Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2016-8-AC2, 2016

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

H. F. Goessling and S. Bathiany

helge.goessling@awi.de

Received and published: 2 July 2016

COMMENT: The study aims at giving an educational perspective of the stratospheric cooling under increased CO2 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 CO2 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

Printer-friendly version

Discussion paper

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 CO2 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.

REPLY: We thank M. Popp for these very constructive comments. We are happy that the intended purpose of our paper seems to be clear. We will comment on the role of convection below.

COMMENT: 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 CO2. 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 = hTP . Applying this definition to equation (8) of the manuscript yields (....) 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 CO2 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 hTP influences the results. The authors could also apply some of the values they obtained from the simulations with ECHAM6 to .... 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 (....) Changes in A would also lead to a change in the steady-state stratospheric temperature even in the gray case without window.

REPLY: The role of the assumption of local radiative equilibrium in our model is indeed an important point which we will make clearer in the revised manuscript. The reviewer wonders how adjustments in the structure of the troposphere during the time of surface warming, in particular a change in the tropopause height, affect our results. Interestingly, these adjustments of the troposphere have no substantial impact on the stratospheric temperature profile, and thus also the contribution of effects. This fact manifests itself in Fig. 9: Above a height of 20 km, there is no difference between the simulations with fixed surface temperature, and the simulations where the surface-troposphere system has adjusted. Hence, the temperature profile of the middle and upper atmosphere only depends on the local chemical composition, and not on the temperature profile in the troposphere. Above the tropopause, the only difference between a simulation with a surface in equilibrium and a fixed SST simulation is a small additional upwelling long-wave flux of approx. 3.5 W/m2. This additional flux however has no impact on the stratosphere because it goes through the atmospheric window. All absorption has happened at lower altitudes. Stratospheric temperature, and the absorption of long-wave radiation thus do not differ between the simulations. This result is very generic and not specific to our simulations. To convince ourselves and the reviewer, we also looked into the models used for CMIP5. They show the same behaviour: When CO2 is quadrupled instantly (simulation abrupt4xCO2), the tropospheric temperature profile remains very close to pre-industrial conditions during the first model years due to the ocean's inertia, but the profile above the tropopause shows a large cooling. Thereafter, the surface-troposphere system slowly warms, while the temperature profile above does not change anymore over time. This result is also in line with previous studies. Most importantly, Forster et al., 1997 use a radiative-convective model and show that the radiative forcing by CO_2 depends on the definition of the tropopause. While this affects the response of the surface-troposphere system, the temperature profile above is not affected by the definition of the tropopause (see their Fig. 8a). A similar argumentation applies to changes in surface albedo: The latter would affect the surface temperature directly and lead to an adjustment of the troposphere, but not to any adjustment of the middle atmosphere as the chemical composition there remains fixed. As changes in the tropospheric profile over time obviously do not matter for the middle atmosphere, biases in the tropospheric profile will not affect our results either. We agree that our assumption of radiative equilibrium is wrong in the sense that it yields an unrealistic temperature profile in the lower atmosphere. However, the aim of our analysis is to explain the response of the middle atmosphere. In these heights, the assumption of radiative equilibrium is much better justified, and our model is thus suitable for its purpose even though it does not have a troposphere. It is an interesting point put forward by the reviewer that one could therefore apply our model to the middle atmosphere directly, with the upwelling long-wave flux at the tropopause as a lower boundary condition. We are certainly going to discuss this in the revised manuscript and take up the modified equations suggested by the reviewer. As reviewer 2 pointed out that the insensitivity of the middle atmosphere to climate change in the surface-troposphere system is not a surprising result and should not be presented as such, we will not elaborate on this result too much in the revised manuscript and see little benefit from an extended derivation of sensitivities to changes in tropopause height or temperature due to the arguments outlined above. As our model is very simple in many ways, it does not allow a quantitative separation of the two reasons we discuss (as well as the transient part of the blocking effect). The lack of a troposphere is not the only reason for this as there are many other simplifications in the model. Applying the simple model only to the stratosphere will therefore not suffice to separate the effects quantitatively. This is the reason we apply the general circulation model, which shows that the transient effect is indeed practically absent, justifying the value of our model. We will make this clearer in the revised manuscript.

COMMENT: 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.

REPLY: We will mention all three points in the revised manuscript.

COMMENT: 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.

REPLY: We intended to say exactly this – we will make it clearer in the revised manuscript.

COMMENT: 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.

REPLY: Our intention with the current structure was to first explain the mechanism of the blocking effect, and then how it changes over time in the model. From our own perspective, Eq. (35) is not so important to understand the text before, but we can easily move the paragraph on page 13, lines 5-13 to the beginning of the section (starting on page 10, line 16). We hope that this will improve the clarity of our paper.

