Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2020-79-RC2, 2020

Earth System Dynamics
Discussions

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

Anonymous Referee #2

Received and published: 30 November 2020

Reveiw of Goodwin and Cael for ESD.

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 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.

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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 of ∼50 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

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.

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?

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 were conducted, and so has a low bias as the unobserved high latitude regions, where there is warming amplification according to climate models, are not included. Cowtan and Way infilled datasets, including that of HadCRUT but also based on other datasets such as COBE. I would suggest referring to them as 'HadCRUT in-filled', rather than

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

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.

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).

53, perhaps delete 'at any given time or timescale'

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.

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

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

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.

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. Stevens 2015, 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.

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

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

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

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

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?

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.

368, not 'multiple' but 'two distinct' timescales.

370, IPCC 'likely' means 66 percent probability or better.

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

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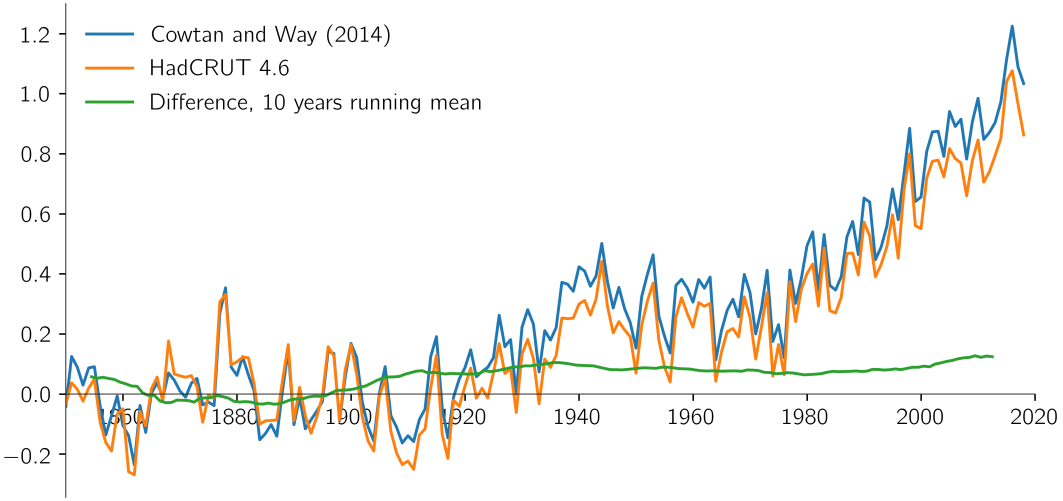

Fig. 1.

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