Atmos. Chem. Phys. Discuss., 12, C8687–C8691, 2012
www.atmos-chem-phys-discuss.net/12/C8687/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A multi-instrument comparison of integrated water vapour measurements at a high latitude site" *by* S. A. Buehler et al.

Anonymous Referee #2

Received and published: 27 October 2012

General comments

This manuscript compares integrated water vapour (IWV) measurements obtained from five different sensors and the ECMWF reanalysis (ERA-Interim). The authors highlight two main objectives. The first is to characterize the systematic and random errors of the sensors used at their subarctic Observatory (Kiruna, Sweden). Their aim is to built a long-term data record for climate research. The second objective is to assess the impact of two specific issues arising in such an intercomparison study: the difference in lower altitude limit for the IWV integration and the representativeness error. These objectives are of special interest to the author's work based on the Kiruna datasets. They are also of general interest to the climate community interested in monitoring water vapour. This manuscript hence fits well the topics of the WAVACS special issue hosted by ACPD journal.

Better quantifying the accuracy of the different observing techniques will give insight into the quality of present climate diagnostics derived from those measurements and help improving the observing systems. To this respect, the manuscript provides valuable results for the Kiruna site which could probably be transposed to some extent to other global climate observing sites (at least in similar subarctic climates). The methodology used therefore is sound in using a regression algorithm that takes into account errors on both datasets and considering specific temporal and spatial scales for each dataset for the temporal matching.

As for the secondary objectives, they are not new and the manuscript does not improve our knowledge about them. The impact of lower altitude limit is a well know limitation which has been corrected for in most past studies (at least for the past 10 years, to my knowledge). On the other hand, the representativeness error is a major limitation when point measurements and areal averages are compared. This is the case in this work and the authors propose two methods to assess this error source. First they compute statistics for the difference of distant IWV values extracted from a high resolution (3.5 km horizontal) cloud resolving model. This approach might provide an estimate for the representativeness error when to non-collocated IWV measurements are compared if computed over a statistical representative ensemble. Unfortunately, the authors use a single model simulation and extract IWV from a global latitude band which is certainly not representative of the time variability of the local climate. The second method is based on a similar approach (though this is not very clear from the manuscript) using AMSU-B data. Both methods give consistent rather high estimates of representativeness errors which lead the authors to the conclusion that representativeness error dominates the random errors between theirs datasets. To my opinion, these results of representativeness errors are not accurate enough to draw such a conclusion.

Therefore I recommend that the discussion of lower altitude limit be reduced and that the discussion of representativeness error be revised according to the specific comments given below. Some minor corrections and remarks are added as well for the revision.

Specific comments

Lower integration altitude limit:

- The two figures (fig. 3 and 4) are probably not necessary as the method is not new and can be well understood from the text alone.

- The text of section 3.4 could be easily shortened by limiting it to the description of the method and giving the empirical relationship used to correct the data.

- It is not obvious to what range of altitude difference the empirical relationship would work.

Representativeness error:

- Section 3.5 refers to O'Carroll et al., 2008, but while reading I could not find where the authors follow the methodology proposed by O'Carroll for analyzing the error sources of their datasets. First, O'Carroll's method is based on a three-way comparison of datasets for deriving estimates of the variance of random error for each of the datasets. The authors of the present manuscript do not attempt this though I guess it would be very valuable to quantify individual errors in addition to the differences of datasets. Second, O'Carroll introduces a strong assumption on the independence of errors which is difficult to prove in the presence of representativeness errors. In view of these two points I recommend to remove that part of the text which tries to make a parallel with O'Carroll's publication.

- Otherwise, the introduction of representativeness error (lines 4 to 24, page 21030) is satisfying.

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- The part of section 3.5 using the NICAM simulations is very debatable in its present state of achievement and would be better removed for now. Moreover, nothing is said about the realism of the IWV field simulated by the model.

- The part using AMSU-B data might be a good alternative to using the NICAM simulation given that the IWV data used to compute the statistics are representative of the time variability of the local climate. The text just mentions that data along the satellite track for the year 2008 is used but it is not clear whether the data was limited to a latitude / longitude box around the study site? I recommend that that part of the study be clarified or improved if it is to remain in the manuscript.

Minor corrections and remarks

On GPS results:

- The authors write in section 2.1 that the radome covering the GPS antenna is cleaned when it is covered with snow. To what extent does the accumulation and removal of snow impact the IWV estimates?

- The authors write also that the antenna PCV can be calibrated but quote a possible 1 kg/m2 bias in their dataset because the data were not analyzed with the proper model. Could this explain the bias in the GPS IWV mentioned in the discussion section (5.1)?

- Other limiting factors are mentioned in section 5.2.1. Could the authors give an idea of the order of magnitude of these biases?

On radiosonde results:

- Three different radiosonde types are used in this study, but the authors do not distinguish results as a function of radiosonde type, though it is well known from the literature that radiosonde biases are sensor-dependent as well as dependent on time of launch (day-night difference). Could the authors present separate statistics or justify about their choice in this respect?

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Interactive Comment

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On ERA-Interim data:

- The authors write that "ERA-Interim grid point data are not area averages, but are valid at the exact location of the grid points indicated by the grid." To my knowledge this is wrong. The model fields are areal values representative of the grid length-scale. Remember that when extracted on a 0.75°x0.75°, model fields are filtered and interpolated from the native grid.

On FTIR results

- The authors mention that "FTIR and microwave data are suitable if and only if instrument and algorithm are kept the same over long time periods" (section 5.3). I wonder if the raw data from these instruments could be reprocessed over long time periods using a fixed algorithm to overcome at least of these limitations?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21013, 2012.