Response to referee comments, referee 1

We appreciate the referees' comments and have addressed their suggestions where appropriate in order to improve the paper. The comments, suggestions and corrections given by the anonymous referee are numbered below as they appear in the text with the response in the following paragraph named Rn (with n being the question).

1. (In general)
The introduction should be expanded for a better guidance of the reader. How does the water vapor distribution in the mesosphere look like? Why is it actually that the diurnal variation of water vapor is determined by dynamics and not by photochemistry? (It said later in the text, but too late.) How are tidal waves forced in the atmosphere? What were the main findings of Haefele et al.?

R1
The introduction has been expanded and rewritten to some extent and the findings of Haefele is now also included.

2.
"high temporal resolution" - How is that meant? With regard to the measurement repetition rate during a certain period of time? For a given location I would not consider the temporal resolution of satellite measurements as high.

R2
Rewritten

3.
Thermal tides were also observed by the MLS instrument aboard UARS, covering the altitude range between 20 km and 85 km (Forbes and Wu, 2006).

R3
The Forbes paper is now included and properly referenced.

4.
For any species to be a dynamical tracer its chemical life time needs to be in a similar order to the characteristic dynamical time scales of interest (Brasseur and Solomon, 2005). If the dynamical time scales are much shorter than the chemical life time of a species this species will become well-mixed, gradients disappear and, hence, tidal waves could not create any diurnal variations. Since the chemical life time of water vapor is in the order of days to weeks in the mesosphere its diurnal variation is dominated by tidal waves.

R4
The part discussing water vapour as a tracer has been rewritten and Brasseur (2005) is cited.

5.
"for some of the mentioned papers above" - Does this refer to the
publications on page 31267 in lines 19 - 21, or to the subsequent list? If it refers
to the subsequent list it is worthwhile to mention what has been studied with the
old instrument data, like the annual variation, sudden stratospheric warmings,
the quasi 5-day wave, QBO signatures or decadal trends at ALOMAR.

R5
Corrected

6.
I suggest to remove this sentence as it begs questions about
consistencies and because the time series is not the topic of this manuscript.

R6
Good remark and the sentence was removed.

7.
"moderate" - This is maybe not an appropriate word here. I guess the
point here is to make clear that the instrument has good sensitivity even at low
temperatures.

R7
Correct, the sentence was rewritten

8.
The data integration method seems to remove the advantage of the
new radiometer system that is advertized here. You could have done that with
the old system as well, of course resulting in poorer error statistics. 180 h of
integration is still quite a lot. It is also somewhat contradictory to what is, at
least, implied in lines 22 - 24 on page 31269. To which altitude can you now
retrieve water vapor reliably with a 6 hour integration under normal conditions?
Tstart and tint should be properly defined or replaced by text.

R8
This part has been rewritten together with the part mentioned on page 31269 to better reflect the
sensitivity if the instrument. Tstart etc has been properly defined.

9
Why is this Fig. 4? This seems to be an obvious Fig. 2, also from the
flow of the manuscript.

R9
Corrected

10.
Did you not set any particular measurement response threshold to limit the influence of the a priori?

R10
In short - no. I could have included a discussion on the advantages of different retrieval processes such as the OEM (used here) compared to the Baccus-Gilbert for example but I judged it to be out of the scope of the paper. The vertical resolution will differ slightly due to different noise conditions while using the OEM, as well as the a priori influence. The difference is however negligible with these integration times and the data set is much easier to handle than if we would have set a measurement response limit which would have resulted in changing integration times and increased the risk for measurement bias.

11.
Can you expand on the results of Charles McLandress' paper.
By itself it is not explained very much. I would also recommend to move this to the discussion section.

R11
The main results of the papers are now included

12
I highly recommend to show the phases.

R12
Included

Technical issues

13
"was" should read "were". - Corrected

14
"Fig. (1)." - Remove parentheses. - Corrected

15
Line 19: "((Zhang et al., 2010a,b))." - Remove double parentheses. - Corrected

16
Lines 25 and 26: "mesopause region at Arctic and Antarctic latitudes" should read "mesopause region at Antarctic latitudes". - Corrected