J

Dear Dr. Frank:

Thank you for submitting your manuscript for publication in Journal of Geophysical Research - Atmospheres. I have now received 3 reviews of your manuscript. It has been examined by expert reviewers whose comments are enclosed. The reviewers have expressed serious reservations about this work that cannot be addressed through any level of revision. The work is fundamentally flawed.. In light of the comments received, I am unable to accept the manuscript for publication in Journal of Geophysical Research - Atmospheres.

I am enclosing the reviews, which you may find helpful if you decide to revise your manuscript and submit to another journal. Do not resubmit again. I am sorry that I cannot be more encouraging at this time.

Thank you for your interest in JGR- Atmospheres.

Sincerely,

Steve Ghan
Editor-in-Chief, JGR-Atmospheres

****
Reviewer #1 (Comments to Author):

Attached as PDF.

Reviewer #2 (Comments to Author):

This paper sets out to show that model projections are too error-prone to make meaningful statements on detection and attribution.

The manuscript it not written clearly enough to follow a logical argument through the paper. The author did not address critical points made by previous reviews, but rather tried to pick holes in the review comments so as not to consider their legitimate points. At least two major points make the paper unsuitable to JGR, and I think it should be rejected.

1. The physical model. The author purports to be using a mathematical (non physical) model, and therefore states that they do have to worry about model physics. Yet their model clearly makes assumptions about radiation and cloud physics and their interaction that are never tested and its limitations never discussed.
2. As stated by an earlier review they assume that errors in cloud forcing translate into errors in climate response. They never justify this approach adequately or explain their reasoning on page 20. Saying that 4Wm-2 of error is felt by the climate system is one thing, but then translating this into an annual error in climate response, as they seem to, is totally unjustified. To say that this error indicates that temperatures could hugely cool in response to CO2 shows that their model is unphysical

Reviewer #3 (Comments to Author):

This paper, which has its genesis in a similar exposition by Frank (2008) in 'Skeptic' magazine, purports to emulate the result of GCM calculations under specific scenarios with a linear, instantaneous function of the forcings. This is only successful in situations where the forcing is basically linear since the thermal inertia of the climate system is ignored. From an emulation of the GCM temperature projections, the author derives a completely erroneous uncertainty due to cloud forcing biases and then purports to 'propagate' errors from the mean state energy flux into absolutely nonsensical (and self-evidently wrong) temperature projections of the future. As the previous reviewers (and others in response to the original paper) have noted, this has no scientific validity or worth. I see no way in which this paper could be publishable in JGR.

Overall comments:

The abstract is incomprehensible to anyone other than the author since multiple non-standard terms are used without definitions or context.

The confusion between errors in the mean state and errors associated with a perturbation are pervasive (equivalent to conflating the error in the constant and first derivative of a complex function). See below.

The authors claim that published projections do not include 'propagated errors' is fundamentally flawed. It is clearly the case that the model ensemble may have structural errors that bias the projections, but these are not derivable in the manner the author claims. Indeed, he has not demonstrated that any of the errors in the climatology that he highlights are even correlated to differing model outcomes, let alone dominant sources of projection spread.

The application of naive error propagation theory for a compound measurement to this case completely ignores the feedbacks and complexity of the models that render any assumption of statistical independence between errors null and void.

The demonstration of the fallaciousness of the reasoning demonstrated here is obvious upon assessing any other climate change simulated by the models. For instance, looking the last glacial maximum, the same models produce global mean changes of between 4 and 6 degrees colder than the pre-industrial. If the conclusions of this paper were correct, this spread (being so much smaller than the estimated errors of +/- 15 deg C) would be nothing short of miraculous. It isn't because they aren't.

Going more deeply into the methodology here, equation 6 is the key assumption, that temperature changes are the instantaneous response to the normalized forcing relative to some t0. However, by the logic so far, it is clear that this delta(T) does not include any feedbacks to radiative forcing (since both the '0.42'was calculated holding everything else constant). The F0 is in the right ballpark for the radiative forcing associated with complete removal of the main GHGs. The formula give 13.9 K for the pre-industrial climate (i.e. 1900 in this case), and so the difference in temperature from the pre-industrial is given by Tanom=13.9 * Fanom/F0 = 0.41 * Fanom. Note that this gives a climate sensitivity to 2xCO2 (Fanom=4W/m2) of ~1.6C, somewhat higher than the expected 1.2 deg C no-feedback response generally estimated. The match of this PWM to the GCM output is simply because the transient climate response (TCR) (which takes into account ocean thermal inertia etc.) is close to 1.6
C, but this is entirely fortuitous.

Once the author decides to fit his model to the individual models, he is simply calculating an empirical TCR and all of the foregoing justification is moot. Of course, this will only work with scenarios that have roughly linearly increasing forcings. Any stabilization or addition of large transients (such as volcanoes) will cause the mismatch between this emulator and the underlying GCM to be obvious.

The author than takes the uncertainty in the TOA energy fluxes (which he uses +/-4W/m2) and then assumes that this is also the uncertainty in the annual forcings. This is clearly nonsense, precisely for the reasons he earlier mentioned - i.e the cloud forcing errors are systematic - not random. Assuming that there is an additional random component in the forcings of this size produces error bars that are effectively a random walk and therefore will increase without bound over time. This neither matches what the models actually do, nor is it physically justifiable. (For instance, even after forcings have stabilized, this analysis would predict that the models will swing ever more wildly between snowball and runaway greenhouse states. Which, it should be obvious, does not actually happen). Given that the PWM is supposed to be an emulator of the GCM results, this a-physical result somewhat undermines its utility.

