Review of

"Annual variation in precipitation δ^2H reflects vapour source region at Barrow, AK"

by A. L. Putman et al.

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1 General Comments

This paper presents an interesting dataset of the event-scale δ^2H and deuterium excess signature of precipitation from northern Alaska. The authors use a very simple back-trajectory-based analysis of the transport and moisture source conditions which they summarise in 3 main characteristics to interpret their data. These are 1) the moisture source dew point temperature at 2m, 2) the total cooling between the lifted condensation level at the moisture source and the precipitation level in the cloud at the measurement site (arrival temperature) and 3) whether the air parcels that are transported to the measurement site across the Brooks and/or the Alaskan ranges.

I recommend publication of this overall well-written manuscript, but I have four major concerns that should be addressed beforehand as well as a many specific comments listed below:

1 **Moisture source identification and particularly the implicit assumptions made:** see specific comments 3-7.

2 **Choice of the parameters that explain the variance of the isotope signature of precipitation in Barrow:**
For me the choice of the parameters that were used to explain the precipitation isotope signal in Barrow seems random. It makes sense to look at moisture source and transport conditions but in my opinion there is no reason for completely neglecting the local conditions. Particularly at Barrow, the precipitation phase (liquid or snow) probably plays an important role for the end isotope composition of the precipitation event as it determines whether there is isotopic exchange (for rain drops, see specific comment 2) or not (for snowfall) with the local vapour. Also precipitation intensity plays an important role. The authors have some detailed information about the precipitation structure from their radar data and could use this to try to further understand the local processes. If this is done in an other paper, then this should be clearly stated. Also I do not fully support the choice of the variable T_d as representative for the moisture source conditions (see specific comment 12).

3 **Expansion of the northern polar circulation cell and its link to moisture source location**
The link between the event-based moisture source location of precipitation and the polar circulation cell is described in a very qualitative way. A link between the weather systems driving the moisture transport at the event timescale leading to precipitation at Barrow and the more climatological description of the polar circulation is not obvious and not trivial to make. The formulations used throughout the paper should be more careful and kept as hypotheses.

4 **Critical discussion of results in view of the existing literature**: in particular see specific comments 24 and 27.

2 Specific comments

1. p. 1, title: It would be nice to include in the title the fact that it is event-scale precipitation samples that the authors analyse in this paper. Something like: "Annual variation in event-scale precipitation δ^2H reflects vapour source region at Barrow, AK". Also Barrow, AK could be replaced by northern Alaska.

2. p. 18-23: The local conditions during cloud formation and during precipitation also play an important role for the isotope composition of precipitation. For rainfall for example below cloud effects (evaporation and exchange with ambient vapour) can have a strong impact on the isotope composition of precipitation (20-40‰ for δ^2H, see Pfahl et al. (2012), Aemisegger et al. (2015)).

3. p. 3, L. 29: The reanalysis dataset (wind fields) that is used for the trajectory calculation should be mentioned here as well as its horizontal resolution.

4. p. 4, L. 2: What do the authors mean with "The first time"? Is the time reference forward or backward? Does that mean the first time when following the trajectory back from the arrival point? And does that mean that one trajectory can have only 1 associated moisture source? This would be a very strong assumption about the moisture source location. Uptakes of moisture can happen all along an air parcel's trajectory (see Sodemann et al. (2008)) and they can sometimes be linked to surface evaporation even though they are not in the boundary layer (PBL), particularly over land. If for each trajectory only the latest passage in the PBL before arrival at the measurement site is considered then this means that the authors assume very strong mixing. This would imply that the air parcel basically looses all its previous humidity by mixing out and takes up only humidity that has just been evaporated at this location. The isotope signature of the air parcel thus is fully determined by the freshly evaporated water. This strong assumption has to be explicitly stated.

5. p. 4, L. 4: The authors say that air parcels that sank below the PBL over land were ignored? Why then do they find a lot of moisture sources over continental Alaska in Figure 1? This is confusing.

6. p. 4, L. 4: Were 71% of all trajectories ignored or kept for the analysis?

7. p. 4, L. 5-12: For me it is not entirely clear how the trajectory starting dates were chosen. Why do the authors choose only a three hours period instead of the whole precipitation event? Why are the individual dates not weighted by the locally measured precipitation intensity to take into account that when the precipitation intensity is higher the trajectories of that date contribute more to the isotope signal? What means the "most homogeneous" three-hour time window? And why with preference to the "middle" of the event? The selection criteria should be more oriented to the quantitative contribution of moisture to precipitation in my opinion.

8. p. 4, L. 13: Does "where" mean the starting altitude? The method that is shortly described in this paragraph sounds original and the idea is interesting but it assumes that the reanalysis dataset's wind field and precipitation rate profile are equivalent with the true fields. The reanalysis data error particularly with respect to the representation of small and microscale processes are ignored. Starting trajectories from different locations around the measurement site would allow to take into account the uncertainty arising from the reanalysis data

9. p. 4, L. 25: How did the authors calculate $\bar{T}_d$ and are the average moisture source conditions computed as an arithmetic mean without taking into account the evaporative contribution to the air parcel's humidity at the different source locations?

10. p. 4, L. 30: The authors should make clear that their ΔT_{cool} is only an estimate of the total cooling that the air parcel has experienced. The same remark for the possibility of multiple moisture sources for one air parcel (see specific comment 4) is valid for cooling and precipitation along an air parcel trajectory. A trajectory can produce rain all along its path and can go through several cycles of cooling and warming. The total cooling would be obtained by integrating the temperature changes along the trajectory.

