2013JD020840 Patrick Frank

Response to Reviewer #1:

Summary:

- Propagated uncertainty is now the CMIP5 ±4 Wm^{-2} long wave cloud forcing error derived in [*Lauer and Hamilton*, 2013].
- The reviewer has overlooked the extensive validation of the linear model.
- The citations provided by the reviewer as examples of propagated GCM error do not in fact include propagated error.
- The reviewer's criticism is based upon a claim not known to be true, namely that GCM simulation error following equilibration to ~1850 conditions is a constant.
- Detailed responses are below.

Reviewer comments are quoted in italics, followed by the author response.

1. *The projected uncertainties from the study are several orders of magnitude larger than the CMIP5 spread arise from the unvalidated empirical model used in the study, as described below.*

The linear model (ms. eq. 6) is fully validated. Please see the response to comment 3 below.

2. The core of the study forms a linear model which directly relates global mean temperatures to net forcing, with no discussion of the effect of ocean heat uptake. The model thus effectively assumes that the earth system has no thermal inertia.

The linear model is not a climate model. It is a model of the numerical structure of the GASAT observables produced by GCMs, and nothing more. The analysis therefore makes no assumptions at all about the thermal inertia of the earth system. Ocean heat uptake is irrelevant to the study.

No claim is made upon climate physics. An empirical claim is made on the behavior of GCMs, specifically on the structure of their global air temperature output. This structure is proved to be just a linear extrapolation of GHG forcing.

The focus on the behavior of GCMs rather than on physics was clearly stated in the final paragraph of the Introduction:

> "An important distinction stated at the outset, and to be kept in view throughout what follows, is that the analysis herein concerns the behavior of the climate models themselves, and is not at all concerned with the physics of climate."

Unfortunately this paragraph apparently escaped the reviewer. The final paragraph of the Introduction has been strengthened in order to make this point more clearly.

In any case, however, the reviewer's criticism is not relevant.

3. *The validation of the model consists of plotting global mean temperatures calculated using the A2 scenario and idealized 1 percent annual increase simulations.*

The linear forcing model was validated by showing that it closely reproduces fifty-nine SRES air temperature projections of twenty-one CMIP3 climate models used for the IPCC AR4. This demonstration appears in the Auxiliary Material.

The validation was mentioned below Figure 2, on page 15 of the original manuscript.

Manuscript Figure 2 itself is meant to show that the linear eq. 6 itself produces completely credible projections across two standard modeling scenarios (original ms. Section 2.1.3).

Taken together, these demonstrations completely validate the linear model as an excellent representation of the GASAT realizations produced by advanced GCMs.

4. The fundamental error made in this study is the confusion of base-state forcing and feedback. The author attempts to measure uncertainty in present day cloud forcing by comparing ISCCP and MODIS derived cloud fractions with GCM output. There are numerous problems with this. Firstly, the cloud fraction diagnostics in GCM output are not directly comparable with the satellite products or each other, (GCM satellite simulators are available for both products, but the author has not used them). The author correctly ascertains that GCM derived cloud distributions have systematic errors, which is not unprecedented (Bony 2011 and references therein).

4.1 The analysis now uses the CMIP5 long wave cloud forcing (LCF) error, rmse = ±4 Wm^{-2}, recently reported by [*Lauer and Hamilton*, 2013]. This is the error in long wave energy flux at the TOA due to model errors in total cloud amount (CA).

LCF represents the upwardly radiant thermal spectrum, i.e., the flux of thermal energy present in the atmosphere. GHG forcing contributes to, and is part of, the atmospheric energy flux. Thus, cloud error in the LCF is most closely linked to the uncertainty of the atmospheric energy flux of which GHG forcing is a part.

Lauer and Hamilton also examined the CMIP5 short wave cloud forcing (SCF) error, assessing it to be rmse = ±8 Wm^{-2}. The total rmse uncertainty in atmospheric thermal flux due to CMIP5 cloud error is therefore ±9 Wm^{-2}.

