I would like to thank the authors for their detailed response to my first review. Most of my concerns have been addressed in a satisfactory way. However, I have three remaining points that need to be more carefully discussed in the manuscript and that I strongly recommend to consider before final publication:

1) The use of Td as a physically meaningful moisture source variable: the author's answer and the respective changes to the manuscript following my earlier comment 12.

Several statements with respect to the use of Td as a relevant moisture source variable are very confusing or physically wrong. I list my concerns below. I copied the authors' changed text (blue) and added my comments to it (green).

The use of Td and related discussion in this paper reflects that our group has done a substantial amount of work to model and understand isotopic variations in the marine boundary layer (manuscripts in preparation), and our understanding continues to improve. We realize that our discussion about Td in the earlier version was not clear, and it is valid for this reviewer to solicit further explanation. For clarity for the reader, we have completely rewritten section 3.2 that pertains to Td, and we hope the new discussion in the revised version is clearer.

In short, the idea of using Td is to indicate the moisture conditions within the PBL, as this is the moisture that forms the first condensate. This is different from the evaporative flux predicted by the Craig-Gordon model. In addition, the Craig-Gordon model does not consider effects of convection on vapor isotopic ratios in the PBL. However, convection is an 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 useful because it is directly related to relative humidity with respect to the sea surface temperature (h2m,SST), moreso than is the 2 m relative humidity. Indeed, when h2m, SST was used in the multiple regression instead of Td, it was a significant predictor of δ2H. However, in both variance explained and AIC, the multiple regression that incorporated Td performed better. Both because it performs better in the multiple regression, and because it is

a measurable quantity, we prefer Td to h2m,SST and have retained it in the paper.

Indeed the Craig-Gordon model does not parametrize effects of convection or boundary layer mixing but this is not its role. The extent of the boundary layer mixing is reflected implicitly by the 2 m dew point but similarly in all other humidity variables near the surface including the humidity gradient towards the ocean surface summarised by hsst (which uses the dew point at 2 m and the saturation vapour pressure at SST). The 2 m dew point is in essence equivalent to the absolute humidity of the air parcel and contains no more information than the specific humidity. Furthermore, Td at 2m is not more directly related to hsst than to h2m, the reference saturation specific humidity is a different one in the two cases. For me the only useful argument that should be mentioned in the text as to why Td 2m could be an interesting variable to look at, is that it can be directly measured. A better performance of Td in the author's regression framework alone is not a good argument for using it.

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'We prefer Td to the classical variables Tss and h for determining isotopic evaporative fluxes. This choice is based on our understanding that the meteorological variable Td characterizes the bulk vapor content and isotopic ratio of the marine PBL, independent of the vapor temperature. When advected to the free troposphere, it is this vapor that will form precipitation. Additionally, through equilibrium fractionation Td also determines the isotopic ratio of the first condensate at the LCL, where Rayleigh distillation begins.

This paragraph is very confusing: 1) It is not clear that Td is Td at 2m, 2) I do not agree that Td at 2m characterises the bulk vapour content and isotopic ratio of the marine PBL, this is a very much simplified view 3) Neither the temperature nor the dew point temperature along an air parcel trajectory can be assumed to be conserved. The humidity of the trajectory (and thus also the dew point temperature) changes due to mixing and rain out. The authors did not look at the first condensate along the trajectory in their analysis and did not consider rain out along the trajectory explicitly.

Within the marine PBL, several inter-related factors/processes are at work to determine the starting point of a Rayleigh trajectory. What is meant by Rayleigh trajectory?

The first is the isotopic flux of evaporation from the sea surface. Most studies estimate this flux using the classic model by Craig and Gordon (1965). In that model, three variables control the evaporative flux: the sea surface temperature, Tss, δ2H above the laminar layer, and the humidity hss above the laminar layer (e.g., at 2m), defined relative to Tss. In essence hss is a humidity gradient just reformulated and expressed in the form of a relative humidity (fraction). Additionally the diffusivity of 2H is needed in the Craig-Gordon model for the non-equilibrium fractionation factor (alphak) and depending on which formulation of alphak the 10 m wind speed. Also the isotope composition of the ocean water is needed but can be approximated to be constant at 0‰.

