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Potsdam, 4th October, 2017

Dr. Lixin Wang
Editor
Hydrology and Earth System Sciences

Re: HESS-2017-164

Dear Dr. Wang,

enclosed please find a fully revised, original manuscript now titled “**What controls the stable isotope composition of precipitation in the Mekong Delta? A model-based statistical approach**”, which is renamed from the previous title “What controls the stable isotope composition of precipitation in the Asian monsoon region?” (Reference #HESS-2017-164) by Nguyen Le Duy, Ingo Heidbüchel, Hanno Meyer, Bruno Merz, Heiko Apel. We are respectfully submitting our revised manuscript for your consideration in Hydrology and Earth System Sciences.

We have fully revised the paper to take into consideration the constructive comments from the two referees. Given the manuscript required major revision, we have not provided a line-by-line list of the changes since line numbers have been altered significantly. Instead, we summarize here the major revisions we have made:

- Rewriting of the abstract
- Rewriting of the introduction (focusing more specifically on highlighting the novelties and recent literature)
- Rewriting of the study area description (including a discussion about the definition of the dry/wet season)
- Rewriting of the methodology – Section 3.5 (adding a discussion about the uncertainties of trajectory analysis and applied measures to mitigate these uncertainties)
- Rewriting of the results and discussion (based on reviewers’ feedback)
- Rewriting of the conclusions (focusing more specifically on highlighting the novelties).

This manuscript has neither been previously published in any language nor is it under consideration for publication by another journal. All authors have carefully read the revised manuscript and have agreed to its submission to Hydrology and Earth System Sciences. All results and innovations were developed by the authors using Matlab. Figures were generated using ArcGIS and Matlab. We also published the isotopic data in the open access data repository of GFZ. The data is already available to reviewers under:

<http://pmd.gfz-potsdam.de/panmetaworks/review/9e1af507c8fce65a8d740033e5fea31c2e7c58ade81762c235c6f6bbab91166e/>

Thank you for handling the manuscript during the review process, and to the reviewers for their valuable feedback and edits. We look forward to hearing from you.

Sincerely yours,
Nguyen Le Duy
Corresponding Author

RESPONSE TO THE REFEREES' COMMENTS

We sincerely thank both referees for their thorough reviews and most constructive comments on our manuscript (Reference #HESS-2017-164). We fully appreciate the reviewers' efforts in providing these informative reports on our research and their insights have led to an improved interpretation of our results. We have taken into full consideration all of these comments and have prepared responses to these as well as information on how the paper was revised following the referees' suggestions. Our responses to reviewers are provided below **in blue** following the individual comments requiring action from both reviewers, followed by a marked up version of the manuscript (all changes in the text are marked **in red**).

Anonymous Referee #1

General Comments:

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 finally 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 $\delta^{18}\text{O}$ 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 control 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.

We thank the first anonymous referee for the positive and constructive comments.

Specific Comments:

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?

Yes, we acknowledge that the results of this manuscript might be sensitive to the choice of the climate dataset driving the HYSPLIT model. Moreover, the backward-trajectory simulations by HYSPLIT are also influenced by other parameters that have to be defined for running HYSPLIT, such as starting time and height of the trajectories, trajectory duration, vertical motion options, and number of trajectories. Studying the sensitivity of HYSPLIT backward-trajectory simulations would be an interesting topic, but exceeds the scope of this study.

In order to discuss the sensitivity with regard to the choice of the gridded dataset as well as the uncertainties of trajectory analysis, we included this paragraph to the revised manuscript:

“Single backward trajectory computations by the HYSPLIT model can have large uncertainties. The horizontal uncertainty of the trajectory calculations by HYSPLIT has been estimated to be 10–20 % of the travel distance (Draxler and Hess, 1998). While errors in trajectory calculation computed from analyzed wind fields seem to be typical on the order of 20% of the distance travelled (Stohl, 1998), the statistical analysis of a large number of trajectories arriving at a study site would increase the accuracy of the trajectory analysis (Cabello et al., 2008). Harris et al. (2005) studied trajectory model sensitivity to the input meteorological data (focusing on ERA-40 and NCEP/NCAR reanalysis data) and to the vertical transport method. They pointed out five causes of trajectory uncertainty, expressed as percentage of deviation of the average travel distance: 1) minor differences in the computational methodology: 3–4%; 2) time interpolation: 9–25%; 3) vertical transport method: 18–34%; 4) meteorological input data: 30–40%; and 5) combined two-way differences in the vertical transport method and meteorological input data: 39–47%. However, it would be difficult to prove that in all situations a single meteorological data set or a single method of trajectory modeling was superior to another one (Gebhart et al., 2005; Harris et al., 2005). More details about the uncertainties in trajectory modeling were provided by (Stohl, 1998), later by (Fleming et al., 2012) and references therein.”

