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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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COMMENT: This paper is an interesting, scholarly, thorough and well-motivated piece of work, and I recommend that it be published subject to some modifications. I have to say, though, that having worked in this field for some time, I did not feel it ultimately helped my intuition much beyond what I learn from a rather simpler model (below). But I appreciate that others (as evidenced by the comment already online) may so benefit. It could be that I am too stubborn with my simpler view, or too easily satisfied.

REPLY: We thank the reviewer for the very helpful and constructive comments which will help us to improve the manuscript. We comment on the reviewer's simple model explanation below.

COMMENT: My simple view: It is simply that the grey-body emission of the strato-

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sphere 2eT**4 (where e is the stratosphere emittance) is balanced by the heat source which is a combination of direct solar heating of the stratosphere (Sa) and absorption of upwelling infrared radiation from the surface and troposphere. In the CO_2 case, where the upwelling radiation mostly originates from the cold upper troposphere, I would approximate this as 2eT**4=Sa, from which a cooling immediately follows when e increases. In the other ("CFC") limit, then clearly the absorption term can come to dominate, yielding a heating, or at least a greatly reduced cooling. I realise that this simple model is encapsulated in the authors' model, but the above seems a simpler expression of it, for those less familiar with radiative processes.

REPLY: We agree that the reviewer's explanation is adequate and very simple, and we will take up this very nice summary in our conclusions section. However, we see this only as an explanation that requires one to know the answer already, as compared to a model where this result is derived based on certain assumptions and boundary conditions. We therefore think that our more elaborate approach is justified. In particular, the reviewer's approach only yields an instantaneous radiative perturbation, but does not allow to calculate how the atmospheric profile would be altered in equilibrium when the fluxes and temperatures adjust. Moreover, the simple explanation alone would not allow to demonstrate the importance of the atmospheric window to explain CO_2-induced cooling of the middle atmosphere (the grey model yields no cooling in equilibrium). To derive these mechanisms and the fact that CO_2 and CFCs act differently, one needs to couple the radiative fluxes to the temperature and solve for the temperature profile. Our model thus connects well to the radiative two-layer model and its variants that are very popular in educational contexts. We therefore hope that our model (though more complicated than the reviewer's explanation) can also help non-experts to build a deeper understanding.

COMMENT: The GCM experiments with the removal of atmospheric solar absorption are very interesting, but the main result, the difference between REF and REF_ns is surprising to me – a cooling of just 1.8 K. If correct, this is noteworthy. But if we consider that about 70-80 W m-2 of solar radiation is absorbed by the atmosphere, one might guess that about 30% of this (the planetary albedo) would now be reflected back to space, as it is not now being absorbed. That constitutes a top of atmosphere forcing of maybe 20 W m-2, or 5 times the CO_2 forcing. If my simple estimate is correct, how come such a small temperature change? It is possible that the increased absorption of UV/vis radiation by the surface/troposphere system when ozone absorption is removed, might compensate for the loss, but the stratospheric cooling would likely compensate for much of this (and I would guess much of the non-absorbed UV would instead be Rayleigh scattered to space). An alternative is that there may a mistake in the model set-up. It is not clear whether it is just the gaseous solar absorption that is set to zero, and the cloud liquid/ice absorption remains – if so, this would likely compensate strongly. It would be good to see how the planetary albedo changes between the two runs. Whatever the answer, this result needs some more discussion and perhaps there is some similar experiment in the literature that could be used to support this new result.

REPLY: We agree that, given the major interference with the atmospheric radiation budget, the small tropospheric temperature change is not at all obvious. As the reviewer suspects, a substantial energy source in the atmosphere is missing in the non-solar simulations; however, a large part of it is absorbed at the surface instead. We will explain the associated processes in more detail in the revised manuscript, and add some quantitative information to support our arguments. (Note that the exact numbers of the global mean temperatures in Table 1 will slightly change in the revised manuscript because we repeated the simulations with a newer version of Echam6 in order to obtain more model diagnostics for our answer).

What we did in the experiments labelled as “ns” is indeed to switch off the solar absorption by all gases, but not cloud droplets or ice. This leads to a reduction of short-wave absorption in the atmosphere from 75 W/m2 to only 13 W/m2, and nothing is absorbed anymore above the tropopause, as intended. We are not aware of identical experiments in previous studies. To this extent, our result is probably indeed new, although the response to switching off atmospheric solar absorption (by gases) is not meant to be the focus of our study, and we thus would not call this the “main result”.

