Reply to Reviewer #2

We would like to thank the reviewer for the supportive comments and helpful suggestions to improve our manuscript. In our new version of the manuscript we try to follow virtually all of the reviewer's suggestions. Below we address the reviewer's comments in detail.

Comments from the Reviewer

- there is absolutely no guarantee that these empirical relationships can be applied to climate change. In my opinion, this point clearly deserves some discussion.

We added a new section and discuss this issue briefly as follows:

4.1.4 Brief discussion on climate change simulations

It is well-known that changes in LCC in warmer climates cannot be explained by changes in LTS (e.g., Williams et al., 2006; Medeiros et al., 2008; Lauer et al., 2010). The mechanism of this discrepancy is also well-understood; inevitable decrease of moist adiabatic lapse rate in the free atmosphere in warmer climates causes increase in LTS (e.g., Miller, 1997; Larson et al., 1999), even though the inversion strength that probably contributes to determine LCC does not change (e.g., Wood and Bretherton, 2006; Caldwell and Bretherton, 2009). It was expected that an index EIS could avoid this problem and could be used for discussion of LCC changes under warmer climates because EIS is a more physics-based index that represents inversion strength at the cloud top more directly. However, more recently, it turned out that LCC tends to decrease, although EIS increases in warmer climates in most climate models (e.g., Webb et al., 2013). Subsequently, it was shown by Qu et al. (2014) that changes (including variations in the present climate and future changes) in LCC can be determined by a linear combination of changes in EIS (positive correlation) and SST (negative correlation). Kawai et al. (2017) derived the linear combination from the index ECTEI and showed that a decrease in LCC under increased EIS in warmer climates can be explained based on the ECTEI change (see Kawai et al. (2017) for more detail). It is true that a usage of empirical relationships obtained in the present climate for climate change simulations has a possibility of causing spurious climate feedback. On the other hand, we would like to note that ECTEI is even more physics-based index than EIS, the relationship is not used directly for cloud formation but used as a threshold for cloud top mixing, and ECTEI can explain positive low cloud feedback, although the risk of spurious climate feedback still cannot be eliminated.

- 1. The partitioning of detrained cloud water into liquid and ice in MRI-ESM2 follows Hu et al. (2010). The same data by Hu et al. (2010) was also used to evaluate MRI-ESM2. It is found that the mass and the frequency ratio in Fig. 4 are both fairly close to the Hu et al. data. Thus overall, the WBF process appears to have a rather small influence (which might be partially explained by still using a mixing ratio threshold). Could you please briefly comment on this? Is the WBF process allowed to act on detrained cloud water in the same time step?

We understand that this could be a little confusing part. First, source terms (to which Hu et al. (2010) function is adopted) are not only detrainment from convection, but also formation of stratiform clouds due to upward motion and temperature decrease. Actually, in MRI-ESM2 (with the new parameterization), production of ice from these source terms are dominant, and contributions from depositional growth (and others including immersion freezing, condensation freezing, and contact freezing) are much smaller. Therefore, the ratio in Fig. 4 is close to the Hu et al. function that is used to divide source terms into ice and liquid. This means that WBF process that explicitly occurs in MRI-ESM2 is very weak. (However, a usage of Hu et al. (2010) function to determine the ratio of newly produced LWC promotes ice production in the case of IWC greater than a threshold in our treatment, and it means WBF process is parameterized in MRI-ESM2.) The following short sentence was simply inserted in the third paragraph in Section 3.2: “In MRI-ESM2, IWC production from the source terms of LWC based on partitioning using a function of Hu et al. (2010) is dominant, and the contributions from a depositional growth and other freezing processes are considerably small.”

- 2. Hu et al. (2010) present an empirical relationship that arises as a the consequence of a large number of physical processes. While the underlying physics remain the same, there is absolutely no guarantee that such a relationship will hold in a future climate. Updrafts and/or aerosol may influence such a relationship and they may change in a

systematic way.

The point is true. We added the following sentence in the last paragraph in Section 3.2: "It should also be noted that empirical relationships including the ratio curve of Hu et al. (2010) may not hold completely in a future climate because a large number of meteorological factors contribute to form such relationships and they may change in a systematic way."