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?

This paragraph was included to the revised manuscript (in section 4.1) to elaborate on the criteria applied to establish when a certain air parcel was considered to produce rainfall in Figure 5.

“Because there is no daily precipitation data recorded at An Long, we used daily precipitation data at Cao Lanh instead. This is the closest national meteorological station, located approximately 37 km Southeast of An Long. Backtracking trajectories in Fig. 5 are plotted for the days when rainfall was recorded at Cao Lanh. This is based on assumption that days with precipitation at Cao Lanh and An Long coincide.”

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?

Thank you very much for this constructive suggestion. We added Figure 6 to the manuscript.

This paragraph was also included to the revised manuscript (in section 3.5) to discuss the trajectory cluster analysis.

“The trajectory cluster analysis is conducted by the HYSPLIT model to group trajectories with similar pathways. The cluster analysis merges these trajectories that are near each other and represents those clusters by their mean trajectory. Differences between trajectories within a cluster are minimized while differences between clusters are maximized. Computationally, trajectories are combined to decrease the number of clusters until the total spatial variance (TSV) starts to increase significantly. This occurs when disparate clusters are combined. This number of clusters is then selected as the optimal cluster number for sorting and combining similar trajectories. More information about the HYSPLIT cluster analysis can be found at <https://ready.arl.noaa.gov/documents/Tutorial/html/>.”

