Atmos. Chem. Phys. Discuss., 14, C6817–C6829, 2014
www.atmos-chem-phys-discuss.net/14/C6817/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparison of in-situ FISH measurements of water vapor in the UTLS with ECMWF (re)analysis data" *by* A. Kunz et al.

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Received and published: 9 September 2014

We thank reviewer 1 for the careful reading and detailed feedback on our paper. After a short summary of the revision the general and specific comments of reviewer 1 are usually repeated first (in italic type) and we reply to the respective statements in detail. The new sections of the revised paper concerning the reviewers' comments are red colored.

Summary of the revision:

In response to the reviewers' comments and suggestions, we have made a revision of the manuscript. The main points of the revision addressed in the text are summarized

here:

- Following largely the suggestions and comments by reviewer 1 many parts of the text are revised. In particular, the abstract, introduction and the summary and discussion sections. All important information on the model data, i.e., reanalysis and operational analyses, can be found in their respective sections (2.2 and 2.3) in the revised manuscript. The information is not distributed at several places in the manuscript anymore. A new chapter 2.4 is included with a discussion on the changes to the IFS over time from 2001 to 2011. Influenced by a special comment of reviewer 1 a further figure is included as a case study to show an area in the LS where an improvement of the operational analysis data is identified.

- According to a comment of reviewer 2 the ratio between simulated and observed water vapor mixing ratio is revised. The former ratio $\Delta(H_2O)$ is an asymmetric quantity, that is, underestimations are related to $\Delta(H_2O) \in (0, 1]$ and overestimations to $\Delta(H_2O) \in [1, \infty)$. The reviewer is right, that this asymmetry has implications on statistical quantities like means and standard deviations. Following the reviewers' suggestion we replaced the ratio through its logarithm (with base 2), i.e., $\Delta(H_2O^{log2})$. This is a symmetric quantity around 0 and there are no issues with statistical quantities anymore. Section 2.4 of the revised version gives a detailed introduction of this new quantity including an additional figure. All other figures and their discussion in the text are revised concerning this new ratio.

Specific comments of reviewer 1:

Abstract:
The abstract is rewritten. Abbreviations are explained and the key findings are listed. The most obvious stratospheric processes that are challenging to the models representation of water vapor are isentropic processes such as quasi–isentropic exchange processes. In the troposphere, convective processes seem to affect large deviations

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between the modeled and observed water vapor distribution. The deviations between the model and the observations are not presented as mean values since the deviation ratio is usually presented as PDF in different bins in our analysis (see Table 1). We therefore refer to these bins in the abstract and mean values are presented at another place in the paper.

Introduction:

The reviewer raised the issue that the introduction would be sometimes a bit confusing and a clear statement about the main aim would be missing. Following the reviewers' comments, we therefore re-structured the introduction and better worked out what the main aims of the study are as a logical consequence of the state-of-the-art. Discussions on the objectives of the different aircraft campaigns contained in the FISH–based water vapor climatology are shifted into section 2.1.2 and the different water vapor fields by the ECMWF are also removed from the introduction and are explained in more detailed in the respective sections 2.2 and 2.3. The many minor suggestions of the reviewer concerning the introduction are all considered and appear as red parts.

2.1.1 Measurement technique of FISH:

The reviewer suggested to add a reference to the discussion of the accuracy of the FISH instrument. The reviewer is principally right. Anyhow, no publication exists in literature which lists the current accuracy values of this instrument. The values given in our publication are the accuracy values which are personally communicated with the current PI of the FISH instrument, Nicole Spelten, who is the first co-author of this paper.

2.1.1 FISH–based water vapor climatology:

Table 1 contains all references which present some overview papers or at least some detailed information on the different campaign performances. Unfortunately, there are not any references with information on the EUPLEX, POLARCAT-GRACE, CIRRUS, and MACPEX. Anyhow, we added at least two URLs to web pages containing some

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information on the campaign performance of MACPEX and POLARCAT-GRACE. The values of the wind contours of Fig. 1 are listed in the figure caption of the revised manuscript. Another question of the reviewer was on the gas-phase measurements of the FISH instrument. These measurements are selected according to water vapor mixing ratios that are lower than the saturation mixing ratio corresponding to a relative humidity with respect to ice of 100%.

