This study evaluates local vs. regional controls on the stable isotopic composition of precipitation in the Vietnamese Mekong Delta by assigning relative weights to multiple linear regression coefficients. As stated in the manuscript, distinguishing local and regional controls on precipitation isotope ratios is a critical concern for accurate interpretation of paleo-proxy records. This study applies a very thorough and novel statistical approach to disentangle these factors. However, it is not entirely clear to me how one would invert this procedure, given a record of precipitation isotope ratios, to reconstruct past climate.

Overall, I find the analysis thorough and compelling, though the methodological descriptions are a bit dense and perhaps lose some clarity in being too detailed. I would like to see the presentation condensed and reorganized in places, as well as a bit more discussion about the broader implications of this work for paleo-proxy interpretations. More specific comments are provided below.

Introduction - could better focus on the main story.

1. I'd like to see a strong beginning, emphasizing the scientific question at hand. Why not make the second paragraph the lede?
2. The 4th paragraph suggests "other relevant processes were identified…" presumably for the Monsoon Region. Do all the ensuing publications specifically address the Monsoon Region?
3. The 5th paragraph suggests statistical models are "not able to represent the actual processes…" Some re-wording/re-phrasing here is required. All models are a representation. GCMs, for example, can only approximate many physical processes.
4. Limitations and assumptions of paleoclimate reconstructions discussed in the 6th paragraph are nicely described.
5. Also in the 6th paragraph: what is the difference in isotopic signatures of Indian and Pacific Ocean air?
6. Paragraph 7 and onward, some of the narrative flow is lost. What is the purpose of discussing advances and limitations of GCMs? There is a statement about developing GCM code being too daunting a task, but there is code and there are researchers actively developing it, so the argument doesn't quite make sense. Are GCMs and Lagrangian models two different ways of approaching paleoclimate reconstructions? How do these models fit with the methods used in this work? It almost seems as though Paragraphs 12 or 13 could directly follow 6: a monofactorial approach has many limitations…therefore this study suggests a multifactorial one. The multiple factors considered include both local and regional meteorological variables, with back trajectories used to characterize the regional ones. Yes, GCMs also allow one to consider both local and regional factors, but, as stated, their complexity can make interpretation difficult. Perhaps the more detailed GCM discussion could be moved to a proper discussion section. This would help focus and condense the Introduction, which would be desirable.
7. Page 5, where the importance of multiple factors in influencing precipitation are discussed, this would be a good place to introduce the need for a multiple linear

regression approach and tie this paper's statistical approach to the larger scientific questions at hand.

8. Page 5, Line 22: LMWLs should be defined for those unfamiliar with isotopic analyses. More broadly, it is not clear to me that the LMWLs play a significant role in this analysis other than to show that re-evaporation may be relevant during the dry season. It seems their presentation could be minimized. More on this below.
9. The Intro ends by emphasizing the drivers of isotopic variation. But isn't the underlying motivation using the isotopic records from the past to interpret hydroclimate? How do we go from one direction to the other?

Study area

10. An Long and its relationship to Cao Lanh should be described here. The best description of this is the first paragraph of Section 4.1. Specifically, the paper should describe why it is okay (or at least necessary) to interchange data from these sites.

Methodology – could be shortened.

11. The section begins with "An overview of the proposed methodology…" Yet this is in fact the methodology used. "Proposed" can be dropped.
12. Describing the LMWL is fairly standard practice, and the comparison of three distinct regression methods seems overkill, particularly since all three give equivalent results. I would suggest moving this sensitivity test to supporting information, which would help shorten the methods and the number of figures.
13. Similarly, the description of HYSPLIT is a bit more detailed than really necessary. I'd like to see Section 3.5 considerably shortened.
14. "Moving distance" is not clear. I believe what is intended is the distance the air parcel moved. It would be helpful to clarify that this is measured (in km?) along the parcel trajectory (as opposed to the Euclidean distance between start and finish).
15. I would suggest removing the clause "In order to derive figures representative for each trajectory…" from Line 13 on Page 9, as it is not clear.
16. Some of the remaining paragraphs on Page 9 related to HYSPLIT assumptions can be shifted to a Discussion section.
17. The first paragraph of Section 3.6 is quite clear and helpful in describing the paper's methodology.
18. Equations 2 and 3 should follow immediately after they are mentioned.
19. The number of ML regressions considered is quite impressive and reflects the thoroughness of the paper's approach.
20. I appreciate the fact that the paper openly acknowledges the correlations among predictor variables and address multicollinearity using relative weight analysis. This method will be somewhat new for many readers and should be given a bit more description. (This is one of the only sections where I would recommend expanding the text!)
21. I had assumed all weights described in the results are relative weights. Is the RPSS used as well? If so, this is not clear. Similar to my suggestion for LMWLs,

I would recommend emphasizing one method and simply stating that other methods did not provide qualitatively different results. This will help streamline the methodology tremendously and help give other researchers a roadmap for conducting a similar statistical analysis for their region(s) of interest.

