REPLY TO COMMENTS AND REVIEWS OF "A COLORFUL LOOK AT CLIMTE SENSITIVITY"

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General Remarks

The paper has been through a second round of review, with one re-review (rev 1) and one new review (rev2). The main issues raised remain related to style more than to substance – specifically how much to review, how to best present complications introduced by CO_2, and ambiguity related to terminology (spectral masking). In the revised manuscript we undertook major changes. Specifically, and as detailed further in the itemized reply to the reviewers, we have made the following changes:

(1) We dropped our attempt to introduce our model axiomatically, for a more direct approach, which helps us be more precise in our introduction of terminology (spectral masking, e.g., Rev2), also through the introduction of schematics.
(2) To better emphasize that our goal is not an elegant asymptotic limit of radiative transfer, but rather a simple model that helps understand the physics of complex radiative transfer calculations, we now discuss our model as a heuristic.
(3) We incorporated the effects of CO_2 more naturally (Rev 1), rather than introducing it after the fact as a correction. Incorporating these effects result in less of a moderating effect than the earlier literature, something that is due (mostly) to simplifications in our model, but is also sensitive to the assumption of an unrealistically cold (and thin) stratosphere in models with a more complete treatment of radiative transfer.
(4) Including CO_2 encouraged us to develop our ideas less directly around Simpson's law, and more by emphasizing what controls the emission at any given wave number, and how the emission temperature of this constituent couples to the surface – Simpson's law is just one reason why emission temperatures might (not) change. This reduced the emphasis on what the reviewers thought was unnecessary repetition.
(5) Throughout we tried to pair back unnecessary material and sharpen the presentation, so that despite the introduction of more explanatory material the manuscript as a whole is not substantially longer.

RC1

RC1 Major comments.

(1) Spectral masking: The rationale for recapitulating the work of Koll, Jeevanjee, and others in sections 3.2-4.2 seems to be that there is a unifying concept of "spectral masking" which is then applied to clouds in section 5. The authors define spectral masking in line 167 as emissions which are "invariant of T_{sfc}", and hence don't contribute to Λ. That is all well and good, and essentially synonymous with earlier literature which referred to such emission as "Simpsonian" (Ingram 2010) or "Ts-invariant" (Jeevanjee and Romp 2018). But then, in section 4.2, the authors apply this concept to CO_2 forcing, arguing that where CO_2 is optically thick it "masks" the surface. But emission from such CO2-dominated wavenumbers is not invariant to warming the way H2O-dominated wavenumbers are, as the authors acknowledge, so it seems that the authors' definition of spectral masking does not apply here. The authors seems to be using the term rather loosely, and in some ways synonymously with simply "large optical depth" or "low surface transmissivity".

We attempted to address this ambiguity through the more direct approach, using Eq. (3), the schematic, and by distinguishing between what we call invariant and sensitive emitters. The main point is to see the control of emissions as being the weighted response of two dominant emitters with the transmissivity of the one that is more optically thick doing the weighting.

(2) State-dependence of Λ: A key point of section 4.1 and Figure 5 is that the "Simpsonian response parameter" Λ has a significant temperature-dependence. However, the calculation of Λ seems to employ several significant approximations, including the neglect of pressure broadening in eq. (4) (more on this below) as well as the neglect of CO_2. More realistic calculations of the response parameter show a more muted temperature-dependence (Koll + Cronin 2018, Kluft and Stevens 2021, McKim 2021). The authors acknowledge this in 5.1.2, where they note that the difference in response parameters between 288 K and 302 K is $3\times$ for their idealized Λ, and more like 30% for more realistic λ values. There is a big difference between $3\times$ and $1.3\times$. It feels misleading to not revise the narrative of section 4.1, or even the whole paper, in light of this. At the very least the authors should present the results of a more comprehensive calculation (including CO_2) in Fig. 5, so that readers can make an informed judgment about the significance of this state-dependence.