2.2 ERA–Interim data:

Summary: Following the suggestion of reviewer 1 we took together all the information on the ERA–Interim water vapor fields that are given in the manuscript. This information is only discussed in this section of the revised version of the manuscript. The paragraph describing the comparison methods of ERA–Interim and FISH water vapor fields is revised according to the reviewers suggestions and open questions. In addition, a short summary on equivalent latitudes and the calculation of the PV gradient tropopause is included.

Individual reviewer comments:

Page 14406, line 21 – what exactly is the "water vapor product"? – specific humidity?

Right. This is also commented in the text.

Page 14406, line 21 to Page 14407, Line 14 – from your description it remains unclear when and where changes are made to the forecasting system and what is different in ERA-Interim. Does "prior" mean during the years before, just before Cycle31r2 or only for the ERA-Interim model version. "Refined for ERA interim" is confusing. The discussion about the influences of the changes ti the model versions your results should be done in the discussion part of the manuscript.

A new chapter 2.3 is included in the revised version of the manuscript. This chapter contains a detailed description of the changes to the IFS over time.

Page 14407, line 16–25 – This paragraph needs some more explanation on the comparison method: - The first sentence is grammatically incorrect. - What does "transforming" mean? Does that mean that the observed air parcel is advected to the tempo-

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rally closest synoptic times? - "Synoptic observation" is a new terminology. Does that indicate the position of the in-situ observation? - What are the weaknesses or errors of the "temporal interpolation" method? Do you assume that the observed values remain you constant over the integration time? What are the advantages compared to linear temporal interpolation? Why do you use Theta to interpolate in the vertical coordinate? What is the vertical resolution of the ERA-interim data?

This passage is rewritten in the manuscript. Here are some additional comments concerning the questions of the reviewer.

The choice of the linear interpolation method is based on our experience with comparison of different (asynoptic) tracer observations with the model (CLaMS). The correlation increased if the linear time interpolation was replaced by a trajectory-based interpolation method (however, we have never published such differences). Of course, for sufficiently high frequency of the meteorological data (like every hour), the differences between the linear interpolation in time and trajectory-based interpolation become smaller.

The use of the interpolation in theta-space is based on the fact that transport in the vicinity of the tropopause occurs almost isentropic. Because the trajectories are calculated in theta-space (with diabatic heating rates for the vertical velocity), we also perform the linear interpolation using this vertical coordinate. One could also use pressure as the vertical coordinate, but this variable would have to be interpolated along the theta related trajectories and, consequently, some additional numerical errors would be caused.

Page 14408, line 4 – Even though it has been discussed in your previous papers (Kunz et al. 2011a, b) you should consider giving a short summary on the calculation of equivalent latitudes and what the idea is behind that. It is not mentioned in the introduction why you use this specific method and what can be expected.

done

Page 14408, line 8 – What do you mean with "the measurement location is also placed in relation with the location of the thermal tropopause on altitude levels"?

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This is better explained in the revised text.

2.3 Operational analysis data:
The section is revised concerning the reviewer comments. The spectral horizontal resolution of ERA–Interim is T255 which is now compared to the higher resolutions of the operational analysis data (T511 until 2006, an increase to T799 in 2006 and to T1279 in 2010). The six IFS time intervals were chosen since each of them represent almost 2 years. Anyhow, in the revised version of the manuscript we only focus on the time interval 1 (Cy28r1-Cy30r1) from March 2004 to August 2006 and time interval 2 (Cy36r1-Cy36r4) from January 2010 to April 2011. These are the time intervals which are well represented by FISH measurements and are studied in more detail in section 4 to show the improvement of the model between 2004 and 2011.

