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Earth System Dynamics Discussions

Interactive comment on "Why CO_2 cools the middle atmosphere – a consolidating model perspective" *by* H. F. Goessling and S. Bathiany

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Assessment of the manuscript

The study aims at giving an educational perspective of the stratospheric cooling under increased CO_2 concentrations. The paper is somewhat unique in that it is neither a classical research paper that puts forward new results or a new piece of theory, nor a review paper that purely synthetizes past findings. Instead the paper uses a simple model to illustrate different effects that contributes to the stratospheric cooling under increased CO_2 concentrations. The results obtained with this simple model are not exceedingly interesting for people who worked with such models before. However, as pointed out by the authors, a publication with such a simple model on the topic is lacking

and therefore the approach to the problem can be considered novel. The study will thus certainly be very useful to scientists that are interested in the topic but not particularly familiar with radiative transfer, especially since the manuscript is well written and easy to follow. The study, however, still lacks a detailed discussion of how a convectively dominated troposphere affects the stratospheric cooling under increased CO_2 concentrations. Without such a discussion, it is hard to evaluate in how far the effects described with the local-radiative-equilibrium model are responsible for the stratospheric cooling in comprehensive models. Therefore, I recommend publication after revising the manuscript to this end.

General comments

The study makes use of a radiative-equilibrium model for the entirety of the atmosphere. These model are usually rather referred to as "local-radiative-equilibrium models" than as "radiative-equilibrium models", to indicate that the atmosphere is in radiative equilibrium everywhere. Such models give good results for optically thin regions of the atmosphere. However, the quality of results is difficult to assess in regions that are strongly influenced by convection such as Earth's troposphere. Changes in Earth's troposphere will most certainly also affect the response of the stratosphere to an increase in CO_2. Therefore, the manuscript would be strengthened, if the local-radiative-equilibrium model was discussed in more detail in the context of present day Earth, and if the role of the troposphere in cooling the stratosphere was quantitatively addressed.

Here are a few suggestions on how this could be done and for some potentially interesting aspects to discuss:

An easy way of incorporating the troposphere into the gray version of the model is to define the tropopause at the level $h = h_{TP}$. Applying this definition to equation (8) of the manuscript yields

$$L^{\uparrow}_{TOA} = \frac{L^{\uparrow}_{TP}}{\frac{\alpha}{2}(1-h_{TP})+1} \quad , \tag{1}$$

where $L^{\uparrow}_{TP}$ is the upward longwave flux at the tropopause. Applying this expression for $L^{\uparrow}_{TOA}$ to equation (10) of the manuscript yields

$$T(h) = \sqrt[4]{\frac{L^{\uparrow}_{TP}}{\sigma}\frac{\alpha(1-h)+1}{\alpha(1-h_{TP})+2}} \quad , \tag{2}$$

or in the case with a window

$$T(h) = \sqrt[4]{\frac{O^{\uparrow}_{TP}}{(1-\beta_W)\sigma}\frac{\alpha_O(1-h)+1}{\alpha_O(1-h_{TP})+2}} \quad . \tag{3}$$

These equations can now serve as a starting point for the investigation of the influence of the upwelling longwave-radiation from the troposphere on the stratospheric response to an increase in CO_2 concentrations. This formulation decouples the stratosphere from the surface temperature and allows for different assumptions on how the changes at the surface impact the upwelling longwave radiation at the tropopause. A potentially interesting case to study would for example be the one of constant tropopause temperature, which is a common feature in climate-change simulations. Another interesting consideration would be, how the deepening of the troposphere and hence the increase of h_{TP} influences the results. The authors could also apply some of the values they obtained from the simulations with ECHAM6 to $L^{\uparrow}_{TP}$, $O^{\uparrow}_{TP}$ h_{TP}, T_{TP} ect..