Our results can be understood in a similar way as stratospheric ozone removal experiments, for example Ramaswamy et al. (1992), Hansen et al. (1997), Forster and Shine (1997) and Stuber et al. (2001). These and other studies agree that the instantaneous radiative forcing of ozone removal is positive as more solar radiation reaches the surface. In such ozone removal experiments, there is also an instantaneous longwave effect due to the nature of ozone as a greenhouse gas, which is not active in our experiments where we only switch off short-wave absorption. The instantaneous long-wave effect after ozone removal in the other studies is negative, but found to be weaker than the short-wave effect (Ramaswamy et al., 1992; Forster and Shine, 1997). Hence, the instantaneous radiative forcing is positive in these studies as well as our own. However, the lack of solar absorption by ozone leads to a very strong cooling of the stratosphere, which reduces the downwelling long-wave radiation from the atmosphere to the surface (due to the greenhouse effect of all present greenhouse gases, not only ozone). After this temperature adjustment, the forcing has reversed its sign, i.e. becomes negative. This explains the cooling despite the increase in solar absorption at the surface.

Another difference between these studies and ours is that we also switch off the absorption of solar radiation by other gases than ozone. However, a similar argument as above holds for the solar absorption by other well-mixed gases: more sunlight reaches the ground (+42 W/m2), but this is counteracted by a decrease in downwelling long-wave radiation and an increase in the turbulent fluxes.

Finally, a large fraction of absorbed solar radiation in the atmosphere is due to water vapour (as our Fig. 1 c indicates). This gas mostly occurs in the lower levels of the troposphere (difference between Fig. 1 b and c) where the temperature is already close to the surface air temperature, and where turbulent fluxes redistribute perturbations vertically. The shift of the absorbed solar absorption from the lower troposphere to the surface has therefore only little effect on the surface air temperature.

In total, the effect of the reduced downwelling long-wave effect from the strongly cooled stratosphere slightly dominates the sign of the surface air temperature response in our simulations. The total flux of shortwave radiation absorbed by the real present-day atmosphere that the reviewer mentions should therefore not be expected to have a similarly large effect compared to a radiative forcing by CO2 of the same magnitude, because a radiative forcing of well-mixed greenhouse gases is defined at the tropopause; the radiative forcing concept appears to be misleading when a vertical redistribution of heating across the tropopause is involved.

The effect of increased scattering that the reviewer mentions also contributes to cool the surface, though perhaps not as much as the reviewer speculates: The planetary albedo increases from 29% to 33% after switching off the absorption by gases, and the outgoing short-wave radiation at the top of the atmosphere increases from 98.8 W/m2 to 112.6 W/m2.

We understand that an explanation of the experiment and these effects will improve the understanding of our manuscript, and we will add an according paragraph and extend Table 1 as suggested.

COMMENT: 2:17-19 I do not quite see the “fails to explain”- the shortwave heating may be weaker than at the stratopause, but it remains substantial, otherwise the middle atmosphere would be much cooler (and would relax to a “polar night” radiatively-determined state).

REPLY: We meant to say that the argument fails to explain why the CO2-induced cooling does not become weaker beyond the stratopause despite the weakening shortwave heating. We do agree that this reasoning is rather subtle, in particular because observed heating rates beyond the stratopause are quite uncertain. We will rephrase this or leave the sentence out.

COMMENT: 2:34: “we show that this is not the case” – perhaps the authors could be clearer here. To my mind the CO2/CFC experiments show very clearly that the low upwelling flux at the tropopause is not very important in determining the CO2 cooling, to the extent that it is very insensitive to changes in that upwelling flux when surface temperature changes.

REPLY: What we mean here is that the existence of a temperature minimum in the profile is not required to explain CO2-induced cooling. Our model does not have a tropopause but still captures MA cooling, therefore we argue that the model proves this point. We will make this clearer in the revised manuscript. We fully agree that the tropospheric adjustment to radiative forcing does not affect the middle atmosphere much, but this is not the point we intended to make here.

COMMENT: 3:10 I agree that this simple model cannot explain the cooling, as the solar radiation is deposited at the surface. But my simple model above, has the solar radiation deposited within the stratosphere and does give a first-order cooling effect as (stratospheric) emittance increases.

REPLY: We agree, but argue that it’s important to note first that the very popular grey-atmosphere model (transparent to solar radiation) cannot exlain the cooling. The model put forward by the reviewer is already slightly more complex by accounting for the absorption of solar radiation within the atmosphere. Furthermore, we argue that the reviewer’s heuristic model, though correct, is not an energy balance model of the full atmosphere and thus does not yield equilibrium temperature changes, only instantaneous local responses to radiative perturbations.

