Manuscript prepared for Earth Syst. Dynam.
with version 2015/04/24 7.83 Copernicus papers of the LaTeX class copernicus.cls.
Date: 26 September 2016

Referee comment from Kristoffer Rypdal

K. Rypdal

Department of Mathematics and Statistics, UiT The Arctic University of Norway, Norway

Correspondence to: Kristoffer Rypdal (kristoffer.rypdal@uit.no)

1 General comments

I agree with most comments of Referee #1, although I don't think the issue of tipping points is so serious. I think it is acceptable to do the analysis under the assumption that no major tipping points will be attained in the relevant period, since any analysis of this type would be impossible in that case, unless one has a completely reliable nonlinear model of the climate system. And, the authors *do* in fact make an attempt to include the effect of nonlinearity and the presence of a tipping point in their analysis of the energy balance model (EBM). In their bistable version of the EBM, they find some effect on the point of no return (PNR) from the reduced stability due to the presence of nearby equilibria. My main criticism here is not that they don't recognise the impact of tipping points, but the lack of realism of their EBM.

The first paragraph of the report of Referee #1 gives a brief description and assessment of the goals and methods of the paper, which I find no reason to repeat here. Referee #1 points out the presence of non-recognised key simplifying assumptions as a major weakness of the paper. There are, however, other and more serious key simplifying assumptions that are not recognised and discussed in this paper, and which strongly limits its potential utility. And worse, some may be right out misleading and harmful if they are adopted by policy makers.

The most serious assumption is the totally unrealistic form of the mitigation scenarios used, which ignore the existence of a carbon cycle. The authors assume implicitly that the atmospheric CO_2 concentration can be manipulated directly. One consequence is that the point of no return (PNR) by definition in this paper always occurs *after* the climate state has become non-viable. In the real climate system, the PNR will occur *before* the climate has become non-viable, which makes a profound difference.

The EBM employed actually describes the Earth's climate in an icehouse state. It should be replaced by a more realistic model that displays bistability, e.g., a model for the small icecap instability.

The analysis of the cost function is inadequate, and a correct analysis would reveal the shortcomings of assuming that the mitigation action comes after the viability limit is breached.

2 Specific comments

2.1 How to use simplified models

There is nothing wrong in using super-simple models for the global climate response in studies of the type presented in this paper. In fact, apart from an enormous reduction of computational cost, well chosen conceptual models are often more correct than some general circulation models (GCMs) in projecting the global mean surface temperature (GMST). In Rypdal and Rypdal (2014)[1] we show that a simple, linear response model (with a power-law Green's function and deterministic and stochastic forcing components) give results for the GMST in the instrumental period that are indistinguishable from those of the CMIP5 archive (Fig. 15 in that paper). The realism of the model is established by the power-law form of the Green's function which reflects the long-range memory (LRM) in the climate response and that the parameters of the model are estimated from observational data for radiative forcing and instrumental GMST. The EBM employed in this paper as well as the PLASIM GCM lack this memory in the response. Both operate with a mixed-layer ocean which yield an exponentially decaying impulse response of the GMST with time constant from a few years to a decade or two. This makes them miss the "warming in the pipeline" associated with the heat transport from the mixed layer into the deep ocean. By leaving out this delay in the GMST response to forcing, the GMST will decay faster after atmospheric CO_2 concentration is reduced. The effect is illustrated in Fig. 1 below, where the GMST in a full-blown

[1] M. Rypdal and K. Rypdal, Long-memory effects in linear-response models of Earth's temperature and implications for future global warming, J. Climate, 27, 5240-5258, doi:10.1175/JCLI-D-13-00296.1, 2014

atmospheric-ocean GCM fails to stabilise at a new equilibrium even centuries after a step-function rise in CO_2 concentration. In contrast, the PLASIM model would stabilise at a new constant GMST after a few years.