Though hss is not a measured quantity nor one that is normally modeled, it is determined by Td above the laminar layer and Tss. Not a directly measured quantity that is true but calculated from 2 directly measurable quantities (dew point temperature and SST). And it is of course normally modelled since all numerical models use it (although in the form of a gradient) in their surface latent heat flux parameterization.

Hence isotopic fluxes can be determined with the classical model using Tss, and Td and delta_2H above the laminar layer as input variables. From a physical point of view, Tss determines the amount of equilibrium fractionation at the water-air interface. Td and vapor δ2H, as well as Tss, control kinetic fractionation as vapor diffuses across the laminar layer. I agree with this.

It should be noted that when Tss is large, Td tends to be large as well, as a result of their change with latitude and season. This is a very general statement and might be true at long (>monthly) timescales but not at the event timescale. Td at 2 m varies strongly at the synoptic timescale whereas Tss does not.

Td and δ2H are also correlated, which will be discussed below. Therefore, all three variables controlling the evaporative flux, Tss, and hss and δ 2H above the laminar layer, are associated directly or indirectly with Td, making Td a good indicator of evaporation conditions.

The second process is convergence. At a moisture source location, low level air is moist due to evaporation near the sea surface. Convergence and uplift transports low-level moist air into the free troposphere where it mixes with dry, isotopically depleted air descending from surrounding regions resulting in strong humidity and temperature gradients near the sea surface (below 2 m). "Convergence" is misused in this context in my opinion.
In contrast, the specific humidity and isotopic ratios in the bulk of the PBL above 2m are relatively constant, resulting from the relative contributions of vertical transport of moist low-level and descending air (Fan, 2016). Td and δ2H at 2m both reflect the outcome of this mixing process, and so it follows that they are positively correlated. I agree that we expect positive correlation between Td at 2m at the source and δ2H. But I do not understand what the authors exactly mean to imply with process 2.

The third process is condensation at the LCL. The temperature of the air mass, which equals or is very slightly less than the local dew point, determines the amount of isotopic fractionation and thus the isotopic ratio of the first condensate. Why is the temperature of the air mass equal the local 2m dew point? What is meant by local? At the moisture source? So, do we have saturated conditions at the moisture source all the time? I would rather expect an air mass temperature that is higher than Td2m except in the case of fog/in a cloud. Td at LCL is not equal to Td2m unless the air parcel has not experienced any humidity change since its last passage in the boundary layer, which is very unrealistic.

It is this isotopic composition that defines the beginning of the Rayleigh part of the trajectory. Only Td;2m, not Tss nor h2m, is directly associated with the condensation temperature at the LCL (which differs only slightly from Td;2m due to the pressure difference between 2 m and the LCL and its effect on saturation specific humidity). This is confusing. What do the authors mean by the condensation temperature is directly associated with Td2m? Td2m and TdLCL are 2 different variables.

Since all three processes before Rayleigh distillation are either directly or indirectly related to Td, we consider Td a better indicator for the source conditions than either Tss or h. I do not agree with this statement. Process 1 is reflected in all moist variables, process 2 as far as I understood what the authors mean (boundary layer mixing) as well, and process 3 is in my opinion irrelevant in this discussion concerning the physical reasons for choosing Td as representing the moisture source conditions.
It is difficult, however, to theoretically assess the sensitivity of precipitation δ2H to variations in source Td, because this would require quantification of the theoretical relationship of Td to δ2H through each of the three processes and perhaps their combinations. We here report the first empirical sensitivity of 3.23‰ °C-1 (Table 1) for δ2H relative to Td. At the sea surface, for Tss between 0 and 25 °C, equilibrium fractionation as a function of temperature yields sensitivities between 1.1-1.6‰°C-1 (Majoube, 1971). However, a large part of this fractionation may be offset by condensation at the LCL. Consequently, the observed

