To Uwe Mikolajewicz:

I feel that the revised manuscript of Köhler et al. 2015 has addressed many of the issues raised in my review, and I am pleased to recommend it for acceptance in *Climate of the Past* as is. Many thanks to the authors both for these revisions, and to the thoughtful reply to my concerns.

I do wish to comment on the response to the recommendation to switch axes.

My thanks to the authors for engaging me on this point, and for performing the analysis that they did. However, I want to clairfy that this point about changing axes is not just one of nomenclature, or one that is related to models vs paleo-studies, or to transient vs. equilibrium situations.

The mechanism by which systems respond to forcings is through feedback processes. The size of the response to a given forcing is determined by the strength and sign of those feedback processes. As an example, for the Earth's climate, the predominant feedback is the Planck effect – a warmer planet sheds more energy – which is a large negative feedback that stabilizes climate. This feedback would certainly not be included in forcing. Sensitivity is a secondary quantity that emerges as a result of these feedbacks – specifically, it is the negative inverse of the sum of these feedbacks – and so we would expect changes in sensitivity to emerge as a result of changes in feedbacks. So, for example, if feedbacks get more positive as the Earth warms, then sensitivity increases.

The strength of the feedbacks predicted in Figure 7 are given by the inverse of the slopes of the lines made by the authors' fits. If the axes were flipped, the feedbacks would just be the slopes of these lines, and the changes of these feedbacks with temperature would just be the higher order terms of the polynomial fits I proposed. This was why I was making this recommendation – that is the physical motivation.

On the other hand, there are physically questionable aspects of leaving the fits the way they are. As an example, consider Figure 7a. The polynomial fit, using the original axes, implies that for a large enough CO_2 decrease, the planet would actually begin warming again.

I am glad to read that the authors investigated the effects of switching the axes, and it sounds like the results were indeed different, in some cases better fits, in some cases worse. I would argue that the fact that Foster and Pagani both became nonlinear while improving their fits is precisely one of the reasons I argued for this approach – they do in, fact, look nonlinear. However, I also wonder why these analysis were not also performed for the R[CO_2] vs. ΔT_g column. Figure 7g, which is right now barely a fit, is very easily fit by a straight line if you flip the axes. Figure 7e also seems like it would be much improved, and the sign of its curvature would match 7a, and perhaps 7c. Figure 7a would no longer have the unphysical aspect mentioned above.

I am open to further conversation on this issue within or outside of the context of this paper discussion.

Finally, to clarify, unlike Gregory et al. 2004, Bloch-Johnson et al. 2015 is concerned with equilibrium sensitivity (the curves are made by fitting the outcomes of multiple equilibrium runs). I agree that considerations are different between the two cases, but I argue that this framework (energy fluxes dependend on temperature, not vice versa) is the best way to look at this situation in either scenario.

I also want to note that there are two small typos in the sentence on lines 239-241: "Since we are interested [in] how CO2 might have changed over the last 5 Myr and [in] its relationship".

My thanks again to the authors for their important and useful research and to the editors for the opportunity to engage with it.

Yours sincerely,

Jonah Bloch-Johnson