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Interactive comment on "The importance of vertical velocity variability for estimates of the indirect aerosol effects" *by* R. E. L. West et al.

Anonymous Referee #1

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General comments

This manuscript describes the impact of the vertical velocity variance for cloud drop nucleation on the magnitude of the aerosol indirect effects. The authors find that the magnitude of the indirect effects increases as the velocity variance is increased. This is a significant finding that to my knowledge has not been demonstrated so clearly before. The manuscript is clear and well written. I would recommend it for publication in ACP after minor modifications suggested below.

Specific comments

* 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 con-

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figurations need to be retuned if they were to be used for coupled integrations?

* 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.

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

* 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).

* 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.

* 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.

* Section 3.4: in addition to ΔCDNC, 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 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).

Technical corrections

* Page 27065, line 25: "campagns".

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