# Response to reviews of "What controls the vertical distribution of aerosol? Relationships between process sensitivity in HadGEM3–UKCA and inter-model variation from AeroCom Phase II"

Z. Kipling et al.

January 14, 2016

We are grateful to the two anonymous referees for their time and constructive comments on the discussion paper. We have made a number of alterations in the revised manuscript to address their points, which we hope is now clearer as a result. Responses to individual points raised, and details of changes to the manuscript, are given below.

## Response to Anonymous Referee #1

> *1. The vertical position metric of the mean-mass-weighted mean pressure level (or vertical centere of mass in pressure coordinates), p(c) is defined in Eq (1) of section 4.3. m(k)*M(k) [which is the product of aerosol mixing ratio and air mass] can be combined into aerosol mass. Then p(c) term is quite similar with Koffi's (2012) "extinction mean height diagnostic". Following Koffi et al, The current p(c) can also be called as the "mass mean pressure height diagnostic".*

Thanks for drawing our attention to the similarity between these two metrics. We have added a new sentence to note this:

> **This construction is similar to the "extinction mean height diagnostic" of Koffi et al. (2012), and this metric could be analogously termed the "mass mean pressure level diagnostic".**

> *2. The current paper only relies on model simulations without any observational constrain. I would think that constraining aerosol vertical distribution with observation is highly difficult task. Vertical distribution of number concentration would be even more difficult. However some satellite data such as CALIOP (Koffi et al., 2012) provides global view of vertical distribution of aerosol extinction. Please discuss how your results are compared with observations.*

We agree that the question of observational constraints on these vertical profiles is an important one, and that observations such as those from CALIOP and large-scale aircraft campaigns will be important in that context. A detailed evaluation against the available observations, however, would be well beyond the scope of this paper which is focussed instead on model diversity and process sensitivity. In this respect, the study presented here should be seen as complementary to the approach of Koffi et al. Some comparison of the major profile features against CALIOP, however, has been added in response to point 3.

> *3. In section 5 and 6, it is stated that HadGEM3-UKCA does not reproduce "inverted S" and "U" shapes that are common to other models. I wonder what is physical implication of the unique patterns in HadGEM3-UKCA. Please discuss what are the controlling mechanisms that determine those shapes in other models. Are these shapes are just model-simulation feature or are they constrained by observations? Also please specify that the "inverted S" shape is inferred from the log scale in Figure 1.*

In the models, these shapes vary considerably amongst the different aerosol components, which makes comparison with the available remote-sensing observations (which are usually not speciated) difficult. However, Koffi et al. (2012, Fig. 6) see a regional variation between decreasing-with-height profiles (e.g. NAF DJF) and more S-shaped ones (e.g. CAF JJA), suggesting that both types of profile do occur. The following paragraph has been added in Section 6 discussing this:

> **Because the profile shapes vary considerably amongst the aerosol components, evaluation against the available observations (which in general cannot separate the components) is difficult. Nevertheless, CALIOP observations suggest that both decreasing-with-height and more S-shaped profiles do occur in certain regions and seasons (Koffi et al., 2012, Fig. 6). It seems likely that this relates to different balances of processes, in a similar way to the varying profiles in the model simulations.**

> *4. Page 25941 L6-11: It needs references. Or please specify if they are assumed.*

These details of the scavenging parameterisation are not currently well documented in the published literature, although it is anticipated that they will be included in Mann et al. (2016), already cited for the UKCA aerosol scheme as a whole. They are also mentioned in passing in Kipling et al. (2013), already cited for the convective scavenging.

> *5. Please provide mean height in numbers in Table 3. Also please add a sentence why DU is zero in NO-BLMIX.*

In many cases this table is showing meridionally-varying effects, which would not be reflected in a global mean height. Adding heights at multiple latitudes would make the table very complex, and it is intended to provide an at-a-glance summary of the effects described in the text and figures, rather than be a source of detailed numerical data.

The following sentence has been added in the penultimate paragraph of Section 5.2 regarding DU in NO_BLMIX:

> ...never mixed upwards and is immediately removed...in the model. **There is thus virtually no mineral dust transported in the atmosphere of this simulation.** (The high altitude shown...)

