

Interactive comment on “Why CO₂ cools the middle atmosphere – a consolidating model perspective” by H. F. Goessling and S. Bathiany

Anonymous Referee #2

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Review of Goessling and Bathiany: Why CO₂ cools the middle atmosphere.

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.

Main comments

1. My simple view: It is simply that the grey-body emission of the stratosphere $2e\sigma T^4$

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(where e is the stratosphere emittance) is balanced by the heat source which is a combination of direct solar heating of the stratosphere (S_a) and absorption of upwelling infrared radiation from the surface and troposphere. In the CO₂ case, where the upwelling radiation mostly originates from the cold upper troposphere, I would approximate this as $2e\sigma T^4 = S_a$, 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.

2. 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 CO₂ 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 be 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.

Other comments:

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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).

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.

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.

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 stratosphere to be homogeneous slabs”.

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 π and a slant path formulation.

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

7: 5 “in an atmosphere where no solar radiation is absorbed”.

8:7 “like the one” – of course, in the Earth’s atmosphere the window is not perfectly

C3

transparent, especially in moist atmospheres where 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.

8:10 Note typos ??-??

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?

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

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.

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.

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?)

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

- 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 tem-

C4

perature (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.

- 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 2xCO₂ 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 CO₂ for surface temperature, but the situation is quite different in the stratosphere, when CO₂ can more effectively cool to space from its band centre.

- 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.

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