Review of

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

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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:

Thanks to the reviewer for the useful points and ideas. We have considered the suggestions, addressed the questions and revised the paper accordingly, and we hope that the revisions are satisfactory to this reviewer.

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

The authors argue that the method employed in this paper is adequate for our purpose. Please see our responses for comments 3-7 for the full discussion.

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).

This is a good point, and one that was considered by the authors before settling on the variables reported. Indeed, half the variance in d2H cannot be explained by the 3 variables chosen! The main reason that other variables (including but not limited to precipitation phase, sub-cloud dryness, precipitation intensity, evaporation below the cloud base, supersaturation in the cloud, and storm event type) were not included is because, the statistical power of the limited number of events we were able to consider is not sufficiently high to go after each of those potentially very important variables. When such variables were included in the analysis, they did not explain any more variance. This may be because the isotopic responses to them are not related to d2H variations, or are related but not sufficiently above noise. For example, sub-cloud dryness may be important for some but not all events, dryness may occur during both high and low d2H events, but the power or the size of the signal may be limited. Nevertheless, to respond to this point, we added a paragraph at the end of Section 3.2 (p. 11 l. 13-20) that includes a list of variables potentially contributing to the 46% of the unexplained variance in d2H.

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.

The authors acknowledge that the relationship between vapor source and circulation is not simple, and the link between the annual and longer timescales is a possibility, not a certainty. However, the work does substantiate the idea that isotope values measured in ice cores may reflect changes in circulation patterns as well as local temperature, which is how they are often interpreted. The phrasing of these statements has been re-formulated in all discussion to suggest hypothetical as opposed to likely links. (p. 7 l. 17-23, p.13, l. 9-12)

4 **Critical discussion of results in view of the existing literature**:
in particular see specific comments 24 and 27.
More discussion has been added to Sections 3.2 and 3.3. In 3.2, which discusses the influence of vapor source on measured precipitation isotopes, greater clarification of the simple Rayleigh model used to contextualize our results has been added, as well as a comprehensive sources of error paragraph (p. 11 l. 13-20) and an expanded discussion of the utility of Td (p.9 l. 7-35, p 10 l 1-10) in characterizing the source. In section 3.3, the d-excess results are discussed in greater depth in light of the suggested papers (p. 11, l. 31). In particular, we have added discussion of the relationship between local water vapor and evaporation conditions (p. 12 l. 13-17).

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 _2H reflects vapour source region at Barrow, AK". Also Barrow, AK could be replaced by northern Alaska.
Title changed as suggested.

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 (20-40h for δ_2H, see Pfahl et al. (2012), Aemisegger et al. (2015)).
This is absolutely true, and we did experiment with including condensation temperature, precipitation type, and sub-cloud humidity in our regressions. The regression presented was the best model in terms of simplicity and variance explained by different parameters. One reason why these local factors may not have been significant influences to our dataset is because of event-to-event variability. e.g., in one case enrichment may be due to sub-cloud evaporation, but in another it may be due to condensation temperature, and within our dataset we did not have the statistical power to disentangle these competing mechanisms. The text has been updated on p. 11 l. 13-20. Also see our response to General Comments 2).

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.
The information has been added on p. 4 l.3-4.

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.
The 'first time' is in reference to back trajectories. Wording in the manuscript has been updated for clarity on p.4 l. 8-14. Yes, each trajectory has one associated vapor source. Though the method described in Sodemann (2008) is a substantially more sophisticated way of identifying the vapor source, it is not necessary in our work for three reasons. 1) For a given parcel, the spatial range over which the parcel moves up and down across the PBL, is small compared to the region covered by 1000 total parcels of an event. The latter is primarily dictated by the vertical distribution of the initial parcels' altitude 2) Our work analyzes the influence of marine source areas on Barrow precipitation. Marine surface conditions are relatively homogeneous, which point strengthens the argument in 1). 3)

Averaging at the precipitation site of condensate of 1000 trajectories from a wide spatial distribution of source locations implicitly accounts for mixing of moisture from distributed source locations. It is thus in effect equivalent to the more sophisticated Sodemann et al. (2008) model which used on average only 2.6 trajectories per column of air per time window, even if each of those trajectories combined source points at its inception.

