

***Interactive comment on “Balloon-borne
radiometer measurement of Northern Hemisphere
mid-latitude stratospheric
HNO₃ profiles spanning 12 years” by M. Toohey et al.***

Anonymous Referee #3

Received and published: 28 September 2007

General Comments:

**This paper presents HNO₃ measurements
obtained at a northern hemisphere midlatitude site
from balloon-borne emission radiometer
instruments during 4 flights spanning 1990–2002.**

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive Comment

The instrument designs, forward model and retrieval algorithms, and error assessment are briefly (in some cases very briefly) described. The balloon data are compared to ACE-FTS HNO₃ measurements and results from the Canadian Middle Atmosphere Model. Although this is essentially a validation paper, it does include a very interesting science component, in which the balloon data over the 12-year period are used to conclude that there has been no significant trend in stratospheric HNO₃ over this interval. The manuscript is well-written and well-organized, and I have no major criticisms of either the work or the manner in which it is presented. A few specific comments for the authors to consider, all minor, are given below.

Specific Comments:

1. In my opinion the inclusion of the MANTRA

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

CFC-11 and CFC-12 retrievals is not well motivated. Although they are mentioned in the abstract, they are not sufficiently important to the paper to warrant being discussed in the conclusions section (let alone the title). It is not clear to me what is gained by having them in the analysis. Furthermore, the discussion on pg. 11574, lines 4-5 ("the mean radiometer CFC profiles are comparable in shape to the ACE measurements") seems a bit of an overstatement, particularly for CFC-12, which displays some oscillatory behavior. I can understand that, having done the work, the authors wish to show these results, and I am not suggesting that the CFC discussion should necessarily be deleted; I am merely saying that its role in this paper needs to be more clearly defined and its presence motivated better.

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive Comment

2. I am confused by the statement on pg. 11570, lines 14-16: "These low altitude scans provide the most information regarding the instrument parameters, since they contain significant radiance contributions from HNO₃, CFC-11, and CFC-12." This seems counterintuitive to me. I would have thought that instrument parameters would be easier to separate out where the atmospheric signal is not strongest. Please clarify and expand this discussion.

3. On pg. 11576, lines 6-8, the authors use their results to infer that the precision of the ACE HNO₃ measurements is high. I guess that an ACE HNO₃ validation paper has not yet been submitted, but surely there is another reference to support this statement; if nothing else, at least the Boone et al. [2005] paper could be cited here. As it is now, this discussion implies that the analysis in this paper

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

provides the best assessment of the precision of the ACE HNO₃ data.

4. This is an extremely minor comment, but to me the conclusion section would read better if sentences 2 and 3 were swapped; that is, make the general statement about where/when the data were obtained before stating that they agree well with ACE data at a similar season/location.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 11561, 2007.

ACPD

7, S5462–S5466, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU