First the authors would like to thank the reviewers for their suggestions and careful reading that helped improve the manuscript. Hopefully the changes implemented will satisfy their requirements!

This manuscript presents an original data set of raindrop size distribution (DSD), an important piece of information to describe the microstructure of rainfall. The data described in this manuscript come from three optical disdrometers: two Parsivel2 from OTT and a PWS100 from Campbell Scientific. The period of observation cover two winter months in the Paris area in France, with a fair amount of precipitation (between 84 and 104 mm in total depending on the considered disdrometer). In addition to the raw data (size and velocity in a number of classes), the authors give a nice introduction with relevant references, and also provide "derived" quantities like the rain rate for instance, as well as useful tools to browse and visualize the data.

Recommendation
The data set is relatively original in the sense that similar data sets (in temperate mid-latitude regions) have been collected and made available to the community, but not with this specific configuration of three collocated instruments among which one of a different type. The fact that these three instruments are collocated, hence making possible the assumption that they sample the same population of raindrops, is attractive to quantify the sampling uncertainties associated to the measurements. The only limitation I see is the rather limited duration of the period of observation: two months is short, and it will be difficult in many related analyses to distinguish peculiar effects due to this short period from more general behaviors. I also have a few minor corrections/suggestions listed below. Overall, I think this is a relevant data set worth to be shared with the community (as is done through the zenodo repository listed in the manuscript). I leave it up to the Editor to decide whether this data set, given its short temporal coverage for rainfall, is worth publication in ESSD or not. I provide in any case a list of relatively minor comments and questions below.

This issue was also pointed out by the other reviewer. As mentioned in the title, the paper dataset contains two months of data. It corresponds to two months of data with a cumulative depth consistent with the local climatological average. No extreme events were recorded, i.e. the maximums observed at both 5 min and 30 min have return periods smaller than one month. Such “common” events are notably relevant for urban water managers because they correspond to ones for which they should be able to fully decontaminate storm water before release in the natural environment. Furthermore over this range of value the devices are expected to be reliable. Following the reviewers remark, this point was clarified in the presentation of the measurement period. Moreover the devices and additional ones are still operating and collecting data. Hence some additional data will be made available through our website (https://hmco.enpc.fr/portfolio-archive/taranis-observatory/) which already contains links to the calendars for the various past and ongoing (daily updates) measurement campaigns in which the devices were used. Following the reviewers remarks, this was clarified at the end of section 4.

Specific comments
1. P.2, l.5: it should be mentioned that assumptions or external information about the scattering properties must be used to derive radar variables from DSD measurements.

Indeed, you are right and this was clarified.

2. P.2, l.14: the need for appropriate parameterizations of the DSD in numerical atmospheric models could also be mentioned as an important application of DSD measurements.

This is indeed another possible application, which is now mentioned in the introduction.

3. P.3, l.14: matrixes should be matrices.

This was corrected.

4. P.3, l.16: multiplying by 10/9 to compensate for the "gap" in the measurements implies the assumption of homogeneity, this should be mentioned.

You are indeed right, and this was clarified in the text.

5. P.3, l.17: made instead of maide.

This was corrected.

6. P.3, Eq.(1): I am not sure about the units to be used in this equation. If S ef f is expressed in cm 2 as suggested in the paragraph below, then there might be an error in Eq.(1). I think it u 6π rather thanπ/6 (or 600π if S ef f is in mm 2 ). The authors should check...

This was checked and units clarified.

7. P.3, Eq.(2): v i is not defined...

It is actually defined at the beginning of section 2.2

8. P.4, l.3: so it user are: rephrase.

This was corrected, thank you for your careful reading.

9. P.4, l.16: is it 0.1 mm? If so, the authors should add 0 to make it clear.

Yes, and the “0” was added.

10. P.4, l.22: the measured amount over the same period of time at the MeteoFrance site would also be relevant to complement the climatological value (that shows that this period is not too specific, at least in terms of rainfall amount over two months...).

Such information is not available to the authors, so it was not added.

11. P.4, end of Section 2.3: in my opinion, some important aspects are not mentioned: in January and February, solid precipitation can occur, was it the case? To this respect, the temperature observations are crucial, but there is a large

discrepancy between the two types of instruments (around 3 deg C in the example in in Figure 4, nicely illustrating its importance: according to the Parsivel, it may snow at the end of the day, while it would only be rain according to the PWS100...); The instruments seem to be close to the edge of the roof, raising concerns about turbulence and wakes; what is the direction of the dominant wind at this site? How does it align with the respective orientation of each disdrometer? These are important aspects to clarify to better assess the quality of the data and the possible applications.

The reviewer is right, and discussion was expanded. Indeed the devices are on edge of the roof, and it might cause some issues notably with regards to the wind. Hence the installation of two perpendicular devices. This was clarified in the text. With regards to solid precipitation, basically no snow was recorded during this period except a little on February 18. Snow can be identified through the recorded fall velocity of the hydrometeors. indeed for a similar size, snowflakes will fall much slower than rain drops

12. P.7, l.7: how are treated the possible zeroes when integrating the DSD in time?

Authors do not understand the point, since p.7 l.7 is not discussing DSD...