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

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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

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Discussion paper

way forward for alleviating the overall uncertainty. I only have relatively minor comments that I would propose for the authors to consider for further improvement of the manuscript.

We thank the reviewer for their excellent summary and kind assessment 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.

The reviewer is right that the Twomey effect, understood as a radiative effect, has to be considered at constant LWP. This was a sloppy formulation in the Discussion manuscript. What rather was meant, was that Eq.Âă2 is better formulated with Nd rather than reff: the middle term, $\partial \ln N_\mathrm{d} / \partial \ln a$ *is much more straightforward evaluated than if one would go for* $\partial \ln r_\mathrm{e} / \partial \ln a$*. In the formulation with Nd, the only other relevant quantity is the vertical wind velocity, while in the formulation with reff one would need to control also for L which is adds a lot of complexity. This clarification is now added to the revised manuscript.*

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.
The reviewer raises a good point that we internally discussed quite a bit, too. We in the end decided not to provide a new "best estimate". The reason is that although there are a number of studies that address important aspects of the problem and overcome several of the shortcomings listed, none yet does address all. It would this provide the false impression that a solution already exists.

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.
Two extra sentences explaining this are added to the revised manuscript.

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.
This is an excellent point by the reviewer, and the high potential of geostationary satellites increasingly receives attention in the field. A corresponding statement is added.

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.
Very valid point by the reviewer. We now explicitly explain which parameterisations we think of in getting from humidified to dry aerosol, citing the relevant references.

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