoot scenario is equal to that of the reference simulation within the bounds of internal, interannual variability for the last 11 years of the simulation. In other words, reversibility is only investigated for the amount of excess emissions/CDR applied – here 250 GtC in the “realistic case”, but not for the total anthropogenic CO2 emissions of 1500 to 2500 GtC.

Overshoot scenarios have been investigated in earlier studies, however, with Earth System Models of Intermediate Complexity. Here, the authors apply a state-of-the-art, comprehensive Earth System which also includes a module to represent permafrost extent and permafrost carbon. The authors apply the model in 11 different setups (control plus 10 scenarios) with some ensemble members and each simulation for a 400-year period. This represents a considerable effort in terms of computing time and human resources. The results are very interesting and comprehensively assessed for a broad range of global parameters. The manuscript is generally clear and well written.

I recommend publications after the following comments have been taken into account.

The comments should not require any new simulations or analyses, but, in my opinion, the text must be carefully revised in the abstract, introduction, and conclusion sections and adjusted elsewhere, to avoid misleading sentences and possible misinterpretations.

- 1) My main concern is related to how reversibility and irreversibility are defined and conclusions are framed. The concept of (ir)reversibility is applied in a very restricted sense in this study. As noted above, reversibility is only investigated for the extra emissions (above the reference case) and for later removal of these extra emissions by CDR, but not for total anthropogenic emissions. (This is o.k. per se, but must be more clearly explained in the abstract and elsewhere).

  Earlier studies have addressed reversibility relative to total anthropogenic carbon emission and found substantial irreversibility for atmospheric CO2, surface air temperature, ocean acidification and deoxygenation and other variables on multi-centennial time scales. For example, ESM simulations where emissions are suddenly stopped in the year 2000 or 2100 reveal large climate inertia and irreversibility (e.g., (Frölicher and Joos, 2010;Joos et al., 2011). Similarly, a CO2 pulse response model intercomparison study documents the long life time of atmospheric CO2 and climate perturbations and thus irreversibility in a hierarchy of models (Joos et al., 2013).

  Under the authors' narrow definition, reversibility is found for overshoots/CDR of up to 500 GtC and variables concerning the fast-reacting climate components such as surface air temperature and ocean surface pH. Properties related to the deeper ocean (steric sea level rise, OHC, deep ocean temp., oxygen, pH) and permafrost show irreversibility even under the authors' definition. Irreversibility for most variables is found for large overshoot/CDR (1000 GtC). The authors judge CDR of up to 250 GtC as "realistic" (e.g., around L 550). Thus, any emissions exceeding 250 GtC have an impact on SAT, CO2, .. that is irreversible on human, centennial time scales.

  The sentence in the abstract: *Many aspects of the Earth system including global average surface temperature, marine and terrestrial productivity, strength of the Atlantic meridional overturning circulation, surface ocean pH, surface O2 concentration, and permafrost extent are reversible on a centennial time scale except in the most extreme overshoot scenario considered in this study*" is very misleading. If cited out of context, the results of this study can be misused.

In brief, the authors must reword the abstract and conclusion sections and clearly explain in the introduction that reversibility/irreversibility in this study is restricted to the (relatively small) amount of overshoot emissions/CDR and that CO2 emissions (exceeding what can be plausibly removed by CDR) cause irreversible climate change.

- 2) The NoESM has a low TCR and a low TCRE compared to observation-constrained estimates (e.g., (Sherwood et al., 2020;Steinacher and Joos, 2016). Irreversibility and hysteresis has found to be higher for higher TCR and TCRE. The authors should discuss this aspect and consider it when making statement about reversibility.

Further comments

Abstract and L558: ".. it seems that reversibility might not be the main concern" This text must be reworded.
First, this statement hinges on the very narrow definition of reversibility applied in this study. (ir)reversibility in this study is only assessed for the carbon emissions that are later removed by CDR. It is only the (ir)reversibility for these "overshoot" emissions that are discussed in this study.
The authors estimate that up to 250 GtC may be removed by CDR and that larger removal is likely unrealistic. Thus, irreversibility for most of the emissions - 1500 to 2500 GtC in their baseline scenarios – is not assessed at all. This must be clearly stated in the abstract, conclusion, and elsewhere. Further, this statement is also subject to the low TCR and TCRE of the NorESM.