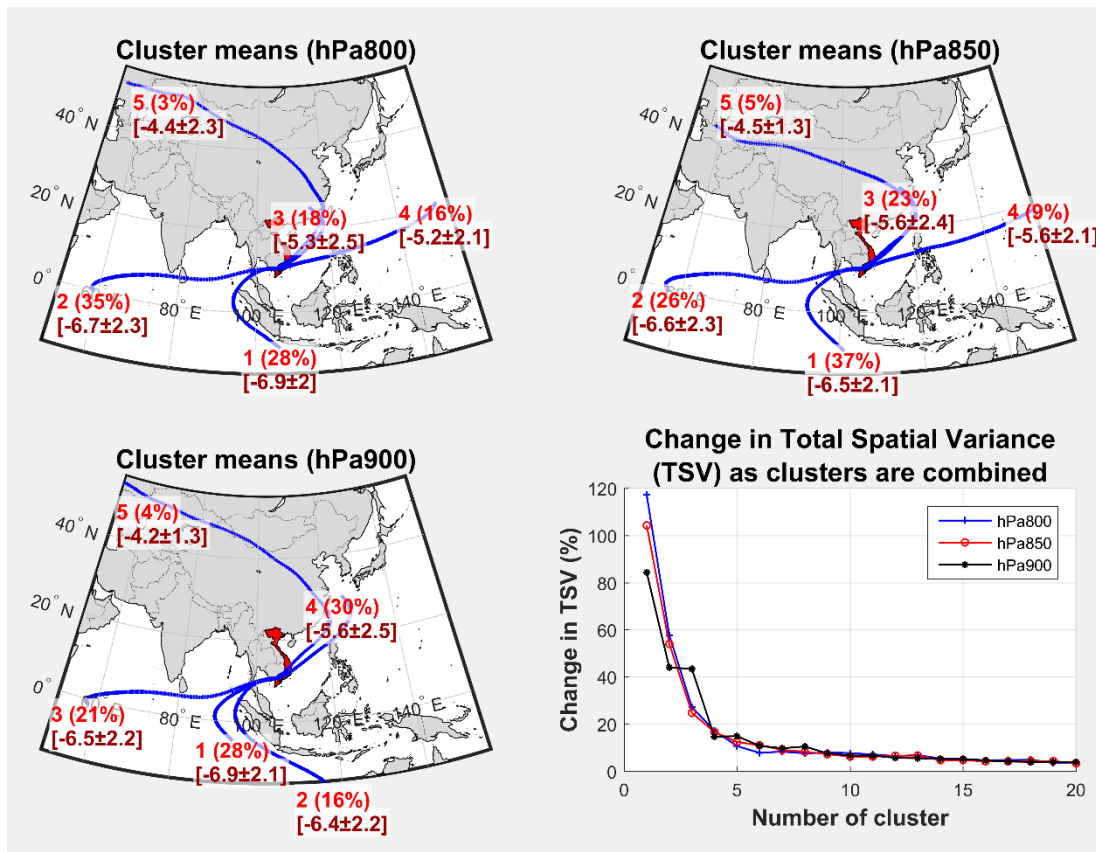


Figure 6: Spatial distribution of vapor trajectories (cluster means) for precipitation days at An Long for 3 barometric surfaces (800, 850, 900 hPa) between June 2014 and December 2015, and change in total spatial variance (TSV) for different cluster numbers. The TSV was used to identify the optimum number of clusters. Red texts indicate the cluster number (1-5) and the percent of all trajectories assigned to each of the five clusters. Brown texts indicate the mean $\delta^{18}\text{O}$ values for each cluster plus/minus the standard deviation of each cluster.

Furthermore, can these tracks be fingerprinted with their typical $\delta^{18}\text{O}$ values? I suppose this should not be too difficult given that authors have access to the $\delta^{18}\text{O}$ values of precipitation.

Thank you for this constructive suggestion. This paragraph was included to the revised manuscript (in section 4.1) to discuss how backward trajectories can be fingerprinted with their typical $\delta^{18}\text{O}$ values:

“The mean $\delta^{18}\text{O}$ values for the 5 clusters are plotted in Figure 6 (in brown). The mean cluster values are similar for the three pressure levels. Also, the mean values of the two clusters from the Indian Ocean, as well as the two clusters from the Pacific, are similar. For a fingerprinting one also has to consider the variation of the values within the clusters, which partly overlap. This means that the $\delta^{18}\text{O}$ values of precipitation in the Mekong Delta cannot be used to uniquely identify the origin of the trajectory. However, they provide a coarse indication of their origin.”

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.

The suggestion of citing paleoclimate studies where our findings could have made a difference seems to be appealing, but we have to admit that paleoclimate is not our research focus and that we don't have an encompassing picture about all the past and ongoing research in this field. We thus don't feel qualified to criticize published studies in this field. We rather hope that the paleoclimate community will become aware of our results and model-based statistical approach, and that they might be considered in their future research.

Anonymous Referee #2

General Comments:

In this paper, the authors used their new weekly precipitation isotope dataset in Vietnam's Mekong river delta region for 1.5 years, and they tried to reveal the controls of the temporal variation of the precipitation isotope ratio. To do so, they conducted some statistical analyses, and they concluded that the isotope ratio is controlled by mainly regional scale phenomena (mainly by the previous rainfall activity along the trajectory of air mass) especially during the early rainy season, and the contribution of the control varies by season.

We thank the second anonymous referee for the constructive comments. Our answers are also included in the revised version of the manuscript.

In my opinion, even though they conducted multiple methods, nothing is quite new. The control of precipitation isotope had been discussed by many researchers as the authors mentioned, and the authors' findings were already pointed out by many, too. For example, the quantification of the controls was attempted by several model studies including Yoshimura et al., 2003; Risi et al., 2008; Kurita et al., 2011; Ishizaki et al., 2012; etc. Some of these studies do not necessarily focus only Asian monsoon regions, but basically, they tried to reveal more general controls. In these studies, they used GCM or equivalent models to reveal the controls, whereas the present paper used statistical models. Furthermore, by the recent efforts, researchers already began to realize that it is indeed not appropriate to make a simple relationship between precipitation isotopes and climate parameters. The present paper's conclusion of necessity of consideration of multiple climate impacts and temporal (and spatial) dependency on the controls have been explicitly or implicitly stated many times. Therefore, nowadays, more advanced techniques of utilization of isotopic information have been utilized. One of them is data assimilation.

We acknowledge the fact that the results are not new, and that the focus of the paper is the development and testing of the model-based statistical method instead. We also recognize that the title can be quite misleading (as mentioned in major issue #2), and thus may lead to a misunderstanding about the novelty of this study. We therefore modified the title to **“What controls the stable isotope composition of precipitation in the Mekong Delta? A model-based statistical approach”** and discussed the transferability to the greater region, i.e. SE-Asia. Actually, isotopic data of rainfall has never been collected for the Mekong delta, and therefore the fact that the isotopic variation of the Mekong data is similar to that of Asian monsoon region has never been confirmed before.

We revised the introduction and conclusion to specifically highlight the novelties of the study. Recent literature was also included accordingly. Because the revised introduction and conclusion are too long to present here, please find them in the submitted revised manuscript.

