Response to Anonymous Referee #1

We thank Anonymous Referee #1 for his/her thoughtful and insightful comments on our manuscript. We have responded to the comments below (in red).

General comments:

The paper "The University of Victoria Cloud Feedback Emulator (UVic-CFE): cloud radiative feedbacks in an intermediate complexity model" describes and evaluates a new method for applying GCM-derived cloud feedbacks to intermediate complexity models. The new method is able to capture the spread in TOA radiative feedbacks between the original GCMs, implying that the tool is generally efficient. Given that cloud feedback plays an essential role in determine the magnitude of global warming, this method is expected to be useful in improving intermediate complexity models that are in lack of cloud feedbacks. Therefore, I suggest the paper be published after addressing my following comments.

1. The cloud masking effect has not been removed from cloud radiative effect when cloud feedback is calculated. This may result in a systematic bias to the TOA radiations. Consider an assumptive situation that there is no change in cloud properties under global warming, then no cloud terms need to be added up to Eq. (6), and the OLW_cloud(t) term in Eq. (15) should be zero. However, the cloud longwave radiative effect in GCM would still change due to changes in water vapor and temperature (cloud masking effect), leading to a non-zero value in Eq. (15). Therefore, an additional term is needed to compensate the cloud masking effect (this may be done with radiative kernels, Soden et al. 2008, doi: 10.1175/2007JCLI2110.1).

Because the cloud masking effect is evident in the 4xCO2 GCM simulations, we choose to include it in our OLW cloud feedback. The OLW parameterization of UVic (Eq. 6) lacks the full cloud dynamics of the GCMs, so by assessing our OLW cloud feedback with the full cloud radiative effect (CRE) anomalies, we aim to incorporate the full OLW cloud changes of the GCMs, including cloud-masking effects. That said, the Referee is correct in pointing out the differences in the CRE and kernel methods, so we make note of potential issues as suggested by the reviewer (section 2.3.2).

2. The calculation of "TOA feedbacks" is inaccurate, so I suggest the authors to either calculate the TOA feedbacks with the standard method, or to replace "TOA feedbacks" with another phrase. In this paper, climate forcing is included in the "TOA feedbacks" (Page 9, line 6), so the TOA feedbacks in Fig. 6 (_0W/m2/K) is much larger than that calculated by Andrews et al. 2012 (-1.08 W/m2/K).

Since the exact greenhouse gas forcing is unknown for each of the GCMs, we favor our current method of calculating TOA "feedbacks". Therefore, we have replaced the phrase "TOA feedbacks" with "TOA radiative-temperature response" so as not to confuse readers with the standard method.

3. The cloud rapid adjustment has not been removed from the cloud feedback (Zelinka et al. 2013, doi:10.1175/JCLI-D-12-00555.1). This may be partially responsible to the loss of spread in UVic-CFE simulations (Fig. 6).I expect that the UVic-CFE would be more accurate after the above comments are addressed.

Yes, by calculating our cloud radiative effect using CRE anomalies instead of the 'Gregory' slope method, we are neglecting influence of cloud rapid adjustment. However, we are unable to include them as part of the forcing (as suggested by Zelinka et al., 2013), so we must include them in the cloud feedback. We make note of this issue in the revised text (discussion) and its possible influence on the spread in UVic-CFE.

Specific comments:

Page 1, Line 29. "The relative magnitude and net effect of these feedbacks depends on cloud altitude. . . High clouds, on the other hand, radiate at much colder temperatures than the surface, which can make the longwave effect dominate and lead to net warming"

The authors may also discuss the effect of cloud optical depth here. The net cloud radiative effect of high clouds could be either positive (high thin clouds) or negative (high thick clouds) depending on the cloud optical depth.

Yes, the referee is correct in noting the importance of optical depth. We now include a statement on cloud optical depth and its relationship with cloud radiative effect.

Page 3, Line 30. It is worth discussing whether the cloud radiative effect is included when the empirical parameters in Eq. (6) were calculated.

The original parameterization of Thompson and Warren (1982) is for clear-sky outgoing LW fluxes. Therefore, there is no cloud feedback within this parameterization. We now make note of this in our description of Eq. (6).

Page 7, Line 17. Please provide more details for the 4xCO_2 and LGM experiment design.

Our experimental design is described in the second paragraph of section 2.4. Is there a particular aspect of the experimental design that you would like to see further described?

Figure 4 and 5. There are some white pixels surrounded by blue/green pixels (for example, Central tropical Pacific Ocean in Fig. 4a,d). Are these white pixels induced by missing values? Is it possible to eliminate them?

These white pixels appear to be due to an improper image rendering of the original postscript image. We have corrected these images by first saving the file to a jpeg.

Technical comments
Page 3, Line 25. How is the variable f calculated?

The variable f is as an adjustment factor that is necessary to correct for a radiative imbalance that arises in our estimate of atmospheric albedo from the CERES observational data. Therefore, this adjustment factor directly affects global mean surface air temperature. We ran a series of control simulations with different f-values to tune our control simulation to observational estimates of ~13.9 C (f=0.95). We have rephrased our description in section 2.2 to address this confusion.

Figure 6, 8. Figure legend: “UVIC control” -> “UVic control”, to be consistent with the figure description.

Thank you for this catch. We have fixed this in our updated manuscript.