

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

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GENERAL REMARKS – THE GOOD, THE BAD AND THE UGLY

On the “good” side, the paper was described as ‘thoughtful, synthetic, provocative, containing,, stimulating new ideas and constituting a good read’. The “bad” could be classified as a lack of clarity in places and the neglect of compensating effects that muted some conclusions. The “ugly” was the concern that the manuscript ‘repeated’ too many recent developments, was in places ‘overwrought’, or unclear as to how results differed from the recent literature, or of the benefit of new terminology (spectral masking). Drawing encouragement from the good, we substantially restructured the manuscript to address the ugly, addressing the bad along the way. Some general points related to the “ugly” are noted first, as they apply to both reviews. An itemized response to the major points raised in each review is presented thereafter. We note that individual changes are presented in a cursory manner below, as difference between the original and revised manuscript pdf files are provided for this purpose.

Repetition/Replication: In rewriting the manuscript (especially §3 and §4) we make use of the existing literature to shorten the presentation where possible, and where results touch on previously published material, we mention these directly, foreshadowing this overlap in the introduction. These changes are most pronounced in §3. We considered to what extent this material could be deleted entirely, and at the risk of perhaps being a bit too attached to wanting to share this information in our own words, and with the excitement that we had upon uncovering it, we concluded that some degree of replication is inevitable, as one of the aims of the manuscript is to develop a unifying conceptual framework – spectral masking – for thinking about more varied contributions to Earth’s climate sensitivity. As the reviewers point out this results in conclusions that are similar to has been published in recent work, in some cases ending up with analogous, or even identical equations. However without repetition, developing these ideas in ways that allow the reader to understand their extension to clouds, which is our most novel conclusion, would have been even more belabored, as we would have had to work backward to the reasoning. A few further, general points on this topic:

- The most onerous replication was the repetition of Eq. (4) from Koll and Cronin (2018), which we didn’t realize, perhaps because their paper used this Eq. to emphasize the flatness of Λ and we emphasize the variation, which is important for understanding the differentiated effects of clouds.
- Repetition with Jeevanjee et al., (2021) with regard to the clearsky feedback is now more directly acknowledged, although here too, while we come up with an equivalent result, as did Ingram much earlier, our way of getting there is different, and that is the point. This exemplifies our above assertion that a presentation which would start from Jeevanjee et al., would have to get the reader from their Eq. (13) to our (new) Eq. (5) in a way that conditions their thinking for what is to come. We thought it better to present the result

from the reasoning rather than the other way around, also because that makes it possible for readers without a deep understanding of the earlier literature to follow. That said, this motivation was not clear in the original manuscript, something our revisions address.

- Even if the repetition weren't necessary to make our broader point, or to establish the conceptual framework used to interpret clouds, it isn't disproportionate. Consider, for instance, the ratio of novel content to repetition in the recent papers on Simpson's Law, or the water vapor feedback as compared to what one learned from Nakajima et al., (1992) for the former, or Ingram's paper for the latter, likewise the degree of repetition in the recent work on CO₂ forcing relative to Wilson and Gea-Banacloche (2012).

RC1

The thoughtful, albeit critical, assessment of our manuscript, and especially the entreaty to more critically engage with the recent literature, spurred a thorough revision. While the reviewer might have imagined a different presentation of our ideas, we believe our revisions address the substance of the reviewer comment, and in so doing considerably improves the presentation. Below we address the major comments raised in the review, critical minor comments were all addressed, and noted as part of the response to the major comments. Of course we very much appreciated the positive minor comments, but don't inflate this response by mentioning them further.

We begin by responding to the introductory remarks raised in the review, in the form of their summary: *My overall feeling is that while this paper contains some useful syntheses and stimulating new ideas, it doesn't function very well in its current form.* We agree that our presentation style is a bit unconventional, and defend it as being reflective of our interest in showing how a particular way of thinking can help better frame the climate sensitivity problem, and give insight into clouds. The comments suggest that our initial attempt to make this point were not successful, something our revisions address. We did so by being more direct in linking the general idea (spectral masking) to the results, and better foreshadowing their implications for our more novel contributions, throughout the manuscript.. In this regard we also note that show that our way of reasoning could be used to replicate the results of others is actually desirable, something the more specific referencing now highlights.

RC1 Major comments.

- (1) The authors acknowledge at the outset that they are replicating recent work, but argue that their ideas, as developed independently, are foundational for their more novel ideas about clouds. I did not find that to be the case, however. The material on the H₂O feedback (sections 3.2-4.1) and CO₂ forcing (section 4.2) seem to end up at the same place as previous authors, and I did not see anything new which was key to the later developments. I think readers would be better served by a more compressed review of recent findings, rather than a lengthy re-development. I expand on this in the next few items.

