

Interactive comment on “Use of O₂ airglow for calibrating direct atomic oxygen measurements from sounding rockets” by J. Hedin et al.

Anonymous Referee #1

Received and published: 13 August 2009

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

The main message of this very nice paper is an improved method to retrieve atomic oxygen density profiles from 130 nm fluorescence observations, demonstrated in the context of the NLTE rocket campaign in 1998. The only serious objection I found is to the paper's suggestion that mass spectrometry is not en vogue today because of any demonstrated unreliability, and, worse, that it falsely cites the inventor of cryo mass spectrometry (which eliminates both the shock front and instrumental chemistry as error sources in atomic oxygen profiles) as having admitted that the observed variability by a factor of 4 or 5 be not the "true range of atmospheric variations". Offermann et al. (1981) do attribute the observed variability by a factor of 4 or 5 to the "true range of

C443

atmospheric variations", while citing uncertainties of different cryo mass spectrometer experiments of only +30%, +35%, +45%. This point (which should be corrected) does not diminish the merit of the present manuscript to enable optical techniques to achieve a precision level competitive with the very expensive cryo mass spectrometer technique used in the 1970ies.

So, my conclusions are: The paper does address relevant scientific questions within the scope of AMT, since the determination of atomic oxygen in the upper atmosphere is an important issue.

The concepts, ideas, tools, and data which the paper presents are novel, in its combination, though earlier steps in this direction are maybe not completely covered (the authors should assess the appropriateness of citing previous attempts at utilizing ground-based airglow observations to determine atomic oxygen, like Haley, C.S., McDade, I.C., and Melo, S.M.L., An assessment of a method for recovering atomic oxygen density profiles from column integrated nightglow intensity measurements, Adv. Space Res. 27(6-7), 1147-1152, 2001).

The conclusions reached are substantial, because they improve the quality of atomic oxygen measurements at relatively low cost.

All the scientific methods and assumptions are valid and are clearly outlined, and the results are sufficient to support the interpretations and conclusions.

The description of experiments and calculations is very well done and sufficiently complete and precise so that other scientists should be able to employ the new technique.

The authors give proper credit to related work and clearly indicate their own new/original contribution, with the minor reservations mentioned above.

Title and abstract do clearly reflect the contents of the paper, and the overall presentation is well structured and clear, the language fluent and precise, mathematical formulae, symbols, abbreviations, and units are correctly defined and used.

C444

Only a few parts of the paper should be clarified, according to the list of details that follows below. This list covers (mostly) minor details that should be attended in a minor revision to results in a paper as nearly perfect as possible.

page 1420, line 8, "In general, accuracies better than a factor 2 are not to be expected from direct atomic oxygen measurements". This sentence must be understood as referring to the resonance fluorescence technique, not "in general", including cryo mass spectrometry! Therefore, "Mostly,..." or "Most often..." would be more appropriate.

p 1421, line 3ff: Without the information about long thermospheric life times, the information about the mesosphere appears out of context. This could be amended by adding "only" to "of the order of hours", or (better), talking about thermospheric lifetimes first.

line 21: this ignores the role of cryo mass spectrometers since the early 70s, and the continued use of mass spectrometers at ambient temperature, at least a decade later!

line 28: "variability ... mostly of instrumental nature". As mentioned above, the Offermann et al. reference is totally unsuitable to support this view! On the contrary, that paper attributes the observed variability by a factor of 4 or 5 to the "true range of atmospheric variations", while citing uncertainties of different cryo mass spectrometer experiments of only +-30%, +-35%, +-45%.

p 1422, line 17: order of references (neither alphabetic nor chronologic)!

line 18: it is always delicate to say something had been "most extensive", if an exhaustive review is not even intended, and neither the meaning of "extensive", nor the context is precisely defined. There have been many more multi-instrument rocket campaigns than what the authors mention here! I suggest change to "the most extensive series of such ... studies", which would put things straight and harm nobodies feelings.

p 1424, line 3ff: this sentence can easily be rearranged to avoid the nuisance to the reader of having to "remap" the information given at the end ("respectively"), by giving

C445

it "just in time" (this advice seems to be copyrighted by Michael McIntyre, but this reviewer agrees).

line 10: replace parenthesis before Jacobsen reference by semicolon to avoid need for double closing parenthesis! And, the reference does not stand for the results of the experiment, but, supposedly, for the technique.

line 12: see comment above, on line 3ff! The explication of the rayleigh unit is for a quite different audience than the main sentence, here harms ease of reading, and should be deleted, since the ap index is not explained, either (and need not be!).