# Response to Anonymous Referee #2

## Specific Comments

> *1) P 25935, L11-12: The abstract indicates that the HadGEM3-UKCA sensitivity simulations replicate the AeroCom diversity in the both the vertical profile and vertical position metric, but the discussions in the text seem to indicate the position metric diversity is not well replicated by the sensitivity simulations. Should this be clarified in the abstract?*

Changed to clarify this:

> . . . in terms of the global mean profile and**, to a lesser extent, the** zonal-mean vertical position.

> *2) P 25935, L13-14: Consider identifying more explicitly what is meant by 'structural differences'. Does this relate to process parameterizations, meteorology, model resolution, whether the model allows feedbacks between the aerosols and meteorology, or otherwise?*

These aspects all potentially contribute to the structural differences which are not reproduced in the sensitivity tests, although we are particularly concerned with the first of these. The text has been expanded to clarify this:

> whether further structural differences between models are required to explain this diversity**. The variety of parameterisations used for physical processes will naturally have an impact, but differences in meteorology, resolution and aerosol–meteorology feedbacks may also play a role.** ~~, in the~~ **We** hope that this will aid in the understanding of which model components are responsible for this diversity.

> *3) P25942: Are these emissions the same as used for the AeroCom Phase II simulations?*

Not quite – the differences between the configuration used here and that used for the AeroCom submission are covered in the final paragraph of this section (p. 25943).

> *4) P 25943, L12: What was the technical problem that caused a different model configuration to be used? Are you able to comment on the influence of model vertical resolution on your results?*

Lack of stability when perturbed outside of well-tested configurations caused many of the sensitivity tests to fail in the newer version. Added a sentence to discuss the implications:

> **Whilst this difference is unfortunate, and we might expect the higher vertical resolution to improve the representation of the vertical profile, we are not aiming to replicate this submission exactly but to compare against the diversity in the ensemble as a whole – and for this purpose, the resolution used here is still well within the range of the other AeroCom models.**

*5) P 25944, L1-2: Are there any other issues related to emissions that can affect the vertical distribution, other than the altitude of injection and the assumed size distribution? What about the magnitude of the emissions or feedbacks between primary emissions and the meteorology?*

We consider these to be the most significant ways in which primary emissions might affect the vertical distribution. We would expect the magnitude of emissions to affect the burden more than the vertical distribution. Certainly feedbacks between interactive emissions and meteorology could have an effect, and would be one of the "additional structural uncertainties" referred to in the paper.

*6) P 25949, L1: Figure 2 is introduced here and there appears to be only one sentence of discussion. Please consider adding to the discussion here, or is this figure needed?*

The following text has been added at the end of the paragraph, which links with the response to point 14:

> **Although this study is primarily concerned with the vertical distribution rather than total burden, it is worth noting that the burdens of all components vary by about a factor of four among the AeroCom models, and by an order of magnitude among the sensitivity tests.**

*7) P 25949, L10-15: The text suggests that the 'variations in the processes we have considered can largely replicate the model diversity'. I am having some trouble making this connection. Could you also replicate this diversity by changing some of model structural aspects as related to the 'structural differences' that you mentioned between models? Then, could this agreement between the AeroCom inter-model diversity and the within-HadGEM3-UKCA-sensitivity-simulations diversity be for the wrong reasons?*

The reviewer is correct, in that while variations in these processes can replicate the diversity, they are not the *only* way of doing so. We only claim that these processes are *sufficient* to produce this diversity, not that they are necessarily its *cause* amongst the AeroCom models.

*8) The sensitivity studies consider the change after reduction of a given process to a negligible influence – are you suggesting then that certain of the models have these processes parameterized with varying degrees of efficiency at affecting the vertical profile? Related to this, within the HADGEM3-UKCA, if a process is parameterized with a relatively low efficiency at affecting the vertical profile, then shutting it off, will appear to have less impact than it would in another model where the base simulation had a more vigorous parameterization for that same process. This makes these results very model specific. I would like to see a bit more discussion in the text about the aspects of the methodology of this study that make the conclusions fairly model specific.*

It is certainly possible that different parameterisations of a given process result in different effects on the vertical profile, in which case we might ask which parameterisation best captures the effect of the real physical process (although that is not the focus of this study).