Methods: I suggest giving some more information in the method section on how permafrost is modelled and whether changes in vegetation distribution and associated feed backs are considered.

L113: Please give also an estimate of the model's TCRE and how this value compares to the TCRE of other models or as constrained in probabilistic approaches.

Line 121: It would be clearer to delete "in total" or say "Each of the six .." or similar

157: Please do not claim that the scenarios are designed to meet the Paris temperature goal(s). The Paris Agreement says: "keep global warming well below 2°C and to pursue efforts to limit it to 1.5°C". The wording "keep below" excludes, at least in my interpretation, warming equal or larger than 2°C. All scenarios, except the reference B-1500, exceed the 2oC limit and none of the scenarios meets the 1.5°C limit as shown in Fig. 1. Please reformulate the text (e.g. "Our scenarios are designed to eventually meet the 2^{o}C limit mentioned in the Paris Agreement after a period of overshoot (Delta-T> 2^{o}C) ..").

L171ff: The explanations on internal variability and the definition of reversibility are not fully clear. (I apologize that the comment is a bit picky. However, it would be good to be very clear on these concepts and how they are used)

- (L173-174) Please define more precisely how variances are computed. Do you use annual-average or monthly-average data or model output for each time step or something else to compute variances? Are variances computed from spatially-averaged time series to estimated internal variability for spatially averaged parameters or are variances first computed at each grid cell and then averaged? I guess you use annual data and compute variance from spatially-averaged time series.
- (L174-175) In the time of emergence approach cited by the authors, it is typically assumed that the threshold is 2*N/S, where S is the signal and N the noise. N is typically assumed to equal the standard deviation (sdv). Here the signal is compared with "the square root of the summed variances" In other words, the average variance from the three ensembles is multiplied with sqrt(3)=1.7. The authors may note that this factor 1.7 is slightly less than the 2*sdv used by others, but more conservative in the current application.
- (L171) "the ensemble mean of the overshoot simulation returns to the reference pathway within the range of internal variability". It may be good to say already here that 11-year averages from the overshoot and reference run are compared.
- L171: The climate system may also feature internal variability on time scales longer than interannual or seasonal variability. Do you assume in your approach that such longer-scale variability is always smaller than interannual variability?

L314: annual mean or annual maximum value of active layer depth?

L319: "which show that the physical state of permafrost is reversible under temperature reduction" Please add "in our model". Do you think that formation of thermokarst lakes, coastal zone erosion, and other landscape changes are adequately treated in your model to make a strong conclusion on permafrost reversibility? Do you consider dynamic vegetation changes in these simulations or is vegetation prescribed? Are albedo feedbacks associated with the potential transition from forest to tundra included in the model? Please elaborate on these points here and/or in the discussion.

L321: What about vegetation distribution, e.g. extent of different ecosystems such as boreal forest, tundra, tropical forests?

L330: Fig 9h -> Fig. 6h?

L369: typo: "As a mentioned"

L394: I find the following statement and the text on line 394 to 399 misleading: “We note, however, that negative emissions are indeed effective in reducing the rate of sea level rise to a value similar or lower than that of the reference simulation (Fig. 8c).” 8c only shows rates of sea level rise for the last 50 years. Earlier rates of change are, however, larger for the overshoot cases than for the reference case without overshoot. This should be made clear. Please add an additional panel in Fig. 8 showing the maximum rates of sea level rise over the course of each 400-yr long simulation.

L490 ff on tipping points: One could mention that the findings here are broadly in agreement with results from emission-driven EMIC studies or emission-driven ESM studies. It may also be interesting to mention the study by (Kleinen et al., 2020) who found methane to respond strongly to warming..

L558: What is a “realistic overshoot scenario”? Do you mean a scenario with only a limited amount of CDR (< 250 GtC). Please say so explicitly.

I hope these comments are useful. Thank you for executing this interesting study,
Fortunat Joos

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