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Comment on egusphere-2022-1460

Anonymous Referee #1

Referee comment on "A colorful look at climate sensitivity" by Bjorn Stevens and Lukas Kluft, EGUsphere, https://doi.org/10.5194/egusphere-2022-1460-RC1, 2023

This paper repeats recent developments in simplified yet still spectrally-resolved models for greenhouse gas radiation, and in particular the modeling of the water vapor feedback and CO2 forcing. It then synthesizes these into an analytical estimate of climate sensitivity, which is only a minor extension of previous work but is still novel. Some of the ideas developed for the H2O feedback are then applied to understand the impacts of clouds on radiative feedbacks, and in particular the role they might play as blackbodies radiating through the H2O window.

The paper is thoughtful, synthetic, and provocative. The section on cloud radiative feedbacks in particular contains some stimulating new ideas. The paper is also, however, quite idiosyncratic. It spends much time covering ground which previous authors have covered, without much obvious payoff to the reiteration. The calculations are also quite idealized on a number of counts, which on its own is fine. But then certain quantitative conclusions are drawn (particularly regarding clouds) which can be demonstrated to be sensitive to the idealizations, and are known to differ in even slightly more comprehensive calculations. I detail these concerns below.

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. It could be shortened into a very nice commentary by compressing the review of recent work, emphasizing the very nice point that clear-sky climate sensitivity is well understood and should form our prior for climate sensitivity, and being more circumspect in the discussion of cloud radiative effects. Or, it could be turned into a more traditional journal article by attempting to flesh out some of the proposed ideas regarding cloud radiative effects, as in the research programme nicely laid out in lines 398-399.

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 H_2O feedback (sections 3.2-4.1) and CO_2 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.

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.

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.

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 CO_2 forcing (Wilson and Gea-Banacloche 2012, Jeevanjee et al. 2021b, Romps 2022 J. Clim.)

5. I 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 CO_2, 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 $W/m^2/K$ 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}.

Minor comments

1. I appreciated the discussion around line 210 that \lambda_cs is a quantity we understand.

2. I appreciated the argument around line 390 for a null hypothesis that the change in LWCRE with warming should be zero. This seems consistent with the fact that simulated changes in LWCRE are more or less symmetric about zero, e.g. Table 1 of Andrews, Gregory, and Webb 2015.

3. Eq. 24 for climate sensitivity is very nice, and a nice synthesis of recent developments.

4. line 323, \lambda_cs -> 0 : This does not occur in the presence of CO2 (Seeley and Jeevanjee 2021, Kluft et al. 2021)

5. How should Eq. 28 be interpreted? Also, note that the canonical forcing estimate of 3.7 W/m^2 includes not only cloud masking but stratospheric adjustment, the latter being a roughly 30% effect.

6. Line 310, "From Fig. 5, clouds with tops at 288 K will radiate twice as much energy per degree of warming than would the surface at 305 K." Again, the results of McKim 2021 and Koll and Cronin suggest this effect is more like a 40% increase (at an RH of 50%), not a doubling.