A discussion of the troposphere would furthermore allow to discuss changes in albedo A through changes in clouds. With A included, the TOA radiative balance simply writes as

$$L^{\uparrow}_{TOA} = (1-A)S \quad . \tag{4}$$

Changes in A would also lead to a change in the steady-state stratospheric temperature even in the gray case without window.

Specific comments

Page 2, Line 27: It would be helpful for interested readers, if the relevant Chapter in Goody and Yung (1989) would be indicated.

Page 4, equation (1): It may be worth mentioning, that equation (1) is obtained by making an assumption on mean angle of the thermal radiation against the vertical.

Page 5, Line 15: Equation (3) is the spectrally integrated, gray-absorption-case of Schwarzschild's equation.

Page 6, Lines 14-17: The last sentence of this paragraph suggests that the local radiative equilibrium is a decent approximation for present-day Earth. This is generally not the case. Despite sharp gradients existing close to the surface on Earth, the discontinuity and the lapse rate of the local-radiative-equilibrium model are by far too large. I suggest mentioning this.

Page 10: I suggest to introduce equation (35) just after equation (31), because equation (35) was quite important for me to understand the following discussion.

Page 10, Line 17 to Page 11, Line 1: This is one of the situations where a comparison between the local-radiative-equilibrium model and Earth is awkward. The stratosphere and the tropopause do not adjust at the same time-scale.

Page 11, Line 26: It may be worth mentioning that this is entirely a consequence of the outgoing longwave radiation (OLR) having to balance S, which is constant in steady state. As a consequence, even in this simple model a solar and a greenhouse forcing act differently: An increase in S would force the OLR to increase as well and, as a consequence, the Skin-temperature would have to increase. In contrast, with this simple model the OLR does not change when CO_2 is applied.

Page 12, Line 11: The definition of "blocking effect" may be a bit confusing, because the authors already discussed the “increased blocking” in the window-less case. Speaking of "transient blocking effect" and "equilibrium blocking effect" may be a possible way of resolving the possible confusion. Note, that it is also arguable that in this case the "blocking effect" applies to present-day Earth, because the surface temperature on Earth has not equilibrated yet.

Page 17, Line 8: 6,5 K / 1000 m instead of 6,5 K.

Page 17, Lines 7-9: I am not sure whether it can be stated that there is an upper limit of the lapse rate of 6,5 K / 1000m. If, for example, the global-mean surface-temperature would change, the lapse rate may change as well. The fact, that the global-mean lapse rate is 6,5 K / 1000 m, does not necessarily mean that this value is a an upper global-mean limit.

Page 17, Lines 11-27: I suggest replacing this paragraph, by a discussion along the lines made in the general comments. I doubt that much meaningful can be learnt from applying typical values for present-day Earth to the local-radiative-equilibrium model without decoupling the surface-temperature from the TOA-temperature to some degree.

Page 18, Line 19: I suggest replacing “confirmed” by “supported”. Model results are no observations.

Page 20, Line 14 to Page 21, Line 2: The value of α_O depends on how the transition from opaque to window region is defined. I am therefore not sure, how well the ECHAM6 results can be applied to the local-radiative-equilibrium model.

Page 21, Lines 17-23: Even with prescribed SST the troposphere may respond to counter the forcing. Therefore, it would be helpful to make additional columns in Table

I for the OLR and maybe also for the albedo. This is exactly the type of effect that could be more closely investigated with the methods suggested in the general comments.

Page 22, Line 1-2: I suggest weakening the first statement in the paragraph for three reasons. The statements, that this is possibly the simplest way to explain the effect, that the explanation is complete and that the explanation is physically correct, are too strong. In my opinion a more fitting description of the paper would be: “In this article, we try to explain a well-known phenomenon that is central to our general understanding of climate change – cooling of the middle atmosphere by CO_2 – in a simple but physically consistent way”.

Page 23, Line 8: I suggest changing “the physical essence” in the last sentence, because the manuscript does not allow to exclude other potentially essential mechanisms leading to a stratospheric cooling.

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