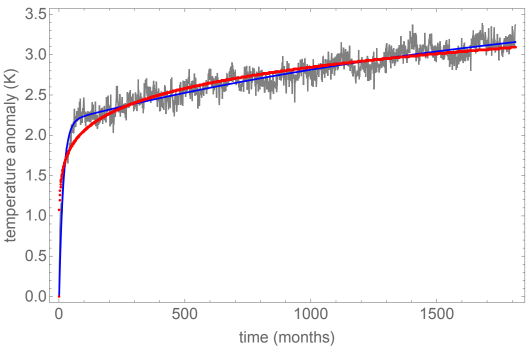

Figure 1. Grey curve is the global temperature response to a sudden 4-doubling of atmospheric CO_2 concentration in the GISS-E2-R model. Blue curve is a fit of superposition of two exponential responses (two-box model solutions); the two exponential time constants being $\tau_1 = 1.3$ yr and $\tau_2 = 176$ yr. Red curve is a power-law fit, and is a poor fit up to several years, but a good fit in longer time scales. Note that temperature will continue to rise for hundreds of years.

An even more serious flaw, however, is the implementation of mitigation scenarios in the form of exponentially decaying CO_2 concentration with e-folding time of 9 and 25 years. This is at odds with anything we know about the carbon cycle and with any realistic emission scenario, even with carbon capture and storage (CCS) implemented on a scale that is economically feasible. I recommend the authors to take a look at my recent ESD paper[2] where I study CO_2 and GMST projections for some idealised (but realistic) emission scenarios by very simple models for the responses. The emission scenarios shown in Fig. 2, display the annual emissions when mitigation measures come into action in years 2030, 2070, and 2110, respectively. The base scenario (blue curve) is a business as usual (BAU) scenario that continues the present-day exponential rise. It is close to the RCP8.5 scenario up to 2070. The mitigation actions considered are an annual reduction of emissions of 1% and 5%, respectively. Economic studies indicate that higher annual reductions than 5% can not be attained without disrupting the global economy, while 1% seems to be a realistic upper limit. 5% reduction corresponds to an e-folding time of about 14 years, while 1% corresponds to 70 years.

The crucial point, however, is that an exponential decay of the emission rate does not correspond to an exponential decay of the atmospheric CO_2 concentration due to the long

[2] K. Rypdal, Global warming projections derived from an observation-based minimal model, Earth Syst. Dynam.,7, 51-70, 2016, doi:10.5194/esd-7-51-2016

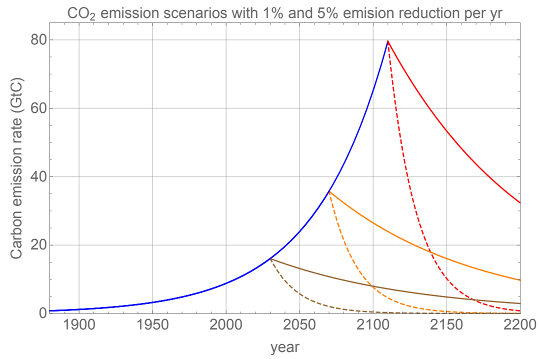

Figure 2. Blue curve is carbon emission rate $R(t)$ scenario obtained by fitting the exponential $S_0 \exp gt$ to the emission rate 4 GtC/yr in 1960 and 11 GtC/yr in 2010 AD. The full, brown, orange, and red curves are the subsequent $R(t)$ after initiation of 1% reduction of emission rate per year. The dashed curves are corresponding rates with 5% reduction per year.

residence time of atmospheric CO_2. The modeled evolution of the CO_2 concentration from the described emission scenarios is shown in Fig. 3. It is apparent that these mitigation scenarios (even in the extreme 5% reduction case) are far less radical than those considered by the authors of the paper under review. In fact, for economically and politically realistic mitigation scenarios (1% annual reductions of emissions), the CO_2 concentration will continue to rise monotonically beyond year 2200.