4.2 [*Bony et al.*, 2011] nowhere mentions systematic error in GCM representations of cloud cover. Consulting the references therein, [*Dufresne and Bony*, 2008] assessed CMIP3 model cloud error but did not explicitly assess it as "systematic" or due to "theory-bias." They did not report on spatial autocorrelation of cloud error or any pair-wise inter-model correlation of cloud error. Nor did they assess the impact of systematic error on predictive reliability. The same is true of [*Bony and Dufresne*, 2005], who explicitly evaluated CMIP3 cloud error. Although the effect of model cloud simulation errors is described in the later paper of [*Vial et al.*, 2013], cloud error is not explicated as due to theory-bias, no inter-model correlation of error is reported, and the impact of cloud error on projection accuracy is not explored.

4.3 The issue of "*confusion of base-state forcing and feedback*" is addressed in item 4.1 and in 6.1 below.

Additionally, forcing and feedback both contribute to the total atmospheric energy flux. Systematic simulation errors in either one lead to an incorrectly simulated atmosphere, including air temperature. This was discussed in original manuscript lines 407-412, lines 429ff, and especially lines 491-533 and 602-632.

5. The author's first catastrophic error is to assume a linear relationship between global mean cloud fraction and net global cloud forcing - an assumption which is not justified in any way, but is clearly nonsense. A less wrong approach here would have been to use Partial Radiative Perturbation, Adjusted cloud radiative forcing or even just basic cloud radiative forcing as an estimate of the cloud radiative effect.

As noted under item 4, the ±4 Wm^{-2} CMIP5 LCF atmospheric thermal flux error as calculated by Lauer, et al., is now used to represent the total cloud fraction (TCF) error.

By way of explanation, however, a linear relationship between forcing and cloud type has literature precedent. [*Hartmann et al.*, 1992; *Ockert-Bell and Hartmann*, 1992] It seemed reasonable, therefore, that a linear fraction would provide a fair estimate of the flux error following from the error in TCF. Lauer and Hamilton, 2013, eq. (1), also expressed mean cloud fraction error as a linear average. [*Lauer and Hamilton*, 2013]

6. Item six divides naturally into three criticisms.

6.1. But, even ignoring the errors thus far and assuming the 5Wm^-2 uncertainty in cloud forcing is accurate, the overwhelming error in this paper is how this uncertainty in cloud forcing is applied in the future projections made using the empirical linear model. Each GCM starts simulations in ~1850 in an equilibrium state, thus all of the errors in base state cloud forcing are already represented in the global mean temperature in 1850.

The reviewer is here implying that whatever error is present in the model realization of the 1850 base climate remains constant across all the climate realizations of the subsequent model years.

That is, the reviewer is assuming that following equilibration, GCM projection error is a constant offset that can be removed by taking anomalies. This assumption is grounded in one of two alternative subtending assumptions that the reviewer has left implicit. I.e., following forcing a perturbation of climate caused by a trend in atmospheric GHGs:

1. All parts of the physical climate system remain constant over the projection time-range, except air temperature. The modeled climate is likewise held constant, except air temperature. The constant parts of the modeled climate and their constant error can be subtracted away, leaving a valid projection of air temperature. The reviewer's position then follows.

2. The physical climate system is viewed as responding dynamically to the perturbation, and GCMs include these dynamics. Following 1850, the modeled climate perfectly reproduces the dynamics of the perturbed physical climate, such that the modeled climate maintains the original "base state cloud forcing" error as a constant. That is, apart from the original 1850 base year offset error, GCMs track subsequent climate dynamics perfectly. Again, the reviewer's position follows.

The reviewer must hold one of these two statements to be true, otherwise there is no critical point in suggesting that the equilibrated 1850 base year already manifests "all of the errors in base-state cloud forcing."