2.4 Ratio of water vapor between reanalysis fields and FISH:
A comment is included in the revised version of the text that contains the most relevant ratio values of the paper, and describes them as multiples of the measured water vapor, i.e., observations are twice, three, or ten times larger than the observations. There is also a new figure (Fig. 1) concerning these values.

2.5 An example flight:
Summary: Following the suggestions of the reviewer, the discussion of this section is rewritten to avoid any repetition of information. Figs. 3 and 4 are still separate figures, but we introduced them differently in the text so that the reader should be able to better follow the discussion now. Fig. 3 additionally shows a panel c with a map of the flight track. Fig. 4 is revised and the mean water vapor mixing ratios of H_2O_{ERA} and H_2O_{ANA} are now presented in different panels regarding the correct ratio bins $\Delta(H_2O^{log2}_{ERA})$ and $\Delta(H_2O^{log2}_{ANA})$, respectively.

Individual reviewer comments:
Page 14409, line 14 – consider adding "Measurements are collected both"
done

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Page 14409, line 20 – add a time window [XX:XX – XX:XX UTC] to make clear where the "poleward side of the jet stream" is.
done
Page 14409, line 27 –"deeper troposphere" – do you mean upper troposphere?
Replaced by 'middle troposphere'
Page 14409, line 28 – I suggest to put the discussion on the impact of different cycles to the end of the paper manuscript.
The discussion still appears in this section.
Page 14410, line 12 – add some explanation on the mean water vapor mixing ratio per Delta (H2O) plot and how this can be interpreted. What FISH water vapor mixing ratios are used to calculate the mean values over the different bins (black line)? Don't they differ for ERA-interim and operational analysis for the different bins?
Many thanks for this comment. We corrected the Fig. 3 that now separately presents the mean mixing ratios of H_2O_{FISH} and H_2O_{ERA} for $\Delta(H_2O^{log2}_{ERA})$ (middle panel), and the mean mixing ratios of H_2O_{FISH} and H_2O_{ANA} for $\Delta(H_2O^{log2}_{ANA})$ (right panel).
Page 14410, line 16-22 – I suggest moving the discussion on the influence of the measurement uncertainty on the calculation of (H2O) to section 2.4.
Done

3.1 Campaign based analysis:
Individual reviewer comments:
Page 14411, line 16 – consider adding "is varying from"
done
Page 14412, line 9-10 where do I see this agreement and what is in between 100 and 300 ppmv?
You see this near perfect agreement between the model and observations for example for the TROCCINOX2005 campaign (H_2O ≈10 ppmv) and for the POLARCAT2008 campaign (H_2O ≈300 ppmv).

3.2 Tropopause based analysis:

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Summary: This chapter is rewritten and restructured and more references to the panels are given.

Individual reviewer comments:
Page 14412, line 21-22: remove this sentence as it is a repetition of the first sentence.
done
Page 14413, line 3 – why "However"?
skipped
Page 14413, Line 6 – please add information about the three domain classification and what the author can see in Figure 1. What happens in cases of double detected tropopauses with the aircraft flying in between, e.g. at 18:30 UTC?
Information concerning this issue is inserted in the text.
Page 14413, line 7 – What do you mean with "influence of the jet stream"?
I mean measurements in the vicinity of the jet stream. This is changed in the text.
Page 14413, line 13 – add e.g. "are found in the"
done
Page 14413, line 21 – add e.g. "are ranging between"
done
Page 14413, line 21 – "more strongly over and underestimate" compared to what. Where do you see that?
Compared to the measurement frequency maxima near the tropopause.
Page 14413, line 21 – Why "may reach"?
'may' is skipped.
Page 14414, line 1 – I suggest to change "on isentropes" to "at potential temperatures"
done
Page 14414, line 2 – From the colorbar it is not clear where the transition at 400 ppmv is exactly located. Is there a change in color? Please indicate this threshold in Figure 5.
The transition should be at 300 ppmv and this can be seen by the colorbar. This is