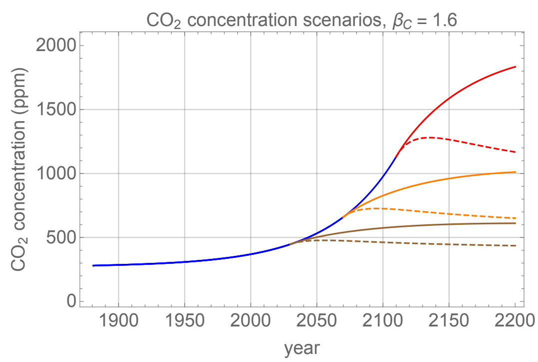

Figure 3. Projections of CO_2 concentration under the emission scenarios in Fig. 2 using the simple response model for CO_2 proposed in K. Rypdal (2016)[2].

When memory effects in the GMST response are taken into account the GMST projections look even more bleak. If 2° GMST rise is taken as the viability limit, none of the emission scenarios I have considered will prevent a monotonically rising GMST beyond year 2200 if mitigation action is taken at a time t_c after the time t_b when this limit is breached. Hence, by assuming that $t_c > t_b$ there is no useful way to define the PNR. It will not exist for a realistic set

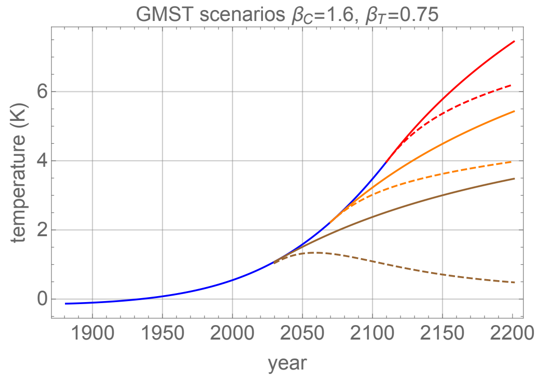

Figure 4. Projections of GMST under the CO_2 concentration scenarios shown in Fig. 2, using a linear model for the GMST response proposed in K. Rypdal (2016)[2].

of mitigation scenarios unless one defines the tolerance time τ_T to be several centuries, which is, of course, of no interest to policy makers. One may argue of course, that the projections I present in Figs. 3 and 4 are too pessimistic. However, in the ESD-paper[2] I present also some projections that most probably are too optimistic (atmospheric CO_2 half-life of 33 years, and weak memory in the GMST response). But also with these projections the GMST will continue to rise beyond year 2100 if $t_c > t_b$.

My conclusion is that if the authors want to deal with the real world they should employ more realistic mitigation scenarios. Mitigation measures should indicate emission, not atmospheric concentration, and some model for the carbon cycle is necessary. Models for the GMST response to CO_2 forcing should take delayed responses into account. A two-box model including the deep ocean in addition to the mixed layer is an alternative to the power-law response used by us.[1] The PLASIM model is not adequate for this purpose, and the CMIP5 model ensemble is too small to be useful for a statistical study.

2.2 Science-fiction scenarios

One can always discuss the utility of studying scenarios that seem impossible with today's technologies, such as rapid depletion of the atmospheric CO_2 content, and super-optimistic assumptions about the memory in the GMST response. But a minimum requirement in a paper like this is a discussion of the realism of the mitigation scenarios considered. As an input to such a discussion I show in Figs. 5 and 6 the radiative forcing and the GMST resulting from abrupt, discontinuous transitions to a zero emission regime in years 2030, 2070, and 2110, respectively (in these plots zero time corresponds to year 1880). Note that in Fig. 5 I plot the forcing, and not the CO_2 concentration. The model used to produce these projections assumes that, after emissions are cut to zero, a certain fraction of excess atmospheric CO_2 content is removed from the atmosphere, such that the concentration decays exponentially towards preindustrial level with e-folding time of 33 years. This time constant was estimated from the historic emission record and the Mauna Lua CO_2 concentration record, using the exponential-decay model.[2] It is well known from carbon-cycle models that the exponential decay is unrealistic on time scale of centuries and longer because the uptake in oceans and vegetation will saturate, but this model can serve as a sci-fi limit. The model also assumes a memory exponent $\beta = 0.35$ for the GMST response which is half the value estimated from instrumental data.[1]

We observe form Figs. 5 and 6 that even in this totally unrealistic scenario the e-folding time for the forcing is around 50 years, and for GMST about 70 years. In comparison, the CO_2 concentration and GMST scenarios studied by the authors in Section 4.2 have e-folding time of 25 years.