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.
The difference between data used for statistics and data used in figure is confusing. Text is added that notes that all trajectories are shown in the figure, but only specific trajectories are used for the calculations (p.4, l. 12-14). This is noted in the figure caption as well.

6. p. 4, L. 4: Were 71% of all trajectories ignored or kept for the analysis?
Changed to 'Ocean originating air parcels' to clarify (p. 4 l. 15)

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.
The three hour time period was chosen so that each event was treated the same. In some cases, multiple 3 hour windows were analyzed, and typically showed very similar results, so we concluded that choosing one three hour window was representative of the whole event. Selection of the specific three hour time period was not performed quantitatively. The qualitative selection used returns from the MMCR and KAZR. Higher Doppler vertical velocity and reflectivity indicate increased precipitation intensity. These criteria, in conjunction with surface analysis maps were used to determine the start and end times. The three hour window reflects the constraints of the reanalysis, i.e. the temporal resolution is three hours. For all of these questions, clarification has been added to the text on p. 4, l 15-24.

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
Yes, "where" means altitude. This is clarified in the text (p. 4 l. 25). Yes, wind direction issues are very important for back trajectories, However, if winds are incorrect, incorporating a wider area will not make them more correct. Furthermore, because the resolution of the reanalysis data is 1x1 degree, using multiple locations may cover a wide region, hundreds of kilometers in size. Such a wide spatial scale could be less representative of local or small-region precipitation events, though it could be helpful for large precipitation events. We are not convinced that looking over a larger spatial location would improve the vapor source estimation. However, sending a large number of air parcels (~1000), as we have done, helps to deal with the wind issue. Wind errors in the reanalysis are a potential source of error for any Lagrangian back trajectory study, and are not unique to our study. Therefore, we hope people who are reading studies using Lagrangian back trajectories are in general cautious with this type of reanalysis product.

9. p. 4, L. 25: How did the authors calculate 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?
Because of the way the condensation profile is divided, it is assumed that each parcel contributes an equal amount of vapor to the final precipitating cloud, so we did not weight them. The calculation of Td is described in detail on p. 6 l. 3-12.

10. p. 4, L. 30: The authors should make clear that their T_{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.
The reviewer is correct, the cooling indicated by Tcool is the net cooling, not the integral of cycles of warming and cooling an air parcel may have experienced along its trajectory. This is a simplification. The following has been added to the text to clarify this point on p. 5 l. 10-11.

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).
The equations have been changed to those in Stull (2015). The previous equation was a linearized approximation that introduced minor, if not insignificant, discrepancy. To be more precise, the equations, calculations, and text have been updated to be consistent with Stull (2015), though the results and discussion require no change. Equations 2, 3, and 5 are affected.

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 ($h_{2m\ SST}$) 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 2m 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 2m with respect to sea surface temperature ($h_{2m\ SST}$). 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.
The idea of using Td is to indicate the moisture condition PBL, the moisture that forms the first condensate. This is not the same as the evaporative flux predicted by the Craig-Gordon model. Our group has done a significant amount of work to model and understand isotopic variations in the marine boundary layer (manuscripts in preparation), and our understanding continues to improve. We realized that our discussion about Td in the earlier version was not clear, and it is valid for this reviewer to solicit further explanation. We have completely rewritten section 3.2 that pertains to Td (p. 9 l. 6-35, p. 10 l. 1-9). Basically, the Craig-Gordon model only predicts the evaporative flux, not the vapor properties in the PBL. In addition, the Craig-Gordon model does not consider effects of convection on vapor isotopic ratios in the PBL. However, convection is very important process that 1) transports PBL air to the free troposphere, and 2) brings dry air from aloft to the PBL. The boundary layer air is therefore a mixture of evaporated vapor from the ocean surface, and the dry air from aloft. The extent of this mixing within the PBL is reflected by (2m) dew point, Td. Td is also important for indicating the evaporation condition in that it is more directly related to relative humidity with respect to the sea surface temperature than is the 2 m relative humidity. We hope the new discussion in the revised version is clearer.

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?
Calculation was done with 4.15b in Stull (2015).

