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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ozonesonde profiles from the West Pacific Warm Pool" *by* R. Newton et al.

Anonymous Referee #1

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This paper reports observations of ozone from ECC ozonesondes obtained at Manus Island in the Western Pacific. Central to the results of this paper are the ozone concentrations in the upper troposphere and how these data are impacted by the preparation of the ozone sonde and their background current.

This paper adds important information to this puzzle; however, there are a number of weaknesses in the discussion of the results, which I detail below. Therefore, I would suggest publication of this paper only after major revisions.

The paper recognizes that issues with the measurement of the background current exist, but it still treats this quantity as a well-defined and well-measured quantity, despite the problems encountered during the experiment. The need to change solutions mul-

tiple times to decrease the background is only one indication that this quantity is not well defined. It would be very helpful, if the authors added a conservative estimate of the uncertainty of the background current encountered in the upper troposphere and use this uncertainty estimate in their comparisons. While some of their measurements may be consistent with uplift of ozone poor air from the boundary layer, I would expect that the uncertainty is sufficient that uplift of free tropospheric air with higher ozone concentrations cannot be excluded. I would therefore urge the authors to improve the discussion of their uncertainties, which has important implications on the interpretation of their results.

The laboratory studies the authors conducted indicate that the so called background current is not necessarily the same value measured during the sonde preparation. This variability, which is extremely difficult to characterize, must be considered in addition to the uncertainty of the measurement during preparation.

The authors prepared their sondes not following standard recommendations by GAW. I can support the deviation of these standard recommendations, but the authors should try to comment on the impact of this deviation to other studies.

Vömel and Diaz (2010) reported on difficulties using ozone destruction filters in tropical regions. The authors should therefore comment on the possibility of incomplete ozone destruction in the filters used during their experiment and possible impacts on their measurements.

The authors use the outdated box temperature measurement instead of the pump temperature measurement, which is the current standard for ECC's. This may be of particular importance in the coldest parts of the atmosphere, i.e. the tropopause region and the authors should comment on this.

The ECC equation also contains a pump efficiency, which is not shown in equation 1. This factor largely plays a role at lower pressures than those studied here, but it would be good to know, which pump efficiency correction was used and which value was used in the upper troposphere.

The authors need to point out that their empirical hybrid correction may only apply to their particular soundings. Since the source and mechanism of contamination was not clearly established it can only be stated, that this approach may work for this experiment and may not be a general result that applies to any other campaign. Furthermore, this empirical correction strongly impacts the uncertainty of the affected measurements.

Unfortunately only soundings #34 and #35 can serve as true comparisons with the aircraft measurements; therefore, the statistics of aircraft validations in not overwhelming. A better discussion of the uncertainties and their significance may help in the interpretation.

At the Quadrennial Ozone Symposium 2012 there have been first indications that with the transition of ECC production from EnSci to DMT the average background measurements may have changed. Their different results compared to previous studies may be another indication of a possible change. While this is not yet well established, this production change may have significant impacts on the UTLS ozone measurements.

The authors should elaborate more on the bell jar measurements. What is their source of air inside the bell jar? Are they just recycling air? Can they exclude any additional impacts from the bell jar?

Figure A1 and Appendix A3: The authors clearly state that the background of the contaminated sondes decays with time. Therefore, a pressure dependence is somewhat misleading, even though it may be the more practical approach to apply the correction. The large scatter in the background measurements as function of temperature indicates that there is significant uncertainty in this correction. This should be described.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 16655, 2015.