Earth Syst. Dynam. Discuss.,
https://doi.org/10.5194/esd-2017-119-RC1, 2018

Earth System Dynamics
Discussions
Open Access

Interactive comment on "Climate sensitivity estimates – sensitivity to radiative forcing time series and observational data" *by* Ragnhild Bieltvedt Skeie et al.

Anonymous Referee #1

Received and published: 23 January 2018

The paper "Climate sensitivity estimates - sensitivity to radiative forcing time series and observational data" (by R.B. Skeie, T. Berntsen, M. Aldrin, M. Holden, and G. Myhre) is a sensitivity study on the use of different observational datasets to infer estimates of the effective climate sensitivity using an energy balance model in a Bayesian framework. The paper is essentially a refinement of the results of a previous publication, including a systematic analysis of how the use of different data sources impacts the estimates obtained with the model. The paper is of interest from a methodological point of view. The presentation of the methods and of the results is however very confusing. Overall the paper is suited for publication in Earth System Dynamics, after a substantial revision of the presentation of the methodology and of the results.

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Discussion paper

The authors employ an energy balance climate/upwelling diffusion ocean model, giving as output the surface temperature in the two hemispheres and the global ocean heat content. The model is combined with a stochastic model representing long and short term variability as well as model errors. The equilibrium climate sensitivity is a parameter of the deterministic model, and is constrained by observations in a Bayesian inference. The authors use several observational datasets, including radiative forcings, global surface temperatures, and ocean heat content, performing a fairly systematic analysis of the role of each data source in determining the final results.

The authors have used the same method in a previous publication (Skeie & al 2014) with partially different data. The estimate of the climate sensitivity and its confidence interval change by 5-10%, remaining well inside the range of values obtained with other methods, including the range given by the IPCC report obtained running complex general circulation models. Such a minor change in the main estimate might question the relevance of the new results. However, the authors show in detail how each of the different datasets included or modified with respect to Skeie & al (2014) contribute to determine the final estimate, and how compensations between changes of different sign lead to an overall small final effect. Particular attention is given to the role of the ocean heat content. The paper is therefore of interest for readers working with this kind of methods. The method has been used previously in other publications and I have no major scientific criticisms, a part from a few questions which I include in a bullet list below.

My main criticism to this paper regards the presentation of the methods and of the results. In order to find informations which are essential to have even a minimal understanding of what the authors describe in the main text, the reader is systematically asked to go back and forth between appendixes, supplementary materials, and the authors' previous publication history. As a result, the paper in its current form is extremely hard to follow. For example, no real description is given of the energy balance model. The reader is referred to Skeie & al (2014) and/or Aldrin & al (2012), and even there the informations are fragmented and partially referring to older publications. And I am not talking about the details of the model. For example, in the description of the model/methods I could not find a qualitative description of how the components of the parameter vector θ are included in the model. However, some of these parameters are later discussed in the paper, out of the blue for a reader who has not worked with this specific model. This is just an example, there are many others. The same holds for the way the deterministic model is combined with the stochastic terms, and many other aspects of the procedures followed by the authors to obtain their results.

While it is clear that the technical details of the models and of the methodology can (must) be left out of the main text of the paper, in particular if they have been described elsewhere, a minimal but clear and comprehensive description of the models and methods must be present in the paper. To the maximum extent possible, the paper has to be readable stand alone. A similar point holds for the use of appendixes and supplementary materials. They should be used to provide technical details not necessary to follow the flow of the main text, or figures giving complementary informations. Instead, in the way the authors use them, there is no logical difference between figures included in the main text and figures included in the supplementary informations, and in order to understand what the authors have done (again, not the details: the very procedure) it is often necessary to stop reading, move to an appendix, and then come back to to main text. This is extremely confusing, and makes the paper unnecessarily hard to read.

The authors should revise their paper in order to make it clear and readable. A simple but comprehensive description of the models and methods they use that are not standard techniques should be provided. The interplay between the main text and appendixes and supplementary materials should be simplified. That said, I have some more specific remarks which I list below.

1. Page 2, lines 3-6. "Since the current climate is in a non-equilibrium state observationally based methods can only account for the feedbacks operating during the historical period. Thus, these estimates are often referred to as inferred or effective climate sensitivity (Armour, 2017;Forster, 2016) and are generally significantly lower than ECS estimates from Atmosphere-Ocean General Circulation Models (AOGCMs).". Just a comment on this. This is an important remark and I agree with the authors in stressing the difference between "real" equilibrium climate sensitivity and inferred climate sensitivity. Actually, this can be seen rigorously and generally in response theory of dynamical systems. In this framework the ECS can be written as a weighted integral of the imaginary part of linear susceptibility of the system over all frequencies, which implies that one needs all time scales in order to correctly compute the ECS. A similar result holds for transient definitions of the climate sensitivity, like the Transient Climate Response. The authors can find a discussion on this for example in Ragone & al (2016) (equations 9 and 14) and Lucarini & al (2017);

2. Page 3, lines 1-7. Here it would be good to give a (very) brief description of energy balance based estimates, of the peculiarities of the method developed by the authors, how they have used it in the past and which results they have obtained, and what the current paper adds to these previous works. This is somehow already done, but it should be more clear and systematic;

3. Page 3, lines 10-28. The description of the model and methods should be expanded and made clear;

4. Page 3, line 21-23. “Most of the data series are provided with corresponding yearly standard errors. However, these are often small compared to the differences between the data series, indicating that the errors reported by the data providers are too small”. This is an interesting observation by the authors, and I agree with them that in this situation using only one dataset would result in

assuming uncertainties that are probably too small. However, to say that data providers provide errors that are too small is quite a statement. If the authors are aware of a discussion in the literature on the statistical non consistency between different datasets of the quantities they refer to, it would be good to be more specific on which datasets are in disagreement with each other and to include some references;

5. Section 2.1. The results about the Transient Climate Sensitivity are actually interesting. If the authors find it possible, I would include them in the main text and discuss them a bit more;

6. Section 3. Forc_Skeie2014 and Forc_AR5 have never been defined;

7. Page 4, lines 29-32. Why the match between prior and posterior distributions in figure 3 changes so much between cases A and B? The authors somehow discuss this in lines 6-12 of page 5, but the change is impressive. Can the authors say something more about this? Note also that a clear definition of the priors is somewhat missing in this paper (the one of the equilibrium climate sensitivity for example is not mentioned at all). The authors probably assume that the reader should go looking for it in Skeie & al (2014), which I did, but it is one of those things that should be repeated also in this paper when presenting the methods;

8. Page 5, lines 25-30. The data from Ishii and Kimoto are completely out of the confidence interval, in particular in case B. Can the authors comment about this?

9. Page 6, lines 22-23. "... and hence no reason to refine the IPCC 2013 aerosol ERF best estimate jet.", there is something wrong with this sentence;

10. Page 7, lines 8-14. If the authors had used the same errors as in Johansson & al (2015), how would the range of climate sensitivity differ? In other words, how much of the larger range is due to considering larger errors and how much to the differences between their methods? Can they comment on this here?

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Discussion paper

Beside this, there are a number of typos and errors that should be taken care of. For example, use CI instead of C.I., consistently with the use of other acronyms. Please revise the paper also from this point of view.

References

Ragone F., Lucarini V., Lunkeit F., A new framework for climate sensitivity and prediction: a modelling perspective, Clim. Dyn., 46(5-6), 1459-1471 (2016);

Lucarini V., Ragone F., Lunkeit F., Predicting climate change using response theory: global averages and spatial patterns, J. Stat. Phys., 166(3-4), 1036-1064 (2017).