Authors' response to referee comments on West et al. "The importance of vertical velocity variability for estimates of the indirect aerosol effects" (doi:10.5194/acpd-13-27053-2013).
MS No.: acp-2013-708

The authors would like to thank the two anonymous referees for their time and helpful comments on this paper, which we have taken on board and aim to address in a revised manuscript. The referees' original comments are reproduced in blue italics below, with our responses in normal type underneath. Text added to the manuscript is shown in quotations, following a bold page reference and line number.

Response to Anonymous Referee 1

Specific Comments

1.1) Please list TOA energy balance values for the four model configurations analyzed. Sigma_w must have a significant impact on TOA radiation. Would some of these configurations need to be retuned if they were to be used for coupled integrations?

The energy balance at the top of the atmosphere (TOA) for each model configuration is shown in Table 1.

Table 1: TOA energy balance

Configuration	PI TOA energy balance [Wm^{-2}]	PD TOA energy balance [Wm^{-2}]
sigw0.1	7.78	6.33
sigw0.4	5.89	4.00
sigw0.7	5.46	3.49
TKE_0.1	6.73	4.98

Page 27075, line 4
Add: "The energy balance at the top of the atmosphere (TOA) ranges from 3.5 to 7.8 Wm^{-2} depending on model configuration and aerosol emissions. Since the model was run in atmosphere-only mode for the purposes of this study, some imbalance is inevitable. As with any change to a model that affects radiative fluxes, retuning of the model's TOA radiation balance would be required before employing the scheme for coupled atmosphere-ocean integrations."

1.2) The material in Sections 1.1 and 1.2 bears significant resemblance to the Appendix in Golaz et al. (2011) with almost the exact same equations and references. It would seem fair to acknowledge at the onset that these sections were summarized from that work.

Thanks for pointing this out. We would like to add the following acknowledgement:

Page 27057, lines 4-6
Remove: "The remainder of this introduction provides a review of the characteristic and pdf-based approaches to the representation of vertical velocity variability."
Replace with: "The remainder of this introduction provides a review of the characteristic and pdf-based approaches to the representation of vertical velocity variability, based on the Appendix to Golaz et al. (2011)."

1.3) Page 27059, lines 25-26: this approach has now been implemented in two separate GCMs (doi:10.1175/JCLI-D-13-00075.1 and doi:10.1175/JCLI-D-13-00347.1).

Page 27059, lines 25-26
Remove: "but, at the time of writing, this approach is yet to be implemented in a GCM."
Replace with: "This approach has recently been implemented in two separate GCMs (Guo et al., 2013, Bogenschutz et al., 2013)"

1.4) Page 27064: can long time steps cause the autoconversion to decrease liquid water below the initial threshold, or is this prevented numerically?

This problem is prevented numerically: autoconversion can only begin once liquid water content exceeds a threshold value. Once in progress, the process of autoconversion is numerically prevented from decreasing the liquid water content below this threshold value.

Page 27064, line 18
After: "Within the scheme, autoconversion occurs when the liquid water content q_c is above a certain threshold."
Add: "Once in progress, the process of autoconversion is numerically prevented from decreasing the liquid water content below this threshold value."

1.5) Page 27066: the fixed value of l=40 m outside the PBL will be much smaller than Δz in Ghan et al. (1997) and hence sigma_w will be larger. I wonder whether this might partially explains the difference on how often the minimum value is imposed as noted in the text (98% on page 27058 versus 58% on page 27084).

Page 27084, lines 21-26
After: "While the spatial and temporal variability of sigma_w found in TKE_0.1 is more realistic than applying a fixed value of sigma_w = $0.4 ms^{-1}$ everywhere, technical restrictions within the current configuration of HadGEM-UKCA (such as the absence of a TKE diagnostic above the planetary boundary layer, and the lack of properly-resolved convective updraughts) cause an unrealistically high frequency of occurrence of the minimum value in this configuration (58 %)."

Page 27084, line 26
Add: "The fixed value of l=40 m imposed outside the planetary boundary layer for the calculation of TKE via Eqn 6 is likely to be much smaller than Δz in Ghan et al. (1997) and hence sigma_w, calculated via Eqn 7, will be larger. This might partially explain the difference in how often the minimum value is imposed here (58% frequency of occurrence of $0.1 ms^{-1}$), compared to similar work by Golaz et al. 2011 (98% occurrence of $0.7 ms^{-1}$)."