However, implicit assumption no. 1 is physically unjustifiable because climate subsystems are oscillatory and intimately coupled; nothing stays constant. The second assumption, that GCMs can perfectly track climate dynamics, is known to be untrue. Therefore, neither statement is correct and reviewer criticism 6.1 can be set aside.

The irony is not lost that constant GCM error is far more radical assumption than is a linear relation between mean TCF and mean cloud forcing.

> *6.2 The author is implying that the uncertainty in cloud feedback (the derivative of cloud forcing with respect to global mean temperature) can be equated to the multi-model spread in present day cloud forcing without any justification.*

Nowhere in the manuscript is it implied that uncertainty in cloud feedback can be equated to multi-model spread. The manuscript concerns the spread between models and <u>observations</u>; i.e., model accuracy. Multi-model spread itself is a measure of model precision, not a measure of model accuracy. The reviewer has apparently missed this core distinction, which is explicitly made in paragraphs two, three, and eight of the Introduction.

Model physical accuracy is estimated as the difference between modeled TCF and observed TCF. In other words, physical TCF error (inaccuracy) is $(\mathrm{TCF})_{\mathrm{model}}$ minus $(\mathrm{TCF})_{\mathrm{obs'd}}$, not $(\mathrm{TCF})_{\mathrm{modelA}}$ minus $(\mathrm{TCF})_{\mathrm{modelB}}$.

The physical TCF inaccuracy of CMIP5 models translates into an inaccuracy in the modeled thermal energy flux bath of the atmosphere. This is because TCF affects the magnitude of radiant absorption, emission, and scattering (albedo) that in turn impact the magnitude of atmospheric flux of thermal energy.

Due to TCF error, the atmospheric thermal flux is continuously modeled incorrectly at every step of a projection. Therefore, the projected air temperature will necessarily be always incorrect.

This situation was described in detail in original manuscript page 28, paragraph 3 beginning line 602, and page 31, paragraph 2, beginning line 668. Unfortunately, the reviewer has not addressed this point.

> *6.3 Of course, the spread of cloud feedbacks in present day GCMs is our dominant source of uncertainty in future global mean climate response, but it is critically the changes in forcing in each model as a function of temperature, and not the absolute values, which are of relevance for climate sensitivity to greenhouse gas forcing.*

6.3.1 The reviewer here again assumes that systematic error invariably produces a constant offset. This point was addressed in item 6.1, above. Differencing does not remove systematic errors, as also discussed in detail on original manuscript page 23, beginning line 491.

The spread in modeled cloud forcing indicates that cloud physics is not accurately known. Inaccurate cloud forcing means that modeled climate is projected inaccurately. This point is addressed in item 6.2 above and in original manuscript pages 28 and 31.

To bring this point to greater visibility, the discussion of anomalies is given its own section, "*2.4.3. Differencing does not remove systematic error*" in the revised manuscript.

6.3.2 To expand on anomalies and error, although not discussed in the original manuscript, standard error analysis shows that taking an anomaly increases systematic uncertainty. For an anomaly $w = (u\text{-}v)$, where the systematic uncertainties in u, v are $\pm du$, $\pm dv$, the uncertainty variance in w is $(dw)^2 = (du)^2+(dv)^2$. [*Bevington and Robinson*, 2003] The uncertainty in the anomaly, $\pm dw$, is necessarily larger than $\pm du$ or $\pm dv$.

The only time this result does not apply is when the magnitude of the error is known, known to be constant, and can be subtracted away. However, none of those conditions are satisfied for the relation between GCMs and systematic cloud forcing error. Therefore taking dT and dF relative to their means, as advised by the reviewer, will cause the uncertainty to increase.

The point about the increased uncertainty of anomalies now appears in the revised manuscript, new section 2.4.3.