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fixed in the text.
Page 14414, line 8 – Where can the underestimation by a "factor of up to 3" be identified? Add values to the red dashed vertical lines in Figure 5, as it is done in Figure 6.
done
Page 14414, line 11-15 – I miss a discussion of the subtropics. Why are the lowest values of FISH and ERA such sharply truncated in the extratropics? Figure 5, caption: where are the "middle panels"? Use maybe column and rows. Here and elsewhere: avoid "water vapor" and instead use water vapor mixing ratio or specific humidity.
A discussion of the SUBTROP domain is included.

3.3 Equivalent latitude-based analysis on isentropes:
Summary: This chapter is rewritten and restructured for a better readability. The methodology of the equivalent latitude concept and the PV gradient based tropopause is better described and most parts of the description can be found in section 2.2 of the revised version as suggested by the reviewer. The difference of the maximum PV gradient compared to the conventional 2 PVU isoline threshold is particularly obvious on isentropes above 350 K. Here, the maximum PV gradients better represent the dynamical and chemical discontinuity at the tropopause than the 2 PVU isoline. This issue is topic of former publications (e.g., Kunz et al., 2011a&b) and is not repeated in the current publications.

Individual reviewer comments:
Page 14414, line 22-26 – "The PV-gradient based tropopause is (located???) in the ..."
'located' is inserted
Page 14415, line 4 - Page 14415, line 4 – It is hard to follow the discussion of Figure 7 and 8. Please improve this paragraph e.g. by adding latitude information to the potential temperature values and discuss stratosphere and troposphere separately. Give more references to the panels.
The paragraph is improved and more references are given to the panels.
Figure 7, caption – does "Zonal mean zonal wind . . ." mean that you just use the u

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component of the wind vector in this case? Why?
The zonal wind component is shown here since the figure should give a zonal mean representation.
Page 14416, line 3 – specify "problems"
Difficulties of the model to represent the water vapor distribution in the UTLS.

4. Water vapor evaluation: operational analysis vs. FISH:
Summary: This section is revised following the reviewers' comments. The results are better referred to the figures in the new version.

Individual reviewer comments:
Page 14416, line 6-9 – confusing sentence that mixes up two things. On the one hand the idea to evaluate the ERA-Interim data by comparing with FISH data to investigate a temporal trend related to changes in the assimilation system, resolution and other changes. On the other hand you want to explain why you analyze the data separately for stratosphere and troposphere. Rephrase and consider to move this to the introduction.
The sentence is rephrased.
Page 14416, line 10-11 – what means "is done separately for the different IFS cycles"?
Thanks for this comment. We revised this sentence in the revised script to highlight that we do the investigation of operational analysis data on a daily base to regard the changing IFS cycles over time.
Page 14417, line 1-2 – this sentence is a repetition.
This sentence is skipped.
Page 14417, line 3-6 – where do I see the overestimations that are related to humidities lower than 5 ppmv. I also see a lot of higher values? Where are the middle panels?
This can be seen in the correlation plots for the SUBTROP domain. We refer to this in the text.
Page 14417, line 12 – what is "the second period of IFS cycles 36r1-37r2"?
Thanks for this comment. The sentence does not make sense and is revised.

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Page 14417, line 19 –"The increase. . .": is this a comparison of the bottom panels of Fig. 9 and 10?
Yes, the reviewer is right. This is clarified in the revised version.
Page 14417, line 21-23 –"This may. . . ": I do not understand this argument and where this comes from. Consider adding this this to the discussion.
We worked on this sentence. Together with the correction concerning the former comment of the reviewer the text should be understandable now.
How robust are the findings from Figure 9 and 10? E.g. you identified a region of improvement in the analysis (i.e. the region where the yellow dots do not overlap with the green dotted area). Is this only data from one case? I would be interested in seeing how the model data of analyses and ERA-interim differ in this case, e.g. by visualizing model cross sections along the flight track. This may show differences in the humidity fields or indicate positional/temporal shifts of the tropopause structure related to differences in the dynamics. This would also be interesting for the case study in Figure 3. Maybe it gives some hints about the processes causing the improvement. Regarding the improvement over time, it would be interesting to calculate an improvement metric for the daily means. You could compare norms of the absolute differences between H2O(ERA) and H2O(FISH) and between H2O(EC AN) and H2O(FISH). The difference of these norms implies either an improvement or deterioration. A time-series would show whether there is a clear signal in improvement or not.

