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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Unveiling aerosol-cloud interactions Part 1: Cloud contamination in satellite products enhances the aerosol indirect forcing estimate" *by* Matthew W. Christensen et al.

Anonymous Referee #1

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Christensen et al. present a new technique of relating aerosol- and cloud retrievals from satellite data. They created an algorithm that searches for the nearest aerosol retrieval for each cloud retrieval. Different from previous approaches (Bréon et al., 2002), no backtrajectories are computed, but the nearest pixel, independent on whether or not the aerosol might actually be advected to the cloudy region. Despite this, it is an innovative approach and may indeed help overcome some issues with the approach still commonly used to relate 1°x1° average aerosol- and cloud retrievals. The authors analyse statistical relationships between the aerosol index and cloud albedo computed on the basis of satellite cloud retrievals using a radiative transfer code, as well as between AI and cloud fraction. They proceed to compute implied radiative forcings.

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Discussion paper

The manuscript is astonishingly superficial in many of the explanations. Many statements are very difficult to follow, or not at all reproduceable from the information provided.

The authors are imprecise in their language. It seems they in general want to assess the effective radiative forcing due to aerosol cloud interactions, i.e. the overall cloud response to the aerosol, including cloud water path and cloud fraction changes.

Nevertheless, it is a useful paper and should eventually be published. However, I have numerous specific points the authors should address.

P1 L17 Not so much in satellite estimates

L20 the "buffering" isn't precisely defined. A better more specific explanation on what is missing is necessary

P2 L1 given the large range of GCM estimates, it needs to be clarified which publications the authors refer to

L4 remains

L4-10 the order is awkward. If one had proper CCN retrievals (in the order the authors impose item 5), items 1-3, perhaps even 4, wouldn't matter. Also not all problems are pertinent to all aerosol-cloud interactions. The authors need to be specific about what exactly they want to study and where which of the issues arises.

L11 the authors need to clarify what they mean by "contamination" (do they mean problem 1, 2, or 3?) 3, to some extent 2, cannot really be called "contamination" since these are plausible physical processes. It is also important that the authors shouldn't forget to mention that clouds are also an actual source of aerosol. Sulfate predominantly nucleates via the aqueous phase.

L19 "larger" than what? And do the authors really refer to a forcing here, or rather to an effect?

L29 that hold of course only if one analyses one grid box over one season. Experience shows that in such attempts, very rarely 90 data points would be available.

L31 of course also the problem of spurious clouds in pixels labelled cloud -free

P3 L1 this statement needs further explanation to be understandable.

L3 While the authors call their method “new” they should acknowledge at the presumably first aerosol-cloud interaction study from satellites (Bréon et al., 2002, doi:10.1126/science.1066434) already applied such a method.

L7 the theoretical maximum for the 1km MODIS retrievals of clouds is about 110x110x90 = 11x10ˆ5. Is the reduction by a factor of 3-4 an empirical result?

L8 Can the authors clarify what the scale of the MODIS retrievals is? I believe it is 1 km for the cloud product, but is it also 1 km for the aerosol product?

L16 It would be good to report the overpass time

L18 “seconds” should be abbreviated (“s”)

l24 the authors should explain their statement “this consistency is essential”. The conclusion is not straightforward, but obviously using the same cloud mask for aerosol- and cloud retrievals also introduces issues.

L26 bracket awkward

p4 l7 The appropriate reference for MODIS collection 6 cloud products is Platnick et al. (2017, doi:10.1109/TGRS.2016.2610522)

l22 “lower” than what?

L27: $\Fˆ\downarrow_\mathrm{clr}$: why the index “clr”, this is just the incident solar radiation, it seems? Why not operate in Eq. 1 simply with reflected fluxes that are actually observed? Also, at a pixel level, CRE is not defined from observations (cloud fraction is either one or zero). At which scale do the authors compute the CRE?

L28: It is a bit misleading to call F_obs “observed”. It obviously rather is the flux computed on the basis of the aerosol and cloud retrievals. Why not “all-sky” as usually defined? Are the clear-sky thermodynamic profiles from reanalysis for the appropriate grid cell? Is the humidity for these the all-sky or the clear-sky humidity? In which sense is “clr” less “observed” than “obs”? Isn’t that applying the retrieved aerosols?