I do not agree with 2, δ2H of the evaporation flux (δ2He) becomes more enriched with decreasing δ2Hv due to the isotope gradient. See the Figure below, x-axis represents δ2Hv, y-axis δ2He from ocean evaporation as computed using the Craig-Gordon model, the equilibrium fractionation factor from Majoube, 1971, the non-equilibrium fractionation factor from Merlivat and Jouzel, 1979, a wind-speed of 6 ms-1 and a sea surface temperature of 15°C. In blue the δ2He(δ2Hv) relation for a hsst of 80%, in black hsst=60% and in red hsst=40%. The lines intersect at δ2Hv=~-83‰ which is the equilibrium vapour equivalent of ocean water (0‰). In this situation (δ2Hv=~-83‰), there is no isotope gradient or humidity gradient effect.
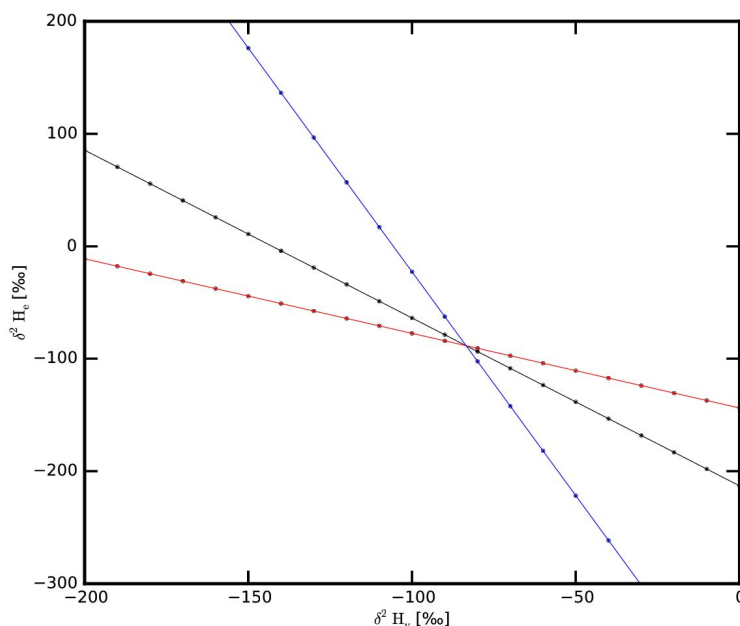
I recommend careful revision of this text. In my opinion it can also be shortened substantially. The authors need to make it clear that a) Td at 2m is used, b) that Td at 2m is not necessarily equivalent to Td at LCL and along the trajectory, the discussion around Td at LCL is not relevant in this part which focuses on the moisture source processes and not the transport and rain out along the air parcel's trajectory, c) Td at 2m is used because it is a measureable quantity, equivalent to using specific humidity at 2m and as such it partly reflects the classically used moisture source parameters. The physical process linking Td and hsst being that strong ocean evaporation occurs when there is a strong humidity gradient towards the ocean surface, that is when hsst is low. A strong humidity gradient at the synoptic time scale is very often achieved through advection of cold dry air over the ocean surface, that is when specific humidity at 2m is low as well.

2) The simplifications involved in the used moisture source diagnostics compared to the more detailed method of Sodemann et al. 2008 should be mentioned explicitly in the manuscript (my previous comment 4): 1) The method adopted by the authors assumes that an air parcel is not further back-traceable once it has been located in the boundary layer, 2) the authors assume very strong mixing in the boundary layer and a dominant effect of recent evaporation on the humidity in the boundary layer (since all humidity taken up by the trajectory is assumed to have been evaporated in the last model data time step and at this location), 3) This strongly enhances moisture sources that are located close to the the measurement site and neglects more remote source locations. These 3 points should be mentioned in the manuscript.
Only considering the latest passage of an air parcel in the boundary layer would account for 5-30% in rare cases up to 70% of the final specific humidity of an air parcel in the framework of Sodemann et al. 2008. Even if one argues that a trajectory is not further back-traceable once it has been in the boundary layer it is rather simplistic to assume that the air parcel has taken up all its humidity from surface evaporation at its latest position in the boundary layer.

3) The proposed method for defining the starting points of the trajectories at the measurement site contains a conceptual gap (see my previous comments 8 and 9): the observational and model worlds are interweaved without a thorough validation. It is not in the scope of this paper to show that the used reanalysis data show realistic condensation rate profiles at the observational site. But it should be clearly stated that it is assumed that the reanalysis' data representation of the rain out process during an event is consistent with the observations from the cloud radar. For me this is an important source of uncertainty in the presented method and should at least be explicitly mentioned.

After the requested changes with respect to the assumptions and implications of the chosen method have been made, I recommend publication of this overall very nice and interesting manuscript.