#Response to anonymous referee #2

We would like to thank the referees for their helpful comments, especially about the description of the objectives and limitations of the method presented in this paper (abstract, introduction and section 2.1), and about the handling of the turbulence (section 4.4.2.). These comments will help to improve considerably the quality of the paper.

In this document, for each comment (black font), we display an answer (red font) as well as the intended changes to the manuscript (italic red font).

Anonymous Referee #2
Received and published: 8 February 2016

This paper describes how 4-D-VAR assimilation can be used to estimate the raindrop size distribution and vertical air motion in the vertical column by combining surface disdrometer and 24-GHz vertically pointing radar Doppler velocity spectra data. I find this technique to be very interesting and would like to incorporate this technique into other radar retrievals.
This paper is well written and demonstrates how 4D-VAR assimilation can contribute to studying rain microphysics deduced through radar observations. After addressing a couple minor points (comments #1 through #9) and a clarification on the influence of turbulence on the assimilation procedure (comment #10), I think this paper would be a good contribution to the journal of Atmospheric Measurement Techniques.

Specific Comments (in page order)

1. Page 12393, line 18. I'm a little confused with this sentence. Is the "last term" referring to the background term "J_b" or to "J_o" (which is the last term of equation (7))? Please clarify in the text.

It refers to J_b.

"This last term keeps the solution…" replaced by "J_b keeps the solution…".

2. Page 12393, line 19, equations (7) and (8). Please be explicit in the text and mention that "J_b" is dropped, or eliminated, from the cost function.

Ok.

"In our case, we suppose that there is no background available" replaced by "In our case, we suppose that there is no background available, so that J_b will be dropped from the cost function."

3. Page 12393, equation (8). Is the cost function evaluated for all time steps and height steps of the storm? If yes, can the manuscript clarify, or emphasize, that the cost function is valuated for all time-height observations and not for individual radar range gates?

We are not sure that we understand the remark. Excuse us if we do not answer correctly.
There are observations regularly, almost at each height (radar in the air and disdrometer on the ground) but not at each time step. But the Doppler spectra and disdrometer histograms on the ground are time averages, so that globally the entire time axe is covered.

We propose to add this paragraph page 12393, after line 25:
"In the end, the cost function J($\widetilde{\boldsymbol{x}}$,$\widetilde{\boldsymbol{w}}$) is a function of both the top DSD parameter field ($\widetilde{\boldsymbol{x}}$) and the wind field ($\widetilde{\boldsymbol{w}}$) that allows to compute the overall distance between a propagated set of

parameters $(\tilde{x},\widetilde{w})$ and the observations. By minimizing the cost function the algorithm will estimate an optimal set of $(\tilde{x},\widetilde{w})$ minimizing this distance. Once the assimilation process achieved, for each time step, we will have a top DSD parameter and wind values for all heights."

4. Page 12400, line 8. Please clarify in the manuscript the significance of 4900 time intervals. Specifically, are there 4900 10-second time steps, or 4900 2-minute time steps?

Here it is 4900 5s time steps (time step of the model). 10s and 2min are the observations resolutions.

We clarify it in the text by adding "We remind that, for numerical purposes, the time and height steps were chosen to be Δz=100m and Δt=5s$".

5. Page 12401, line 22. For the simulations, the top DSD parameters are set using the surface disdrometer observations. Thus, for the simulation, the column is bounded on the top and bottom by similar DSD parameters.

At this stage, we want to be able to run a twin experiment. So we need a realistic set of true unknowns of the propagation model to generate the corresponding observation set.
In the simulations, we indeed use the surface disdrometer observations (only rain rates, not DSD shape records) to obtain true unknowns of the parametric field (DSD parameters field $(\tilde{x})$). We also generate true unknowns corresponding to the wind field $(\widetilde{w})$. The $(\tilde{x})$ are then propagated through the model to obtain the "true" observations. But these true observations are not exactly the disdrometer data (they have been merged during the fall through the propagation model).
Moreover, at the very beginning of the assimilation phase, the top DSD parameters are set to arbitrary constant low values (alpha=1 ; k=0.8 ; theta=0.0002, leading to a 0.19mm/h rain rate), independently of the simulated truth.

