Review of "Spatial variability of northern Iberian rainfall stable isotope values: Investigating climatic controls on daily and monthly timescales"
by Moreno et al.

This paper presents a set of multi-year time series between 2010 and 2017 of daily isotope data in precipitation from six station in the northern Iberian Peninsula. This high-quality dataset is very valuable for the isotope community, in particular because it is compiled at high temporal (daily) resolution and comprises multiple years with simultaneous data from several stations. In addition to the very valuable dataset, which could also be submitted to a dedicated data journal such as ESSD, this paper discusses several relevant meteorological factors for the large day-to-day variability in the collected isotope data. As the authors write in their introduction, an in-depth understanding of the regional climatic controls on modern precipitation water isotopes is still missing, but is of key importance for a better understanding of terrestrial climate proxies. The interesting and important discussion on the climatic controls of the isotope composition of precipitation is what makes this paper fit in principle into the scope of ACP. However, a substantial effort is needed to address several important structural and methodological weaknesses in this paper. I consider this paper acceptable for publication if the following major comments can be addressed adequately:

1) **Shorten and strengthen the structure of the discussion on the climatic controls**: This paper is rather long and there is frequent repetition of facts that are in my opinion of (very) minor importance such as the difference in oceanic surface water isotope composition that is mentioned at lines 292ff, 309ff, 369ff, 387ff, and 442ff. As the authors mention themselves, I believe the effect of such small differences (order of 0.5‰) in $\delta^{18}O$ has a negligible impact on precipitation isotopes. Much rather, I think, the authors should mention differences in moisture source conditions (temperature, relative humidity with respect to sea surface temperature) at the location of the moisture uptake as an important factor. This has been shown by many recent studies that focus on water vapour in the marine boundary layer and/or on downstream precipitation (e.g. Dansgaard, 1964; Craig and Gordon 1965; Pfahl and Wernli 2008; Steen-Larsen et al. 2015; Aemisegger, 2018; Thurnherr et al. 2020).
My concrete suggestion to remediate this is:
   a) To mention the aspect of the difference in ocean surface isotope composition once, but only shortly, in section 5.3 and to add a discussion about the other, more important moisture source controls, which I detailed above.
   b) To strengthen the structure of the paper by not mentioning the temperature and precipitation amount effect at multiple locations (e.g. in Section 5.2 and p. 16 l 487-495), but to confine this analysis to Section 5.2.
2) **Temperature and precipitation amount controls on the variability of the isotopic composition of precipitation**: (e.g., P. 2, L. 34; Section 5.2, P.16 L487-495) I am a bit unhappy with these statements about the "air temperature exerting the most significant influence on $\delta^{18}O_p$". Based on a correlation analysis it is not possible to infer any mechanistic control or directed influence. In my opinion this "temperature effect" is very complex and not yet so well understood. Usually, it is conceptualised by using a simple air parcel model with continuous uptake in the marine boundary layer and progressive rain out thereafter following a Rayleigh distillation model approach. To formulate it a bit provocatively: it could also be the $\delta^{18}O$ (or much

rather the amount of remaining water vapour, i.e. the specific humidity) that is actually controlling the temperature. To me, such correlations just indicate an overarching coherence in the hydroclimate system, in which a change in one variable necessarily implies concurrent changes in many of the others. Progressive rain out along an air parcel is one very good example, in which this seems to be the case to me. I would encourage the authors to formulate the result of their correlation analysis more cautiously, with this thinking in mind and to avoid inferring any influence without precise description of the implied process.