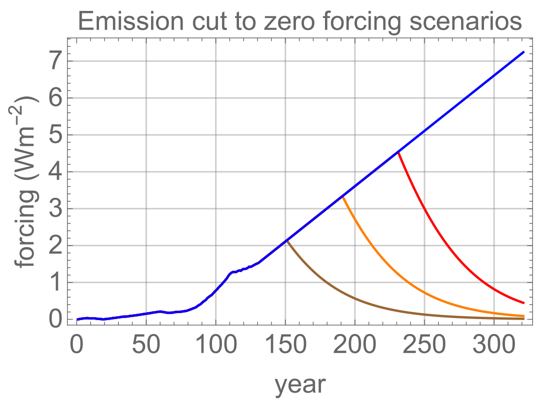

Figure 5. Projections of CO_2 forcing under emission scenarios with abrupt (step function) transition to zero emission, using an exponential-decay model with e-folding time 33 yr for CO_2 concentration.

2.3 The energy balance model

The Budyko EBM employed by the authors, with a temperature-dependent albedo as the main nonlinearity, is useful to illustrate the possibility of an abrupt transition between a "icehouse Earth" state and a "greenhouse Earth" state with small or no icecaps. The authors consider two albedo profiles one with $\alpha_1 = 0.2$ in the warm state, and one with $\alpha_1 = 0.45$ in the warm state. In both cases $\alpha_0 = 0.7$ in the cold state. For the former the model yields on one stable branch with GMST of about 255 K. This monostable model is unrealistic because of the high albedo α_1 chosen for high GMST. There is also no explanation for why the emissivity is set to $\epsilon = 1$, while the true value is around 0.6. If α_1 is reduced beyond a certain point there will also be a warmer stable branch and the two stable branches are connected by an unstable branch. The authors have decided that they want the present-day climate to reside on the cold branch, so they sim-

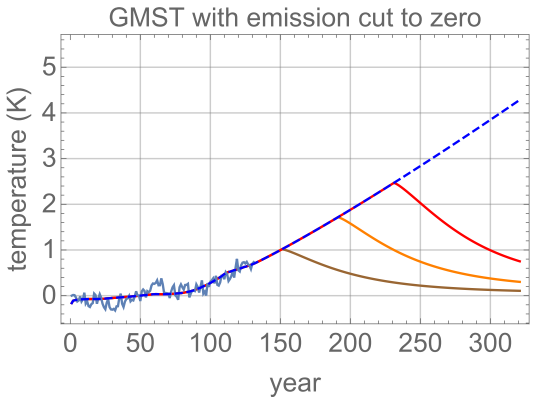

Figure 6. Projections of GMST under the forcing scenario of Fig. 5 and a low-memory model for the GMST response. The start year is 1880. The jagged curve is the historic instrumental GMST for 1880-2010.

ply shift the temperature up by 30 degrees and pretend that this cold branch in this model can be used to describe 21st century climate. The fact is that this model (with $\alpha_1 \approx 0.3$) is a reasonable model for our climate if we assume that it resides on the warm branch. The authors do not explain their reasoning, but I suspect that they want a model which displays an additional warm state without polar icecaps which can be attained via an abrupt transition in a bifurcation diagram. If so, they should study a model designed for this purpose. For instance one that contains a “small icecap instability."[3]

