Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-318-SC1, 2019

Atmospheric Measurement Techniques Discussions

Interactive comment on "A review and framework for the evaluation of pixel-level uncertainty estimates in satellite aerosol remote sensing" *by* Andrew M. Sayer et al.

Oleg Dubovik

oleg.dubovik@univ-lille.fr

Received and published: 5 October 2019

A comment on the discussion paper by M. Sayer et al., entitled: A review and framework for the evaluation of pixel-level uncertainty estimates in satellite aerosol remote sensing.

The paper suggests to present a comprehensive and rigorous approach for the evaluation of uncertainty of remote sensing retrieval. It is useful and timely research work. However, I have noticed a pronounced unjustifiable methodological bias in consideration of the current retrieval approaches and in acknowledging previous retrieval efforts.

The authors base their consideration on only two main equations that as they suggest come from general concept of optimal estimations by Rodgers (2000). I understand that several of co-authors originated from Oxford and do have their scientific background views on the Clive Rodgers remarkable retrieval development. Nonetheless, it is quite clear to me that the authors are well aware of the details of optimal estimation approach and could credit more precisely the approach for its merit as well as to be fair in crediting other works for their contribution to the formed approach. For example, the authors showed in Eq.(1) the cost function with three terms and introduced the equation using the following referencing: "While notation differs between authors (cf. Thomas et al., 2009; Dubovik et al., 2011; Govaerts and Luffarelli, 2018), following Rodgers (2000) a general form of the cost function J can be written:" This is quite misleading statement since for all who really read Rodgers (2000) it is obvious that Clive never considered more than two first terms, same as the paper Thomas et al., 2009. If I am mistaken my remark it would be nice if the authors pointed at such formula in the Rodgers (2000) textbook. The fact is this multi-term fitting concept comes from Dubovik et al. (2011) and earlier AERONET retrieval works by Dubovik and King (2000), Dubovik (2004), etc. Here this no difficulties to point out the equations analogous to Eq.(1). For example, Eq.(18a) in Dubovik et al. (2011), or Eq.(48) in Dubovik (2004). The paper by Govaerts and Luffarelli (2018) does contain such formulation but it was also adapted from Dubovik et al. (2011) while authors Govaerts and Luffarelli were not fairly generous to credit previous work either. (The fact was brought up to the attention of the authors and editor (A. Sayer) by the reviewers of the discussion paper, but this detail never was addressed.). This is pretty disappointing approach from the group of rather respected scientists.

Another critical aspect in the proposed methodology is the fact that the authors consider only random component of retrieval error and do not suggest any quantitative approach to access the effect of possible biases. This is very dangerous practice. For example, from the structure of Eq.(2) it is rather clear that by increasing weight of second and third terms by adding a priori constraints one can suppress the level of random

errors very strongly. That is justifiable if a priori constraints are adequate. However, if the false a priori information is inadequate (i.e. doesn't not correspond to the reality), the random errors would be suppressed also. However, in such situations, the solution would be strongly biased, and this would never appear as results of using Eq.(2). This fact is not captured neither by used equation nor by profound discussion.

Overall, the paper needs critical and honest revision by the authors before the publication.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-318, 2019.