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Hydrology and Earth System Sciences
Discussions

Interactive comment on "What controls the stable isotope composition of precipitation in the Asian monsoon region?" *by* Le Duy Nguyen et al.

Anonymous Referee #1

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In recent years, a number of empirical, theoretical, and modeling studies have attempted to identify, characterize, and quantify the dominant controls of the stable isotopic composition of rainfall in tropics, particularly in the Asian monsoon domain. Duy et al manuscript, which at a first glance, seems like yet another manuscript along this line, indeed dives much deeper than the previous studies and attempts to provide more rigorous and quantitative assessments of various climatic factors that control stable isotope composition of rainfall in the Asian monsoon domain. Authors present a robust body of observational precipitation isotope data (weekly to bi-weekly samples over ∼1.5 years) collected from Vietnamese Mekong Delta region. This observational isotope data has been examined in the context of both local-and-regional-scale station-based climate data (temperature, precipitation amount, humidity), GNIP data, and fi-

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nally climate data extracted from GDAS gridded dataset, the latter being used to drive the NOAA's HYSPLIT models. Authors conclude that the influence of the different factors on the isotopic condition is best quantified by multiple linear regressions (MLR) of all factor combinations and that explains up to 80% of the variation of δ18O of precipitation. This study, like many previous studies, shows that local rainfall amount and temperature play a minor role in controlling the isotopic composition of the rainfall with upstream precipitation amount emerging as the dominant regional controlâĂŤ again a result consistent with previous studies, but the author's conclusion is backed by solid quantitative analysis. The manuscript is well-written, free of excessive jargon, logically structured with high-quality figures and graphics that are instructive and easy to understand. In sum, I did not find any major issues with this manuscript and I highly recommend its publication. I have provided here a few comments, which authors may find useful in further improving their manuscript. 1. Are results of this manuscript sensitive to the choice of gridded dataset (for example, R1/R2) vs GDAS, which was used to drive the HYSPLIT model? 2. Figure 5 shows backtracking trajectories (only those which produced rainfall). Perhaps I missed reading about it but can authors more clearly elaborate on the criteria they applied to establish when a certain air parcel was considered to produce rainfall? 3. Additionally, I think it will be useful to have another figure that shows major cluster tracks (instead of trajectories) and their relative weights). For example, what percentage of trajectories originate from the Indian Ocean vs continental sources during the rainy season? Furthermore, can these tracks be fingerprinted with their typical d18O values? I suppose this should not be too difficult given that authors have access to the d18O values of precipitation. 4. I think the authors need to be more specific (as opposed to providing generic comments) in suggesting how their conclusions need to be considered in paleoclimate studies. It would be helpful if they can cite some paleoclimate studies where proxy data may have been misinterpreted in light of the results obtained from this study.

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