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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Deuterium excess in atmospheric water vapor of a Mediterranean coastal wetland: regional versus local signatures" *by* H. Delattre et al.

Anonymous Referee #2

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In this manuscript, Delattre and coauthors present measurements of the isotopic composition of water vapor from a near-coastal site in France and discuss the daily and subdaily variability in terms of synoptic-scale air mass transport and contributions from local evaporation. In my opinion, this carefully compiled dataset can be very valuable for an analysis of the regional atmospheric water cycle and is certainly worth publishing. However, I have some doubts with respect to the proposed interpretation of the data, and in particular I think that the quantitative budget analysis employing the two-component mixing model is flawed, as outlined in more detail below. The presentation of the results is generally adequate, but in some parts the English writing is a bit

unclear.

Major comments:

1. I don't think the distinction made by the authors between the interpretation of daily and subdaily variations makes much sense. In particular, I do not believe that the daily variations in deuterium excess are mainly driven by variations in the isotopic composition of different advected air masses (as stated, e.g., in the conclusions). Such a connection cannot be deduced from the fact that different deuterium excess values have been measured during periods with transport from the Mediterranean or North Atlantic region. This can be explained by differences in local evaporation: North Atlantic air masses (as described by the authors) are relatively cold and dry, and are thus associated with strong evaporation with high deuterium excess from the local lagoon, in contrast to the warmer and more humid Mediterranean air masses. I think this mechanism (and thus the effect of local-scale evaporation) can fully explain the measured differences in deuterium excess of ca. 5 permil. Accordingly, in my opinion both daily and subdaily variations of d are mainly driven by local evaporation conditions. Nevertheless, this does not mean that deuterium excess looses the signature of its moisture source (page 1725, line 17). With the simple trajectory approach applied by the authors, the moisture sources of the sampled air masses can simply not be determined. In many cases, evaporation from the lagoon can be a very important local contribution (as also evident from the quantitative estimates presented in the second part of section 4.2.6). Via the mechanism described above, the local source conditions can drive the measured d variability, and the measurements are thus perfectly consistent with previous studies relating d to the relative humidity during evaporation (Craig and Gordon, 1965; Uemura et al, 2008; Pfahl and Wernli, 2008).

2. The results from the isotope-based budget analysis in section 4.2.6 (smaller local contributions during North Atlantic compared to Mediterranean advection) do not make sense physically (one would expect larger evaporation into dryer North Atlantic air masses, as described above). The differences to the subsequent evaporation-based

estimates are so large that they cannot just be argued away, rendering the quantitative results rather useless. I think the isotope-based analysis is flawed due to two reasons: First, if I am not mistaken there is an error in equation (5); instead of delta_v(min), there should be a delta_v(max) in the denominator. Second (and probably even more important in a quantitative sense), I don't think that the end member for the background conditions during North Atlantic advection equals the value from the nighttime measurements. As evident from Fig.5, in this case the PBL still has a certain extend during night, and the relative humidity is well below 100%, both pointing to a contribution of local evaporation also during nighttime. A dryer and more depleted end member would increase the nominator in equation 5 and thus yield a larger local contribution.

Minor comments:

The notion "twenty-four average hourly data" applied for the daily cycle analysis (e.g. in the abstract, but also throughout the paper) is very confusing.

P 1704, L 22: "entrainment of free atmosphere" is awkward; I also think that this issue has only been treated in a very implicit way and should thus not be mentioned in the abstract.

P 1705, L 25 ff: Isotope-enabled models are used for various studies, not only of land-atmosphere interaction. The number of references could be greatly reduced, as modeling does not play any role for this study.

P 1706: Another study presenting high-resolution isotope measurements from Europe is Iannono et al. (2010).

P 1711: If possible, please provide references for the HYSPLIT model and GDAS dataset. The wording "parameterized to end up" is unclear (probably you have just started the trajectories there and performed the calculation backwards in time). Furthermore, such simple trajectory calculations cannot be used to "get the oceanic source" of moisture, for which there are much more sophisticated methods (e.g. as

used in Pfahl and Wernli, 2008). Please be more specific on how you perform the source corridor attribution of the air masses.

P 1712: Jacob and Sonntag (1991) used data from Heidelberg, Germany.

P 1713, L 19 ff: I don't think that this is the reasoning of Welp et al.; they just argue that a correlation between d and locally measured RH implies local moisture sources. As noted before, RH at the moisture source, not as an "air mass property", is important for d variability.

Section 4.1.2: As noted before, simply calculating backward trajectories is not enough to determine moisture sources. Nevertheless, the trajectory analysis is useful for determining the prevailing air mass advection that can (as demonstrated by the authors), e.g., affect local relative humidity and surface evaporation. This should be made clearer, also in connection to Table 5 (where it should be clearly stated that the parameters represent local conditions during the advection of specific air masses, and not the conditions at the moisture source).

P 1714, L 17: I don't think that rainout necessarily occurs along these trajectories. Already climatologically, the vapor from northerly air masses is more depleted.

Section 4.1.3: The very scarce rainfall measurements do not contribute a lot to the merit of this study. In particular, I cannot identify any temporal trend from these data (cf. P 1715, L 15). The point that precipitation d is lower than d in vapor could also be made in one sentence.

P 1717, L 7: Why should this transition always happen in the evening? May this also reflect variations in the timing of the PBL evening transition?

P 1717, L12-13: I don't understand this sentence. Does "subsequently" imply a temporal relationship?

P 1717: I think the differences in PBL heights between North Atlantic and Mediterranean advection are mainly related to the vertical temperature structure: Northerly advection transports a relatively cold air mass over a warmer surface, leading to a weaker stratification (and the other way around).

P 1718, L 6: "... do not directly depend on climatic factors": I don't understand this.

P 1718, L 13: Why should these processes affect d in the same way?

Sections 4.2.3 and 4.2.6: The notation with respect to q/Q is very confusing. Does q represent a mixing ratio or a flux (as stated on page 1719)? What is the difference between q and Q?

Section 4.2.3: The determination of the end member ("surface conditions") in the mixing model uses a linear relationship to estimate isotope values far away from the humidity range in which data are available (see Fig 6). Such a procedure is associated with substantial uncertainties, which should definitely be quantified (by taking into account the uncertainties of the linear regression). I suppose that this yields large error bars particularly for deuterium excess.

P 1722, L 1-6: This passage is particularly difficult to understand.

References:

Craig, H., and L. I. Gordon (1965), Deuterium and oxygen 18 variations in the ocean and the marine atmosphere, in Stable Isotopes in Oceanographic Studies and Paleotemperatures, edited by E. Tongiorgi, pp. 9-130, Lab. Geol. Nucl., Pisa, Italy.

Iannone, R. Q., D. Romanini, O. Cattani, H. A. J. Meijer, and E. R. Th. Kerstel (2010), Water isotope ratio (d2H and d18O) measurements in atmospheric moisture using an optical feedback cavity enhanced absorption laser spectrometer, J. Geophys. Res., 115, D10111, doi:10.1029/2009JD012895.

Pfahl, S. and Wernli, H.(2008): Air parcel trajectory analysis of stable isotopes in water vapor in the eastern Mediterranean, J. Geophys. Res., 113, D20104, doi:10.1029/2008JD009839.

Uemura, R., Matsui, Y., Yoshimura, K., Motoyama, H., and Yoshida, N. (2008): Evidence of deuterium excess in water vapor as an indicator of ocean surface conditions, J. Geophys. Res., 113, D19114, doi:10.1029/2008JD010209, 2008.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 1703, 2015.