3) **Moisture source identification discussion:** there is substantial literature that investigates the moisture sources of precipitation and water vapour with sophisticated methods based on Lagrangian moisture source diagnostics (e.g. Sodemann et al. 2008, Gimeno et al. 2010) or Eulerian tracer studies (e.g. Winschall et al. 2014). The method adopted here, with only three trajectories calculated per station accompanied by the wind rose approach to quantify source regions is not adequate for addressing the question of the moisture source of precipitation at the different stations. What I suggest here are the following changes:
    a. Reword all places where moisture sources are mentioned and formulate it as the "origin of the air parcels associated with precipitation" or a similar wording.
    b. Recompute trajectories (vertically stacked from the surface to 300 hPa), and only select those that are associated with a relative humidity of >80% at the arrival (station) and then compute the wind roses. For the wind roses, the relevant timescale for the largest part of the moisture uptakes (in particular in summer) is up to 5 days before arrival (e.g. Läderach and Sodemann, 2016). Doing the wind roses separately for 0 to 5 days before arrival (in the paper) and 5 to 10 days before arrival (in the supplement) would seem adequate. This will most probably provide a better rough estimate of the precipitating air parcels' moisture uptake region.
4) **Seasonality of moisture sources:** Many climatological moisture source studies have highlighted a strong contrast in land vs. ocean sources for continental precipitation in winter vs. summer. A strong seasonal cycle is usually observed with a dominant contribution of continental moisture recycling in summer and a much more important contribution of oceanic sources in winter. A recent study analysing the trace element composition of precipitation in the Pyrenees (Suess et al., 2020) also shows this tendency. This seasonality in land vs ocean moisture source contribution to Iberian precipitation is most certainly an important driver of the seasonal cycle of the isotope signals in precipitation. This key aspect should be mentioned upfront in the introduction around lines 72-73.
5) **Meteorological context of precipitation events:**
I find the discussion around the meteorological context of precipitation events very important and interesting. However, it would be great if the authors could write a bit more precisely, how they expect the different precipitation mechanisms (frontal systems in winter, convective precipitation in high pressure systems in summer) to impact the isotope signature of precipitation. There are a few studies that analysed isotope signals during cold frontal passages in Europe (Aemisegger et al. 2015, Graf et al. 2019) and squall lines in Africa (Risi et al. 2008), as well as a climatological study over Europe (Christner et al. 2018) addressing the impact of continental moisture

recycling on isotope signals in vapour and precipitation. Furthermore, the impact of convection during a cold front passage in the Mediterranean was investigated in a modelling study by Lee et al. 2019. The recent paper by Rüdisühli et al. 2020 would provide the authors some guidelines, about which weather system dominates precipitation in which season on the Iberian Peninsula (cold fronts in winter, high pressure systems in summer).

Minor comments:

1) P. 1, L. 29: **the** rainfall isotopic variability
2) P. 1, L. 30: determining **the** rainfall isotopic variability
3) P. 2, L. 32: "Atlantic fronts are found to be the dominant source": Fronts delimit two different air masses with contrasting thermodynamic properties (e.g. warm and moist air mass from a cold and dry air mass). They are not "sources" of rain events. I suggest to rewrite this into something like: "Frontal systems associated with North Atlantic cyclones are the dominant mechanism inducing precipitation over Northern Iberia, in particular in winter".
4) P. 2, L. 37: why "but the type of precipitation…" shouldn't it be "in addition".
5) P. 2: It is a bit unclear in the abstract which factors the authors think are the most important. Aren't the investigated aspects overlapping to some extent and just show consistent relations but from different angles? To me the continentality aspect is very much related to the source aspect and the type of mechanism inducing rainfall is related to the precipitation amount and the temperature. I would recommend the authors to revise the abstract and structure it in a clearer way.
6) P. 2, L. 47: in modulating $\delta^{18}O_p$ **in** a particular region
7) P. 3, L 67: for example (Aggarwal et al., 2016)**.** Regrettably
8) P.4, L109- 122: While I would find it very interesting to investigate the role of cyclones and the interannual variability of the North Atlantic storm track on the isotope signals on the Iberian Peninsula, I think this discussion about the NAO is a bit out of scope here. It is not taken up later on in the analyses performed.
9) Section 3.1: Given the importance of the sampling procedure, it would be nice if the authors could roughly quantify the uncertainty associated with their sampling system. A recent study, that has performed a targeted uncertainty analysis in this respect is Fischer et al. 2019.
10) P. 7. L. 200: **no** good data
11) P. L 225ff: I did not understand how the authors proceeded here, only when reading their results (P. 13, L. 392-399). Lines 392-399 are explaining the method and should be brought forward to the methods section and be removed from the results.
12) P. 8, L. 232: I did not understand, how the authors used the disaggregated precipitation time series and why this was important.
13) Figure 3 should be split into different panels with one panel per station. Currently it is very difficult to distinguish the different time series. Furthermore, I would urge the authors to add the time series of δD, the deuterium excess and precipitation totals. They are equally important.
14) P. 8, L 253: Here I think the authors could make it clearer that overall, there is a very large day-to-day variability in their isotope data that is as large as the seasonal cycle. This would emphasise the value of the high temporal resolution of their measurements.

