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Interactive comment on "A new interpretative framework for below-cloud effects on stable water isotopes in vapour and rain" *by* Pascal Graf et al.

Anonymous Referee #1

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Review results for Graf et al. "A new interpretative framework for below-cloud effects on stable water isotopes in vapour and rain"

In this research, the authors analyzed stable isotope ratio timeseries of a rain event in Switzerland to investigate below-cloud hydrological processes. Importance of below-cloud processes has been increased because of emergence of high-resolution cloud resolving atmospheric model, for example. A new approach to use both rain and vapor isotope ratio timeseries is proposed. In this approach, anomaly of the observed surface vapor isotope ratio is subtracted from equilibrium isotope ratio from the observed precipitation isotope ratio. Then, according to the authors, only the effect of below-cloud processes is extracted.

The approach seems indeed interesting and novel. However, there is significant ig-

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Discussion paper

norance of the theory of kinetic fractionation. In case of unsaturated condition, there is always kinetic fractionation occurring during either evaporation or isotopic exchange (e.g., Craig and Gordon 1965, Merlivat and Jouzel, 1978, Stewart 1975, etc.). Equilibrium fractionation is defined in case of saturated condition. In the present paper, the situation of $\Delta\delta$=Δd=0 occurred when RH<100%. That means, the situation did not happen because the vapor and liquid were in the equilibrium state. Rather than that, it was occurred by kinetic fractionation process depending on specific RH and the initial dD and d18O values of vapor and liquid.

The kinetic fractionation would behave quite complicatedly in $\Delta\delta\Delta$d diagram, according to Stewart's (1975) formulation, which is the most popular parameterization in the isotope general circulation models, for example, $\Delta\delta$ and Δd are highly sensitive to the initial isotopic values of rain and vapor and RH of the ambient air. On the other hand, "the degree of equilibration" would not make such a big difference in $\Delta\delta\Delta$d diagram. As stated above, there is always kinetic fractionation occurring in unsaturated condition, so if such kinetic fractionation's final state is practically called "equilibrium" (by the way, this is what is parameterized in the most of the models, e.g., Hoffmann et al., 1998; Yoshimura et al., 2008), this equilibration would not always become $\Delta\delta$=Δd=0, because there is kinetic fractionation process going on.

This ignorance of kinetic fractionation processes significantly influences the interpretation of the paper. For example, P9L17 "Strongly equilibrated sample are thus located close to the origin" is probably misleading. That is true when RH=100%, but not true when RH<100%. P9L21 "Rain samples that area strongly affected by evaporation will thus be located in the bottom right quadrant" may be misleading too. It is highly depended on initial condition and RH, and different initial condition and RH may cause different trend in evaporation line in $\Delta\delta\Delta$d. So, the right bottom position would not be reflected by "stronger evaporation".

By these reasons, I'd recommend the editor to reject the manuscript ang give them plenty time for resubmission.

Minor issues follow: P2L16: Did the authors come across any new understanding of below-cloud processes? P8L8: What is "cloud signal"? P8L12: More explanation for the other three events are necessary. Another big issue of the paper is lack of observation data. Are the characteristic of cold fronts similar? How about temporal tendency of the evaporation strength? P8L16: "less affected by blow-cloud processes": What does it mean? Isn't it contradicted from "stronger cloud process" in L8?

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