From the above aspect, I have to tell that this paper's methods (multiple regression and trajectory analysis) is no longer insufficient to fulfill the objectives of this study. What I mean is, there is no guarantee that this study's number of 70% regional control can be applied to any other year's temporal variation of precipitation isotopes. In this regard, 1.5-yr long data is not sufficient, too.

Of course, due to the limited length of the time series we cannot be 100% sure that the identified contribution of local and regional factors will be the same in other years. However, as shown in figure 7, the long term monthly isotopic values in Bangkok and the values of our two rainy seasons in the Mekong delta are quite similar. Considering also the climatic similarities between the two locations, this indicates that the recorded isotopic variation is likely to be representative for a longer period and a wider area. This suggests in turn that the identified contribution of the factors could also be the same in other years. Also, the fact that our findings agree with the ones of Ishizaki et al. (2012) supports this assumption.

Figure 7 (in the old-version of manuscript) was edited to include the short-term mean monthly

isotopic signature of precipitation of Bangkok, and renamed to Figure 8 (in the revised manuscript). The number of the other figures was edited accordingly.

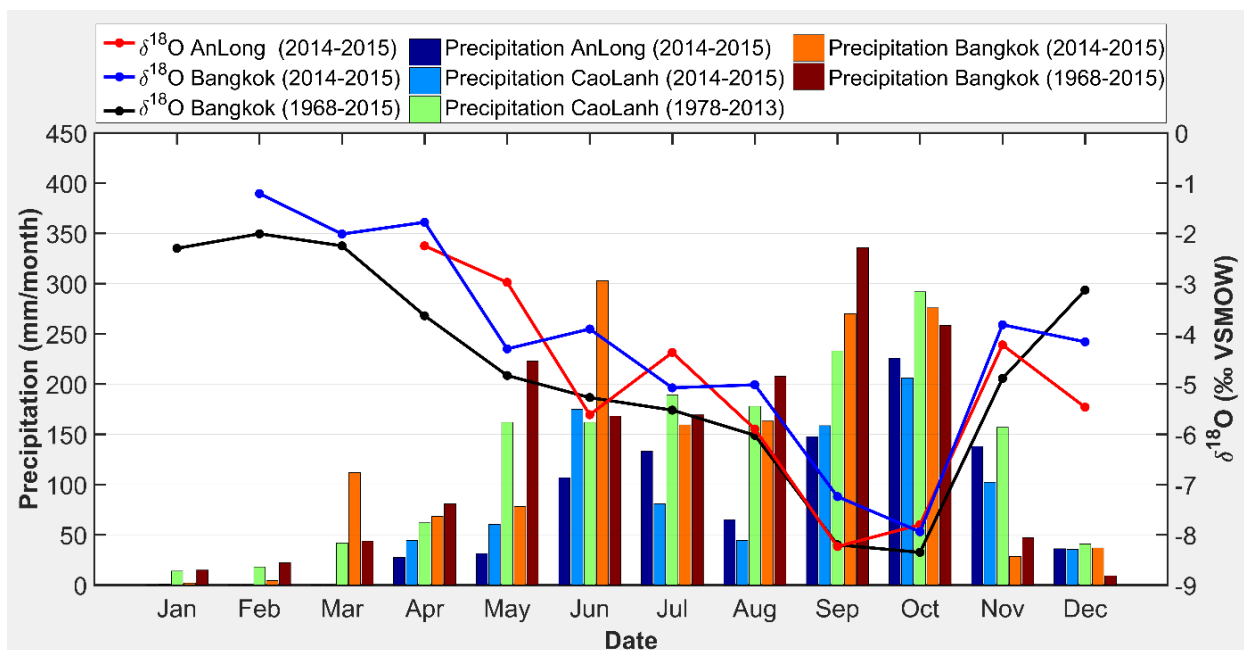


Figure 8: Seasonal variation of the average monthly precipitation for An Long and Cao Lanh and $\delta^{18}\text{O}$ values of precipitation for An Long (for the period of observation (red)) and Bangkok (both for the period of observation (blue) and the long-term mean (black)).

Major issue:

1. Drop unnecessary and unrelated analyses. Especially the parts with local meteoric line is not directly related to the conclusion of the study. It is too simple analysis. Even global meteoric line is just conceptual idea (slope of 8 and intercept of 10 is not certain). There maybe some physical reason to have smaller slope, especially by kinetic effect, but in this study, it is out discussed enough. It's better to drop the part.

