Reviewer #6 Evaluations:
Recommendation: Reject
Grammar improvements needed: No
Commentary: No
Willing to review a revision: Yes
Do you have a potential conflict of interest?(Required): No
Annotated: No

Reviewer #6 (Comments to Author):

This paper addresses a topic of interest to the community, namely the uncertainty in projections of future climate change. I read the paper with interest, but unfortunately I discovered a fatal error that leads me to reject the conclusions. In the end, I recommend rejection.

The paper makes an elementary but fundamental error: it confuses errors in the models' base state with errors in the models' predictions of how the climate will change. The fact that models can have large biases in their base state is well documented; e.g., previously published work has shown biases in their water vapor and temperature fields (e.g., John and Soden (2007), Temperature and humidity biases in global climate models and their impact on climate feedbacks, Geophys. Res. Lett., 34, L18704, doi: 10.1029/2007GL030429), and I have no doubt that some GCMs have large biases in their cloud fields (as this paper argues).

However, this does not mean that the *change* in these fields as the climate warms in the models is wrong. John and Soden showed that, despite the biases in the water vapor fields, the **change** in water vapor in response to warming is nearly identical among the GCMs, meaning that the water vapor feedback is nearly identical. Comparisons of the cloud feedbacks in the GCMs shows good agreement among the GCMs, and with observations (e.g., Dessler, A. E. (2013), Observations of climate feedbacks over 2000-10 and comparisons to climate models, J. Climate, 26, 333-342, doi: 10.1175/jcli-d-11-00640.1). This means that, despite large differences in the cloud fields, the change in clouds as the climate warms is basically the same.

Thus, taking an error in the base state and assuming that error translates into the error in the climate response is unsupported by previously published analyses. It also leads to some ridiculous conclusions. For example, Fig. 7b of the paper shows that the uncertainty envelope of future temperatures ranges from -15{degree sign}C to +20{degree sign}C. In other words, the author suggests that anthropogenic forcing could lead to **cooling** of the climate. That's an absurd conclusion: simple physics tells us that a positive radiative forcing will lead to warming. The fact that the uncertainty envelope includes cooling tells me that this

uncertainty calculation is fatally flawed.

There are many other reasons to suspect that this uncertainty analysis is wrong. If errors in the base state translated into errors in the climate response, then why do all of the models predict very similar values for the 1% runs in Fig. 2a? And why do the models have (relatively) similar climate sensitivities? The reason is that the feedbacks are similar in the models (Dessler, 2013) and the forcing from carbon dioxide is also similar (e.g., Andrews et al. (2012), Forcing, feedbacks and climate sensitivity in CMIP5 coupled atmosphere-ocean climate models, Geophys. Res. Lett., 39, doi: 10.1029/2012gl051607). Thus, despite the documented biases in the models, all of the evidence we have tells us that those biases don't affect the climate response of the model.

In addition to the fundamental error noted above, the paper is littered with other serious errors, many of which would merit rejection on their own. Here are two examples:

The "passive warming model" ignores important physics - namely, the heat capacity of the ocean and how it slows warming of the planet. Simple models incorporating this have been calibrated to the GCMs by other researchers (e.g., Geoffrey et al. (2013), Transient Climate Response in a Two-Layer Energy-Balance Model. Part I: Analytical Solution and Parameter Calibration Using CMIP5 AOGCM Experiments, J. Climate, 26, 1841-1857, doi: 10.1175/jcli-d-12-00195.1), and the author should look at these other papers to see how it should be done.

The author calculates the fraction of the greenhouse effect due to CO2 and comes up with 42%. As the author acknowledge, this is much higher than previously published estimates, which put the number closer to 20%. In order to get 42%, the author assumes that clouds contribute nothing to the greenhouse effect, which is absurd. This seems like a minor issue, but the 42% number plays a key role in the analysis (e.g., eq 6) and replacing it with a more reasonable choice might create grave problems. If the author wants to stick with 42%, then they have to provide some evidence that clouds contribute little to the greenhouse effect.

I could go on and provide more examples of problems in the paper, but I hope I've made my point that this paper is not publishable in anything close to its present form.