Authors have correctly addressed most of my comments. Now the manuscript benefit of the publication of the snow drifting module and on a more quantitative analysis of the case study. I want to also recognize the effort of the authors on calculating the compactation, to better evaluate the drifting model. I think that this paper is now acceptable to be published in TC after addressing some few minor comments more that arised in this second lecture of the paper.

L69: accordingly Wagner et al. (2014)

L124: southerly without caps

L125: If the trough is over France, are you sure is moving westward? Or is it eastward?

L170: there is a problem here with the sentence going beyond the margin and the end of the page.

Figure 8. For better comparison I would suggest to represent b and c panels only for those points with data in a. In the current figure I find difficult to compare the points between a and b.

L387: line typo

L412: I think that this sentence does not totally agree with the results showed.

- Only large-scale spatial structure is realistic (L390: model is not able to capture the small-scale snow depth structure at the slopes), and it links with the following sentence.

- Magnitude is not realistic (L372: The order of magnitude of the snow depth changes from the observations is twice as large as the simulated snow redistribution due to snow drift from the simulation).

Maybe the authors can rephrase lowering the claim.