We have reformulated the presentation so that the effect of CO_2 is now part of the model and included throughout, rather than as an afterthought (new §3.3 and additional Fig. 6). Hence the moderating effect of CO_2 becomes apparent from the start, as recommended by the reviewer. We note (also in the manuscript) that the moderating effect still appears less in our simple model than in more complete calculations ($\Lambda(305\,\mathrm{K}) \approx 0.5\,\mathrm{W\,m^{-2}\,K^{-1}}$ as compared to $\approx 1\,\mathrm{W\,m^{-2}\,K^{-1}}$ in the other mentioned studies. However all of those studies adopted a rather cold stratosphere, which influences the relative weight of the CO_2 masking versus wing emission. If we adopt $T_{\mathrm{cp}} = 150\,\mathrm{K}$ as (apparently) used by McKim et al., or Koll and Cronin, we get $\Lambda(305\,\mathrm{K}) \approx 0.65\,\mathrm{W\,m^{-2}\,K^{-1}}$, so only part of the discrepancy comes from our simplified treatment.

(3) Pressure Broadening: Despite the author's contention otherwise, Eq. 4 (and hence the calculations shown in Fig. 5, which as far as I can tell are based on Eq. 4) does not include pressure scaling due to collisional broadening. Certainly the reference mass absorption coefficients shown in Fig. 3 have the effects of collisional broadening baked in. But the question is how these coefficients are scaled when evaluated at pressures different from the reference pressure. A standard approximation is to take the far-wing scaling, which is linear in p, and apply it to all wavenumbers. It is this approximation which was justified in Romps 2022. But Eq. 4 has no p-scaling at all, and in this sense neglects pressure broadening.

ARTs calculates the absorption spectra (not the absorption coefficients) as a function of pressure, to include broadening effects from all lines. The spectra would thus vary with P were we to use it to perform radiative transfer level by level. We neglect this and assume an effective absorption spectrum to estimate the optical thickness of the atmosphere. This simple minded approach is motivated by a desire to link the radiative response and forcing to bulk measures of the absorption, rather than trying to compute their effect on radiant energy transfer level by level. Essentially we are saying that the optical depth at the surface, which normally would require integrating pressure scaled absorption coefficients over the atmosphere can be approximated in the sense of the mean value theorem. To assess if we were being too simple minded we repeated our calculations with line shapes taken at $P = 700\,\mathrm{hPa}$ and $T = 270\,\mathrm{K}$ instead of at $P = 850\,\mathrm{hPa}$ and $T = 280\,\mathrm{K}$ leads (as expected) to a more transparent atmosphere, but the effect is small with Λ increasing by 2 %.

(4) Derivation of CO_2 forcing: Section 4.2 on CO_2 forcing is somewhat clearer, but still confusing. Why is it permissible to ignore tropospheric temperatures entirely? How does assuming an isothermal stratosphere imply that there should be no stratospheric adjustment? Couldn't the stratosphere cool uniformly? What is the "absorption feature" mentioned in line 249 – simply the edge of the CO_2 band? The integral in Eq. 13 is evaluated in line 254 – how is this done? Numerically using $\tau(\nu)$ calculated from the spectroscopy shown in Fig. 3?

We now introduce a schematic to clarify these issues, and refer to the absorption band of CO_2. An isothermal stratosphere accounts for stratospheric adjustment (which renders CO_2 to be an invariant emitter when it is optically thick within the stratosphere. As the schematic shows, the emission in the troposphere is not neglected, it just doesn't matter for the estimate of the forcing as the area of the parallelogram is given by its 'height' (new Fig. 8) which depends on $T_{\mathrm{sfc}} - T_{\mathrm{cp}}$. This schematic is not so different from what has been used in the past, i.e., in the Jeevanjee et al., study, and to offset some of the repetition it introduces we have tried to pair back our discussion of the forcing, focusing more on how the now familiar way of thinking is manifest in our heuristic.

(5) Derivation of Equation 5: Eq. 5 appears rather abruptly after the discussion of spectral masking, and may confuse readers. In addition to Jeevanjee et al. 2021a, the authors could reference Eq. 5 in the SI (not main text) of Koll and Cronin 2018, which derives the authors' Eq. 5. This derivation is short but insightful, and readers might benefit from seeing it reproduced here.