You are right that the derivation of a local meteoric water line is a very simple analysis. We still think it provides valuable information for the following reasons:

- From our point of view the analysis of isotopic data by means of a meteoric water lines is a standard for such kind of data and should always be conducted, just as descriptive statistics of other data.
- Up to now, there is neither a LMWL for the Vietnamese Mekong Delta (VMD) nor for the Indochinese Peninsula, which could be used as a baseline for other studies using isotopic data to investigate hydrological processes in this area.
- The close fit of all considered regressions is one piece of evidence indicating that secondary fractionation processes, e.g. sub-cloud evaporation, are insignificant in the study area. This provides support for the discussion of sub-cloud evaporation in Sec. 4.3.1.

2. One point data cannot represent Asian monsoon. Perhaps Mekong river delta data had some similarity with Bangkok, but with only 1.5-yr long data, the authors cannot reject possibility of “by chance”. Furthermore, such similarity is nothing related to that Mekong data represent all Asian monsoon region. The title is quite misleading.

We acknowledge that the title is too generic. We changed it to “What controls the stable isotope composition of precipitation in the Mekong Delta? A model-based statistical approach” and discussed the transferability to the greater region, i.e. SE-Asia. Actually, isotopic data of rainfall has never been collected for the Mekong delta, and therefore the fact that the isotopic variation of

the Mekong data is similar to that of Asian monsoon region has never been confirmed before.

We also went at length to illustrate that the variability of the isotopic data is similar to the long term data from Bangkok in order to provide evidence that the derived results might be representative for SE-Asia. This was already discussed in section 4.2, but we added some critical discussion of the issue of representability in the discussion and conclusion of the revised manuscript.

3. Organize the previous literature with focused temporal and spatial scales. The authors listed many previous studies, which partly investigated on precipitation isotope controls, and (implicitly) stated that there is still huge discussion on the controls. However, it is misleading and not true. What is confusing is the controls can be different dependent on temporal and spatial scales. For example, daily variation of precipitation isotopes in some parts of the world is quite likely determined by synoptic-scale moisture circulation, in which previous rainfall activity along the trajectory matters a lot, rather than local precipitation or temperature, and nowadays there is consensus on this in the research community. However, even in the same place, the controls of monthly or interannual time series can be different. It is simply because those smaller scale impact can be offset each other in those scales, so that local signal only remains.

We completely agree that scales matter. This is fundamental to hydrology. What we present is the result for daily variation (or bi-weekly, to be exact) in rainfall, in a monsoonal climate region with a strong seasonal variation. We stressed this more in the discussion and conclusion, and sorted the cited literature according to the scales considered.

4. Limitation of statistical approach with such short-term data. The conclusion of the study is based on the statistical regression using all samples. The authors should validate their statistical model(s) with different independent samples. In this regard, the observation data is perhaps too short.

As described in section 3.6, we use PRESS for selecting the best model. Within PRESS the model is fitted to all data except one, and the missing value is predicted with the fitted model, i.e. not all data is used for fitting the models at once. This procedure is repeated for every data point. Thus PRESS is equivalent to a so called leave-one-out cross validation (LOOCV), as described in section 3.6. LOOCV is the cross validation procedure appropriate for a limited data set, when a standard split sample validation cannot be applied. There are numerous papers available employing this method in different fields of environmental sciences. LOOCV is actually a split sample validation of the regression, where the data is split as often as data points are available. This means that our results are in fact validated.

5. Most importantly, what is new in this study? As I wrote above, it is well known that precipitation isotope is not controlled by a single factor and the relationship can be different in time and space. The finding in this paper is nothing more than these.

We revised the introduction and conclusion to highlight more specific the novelties of this study. As we have stated previously, we acknowledge the fact that our methods (trajectory analysis, multiple linear regression and relative importance analysis) are relatively simple and easy to apply, but we would like to stress again that the combination of these methods to investigate factors controlling isotopic composition in precipitation has never been applied before.

Moreover, our study focuses on the quantification of the impact of the various factors controlling isotopic composition in precipitation. This has not been performed in such an exhaustive way as presented here (as reviewer 1 actually points out particularly). Of course, the qualitative outcome of the study is not novel in itself, but the way we achieved these results constitutes a novel approach. Furthermore, this approach is easily reproducible and contains a rigorous analysis and

quantification of the interplay of the different factors. Thus we argue that the manuscript indeed goes beyond just stating that regional factors are more important than local factors for the daily rainfall isotopic composition of the study region. It rather supports this finding by a thorough and reproducible method that combines trajectory modelling and statistical analysis.