The methodology used here is more concerned with the possibility that the balance between different processes, each with a distinct signature on the vertical profile, varies among the models. This would then be expected to lead to diversity both in the resulting profiles, and their sensitivity to different processes.

The reviewer is correct to point out that a process that has relatively little effect on the vertical profile in one model (either because of the way it is parameterised, or because it is simply not very active) may nevertheless have a strong effect in another model where it is more active or parameterised differently. The following paragraph has been added at the end of Section 6:

> **It should be acknowledged, however, that the dominant processes controlling the vertical profile are not necessarily the same in different models (e.g. a process which has little impact on the vertical profile in HadGEM3–UKCA may nevertheless have a strong impact in a different model). Parameterisations of a given process may vary in how they capture the effect on the vertical profile, and the balance of processes may well differ amongst models. Both of these factors, along with other structural differences between the models, will contribute to diversity both in the vertical profiles themselves and their sensitivity to different processes. It would therefore be informative to conduct similar experiments with a range of models to assess how model-specific these dominant processes are.**

> *9) P 25949, L 25: Are you able to comment on why the model has the vertically uniform sulphate profile, unlike most of the AeroCom models?*

The sensitivity tests suggest this may be related to the parameterisation of large-scale scavenging and convective transport (as NO_LS_RO and NO_CVTRANS have a less uniform profile). This is discussed in the third paragraph of Section 6.

> *10) Figure 3: The text discusses the vertical position metric for the AeroCom models and then comments that none of the sensitivity simulations can reproduce the U shape seen for the AeroCom models (except NO_WETOX for dust and sulphate, whereas most are flatter and with smaller vertical range). How then do we interpret this result relative to the result from the discussion of Fig. 1 that indicated the model was replicating the global mean profile diversity of the AeroCom models?*

This suggests that some of the additional structural differences between the models alluded to elsewhere must be required to explain the meridional variations in the vertical profile. The following sentence has been added in (what was) the penultimate paragraph of Section 6 which discusses aspects that were not reproduced, and further structural differences:

> **In particular, it appears likely that such factors are responsible for the difference between “U”-shaped and flatter meridional profiles, which was largely unreproducible in HadGEM3–UKCA in this study.**

*11) P 25952, L10-11. The discussion of Table 3 is only one line. Consider introducing the table earlier.*

The table is a summary of the main effects described throughout the preceding section, thus referring to it briefly at the end seems appropriate.

*12) P 25954, L6: Consider starting the paragraph with the most important effect as opposed to placing as the last sentence of the paragraph. There are numerous effects discussed in this section and it would be helpful to have the main points about what we are learning from the figure placed more towards the start of the discussion or at least start of each paragraph.*

This is a good suggestion – we have reorganised the paragraph to lead with the strong effects of convective transport and wet deposition, and the other processes whose importance is increased, before mentioning the microphysical effcts which remain important but are less dominant.

*13) P 25954, L13: The most important effect is imbedded in the paragraph but could be moved earlier.*

Again, we have lifted the mention of convective transport (the strongest effect) to the start of the paragraph as suggested.

*14) P 25955, L3: The authors comment that the sensitivity tests are not physically realistic and lead to large changes in aerosol loading. It would be instructive to have some indication about how physically realistic these simulations are in comparison to observations. Is this possible to evaluate with the global profiles or have you some indications from examining more regional scales?*

From Figure 2, aerosol burdens vary by about an order of magnitude among the sensitivity tests. Burdens are not particularly well constrained by observations (and evaluation against observations is not really within the scope of this study, as discussed in response to Referee #1's point 2); however those from the AeroCom models vary over about a factor of four.

This suggests that, while the sensitivity tests lead to burdens that are probably unrealistic, for the most part they are not so extreme as to cast significant doubt on their effects on the vertical profile. Correcting for this variation in total burden is also the reason behind the use of normalised forcing (or forcing efficiency) in Sections 4.4 and 5.4. See also the response to point 6.

*15) P 25955, L18-23: What can we learn from this indication that the HadGEM3-UKCA simulations have a similar diversity to the AeroCom vertical profiles, but not for the zonal vertical position metrics?*

See response to point 10.

*16) The basic conclusions about the process sensitivity in HadGEM3-UKCA are very model specific, consider discussing this more explicitly in the discussion.*

See response to point 8.