1.6) Figures 2c and 8c: are global mean CDNC weighted by cloud fraction? If not, shouldn't they be? Also, CDNC is defined differently in Fig 2c (cloud top) and Fig

8c (720 m). Is this really necessary? A single definition would be more desirable.

Page 27069, line 1

Add new opening paragraph: "In this paper, annual mean values of CDNC are not weighted by cloud fraction. Instead, a flag is used to identify "cloudy" grid-boxes at each time step, and thus to produce in-cloud temporal and spatial averages of CDNC and other cloud properties (where "cloudy" grid-boxes are defined as those in which both the cloud liquid water content and liquid cloud fraction exceed zero). While there are shortcomings to this choice, primarily that grid-boxes with small and large cloud fractions are weighted equally in the time mean, it was chosen for more realistic comparisons with satellite observations and in situ measurements of CDNC, which are not weighted by cloud fraction. CDNC is presented both at liquid cloud-top (Figure 2c) and at a typical cloud-base level (720m, Figures 8c and d) for illustration purposes and for comparison with satellite measurements and in situ observations respectively."

1.7) I enjoyed reading the analysis in Section 3.3 and corresponding figures. The compilation of the various observational datasets in such a concise form could serve as a valuable example for the evaluation of other models. I would encourage the authors to make their compiled data available.

We are grateful for this encouragement and are looking into making the data available, e.g. as part of an evaluation of AeroCom models.

1.8) Section 3.4: in addition to ∆CDNC, it would also be interesting to list CDNC separately for PI and PD. Is PI CDNC also dependent on sigma_w, and if so, do both PI and PD CDNC contribute to the range in aerosol indirect effects? I'm reminded of a recent work pointing out the importance of PI aerosols (doi:10.1038/nature12674).

Page 27075, line 18. After: "activate."

Add: "Table 2 shows the annual area-weighted mean values of in-cloud droplet number concentration at 720m for both pre-industrial and present-day conditions for each model configuration. The aerosol activation scheme was used in all simulations presented in this paper, and therefore both pre-industrial and present-day CDNC depend on sigma_w and contribute to the range of indirect aerosol effects." **(Insert Table 2)**

Table 2: Global mean in-cloud droplet number concentration at 720m

Configuration	PI CDNC [cm^{-3}]	PD CDNC [cm^{-3}]	CDNC diff (PD-PI) [cm^{-3}]
sigw0.1	37.29	50.57	13.27
sigw0.4	51.93	79.99	28.06
sigw0.7	55.74	90.13	34.40
TKE_0.1	42.79	63.97	21.17

1.9) Page 27075 and 27083: the range in RFP values from -1.4 to -2.0 W/m2 is large. Such a range would have a significant impact on the temperature evolution from PI to PD conditions in a fully coupled model (doi:10.1002/grl.50232).

Page 27075, line 8

After: "-1.4 to -2.0 Wm^{-2}",

Add: "It is possible that such a range could have a significant impact on the temperature evolution from PI to PD conditions in a fully coupled model (e.g. Golaz et al., 2013)."

*1.10) Technical corrections: * Page 27065, line 25: "campagns".*

Corrected.

Response to Anonymous Referee 2

Specific Comments

2.1) The model simulations used in this study were 1-year long nudged runs with 3 months spin-up, as presented in Section 2.3. If practical, the comparisons between different simulations to model configurations (e.g. Figs 3 and 9) might benefit from extending e.g. 5 years, since the signal-to-noise ratio for annual mean fields using 1-year model runs is inevitably poor for quantities of significant variability, such as those related to clouds.

We agree that using five-year simulations would improve the signal-to-noise ratio but, as the reviewer notes, this is unfortunately not a practical option, due to time and resource limitations. Aside from the slightly noisy figures, we do not have reason to believe that this limitation significantly affects our results.

2.2) Could you please comment briefly on the SW cloud radiative effect (CRE) as such for present-day simulations in addition to presenting the RFP? In particular, it would be interesting to see the difference in the local features of the CRE between the TKE_0.1 and sigw0.4 configurations.