In order to stress the novelty of this study, we also included this paragraph to the conclusion:

“The validity of the approach is confirmed by similar, but mainly qualitative results obtained in other studies. The comparable results provide a strong indication that the method is able to identify the dominant factors responsible for the isotopic composition of rainfall without a priori knowledge or assumptions. In contrast to previous studies, the presented approach and results provide, however, a quantitative assessment of the impact of different factors, and thus information about the dominant processes of isotopic fractionation. It can support the interpretation of processes responsible for observed patterns of isotopic composition. The rather simple approach can, of course, not provide detailed information about atmospheric dynamics, but it provides a relatively simple and easy to apply approach supplementing or preceding more complex studies of isotopic composition with circulation models. Due to the simplicity, any scientist can easily apply this method in order to investigate factors controlling isotopic composition in precipitation at any given study area around the world without the requirement of setting up and in-depth knowledge about running a complex numerical atmospheric circulation model. Furthermore, the approach is easily reproducible and contains a rigorous quantitative analysis of the interplay of different driving factors. Moreover, the analysis can easily be extended to other factors and processes of importance in order to capture particularly the d-excess better, e.g. the sea surface temperatures at the source regions.”

Minor issues:

P2L17: what is “circulation effect”? Describe.

The term “circulation effect” (Tan, 2009;Tan, 2014) is used to describe the changes in isotopic composition in precipitation that appear because arriving moisture is coming from different areas of the ocean. The revised manuscript now includes this explanation.

P2L23: what is difference between “distillation during vapor transport” and “upstream rainout”. Aren’t they essentially the same?

Yes, thank you for pointing this out. We used only the term “distillation during vapor transport” in the revised manuscript.

P2L22-P3L3: Different temporal scales are mixed.

We sorted the references according to scale. The paragraph was revised as follows:

“Recently, many studies have presented evidence that large-scale monsoon circulation is the primary driver of variations in precipitation isotopes instead of local controls (e.g. local precipitation amount or temperature) in some parts of the Asian monsoon region. This evidence has been found at different temporal scales including daily isotopic variability (Yoshimura et al., 2003;Yoshimura et al., 2008), seasonal isotopic variability (Araguás-Araguás et al., 1998;Kurita et al., 2009;Dayem et al., 2010;Peng et al., 2010;Baker et al., 2015), and/or interannual isotopic variability (Vuille et al., 2005;LeGrande and Schmidt, 2009;Ishizaki et al., 2012;Tan, 2014;Kurita et al., 2015).”

P3L21: Before the authors’ conclusion, there are many studies which state necessity of consideration of multiple parameters.

Yes, the paragraph is misleading. We replaced the whole paragraph in the introduction with:

“It has been frequently stated and agreed to that local and regional factors should be considered simultaneously to explain the isotopic variation in rainfall (e.g. Johnson and Ingram, 2004). Hence, it can be hypothesized that using multiple factors in a single linear model is able to explain a larger share of the observed variance in isotopic composition. We aim at developing and testing a model-based statistical approach for the quantification of the contribution of isotopic separation processes for explaining the isotopic variation of precipitation. Such a model-based statistical method could also be applied in paleoclimate studies, separating and quantifying the impacts of local and regional factors on the isotopic composition of local precipitation (Sturm et al., 2010), thus overcoming the shortcomings of single factor analysis.”

P3L27: For quantification of the controls, usually researchers try to develop a physical simulator. Any statistical model principally cannot explain the real control.

Physical models are one way to address this problem. But statistical models are an alternative way and have in fact be applied many times in all sorts of environmental studies. Both approaches have their advantages and disadvantages, and they coexist, respectively supplement each other. And while statistical models are not able to represent the actual process causing a phenomenon, they are able to detect results of a process. And this is what we actually are aiming at. Therefore we are arguing that the proposed model-based statistical approach is valid and accepted by the majority of researchers, as long as the limitations are clearly taken into consideration. We underlined this point in more detail in the introduction (P3L3-P4L33) of revised manuscript.

P4L20: There are many other definition of dry/wet season. What is the impact?

We included this paragraph in the revised manuscript (Section 2. Study area) to discuss the impact of the definition of dry/wet season.