- 3. Many climate models show a decrease of cloud cover at the same time as LTS increases in a future climate. In a regional model, Lauer et. al 2010 (https://www.doi.org/10.1175/2010JCLI3666.1) also find that changes associated with global warming do not follow empirical relationships between LTS and cloud cover. Therefore, such empirical relationships should not be used in climate models.

The response to this comment is included in the response to the reviewer's major comment.

- 4. I know that this would mean breaking a tradition, but I would be very interested in seeing a plot that shows the effect of the bug fixes mentioned in 3.7.

It is not easy to take back all the bugs and to convince our colleagues to spend computer resources to investigate influences of the bugs. Instead, the impact of the bug related to number concentrations of the cloud particles that is mentioned in Section 3.7 is shown in Fig. R1 (attached to this reply) for the reviewer (and the readers of this open discussion). The figure shows that the bug caused excessive reflection of solar radiation, particularly for stratocumulus and stratus over the subtropics and northern Pacific region for July.

- 5. Do the the changes due to the individual model modifications add up linearly to yield the final result? In Fig. 11, a plot could be added that compares the sum of the individual contributions to the difference between MRI-ESM2 and CGCM3.

We agree that it is an interesting issue. Actually, the sum of impacts from the individual model modifications described in this manuscript (in Fig. 11b) does not match the difference between MRI-ESM2 and CGCM3 (in Fig. 11c) so well. There are several reasons for that. First, several bug fixes related to clouds significantly (as describes in the response above) contribute to the radiation flux bias. Second, modifications in some other physical processes, for example, convection parameterization contribute the radiation flux as well. Modifications in other component models, for example, the aerosol model (through changes in optical thickness of clear sky and optical thickness of clouds via changes in aerosol concentrations) contribute the radiation bias. In addition, there should be non-linearly on each impact from each modification. Therefore, we decided not to discuss this summation to avoid confusion and complexity.

- 6. Fig. 10 shows a strongly decreased time step dependency of the ice water content in MRI-ESM2 compared to MRI-CGCM3 which the authors attribute to the I2S conversion term. This seems very plausible to me. However, if I understand it right, in MRI-ESM2, the partitioning of IWC into IWC>100 and IWC<100 is still performed at every time step (according to Eq. S10), right? As far as I can see, in the absence of a threshold below which conversion from cloud ice to snow starts to be active, this could in principle still cause a problem similar to the one described in lines line 423f. Nevertheless, at least the time step dependence seems to be addressed. Without first looking at Fig. 10, I would have expected that in the case of a long-lived non-precipitating cloud the ice water path would still become depleted during a successive time steps because of this partitioning. And, on the other hand, does partitioning every time step mean that a part of the IWP will always be be particles smaller than 100 micrometer (IWC<100)? Again, all of this seems less of a problem than what has already been addressed here.

The situation is a little complicated.

First, it is important to confirm that the ratio of Eq. (S10) is not used to actually (directly) determine the ratio of large ice and small ice in total IWC. Therefore, large ice production during one time step (= $C_{I2S}\Delta t$) (not $(1-\alpha_i)\, q_i$) is basically proportional to Δt. (Note also that a ratio r_{iw} is in Eq. (2) but a ratio α_i is not in Eq. (6). If, for example, the ratio α_i is used as a substitute for r_{iw} in Eq. (2), similar timestep dependency is found.)

It is true that the ratio α_i is calculated from Eq. (S10) every time step. But it is used only to determine the conversion rate of small ice to large ice C_{I2S} (Eq. (5)). To determine the conversion rate, the ratio α_i calculated from Eq. (S10) based on observation (at the observation depth from the top of the cloud) should be used and the ratio calculated in the model that depends on model time step should not be used. For clarification, the following sentence was inserted after Eq. (6): "Note that although the ratio α_i obtained from Eq. (S10) is used to calculate the conversion rate C_{I2S}, it is not used to directly

determine the ratio between small ice crystals and snow differently from in Eq. (2)."
It is true that in the case of a long-lived non-precipitating cloud the ice water path would still become depleted if we don't introduce the conversion threshold. But it would occur regardless of the model time step.