line 24: "Atomic species are particularly suitable..." - a dangerous generalization, only valid for allowed transitions, imho... Also, the sentence does not fit into this context, where only atomic oxygen is discussed (which suggests a simple solution to criticism #1).

p 1425, line 17: Understanding this sentence requires a comma after "NLTE", but does not require the commas around "supported by numerical simulations".

line 19, 20: the two references describing the method were just given a few lines before, for the same purpose, and need not be repeated here.

line 23: I suggest to insert "thus" and "the" to read "... payload, thus positioning the absorption detectors..." to avoid a long string of words ("payload detectors") that is grammatically ok but, as such, makes no sense.

line 28 etc: better, spell out the terms pointed to by "the former", "the latter" (it's not the subject of the previous sentence, "the absorption process"!).

p 1427, line 9: "For almost all...". Oh yes, with the exception of cryo instruments like Offermann's (see comments above) that "freeze out" the shock front! It would not harm to be a bit more specific here, for historical truth's sake (of which young readers might be completely unaware); see, e.g., D. Offermann and H. Tatarczyk, A cryo-cooled mass spectrometer ion source for atmospheric composition measurements at supersonic

C446

rocket velocities, Rev. Sci. Instrum. 44(11), 1569, doi:10.1063/1.1686002 (1973), and references therein.

p 1431, line 17 ff: Rather than obliterating figures 7 and 9 with two curves that overlap most of the time, without any comment, it should be stated that the omission of quenching has little effect (if at all necessary, this could be shown in a separate figure, but need not be included in several figures).

p 1433, line 25: did I miss any comment about the fluctuations in the red line of fig. 9 (that coincide with the black line where quenching is ignored)? Are they supposed to be real, or instrumental artifacts?

p 1434, line 11: shouldn't the reason for this difference be made explicit?

lines 19, 21, 22, 24, 26: style problem by awful repetition of "way" ("this way ... is the best way, a simple way is desirable, our way ... is the second best, in this way... are not needed"). Also, why should the authors diminish their own contribution by calling it "second best", if the "best way" is too costly?

p 1435, line 8: delete "to" between "of" and "high"!

line 9: self-diminishment still lingers on, in "As replacement..." for the unobtainable objective to apply the nocturnal technique during daytime. We should all be grateful that an alternative does exist!

line 10: I question the appropriateness of the theoretical Mlynczak et al. (1993) paper as a reference to support this statement. The experimental work by Mulligan, F.J., and Galligan, J.M., Mesopause temperatures calculated from the O₂(a¹DELTA_g) twilight airglow emission recorded at Maynooth (53.2°N, 6.4°W), Ann. Geophys. 13, 558-566 (1995) would be more useful to cite here, but also the later Mlynczak et al. (2001) paper could serve this purpose.

p 1436, line 19: Murtagh's second initial was not published in this paper; change "u" to umlaut in Bruckelmann.

C447

line 28: change comma to dot in "Llewellyn, E. J."

line 29: missing space between "data" and "base"

p 1437, line 9: "Espy" should go before "Witt"

line 10: campaign name needs be spelled out - "Noctilucent Cloud 93 rocket campaign"

line 31: missing plural "s" in "precursors"

p 1438, line 7: change to upper-case "Infrared Atmospheric"!

p 1439, line 23: missing space between "molecular" and "oxygen"

line 30: change initial of Makhlouf from "O." to "U."

p 1441, 1st line: "All coefficients are in units" does not transmit the message well. Maybe, better "are in the usual units in terms of molecules, cm³, and seconds", or something like that. Also, the production efficiency symbol should be explained (or reference to the text be made).

p 1443, Fig. 2: "Direct Simulation Monte Carlo simulation"; first two words are "over-kill"

p 1448, Fig. 7: solid red and black lines overlap so much that little can be seen of the red line. It is preferable to only show the more essential one. Moreover: here, and in many other places in the text, the term "inverted" is used in the sense of "retrieved from", and only rarely in its precise meaning as "the opposite way" (as in "inverted equation"). Consider reformulating!

p 1450, again, "inverted ... emission", and "with and without quenching" (see corresp. comment).

(the authors should assess the appropriateness of citing previous attempts at utilizing ground-based airglow observations to determine atomic oxygen, like Haley, C.S., McDade, I.C., and Melo, S.M.L., An assessment of a method for recovering atomic oxygen density profiles from column integrated nightglow intensity measurements, Adv. Space

C448

Res. 27(6-7), 1147-1152, 2001). _____

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1419, 2009.