Review #2.

We thank the referee for the thoughtful comments that helped us to improve the manuscript. Please find our responses to the referee's comments below.

Referee: Comparisons were provided versus SBUV/2, sondes, and Aura MLS profiles. Total column comparisons were shown versus OMI. But why not also show more of the comparisons versus SBUV/2 columns? This is only mentioned briefly before Section 4. Is this because a separate set of studies are forthcoming with more details, or some other reason? Some clarification would be useful, even if a lot more details regarding such comparisons are not provided.
Authors: Done. We added two tables that summarize the results of our comparisons. Table 2 shows total ozone biases and standard deviations for each 5-degree latitude bin in the South Pole region (55ºS–80ºS) for the following pairs of instruments: OMPS NP vs. OMPS TC, OMPS NP vs. SBUV (NOAA 18 and NOAA 19), and OMPS TC vs OMI.

Referee: Figure 2 shows that some variability is clearly seen from both satellite profiler data (OMPS LP and Aura MLS) near 17.5 km altitude; the sondes do not appear to capture this. Is there a possible reason for such a difference (where it looks like the sonde data do not match up with the satellites)? Apart from sampling issues, it is hard to see what might have caused this specific difference.
Authors: Fixed. We thank referee for drawing our attention to sonde/satellites disagreement. The positive biases below 22 km (including the peak around 17.5 km) are artifacts of the method that we used to calculate biases and express them in % units. Our analysis of the data in absolute values did not reveal significant differences between satellite and sonde measurements (see Figure 1c). To calculate biases we used the following expression: $(k) = 100\% * \overline{(L(k) - S(k)) \big/ S(k)}$, where k is altitude, L is limb satellite measurements

(OMPS LP or MLS), and S is sonde observations. Sondes report ozone profiles with a high vertical resolution (~0.1 km). In a few cases, at altitudes below 22 km sonde profiles show local minimums in vertical ozone distribution with the ozone concentrations near zero. Both limb satellite instruments, OMPS LP and MLS, have coarser vertical resolutions (about 1.5–3 km), and do not reproduce the fine vertical structures that sondes measure. In these few cases the percent difference, calculated using the above expression, can be very large (>100%), and few large positive values “shift” the mean differences to the positive side. To avoid this complication we use the mean sonde values as a denominator in the

expression: $b(k) = 100\% * \overline{(L(k) - S(k)) \big/ \overline{S(k)}}$. We have updated Figures 1 and 2 and the

related discussions in the text.

Referee: There is almost sufficient material for this “first look” paper to not mention a lot more details (see above). However, some mention of possible reasons for some differences should probably be provided. In particular, Figure 3 shows some interesting patterns in the 10-15% levels of disagreement between OMPS and OMI columns (panel c). Are there any likely or potential explanations at this stage?

Authors: Fixed. We added the following brief explanation: “Figure 3 shows the monthly mean October 2012 total ozone amounts for OMI and OMPS TC-NM, along with the differences between them. The bias for measurements poleward of 55ºS are relatively small, ranging from -3.6% to 4.9% (zonal mean biases are shown in Table 2). OMPS is generally higher than OMI everywhere due to different ozone cross sections used in the OMPS and OMI algorithms (see Sect. 2). But near and within the ozone hole region OMPS TC-NM is lower in comparison to OMI. A preliminary analysis shows that OMPS overestimates reflectivity over ice and snow surfaces at large viewing angles, and as a result underestimates ozone column. Changes to the calibration of the OMPS TC-NM sensor will resolve this issue in the next released version of the data.”

Referee: There is some discussion relating to Fig. 4 that I find too speculative and marginal – see line 26 at bottom of page 7. The authors state that there may be a slight downward trend over the 1995-2012 time period for the ozone hole size (area with column amounts less than 220 DU). The words used by the authors are “visually suggesting” (in reference to a potential decrease in ozone hole area). But this is not clearly obvious to this reviewer, and it would only be a rigorous conclusion if error bars were taken into account after regression analyses of some sort; the peer-reviewed work by Salby et al. seems to have followed this more rigorous path. But “visually”, even the last 10 years do not “show” something that could be called an obvious trend. However, I would say that a flattening after 1995 would be a clear enough statement for a majority of readers, even if this is also qualitative in nature.

