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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Thermodynamics of climate change: generalized sensitivities" *by* V. Lucarini et al.

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[See Supplement for new Figures]

The manuscript presents a novel approach to climate sensitivity focusing on the thermodynamics and shows some evidence of its potential utility by using a simplified circulation model. While this approach seems to be very promising I think the authors should provide more evidence of the success of the predictions of this approach. I think this is actually one of the major advantages of using simplified models that one is able to thoroughly check new ideas and discuss there strengths and weaknesses. Before the manuscript can be accepted for publication the authors should provide more evidence of the validity of their predictions. I recommend a publication of the manuscript after a major revision and the authors provide more evidence for the utility of their approach

and all my below comments are satisfied.

We wish to thank the referee for the insightful comments, which have stimulated us to improve the paper. In the new version of the mansucript, we have tried to improve the discussion of the physical properties of the system by combining information on the global thermodynamical properties with more detailed analysis in the changes of some relevant fields (temperature, surface winds, latent heat forcing). We have shown that the areas where on the average positive and negative heat balances are found are well distinct and the way they change in altered conditions explain well the observed changes in the global thermodynamical properties of the system.

1) The authors claim that their approach predicts weaker surface winds in a warmer climate. Is this indeed the model response? The authors should provide some evidence for their claim like figures of the surface wind fields. Does the wind strength also changes almost linearly with CO2 concentration? The authors should also provide some evidence for the predicted changes in vertical temperature gradient.

We have added figures and comment to take care of these important points and related them to the thermodynamic quantities we have computed. We were not exactly interested in studying the linearity of the surface winds, but rather found it useful to prove that surface dissipation does indeed decrease in a warmer climate, and geographical features have been evidenced. The largest changes occur in the mid-latitudes in the SH. The wind stress is greatly reduced in the 1000 ppm with respect to the 100 ppm simulation, whereas changes between the 1000 and the 350 ppm suggest that ACC is reinforced (so that wind stress locally increases), but overall a decrease is realized. See Figs 3, 4, 6 and related comments.

2) The surface temperature response in Fig. 1 does not seem to be very linear to me. While the generalised sensitivities seem to be well approximated by straight lines the quantity of main interest for many scientists, policy and decision makers, the global mean surface temperature, is not. This should be more clearly stated in the manuscript.

The different responses of the temperatures makes it also questionable if a simple linear relationship is sufficient to re-parameterize the different thermodynamic quantities.

We have now specified that the change is linear with the log of the CO2 concentration for concentrations larger than 100 ppm. Anyway, the crucial point is that we have smooth monotonic dependence of the variables on the carbon dioxite concentration..

3) Does the statement that the system becomes less efficient with increasing temperatures mean that the storms/storm tracks become weaker? Does this in turn imply that more heat is transported as latent heat? Some more evidence for these predictions would be very much appreciated; e.g. figures of the storm tracks and heat fluxes.

We have partially addressed this point by showing that changes latent heat fluxes are the main contributors to changing the temperature structure and the entropy production of the system. See Fig. 5 and related comments. We will tackle the problem of studying the relationship between intensity of the storm tracks and thermodynamic efficiency in a more complete way.

4) What is the difference between S and s in Eq. 2? s is nowhere defined.

This has now been taken care of: capital S refers to the integrated entropy, small refers to entropy density.

5) In section 4 you refer to the appendix which is missing in the manuscript.

We apologise, this has been corrected.

6) In the last paragraph of section 5 the authors mention changes of the solar constant which seems to be a misnomer. The 'solar constant' is a constant, however, what it describes, the solar irradiance, can chance and is one of the main causes of paleoclimate variation.

We totally agree, this was incorrect and has now been taken care of.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C2392/2010/acpd-10-C2392-2010-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 3699, 2010.