P5 l4 This is a very loose definition of an “indirect effect”. The authors can of course define such a quantify. Usually one would call the definition in Eq. 4 something like a “cloud radiative effect sensitivity”, and if one multiplies this with the anthropogenic \Delta AI, one would obtain a proxy for the effective forcing due to aerosol-cloud interactions (proxy since it only accounts for column physics).

L8 it is peculiar that the authors make use here of the annual-mean incoming solar radiation. The factor in the brackets presumably has a strong diurnal and annual cycle. Co-variation of this factor with the incoming solar radiation then leads to possibly substantial differences in the radiative effects compared to the ones proposed by the authors.

L9 “is called”, I propose the authors rather specify “can be called” or “is called here” (or provide a reference if they use this term from a definition elsewhere).

L13 The authors should provide the reference of where this “has been shown”

l21 What is assumed about the anthropogenic fraction of the Ångström exponent?

L28 “Square” in terms of pixels?

L29 Is the 250x250 pixel square moving with the cloud retrieval? If not, couldn’t it easily appear that the nearest aerosol retrieval is in the next, not analysed, square? Maybe the authors can provide a sketch to clarify what exactly they are doing.

L31 Again, it is necessary that the authors define the scale at which they determine a cloud fraction. So far, I understood from the text that they work at the pixel level (1x1

km^2). At this scale, cloud fraction is simply zero or one.

P6 l12: ", whereas"

l14: This statement is inconsistent with the "methods" section where it was stated that the aerosol is retrieved at 1 km resolution.

L19: why not describe what actually is found, namely that the Ångström exponent decreases for pixels nearer to the clouds? Of course it is possible to interpret this in terms of particle size, but this cannot be quantified.

L21: this is hard to see from Fig. 2. Could the authors help the reader with a more readable figure, e.g. by horizontal lines?

L23: It should again be made clear what is meant by "contamination". Is the limitation to the inner half of the points in terms of brightness authors the only thing done in the MODIS retrieval to address such issues? Don't they also use different cloud fractions for cloud- and aerosol retrievals?

L25: Of course AOD is also large near clouds due to swelling, but this is not "artificial".

L28: Once more "contamination" - it could be cloud contamination, but could also be 3D effects or swelling, one cannot tell these apart from the analysis.

P7 L28: Obviously the standard error decreases with the sample size as nˆ-1/2. But didn't the authors discuss standard deviation?

P8 L8: To me it seems that the differences are mainly due to the result that for CAPA-L2, the regression coefficients seem to be mostly positive, while for CAPA-2L_15km, there are very large areas where the regression coefficients turn positive.

L12: A better approach would be to show joint histograms. It would be advisable to use the method of Gryspeerdt et al. (2016; doi:10.1002/2015JD023744). It is astonishing that this analysis yields no relationship between cloud albedo and AI, while Fig. 3 and 4 show a substantially positive relationship in the same region. Or do clouds in this

region usually have COD < 5 in the AATSR retrievals?

L23: “Independently derived” seems exaggerated. After all, as I understand, the retrieval algorithm is the same in both cases, as is the way to compute cloud albedo using the radiative transfer model?

P9 l5: Does not Fig. 2 suggest that swelling is negligible at scales > 15 km away from cloud edges?

L18: Which references used in assessment reports do the authors refer to? I’m not aware of many estimates that also include what is called here “extrinsic” forcing.

L26: Is this really the standard error, or not rather the standard deviation of the spatial distribution?

L32: This is only true for some GCMs.

P11, References General comment on references: The authors should consistently show or not show dois and URLs. Journal names should be abbreviated

P14, Caption Fig. 1: 512\times 100 km$ˆ2$

P15, Figure 2: It would be useful to show in addition the product of Ångström exponent and aerosol optical depth. Is AI actually approximately constant with distance from cloud?

P16, Caption Figure 3: It seems what is provided as “mean” is the global mean values? And “Standard deviation” the standard deviation of the spatial variability of the regression coefficients?

P20, Table 1: “CAPA_L2”: this seems to correspond to all aerosol retrievals, not just the green ones in Fig.1 “CAPA-L2_15km” I believe these are the green pixels aren’t they?

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