*17) P 25957, L5: Please clarify what is meant by 'Arctic processes'.*

Changed to clarify:

> ... high latitudes**. This suggests that the processes controlling transport to, and lifting and removal within, the Arctic** may be key...

*18) P 25957, L14-15: The start of the paragraph suggests a shift towards more accumulation-mode particles might contribute to this U shape. However turning off the nucleation did not have this effect. Are the authors able to offer any insights on any other possible contributions to this U shape?*

The experiments carried out here do not provide an explanation for this variation in shapes between the AeroCom models, but as discussed in response to points 10 and 15, it seems likely that some of the structural differences between models not included in this study may be involved.

*19) Consider explaining more clearly, if possible, what we can learn from the comparison between the set of sensitivity studies and the AeroCom ensemble. It is interesting to check if the HadGEM-UKCA sensitivity simulation diversity agrees with the AeroCom diversity, but are you able to help make the connection between the two any more meaningful? Despite having the same diversity, it is difficult to understand whether or not this is agreement for the wrong reasons – it seems very difficult to rule out the possibility that the 'structural differences' could contribute strongly to the AeroCom diversity. As a result it is difficult to interpret these results without knowing the extent of this influence. The discussion at the end of P25957 and beginning of P 25958 does indicate these issues, but I am still left wondering how to interpret these comparisons between the AeroCom ensemble and the HadGEM-UKCA sensitivity simulations.*

See response to point 8.

*20) P 25959, L 20-25: This is a good point that the study can not determine if the processes identified in this study as being important in controlling the vertical aerosol profile are universally the most important in all models. Thus, the authors suggest that the same study be conducted with other models. Based on this study's methodology, if the same results for what controls aerosol vertical profiles were obtained after con- ducting this same study among a set of other models (i.e. shutting off the processes one by one), would this then imply that there would be less diversity in the vertical pro- files within that model set, considering the set of simulations with all the processes left intact in those models? In other words, does an agreement on what controls the vertical profiles under this methodology imply not much diversity between modeled vertical profiles?*

Not necessarily – the controlling processes may be similar amongst a set of models, but if the balance of these processes differs then this may lead to variation in the profiles.

## Technical Corrections

*1) P25942, L21: Could 'model levels 2-12' be removed since the subsequent altitude range is more meaningful to most readers?*

Although the altitudes are more widely meaningful than the explicit model levels, we prefer to keep it explicit that the emissions are over a fixed range of hybrid model levels – and thus the exact altitude range varies over orography.

*2) P 25943, L3: Should the year be added for the Diehl et al. reference?*

Added (2012).

*3) P 25946, L4: Add 'with diameters' before 'greater than 3, 30,100…'*

Changed:

…(CN) **with dry diameters** greater than…nm ~~dry diameter~~.

# References


Kipling, Z., Stier, P., Schwarz, J. P., Perring, A. E., Spackman, J. R., Mann, G. W., Johnson, C. E., and Telford, P. J.: Constraints on aerosol processes in climate models from vertically-resolved aircraft observations of black carbon, Atmos. Chem. Phys., 13, 5969–5986, doi:10.5194/acp-13-5969-2013, 2013.

Koffi, B., Schulz, M., Bréon, F.-M., Griesfeller, J., Winker, D., Balkanski, Y., Bauer, S., Berntsen, T., Chin, M., Collins, W. D., Dentener, F., Diehl, T., Easter, R., Ghan, S., Ginoux, P., Gong, S., Horowitz, L. W., Iversen, T., Kirkevåg, A., Koch, D., Krol, M., Myhre, G., Stier, P., and Takemura, T.: Application of the CALIOP layer product to evaluate the vertical distribution of aerosols estimated by global models: AeroCom phase I results, J. Geophys. Res., 117, D10 201, doi:10.1029/2011JD016858, 2012.

Mann, G. W., Johnson, C. E., Bellouin, N., Dalvi, M., Abraham, L., Carslaw, K. S., Boucher, O., Stier, P., Rae, J., Spracklen, D. V., Telford, P., Pyle, J. A., O'Connor, F., Carver, G., Pringle, K. J., and Woodhouse, M. T.: Evaluation of the new UKCA climate–composition model. Part 3: Tropospheric aerosol properties, in prep., 2016.