6.3.3 Finally, the author thanks the reviewer for bringing up the structure of error, and the accounting of it, because it is the core of the analysis. Generally, GCM cloud fraction error, and thus cloud forcing error, is shown to reflect theory-bias. This means that the 1850 equilibrated state mentioned by the reviewer is an incorrect representation of the

1850 climate energy state. Therefore the initial conditions entering the subsequent projected state (1851) will be wrong.

6.3.4 Further, theory-bias also means that even perfect initial conditions will be incorrectly projected. Initial state errors plus theory bias errors mean that every subsequent projected climate state will also be an incorrect representation of its energy state. Under conditions of theory-bias, initial state errors never equilibrate away, because they propagate into the new errors generated by a biased theory in every step of a sequential projection of states.

The reviewer apparently overlooked section 2.4.2, page 23 of the original manuscript, where this point was discussed in detail.

> *7. An entirely equivalent argument would be to say (accurately) that there is a 2K range of pre-industrial absolute temperatures in GCMs, and therefore the global mean temperature is liable to jump 2K at any time - which is clearly nonsense, but no less so than the arguments presented in this paper.*

The reviewer has here mistakenly supposed that an uncertainty statistic, i.e., ±1.36 K with 0.997 range = 2 K, is a thermodynamic quantity.

To clarify using the reviewer's example, a correct representation of the approach taken in the manuscript would be to observe that a pre-industrial absolute air temperature range of 2K among GCMs is approximately equivalent to (±1.36K/3K)×3.7 Wm^{-2} = ±1.7 Wm^{-2} average uncertainty in the basic thermal energy flux of the atmosphere as modeled by these GCMs. This uncertainty follows from the average climate sensitivity of 3 K increase in air temperature per 3.7 Wm^{-2} of forcing, and would have to be propagated forward through any air temperature projection made using these GCMs.

> *8. For these reasons, I consider the study to be incorrect and unpublishable. There are however, several other minor issues, for reference.*

At this point the reviewer's criticisms have either evidenced a misunderstanding of the substance of the manuscript, or else a misunderstanding of the meaning of systematic error.

> *9. The author consistently ignores all literature in feedback theory. Water vapor feedback is well understood and remarkably consistent between present day GCMs (see Soden et al (2011), Colman (2002) etc.), and the spread in water vapor effect referred to in the model is due to the realization of other feedbacks on temperature which then influences water vapor forcing.*

9.1 It is presumed "*Soden et al (2011)* (not specified among the reviewer-provided citations)" refers to [*Soden and Vecchi*, 2011]. That paper compared water vapor feedback among models, with no reference to observations. A basic of experimental science is that consistency among models conveys no information about their physical accuracy. Accuracy is determined by a consistency between models and observations.

For example, Soden and Vecchi report consistency among CMIP3 models in high cloud forcing, while [*Zhang et al.*, 2005] reported significant inconsistency between observed and CMIP3 modeled high clouds.

It is hard to reconcile a significant disparity between observed and modeled with a claim of "well understood," and difficult to understand a confusion of inter-model consistency with physical accuracy.

9.2 Table 1 in [*Colman*, 2003] reported an uncertainty range of 1.2 Wm^{-2} K^{-1} for modeled water vapor feedback, exceeded only by uncertainties in cloud and lapse rate feedback.

9.3 "*the spread in water vapor effect referred to in the model*" It is assumed the reviewer refers here to the model-dependent variation in f'_{cd} (Auxiliary Material Tables S1-S3). All the models projected identical SRES forcings. Well-understood feedbacks should have approximately the same magnitude among models within each scenario. The derived magnitudes of their water vapor forcing fraction should then also have been similar.

> *10. The 3 pages of fundamental radiative transfer in the paper dedicated to deriving the log relationship between CO2 concentrations and climate forcing is completely unnecessary, and adds nothing to existing textbook literature.*

10.1 The calculations of radiative mean free path (*1/e* attenuation length) are meant to estimate the $[CO_2]_{atm}$ necessary to the onset of climatologically significant forcing.

