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Interactive comment on "Bayesian estimation of Earth's climate sensitivity and transient climate response from observational warming and heat content datasets" *by* Philip Goodwin and B. B. Cael

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We thank both reviewers for their helpful and insightful comments. Below we show how we shall amend our manuscript for a revised submission to address the points made by Reviewer #2.

"Review #2 This study uses a series of observations and a relatively simply climate model with explicit parameters to try to constrain climate sensitivity (ECS) and transient response (TCR) to CO2 doublings. The model includes feedbacks on two timescales

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which leads to larger ECS than what would be the case if feedback is assumed constant. Overall, I find the paper is fairly clear and fills a niche in the literature, nevertheless, I did not notice some room for improvements. Therefore I recommend only to accept this study for publications after major revisions have been undertaken."

We thank Reviewer #2 for their careful reading of the manuscript and insightful comments. We are pleased the reviewer finds our manuscript clear and to fill a niche in the literature. Below, we identify how we will improve the manuscript during revision in light of the points raised by the reviewer.

"Major points I am worried that the authors are overconfident in the ability to constrain slow feedback based on historical warming. Slow feedbacks are known to evolve continuously from years to centuries (e.g. Rugenstein et al. 2020), but in this study they are limited to acting over timescales of a few decades. It is in this conjunction, where in historical warming happened for the most part over a period ofâĹij50 years (since the 1960-70s), that I am concerned as to whether sufficient signal is available to constrain the slow feedback. At the very least the"

We agree that the method used in the manuscript does not constrain the 'slow feedback' consisting of all feedbacks evolving over timescales from years to centuries. On re-reading the manuscript in light of the reviewer's comments, we agree that the manuscript could appear overconfident in its ability to provide a constraint on this 'slow feedback'. A revised manuscript will introduce clarity in this area, carefully caveating the methodology and results.

The following changes will be employed:

(1) We will re-name lambda_slow to reflect the timescales that it does (and does not) consider:

What our manuscript has named 'lambda_slow' is not in fact the total slow feedback evolving from years to centuries, and we see how this naming of lambda_slow may cause confusion for readers into thinking that it does represent all slow feedbacks.

In fact, ‘lambda_slow’ in our mansucript represents only the feedbacks acting with multi-decadal ($\sim$ 25 to 40 year) e-folding timescales. One key slow feedback acting on such timescales identified within the literature is often termed the pattern effect, whereby sea surface warming patterns evolve over a multi-decadal timescale and change lambda. However, since our method cannot separate this pattern effect from contributions of any other feedbacks acting on similar timescales, we adopted the term lambda_slow as a catch-all. The problem with lambda_slow (as identified by the reviewer) is that it implies we are considering slow climate feedbacks acting on longer timescales as well (which the reviewer rightly points out that our method does not consider).

In a revised manuscript, we will adopt a different name for lambda_slow that explicitly reflects the multi-decadal timescales that it considers. We will also explicitly state that our multi-decadal feedback term does not include other slow feedbacks that evolve over longer (e.g. century) timescales.

What our methodology has achieved is to explore possible combinations of fast climate feedback and multi-decadal feedback that are consistent with historical observations of global mean temperature and heat content anomalies. We agree that the historical record is insufficiently long to fully explore longer timescale (century) feedbacks, and we have not done so.

(2) We identify regions of parameter space with both amplifying and damping multi-decadal feedbacks that are consistent with observations. We will highlight that our results are perfectly compatible with multi-decadal feedbacks that act to dampen the increase in global temperatures – even though our best estimate is for multi-decadal feedbacks that amplify warming.

“That said, it will probably attract attention that the authors claim to be able to constrain slow feedbacks as amplifying slow warming. Here, however, the prior assumption appears to by a uniform distribution from -3 to +2 Wm-2K-1, i.e. skewed to negative values, and thus assumed a priori to be amplifying. I would like to have the authors choose a prior that is symmetric about zero for lambda_slow."

Agreed that it is important to show that the best estimate of amplifying multi-decadal feedback in our results is not a result of the prior distribution we have adopted. In a revised manuscript, as suggested, we will extend our exploration of parameter space by also considering the section with multi-decadal feedback from +2 Wm-2K-1 to +3Wm-2K-1. This will then have sampled parameter space from -3Wm-2K-1 to +3Wm-2K-1, and so will not be a prior skewed to amplifying values.

Note that the posterior distribution for lambda_slow (the culti-decadal feedback) is already firmly in the tail of the distribution before getting to +2 Wm-2K-1 (Figure 2, dotted and dashed red lines), and so we anticipate relatively small numbers of posterior simulations to be identified beyond +2Wm-2K-1.

