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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A method for merging nadir-sounding climate records, with an application to the global-mean stratospheric temperature data sets from SSU and AMSU" *by* C. McLandress et al.

Anonymous Referee #2

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Summary:

McLandress et al develop a method of intercalibrating distinct remote sensing measurements that have different vertical sampling. The method assumes a constant bias between different instrument measurements as well as a constant bias to account for errors in applying the weighting function to the data. The method is developed for generic datasets (though clearly with SSU/AMSU intercalibration in mind) and allows for the use of a transfer function with a third independent dataset to link two datasets

(e.g. linking AMSU to SSU via MIPAS).This transfer function method is applicable in the case that the two datasets to be intercalibrated do not overlap in time.

McLandress et al are able to continue SSU measurements that end in 2006 with AMSU measurements that begin in 2001 and continue through the present. Even though the two datasets have significant overlap, the authors use MIPAS data, which allows for a quantification of the biases incurred from the application and fitting of the weighting functions. The authors demonstrate that it can be misleading to compare vertical profiles with deep atmospheric layer measurements, that SSU/AMSU/MLS data are in good agreement during overlap periods, decadal variability is strongly modulated by anthropogenic and natural forcing, and that models and SSU observations largely agree.

This work is valuable in that it extends the observational record to allow comparison with model simulations of the stratosphere, extends a long-term stratospheric climate dataset to the present, and develops a simple framework for connecting various remote sensing measurements that have different vertical sampling.

Despite these strengths, the model-observational comparison is not sufficiently novel, the manuscript should provide more background on the datasets in use, the use of a third dataset (MIPAS) to calibrate SSU with AMSU is not sufficiently motivated since these datasets have significant overlap and because the MIPAS data is unstable, and there is no uncertainty analysis for the derived constant biases and its effect on the resulting time series.

Recommendation: I recommend that this work be considered for publication after the following major and specific comments are addressed.

Major Comments:

The abstract insinuates that this extended-SSU time series compares well with chemistry climate models, but the extended time series has nothing to do with this agreement

(since SSU is phased out after the time periods mentioned). Isn't this finding largely a result of the updated (e.g. v2.0) NOAA STAR data? Incorporating the few CMIP5 models that have data for MSU 4, SSU 1 and 2 could make this analysis more valuable (this would be historical + RCP scenario).

There should be more background provided on these datasets, especially the SSU. This includes: 1) a basic summary of the corrections made to the data (e.g. limb correction); 2) the type of measurement (e.g. microwave); 3) spatial coverage; 4) brief description of why NOAA SSU v1.0 and v2.0 are so different, since the agreement of SSU v2.0 and CCMVal2 is a major finding in this work; 5) The Wang and Zou, 2014 paper is for NOAA STAR AMSU version 2.0. Can this data be updated to the most recent version?

The text does not explain the time averaging periods in Eqs. 6 – 9. Is a common reference period used? If not, isn't the large drifts in MIPAS an issue for this intercalibation? If so, why bother with MIPAS? Further, should we trust that these two versions of MIPAS data (presumably on two different platforms) are absolutely consistent? In general, cn is not important, but the stability of the constant offset is very important. The agreement between MLS and AMSU is impressive, if the authors desire a proof-of-concept, why not use MLS data?

The inclusion of MIPAS detracts from what I see as the main value in this work (extending SSU). MIPAS is valuable if SSU and AMSU don't overlap (but they do overlap) and/or if it is stable (it isn't). The transfer function framework is theoretically valuable, but a convincing case is not made for it, and it makes the manuscript much less clean. Couldn't the weighting function error budget be computed using reanalysis (or some reference atmosphere)? It takes some effort to work through this framework, even though the idea is fairly intuitive. So I suggest the authors simply remove the transfer function framework, move it to an appendix, or potentially relabel A, B, C as AMSU, SSU, MIPAS; the analysis can be done merging AMSU directly to SSU.

The offsets in merging different datasets can have a large effect on trends. Uncertainty analysis should be incorporated into the offsets and this should be propagated into the derived trends/timeseries.

Specific Comments:

10087 13-15: Provide a reference for the discussion of GPS occultation discussion

10087 20: The MSU Channel 4 record is continued by AMSU channel 9 (e.g. Christy et al. 2003, Mears et al. 2009)

10091 22 – 25: More detail about the exclusion criteria is needed. What was the top boundary condition required and how many models were eliminated because of this? Ideally, you could just ignore/mask temperatures if they are extrapolated into the Antarctic land mass, but you should at least tell the reader how many models were thrown out because of this? What do you mean the models were outliers, how many models does this include, and why should they be excluded?

10092 15 – 16: Why did you use near-global? AMSU measurements go to $\sim$82 N/S, does another dataset only go to 75 degrees N/S?

10093 7: Does choosing a single H for the globe affect the results? For instance, if you chose another reasonable H-value (e.g. 7.5) does the resulting weighted time series have a different trend or variability that could effect the results? Would H(latitude, season) make this more accurate? The concern is that the time series may be sensitive to the assumption of a global mean H for the weighting function. This modulates the weighting function height and the authors show that trends vary quite strongly as a function of height. The global mean weighting function applied to near-global data is borderline overly simplistic since the spatial structure may be important to the results and uncertainties in this work.

10094 4 – 6: I do not agree that the agreement between MIPAS and SSU is uniformly "quite good." They have a large offset, relatively large residuals, and a large inter-

dataset trend.

Eq. 10: Is there a sign error in this equation?

Section 3.1.3: MLS has excellent agreement with AMSU and SSU, did you consider using it as a transfer function? It appears to have a little more than a year of overlap with SSU and several years of overlap with AMSU and apparently has fewer known biases than MIPAS.

10098 21: The individual biases are "less than 1.2 K," less than "$\sim$1 K" is misleading.

10101 6-7: The trend profile should be described without using "(tilted) axes."

10101 19 – 21: I'm not sure how "given the similarity of the extended SSU and AMSU trends" tells us anything about whether the strong vertical structure of the MLS trend profile – can you explain?

10101 23: Why did you calculate anomalies with respect to 1980 – 1985? This makes the later comparison (10102 21 – 25) more difficult to see.

10101 25 – 29: Are the weighting function normalized if the model top is below the top of the weighting function?

10102: Why isn't ERA-I extended through to the end of extended SSU? Can you say something about whether ERA-I should be considered a reasonable comparison dataset (my default assumption is that reanalysis is generally not trustworthy for long-term time series)?

10103 20 – 24: Please list the major ideas for why there may be discrepancies in lower stratospheric cooling. Some relevant papers may include, Lu et al., 2014, Solomon et al., 2011, Shindell et al, 2013, Eyring et al. 2013, Hassler et al. (2013), and others.

10104 17 – 18: It was not immediately obvious what you meant by the the correlation between channels is geophysical. Something like "interannual temperature variability is coherent with altitude" or something similar might be more intuitive.

Discussion: Given the poor weighting function representation of SSU3 using AMSU and because trends vary as a function of height, some caution should be suggested regarding the results for SSU 3.

Figure 9: Do the model trends look anything like this? It would be useful to try to shed light on this weird zig-zag structure being an observational issue or model issue.

Figure 10: Use 95% confidence intervals.

References:

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 10085, 2015.