Results – could be reorganized.

22. I might suggest a bit of reorganization (and condensing!) here: what if the section began by describing the local data, contextualized it within the larger region, then discussed the distant moisture sources to the region? This would give some additional motivation for evaluating local vs. regional controls on precipitation as the final, most important segment of this section.
23. The first paragraph really belongs in the Methods, as does description of TSV.
24. Line 30, Page 12: the d18O values are "noted" or "written" not "plotted." How about an isotopic bar chart to actually plot them? This would be much easier to "read" than the text.
25. As written, it is not clear how section 4.2.1 (LMWLs) answers the local vs. regional control question. See previous comments about shortening the presentation and discussion of LMWLs. The seasonal LMWLs do provide some evidence of secondary fractionation (re-evaporation), which is presumably a local process. But that's really the only message I took from their inclusion in this work (and it's not clear that this is the intended use of the LMWLs in the paper.)
26. It's not clear from the Methods that the GNIP data will be used to set this paper's measurements within a larger regional context. This could be stated earlier in the paper so that the reader knows to expect this and to understand how the GNIP data will be used.
27. Top of Page 14: the paper highlights differences between An Long and Bangkok, but the figure doesn't really show substantial differences. Moreover, wouldn't an unusually dry period tend to enrich An Long compared to Bangkok's climatology? I don't see this in the data. Lastly, it doesn't really make sense that one would use the sites to "represent or complement each other." Perhaps one could rephrase to say the overall similarity suggests an important role for regional or larger-scale controls on An Long precipitation isotope ratios.
28. The Levene test description can be moved to Methods.
29. Page 15 first sentence: we can't yet know that precipitation is "mainly controlled by large-scale circulation." What we infer is that it is influenced by other factors such as the large-scale circulation.
30. Page 16, Line 7: the correlations can only show a correlation, not that P-hysplit is the dominant control. Our physical understanding of isotopic responses to precipitation is what suggests precipitation is the control.
31. Section 4.4: I'm a bit confused how the MLR models are evaluated. Aren't all factors, including met variables at various heights and for various trajectory lengths considered all at once to select the best model? The section almost suggests the height and length are picked first, and then the best met variables are identified second, which wouldn't make sense. Some re-phrasing is needed.
32. Page 17 is really quite compelling and well written.

33. Up to 7 predictors for seasonal regressions with 42,18, and 14 data points is not ideal. Some discussion of this potential limitation would be useful in a proper discussion section.
34. Moreover, it would be useful to see the final best model (and which predictors are included!) for both the annual and seasonal analyses.
35. Some discussion of why dxs seems to reflect regional processes more than the individual isotope ratios would be useful. Again, this could go in a proper discussion section.

Conclusion

36. Page 20, Line 20: Perhaps “play a smaller role in influencing” rather than “modulate.”
37. Page 21, Line 8: scratch “without a priori knowledge or assumptions.” The method of course makes a priori assumptions when picking variables like P and T as predictors of the isotope ratios. Also, assumptions are made about the importance of both local and regional factors.
38. Page 21, Line 20: Where are the LMWLs of all stations compared? Perhaps this statement should just be eliminated as the LMWLs don’t seem to add much to the analysis.
39. Last paragraph: again, how can we go from understanding controls on isotopes to using isotope ratios to reconstruct climate?

Tables and Figures

40. Table 3: d18O-d2H order should be swapped in first column, second row, to be consistent with other rows.
41. Figure 6, in addition to the isotopic bar chart suggested above, the brown text could be re-colored so it is more distinguishable from the red text.
42. Figure 10: Consider plotting the arithmetic vs. amount-weighted means as a difference for faster viewing and interpretation.
43. Figure 11 caption: the best model is “marked” or “annotated” with red text.
44. Figure 12 caption: the dots and bars in the top panel should be identified.