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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.

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We thank Anonymous Referee #2 for the constructive comments on this manuscript. Here we will discuss the general concerns with the paper and then specifically address points that that the reviewer raises with an asterix (*) followed by our reply. Please also see our replies to the other referee and the two comments.

The purpose of this paper was to outline a series of methods to convert slant column densities to vertical column densities or concentration measurements rapidly for use as a first-look in flight campaigns or as a validation for optimal inversion methods. To this end we attempted to present the bare-bones of the methods so that the paper would be of reasonable size, as well as, focus on the conversion of slant column densities to vertical column densities or trace gas concentrations. We therefore, limited the discussion on well developed aspects of MAX-DOAS and minimized the discussion of the literature. As is evident by the community response this approach was unsuccessful and resulted in an oversimplification of some aspects of this work, missing references, and incomplete error analysis and left to many questions for readers to evaluate the methods presented. The reviewers present a series of important points to be addressed in a revised version of this paper: specifically a disconnect from the current literature, a lack of a detailed description of the method used to obtain the slant column densities, and a need for a more complete error analysis. We will address these concerns in turn.

First, we agree with the reviewers and comments that this work builds extensively on previously publish work. In an effort to place the focus on methods to convert slant column densities to vertical column densities or concentrations we attempted to limit the literature review. In doing so the reviewers are correct in saying that we did not highlight how our work builds on each many important papers and missed some references specifically Sinreich, 2005, Volkamer, 2009, Stutz and Platt, 1996 and Platt and Stutz, 2008 (DOAS book). To this end the revised version of this paper will include these citations and a careful review of how this work builds on previous work. In the mean time we would direct readers to the following publications

1) For a general overview of the history and basic method of DOAS. (Platt and Stutz, 2008)

2) For a detailed discussion on the statistical errors associated with the DOAS technique (Stutz and Platt, 1996)

3) For details on the MAX-DOAS technique (Honninger, 2004a)

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4) For details on the use of box AMFs (Wagner, 2007

5) For details on the spectral fitting of BrO (Aliwell, 2002)

We also recognize that two of the method were presented in the literature prior to our work and that the use of the word develop with regard to all three methods was inaccurate. The elevated viewing VCD method is well established, presented in Honninger, 2002. Also at the time of publication we were not aware of the Volkamer et al. paper which presents the horizon viewing concentration estimation method and appreciate the comments which brought it to our attention. Finally, with respect to the box profile estimation method, the comments imply that this method has been presented before in the Sinreich et al. paper. While we acknowledge the oversight of the Sinreich et al. paper and agree it must be included for a complete discussion of methods of converting slant column densities to vertical column densities the two method are significantly different. The Sinreich et al method is a two step procedure where first the observed O4 AMF were compared with one of ten aerosol profiles to obtain the aerosol scenario which as the aerosol conditions to determine AMFs for NO2. The ratio of the measured dSCD at NO2 at two different elevation angles was then compared to the ratio of the calculated AMFs to determine the best NO2 box profile and thus derive layer height and concentration. However, these methods differ in three distinctive ways:

1) The method presented in this paper is a single step method in which the O4 and trace gas profiles are used to fit the two parameter while the Sinreich et al. paper first fit the aerosol loading and then the trace gas species.

2) This method fits in dSCD space while the Sinreich et al. paper fits ratio.

3) This method uses all measured slant column densities to derive the VCD (In this case 12 measurements) where as the Sinreich et al. paper uses all the O4 dSCDs but only 2 of the traces gas species dSCDs.

To understand and evaluate the difference, advantages and disadvantages of each method a revised version of this paper will include an intercomparison of the Sinreich box method and our box method.

The next concern was that many of the comment mention that we did not include information that the reader would like to evaluate the quality of the data, specifically the actual process of measurement and more details on the spectral fitting process. As mentioned before the focus of this paper was to be on the conversion of dSCD to VCDs, thus the paper would assume that a set of well defined dSCDs were obtained for a series of elevation angles. We did not discuss these procedures in this work for two reason. First, the measurement details will vary from study to study; details on the instrumentation and measurement sequence used in this work is presented in the reference paper, Carlson et al. Second, the methods for obtaining dSCD from spectra are well established. We referenced a well known standard Aliwell et al. as a baseline and only discussed the slight variations that we used in this work. We believed that inclusion of these well know or references method would make the paper unnecessarily verbose. However, we recognize that reader need more information to ensure that the retrieval were done properly these concerns will be addressed in a revised version of the paper.

Third, the reviewers discuss the need for a more complete error analysis. While we tried to present a detailed analysis of the errors the reviewer bring up a series of legitimate concerns. First, the reporting of the one sigma error as the error associated with the slant column has been shown to underestimate the error associated with the calculation of a slant column density due to the error associated with the shift and stretch parameter. In a revised verison of this paper, we will perform the error analysis discussed in Stutz and Platt. However, some effort was already made to ensure that the shift and stretch parameter were minimized (average of the absolute value of the shift and stretch respectively for the LZ fitting scenario; (4.11 $\pm$ 0.61)E-4 and (1.9 $\pm$ 1.1)E-5. Additionally, concern over the conversion of the dSCD error into a VCD error was raised. We recognized that by the method presented in this paper we do not account to the error in the AMF used to convert the SCD to a VCD; however, we wish to highlight the fact that the goal was not to reduce the error but to convert the error from dSCD error to a VCD error. In the revised version of the paper we include the error associated with the AMF in this conversion.

Finally, we would like to address some specific comments made by reviewer

* The reviewer expresses concern of the use of the term ‘visibility’ in the title and text.

We agree with the reviewer that the term visibility in the title is somewhat misleading, while the text refers to a visibility parameter and we did not mean to imply a true visibility calculation the title does infer that and will be changed.

* The reviewer noted that the use of the term surface associated VCD was somewhat confusing.

The use of the term SA-VCD we carefully selected as to distinguish the SA-VCD reported here from a true VCD as it is not a true VCD. Therefore, we do not think it appropriate to report it as a VCD but rather a revised version of the paper will include a more detailed discussion on the SA-VCD and revised mathematical symbols to avoid confusion with subtraction symbol.

* The review asks: What is the reason that LZ-MAXDOAS should not be useful to provide information on daily or seasonal variations in the trace gas abundances? Such a general claim appears to be unsupported.

We will reword this section to clarify. The LZ-MAXDOAS method is useful to provide information on daily and seasonal variations in trace gas abundances in the lower troposophere. However, by using the reference spectrum at the same time as the low elevation observation spectrum, the LZ method reduces the dependence on stratospheric (and upper tropospheric) daily and seasonal variations. Using the daily zenith reference, or a single campaign zenith reference allows one to observe these longer term stratospheric variations, but also can cause increased sensitivity to long-term in-

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strument drifts.

* The reviewer stated that the two statements appear to be in contradiction: ‘the O4 concentration is independent of the height of the boundary layer’ and it ‘exponentially decreases with height’.

This section need clarification as these statements are not in contradiction. The O4 vertical profile decreases exponentially with height as it is controlled by pressure temperature and O2 concentration. This means that the location of the boundary layer height is not a variable that effects the vertical profile of O4. This point will be clarified in the revised manuscript.

* The reviewer noted that a discussion of how cloud types were classified was missing.

A discussion of the cloud type was included in the reply to the comment by X. Li we did not provide a detailed discussion of cloud type as the goal was to roughly describe the degree of absence from UV light scattering from excellent to poor.

The reviewer mentions a number of other specific points that will be corrected in the revised version. We thank the referee for these comments.

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

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Interactive Comment