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Atmospheric Measurement Techniques Discussions

Interactive comment on "Use of O_2 airglow for calibrating direct atomic oxygen measurements from sounding rockets" *by* J. Hedin et al.

Anonymous Referee #2

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Review of "Use of O2 airglow for calibrating direct atomic oxygen measurements from sounding rockets", by J. Hedin, J. Gumbel, J. Stegman and G. Witt

Understanding the chemistry and dynamics of atomic oxygen in the MLT region remains the key to enhancing our knowledge of this region. For this reason, this paper is welcome for its contribution, and its suggestion about simplifying the approach for future measurements, and for this reason in my opinion deserves publication. The authors have made significant contributions to the field, and their experience is of value to the reader. However, before submitting the final version, I would urge the authors to reconsider their approach in its presentation. Some elaboration of this suggestion follows, and after that some more minor comments.

As it presently stands, the work reads partly like a review paper, and partly like the

analysis of data from two rocket flights. In the abstract, the NLTE campaign is said to be presented as "an example", implying this is not the main thrust of the work. This presumably arises from the authors' intention to set the NLTE results in the context of the past history, but some problems arise in doing this. First of all, to make a review paper complete is challenging; in this case the considerable work of Iwagami of the University of Tokyo has not been included – those experiments may be traced back to the work of W. Morrow of Resonance Ltd., which in turn go back to R. Young (whose work was cited). The other problem is that not all of this background contributes to the main point that the authors are making, which is that a relative resonance fluorescence measurement combined with an absolute airglow measurement is the most effective way to make "direct" atomic oxygen measurements from sounding rockets in future.

This proposal itself, which must be the main body of the paper since it appears in the title, unfolds only in the conclusions of the paper. This proposal is made with little or no analysis so that the implications of the proposal are hardly addressed. This is clearly a place for some analysis and comparison. Some of these implications are: 1. If the resonance fluorescence technique cannot (readily) be an absolute method, then why use it at all? The argument seems to be that it gives better vertical resolution than is possible for airglow, but this argument is not supported by any analysis whatever. Fundamentally it is a local measurement, but some time is required to make the measurement which causes some vertical smearing and thus degrades the resolution. This smearing is determined by the signal-to-noise ratio, which also isn't discussed. From Figure 4 one cannot see the error bars for the signal, nor an indication of the sampling time. On page 1433, line 23 it is stated that for NLTE-1 the airglow calibrated peak is 30% lower than for the direct technique while for NLTE-2 it is 72% higher. If these two experiments were essentially identical, then the geometrical and shock wave problems described earlier should be the same. Can some explanation be given for this difference? In any case it does demonstrate the problems with the resonance lamp technique that are described. 2. The airglow method is simple, but requires inversion to a height profile. Why is the vertical resolution for this less than for resonance fluorescence, as is implied? This relates to the differentiation and the signal-to-noise ratio, but without working out the numbers and comparing to those for the resonance lamp instrument, one cannot say that the airglow results will be inferior. 3. As I understand it, with the proposed process, all the results would be linked back to the ETON resonance values. This would only change if at some future date a superior resonance lamp measurement were to be made. I agree that the results from this campaign are recognized as a standard, but are there any other rocket flights with comparable quality?

Some more minor comments are as follows. 1. Page 1421, line 15. For the Shepherd et al., 2005 reference there is a more substantial later paper describing this work, “Seasonal variations of the nighttime O(1S) and OH airglow emission rates at mid-to-high latitudes in the context of the large-scale circulation”, Liu, G., G.G. Shepherd and R.G. Roble, J. Geophys. Res., doi:10.1029/2007JA012854, 2007. 2. Page 1421, line 28. It is recognized now that there is much greater variability in airglow (and atomic oxygen) than was the case in 1981 (Offermann). See also item 3. below. 3. Page 1424, line 11. It does not seem correct to describe 250 R of O(1S) emission as “weak”, or even 120 R; weak would be 10% of this. It would be best simply to delete the word. The paper “Longitudinal Structure in Atomic Oxygen Concentrations Observed with WINDII on UARS”, by G.G. Shepherd et al., GeoÂňphys. Res. Lett. 20, 1303-1306, 1993, shows a factor of 10 variation in emission rate over longitude, for a fixed latitude, for a single day. 4. Page 1428, Section 2.2. Although this section falls within the presentation of results, this section seems very general, and would more properly be placed in the introductory material. 5. Page 1429, line 3. As for the preceding comment, this is a very general statement (about Heppner and Meredith) that could go in the introduction. In the following sentence it is not clear whether this is a general description of “a” rocket photometer, or the one used in the NLTE campaign. 6. Page 1433, line 20. There is no explanation of how the neutral density and temperature were obtained. A few lines would be helpful.
