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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new method for deriving aerosol solar radiative forcing and its first application within MILAGRO/INTEX-B" *by* K. S. Schmidt et al.

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These comments were very constructive; and we are glad that we had such an interested and qualified reviewer. We appreciate both the encouragement and criticism which has helped identify inconsistencies that we had not been aware of. We are currently implementing the reviewer's suggested changes and improvements in the text of the revised manuscript. See below a point-by-point response:

(1) No action required.

(2) "This text should be re-written"

We agree, we will move the description of the cloud to the beginning of the paragraph. That will also help explain the grouping of the data in Figure 3 early on.

(3) "But the author goes on to state that the regression line I used to extrapolate the (net flux?) measurements to tau(0) and tau(max). I could never then figure out how these extrapolated measurements are used in the retrieval."

The wording in the manuscript is currently unclear, and we will add an explanation in the revised version along the following lines: Pairs of irradiances (downward / upward, above / below the layer), as well as the optical thickness are needed as input for the retrieval. The difficulty in spirals is that due to the uncertainty of the measurements, one cannot just pick an arbitrary point above and an arbitrary point below the layer while the aircraft is spiraling or profiling through the layer. However, the uncertainty can be much reduced when using data all along the spiral measurement, that is, plotting F down and F up as a function of optical thickness, as measured by AATS, and doing a linear correlation. The AATS measurements are used instead of the altitude as "vertical coordinate" because it is precisely the optical thickness (integrated extinction from the top of the atmosphere down to the current location of the aircraft within the layer) that modulates the irradiances. The irradiance pairs are then established on the regression line, at tau=0 (irradiances above the layer), and at tau=tau_max (below the layer). We realize that this section of the manuscript needs substantial improvement to make that clear.

"Also, how is tau(max) determined? Does the determination of tau(max) and tau(0) add some arbitrariness to the retrieval?"

tau_max denotes the optical thickness that AATS measures below the layer. That is indeed somewhat arbitrary because the layer might continue below the current flight altitude where AATS measurements are taken. Hence, if the aircraft descended further down, the irradiance pairs below the layer would indeed change if the AATS-derived optical thickness changed. However, in the retrieval, the optical thickness is one of the inputs (this needs to be added to the text!). The retrieval uses the *same* optical thickness that is used to derive the below-layer irradiance pairs in the extrapolation described above. From that, single scattering albedo, asymmetry parameter and surface albedo are derived, and forcing efficiencies are calculated. When using a different tau_max (and hence different below-layer irradiances), these quantities are not affected. The forcing itself, however, does change with changing tau because it is an extensive property, unlike the forcing efficiency, which is an intensive property. All of this will be made clearer in the manuscript, hopefully without getting too "wordy".

(4) "Is “alpha” the surface albedo or the albedo at the bottom of the aerosol layer? It is stated in step “2b” that F(down, measured) is the measured irradiance below the layer. Yet, it is implied immediately below in step “2c” that “alpha”, which depends on F(down, measured), is the surface albedo. This seeming contradiction is very, very confusing."

Alpha is indeed the surface albedo, not the albedo just below the layer. In step 0, F up / F dn (BOL) is only used to *initialize* the surface albedo in the model. To make that clear, we will put an index on alpha (as requested by the other reviewer), to denote the various stages of the iterations. Step 0 would then read alpha_0=F_up(bottom)/F_dn(bottom). In step 2c, the surface albedo from the previous iteration step is *adjusted*, according to the ratio between [F up (bottom, measured) / F dn (bottom,measured)]/ [F up (bottom, calculated) / F dn (bottom, calculated)]. In other word, it does not directly depend on F up (bottom, measured) / F dn (bottom,measured). alpha is adjusted until the ratio between modeled and measured albedo below the layer is close to unity. Hopefully the indices will make this clearer.

However, this comment brought up a concern that was better addressed in Wendisch et al. [2004] and Coddington et al. [2007]: The aerosol *below* the lower level is basically unknown in our method because the aircraft did not profile all the way to the ground. In Coddington et al. [2007], AERONET measurements are used to get the aerosol optical thickness all the way to the ground. In principle, this retrieval gets the correct surface albedo if the aerosol properties below the lower leg are known, or if no aerosol

layer was present below the aircraft. For the extremely polluted Mexico City area, that is almost never the case. So the parameter alpha should be called *effective surface albedo* that is equal to the actual surface albedo only if the aerosol optical properties are correctly represented in the retrieval algorithm. To retrieve single scattering albedo, asymmetry parameter and forcing efficiency, this is not required, and the assumption of an effective surface albedo is sufficient. We will point that out in the revised manuscript.

(5) "Maybe the word “optional” is not quite the right word to use in this circumstance? In any event, this confusion needs to be cleared up. Is it the case that one can either choose the rescaling factor, or the difference in “g” and “g(hat)”, as the success criterion?"

We used the consistency between g and g(hat) as success criterion, not the rescaling factor. In fact, we will omit "optional" altogether, as suggested, because we always had the rescaling switched on, and to avoid confusion. In most cases, the correction factor was close to unity (details in the manuscript). It could be switched off (we tried that), in which case we do not get the asymmetry parameter while we still obtain single scattering albedo.

(6) "Does the method described in section 2.4.2 differ if an aircraft spiral, or if TOL and BOL legs, are used? If so, this difference should be make clear."

No, once the irradiance pairs are established, there is no difference. We will state that in the revised manuscript.

(7) "Table 1: Given that the AERONET data finds COLUMNAR aerosol properties, how can the AERONET measurements be used to find BOL and TOL forcing? When using AERONET data, does the BOL mean surface and TOL, top of atmosphere?"

We only use the AERONET retrieved values for single scattering albedo, asymmetry parameter and optical thickness to *calculate* forcing efficiency. We distributed the AERONET optical thickness over the boundary layer and derived forcing efficiency (top of layer and bottom of layer). The "top" and "bottom" height were chosen at the same altitudes as in the nearest aircraft measurements. All of this will be better explained in the revised manuscript. We should stress that we are talking about forcing *efficiency*, i.e., the forcing normalized by optical thickness (i.e., it is an intensive property). Therefore, the value of the optical thickness or vertical distribution thereof does not have a substantial impact. However, we fully agree with the reviewer that the values of single scattering albedo and asymmetry parameter *is* representative for the entire column, whereas it is only representative for the aerosol layer in the case of the aircraft-based retrievals. This substantial difference is only alluded to in the current manuscript and will be stressed more in the revised version.

(8) "Table 1 and page 2745, second paragraph. What is meant by “derived directly from the measurements?” Is this the gradient method?"

Yes, we will change that to make it clearer.

(9) No action required.

(10) "Figure 8 is too small"

We fully agree. It is the ACPD page format that has led to this compression. In the ACP printed version, the Figure should spread across the entire page, and that will hopefully make it more legible. We will push for the full page format. If this cannot be achieved, we will split the Figure into two parts, as suggested by the reviewer (one ocean and one land case). We have had a similar experience with a previous paper by Russell et al. (2010). In its current state, the Figure is not acceptable.

(11) " The caption for Figure 8 states the figure plots “spectral forcing efficiencies”, yet the figure plots “relative spectral forcing efficiencies”."

We will correct that. It is *relative* spectral forcing efficiencies. (Technical comment #1): " symbols Z and Zo did not print" We will check that again in the pageproofs. In this version of the manuscript, there were no issues printing them.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2731, 2010.