Following the reviewer's comment, a search of texts was carried out to determine whether the manuscript analysis duplicated an early pedagogical element of the field. [*Aherns*, 2009; *Andrews*, 2010; *Bohren and Clothiaux*, 2006; *Houghton*, 1985; *Jacobson*, 2005; *Nazaroff and Alvarez-Cohen*, 2001; *Seinfeld and Pandis*, 2006; *Taylor*, 2005; *Wallace and Hobbs*, 2006] These texts cover radiative physics at several levels and several appear quite comprehensive. Among these, only two address radiation in the suggested manner. Wallace and Hobbs mention a *1/e* optical depth (p. 135), but do not identify it as the radiative mean free path.

Chapter 2 in Bohren and Clothiaux includes a very nice discussion of the *1/e* attenuation length (p. 53), where it is called the *e*-folding length. However, their analysis is not extended to discuss the radiative onset of CO_2 forcing.

The other examined texts did not mention a radiative mean free path at all. No text mentioned or discussed the atmospheric concentration of CO_2 necessary to initiate climatologically significant forcing.

Therefore, it appears that the radiative analysis in the manuscript pertaining to the onset of significant CO_2 forcing, while straight-forward, is not a common textbook subject.

10.2 The manuscript does not derive the log-linear relationship of CO_2 forcing. It points to the literature derivation, and discusses the lower limit of $[CO_2]_{atm}$ at which the relationship emerges.

Knowledge of both the atmospheric concentration of CO_2 necessary for the onset of significant forcing (>1 ppmv) and of the $[CO_2]_{atm}$ at which the log-linear relationship emerges (1 ppmv < $[CO_2]_{atm}$ <2 ppmv), is a necessary pre-requisite that justifies the extrapolation shown in Figure 1b, the CO_2-temperature relation of Manabe and Wetherald. [*Manabe and Wetherald*, 1967]

Therefore, it is respectively suggested that this analysis should remain part of the study.

> *11. The literature review is incomplete and misrepresentative. The IPCC is portrayed as making conclusions based on model results alone, and does not reflect multiple lines of observable historical data which can constrain large scale climate parameters.*

In science, the meaning given to the results of observation and experiment is strictly dependent on the existence of a falsifiable theory. Absent such a theory, observations may have several meanings, or even contradictory meanings that vary with the personal biases and outlooks of the observers.

Therefore it is necessarily true that observations used in predicting the future direction of climate must derive their meaning from a falsifiable theory of climate. That theory is represented by climate models.

> *12. The repeated statement that Knutti (2008) considers only model variability is simply incorrect, as the paper discusses a wide range of systematic and parametric uncertainties in GCM projections [line 61 and others].*

12. Manuscript lines 60-61: "Published projections of global averaged surface air temperatures typically present uncertainties as model variability relative to a mean, [*R. Knutti et al.*, 2008, ...]"

As the reviewer states, [*Knutti et al.*, 2008] discuss a variety of parametric uncertainties. However, all four Figures in that paper present the uncertainties as relative to a modeled mean.

From Knutti, et al., 2008:

- Figure 1b: "shaded bands denote one standard deviation of the multimodel response."
- Figure 2: "Central values and ranges [of the cumulative distribution functions] are given as means and 5%–95% from a fitted normal distribution to the AR4

AOGCM simulations..."

- Figure 3: "Relative uncertainty of global-mean surface air temperature increase ... defined here as half of the 5%–95% range, divided by the median of the PDF [or] as 1.65 times the standard deviation across the models divided by the mean."
- Figure 4: "Uncertainty in global-mean surface temperature increase for the end of the century ... for the SRES A2 scenario. ... Means and ... ranges for the MAGICC [model] ... the Bern2.5CC models ... the C4MIP models [and] the Bern EMIC [model]..."

These quotes verify the message of lines 60-61. As noted in the manuscript, therefore, the uncertainties of Knutti, et al. 2008 are measures of model precision, not of model accuracy. This distinction is at the heart of the present study.