We revised the introduction to better explain our motivation for including this material. The presentation in §3.2-4.2 has been shortened, when results replicate earlier work this is pointed out more specifically, and justified either as a way to demonstrate how these results can be conceptualized using a common framework, or because they are necessary for what comes later. As an example, the section (§4.2) on CO₂ forcing – which recent papers devote an entire manuscript to – is now 3 equations and ca 20 lines. While it gets to the same place as Jeevanjee et al, it does so somewhat more simply (using the ideas of spectral masking),

albeit less rigorously. It become equivalent to Jeevanjee et al's Eq. (14) after applying the simplifications of Wilson and Gea-Banacloche (2012) which are summarized in only 12 lines (§4.3.1).

(2) Section 3.1 on the water vapor path W seems overwrought. The theory in 3.1.1 seems very close to that of section 2 of the SI of Koll and Cronin, and could just be quoted as such. Also, while the comparison to observations in Fig. 2 is laudable, it feels unnecessary in a conceptual paper such as this. Furthermore, the difference in slopes between theory and obs in Fig. 2 is noticeable and goes entirely unexplained. This difference also results in the authors carrying around two forms of $W(T)$ for the rest of the manuscript, despite the fact that the choice of $W(T)$ doesn't seem to have much bearing on the results.

We keep this section because we needed the geographic distribution of W for the treatment of cloud effects on the geographic distribution of radiative responses §5.1.4. The reason for the differences between \tilde{W} and W_R , which were only mentioned in passing in the original manuscript, are now better explained. Most other work also uses a fixed value of RH, or a dry adiabat (e.g., Kroll and Cronin, SI), which makes things simple (as we point out), but we wanted to establish for the reader how close this is to more reasonable distributions. Throughout we try, where possible, to connect our conceptual ideas to the empiricism, as in this case, or to more rigorous derivations, as for the case of CO₂ forcing. As a footnote, while our results (Fig 1, lower panel) for theoretical profiles is similar to Fig. 3 inset of Koll and Cronin (2018) their results essentially replicate Fig. 2 of Stevens and Bony, *Phys. Today* (2013), who in turn probably replicate results in earlier literature.

(3) The discussion of Simpsonian physics and its implications in 3.2 and 3.3 is nice, but the main results are virtually identical to those already found in the literature: Eq. 9 is equivalent to Eq. 13 of Jeevanjee et al. 2021a, and Eq. 10 here is actually identical to Eq. 10 of Koll and Cronin 2018. While these papers and others are cited in general in the introduction, they are not mentioned when these specific results are derived, so readers may wonder whether or not these results differ at all from those already in the literature.

We address this by connecting more directly to this literature, pointing out the similarity of our findings with what was found before. This however illustrates part of the challenge we faced. Had we relied on Koll and Cronin for our Fig 5, we would have been hard pressed to explain the warm and cold regimes, as their Figure was constructed in a way to emphasized the constancy of Λ , while for clouds its local maximum and lack of constancy is what is interesting. Because this might leave the reader wondering if the curves are similar, we add a quantitative comparison to show that this is indeed the case.

(4) I found Eq. 15 and its interpretation below Eq. 16 confusing. How would one derive this? As far as I can tell, it is an exact expression for the TOA flux for an isothermal stratosphere at temperature T_{cp} overlying a surface with temperature T_{sfc} . I did not see a value-add to this section relative to existing treatments of simplified formulas for CO₂ forcing (Wilson and Gea-Banacloche 2012, Jeevanjee et al. 2021b, Romps 2022 J. Clim.)

We discussed this above. In addition, in the revised manuscript we have adopted a different presentation: first we rearrange the initial expression to bring out the approximation being made to get the OLR; second we use the emission temperature at the outset (similar to Jeevanee et al., 2021). The former hopefully addresses the confusion, and better highlights the continuity of ideas developed in previous sections. The latter made it easier to address the overlap and allowed for a presentation that is both more precise and better rooted in the existing literature.

One advantage of using an isothermal stratosphere is that it effectively accounts for stratospheric adjustment (minor comment 5), also we now reference the Hansen work on this point.

(5) found section 5.1 to be the most provocative and stimulating of the paper. I appreciate this alternative approach to thinking about how clouds interact with climate feedbacks, as well as the underappreciated point that warming clouds will radiate through the window just as the surface does. But, the idea that the ratio of cloud radiation increase to surface radiation increase (denoted η) might differ significantly from 1 hinges on the exact shape of $\Lambda(T)$ in Fig. 5, which was derived under a variety of strong assumptions (no CO₂, no pressure broadening, etc.). Furthermore, somewhat more comprehensive calculations, such as in McKim 2021 and Koll and Cronin 2018, show a reduced sensitivity of Λ to temperature, with values plateauing near 2 W/m²/K over a large range of T_{sfc} . Indeed, the point of Koll and Cronin 2018 was to understand *why* Λ varied so little with T_{sfc} .