“The definition used here is particularly developed for the local climatic conditions, the problem to be solved, and the data available. Other definitions could cause some data points to be assigned to the other season. However, those data points will most likely be from the transition period from one season to the other, i.e. other definitions would affect samples that have the least explanatory value for the actual dry and wet seasons.”

P5L5: “three methods” are not really regarded as different “method”.

Thank you for this point. “three methods” was changed to “three regression methods”

P6L4-L20: drop

As discussed in the 1st comment under ‘major issues’, we consider this part relevant and important for the manuscript. Therefore we would like to keep it.

P7L18: what is TRATIO?

We modified the sentence from P7L17-L19 in the old version of the manuscript as follows:

“Secondly, we use the shortest possible integration time step (i.e. 1 h) and a small value for the parameter TRATIO (0.25), which is the fraction of a grid cell that a trajectory is permitted to transit in one advection time step. Smaller values of TRATIO help to minimize the trajectory computation error using the HYSPLIT model”.

P7L20: The uncertainty of trajectory analysis is not quantified. Perhaps it is minimized in the suggested framework, but how large is the “minimized” uncertainty and what is its potential consequence?

In order to discuss the sensitivity with regard to the choice of the gridded dataset as well as the uncertainties of the trajectory analysis, we included this paragraph to the methodology (Section 3.5) in the revised manuscript:

“Single backward trajectory computations by the HYSPLIT model can have large uncertainties. The horizontal uncertainty of the trajectory calculations by HYSPLIT has been estimated to be 10–20 % of the travel distance (Draxler and Hess, 1998). While errors in trajectory calculation computed from analyzed wind fields seem to be typical on the order of 20% of the distance travelled (Stohl, 1998), the statistical analysis of a large number of trajectories arriving at a study site would increase the accuracy of the trajectory analysis (Cabello et al., 2008). Harris et al. (2005) studied trajectory model sensitivity to the input meteorological data (focusing on ERA-40 and NCEP/NCAR reanalysis data) and to the vertical transport method. They pointed out five causes of trajectory uncertainty, expressed as percentage of deviation of the average travel distance: 1) minor differences in the computational methodology: 3–4%; 2) time interpolation: 9–25%; 3) vertical transport method: 18–34%; 4) meteorological input data: 30–40%; and 5) combined two-way differences in the vertical transport method and meteorological input data: 39–47%. However, it would be difficult to prove that in all situations a single meteorological data set or a single method of trajectory modeling was superior to another one (Gebhart et al., 2005; Harris et al., 2005). More details about the uncertainties in trajectory modeling were provided by (Stohl, 1998), later by (Fleming et al., 2012) and references therein.”

P8L4: PRESS is essentially the same as root mean square error (RMSE), which is more popular in the community.

RMSE is calculated from the residuals of the model fitted to all data, while PRESS is based on the residuals resulting from the model fitted to all data except one, for which the residual is calculated. Repeating this for all data points and summing the calculated residuals results in PRESS. PRESS is therefore a cross validation method. See also our comment above, and for example the definition in WIKIPEDIA as reference (https://en.wikipedia.org/wiki/PRESS_statistic).

P8L5: what is “leave-one-out cross validation”? and what does it mean by “equivalent to” it?

See our reply to major comment 4 and to the previous comment.

P8L16: what is physical meaning of using “mean values of their combinations”? Combination of 800hPa and 850hPa represent 825hPa level (somehow the precipitation was formed at that level at that time)? In this regard, what is meaning of 800/850/900hPa combination?

In P7L4-L7 we discuss that the three levels at 1000, 1500, and 2000 m above ground are corresponding to barometric surfaces of approximately 900, 850, and 800 hPa. These barometric surfaces were chosen because the 850 hPa vorticity is highly indicative of the strength of the boundary layer moisture convergence and of rainfall in regions away from the equator (Wang et al., 2001). Hence rainfall is expected to mostly originate from these altitudes. We included this paragraph in the revised manuscript (in section 3.5) to elaborate on the physical meaning of using “mean values of their combinations” as follows:

“Consequently, the combination of 800 hPa and 850 hPa barometric surfaces accounts for the fact that rainfall is expected to mostly originate between 1500 and 2000 m above ground level. Correspondingly, the combination of the barometric surfaces of 800, 850 and 900 hPa means that rainfall is expected to mostly originate between 1000 and 2000 m above ground level.”

P10L4-L27: drop

As discussed in the 1st comment under ‘major issues’, we argue that this part is relevant for the manuscript. We would like to keep it.

P11L23-L24: I don’t agree with this statement. More evidence is needed.

