Reviewer #3 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 #3 (Comments to Author):

This is a second review of a paper that addresses a topic of interest to the community, namely the uncertainty in projections of future climate change.

In my first review, I suggested that there were fundamental problems with the paper. Unfortunately, the author made no changes to the paper to address these concerns. He did write a response to my review, but I found his arguments to be extremely unconvincing. Given this experience, I see little chance that the author is willing to make even the most cursory effort to address serious defects in the paper and I therefore reluctantly recommend REJECTION.

Just to make sure we're on the same page, let me restate my primary problem with this paper: the crux of the paper is the key assumption stated on lines 590-594: "The rationale for eqn. 8 is straightforward. The response of the climate to increased CO2 forcing includes the response of global cloud cover. However, global cloud cover is not simulated to better than average {plus minus}12.1 %. The resulting LWCF error means the magnitude of the change in tropospheric thermal energy flux in response to GHG forcing is not simulated to better than {plus minus}4 Wm-2."

What he's saying is this. Imagine you have a model that simulates some cloud coverage at some temperature C[T]. But climate change is a problem of derivatives, i.e., dC[T]/dT. The author assumes that the error in C[T] is equal to the error in dC[T]/dT. This assumption is key - without it, the entire paper collapses. The author makes no effort to prove it, but rather simply asserts it is true.

I believe that this assumption is wrong. It is neither obvious nor common sense that this is true and I think the published literature shows the assumption to be wrong. As I mentioned in my original review, John and Soden (2007) showed that, despite large differences in the base state, all of the models predict the same water vapor feedback - in other words, d(H2O[T])/dT is the same throughout the model ensemble despite significant differences in H2O[T]. And Dessler (2013) showed that the models all produce similar cloud feedbacks, despite the large differences in the cloud fields documented in this paper. Both of these papers tell me that errors in the base state are not necessarily the same as errors in the derivative.

The response from Frank to the John and Soden paper is extremely long and detailed, but seems to misunderstand my point. I am not arguing that the models are correct, only that the models show that the errors in the base state are not the same as the errors in the derivative. The Dessler paper doesn't explicitly make this point, but it does show that the feedbacks are generally similar between the models - there are some differences, of course, but these differences are much smaller than the suggested error here.

Much of the author response is also based on the fact that the papers I cited are mainly model studies and don't explicitly validate the response in the models. If one is interested in specific validations of the model response, then there are many many papers that have estimated climate sensitivity, and they generally cover a range of 1.5-4.5{degree sign}C for doubled CO2, in reasonably good agreement with the model ensemble. It also means that the spread in the models is a reasonable estimate of the uncertainty in future temperatures. This is yet another reason I suspect the results of this paper are entirely wrong.

Let me be clear: there certainly ARE errors in the response derivatives in the models - after all, equilibrium climate sensitivity does vary by a factor of about 2 in the models. But the uncertainty implied by this is much smaller than the uncertainty this author derives.

The author did address a few of my many other comments, but others were not adequately addressed.

One problem that remains is the estimate of CO2 fraction of the greenhouse effect. In my original review, I pointed out that the author assumes clouds have zero greenhouse effect (in his response, he protested that he didn't, but he should re-read lines 187+188 to see that he indeed does). In so doing, the author arrives at CO2's fraction of the GHE is 42%.

This is larger than previously published estimates using the GISS climate model, which gets 20%. In his response, the author says that we shouldn't believe the 20% number because it comes from a single climate model, and other models might disagree. So what does the author do? He bases his estimate on a 1967 paper by Manabe and Wetherald, a single 50-year old study that uses a single, radiative-convective model. I agree that one must look skeptically at a single GCM result, and an average of the ensemble of GCMs would clearly be preferable, but the Manabe and Wetherald paper should not be taken as more accurate than the far more modern GISS model analysis. That's particularly true given that an answer of zero is physically unreasonable: we KNOW that clouds cover a large fraction of the planet and they have a large impact on top-of-atmosphere long wave flux. There may be uncertainty in the value, but zero seems like a poor choice.

In fact, there's a weird contradiction here: this entire paper is based on propagating the LW error of clouds. Yet the fact that the LW effect of clouds is important to the energy budget contradicts the assumption that the GHE of clouds is zero.

There remain other major problems in the paper, but documenting all of them would be a substantial investment in energy that I don't think the paper merits.

Other comments:

Comparing AMIP models to CMIP models is not a fair comparison (around line 506).

If this paper is published in anything close to its present form, which I don't recommend, I would insist it be substantially shortened. It reads like a Masters Thesis, which includes a lot of tutorial info, and I suspect it could be cut to about one-third of its present length by removing that which is well known in the field and other completely unnecessary text (e.g., lines 132-180).

The paper focuses on LWCF error. However, the radiative error will tend to be offset by a compensating error in the SW. The author needs to discuss and estimate the magnitude of this compensation.

Confusions abound in this paper. Sect. 2.4.3 is a hard-to-follow discussion about "differencing from a base state" and how that doesn't remove errors. On the other hand, the author put into his response to me (Fig. R2) a plot showing that the models have large differences in the absolute temperature of their simulated climate, but subtracting off the biases leads to quite good agreement in the temperature trend over the 20th century. This seems to contradict Sect. 2.4.3 and it points up another problem with the construction of the paper. There seems to be a lot of shoddy philosophizing in the paper (e.g., sect. 2.4.3) that contain no numbers or calculations and which are clearly contradicted by calculations made elsewhere.