	Anonymous Referee #1 Received and published: 7 March 2013 This excellent manuscript describes an important new algorithm for the separation of stratospheric and tropospheric NO2. The quality of this SP2 algorithm addresses several issues with the earlier SP1 algorithm. The reduction of algorithmic dependence on a priori information and on assumptions such as wave-2 is commendable. The manuscript is well written with a thorough introduction. I recommend publication after considering the questions and suggestions below.	
1.01	How is the tropopause defined (thermal or dynamical) in the GMI CTM when separating the NO2 subcolumns of the stratosphere and troposphere as used in the air mass factor calculation? How would the alternative definition affect the results?	We have now included the definition of the tropopause in the CTM description near the end of section 2.2. Alternative definitions of the tropopause pressure were examined and found not to differ appreciably from the one currently used. All were well below the stratospheric peak in a region where the NO_2 concentrations are near a minimum. The precise definition primarily affects the a priori tropospheric background subtracted from Vinit, although this effect is very small. The effect on the air mass factors was found to be negligible.
1.02	How are stratospheric aerosols treated in the stratospheric air mass factor calculation?	These are neglected in the Astrat calculation as is now stated in the text. Both stratospheric and tropospheric aerosols may be considered in future versions of the algorithm, although we suspect the effect of stratospheric aerosols would be small.
1.03	In Section 2.2 it appears that the monthly NO2 profiles change abruptly with changes in the calendar month. A 30-day running mean NO2 profile would eliminate those sharp transitions.	This is a good suggestion. We will consider such a modification for a update of SP2 and have mentioned it now at the end of section 2.2.
1.04	Would it be useful to refine the algorithm described in section 2.4? The algorithm masks regions where tropospheric contamination (based on modeled NO2 columns) exceeds a threshold, and then eliminates hot spots. How about (partially) replacing the modeled NO2 columns with an average of the OMI tropospheric NO2 product produced from recent observations? That could allow you to reduce the dependence on the modeled NO2 column (which has errors), and possibly to reduce the need for the subsequent hot spot detection.	Using OMI SP2 tropospheric column climatology in place of the CTM climatology would have little effect on the mean background troposphere in unpolluted areas. However, it would help capture persistent features at smaller resolution than that of the CTM. This could lead to more masking in some spots and less in others, and could also reduce the need for hot-spot detection. We will definitely consider this idea in algorithm updates.

1.05	Why is the hot spot detection done at 1 degree resolution? Wouldn't finer resolution be more effective?	Hot-spot detection on the (large-scale) 1 x 1 deg^2 grid is done for computational convenience. Doing it on an OMI-pixel basis seems like an excellent idea and will be considered in the next update of the algorithm.
1.06	The error formulation is well presented, but it was surprising to see little discussion of the errors in air mass factors developed at the end of section 3.1. It would be instructive to show the air mass factor uncertainty as a function of cloud fraction. Or at least consider including a table stating the expected errors for common choices of cloud fraction.	We now have added a figure (Fig. 4) that summarizes the tropospheric vertical column uncertainty as a function of cloud radiance fraction.
1.07	The error discussion says little about the a priori NO2 profile. What are the expected implications of unresolved horizontal variation in NO2 sub-columns in the GMI CTM at its coarse resolution of 2x2.5 degrees?	Yes, the horizontal variability of the actual NO_2 sub-columns within the model grid cells is an important effect that has been neglected in the present version of the algorithm. This issue is discussed in detail by Heckel et al. (2011) and Lamsal et al. (2013) and we have now added mention these references in section 3.1.
1.08	Specific: L9, p1373, the resolution should be stated for geographically gridded	We now state the resolution to be 2° latitude × 2.5° longitude
1.09	P1375, is this interpolation done on the stratospheric vertical column? Interpolating the slant column would introduce errors due to spatial variation in geophysical fields (such as clouds) that affect the air mass factor.	Yes the interpolations are performed on the vertical column fields. We have added words to the text to clarify this.