15) P. 8, L. 255: over which time period were these sums accumulated?
16) P. 9, L. 264: “bi-model pattern” mention with peaks in Spring and autumn
17) P. 9, L. 265: remove “quite”
18) Section 5: in the short introductory paragraph of this section, it is important to add a statement about the fact that the hydrological cycle is complex and that many processes play a role in the formation of the isotope signals in precipitation ranging from source processes, transport processes, as well as cloud and rainfall formation at the sampling site. Furthermore, the authors should mention that they discuss several factors, that they believe play an important role, but they should also make it clear that these factors can play an overlapping role (i.e. due to the concurrent changes in many variables taking place along an air parcel’s transport pathway, see my major comment 2).
19) P. 9, L. 281: why are moisture sources and air masses history mentioned here? They should be taken out of this sentence and it should be made clear in a subsequent remark, that the latitudinal location and the regional orography influences the circulation and therefore the air mass history.
20) P. 9, L. 287: it should be made clear here, which are the high elevation and which the low elevation sites.
21) P. 9, L. 289ff: this part should go to the source effect Section in 5.3.
22) P. 10, L. 298: “when **moist air** and clouds”
23) P. 10, L. 303: not the **main** synoptic pattern, see climatological analysis of the weather systems that induced precipitation in different seasons by Rüdisühli et al. 2020.
24) P. 10, L. 306-3012: this does not fit into this section
25) P. 10, L. 320: “separated from the Mediterranean”
26) P. 10, L 324: no comma before controlling factors
27) P. 11, L. 348: remove vertical, this is clear for deep convection
28) P. 11, L. 349: in the extratropics in summer
29) P. 11, L. 355: where a significant ... correlation is found.
30) P. 11, L. 356: I would not necessarily expect such a correlation at the monthly timescale
31) P. 11, L. 357: at the other sites
32) P. 12, L. 360: origin of air masses producing rainfall
33) P. 12, L. 361: also spatially variable in northern Iberia, these properties, and their relation to the observed $\delta^{18}O_p$ variability…
34) P. 12, L. 381: “with the Villars station”
35) P. 12, L. 381: “precipitation type and geographic origin” this is the topic of the next section
36) P. 12, L. 383: there are certainly much less precipitation events that are associated with fronts in summer than in winter
37) P. 12, L. 386: Remove the sentence about the isotope composition of the ocean at the moisture source and mention it only once (you do it already at lines 369ff).
38) P. 13, L. 389: Replace “moisture source” by “airmass origin”
39) P. 13, L. 409: I don’t understand the sentence “In that way, it is clear the dominant WNW…”.
40) P. 13, L. 415: what does more stable mean?

41) P. 13, L. 415-420: maybe a classification into subregions would make the discussion in the whole paper easier to follow.
42) P.13, L. 420: I of course agree that most probably the moisture source plays an important role, but the authors don't show this in a quantitative and methodologically convincing way.
43) P. 14, L. 435: "evaporated"-> evapotranspired
44) P. 14, L. 442: remove the reference to the composition of the ocean surface
45) P. 15: at several instances, the percentages should be indicated in integer precision and not floating numbers
46) P. 15, L. 462: when they can cause heavy precipitation and flooding
47) P. 16: I find the conclusions a bit disappointing in the sense that it is simply a list of findings and there is no opening of the study to new questions that have raised from this study. The authors should rewrite their conclusions in the light of their revisions of the structure and line of interpretation of the climate signals reflected by their isotope data.
48) P. 16: first conclusion: what does this mean for dexcess? I would be much more interested in seeing the precipitation deuterium excess signals and a bit more discussion on them (dexcess is a frequently used proxy for the conditions at the moisture source) instead of the many repetitions of the role of the ocean surface isotope composition at the moisture source.

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