The (now) Eq. (4) should now follow more directly from the explicit formulation Eq. (3), but the reviewers suggestion to also reference Koll and Cronin has also been adopted.

RC2

RC2 Major comments. Many of the major comments are related to what we think was a common deficiency in the previous version of the manuscript. This motivated us to be more explicit in the introduction of our model,. This lead not only to the new Eq. (3) but also the introduction of the schematic, Fig. 1.

The minor comments raised in RC2 have also all been addressed, either by adding information that was missing, or reformulating sentences that were unclear. Here we note that there is a tradition of using 'vapor' to denote a condensible 'gas', one whose temperature is below its critical temperature.

(1) I found the word masking inappropriate. A fundamental difference between radiative exchanges in the SW and LW domains is that in the first case a screen (at room temperature) can actually mask radiation, whereas in the LW domain, if a screen absorbs (and therefore masks) radiation, then it also emits some. The vocabulary used must be consistent with this fundamental difference. The use of "cancelling", or probably better "cancelling changes" for example, might be more appropriate because it is not commonly used for visible radiation.

    In addition to the general comment above, wherein the simple model has been introduced to help make clear why we use this language, we also discussed this with the reviewer, and have modified or qualified our usage of the term to avoid any possible confusion.

(2) statement S3 is always true. I believe the authors are referring here to the Simpson effect, or one of its consequences, where the temperature of the atmosphere also varies. This statement requires some additional hypothesis, mainly: the H2O is the only absorber and its optical thickness is large. This does not hold for the CO2 or for clouds. The discussion after Eq. 5 needs also to be revised.

    This has been reformulated to identify it as a form of Beers law, which is recovered when the emission temperature of the masking constituent doesn't change (so the source term that emerges in Schwarzschild's generalization of Beer) vanishes). This is also addressed by adding a reference to the derivation by Koll and Cronin, as suggested by Reviewer 1.

(3) The masking for CO2 and for clouds is very different from the masking by H2O, and I think two different names should be used. For CO2 and clouds, the key point is that they drive the emission temperature, and their effect depends on the difference between the emission temperature when they are not there and when they are present. For H2O, the masking refers to the fact that the emission temperature does not change when the atmospheric temperature changes. Significant parts of the CO2 and the clouds sections need therefore to be modified

    We made several changes to address this point. In particular we try to differentiate between our model, and the spectral masking it implies. We also no longer refer to unmasking. Instead we now talk about restoring the spectral response to warming.'

(4) The value of 275K is present in various places. A variable name, instead of a value, with its interpretation will be welcome.

In an effort to address this comment we have considerably reformulated the presentation of the forcing. Now we introduce an emission temperature, T_W that depends on $\kappa_{\nu,\mathrm{v}}$ and show how to calculate it, when using our heuristic to estimate forcing. We also better separate our heuristic, which is colorful, from the band-averaged approach used int he recent literature, which then uses a band averaged value of T_W.

(5) line 225 The value in McKim et al. 2021 has been obtained with an atmosphere with CO2. The comparison is not relevant here but may be relevant later.

This is now addressed through the inclusion of CO_2 in our model.

(6) The status of section 4.3.1 is not clear for me as it mainly repeat previous developments. The goal is to present a synthesis?

This has been substantially reformulated. The material that shows how our heuristic allows one to write an expression for the climate sensitivity in terms of basic physical constants, with no real empiricism, and how this proves quite informative. The simplification of this to a form that one could have pieced together from the literature is also pointed out, but is no longer the main point of emphasis..

(7) The 'polar' section (5.1.4) presents new and interesting points, especially the 'polar radiative paradox'. However, the authors should better introduce the goal of this section and make clear that some key assumptions made previously, mainly relevant for the tropics, are not valid for the polar regions.

We reformulated the introduction to this section in light of the reviewer comments. We now write:

> *This is less of a paradox when one considers the differences between the poles and the tropics, whether it be by virtue of surface albedo changes, or the decoupling of the polar surface from the polar atmosphere. Here we point out the potential for clouds to also cause a differentiated response of the cold poles, versus the warm tropics, to warming.*