We would like to thank the reviewer for his/her comments. The original review is shown below in blue with our comments in green.

This is a well-written and interesting paper looking at merging the data for tropospheric ozone from the TES and IASI instruments to provide a long-term time series. The selection of time series that have been derived show some interesting features, and such consistently merged time series will clearly be useful in studying tropospheric ozone trends. There are a few points of the paper that could have been discussed in more detail, and I think would benefit from further elaboration as detailed below:

Section 3.1, p.31030, lines 26-28: There is a statement that the 'average results did not change' with latitude or season. It would be useful to quantify this agreement or else show a plot showing evidence for this.
We acknowledge that the sentence is confusing in this context and we added instead more explanation further down in the paragraph when discussing specifically Fig 1D: *Despite the fact that the ozone profiles themselves vary significantly with latitude and season owing to variations in tropopause altitude (Fig 1B), the differences between IASI and TES ozone (Fig 1D) are relatively consistent in shape and magnitude across seasons and latitudes, with the IASI-TES differences showing a standard deviation of only ~20 % in the upper troposphere for the whole dataset, compared to the extremely large standard deviations in the ozone upper tropospheric volume mixing ratio.*

Section 3.2 and Figure 2:
a) This mentions in separate places that the differences between IASI and TES seem independent of the actual ozone amount, and a little later that very large differences coincide with large IASI ozone values, which seems contradictory. The figure is rather crowded, so it is hard to see if there is a dependence on ozone – it would be good to see this shown in a different way too for clarity.
We can appreciate that the discussion of Fig. 2, as worded, could appear contradictory. It is not that easy to separate the effects since the sensitivity of a measurement increases with the amount of ozone present. Hence we chose to combine the 3 parameters in one figure. However, using the size of the markers to represent the IASI ozone amount was an unfortunate choice since this draws the eye to the outliers. We changed the figure to have the ozone amount in colour-coded, same-size markers. We also removed that specific contradictory sentence to avoid confusion.

b) p.31032, line 14-16. It’s stated that the width of the frequency distribution is determined by the precision of the measurements and the collocation error. Have you looked at whether this matches what you would expect theoretically?
The FWHM in Fig 3 is 17.6 ppb. The precision for the IASI-TOE retrieval was estimated to be better than 20%. Those 20% were calculated from comparison with ozone sondes with a coincidence criterion of also 55 km (Oetjen et al., 2014). Hence a possible spatial colocation error is included in this estimate. This translates into 10.4 ppb for a mean IASI-TOE ozone of 51.9 ppb for the IASI precision plus spatial colocation. The TES precision of 15% translates to 8.3 ppb for 55.4 ppb mean ozone. To estimate the temporal collocation error we compared model fields (GEOS-Chem, version 10.1 [Bey et al., 2001; Eastham et al., 2014]) for the dates of the 4 global surveys for the overpass times of IASI and TES and calculated the standard deviation of the ozone difference which is 1.7 ppb. Adding the IASI combined precision and spatial colocation estimate, the TES precision and the temporal colocation estimate in quadrature gives ~14 ppb, which is slightly smaller than, but not dramatically different from the FWHM in Fig 3. We added this to the text.

c) p.31032, Line 16-17: Are the outlying points all associated with realistic ozone retrievals, or could they also be symptoms of a problem with those retrieval points (e.g cloud contamination?)
As the reviewer suggested in a) we removed this specific sentence from the manuscript (s.a.). However, the high values are all within the range of what one would expect from stratospheric intrusions or biomass burning plumes. The cloud filter with a cloud fraction of 6% or less for IASI retrievals is already very strict.

Section 4: The discontinuity in South East Asia is potentially very interesting. Have you investigated whether this is definitely a real affect in tropospheric ozone or if there could be any other possible reasons for this, e.g. instrumental factors, differences in inputs to the algorithm? Is this change seen in any of the input datasets for IASI? It is unfortunate that this seems to coincide exactly with the end of TES - are there any other data sources this could be validated against? You mentioned that there will be a more detailed paper on this, but it would still be useful to discuss here how confident you are that this is a real effect.
We added a figure to the manuscript showing a similar drop in ozone over Hilo, Hawaii. For that we averaged ozone sonde data over the same pressure range as TES and IASI (we did not apply the averaging kernels to the data).

Minor points:

Section 2, p31029: Line 16: 'For TES we use the publically available v05 level 2 Lite data'. Please clarify if this is the ozone dataset you are using, or just what you are using as input to the algorithm – it's a bit ambiguous at the moment.

The TES ozone results shown here are from the v05 Level 2 Lite data. We have updated the text to clarify.

Section 3.3, p 31034, line 2: 'We are aiming to sample' – this is in the future tense, whereas everything else talks about what has been done?

We changed this to present tense.

Technical corrections:

Section 1, p31027, line 6: 'De Smedt et al, 2009' – in the references there is given as 2010.

Corrected

Section 1, p31028, line 10: Metop was launched in 2006, not 2007 as stated.

Corrected