Page 27075, line 27

Add new paragraph: "Table 3 presents the shortwave cloud radiative effect (SW CRE) for both pre-industrial and present-day simulations for each of the model configurations. The SW CRE increases with increasing sigma_w for both PI and PD simulations." **(Insert Table 3)**

Table 3: Annual mean SW cloud radiative effect

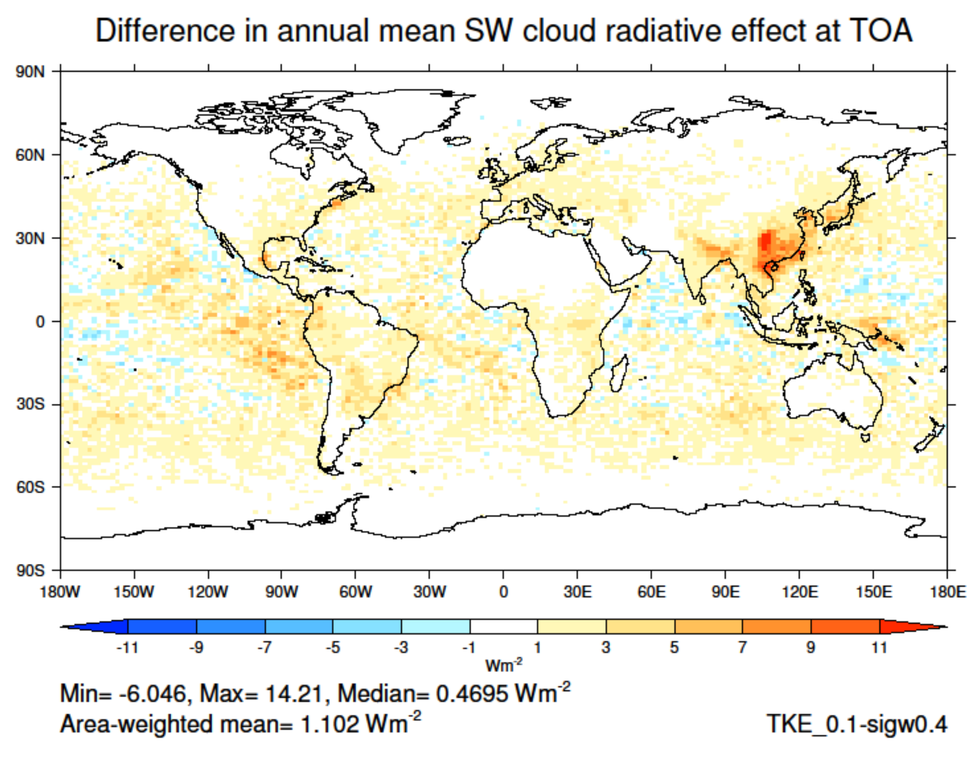
Configuration	PI SW CRE [Wm^{-2}]	PD SW CRE [Wm^{-2}]
sigw0.1	-41.09	-41.45
sigw0.4	-43.12	-43.97
sigw0.7	-43.58	-44.52
TKE_0.1	-42.18	-42.87

Page 27077, line 2
Add new paragraph: "Table 4 shows the difference in present-day SW CRE between pairs of model configurations. The spatial distribution of local features of the CRE between the TKE_0.1 and sigw0.4 is displayed in Figure 9d and closely mirrors the difference in RFP between those configurations shown in Figure 9c." **(Insert Table 4 and Figure 9d)**

Table 4: Difference in annual mean SW cloud radiative effect between pairs of present-day simulations from different model configurations

Model configurations	Difference in SW CRE (PD) [Wm^{-2}]
TKE_0.1 - sigw0.4	1.10
sigw0.7 - sigw 0.1	-3.07

Figure 9d:



2.3) Figure 2: Is the cloud top CDNC sampled as in-cloud or grid-box (i.e. normalized by cloud fraction) mean values? As a follow-up, how exactly do you calculate the global mean?

This is addressed in the response to comment (1.6) from Referee 1.

2.4) Page 27059, lines 10-15: Hogan et al. 2009 (doi: 10.1002/qj.413) presented negative skewness of vertical velocity associated with cloud driven mixing and that the sign of the skewness can vary within the same column below the cloud deck when under the influence of both surface-based and cloud-driven turbulence.

Page 27059, line 14

Before final sentence, add: "Hogan et al. (2009) presented ground-based measurements of negative skewness of vertical velocity associated with cloud-driven mixing and showed that the sign of the skewness can vary within the same column below the cloud deck when under the influence of both surface-based and cloud-driven turbulence."

2.5) Section 3.2.3 + Section 4: Reutter et al. 2009 (doi:10.5194/acp-9-7067-2009) identified updraft- and CCN-limited regimes for cloud activation in convective clouds. The results seem at least qualitatively applicable also for a more general case and agree with the findings in this manuscript as well.

Page 27084, line 2

Add: "There are parallels here with work by Reutter et al. (2009), which also identified vertical velocity-limited and CCN-limited regimes for cloud activation in convective clouds."

2.6) Table 3, typo in CSTRIPE: "Sstratocumulus".

Corrected.