"The difference in slow feedback between the two temperature datasets is interesting. However, the explanation provided that they differ mostly be Cowtan and Way having more warming in the recent years seem insufficient. If one plots the difference over the entire record, and not just since 1960, then you realise that mostly the difference arises around the year 1900, and after 1910 the correction is remarkably stable (attached). It would seem that it should be possible to figure from where in the time series the signal that constrains slow feedback comes from?"

We thank the reviewer for pointing this out, including with their useful figure. We Agree that the difference in temperature records is not simply that the Cowtan and Way record has more warming since 1960, and that it is more complicated. In a revised version we will amend the manuscript to reflect this. We do not think that our methodology (so far) will allow us to state with clarity where in the temperature records the key differences are that affect the constraint on the slower feedback. We agree that this is interesting and will reserve this for future work.

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“The treatment of constraining data is also troublesome. 1) There is no particular reason to use HadCRUT without infilling. HadCRUT is only available where observations wereconducted, and so has a low bias as the unobserved high latitude regions, where thereis warming amplification according to climate models, are not included. Cowtan andWay infilled datasets, including that of HadCRUT but also based on other datasetssuch as COBE. I would suggest referring to them as ’HadCRUT in-filled’, rather than ‘Cowtan and Way”’

We include HadCRUT4 without infilling as it contains more sources of uncertainty in the published uncertainty estimates. We use Cowtan and Way (i.e. ‘HadCRUT with infilling’) because it statistically infills missing regions of data. In our original manuscript we avoided making judgements on the relative merits of different datasets. Reviewers #1 and #2 have both highlighted valid reasons for preferring particular datasets due to their methodologies. In a revised manuscript we will also highlight these reasons for preferring the estimates of climate sensitivity from infilled records of temperature anomaly.

“2) I am not sure why the authors include HadSST3.1 as a separate constraint, this data is already part of HadCRUT.”

We agree that the reasons for including a sea surface temperature constraint in addition to global mean surface temperature should be explained in a revised manuscript. Briefly: The WASP model contains an input parameter stating the ratio of global sea-surface warming to global mean surface warming at equilibrium (r1 noted in the supplementary material). As this parameter varies between ensemble members, simulated global mean surface warming and sea surface temperature warming may vary differently (relative to each other) across the ensemble. Therefore, one constraint for ‘SST warming only’ is required to help constrain the posterior values of r1 within the WASP ensembles, hence the use of HadSST3.

“3) I am worried about including ocean carbon content as a constraint, atmospheric

CO2 is prescribed so all this does is to help constrain the exchange rates which are apparently shared with heat transfer. It is, however, well-known that the physical processes of ocean heat- and carbon uptake are different. I suggest removing this constraint."

We agree that the use of ocean carbon uptake should be explained in a revised manuscript, and we agree that the processes of heat and carbon uptake by the ocean are different.

The WASP model contains a specific parameter that speficies how different the processes of heat and carbon uptake by the ocean are in the simulation (r2, noted in the supplementary material). This parameter (r2) is varied between ensemble members, and the only way to constrain the values of r2 that reach the posterior distribution is to include ocean carbon uptake (alongside ocean heat uptake) as one of the historic constraints. If we did not use ocean carbon uptake as one of the historic constraints, then the WASP model could achieve acceptable ocean heat uptake levels with unrealistic input parameter values for ocean circulation timescales alongside a compensating unrealistic value for r2 – hence the use of ocean carbon uptake alongside ocean heat uptake constraints. We will explain this in a revised manuscript.

"Minor suggestions

29, Please mention here the sign convention. It seems the authors use a positive sign for the Planck feedback, which is a negative stabilising feedback, and negative signs for the positive feedbacks in the climate system (water vapor, surface albedo). Most readers will be confused over this, although I realise many British authors apply this convention."

Agreed that there are two sign conventions in use in the literature for climate feedback. We will explain in a revised manuscript that our sign convention derives from the definition of lambda as the 'increase in outgoing radiation for a 1K rise in global mean surface temperature'. We will also highlight what this means for amplifying and damping feedback processes in terms of the sign of lambda.

“47, Tokarska et al. (2020) only did TCR, not ECS. ECS was constrained based on recent warming by Bengtsson and Schwartz (2013), Jimenez-de-la-Cuesta and Mauritsen (2019) and Nijsse et al. (2020).”

We thank the reviewer for these recommendations, we will amend the references cited here in a revised version.

“53, perhaps delete ’at any given time or timescale”’

Agreed, this will be deleted in a revised manuscript.

“58, perhaps worthwhile mentioning those studies that are relying on these models, and why the authors of this study believe their method makes avoiding GCMs for estimating time-dependence is possible? See also major points.”

