Author Response to RC1

Thank you very much for your very useful and to-the-point comments to my manuscript.
Although you conclude that the paper presents some interesting ideas (from the world of electronics) you find it difficult to follow and you think it needs to account for more of the recent progress concerning emergent constraints (in the field of climate research).
Below, my responses and corresponding (proposed) changes to the manuscript are provided, with your original comments in italics.

Comment
In the paper Earth System Sensitivity: a Feedback perspective, Peter O. Passenier discusses emergent constraints for equilibrium climate sensitivity (ECS) and argues that slow feedbacks (e.g. permafrost and ice sheet dynamics) are not properly accounted for in previous work. The paper is short - which is nice in many respects, but I also have a concern whether it adds new information. Science papers need to explain the current state of science on the chosen topic to demonstrate that they are up to date (scholar.google search with '"emergent constraints" AND ECS' gave 239 hits, many of which were published since 2018 - most of the cited literature herein are older than those). This manuscript doesn't do that. It may nevertheless, present some new ideas and insight, but I'm not able to say if it is or isn't. Analogies from the world of electronics, however, are interesting and probably quite novel within climate research.

Response
An important aim of my study is to support a better mechanistic understanding of interactions *in* (as opposed to improving the predictability *of*) the Earth climate system (see the Introduction). The starting point was the Cox et al. study (2018), who used the simple but elegant Hasselmann (1976) model as a basis for their emergent constraints 'update' of the ECS 1.5-4.5 K likely range, the well-familiar IPCC AR5 (2013) range already dating back to Charney (1979). I think, given the mainly pedagogic purpose of my paper, the 2018 references made still can be justified, although as you rightly point out there have been recent important community developments in this area which at least should be mentioned in the Introduction of my study (an overview of more recent research is provided in Sherwood et al. 2020 and IPCC AR6 WG1, see RC2)

Proposed changes
The Introduction section shall be extended, placing the Cox et al. 2018 and IPCC AR5 references in the more recent (Sherwood et al. 2020 and IPCC AR6) context as described above.

Comment
Another question is whether some of the derivations and mathematics presented in the Methods section should be left in an appendix.

Response
The idea behind the division between the Method section and the Appendices is to separate the 'standard' feedback analysis parts ('Simple transfer analysis' and 'Combined feedback analysis') from the main body of the paper, where these techniques are applied as a method to investigate the interactions between climate sensitivity ECS and earth system sensitivity ESS. My approach was to use the 'stochastic' Hasselmann (filter) model in a 'deterministic' (control) mode (Appendix A of the paper) as a methodological basis for the combined feedback analysis, described in more detail in Appendix B.

Comment
It is possible that slow feedbacks also affect the fast ones and that the dynamics and thermodynamics involve nonlinear interactions so that the total feedback no longer is the sum of individual feedbacks. Hence, the paper assumes that the effect from various processes are additive, which I don't think has been convincingly demonstrated. The paper does, however, discuss combined earth-system feedbacks in the context of earth system sensitivity. I think that this part needs to be explained more carefully.

Response
I agree with you that in 'reality' the interactions mentioned are probably highly nonlinear. A common approach within the community research on climate feedbacks and sensitivity (see for instance IPCC AR6 WG1 Chapter 7) is to decompose, to first order, the net feedback parameter (in my paper defined as the 'radiative damping coefficient') into a sum of terms. This also constituted the methodological basis for my extension to the assessment of earth system sensitivity. As demonstrated in the paper however, already in this simple case of additive feedback gains g_i the combined scaling relation between input and output (feedback factor f_c) becomes highly nonlinear, with possible consequences for system stability.

Comment
I find it a bit hard to see the 'red thread' in this paper, which presents a selection of 'facts' without sufficient context or explanation for why. It would be easier to follow the train of thoughts with a clearly stated hypothesis and explicit definitions. Explain why the mathematical derivations and why presenting e.g. Fig 1. It doesn't suffice doing so only in the introduction.

Response
Realizing that a lot of the above considerations, though (implicitly) addressed in the manuscript, are more or less hidden to the reader, I propose an 'Outline' section between the (extended, see my first response above) Introduction and the Method section. This section should serve as a more explicit guide in defining the main research questions and managing reader expectations to answer these questions throughout the rest of the paper.

Proposed changes
Adding of an 'Outline' section between the extended Introduction and the Method section.

Comment
In conclusion, the paper presents some interesting ideas, but I find it difficult to follow and think it needs to account for more of the recent progress concerning emergent constraints. Also, a more careful guidance through the ideas and concepts will make the paper easier to follow. It is always a bit more difficult to follow interdisciplinary work because some aspects often are a bit unfamiliar. Here, the paper relied on ideas from electronics in addition to maths.

Response
Indeed, the paper has been written in a way which implicitly assumes some familiarity with the basic concepts of feedback analysis, originating from electrical (control) engineering, as described by Roe in his pedagogic review of 2009 (see the Introduction of my paper and reference below).
I hope my responses and proposed changes above are sufficient and adequate to solve these issues mentioned by you.

Comment
***Minor**; 'IPPC' should be 'IPCC'.*

Response
Noted

References

IPCC AR6 WG1 Chapter 7:
https://www.ipcc.ch/report/ar6/wg1/downloads/report/IPCC_AR6_WGI_Chapter_07.pdf

Sherwood et al. (2020). An assessment of Earth's climate sensitivity using multiple lines of evidence. Reviews of Geophysics, 58, e2019RG000678.
https://doi.org/10.1029/2019RG000678

Roe, G.H. (2009). Feedbacks, Timescales and Seeing Red. Annu. Rev. Earth Planet Scie, Vol. 37:93-115. http://dx.doi.org/10.1146/annurev.earth.061008.134734