

# ***Interactive comment on “A decadal time series of water vapor and D/H isotope ratios above Mt. Zugspitze: transport patterns to Central Europe” by Petra Hausmann et al.***

## **Anonymous Referee #2**

Received and published: 31 March 2017

### General comments

This paper presents results of recently produced H<sub>2</sub>O and  $\delta$ D FTIR measurements from the Mt. Zugspitze observatory. It uses an extensive set of backward trajectory analysis runs to show there are distinct transport patterns through which moisture reaches the site: intercontinental transport from North America (TUS) and North Africa (TNA), as well as stratospheric air intrusions (STI). Lidar profile measurements verify TNA and STI events. This study shows Mt. Zugspitze can monitor long-range transport events potentially impacting the Central European free troposphere and stratospheric ozone, and demonstrates the value of using {H<sub>2</sub>O,  $\delta$ D} pair analysis as a tracer for transport processes.

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I recommend the manuscript be published in ACP.

I have a few minor comments related to the clarity of the text for the authors' consideration:

#### Specific minor comments

Throughout the text, the authors use both "Mt. Zugspitze" and "Zugspitze" interchangeably to refer to the observatory site. I suggest remaining consistent, and to consider using the latter for the observatory site exclusively (or an alternative) because "Mt. Zugspitze" is needed in the manuscript as a reference to the geographic landform, e.g. page 12 line 9.

Page 5 line 13: The authors say they've selected an "exemplary" measurement to represent the mean state of the Zugspitze time series. What criteria were applied to justify "exemplary"?

Page 7 line 20: I suggest the authors clarify what is meant by "based on positive experience". This is unclear.

Page 7, line 25: The decision to use 120 hour trajectories if no condensation point is found aligns with other studies; however, I suggest adding a clarification about why this duration is appropriate.

Page 8, line 2 and 3: I suggest giving the reader threshold values for what is considered "extraordinarily low" and "very high"  $\delta D_{col}$  in the text.

Page 9, line 25 (and elsewhere): STI already defined. Re-check the manuscript and remove extra definitions.

Page 13, line 2: The reported R value of -0.295 indicates a very weak correlation. I feel it is necessary for the authors to discuss this result and justify the asserted relationship between  $\delta D$  and Be-7.

Page 29, Figure 4: It is regrettable that the backward trajectories overwhelm the figure's

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geographical information. Interpretation of the results would benefit if the figure could retain the outline of the continents. Most of the map is not visible.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1029, 2017.

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