2.4 The model for and analysis of the cost function

I agree with referee #1 that this model is too simplistic, and in particular assumption (ii) seems unjustified. But also the analysis seems flawed an pointless. Instead of analysing the cost function $\Psi(\lambda,\Delta t)$ as a function of two variables, $\lambda(=C_{st})$ and Δt, the authors find a minimum of Ψ at a certain λ_{min} under the arbitrary constraint $\Delta t = 4$ years. Then they plot $\Psi(\lambda_{min},\Delta t = 4)$ in the range $\Delta t \in (4,28)$ and find (perhaps?) a local maximum close to the upper end of the range ($\Delta t = 28$ corresponds to the PNR). It would be reasonable that the cost increases as the PNR is approached, since the time the system is non-viable becomes longer. But an optimal mitigation strategy should search for the value of $(\lambda,\Delta t)$ that *minimises* Ψ, and a plot of the surface $\Psi(\lambda,\Delta t)$ (also for $\Delta t < 4$ years, would probably tell us that the minimum is at $\Delta t = 0$. The authors' formulation of the problem requires that $\Delta t \geq 0$, so this would tell us that the optimal mitigation strategy is to act immediately after the viability limit is breached. But a formulation that allows action before t_b would probably minimise cost at a negative Δt.

3 Some minor points

Page 6, lines 163-167: The variance σ_s^2 of the noisy forcing in the stochastic differential equation Eq. (4) should be determined from comparing the variance it produces in T with the observed variance in the detrended instrumental GMST. The description of how it is determined is obscure, and the authors do not give the value they find. The authors should provide more detail.
Figure 3: I have problems grasping the meaning and significance of this figure. Maybe plots of the evolution of the confidence interval of $p(x,t))$ for different values of T_0 and C_0 could be helpful?
Page 10, lines 261-263: Discuss why the seasonal cycle has this radical effect.
Page 15, line 384: The authors highlight the use of linear response theory as their key innovation. In the description given in Section 4.1 they determine Green's functions for the mean $\langle T\rangle$ and the variance $\langle (T-\langle T\rangle)^2\rangle$ separately from the ensemble of PLASIM runs with step function forcing, assuming that both respond linearly to forcing. The result and the linearity assumption are tested on an ensemble of PLASIM runs subject to the transient 1% forcing scenario up to CO_2-doubling. The mean is predicted quite accurately, but the variance not that well.

A related method could be to assume a parametrised model for the response function $G(t)$(for instance $G(t) = \alpha t^{\beta/2-1}$) and write the convolution as a stochastic integral;[1]

$$T(t) = T_0 + \int_0^t G(t-t')[f(t')\,dt' + \sigma_s dW_{t'}].$$

The parameters α, β and σ_s can be estimated from empirical data or model runs. The advantage is that there is only one Green's function (not separate for mean and variance), and that parameters can be estimated even if there is only one realisation in the ensemble. Maybe the authors could comment on the advantages and disadvantages of these different approaches?

4 Recommendation

The weaknesses of this paper are the unrealistic mitigation scenarios, which influence the formulation of the PNR problem, and the lack of discussion of the relevance of these scenarios and the models employed (the EBM, PLASIM, and the cost function). A paper published in ESD should not merely be a mathematical exercise, but should contribute to understanding of the Earth system. I see two possibilities for revision: (i) The authors add a thorough and honest discussion of the limitations of the validity of their work, without

[3]T. J. W. Wagner and I. Eisenmann, How Climate Model Complexity Influences Sea Ice Stability, J. Climate, 28, 3998-4014, 2015, DOI: 10.1175/JCLI-D-14-00654.1

adding to much new analysis. (ii) They make a major revision which includes more realistic scenarios and models, and a reformulation of the PNR concept that includes the possibility $t_c < t_b$. The latter option would of course result in a much more interesting paper.

Acknowledgements. I thank Hege-Beate Fredriksen, Martin Rypdal, Per Jakobsen, and Ola Løvsletten for useful discussions.