Agreed that citing literature using GCMs to estimate time-evolving climate sensitivity here would improve the manuscript for the reader. In a revised manuscript, we will also explain here how our methodology works: We sample values of fast climate feedback and multi-decadal climate feedback (and other parameters) looking for combinations that give rise to historic warming and heat content anomalies that are consistent with observations.

“71, ’Quisque’ is not a word in my vocabulary. According to wikipedia it is a pre-historic herring.”

Thank you, this word was a typo and shall not appear in a revised version.

“81, perhaps nit-picking, but surface albedo feedback, at least that associated with seaice, is not as fast as water vapor, see for instance Tietsche et al. (2011) that find a 1-2year timescale.”

Agreed, it is true that the surface sea-ice albedo component of the fast feedback does strictly have a timescale longer than the residence timescale of water vapour in the atmosphere. We shall mention this in a revised manuscript.

“89-90, It would be useful to display the used forcing in a figure, for example to show priors and posteriors of for example aerosol forcing, equivalent to Figure 2.”

Thank you for highlighting this, we agree this would be beneficial. Yes - in a revised manuscript we will display a range of forcing figures, either in the main text or supplementary material, showing the priors and posteriors for different sources of radiative forcing (including aerosols).

“161, However, very strongly cooling aerosols would result in mid-century cooling because of the different evolutions of aerosol and greenhouse gas forcing (e.g. Stevens2015, Bellouin et al. 2019). Supposedly the bayesian method applied automatically filters out these values, which is why I would like to see the posterior distribution of aerosol forcing.”

We agree that very cooling aerosols would result in mid-century cooling. In a revised manuscript, we will present the posterior distribution of aerosol forcing as a new figure in the main text or supplementary material. We also agree that our Bayesian approach filters out combinations of aerosol and greenhouse gas forcing that result in unrealistic evolutions of historic temperature or heat content anomalies.

“204, Here, I suggest to again remind the reader of the sign convention”

Agreed, in a reivsed manuscript we shall remind the reader of the sign convention adopted for climate feedback again here.

“218, why not use a doubling of CO2? This is how ECS is defined.”

We agree that a CO2 doubling would also work here. We have chosen to use a 4xCO2 perturbation, in line with on of the standard idealised scenarios for CMIP-class models. Note that WASP does not (yet) have a state-dependence on lambda and so the results of a 2xCO2 experiment would be equivalent (but with a slightly lower signal to noise ratio, where the noise is driven by the imposed interannual variability in Earth energy balance in WASP: see line 96).

“224, by 90 do the authors mean 5-95?”

Agreed that this was unclear, in a revised manuscript we will specify the upper and lower percentile bounds of confidence intervals as well as the sizes of the intervals.

“248-250, or perhaps a better constraint on total lambda, say based on paleoclimates?”

Agreed, we will mention that other approaches (such a palaeoclimate) would help with constraining total climate feedback.

“270, this section added no new information that had not already been provided. I suggest removing it. 277, yet these components only explain 1/3 of the total variance?”

We agree our manuscript that lacked clarity on the benefits and motivation behind the principle component and stepwise regression sections (4.2.2 and 4.2.3 respectively). We will explain the motivation and benefits of these sections more clearly in a revised manuscript. Briefly: it is advantageous to understand how many degrees of freedom a climate model has for being observation-consistent with global temperature and heat content constraints up to the present day. The fact that a large fraction of the variance (around 1/3) in the posterior ensemble is explained by a much smaller number of degrees of freedom than exists in the prior model ensemble is a significant finding. In part, understanding the posterior ensemble's degrees of freedom may help to sample parameter space with a smaller number of ensemble members – which becomes more significant for constructing ensembles with complex model that are more computationally expensive.

“353, this statement requires there are no slow feedbacks acting on timescales from decades to millennia. I recommend to remove this statement, or strongly caveat.”

Agreed, there are many slow feedbacks acting on many timescales from multi-decadal to millennial. What our manuscript explores is a particular multi-decadal feedback-timescale, rather than all slow feedback timescales. In a revised manuscript we will re-

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name lambda_slow to (e.g. lambda_md, for lambda_multi-decadal). This will remove confusion (both in this section and elsewhere) about which slow feedback timescales we explore, and which we do not explore, throughout the manuscript.

“368, not ’multiple’ but ’two distinct’ timescales.”

Agreed that greater clarification is required. The WASP model code allows for ‘multiple’ timescales to be considered, but we will specify in revision that in this manuscript we have considered precisely two distinct timescales (in addition to the instantaneous Planck feedback).

“370, IPCC ’likely’ means 66 percent probability or better.”

Agreed, we will clarify the sentence in a revised manuscript to specify IPCC definition of ‘likely’.

“371, for Sherwood et al. (2020) probably the number referred to is 17-83 percent.”

Agreed, we will clarify this in a revised manuscript.

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