COMMENT: 4: Fig 1 caption – (a) perhaps say how normalised (I know the answer, but perhaps readers will not). (b) “vertical column” – it is clear in the appendix that this transmittance is simulated from a homogenous slab approach. I have no objection to this, as it is fine for the illustrative purpose used here, but I think the caption should make clear that this has been done – perhaps “assuming the troposphere and

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stratosphere to be homogeneous slabs".

REPLY: We will take up these suggestions in the revised manuscript.

COMMENT: 4:6 "radiance" – since one is dealing with energetics, I feel this should be modelled as irradiance and not radiance, and indeed equations (1) and (5) seems a slightly odd mix of radiance and irradiance formulations, assuming the normal definition of the Stefan-Boltzmann constant. I'd slightly prefer to see a and a slant path formulation.

REPLY: We agree that we should distinguish radiances and irradiances more clearly here and will correct this in the revised manuscript. We will introduce one more step in the derivation to first go from an arbitrary direction of a radiance (in W/m2sr) to the vertical direction (introducing the geometric factor mu mentioned later), and will then integrate over the half sphere to obtain irradiances in W/m2. The factor pi would then indeed occur before the source term J, and this product yields sigma T**4.

COMMENT: 5:13 "insolation" – this is confusing because, at this stage, insolation is not represented. This is related to my irradiance/radiance comment above.

REPLY: With insolation we do not mean solar insolation here, but the source term J which only describes the emission of other parts of the atmosphere according to Planck's law. We will make this clearer.

COMMENT: 7: 5 "in an atmosphere where no solar radiation is absorbed".

REPLY: We agree that this formulation is clearer and we will take it up.

COMMENT: 8:7 "like the one" – of course, in the Earth's atmosphere the window is not perfectly transparent, especially in moist atmospheres were the continuum absorption is strong but (because of the vapour pressure squared dependence of the continuum) the argument still holds as most of this absorption/emission is in the lower troposphere.

REPLY: We will make clearer that the notion of "a window" where all radiation is transmitted is idealised.

COMMENT: 8:10 Note typos ??-??

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

COMMENT: 10:4 I find “skin temperature” a strange name here, as this is also used for the topmost layer of the ocean. Perhaps another name could be used?

REPLY: We have adopted the usage of “skin temperature” as a synonym for “TOA temperature” in our model, as for example in Pierrehumbert (2010; e.g., chapter 3.6). However, to avoid ambiguity, we will use “TOA temperature” consistently in the revised manuscript.

COMMENT: 11:1 I would say “decadal to centennial” rather than “multi-centennial”.

REPLY: We will change this accordingly in the revised manuscript.

COMMENT: Section 4.2: I am always a bit suspicious about such analogies and don’t really think they help the argument much more than a direct appeal to the actual physical situation at hand. I personally would delete this whole section.

REPLY: We understand that among experts such analogies may sometimes cause more suspicion than illumination. However, we are convinced that this section can be useful for readers who are no experts in atmospheric radiation but who want to understand and remember the mechanism of the blocking effect. These readers are an important target group of our paper. The analogy will also be useful for readers who do not want to go through the many equations, but still look for a quick explanation. We feel confirmed in this view by the Short Comment posted by A. Ferraro in the open discussion. We therefore prefer to keep the analogy, but we realise that it may seem a bit isolated from the more technical rest of the paper. Hence, we are going to move the building analogy to an appendix.

COMMENT: 15:17 “indirect” – I didn’t quite understand why the solar effect was labelled as “indirect” – it seems rather direct to me, and of first order importance.

REPLY: If we termed this effect just a “solar effect”, it could easily be confused as a result of reduced shortwave heating. In our view the term “indirect” helps to prevent that confusion, and we also do not think that “indirect” would be mistaken as meaning “weak”. We would therefore like to keep the terminology, but will add a clarifying remark.

COMMENT: 17:1 I had a similar feeling to Section 4.2 – I felt that this section could be removed as it seems hard to come up with truly realistic values given the idealised form of the equations derived to this point, especially given Section 5.2. (Part of my thinking is that the paper would be more easily “digestible” if it was a little shorter, although I appreciate the thoroughness – perhaps this section could be moved to an appendix or supplementary information?)

REPLY: Given the criticism of both reviewers, we are going to remove this section, and discuss the most important differences between the simple model and ECHAM6 in a new section after the presentation of the simulations.

