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Multivariate statistical air mass classification for the high-alpine observatory at the Zugspitze mountain, Germany

My two most important comments on the first version of this manuscript were:

(1) Better classification results could have been obtained by adequately pre-processing the observations (e.g., removing seasonal and daily cycles, or transforming variables to achieve approximately Gaussian PDFs).
(2) The matching between air-mass regimes and air-mass classes varies seasonally and is based on subjective considerations, limiting the applicability of the method to other sites.

Generally, the authors used reasonable arguments to defend their work. In particular:

(1) Seasonal variability cannot be filtered adequately without a very long dataset at hand, especially for some quantities with marked interannual variability. Diurnal variability must be retained in the classification design, because it is important in order to identify BL air masses.
(2) The seasonally-dependent correspondence between air-mass regimes and air-mass classes is justified by the fact that the characteristic properties of air masses in terms of the PCA input variables change seasonally (e.g., BL air has relatively high/low CO2 concentration compared to other air masses, respectively in winter/summer). While unclear in the previous version of the manuscript, this aspect is now appropriately explained.

Two other recommendations of mine were: i) To consider transforming variables before the PCA; ii) To work towards a fully objective classification method, i.e., not based on subjective analysis of air-mass regimes. The authors opted for not following these suggestions at the moment, and I find it understandable. Anyway, I encourage them to explore these issues in future work.
I have two final minor comments:

(1) Concerning my previous comment 7: I appreciate the improved presentation of foehn-related issues, nevertheless I suggest to rewrite lines 11-12 this way: “Foehn winds descend on the lee side of a mountain range BUT can be associated with air mass lifting on the windward side, AND WITH ENHANCED TURBULENT MIXING IN THE LEE”.
(2) Concerning my previous comment 11: Equations R1 and R2 in the author response are indeed an approximation of the Clausis-Clapeyron equation, This formulation assumes that the specific volume of liquid water is negligible and that the latent heat of vaporization does not depend on temperature. It would be possible to get a better estimate of the vapor pressure in a larger temperature range using different approximations. Anyway, the errors are expected to be small and justifiable for the purposes of this article.