> *13. Stainforth et al (2005) has nothing to do with internal variability [line 62], and discusses the relationship between parameter uncertainty and climate sensitivity.*

13 Stainforth, et al., 2005 includes three Figures; all present uncertainties relative to a model mean:

- Figure 1: "Frequency distributions of [global temperature] through the three phases of [model] simulation."
- Figure 2a,2b,2c: "The [modeled] response to parameter perturbations." All the uncertainty bars in Figures 2a-2c reflect model variability.
- Finally, from page 405 of Stainforth: "Figure 3 shows the initial-condition ensemble-mean of the temperature and precipitation changes for years 8–15 after doubling CO_2 concentrations, for three model versions..."

These quotes again verify the point made in the manuscript, namely that uncertainties are represented as variations around model means – precision – rather than as disparities from observations – accuracy.

> 14. *The repeated statement that no prior papers have discussed propagated error in GCM projections is simply wrong (Rogelj (2013), Murphy (2007), Rowlands (2012)).*

14.1 Rogelj, et al., concerns economic costs of mitigation. Only Figure 1b includes a global temperature projection plus uncertainty ranges. Figure 1b Legend: "b, Probabilistic temperature projections for the blue trajectory in [Figure 1]a."

The air temperature projection in Figure 1b shows shaded confidence intervals of 50, 66, 90, and 98% probability. Concerning these intervals, the text (p. 79) says,

> "Temperature projections for any given pathway have a spread owing to geophysical uncertainties[18] (Fig. 1b)."

Rogelj, et al., reference 18 is to [*Knutti et al.*, 2008]. Knutti, et al., 2008 , as discussed in item 12 above, provide projection uncertainties as model variations about a mean. Therefore the uncertainty intervals of Rogelj, et al., Figure 1b do not represent propagated error.

From the "Methods Summary" of Rogelj, et al.:

> "We then compute probabilistic estimates of global temperature increase for each scenario with the MAGICC climate model[16,17,30]. These estimates are based on a 600-member ensemble of temperature projections for each scenario, which together closely represent the carbon-cycle and climate uncertainties as assessed in the Fourth Assessment Report of the Intergovernmental Panel on Climate Change[17]."

Rogelj, et al., reference 17 is, "Rogelj, J., Meinshausen, M. & Knutti, R. *Global warming under old and new scenarios using IPCC climate sensitivity range estimates*. Nature Clim. Change 2, 248–253 (2012)."

Therein, the uncertainties presented in three Figures and three Tables are all variations around model projection means, which model means in turn chiefly reflect the parameter uncertainties given in the IPCC 4AR.

In neither paper by Rogelj, et al., therefore, are physical uncertainties propagated step-wise through a projection. Instead, projections are run under various constraints, means are taken, and the model variances about the means are computed. These variances all reflect model precision.

14.2 [*Murphy et al.*, 2007] is particularly instructive. From paragraph 2 of the Introduction:

> "Here we focus on the quantification of modelling errors and internal variability for an assumed pathway of future anthropogenic emissions. The effects of internal variability can be estimated by varying the initial conditions in multiple simulations of a single climate model. In order to sample the effects of model error, it is necessary to construct ensembles which sample plausible alternative representations of earth system processes."

Both of the described assessments – of model errors and of internal variability -- involve the departure of model ensembles from their modeled mean. Once again, the evaluations concern precision. Nowhere do Murphy, et al., mention or carry out an accuracy assessment by step-wise propagation of error through a projection.

Systematic error is mentioned on page 1996 of Murphy, et al.:

> "... some [HadCM3] variables, such as cloud types in the middle troposphere, show large systematic biases which cannot be resolved by varying its uncertain parameters. The PDFs obtained to date from PPE [perturbed physics ensemble] experiments would be wider, were structural model errors to be accounted for. The methodology of §3 therefore includes plans to inflate PDFs obtained from

> PPE experiments to account for the estimated impact of structural modelling errors. For this purpose, we adopt a specific definition of structural error as the difference between simulated and observed climate which remains once that error has been minimized...”

