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Interactive comment on "A lower and more constrained estimate of climate sensitivity using updated observations and detailed radiative forcing time series" *by* R. B. Skeie et al.

Anonymous Referee #1

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Review Comments on:

Journal: ESD Title: A lower and more constrained estimate of climate sensitivity using updated observations and detailed radiative forcing time series Author(s): R.B. Skeie et al. MS No.: esd-2013-20 MS Type: Research Article

General Comments

Using an energy balance model, the authors estimate the equilibrium climate sensitivity (ECS) using updated observations of global temperature and ocean heat uptake change. The method is based – and extended here – on previous papers of Aldrin et al. 2012 and Skeie and co-workers. As the authors note, there are five main differ-

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ences between this study and Aldrin et al. 2012 (end of section 2.1.). As main result of their study, the authors find a lower and more constrained estimate of ECS in their model. A broad background into the general topics of the paper (constraining climate sensitivity, observations of ocean heat uptake) are given. However, it is suggested to focus on the new additions presented here (for example their treatment of long-term internal variability).

Their results and approach is interesting and their lower values of ECS is in line with results from previous study (e.g. Otto et al. 2013; Lewis et al. 2013). Since the method and approach of using reduced complexity models to estimate ECS has been extensively used over the last ten years, it is suggested that the authors strengthen the focus of the paper on their results and not so much on general background, and in particular on the physical interpretation of their lower climate sensitivity estimate. The main results of their study are presented in the paragraph starting "The drastic reduction in uncertainty [...] with ten more years of data may be surprising. We believe there are two main reasons for this". It is suggested that this important finding is discussed in more detail. In particular, the limitations of the climate model in reproducing the observed record and its relation to low climate sensitivity values should be discussed. In that sense, it is also suggested that already the Abstract highlights that this is a model based result and the limitations and advantages of both the climate model and the method employed here could be more highlighted.

In that regard, the structure of the Results section could be particularly strengthened. In its current state, the main analysis presents the results and their discussion at the same time. For example, section 3.1. presents the estimates of ECS, but also includes a discussion of their relation to previous results and to the likely range presented in the IPCC AR4. It is suggested that the Results section presents the results in a short, concise way and the discussion of the results is shifted to the Discussion section.

The discussion could also be shortened and more focused. For example, it gives a broad overview of ocean heat content and for example instrumental issues associated

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with measuring ocean heat uptake (section 4.2). This is clearly important and has to be mentioned, but the focus of the paper is in using these observations as constraints for the model.

Also in that regard, it is suggested that the general use of references is reconsidered in the sense that mentioning of previous work and results of the references is more focused on the context and content of this paper. For example, the authors often use the expression: Author et al. (xxxx) investigated / Author et al. found. Albeit important, distilling the essence of the references and how they are specifically related to the paper might enhance the readability of the paper. The references are very up to date and their extent is impressive.

The authors present a very interesting sensitivity study of their results. The results of the sensitivity study could be presented in special section dedicated to this and not as currently combined with the results of the main analysis. This would make it easier for the reader to understand the results of the main analysis.

Specific Comments

The authors use a uniform prior for ECS – is there a possibility to test the results also for a non-uniform prior to test their sensitivity to the choice of ECS prior? An important and interesting approach is the use of multiple observational timeseries simultaneously (putting them into a vector). The observations are strongly correlated - is this correlation across the observations taken into account in the parameter estimation process (in a co-variance matrix or similar)? If not, it would be like having three independent observational constraints (both for temperature and ocean heat uptake), which would put a too strong constraint on the model parameters. A short clarification would be appreciated.

The authors employ anthropogenic RF series of Skeie et al. (2011). How are they related to the data for the historical Representative Pathway Concentration (RCP) data that other models use? Maybe a figure (in the Supplementary) comparing the two could

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help the reader to see the size and evolution of the two forcing datasets. Regarding the standard deviations of all stochastic terms: the authors note that in constrast to other studies, they are estimated here from the data. Is there a chance of compensation between different uncertainties?

Regarding the comparison between the observed temperature and simulated temperatures with the posterior model parameters: the authors mention in section 3.1. that observations are outside the 90% C.I. of the fitted temperature increase in the Northern Hemisphere over the last two decades. The authors also note that this uncertainty range includes only deterministic terms. This could be a very important point in the physical interpretation of the results. How can the climate model used in the study reproduce the last decade of only very small temperature increase while there is a positive forcing? Is there a chance that the low climate sensitivity value is a statistical effect - in the sense that if there is a positive forcing, the climate model responds by tuning down its climate sensitivity. A discussion of this would be very helpful and appreciated.

The introduction of the long-term variability term is very interesting. A comparison between its posterior estimates and the model error term in Fig. 4. suggests that the model error is largely dominated / represented by this long-term variability. I would just have a little comment regarding its interpretation: there could be a similarity between structural uncertainty of the climate model used here in reproducing the observed record and the notion of unforced internal climate variability (and Fig. 4 also shows that they are correlated). In terms of attribution, the authors note thate “during the period 1910-1940 and 1970-2000 a warming of about 0.2 K can be attributed to internal variability”. Or could it be that this warming could be due to climate processes not represented in the climate model ? A short comment regarding the interpretation of the internal variability term and structural model limitations in an attribution framework would be much appreciated.

Technical Comments

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Both the size and font of the Figures could be enhanced to improve readability.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 785, 2013.

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