11. p. 5, L. 3-9: this way of computing T_{LCL} is confusing for me. Where does Eq. 2 come from? See Bolton (1980) and Lawrence (2005).

12. p. 5, L. 12-16: The idea to use T_d as a summary variable for both relative humidity with respect to sea surface temperature ($h^{2\,\mathrm{m}}_{\mathrm{SST}}$) and SST-effects seems not justified to me from a physical point of view. The influence of SST on T_d is only indirect and a strong coupling of the ocean surface conditions with near-surface air characteristics is not necessarily given particularly at the event timescale. From a theoretical perspective and for all isotope-enabled numerical modelling experiments it is the Craig-Gordon model and thus the other two variables that are used to determine d of the fresh evaporate. So I am not convinced that it is sensible to introduce a third variable that does not contain more information than the specific humidity at 2 m. Furthermore, it should be made clear in the manuscript that it is not the 2 m relative humidity that is important for the non-equilibrium fractionation part during surface evaporation but the humidity gradient towards the surface which is represented by the relative humidity at 2 m with respect to sea surface temperature ($h^{2\,\mathrm{m}}_{\mathrm{SST}}$). The authors should make a stronger case for why they use T_d rather than the classical variables. Also the sentence "T_d depends on the specific humidity of saturated air at the sea surface and on the

amount of dry air from aloft that has subsided and mixed into low altitude air" is a confusing statement.

13. p. 5, L. 17: Where does Eq. 3 come from? What is the impact of the simplification involved, the authors should add a chapter reference to Stull (2015). Why did they not use Stull (2015), Equation 4.15a or b or extract directly T_d from the reanalysis dataset?

14. p. 5, L. 23: How was *mtn* defined? Using an objective criterion or subjectively by looking at the trajectory plots?

15. p. 6, L. 2: remove parentheses.

16. p. 6, L. 10-11: It would be useful to add the geographical names in one of the panels in Figure 1.

17. p. 6, L. 16-20: Is it really the variation in the moisture source latitude that is relevant or the mean transport distance? I am not convinced about the role of Figure 2. Also see major comment 3.

18. p. 6, L. 21-32: For me this relatively long paragraph is a general discussion of the possible link between polar atmospheric circulation and the location of vapour sources and not a result from this study. Either the link with the findings in this paper should be illustrated more clearly or this section should be strongly shortened or even left out. See also my general comment 3: the link between the different timescales that are involved here is not trivial to make at this stage, a more open formulation should be chosen here.

19. p. 6, L. 24-26: In Europe several studies found that during summer the regional moisture recycling and the contribution from continental evaporation is much more important than in winter (see Sodemann and Zubler (2010) and Aemisegger et al. (2014)). Even though on p.4 L.3 the authors say that "only trajectories that sank into the PBL over the ocean" a substantial contribution of evaporation from continental Alaska is found in Spring but also in the other seasons in Figure 1. This possible contribution of continental evaporation should also be discussed as its moisture source isotope signature is different than the one from ocean evaporation.

20. p. 6, L. 3: Add mid- to high latitudes here, other studies could be cited as well (e.g. Bonne et al. (2014))

21. p. 7, L. 10-11: References to figures are confusing.

22. p. 7, L. 13: Do the authors mean the regression slopes? It would be useful to add the units of the slopes in all tables. Also in Table 3 it would be useful to add the explanation on what β and S.E. are.

23. p. 7, L. 27: Here and elsewhere the references should be listed chronologically.

24. p. 7, L. 34 - p. 8, L. 10: Here more detailed explanations on the theoretical cooling/Rayleigh experiment are needed to be able to follow. Also the sensitivity range of δ^2H to the diagnosed cooling should be put into context and compared to literature values.

25. p. 7, L. 21: Table 1: do the regression slopes from Table 1 result from multiple linear regression?

26. p. 9, L. 23: "within storm" is a confusing term here as it suggests that the precipitation is due to the passage of a cyclone, which is not always the case. I would suggest using "intra-event" instead.

27. p. 10, L. 17: I am surprised at the d–h slope which is not at all in agreement (opposite sign and different order of magnitude) with other literature values ($\sim$-0.6‰%$^{-1}$ to -0.32‰%$^{-1}$, though a difference with literature values is that $h_{2\,\mathrm{m}}$ is used and not $h^{2\,\mathrm{m}}_{\mathrm{SST}}$). This mismatch should be explained and the relevant literature should be cited (Pfahl and Wernli, 2008; Steen-Larsen et al., 2014; Aemisegger et al., 2014). Also the d-SST regression slope is of opposite sign to what we would expect from the Craig-Gordon model.

28. p. 10, L. 20: What is the theoretical expectation for the sign of the correlation between d and T_d? This should be explained in more detail. I do not agree with the statement made here, I would expect a negative d-T_d slope from theory since the physical relation between relative humidity and T_d should generally lead to a positive correlation between the latter two (see e.g. Lawrence (2005)).

29. Figures 3 and 4: more details are needed on the used spline fits. Also the strong inter-event variability that is sometimes of similar amplitude as the seasonal cycle should be discussed.

30. Figure 5: the role of this Figure is unclear to me, it is only referenced once and not further discussed in the text. Either this Figure should be better embedded in the text or it should be left out. If it is kept: is this figure an average over all events?

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