- p. 2, l. 46: ratio of supercooled water -> ratio of supercooled water to cloud (liquid and ice) water

Corrected.

- p. 9, l. 281f: I completely agree on this (https://doi.org/10.5194/gmd-2018-307-RC1).

Thank you for your agreement. We are pleased to know that the reviewer recognizes the value of non-use of a lower limit of cloud droplet number concentration.

- The advection term in Eqs. 2 and 6 is in flux form and not in advection form. In order to convert an advection equation to flux form, it is necessary to use the continuity equation. This implies that the flux form equation is valid in cases in which the tracer (here: ice) is advected with the flow. The flux from equation is not valid for sedimentation.

The reviewer's comment is correct. The equations in the previous manuscript were not correct. (We wanted to make the equations easy to understand, though they were forms not actually used. But it is true that it is confusing.) The equations that we actually use are as follows:

$$\frac{\partial q_i}{\partial t} = C_g + \frac{R_i}{\rho_a \Delta z} - \frac{v_{\text{ice}}}{\Delta z} r_{iw} q_i - \frac{(1-r_{iw}) q_i}{\Delta t} \quad (2)$$

$$\frac{\partial q_i}{\partial t} = C_g + \frac{R_i}{\rho_a \Delta z} - \frac{v_i}{\Delta z} q_i - D_{\text{I2S}} q_i \quad (6)$$

where R_i (kg m^{-2} s^{-1}) is the ice sedimentation flux into the layer from above, and Δz (m) is the layer thickness. (This form is generally used for ice sedimentation calculation (e.g., Smith, 1990; Rotstayn, 1997).)
Therefore, we corrected these equations as noted above. Thank you for your insightful comment.

- p. 13, l. 396f: Morrison and Gettelman, 2007 (https://www.doi.org/10.1175/2008JCLI2105.1) use substepping.

Thank you for the information. The following sentence was inserted:
"Although adopting shorter time steps for selected processes that is called substepping (e.g., Morrison and Gettelman, 2008) would be an ideal solution, it can increase computational cost to some degree."

- p. 15, l. 445f: this is not unavoidable. see my comment regarding l. 396f.

The sentence was modified as follows:
"Therefore, the sedimentation cannot be calculated appropriately with the time step used in our climate models, and the treatment of instant fall of snow (large ice) through to the surface is unavoidable, unless substepping is introduced."

- Fig. 4, caption: should the Hu et al. data be compared to the mass or the frequency weighted ratio?

The part in the caption was clarified as follows:
"An observational curve from Hu et al. (2010) that corresponds to a frequency ratio"

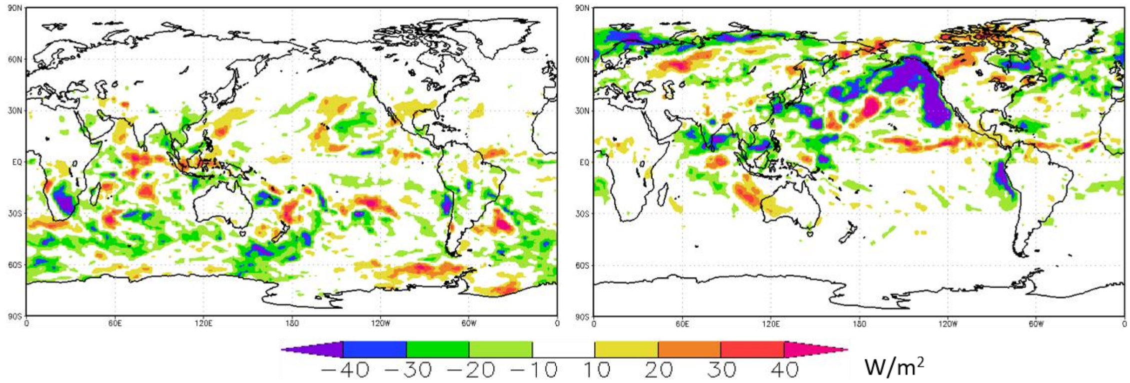

Figure R1. Impacts of a bug fix (bug fixed – with the bug) on TOA upward shortwave radiative flux (W m^{-2}) in amip type simulations for January (left panel) and July (right panel) in 2001.