Response to Reviewer No. 1

The reviewer's comments are in italics.

Summary:

This paper proposed an approach to merge the global mean SSU and AMSU observations using MIPAS as a bridge, accounting for residual biases between the two observations which were left over from directly merging the weighting functions of the two observations. The merging results were reasonably well, given by near zero difference time series between the SSU observations and its equivalent merged AMSU observations during their entire overlapping period from 2001-2006. The paper then compared the merged SSU/AMSU observations with MLS observations and chemistry-climate model simulations. Favorable results were obtained for both of the comparisons. The approach is potentially useful for other nadir-sounding observations even when no overlaps exist between them as long as a bridge exists from the limb-soundings to link the nadir-sounding observations to be merged. Although the merging approach appeared to be useful and the results were interesting, I have a major concern on the conclusion and some specific comments relate to it. I recommend publishing the paper only after these issues were suitably addressed. These comments are listed below.

Major Comments:

In the abstract, the authors stated that the 'The extended SSU data set also compares well with chemistry-climate model simulations over its entire record' and Figure 7 gave the comparison results. However, the chemistry climate model simulations in Figure 7 extended only from 1979 to 2006 where the SSU-only observations were available for the entire period. It is desirable to understand how the SSU-only observations compare with climate model simulations. In my view, this conclusion has nothing to do with the merging and extension. Good agreement between the merged SSU/AMSU observations and chemistry climate model simulations should be due to good agreement between the NOAA SSU Version 2 data and model simulations. This is because only if the SSU observations agree well with model simulations, good merging between SSU and AMSU could also agree with model simulations. Otherwise, good agreement between the merged observations and the model simulations cannot be achieved unless the merging is poor. To my knowledge, independent studies by other groups (not published yet) also suggested that the NOAA Version 2 SSU data agreed very well with model simulations. It appears that the conclusion of this paper claims a credit or achievement that does not belong to the proposed merging approach. This should be addressed in the revised manuscript.

The reviewer seems to be saying that in the abstract we suggested that the good agreement between the extended SSU data set and the models was somehow due to our merging procedure. Rather we were simply describing the characteristics of the extended SSU data set, and not using the comparison to models as a validation of our method. Our meaning was clearly stated in the body of the paper: On lines 5 and 6 of page 10102 of

the Discussion paper we clearly stated that the CCMVal2 models cannot be used to compare to the extended SSU results post 2006, since the CCMVal2 simulations end in 2006. In the Conclusions (lines 10-13 of p. 10106 of the Discussion paper) we stated that the good agreement between SSU and the models is because we were using the Version 2 data, not Version 1 in Thompson et al. (2012).

In the Conclusions we also discussed the agreement between our extended SSU data and the CMIP5 model results shown in Thompson et al. Unfortunately that comparison had to be made by visually comparing Fig. 7 from our paper to Fig. 1 of theirs, which we now realize was asking too much of the reader. We have therefore included the CMIP5 data in our paper, the results of which are now shown in Figs. 7 and 10. This allows us to discuss the comparison with models over the extended period 1980-2012 in the revised abstract, which should help alleviate any misunderstanding.

To further avoid any misunderstanding, we have also added a statement in the third last paragraph of the Introduction that "the merged data set can only be as good as the component data sets going in, and relies on the extensive efforts spent on homogenizing the SSU and AMSU data records themselves."

Specific comments:

1. Page 4, lines 10-15: It should be noted that the NOAA Version 2 SSU data and its paper by Zou et al. (2014) intended to address the differences between climate model simulations and observations found in Thompson et al. (2012) paper. The NOAA SSU Version 2 data had big improvement over its Version 1 data and how this improvement resolves the mystery of the stratospheric temperature trends need to be addressed here since, as mentioned in my major comment, this is a key issue that will affect the SSU/AMSU merging and its comparisons with climate model simulations.

In the revised manuscript we have added a paragraph in the Introduction in which we discuss the Zou et al. (2012) results and make clear the reason for the development of NOAA SSU Version 2.

2. Page 5, lines 20-25: Again, the NOAA SSU V2 data are critical for interpretation of the merged SSU/AMSU data and its comparisons with climate model simulations. More information should be provided here on this dataset such as its improvement over V1 and spatial and temporal resolutions, etc.

In Section 2.1 we have added a brief discussion of the main differences between SSU V2 and V1. There we also now explain that the motivation behind the development of V2 was to address the reasons for the mismatch between V1 and SSU Met Office and between V1 and models discussed by Thompson et al (2012).

3. Page 6, line 5: I noticed that most of the results presented later on were from 2001. The authors should clarify here how starting dates of different channels affect the merging.

Without AMSU channel 14 (which is not available until 2001), the approximation of SSU

channel 3 is hopelessly bad, and even that of channel 2 is compromised. Thus we can only consider merging from 2001 onwards when channel 14 is available. In any case the c_n's are computed with the use of MIPAS data, which starts only in 2002.

4. Page 8, lines 1-5: ERA-I and its adjustment were not fully justified for long-term trend investigations, especially for SSU channel 3. I don't see how including this dataset will help justify the conclusions of this study. I recommend removing ERA-I to focus on the SSU/AMSU merging and its related analysis in this paper.

We agree and have therefore removed the section on ERA-I as well as the SSU-weighted ERA-I results shown in Fig. 7.

5. Page 18, lines 1-5: As mentioned in my major comments, the good agreement in Fig. 7 has nothing to do with the SSU/AMSU merging. I recommend to only including the SSU V2 data in this comparison (Fig. 7), while adding another plot to compare the merged SSU/AMSU data with CMIP5 data where one can extend the time series to 2011 or present.

Figure 7 now shows only the CMIP5 models and the extended SSU data set for the 1979 to 2012 period.

6. Page 18, lines 5-15: See my comment #5. Since the authors already did this, I recommend to including the plot showing comparisons between CMIP5 and the merged SSU/AMSU data from 1979-2011.

Done (see above). We have also revised Fig. 10, which now shows extended SSU and CMIP5 trends for 1980-2012.