Atmos. Meas. Tech. Discuss., 5, C2635–C2637, 2012
www.atmos-meas-tech-discuss.net/5/C2635/2012/

Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "Ground-based remote sensing of tropospheric water vapour isotopologues within the project MUSICA" *by* M. Schneider et al.

Anonymous Referee #2

Received and published: 23 October 2012

This paper describes retrievals and associated error characterization of water vapor isotopologue measurements from a global ground-based FTIR network. The paper also describes some preliminary comparisons between these measurements and a global model. The paper is clearly written and the subject matter is highly appropriate for AMT. The authors have clearly stated the motivation for their work and the context under which it has been performed. They present a careful, considered and detailed error analysis, instilling confidence in a dataset that appears to offer promise for contributing to water cycle science.

Nonetheless, I believe that there are areas where the manuscript could be improved.

The term "interference from humidity" is used extensively throughout the manuscript. I think that this term is misleading. Interferences are generally considered to be things that one is not retrieving – here, the retrieval of humidity is an integral component of the approach. It is not really an interference. Perhaps the authors might consider the discussion in the context of "cross-state errors" instead?

Page 5370, lines 19-23: I think the authors ought to provide better justification of why their approach is superior to that proposed by Worden et al., 2006. The authors state that Worden et al.'s method uses "rather complex formulae" and is "not optimal". Are the formulae really any more complicated than those presented here? In what sense is the Worden et al. approach not optimal? What, specifically, is better about the method here? Something about the way that the averaging kernels for the end product are supplied? Additional clarification of what the advantages of this method are over previous work would improve this paper.

There are a couple places where the citation of references could be more complete.

Page 5363, line 10: "Schneider and Hase presented the first middle tropospheric optimal estimation retrieval. . ...using IASI. . ." What about Herbin et al.'s 2010 ACP paper?

Page 5363, lines 4-5: Lossow et al. (the SMR reference) is cited, but the relevant MIPAS (e.g. Steinwagner et al., 2007; Payne et al., 2007; Nassar et al., 2007) are not. While this is technically covered by "references therein", it strikes me that it would be good practice to just cite references for retrievals from the other two instruments.

Also, there are a couple of places where the language leans towards a proposal or sales pitch rather than a journal article.

"PROFFIT introduces innovative retrieval options" (such as logarithmic retrievals). The capability to do logarithmic retrievals is certainly useful, but I am not sure I would call it innovative. Plenty other people have thought of doing that.

“PROFFIT is currently the only retrieval code for ground-based remote sensing that supports an operational calculation of error Jacobian matrices.” There are so many retrieval codes out there (presumably many that are applicable to ground-based remote sensing) that I find this a little hard to believe. Is it the operational part that is considered unique? What does it take to classify something as operational?

I realize that the XCO2 retrievals are not the focus of this work, but I think the paper might benefit from some additional explanation in Section 5.

For example, Page 5381, lines 22-25: “The de-seasonalised annual mean total CO2 column should be very similar at all the different sites around the globe.” Can the authors quantify “very similar”? What about latitudinal gradients in CO2? Should these really be negligible in the de-seasonalised annual mean?

Figure 11: What exactly is meant by the 1 sigma scatter between stations?

In the conclusions, the authors state that the H2O profiles reflect real atmospheric variability between the lower and upper troposphere. What exactly is meant by this? Just that there are enough DOFS to distinguish between different altitude regions?

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 5357, 2012.