This point was addressed above. It is worth noting again however that the Koll and Cronin Λ is not as flat as it looks. To convince ourselves we took a screenshot, blew it up, and measured it with the help of Adobe Illustrator. Their Λ is only a little flatter, and this by virtue of RH=1 which lowers their peak. Our calculations include pressure broadening, in calculating the line-shapes, and always did. In the original manuscript we neglected the 'diffusivity' factor, this is now included (which meant revising most figures), and while it makes our Λ better match that of Koll and Cronin, it does not change our conclusions or the degree of temperature dependence of Λ . The point that CO₂ lessens what would have otherwise been a heroic effort of diminutive clouds is something we overlooked (Minor comment 6), and is now added as an important caveat where around line 334 we write: "This is a bit misleading however, ... closer to 30 % ..."

RC2

This is a thought provoking paper and a good read. The most novel and interesting part is Section 5. The earlier sections essentially repeat earlier work by Jeevanjee and Romps (2018) etc. and Koll and Cronin (2018) etc.. I think it would make a really useful paper if it is rewritten to bridge better to past literature and focus on what is really new.

This was a major focus of our revisions as explained above.

RC2 Major comments.

(1) Their spectral blocking approach is new phrasing. However, such approaches have a long history in the radiative transfer literature, e.g. correlated K codes and narrowband codes all split the spectrum into weak absorbing and strong absorbing spectral intervals - the tricky parts being those spectral intervals in between: which are often the same intervals that contribute most to forcing and feedback. I think the paper needs to say up front that we have radiative transfer models and radiative convective models that can solve things properly (at least the radiative transfer parts can be solved), and what the paper is developing a simple concepts to help understand models and observational results?

We agree, and have modified the introduction to address this point. Specifically we note that this old idea becomes particularly powerful when one is merely interested in explaining fluxes at the top of the atmosphere, and that the point of the manuscript is not to replace detailed radiative transfer, rather to say that conceptually it is easy to understand the results it produces.

(2) I found the radiative forcing section quite weak. It seems to be a less complete version of work by Jeevanjee, Huang and others - it also missed important concepts such as stratospheric and other adjustments which significantly alter the forcing. I think it would be better just to cite other work when building your climate sensitivity arguments.

This has been reworked to better emphasize our goal of relating the derivation to the idea of masking. As noted in the response to RC1 one advantage of the isothermal stratosphere is that it effectively includes stratospheric adjustment, a point we now state explicitly.

(3) I'm not sure how much spectral blocking is really needed for the arguments. A paper that should be cited and contrasted with is Hartmann et al. (2022) <https://doi.org/10.1175/JCLI-D-21-0861.1>. They develop very similar argument using a more general spectral sensitivity concept.

As some of the cited literature shows, many of our results can be obtained in other ways. Our point is that spectral masking is a heuristic that can be used to help understand and anticipate the results of much more complex calculations. Which we now state directly two times (lines 31 and 86) for emphasis. Regarding the reference, we were not aware of this work, which we now cite.

(4) I wanted to learn from the cloud feedback section how concepts such as the increase in height on anvil clouds fair within the ideas presented - the same goes for decreases in stratocumulus decks. I felt that it would be useful to spell these out more explicitly.

We see this as part of the research programme that the manuscript articulates. The main point however, is that the focus should be less on the height and more on the temperature. To bring this out we have restructured the presentation of different cloud regimes in §5.

(5) I felt Section 5.3 on climate sensitivity was quite speculative and hand wavy. It might be better to explore what would be needed to make the cloud feedback large and positive - or negative? Also I was not persuaded by the overlap with albedo feedback. The albedo

feedback is mostly continental , whereas the SW cloud feedback is mostly over the oceans....

The numbers provided for cloud masking of the albedo feedback are taken directly from the cited literature. Having spent some time flying over the sea-ice margin along the Framm Strait, the first author can attest to the strength of cloud masking that this literature quantifies. We have modified the presentation on climate sensitivity to avoid the impression that our goal is to provide a new assessment, and rather to support the claim that the net cloud effect is more ambiguous than generally appreciated, which adds weight to the value of the clear-sky estimates. In ongoing work being led by the second author, the outlined research programme is being adopted, as we work to quantify the effects of clouds more precisely using full radiative transfer.