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Interactive Comment

Interactive comment on "A Multi-sensor Upper Tropospheric Ozone Product (MUTOP) based on TES ozone and GOES water vapor: derivation" *by* S. R. Felker et al.

Anonymous Referee #1

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This paper presents a new method to derive high-resolution fields of ozone in the UTLS region. It is generally well-written although it could benefit from some critical editing as the argument is in places rather verbose. The figures are good.

I have one major comment however. The paper hinges on a multiple regression between a TES ozone product with two other fields - upper tropospheric humidity from GOES and potential vorticity from GFS. On p. 30066 l. 11 these are described as 'independent variables' - but whereas they are certainly derived from different sources these two datasets are themselves highly correlated and so are not statistically independent. At no point in the paper is this point discussed. The obvious question to ask is why

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have the authors decided to use multiple regression with two such highly-correlated variables? What happens if they just use GOES or GFS by themselves? Reference is made in section 6 to Yang et al (2007) who used PV to 'improve' their ozone residuals, but this is simply stated rather than compared with the present paper.

The argument as to why two datasets showing essentially the same morphology are preferred to one of them may be obvious to the authors but they must make a convincing case for this in the paper, otherwise Occam's Razor cuts in. It is not sufficient to argue that two datasets are better than one (as it would be if they were genuinely independent) nor to point to smaller residuals (which will inevitably be the case). An ozone dataset dependent only on PV (for example) would have obvious strengths and weaknesses related to the accuracy of the PV fields and the robustness of the ozone-PV relationship (which has been examined many times). By mixing two fields it is not clear where the strengths and weaknesses of this product lie, other than the factors discussed in 5.1. Could it be that high ozone values follow PV whereas lower values are better correlated with water vapour?

The authors should also be more critical of their results (see last point below)

Minor points. p. 30058 l. 2: this gives the impression that satellites measure PV p. 30059 l. 25-27 misrepresents STE: usually it is the vorticity that adjusts to its surroundings because of the thermal wind equation, leaving static stability as the anomaly. It is hard to think of a case where stratospheric air adjusts its static stability to match its surroundings. p. 30061 l. 6 'The current work presented in this paper' is tautological p.30064, l.21 GLASK brightness temperature is not a dynamical tracer p. 30068 l. 16. 'good representation' and 'low error' are meaningless terms. I would argue that the scatter in fig 5 is rather large - 'good', 'low' and 'large' only have meaning in the context in which the data are being applied. A rms error of 19 with most of the values below 100 ppbv does not suggest a particularly accurate representation. Indeed, it would be remarkable if, having chosen a domain intersecting the tropopause, a generally positive correlation was not found.

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