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Atmospheric Measurement Techniques Discussions

Interactive comment on "Rapid methods for inversion of MAXDOAS elevation profiles to surface-associated box concentrations, visibility, and heights: application to analysis of Arctic BrO events" *by* D. Donohoue et al.

Anonymous Referee #1

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This paper presents and intercompares 3 methods used to initially 'in the field' analyse MAX-DOAS measurements. The paper is clearly structured and generally well written. This paper could possibly be a 'basic' paper that presents a widely used analysis strategy, useful for new groups starting out with MAX-DOAS measurements. While I didn't find anything new or exciting scientifically, I do see some merit in having a paper that compiles all this information in one place. I think however, the authors fail to do this adequately. The formulations were at times errornous and verbose. Such a paper would require to supply the readers with a simple to follow error analysis, which this paper fails to (correctly) and adequately do. For these reasons I don't recommend its publication in AMT without major and careful revisions.

Everywhere: The use of "Surface associated VCD – "SA-VCD"" is very confusing especially in formulae were the dash is easily confused as a minus symbol. In general equations should be constructed with symbols and not words or abbreviations for clarity. It would be better to just state that VCD here means the column heavily weighted towards the surface.

Page 4648, line 4: "RTMs require some assumptions" it is rather that RTMs have model parameters that need to be specified in the case of SZA and Az. angles, - in the case of albedo it is likely that the albedo is unknown and similarly the vertical profiles also (i.e. it is likely that a temperature/pressure and ozone profiles needed to be specified are unknown). But these are not assumptions in that they are parameters provided to the model and the sensitivity of the answer can be tested. Assumptions are more that refraction is not included or that the Earth is flat (which is assumed by Hönniger and Platt 2002 with the 'simple geometric approximation'). I am sure the SZA and Az are not assumed but rather known?

Page 4649, line 5: In this work, we developed... did the authors really develop these methods – I believe that the methods existed previously (eg Wagner 2007)? It would be more appropriate to state that the methods are intercompared by the authors.

Page 4649 an error analysis should be eluded to also.

Page 4653, Equation 1: (and elsewhere): SA-VCD needs to be altered – best convert all equations to symbols.

Page 4653, Equation 2: should read:

dAMF(a) = AMF(a)-AMF(90)=SCD(a)/VCD(a) – SCD(90)/VCD(90) approx equal dSCD(a)/VCD where VCD assumes VCD(a)=VCD(90) i.e. horizontally homogeneity (this is OK for SZAs less than 80o (where not photochemistry occurs) but not appropriate above these SZAs, a note to this effect should be made.

Page 4655, equation 3: something is wrong here. (A+B)/(C+D) is not equal to A/C + B/C (this is how VCD would often be calculated when each 'box' is independent) – here of course it is not an independent measure of VCD – so you wish really to present a mean VCD derived from the measurement set? In the text it is noted that this is the average dSCD divided by the average dAMF this is not the same as a mean VCD – I am not sure what the current formulation is useful for (or physically means?). Why don't you show a physically more meaningful average or mean VCD?

Page 4657, line 15 on: derivation of errors, the division of two numbers does not reduce the error! See for example error propagation from: http://www.google.com/url?sa=t&source=web&cd=4&ved=0CCcQFjAD&url=http%3A%2F%2Fwww.fas.harvard.edu%2F~ t44bwPSx2UCbA&cad=rjt

I can not stress enough how vital it is that the errors be expressed correctly, if as the authors hope that these 3 methods are used as the first (and maybe only) analysis of MAX-DOAS measurements. The authors here give errors only a very short discussion, but their methodology is flawed – it must be corrected and discussed in more detail.

Page 4659, equation 8 – equations should not have words in them use the greek letter chi instead for trace-gas concentrations.

Page 4659, use of L_HV: L_HV is derived from O4 concentrations, therefore the highest sensitivity to the surface, when the trace-gas is in a layer high in the BL, L_HV will be a poor estimate of the true radiative pathlength – a sentence here of caution to the use of L_HV would be valuable.

Page 4660, line 13 paragraph/sentence needs reworking very hard to read.

Page 4662, line 23 'based on mathematical standards' is far too strong given the quality of the mathematics and formulations in this work presented here. I would remove this

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entire sentence – containing 'the influence of the human observer . . . uniform manor' [sic]. The advantages of the methods presented in this work are that it is simple and therefore can be applied quickly and uniformly across an entire observational dataset to provide a 'first-look' to identify interesting features that then can be interpreted with more accurate and detailed physics.

Minor Page 4651, line 21: We have 'made' Page 4655, line 19: Remove 'of the instrument' Page 4657, line 8: based on the dSCD. . . Line 19: programs to calculate the. . . Page 4658, line 1 visibility decreases Page 4662, line 16 This approach can be used during

References: Wagner et al. Atmos. Chem. Phys., 7, 1809-1833, 2007, www.atmos-chem-phys.net/7/1809/2007/ doi:10.5194/acp-7-1809-2007

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 4645, 2010.