14. p. 5, L. 23: How was *mtn* defined? Using an objective criterion or subjectively by looking at the trajectory plots?
Removed 'for the event, not to individual trajectories' and added 'observed in trajectory plots' to clarify how mtn was defined.

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

16. p. 6, L. 10-11: It would be useful to add the geographical names in one of the panels in Figure 1.
A nice idea, but would obscure the data presented in the plot because it would be too busy.

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.
Because most vapor transport is from mid-latitudes to high latitudes, latitude is actually relevant for this site. Yes, distance might be another reasonable metric to investigate. However, latitude was chosen because latitude covaries with evaporation conditions, so it's more physically useful than distance.

Figure 2 is useful as it shows the relationship of latitude to vapor source and distillation. A similar figure could have been made for distance, but the outcomes would be very similar, as in our case distance variation is roughly the same as that of latitude.

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.
The link between seasonal changes to general circulation and seasonal change in vapor source makes sense because general circulation is the background pattern from which weather events deviate. This paragraph only links the seasonality of vapor source with the seasonality of circulation patterns- nothing over longer timescales. However, the language has been updated to be less causal. (p. 7 l. 17-23)

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.
Local evapotranspiration during spring and summer is likely an important vapor source. However, given the heterogeneity of the event conditions and sources, we did not have the statistical power to pull evapotranspiration out relative to the other factors. Nonetheless, in the discussion, evapotranspiration is added as a third potential mechanism of seasonal change (p. 7 l. 9-14), as it likely does contribute to the d2H measured in precipitation. Ignoring continental air may also contribute to the unexplained variance in the multiple regression in Section 3.2. We included discussion of this factor in the new version at the end of section 3.2 (p. 11 l. 13-20)

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

21. p. 7, L. 10-11: References to figures are confusing.
The references to figures were removed.

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.
Changed to regression slopes. The units, originally just of the variable, are now the units of the slope in all tables. Beta and S.E. are described in the text that refers to Table 3.

23. p. 7, L. 27: Here and elsewhere the references should be listed chronologically.
Checked references throughout manuscript and reordered where necessary.

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 _2H to the diagnosed cooling should be put into context and compared to literature values.
The model is explained in greater detail given the following: 'In the simple model a saturated air parcel with a specified temperature and vapor d2H is cooled by 1C steps. At each temperature step the condensation amount, remaining vapor, d2H and vapor d2H are calculated. No re-evaporation or non-equilibrium conditions are considered.' The simple model is meant to contextualize the numbers. (p. 8 l. 27-29)

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

Yes, this is stated in the text (original p. 7 l. 11) though it has been added to the Table caption now. The regressions in Tables 2 and 3 are now clearly indicated as simple linear regressions.

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.
Changed to intra-event.

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 (d-0.6h%$^{-1}$ to -0.32h%$^{-1}$, though a difference with literature values is that h_{2m} is used and not $h_{2m\ SST}$). 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.
We too were also surprised at the outcome. Efforts were made in the text to explain it, focusing on the effect of the larger-scale humidity gradients and potential mix-phased cloud effects. However, considering that evaporative condition is only one process controlling the vapor properties in the PBL, as we explained earlier and also in the revised manuscript, this result is not entirely unreasonable. See our response for 28 below.

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)).
Our mistake. Thank you for catching this. We now discuss this negative relationship in the context of two processes, 1) dry air (low Td) causing larger kinetic fractionation and higher d, and 2) the descending air may have high d values. Both processes, independently and together yield negative association between d and Td. We now also state that this result is consistent with our argument that Td is more representative of the PBL conditions than are Tss and h. (p. 12 l 13-17)

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.
Details on the spline fits have been added to the figure caption, and the similarity in amplitude among seasonal and event variability is noted for both datasets. (Figs 3 and 4, and p. 7 l. 29-34)

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?
Yes, this figure shows the average value of d2H of precipitation coming from a specific vapor source, which indicates that certain regions tend to be vapor sources for precipitation events that are either more or less enriched than average for that time of year. Mountains along the trajectory appear to be the mechanism at work in producing the spatial structure. The figure and its meaning are also discussed on p. 8 l. 14.

References

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All of these suggested references have been checked and cited when appropriate.