

## ***Interactive comment on “Validation of northern latitude tropospheric emission spectrometer stare ozone profiles with ARC-IONS sondes during ARCTAS” by C. S. Boxe et al.***

**Anonymous Referee #2**

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### 0.1 Overview

This paper by Boxe et al. presents a timely and much needed comparison of TES O<sub>3</sub> profiles with sondes at high latitudes with a high degree of spatial and temporal overlap. As such, the validation efforts presented here take significant steps forward over previous studies. The ability to quantify the empirical RMS error through multiple TES samplings of a single air mass is important. Overall, the content is well suited for ACP and while the paper could use some clarifications in a few areas, I believe it will be ready for publication in short order.

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### 0.2 General comments

1. The comparison between the empirical and theoretical expected errors comes across as overly optimistic. As this is one of the major findings of this work, it deserves to be treated a bit more carefully. For example, if the theoretical errors are 6% and the empirical errors are 9%, the authors are correct in saying that the difference between the two is small, only 3%. What they don't say, however, is that the theoretical errors are underestimates by 50%. Surely the latter framing of the comparison isn't as flattering, but it shouldn't be overlooked. Regarding the overall trends, I calculated a correlation of the mean theoretical errors and mean empirical errors in Table 4 to be only 0.52. So a regression of the empirical errors on the theoretical errors has an  $R^2$  of only 0.27. It would be preferable to provide statistical quantification like this rather than qualitative statements such as “generally consistent”. This type of analysis should be performed on the actual profile errors, not the mean profile errors as I did here. Statistical results should be presented in the discussion, abstract and conclusion. Statements therein that the empirical and theoretical errors are “consistent” might need to be reevaluated, or might be better supported by the new quantitative analysis.
2. The discussion of the averaging kernels is bit sloppy. Some of the text in section 6.2 is repeated, e.g., “For instance, Fig. 4a and b show ...” The discussion of aspects of Fig 4 following this doesn't clearly refer to 4.a vs 4b, which are different enough to warrant distinction. Overall, it's hard to follow the interpretation of the averaging kernel plots because the following was never clarified: the plots show  $\frac{\partial \hat{x}}{\partial x}$ . Does the y-axis or the orange color refer to the vertical coordinate of  $\hat{x}$  or of  $x$ ?
3. In Fig 1, the smoothed sonde profile nearly matches the a priori profile. This means that either  $x_{sonde} - x_a = 0$  or  $A_{xx} = 0$ . Since the TES O<sub>3</sub> profile doesn't match  $A_{xx}$ , it looks like the instrument did have some sensitivity and  $A_{xx}$  is not

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zero. But from the figure, it also doesn't appear that  $x_{sonde} = x_a$ . Could this type of behavior be explained?

### 0.3 Specific comments

- 27269, 25: suggest “altitude” – > “vertical distribution”
- 27277: was  $S_b$  defined?
- 27278:  $S_{\%}^s$  ?
- 27279, 10: is it standard practice to take a straight average of the averaging kernels? Or should it be a weighted average that accounts for varying measurement noise?
- 27282, 2: without repeating too much what is left to previous papers, could the “temperature difference criteria” be explained a bit more?

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 27267, 2009.