COMMENT: 18:20 I have quite a lot of comments on this section. The earlier parts of the paper were very thorough in reviewing the prior literature, but this section was less good – many of the results could already be anticipated from the prior literature and they should be mentioned/compared explicitly

REPLY: We understand that we can refer to previous literature at this point, some of which we already cite earlier in the manuscript. As discussed in our response to the reviewer’s main comment 2, we will discuss the simulations without solar absorption in more detail, and refer to the relevant papers mentioned above. We can also refer more explicitly to previous papers that investigated the effect of CO2 and CFCs on the temperature profile.

COMMENT: It is assumed, but not said, that this model configuration has fixed climatologically specified ozone – otherwise it would also respond to changes in temperature and CFCs. Similarly, the stratospheric water vapour is very sensitive to tropopause temperature (see for example Joshi et al. 10.5194/acp-10-7161-2010) and so might dramatically change in the no-solar runs, where there is so much cooling.

REPLY: Indeed, the ozone concentrations are fixed to a climatology in the model. We will mention this in the revised manuscript. Regarding the effect of stratospheric water vapour, indeed the stratospheric humidity in the non-solar simulations decreases due to the very low temperatures (though the changes are tiny compared to the tropospheric changes), and this could have a contribution to the reduced long-wave emission and the cooling at the Earth's surface. We will mention this effect together with the others discussed above (though as far as we can see, it is not possible to clearly separate the effects in the model output).

COMMENT: 20:7-15 I found this discussion hard to follow. The "virtually unchanged" is not surprising from earlier calculations of the impact of CFC changes that the authors refer to, and results from a closer balance between increased absorption of upwelling radiation and increased emission. The "solar effect" is quite wavelength dependent, and so the second sentence needs to be clarified. But it seems to me that there is some expectation by the authors that the 2xCO_2 and 15xCFC experiments, because they yield similar surface temperature change, should somehow be expected to yield similar stratospheric temperatures. But since these two gases are in very different regimes (strong and weak) at current tropospheric concentrations, I don't think such an equivalence should be anticipated –small changes in CFCs can have an equivalent effect to large changes in CO_2 for surface temperature, but the situation is quite different in the stratosphere, when CO_2 can more effectively cool to space from its band centre.

REPLY: We fully agree with the reviewer's explanation, and did not intend to say that we anticipated any other result. It is not the aim of our paper to present the differences in CO_2 and CFCs as any new or surprising result, but to formalise the explanation in a simple but comprehensive way. Our main point here is that the simple textbook explanation for stratospheric cooling, where solar and longwave radiation are distinguished but no further distinction in spectral characteristics is made, does not work for CFCs. Apparently the according paragraph does not make this clear enough. It will benefit from a rephrasing and from adding references to previous studies as suggested by the reviewer above.

COMMENT: At 21:20-23 there is not so much surprise that the stratospheric temperatures are not so sensitive to surface temperature change – this is shown, for example, in the figures in Forster et al. (1997). I am not sure that this is surprising (the authors say it is "not obvious"), partly because the change in upwelling radiation at the tropopause in the CO2 bands is rather small after a climate warming (most of the extra upwelling radiation will be at wavelengths where CO2 absorbs little). And so the following discussion on the possible role of water vapour feedback seems very speculative.

REPLY: What we intend to say here is that the result is different from the window-grey model which predicts a transient effect. We will rephrase this section to clarify that, taking the cited literature into account, this is not overly surprising. Yet we are not aware of any publication that focusses on this interesting point of a lacking transient effect, and the mechanisms involved, in comprehensive models. The lack of a transient effect can be clearly seen in Fig. 8a of Forster et al. (1997) like the reviewer says, but it is not explained or analysed further. We are therefore convinced that this result is noteworthy. We think that it is safe to argue that the water-vapour feedback must contribute to the fact that the stratospheric response to surface warming is small compared to the window-grey model, because it allows for a large surface temperature change despite a relatively small long-wave flux change at the tropopause. The reason we mention this effect is that the window-grey model cannot be tuned in a way to show realistic changes in the temperatures and a negligible transient effect at the same time. It still appears to us that the mere presence of an atmospheric window (the effect the reviewer refers to) cannot explain why there should be no transient effect; after all, the window-grey model does have a transient MA adjustment despite an atmospheric window. Nevertheless we agree that this aspect of the discussion is not a proven result but a (well-motivated) speculation. We will make this point clearer in the revised manuscript and keep the speculative part to a minimum.

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