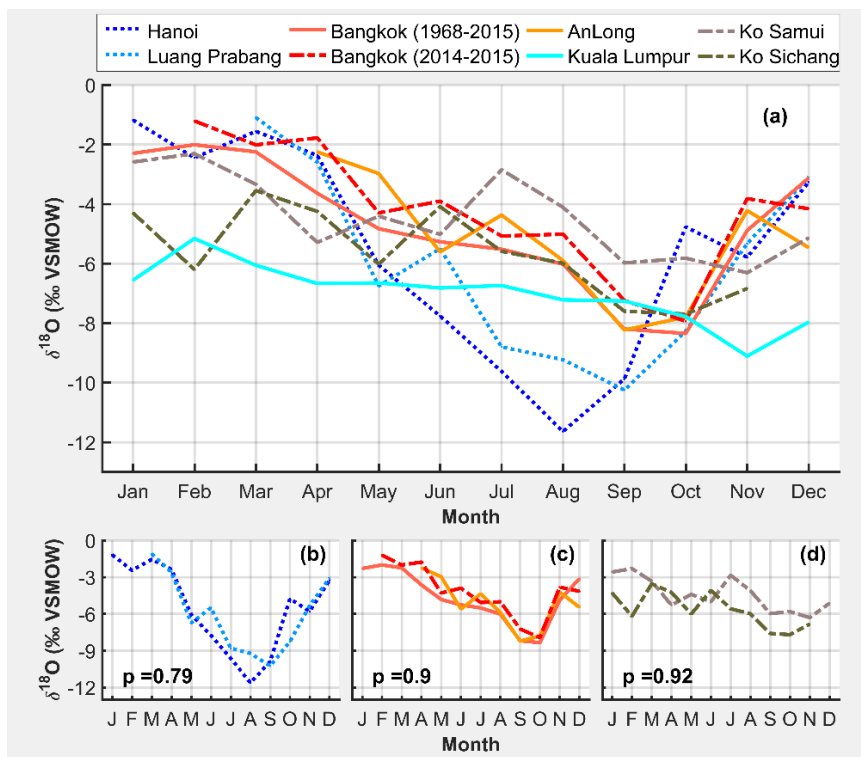
The Levene test (Levene, 1960) for equality of variances was used to compare the data of the different stations across the Indochinese Peninsula. We argue that the observed similarity of the isotopic values and their seasonal variances between An Long and the long term time series of Bangkok (Fig. 8c) (of which the visible similarity is also confirmed with high significance by the statistical Levene test) provides sufficient evidence for our statement. In order to substantiate this finding we added the time series of Bangkok covering the same time span as our data collected in the Mekong Delta to the analysis (new figure 8c, shown below). This time series is even more similar to the one of An Long, resulting in a highly significant Levene test statistic of 0.98. This means that the isotopic variation of the An Long time series is almost identical to the one from Bangkok, and that the variation of the short term time series of Bangkok and An Long is also very similar to the long term time series. In turn, one can infer from this that the data collected in An Long are likely to be representative for the area (i.e. the southern part of SE-Asia). This evidence was included in the revised manuscript (Section 4.2.2) as follows:

“In addition, the short-term time series of Bangkok and An Long (i.e. 2014-2015) show similar variances, resulting in a highly significant Levene test statistic of 0.98. The variation of the short-term time series of Bangkok and An Long is also very similar to the long-term time series, again shown by a highly significant Levene test statistic of 0.90 (Fig. 9c). This indicates that the isotopic variation of the An Long time series is almost identical to the one from Bangkok.”

We also modified the statement acknowledging the remaining uncertainty to:

“In summary, the analyzed GNIP data suggests that the data and results from this study are likely to be representative of the Southern continental part of the Indochinese Peninsula.”

Figure 8 (in the old version of the manuscript) was replaced by the following figure (Figure 9 in the revised manuscript), where the time series of Bangkok for the same period as our observation is added:



P14L9: Why was 124th model chosen as best?

Because the PRESS value of the 124th model is smallest. The sentence provides this information. We also stated this in the methodology section (P8L13) in the old version of the manuscript. In revised manuscript, this evidence is at P11L4.

P15L2: It is good idea. Why don't you do this trial?

We actually did this. The result are shown in Figure 12 and discussed in section 4.4 (from P15L6 to P16L8) in the old version of the manuscript. In revised manuscript, the result are shown in Figure 13 and discussed in section 4.4 (from P18L6 to P19L7).

References

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