Thanks to the reviewer for these interesting 2 points.
Regarding the improvement of H_2O_{ANA} over time we already had calculated a socalled improvement metric that is slightly different from the metric suggested by the reviewer. We calculated for the mean values shown in Figs. 9 and 10 (left panels) the difference between the ratios, i.e., $\Delta H_2O^{DIFF} = \Delta H_2O^{ERA} - \Delta H_2O^{ANA}$. Positive (negative) values of that difference indicate times when H_2O_{ERA} is worse (better) than H_2O_{ANA} in representing H_2O_{FISH}, a zero value indicates no difference between H_2O_{ERA} and H_2O_{ANA}. Anyhow, there is no obvious improvement of the representation of the H_2O

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distribution in the UTLS over the time between 2001 and 2011. The metric ΔH_2O^{DIFF} fluctuates around the value 0. In the LS, this fluctuation is around 0 ± 0.5, in the UT, it is around 0 ± 1.0. The conclusion of this investigation was that there is no clear difference between ERA-Interim and operational analyses, indicating that problems remain irrespective of the data assimilation cycle and model resolution. Figs. 9 and 10 both show this tendency and there is a discussion on this point at the end of Section 4 and in the Summary and Discussion Section 5.

A second point of the reviewer was on a region with a clear improvement in the analysis data, e.g., the former Fig. 9 (or Fig. 10 panel f of the revised version) shows a correlation plot between H_2O_{ERA} and H_2O_{ANA} with H_2O_{FISH}. In particular, H_2O_{FISH} >20 ppmv are better represented by H_2O_{ANA} than by H_2O_{ERA} in the subtropical LS. We looked into the details what kind of flights represent these areas in the correlation plots. The result is that these areas of an improvement in the analysis data can be traced back solely on MACPEX flights from April 2011. In particular, the flights from 11 April and 25 April 2011 represent these areas whereas the example flight from 1 April 2011 (Fig. 3 of the revised version) does not represent these improvement areas. A detailed study of the two flights on 11 and 25 April 2011 in the subtropical LS with H_2O_{FISH} >20 ppmv shows two areas of improvement with H_2O_{ANA} better representing H_2O_{FISH} than H_2O_{ERA}: 1) In the vicinity of the thermal tropopause where large gradients of H_2O appear. 2) In-between a thermal and a second thermal tropopause. We included some relevant text and an additional figure concerning this at the the end of Section 4.

We also wanted to reproduce the cross-sections (e.g., Fig. 3, panel a) with static stability and the height of the thermal tropopause along the flight track based on operational analysis temperature and wind fields. This would indeed be a valuable analysis to study whether there are improvements of the resolution of water vapor gradients around the tropopause as well as improvements in the dynamical and tropopause structure. Anyhow, we are sorry that we are not able to do this investigation. There had been a computer crash some months ago and the respective operational analysis fields are lost. Thus, we desist from reproducing these data since there are also changes in the

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trajectory and interpolation tools and we cannot guarantee a same data processing of these new files. Thus, differences between the ERA-Interim and operational analysis cross sections may well appear due to further error sources, which we cannot quantify.

5. Summary and discussion:

Following the reviewers' comments, this section is completely rewritten and the key findings are elaborated. This is done with consideration of the valuable suggestions and comments of the reviewer concerning the summary section. Thanks again to the reviewer.

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