Despite recognizing the systematic error due to theory bias (“structural error”), the approach to model skill developed by Murphy, et al. (article Figure 1), neither mentions nor includes step-wise propagation of error through a calculated projection.

14.3 In their Introduction, Rowlands, et al., 2012, state:

> “Here we present results from a multi-thousand-member perturbed-physics ensemble of transient coupled atmosphere–ocean general circulation model simulations. “ and go on to state that, “Perturbed-physics ensembles offer a systematic approach to quantify uncertainty in models of the climate system response to external forcing, albeit within a given model structure.”

Rowlands, et al., go on to exclude models that produced hindcasts departing too radically from the known 1961-2010 temperature trend. For the remaining models, projection uncertainty is then (Figure 1) the,

> “goodness-of-fit between observations and ensemble members, plotted in order of increasing agreement.”

That is, uncertainty is the variation of the remaining projections about the 2001-2010 observed trend. Again, therefore, projection uncertainty reflects model variation, and is not a step-wise propagated error.

Rowlands et al., also mention that they, “retain only model versions requiring a global annual mean flux adjustment in the range ±5Wm^{-2}, comparable to estimates of observational uncertainty in top-of-atmosphere fluxes.”

In short, none of the three studies offered by the reviewer as examples of published propagated error, in fact provide a propagated error. All three instead provide an uncertainty based strictly upon model variation.

15. Reviewer-provided citations.

Rowlands, Daniel J., et al. "Broad range of 2050 warming from an observationally constrained large climate model ensemble." Nature geoscience 5.4 (2012): 256-260.

Murphy, James M., et al. "A methodology for probabilistic predictions of regional climate change from perturbed physics ensembles." Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences 365.1857 (2007): 1993-2028.

Rogelj, J., D. L. McCollum, A. Reisinger, M. Meinshausen and K. Riahi (2013). "Probabilistic cost estimates for climate change mitigation." Nature 493(7430): 79-83. (HTML)

Colman, Robert. "A comparison of climate feedbacks in general circulation models." Climate Dynamics 20.7-8 (2003): 865-873.

Soden, Brian J., Isaac M. Held, Robert Colman, Karen M. Shell, Jeffrey T. Kiehl, Christine A. Shields, 2008: Quantifying Climate Feedbacks Using Radiative Kernels. J. Climate, 21, 3504-3520.

Bony, Sandrine, et al. "CFMIP: Towards a better evaluation and understanding of clouds and cloud feedbacks in CMIP5 models." Clivar Exchanges 16.56 (2011): 20-24.

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Aherns, D. C. (2009), *Meteorology Today*, 9th ed., Brooks/Cole, Belmont.

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Bevington, P. R., and D. K. Robinson (2003), *Data Reduction and Error Analysis for the Physical Sciences*, 3rd ed., 320 pp., McGraw-Hill, Boston.

Bohren, C. F., and E. E. Clothiaux (2006), *Fundamentals of Atmospheric Radiation*, 472 pp. pp., Wiley-VCH, Weinheim, doi:DOI: 10.1002/9783527618620.

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Jacobson, M. Z. (2005), *Fundamentals of Atmospheric Modeling*, 656 pp. pp., Cambridge University, Cambridge, UK.

Knutti, R., et al. (2008), A Review of Uncertainties in Global Temperature Projections over the Twenty-First Century, *Journal of Climate*, *21*(11), 2651-2663, doi:10.1175/2007jcli2119.1.

Lauer, A., and K. Hamilton (2013), Simulating Clouds with Global Climate Models: A Comparison of CMIP5 Results with CMIP3 and Satellite Data, *Journal of Climate*, *26*(11), 3823-3845, doi:10.1175/jcli-d-12-00451.1.

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