I will give one simple example of why this whole exercise is a waste of time. Take a simple energy balance model, solar in, long wave out, single layer atmosphere, albedo and greenhouse effect. i.e. sigma Ts^4 = S (1-a) /(1 -lambda/2) where lambda is the atmospheric emissivity, a is the albedo (0.7), S the incident solar flux (340 W/m2), sigma is the SB coefficient and Ts is the surface temperature (288K). The sensitivity of this model to an increase in lambda of 0.02 (which gives a 4 W/m2 forcing) is 1.19 deg C (assuming no feedbacks on lambda or a). The sensitivity of an erroneous model with an error in the albedo of 0.012 (which gives a 4 W/m2 SW TOA flux error) to exactly the same forcing is 1.18 deg C. This the difference that a systematic bias makes to the sensitivity is two orders of magnitude less than the effect of the perturbation. The author's equating of the response error to the bias error even in such a simple model is orders of magnitude wrong. It is exactly the
same with his GCM emulator.

The summary conclusion section is based wholly on the unsupported and erroneous results from the previous sections and does not need to be reviewed in depth.

Minor comments:

l42+l574 The 1988 GISS model was 'Model II' (Hansen et al, 1983), not Model E, a much later version.

l54. IPCC AR4 high end temperatures are twice this.

l109. The author is simply asserting that uncertainties in published estimates are not 'physically valid' - an opinion that is not widely shared.

l113. This is not true. Structural uncertainties (which go beyond individual parameter uncertainties) are very often assessed, most recently in the AR5 report. 'Systematic energy flux errors' are not a specific thing which can be propagated either.

l145. This is wrong. First, the terms are not properly defined, secondly, the role of clouds as important long-wave absorbers contributing the climatological greenhouse effect are ignored. And no, clouds are not the same as water vapor.

l154. This is fundamentally wrong. Calculating the magnitude of the greenhouse effect by removing a single absorber (in the case CO2) does not produce the effect of what remains due to the spectral overlaps. The paper by Schmidt et al (JGR, 2010) shows clearly that this will underestimate the effect of CO2 (i.e. their Table 1).

l156-279: this is all completely irrelevant for what is actually being done. Firstly, the whole calculation is contradictory to the earlier claim that this paper is purely concerned with GCM results and not the real world. Secondly, the answer would have been better derived directly from sensitivity studies from radiative/convective models themselves (ie. Ramanathan and Coakley, 1993).

Third, the answer is wrong because of the neglect of spectral overlaps (which are as present in the models as they are in the real world).

l253. There is no 'asymptote' at 1ppm CO2.

l261-269. All of the calculation here is just wrong. First, the attempt is being made to calculate the no-feedback contributions to the 33K GHE. The results of Hansen et al (1988) are applicable here and show that about 7K of the 33K is directly attributable to the single factor removal of CO2. Allowing for overlaps (as discussed in Schmidt et al (2010)), you would get a slightly larger value of ~10K. Note that percentage attribution of the temperature change is not the same as for the LW absorption (because of the non-linear dependence of LW on T^4).

l273. I am still unclear as to what the author is defining 'water-vapor enhanced' CO2 forcing to be. As far as I can tell it is the radiative forcing associated with the removal of CO2 holding all other factors constant, but the logic of the nomenclature escapes me.

l320-322: this is complete nonsense. The author is equating the attribution of the climatological GHE to the changes of GHE as a function of the feedbacks. This makes no sense whatsoever.

l373: that models have systematic biases is well known (marine stratus decks, double ITCZs, excessive southern ocean absorption). I have no idea what 'theory-bias' has to add to the already extensive literature on why these biases exist.

l388: why not just look at the spatial errors? of course they are non-random!

l512: this is the main paper error complete in one line. i.e. errors in the mean are not the same as errors in the perturbations.

l525: 'even'? What model in the history of the world has been perfect? This is a straw man argument.

l527-528: I fail to see how 'theory-bias' as a concept leads to a specific conclusion that models are partitioning energy incorrectly among climate modes.

l563 etc. All of this assumes that the errors always add and never interact. This is not valid for GCMs given the large negative feedback associated with the Planck response. This reduces the magnitude of error growth significantly.

l584-593. This is patently absurd. The scenarios A, B and C are clearly distinguishable and make clear predictions as a function of the different scenarios.

Fig 4: this is completely irrelevant. Of course a smooth function over latitude with perhaps 2 or 3 degrees of freedom will show latitudinal correlations.

I have very few detailed comments on this manuscript.

Too much of this paper consists of philosophical rants (e.g., accuracy vs. precision), several pages of basic radiative transfer theory to outline would should take only a few citations. The bulk of what the author presumably feels is novel here is completely wrong. In particular, the author has not actually shown that errors are propagating in future projections, but misunderstands the distinction between a base-state "forcing" and the uncertainties surrounding total cloud cover/forcing, from the uncertainties in climate change imbalances. The fact that GCMs do not have correct "absolute values" in variables such as TOA radiation balance, global mean temperature, cloud cover, etc is not novel.

There is no evidence provided by the author those known issues contaminate our understanding of attribution (which depends on the spatio-temporal evolution of patterns in stratospheric cooling, global OHC increases, etc) or in climate sensitivity (for example, the IPCC AR5 plotted absolute global mean temperature against the equilibrium climate sensitivity of the CMIP5 ensemble (Figure 9.42) and found no correlation between the absolute offsets in temperature and the sensitivity of the correlation between the absolute onsets in temperature and the sensitivity of the models). There is much further extensive discussion of the model performance and biases in that chapter, which I urge the author to read.

I have not seen all of the review comments to the previous manuscript, but I was provided with the author responses to those reviews, and was able to see several italicized portions of previous review comments. I think that previous reviewer #1, in particular, already diagnosed many of the problems in this current study. The responses provided by the author are not compelling.