COMMENT: 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

Printer-friendly version

Discussion paper

stratosphere and the tropopause do not adjust at the same time-scale.

REPLY: This separation of time scales is exactly what we analyse here. Compared to the surface temperature that changes over decades, the atmospheric radiation field responds very quickly to composition changes. Indeed, the limitation of the model is that there is no tropopause, and we will make this clear again at this point. However, the separation of time scales can still be discussed in the model by keeping the surface temperature fixed as we do in this section.

COMMENT: 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 CO2 is applied.

REPLY: We fully agree with this comment and will take it up in the revised version of the manuscript.

COMMENT: 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.

REPLY: We understand that the naming of effects can be confusing, and had in fact changed the wording also before submission of the manuscript. We decided to name the effects based on their physical mechanism (blocking effect and solar effect). We will make clearer where we address the equilibrium blocking effect, and where the transient contribution. The parallel to the present-day Earth is indeed noteworthy, though it should also be pointed out that we change CO2 abruptly in the model, while it accumulates slowly in reality. Moreover, there is hardly any transient contribution of the blocking effect in complex models (see discussion of main comments above).

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

REPLY: We will correct this mistake in the revised manuscript.

COMMENT: 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.

REPLY: We agree with this comment, and will rephrase this to something like "Convection acts to reduce the lapse rate considerably, to approx. 6.5K/km in the current climate".

COMMENT: 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.

REPLY: We understand that this discussion can appear too speculative. What we mean to discuss here is the fact that the surface temperature response in the simple model is too large relative to the stratospheric response compared to more realistic models. This might be assumed to just result from the negligence of convection. However, we argue that the vertically inhomogeneous distribution of water vapour must be an aspect that cannot be neglected. The atmospheric window above the tropopause is much larger than at the surface. We use the model to demonstrate that this makes a huge difference for the ratio of the temperature responses. We think that this result is meaningful despite the negligence of convection because convection does not change the fact that water vapour is most abundant near the ground. We will certainly rephrase this section to make this clearer, and will include the discussion of the model as applied to the middle atmosphere only. To this end, we will remove Sect. 5.1 and discuss the role of water vapour in a new Sect. 5.2 after the analysis of the ECHAM6 simulations (as reviewer 2 suggests).

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

REPLY: We agree and will change this in the revised manuscript.

COMMENT: 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.

REPLY: Indeed, what we mean to say here is that the distinction of parameters alpha and beta, as well as their separate variation in the simple model, is an idealisation. This approach would not work in the complex model, where there are no such parameters, and the comparison between the models can only be qualitative in the sense that we compare the effect of CFCs and CO2. It is not possible to identify a unique realistic value for alpha, as the reviewer says. We therefore have the impression that we fully agree with the reviewer on this point. We do not see though how this would constitute a problem here because we do not fit the model parameters to ECHAM6.

COMMENT: 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.

REPLY: We agree that it will help to add such quantities to Tab. 1 and will do so in the revised manuscript. However, we are not certain how the first sentence of this comment is meant. If the troposphere acted to "counter the [radiative] forcing", this would imply no radiative imbalance, meaning that surface temperatures would not change even if surface temperatures were dynamic – which is obviously not the case (as the climate sensitivity is non-zero). Moreover, as can be seen in Tab. 1, the global mean surface air temperature changes very little when SSTs are fixed (0.28K for CO_2, 0.15K for CFCs). As we explained above (and as can be seen in Fig. 9), changes in the troposphere have a negligible effect on the stratosphere, which is even more true for such small temperature changes near the surface.

COMMENT: 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".

REPLY: We will take up this suggestion in the revised manuscript.

COMMENT: 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.

REPLY: We chose this wording based on previous literature explaining CO_2-induced stratospheric cooling. It is true that we do not show in our manuscript that these are the only relevant mechanisms. We only formalise the heuristic arguments from previous studies and textbooks. However, we are not aware of any literature indicating that other effects are of similar importance (or will be in the future), and therefore see no reason against the statement that our article has "the potential to convey the physical essence". To leave room for other mechanisms we can replace "this important phenomenon" by a reference to the mechanisms instead of the whole phenomenon, e.g.

"The reconsideration of CO2-induced MA cooling as put forward here has a distinct educational element, with the potential to convey the physical essence of the involved mechanisms to a broader audience."

REFERENCES:

Forster, P. M. F., Freckleton, R. S., and Shine, K. P.: On aspects of the concept of radiative forcing, Clim. Dyn., 13, 547–560, doi:10.1007/s003820050182, 1997.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2016-8, 2016.

Printer-friendly version

Discussion paper