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Interactive comment

Interactive comment on "Estimating Uncertainties in the SBUV Version 8.6 Merged Profile Ozone Dataset" *by* Stacey M. Frith et al.

Anonymous Referee #2

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This manuscript provides a timely, and important update of the SBUV MOD merged dataset, and a natural follow on from Frith et al., 2014's work on total column ozone that use the same instruments and a similar uncertainty estimate approach. The manuscript is well written, with clearly presented analysis, and careful consideration of uncertainties. I commend the authors for their effort in these regards. Given that the main problems in the trend estimates are due to how artefacts (drifts and offsets) are accounted for in the merging procedure, and this is the most prohibitive component in estimating decadal trends in merged records (e.g. Harris et al., 2015), I have put forward some suggestions below of things authors should perhaps consider to further improve/reduce-uncertainties on their estimates. Such considerations may point towards better approaches to merging the data in future iterations.

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Discussion paper

Following consideration of the points below, I recommend publication.

Main comments: P3, L27-29: Does a period of 24 months, instead of 12, when volcanic aerosols likely persisted (at least in the lower stratosphere), make a difference to your uncertainty estimates? Have you performed any sensitivity tests, or does it simply make very little difference once you get beyond 12 months. Some ozone records continue to have artefacts clear in the data even after 12 months.

P4, L12: Given the much higher sampling (by time and latitude) of MLS, and given the agreement with the diurnal variation as observed from Mauna Loa ground based station, is MLS therefore providing a more ‘daily-average’-like ozone estimate? If this is correct, then it would further strengthen your argument to state this so that your biases are clearly a result of the diurnal cycle.

P4, L22: I think it may also be possible that the higher uncertainty is a result of a ‘non-linear’ divergence of instrument records, given that all the other SBUV datasets presented retain a relatively stable equator-crossing time. NOAA 16 has the most rapid changes, and I wonder if you compare with earlier instruments (in terms of their uncertainty and change in equator-crossing time) if this is the case. If true, then a non-linear fit (rather than a linear) might account for much of this uncertainty, and is a valid model given you know that the change in equator crossing times become more rapid as you approach/leave the terminator (e.g. this might improve estimates discussed on P6, L33 regarding time-dependent drifts), and therefore tighten your uncertainties both in the merged dataset, and in your analysis.

P4, L22-24: Following the argument in the previous point, your comment about a short overlap influencing the uncertainty is a valid point. But, the stabilisation of the drift as reported by Kramarova et al., 2013b taking 70 months to becomes stable with longer periods, also may hint at a non-linear fit being better than a linear one to account for the drifts. My interpretation of Figs 1 and S19 in Kramarova et al, 2013b, look like there may be a non-linearity in the drift estimate, in line with the turn around in the latitude-change of the orbital drift (which would then cancel out a drift in one direction later on as we see in Fig. S19); but this is eyeballing and perhaps I have misinterpreted Fig S19. Have you considered such an approach, or otherwise why would such an approach be inappropriate?

Fig. 4 & P4, L34/35: Could this difference in seasonal cycle also be due to slightly different times of day being observed, and slightly different observational properties of the instruments (rather than being due to chlorine)?

P6, L4-5: "We scale. . . pairs." Do you mean that you compute (and redistribute) the uncertainty between pairs (and only pairs) of instruments (i.e. you only consider pairs, not triplets etc when more than two exist)?

P6, L13: Why is 24 months required for an overlap? Don't shorter periods simply lead to higher uncertainties, and therefore lower weights?

P7, L1-12: The procedure discussed here is reasonable to first order. However, I have strong concerns that the assumption of Gaussianity to represent drift and offset uncertainties (given the examples presented in Fig 5 and 6) is clearly inadequate. It would be better to sample directly from the distribution itself or, further, the joint-distribution between drift and offset, since these are not-independent quantities; the additional use of a Markov Chain would correctly sample the joint distribution and provide better uncertainties. While perhaps infeasible to address at this point, this would perhaps be something at least worth mentioning and considering/proposing for future improvements in the merging procedure (and perhaps in the conclusions; see below). As it stands, the current approach considered perhaps leads to rather conservative uncertainty estimates that might be reduced with a procedure more akin to that mentioned here; such conservative uncertainties propagate into the linear regression approach taken (e.g. P8, L20–21), as made clear in the analysis section.

P7, L1-12: This procedure would benefit from a brief schematic showing the steps taken (e.g. as in Fig. 11 of Laine et al., 2014); but this is simply a suggestion for clarity.

Fig. 7: It would be instructive to mark the satellite periods (perhaps with horizontal lines/bars) to indicate satellite periods and am/pm drifts, making clearer to the reader the source of drifts/jumps.

P8, L29-30: “Smaller. . . (1999).” So what does this physically imply? Is it that chlorine related recovery is not strong here, or just that the relative increase is smaller due to higher background levels? (i.e. are absolute changes are similar?).

P9, L15-16: When both MOD and NOAA datasets use only Nimbus7, why is there a scatter in the data in Fig 10 (i.e. prior to 1989)?

P10, L13-14: As you very nicely explain later in the paragraph, fitting EESC is not giving you the trend. Therefore, this sentence sounds slightly misleading that by using an EESC term in the regression analysis, you get a (more) significant trend; but isn’t this simply leading to a more significant estimate of the EESC component. Please rephrase this to ensure this isn’t misinterpreted.

P10, L24-26: Perhaps it would be good to address the major point above here, in terms of future approaches to merge data and account for uncertainties.

Minor points (grammar etc.): P10, L7: Nosier -> Noisier

References: Frith et al., 2014: http://onlinelibrary.wiley.com/doi/10.1002/2014JD021889/full Harris et al., 2015: http://www.atmos-chem-phys.net/15/9965/2015/ Laine et al., 2014: http://www.atmos-chem-phys.net/14/9707/2014/

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