Authors: Text modified. Our comment here was based upon a linear trend analysis of the area data. We computed a -1.1 x 10^6 km^2/decade downward trend of the ozone hole area in the 1995–2012 period. This trend is NOT significantly different from zero when compared to the 95% confidence limits on a Student’s t-test of the trend. Hence, strictly speaking, the area is drifting downward (as shown by the trend and a visual inspection of the observations), but this cannot be distinguished from a zero trend (flattening) of the area. The interannual variability of the observations is clearly greater than a -1.1 x 10^6 km^2/decade downward drift. We have re-phrased our statement and replaced "Second, the area shows a slight downward trend over the 1995–2012 period, suggesting that the Antarctic ozone hole may be recovering." with

"Second, the area of the ozone hole has stabilized and stopped growing since the mid-1990s, even though the level of active chlorine and bromine continued to grow until around 2000 (Newman et al, 2007). By the early 1990s active chlorine and bromine eventually reached the levels necessary to destroy 100% of the ozone in the lower Antarctic stratosphere. Thus further increase of chlorine and bromine levels could not make the ozone hole any larger. The Newman et al. (2006) parametric model has been updated using additional years of OMI and OMPS data (2006–2012) and recent observations of chlorine and bromine species in the estimate of equivalent effective stratospheric chlorine (EESC) (results are not shown here). The parametric model shows that after reaching saturation in the mid-1990s, the year-to-year ozone hole area variability has been mostly controlled by stratospheric temperature variability in the polar cortex collar region (specifically 50 hPa, 60°–75°S, and 21–30 September), in agreement with Newman et al. (2006). This analysis did not reveal any sign of the ozone recovery over Antarctica. The small downward trend of the hole's area over the 1995–2012 period is not statistically distinguishable from a zero trend. The model predicts that the ozone recovery should become detectable (or statistically significant) sometime between 2015 and 2033, most likely in 2025."

Referee: The potential implications of Fig. 5 also seem to be significantly exaggerated. Page 9 (lines 20-28) mentions that the rate of increase in ozone hole area is similar ("at the same rate") as in previous years. There is a wide range of curves implied by the shaded regions and there is a very qualitative nature to such a statement. Given that the chlorine burden decreases at roughly 1%/year, this is not a statement that can be made quantitatively, given the presentation that is currently provided, certainly. I would expect that the implications regarding ODS amounts should be removed from such a discussion if nothing more quantitative can be provided as a response (although a mention of Salby et al. could be used). The portion of the discussion dealing with the breakup of the ozone hole and dynamical factors is not well developed either in this brief manuscript, but it is more believable. But any relationship versus changes in chlorine or bromine should be a subject of detailed calculations and a more rigorous defense of these types of statements. Without this, I would have to view this as wishful thinking or a fairly crude overstatement rather than a scientifically robust discussion. At least, the authors did not include such a discussion or "conclusion" in the Abstract, but the current summary section contains this sort of overstatement, and should not do so, in my view. The peer-reviewed work by Salby et al. uses satellite data over Antarctica and appears to be more robust than (or as robust as) the use of sonde data (in reference to the Hassler et al. work mentioned in this manuscript). Further detailed quantitative discussions of these issues are undoubtedly warranted, but simple "visual" qualitative statements cannot be considered to be sufficient.

Authors: We added more details and reference to support our statement:

"Some have implied that the smaller ozone hole area results from a decrease in chlorine in the stratosphere (Salby et al., 2012). Inspection of Fig. 5 shows that the 2012 ozone hole develops at approximately the average rate during the August and early-September period. This is the period when chlorine chemical effects on the ozone hole area are most prominent (Newman et al., 2004; Kawa et al., 2009). These results indicate that the level of chlorine and bromine in the lower Antarctic stratosphere is high enough to cause severe ozone depletion. Current EESC levels in the Antarctic stratosphere are declining by about 30 ppt y^{-1} (ppt = 10^{-12} mol/mol), less than 1% per year with respect to the Antarctic EESC peak of about 4040 ppt occurring around 2000 (Newman et al, 2007). Strahan et al. (2014) show that the level of chlorine can significantly vary (up to ±5%) from year-to-year due to interannual changes in the transport during austral fall and winter. Currently, the declines in chlorine levels are far too small to show an ozone hole recovery in comparison to year-to-year variability. Both the area and minimum values indicate a more rapid than normal ozone hole recovery in the late-September through November period. The ozone hole is strongly controlled by the wave dynamics and the direct influence of the waves on temperature (Newman et al., 2001; Weber et al., 2003: Newman et al., 2004). Strong wave events revealed in both the ozone distributions (wave-2 split in Fig. 7), and dynamical proxies such as the Eliassen-Palm flux (not shown) indicate that this more rapid recovery is dynamically driven. The smaller area in 2012 resulted from the warmer polar lower stratospheric temperatures in September and not chlorine decreases."

Authors: Minor comments and suggestions listed by the referee have been addressed.