Reply to the general comments of Referee 2

First, the paper essentially demonstrates (section 3.2 – Fig 2) that the proposed normalization technique fails to provide the expected 'universal' normalized DSD. What worked in part 1 when comparing stratiform vs convective DSD, in the single site of Darwin, cannot be generalized to another location and when comparing orographic vs non orographic precipitation.

We disagree with this comment, and in the next we explain why.
1) Section 3.2 is saying only that the normalized drop size distribution in two orographic precipitation sites(Bodega Bay, BBY, and Cazadero,CZC) is different from the one found in Darwin where the precipitation in of convective or stratiform types. We have to acknowledge this difference never pointed out in Literature. Note that this difference emerges even if you use one of the other normalization available in literature, in other words, this result is independent by the normalization that you can consider. This "failure" is not a problem of the specific technique. This difference is the result of the physics of the phenomenon. 2) Note that in GRL 2011 we have showed how in two sites Darwin (AU) and Chillbolton (UK), where the precipitation is only of convective or stratiform types the normalized drop size distribution coincides. Thus we question the sentence of the Reviewer "… *cannot be generalized to another location*". Moreover renormalized spectra relative to stratiform precipitation at Bodega Bay, Cazadero and Darwin are the same. This is an important result (other renormalization procedures do not do this).

Some of the analysis is applied on the normalized spectra (kurtosis analysis) and some of the analysis is applied on the raw spectra before normalization (gradients etc …). The objectives and the organization of this second part of the paper are not very clear ; It appears as a juxtaposition of several empirical methods to analyze the spectra ; the physical reasons behind the observed differences are mentioned only superficially. This part of the paper does not fit in the announced scope of the paper/title and seems somehow 'unfinished'.

There is a difference between the normalized spectra of convective/stratiform and orographic precipitation types, the objective here is to quantify this difference.
It is clear that we have considered different statistical metrics to quantify this difference in DSD (see 2.2.1).
The rationale behind the statistical metrics (or parameters) considered is to catch and highlight the orographic precipitation. In particular we use a "steepness filter". As we have stated in the manuscript, one could envision/propose a different metric, however 1) it is a first tentative in this direction; 2) it puts in evidence the similarity between BBY, CZC and Darwin.
Finally, this remarks also that the orographic precipitation is a delicate issue and it deserves a detailed analysis per se and not combined with other issues. This is why we have presented the work in a companion paper.

My suggestion : the result presented in section 3.2 (i.e. the normalization method fails to provide a universal shape that includes orographic/non orographic DSD) could be included in the companion paper part1, and would illustrate the limitation of the method, when applied to other location /other rain type.

We disagree with this comment and with the suggestion. It would alter the message that we would like to convey. Our point is not: the renormalization technique works or does not work, but it is to show the footprint of the orographic control on the DSD, in particular on a normalized DSD.