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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraining the Twomey effect from satellite observations: Issues and perspectives" *by* Johannes Quaas et al.

Anonymous Referee #1

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General comment:

This article provides a comprehensive review on how to estimate the Twomey effect from satellite observations. The review builds upon simple formulations that decompose the radiative forcing due to the Twomey effect into several terms corresponding to different physical processes accounting for spatial (horizontal) and temporal variabilities of cloud, aerosol and dynamical fields, as represented by Equations (2), (3) and (4). These equations well serve as a basis for discussing and pointing out issues in quantifying the Twomey effect at a scale relevant to climate, which is of particular interest in this review. Key sources of error or uncertainty in quantifying the Twomey effect are then reasonably identified and separated to facilitate the discussion and propose way forward for alleviating the overall uncertainty. I only have relatively minor com-

ments that I would propose for the authors to consider for further improvement of the manuscript.

Specific comments:

1. This may be just my misunderstanding, but the authors seem to argue that a use of Nd, instead of Reff, can circumvent constraining LWP for quantifying the Twomey effect. Is it correct? To my understanding, estimates of the Twomey effect, by its definition, always require the LWP to be constant so that the data always need to be stratified by LWP whether Nd or Reff is used for analysis. Can the authors clarify why Nd is more advantageous than Reff for estimating the Twomey effect? Explanations in Section 3.1 are not convincing enough.

2. The authors show several lines of evidence that past studies likely underestimated the radiative forcing due to the Twomey effect with some quantitative information of how large is the underestimates (such as those shown in Figures 1 and 3). I am just wondering if the authors could propose a range of estimate for the radiative forcing that is "corrected" from the existing estimate (like IPCC AR5) accounting for the factors listed in the manuscript that may have caused the underestimate. Such a quantitative estimate would be desirable to show if it is possible.

3. In section 2.1, the authors should explain in more detail why and how the EarthCARE lidar can improve the accuracy of retrieving and discriminating aerosols and clouds, particularly for those of readers who are not familiar with EarthCARE lidar specification. In particular, more explanations would be useful for how ATLID can (i) better distinguish the optically thin clouds and aerosols and (ii) better profile the aerosol extinction, with the capability of HSRL enhanced from CALIOP.

4. In section 2.2: How can recent geostationary satellites with unprecedentedly high spatial and temporal resolutions provide potentially useful information for horizontal collocation in the context of trajectory approach? For instance, Kikuchi et al. (2018) exploited the high frequency sampling of Himawari-8 to create a new data set of AOD

interpolated to the location collocated with clouds that is likely more relevant to CCN.

5. In section 2.3: Is there any specific way of parameterizing the dry aerosol properties from the humidified one? Some literature information would be desirable to let the readers to have more specific ideas of the issue of swelling.

Reference:

Kikuchi, M., and Coauthors, 2018: Improved hourly estimates of aerosol optical thickness using spatiotemporal variability derived from Himawari-8 geostationary satellite. IEEE Trans. Geosci. Rem. Sens., 56, 3442-3455, doi:10.1109/TGRS.2018.2800060.

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