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Interactive comment

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

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This overview paper is a pretty substantial and concise overview of Twomey effect diagnostics from space, principally with passive solar observations. The paper is generally well-written (save a few passages – something not unexpected given the many co-authors and the unavoidable mixing of styles) and breaks down the problem in an intelligent and intuitive manner. The heart of the paper is eq. (4) which is then further recast as eq. (5). These equations indicate that assessing the strength of the Twomey effect rests on being able to predict the change in cloud droplet number concentration given an anthropogenic CCN perturbation. The latter is not examined; rather the paper focuses on whether the sensitivity of droplet concentration to changes in CCN can be inferred from space observations. The issues investigated are whether aerosols

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(and what aerosols in terms of vertical location) can stand-in for CCN and at which level in the cloud the knowledge of the droplet concentration is relevant to calculate the Twomey radiative perturbation. Given the nature of the paper, there is really no original research, but there is plenty of good insight. The paper lacks visual support: there are only three figures in 18 pages. To me at least, it seemed as if the paper loses steam starting in section 4 when text appears to suffer from deteriorating clarity and appears to be more hastily written. But all in all, this is a very noteworthy effort that does not need much of a revision before it becomes a reference to be frequently visited by the aerosol-cloud interaction community.

We thank the reviewer for their thorough assessment of the manuscript. The impression that sections 4-6 seem to be less substantial is certainly not because these issues are less relevant or that we paid less attention – it is merely the fact that one cannot rely on as large a body of research as is the case for the first two issues (Sections 2 and 3).

Some remarks/suggested edits:

Line 10 and many instances thereafter: “vertical wind” does not seem the right term; rather people traditionally use the term “updraft velocity”, or, given the convention of this paper, “updraught velocity”.

Modified as suggested.

Line 11: “10s”, this read like 10 seconds to me, so better write explicitly “tens”.

Modified as suggested.

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Line 21: “the impossibility” (of retrieving base CCN): Well, some would disagree, and the paper itself does cite Rosenfeld et al. (2016) who claim that such retrieval is possible. See line 289.
Agreed! The word is changed to “difficulty”.

Line 53: Cloud horizontal extent is actually irrelevant, if the quantity of interest is cloud albedo. Cloud fraction becomes relevant only when the dependences of the Twomey effect on spatial scales is discussed and then only when mixtures of clear and cloudy skies are considered, namely the Twomey effect is expressed in terms of the cloud radiative effect.
The reviewer is correct, and this mistake is corrected!

Line 54: “a_c is a monotonic function of N_d”: only when the cloud condensate is constant.
The reviewer is right. The statement is corrected by specifying that this is true in the partial-derivative-sense.

Eq. (2): A derivative of absolute a_c change with respect to a relative (logarithmic) N_d change is shown, while eq. (1) is expressed in terms of relative changes for both quantities. It may make sense to keep these consistent. See also line 81.
This is a very good suggestion by the reviewer. We opted for modifying Eq. 1 accordingly.

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Line 66: SOLAR zenith angles.
Modified as suggested.

Lines 75-79: N_d is also a function of L (you say that actually in line 323), so I don't understand the argument here, which is fundamental for insisting that Twomey effect studies are conducted in terms of N_d (not a directly retrievable quantity) and not r_e (which is directly retrieved). Changes in L can be distributed as both droplet size and droplet number changes, no? See also lines 435-436 about the need to stratify by L when using r_e.
More detail on this is added now. The idea that re and L are both extensive quantities (dependent on mass), Nd is intensive is now explicitly formulated here, too.

Lines 169-171: Need to clarify that this is the case for passive SWIR observations. Lidar retrievals are discussed elsewhere in the paper.
The reviewer is right. We added "passive" to the sentence.

Line 200: I suggest "become less representative of aerosol variability".
Modified as suggested.

Line 201: To be consistent with elsewhere in the text: "updraughts".
Modified as suggested (in fact, ACP encourages British English).

Lines 271-272: It is implied here that AI is routinely available from space. Is it? For example, MODIS dark target provides AI only over ocean. Is it reliably retrieved? Fig. 2 excludes the land, probably because of this exact unavailability of AI over continents.
The reviewer has a good point. AI is available, but not very reliable. That information is now added.

Line 284: The MERRA-2 aerosol re-analysis is also another popular product. Later in lines 287-288, it is not clear how one can evaluate re-analysis aerosol, especially underneath cloud. One has to use observations that are not part of the assimilation process.
The reviewer is right. A reference to MERRA-2 is added. Indeed, for evaluation one would need other data, such as from the ground; this is clarified now.

Line 294: I suggest "derivations of supersaturation".
Modified as suggested.

P. 12 discussion on N_d retrieval uncertainties: The discussion seem to suggest that higher resolution measurements are needed to reduce cloud heterogeneity effects, yet the retrievals should eventually be coarsened anyway to reduce the random error.
The reviewer is of course right. The point we wanted to make was probably a bit unclear since we did not provide the precise reference, which is now corrected (Zhang et al., 2016).

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Lines 359 and 362: Deriving cloud base and cloud physical thickness is of course one of the most difficult problems in space-based remote sensing. Lidar can be useful only when the clouds are optically thin (optical thickness below 3-4). So, I wouldn't count too much on space-based lidars for many of the clouds that are relevant to the Twomey effect.

On the one hand, we agree with the reviewer that this is a difficult problem. On the other hand, a couple of studies are referenced that discuss the problem and propose solutions.

Line 401: "$\hat{\beta}$ is smaller than unity". Earlier, line 87, it was established that beta is smaller than unity. No range was given for $\bar{\beta}$, but presumably the same implies. Do the authors then mean to say in line 401 that $\hat{\beta}$ is smaller than $\bar{\beta}$?

It is indeed not completely evident. But what we meant is that it is indeed less than, not equal to, unity (the physically plausible range would include 1). We add the word "somewhat" to make this more clear at this point.

Line 438: conditions cannot become small, so the authors need to rephrase.

The reviewer is right. What really was meant is too homogeneous. It is reworded.

Line 445: I suggest you say "closer to $\sim$50 km scales".

Modified as suggested.

Section 6: I found this section about confusing, but I think mostly because of my un-

familiarity with the “regression dilution” concept and the ways its impact is assessed. The term does indeed exist and describes the biasing of the regression slope towards zero values, but you may want to provide a brief definition and description. For people who are familiar with this bias tendency this section may make more sense. Please revisit and ensure that you provide maximum clarity to the uninitiated.
Accepted, it is indeed helpful to provide some more explanation, which we did in the revision. Also more references are now added.

Lines 473-474: “the impossibility to retrieve it in cloudy skies”. This is a sweeping statement which need some qualifiers. Yes, you can’t probably retrieve aerosol under clouds in most situations, but with lidar it is possible both above and below clouds for certain clouds. Also you can retrieve aerosol between individual clouds of a cloud field from both passive and active. Such a cloud field is still “cloudy skies”.
The reviewer is right, this was a sloppy formulation. We revise to say “below clouds”.

Line 480: I suggest “in addition to retrievals”.
This was confusing indeed, but meant in a slightly different way. Reworded to “The hygroscopic swelling can be addressed by parameterisations that use retrievals and ancillary data to compute the swelling.”

Line 486: I suggest “relates imperfectly to the N_d”.
Modified as suggested.

Line 487: You mean sensitivities less than one? I don't understand as it is currently written.
Indeed, the formulation the reviewer suggests is better!

Line 504: I suggest "quantification supported by data"
Modified as suggested.

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