

Interactive comment on “Trends and variability of midlatitude stratospheric water vapour deduced from the re-evaluated Boulder balloon series and HALOE” by M. Scherer et al.

Anonymous Referee #3

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This is a paper with some interesting results. Given the paucity of long-term stratospheric water vapor measurements any reanalysis of the Boulder balloon dataset is of importance. The presentation could be improved, and some of the most important points upon which the analyses are based need to be explained in more detail. As I read it, the two most important points of this paper are the new bias correction and the new screening process. The bias correction process is adequately described, but given its importance I don't understand why it is relegated to the Appendix. Because of this, the casual reader (as I was on my first read through) can become confused as to when differences are caused by the new bias correction and when they are caused by the screening process. The new screening process affects everything which fol-

lows in this paper, hence this section needs to explain in detail what screening criteria were used. This is especially true because the authors are tinkering with a dataset which has such long-standing prominence in the community. What “large oscillations” are too large? What exactly are the screening criteria for “systematically lower values” during ascent? What level of mirror oscillations is too high? Also, are measurements which fail these screens clearly separated from the others, or are the exact screening levels somewhat arbitrary (as is, unfortunately, usually the case). Finally, why do these problems seem to preferentially affect soundings from 1997-2000? As far as this last question is concerned, even a statement saying something like: “we investigated possible causes of these problems and were unable to find a clear cause” would be better than nothing. I would think that 2 of the screens used (large oscillations and less water in descent than in ascent) could be just as easily done with the pre-1991 soundings with the chart recorder strips. If not, please explain why? Is HALOE lower stratospheric data in June 1992 really aerosol contamination free? Is there a reference for this? Are the HALOE trends the same if you start a year later? The phrase: “Neither the QBO nor the equivalent latitude proxy shows a trend over the periods 1981–2006 or 1992–2005, and cannot contribute to a trend in water vapour of these periods.” is certainly not generally true. Getting oscillatory terms wrong can result in an incorrect trend (although this is, admittedly, probably not the case here). “Variability for the whole NOAA FP dataset (dotted lines) is slightly lower, which may be counter intuitive.” Given the lower variability it is really not clear to me that a new screening is even justified. It would be good to find some kind of reason for this lower variability. E.g., are points being preferentially removed when the fit is generally good. The authors need to make every effort to assure the reader that their screen makes sense. Also, and somewhat contradictorily, I have to admit to being a bit surprised by the statement. Just from looking at the green datapoints in the figures it generally looks like they have more scatter. Maybe I’m missing something here. It seems to me that Figures 3c and 3d would go better with Figure 4, since these are all amplitudes. Also, it would be much

nicer if the HALOE and FP lines could be on the same plot to help the reader to visually compare the results. The comment at the end of Section 4 in reference to the 2 FP datasets: "the drop in 2001 is larger"; is a bit strange. While it's okay to make a comment about the drop in the "ALL" dataset, any comment about a drop in 2001 based on the HQ dataset doesn't even make sense to me, since the authors have removed almost all of the data in 1999 and 2000. What is the dominant contribution to the change in Figure 8? Is it the change in entry level CH₄, or in the stratospheric CH₄? Or is there some other contributing factor here which I've missed? Is the stratospheric CH₄ component calculated primarily from HALOE measurements? Also, it would be good to state explicitly already in 5.1 that the age spectrum has been kept constant (as is now stated in 5.2).

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