For the assimilation of the real observations, are the top DSD parameters initially set using the surface disdrometer observations so that the column is bounded with similar DSD parameters? Please address in the manuscript the initial conditions of the top DSD parameters.

For the real case study, we initialize the top DSD parameters in the same way, and so independently of the disdrometer data. In any cases, we found that the outputs of the assimilation process are poorly dependant on the initialization states.

*To make it clearer, we add the following lines at the end of section 2.2.5. (page 12397) :
"We finally note that we have to initialize the unknowns of the problem $(\widetilde{w},\tilde{x})$ at the very beginning of the assimilation process. We found that this initialization is not critical. Consequently, we will initialize the wind field $(\widetilde{w})$ at 0 and the 3 gamma parameters time serie $(\tilde{x})$ at constant very low values, namely: $\alpha = 1\,; k = 0.8\,; \theta = 2.10^{-4}$".*

6. Page 12402, line 26. Understanding the influences of the convolution is very important for DSD estimation. Since the forward operator in the assimilation algorithm does not include a convolution broadening term, the assimilated DSDs will be broader than the true DSDs to account for the missing broadening effects. This is discussed in work earlier than Tridon et al 2013, in particular, include the work of Williams (2002, Radio Science, 10.1029/2000RS002603) and Gossard 1994, J. Atmos. Oceanic Technol., 712-726) in the discussion.

This is was we thought at first but it is not necessarily the case. The convolution broadening term is indirectly modelled in the forward operator. See answer to note 10 for details.

The missing references will be added in the text.

7. Page 12403, equation (17) and line 5. The assimilation procedure is using the convolution in the forward model to broaden the Doppler spectra which was also used in Williams and Gage (2009, Ann. Geophys, 555-567) for DSD retrievals using vertically pointing radars.

Ok.

The reference will be added in the text.

8. Page 12405, line 16. Obtaining the result that turbulent broadening only impacts the spectral width and not the reflectivity-weighted mean downward velocity in the simulations confirms the robustness of the assimilation procedure and are expected results from prior work (Williams (2002, Radio Science) and Williams and Gage (2009, Ann. Geophys.).

Ok.

We propose to add this sentence page 12405, at the end of line 19 :
"Obtaining the result that turbulent broadening only impacts the spectral width and not the reflectivity-weighted mean downward velocity in the simulations confirms the robustness of the assimilation procedure and are expected results from prior work (Williams 2002 and Williams and Gage 2009)."

9. Page 12405, line 27. I'm confused by the term "second solution". This term appears to be out-of-place and not used in other sections of the manuscript. Please clarify or correct this section of text.

Ok.

The sentence is replaced by "Secondly, and because this way of acting (modification of DSD shapes) is too costly, the algorithm uses another way, which consists…"

It should also be made clearer by the modifications proposed for your comment 10.

10. Page 12406, lines 1-17. I don't understand how the fluctuations at the 5 s resolution (line 9) which lead to broader Doppler velocity spectra at the 2 min resolution are a result of turbulence and not a result of broader DSDs. The assimilation procedure does not include a convolution to produce Doppler velocity spectra at the 5 s resolution which would then, in a real radar system, be accumulated over a 2 minute window to produce broadened Doppler velocity spectra. From my reading of the assimilation procedure, air motion and 3-parameters of the DSD are estimated at each time-height grid box (from page 12392, lines 14-15) and are accumulated (or averaged) over the 2 minute window.

Not exactly: the wind field $\tilde{\boldsymbol{w}}$ is estimated at each time-height grid box, but the DSD parameters field ($\tilde{\boldsymbol{x}}$) is only estimated at the top of the column, and then propagated at every heights by the forward model.

Since the assimilation procedure remains in the DSD parameter domain over the 2 minute window, any increase in Doppler velocity spectrum (in the radar domain) is due to a spread of the DSD parameters. If the Doppler velocity spectra were estimated at each 5 s interval and the Doppler velocity spectra were estimated and accumulated (and averaged) over the 2 minute window, then an increased Doppler velocity spread would be due to the time-evolution of Doppler velocity spectra over the2 minute window. As written, the conclusion that the turbulence is reproduced and the DSD is not modified needs to be clarified (see page 12406, lines 15-17 for the text that needs to be clarified). This conclusion may be true, but the logic leading to this conclusion is weak and should be addressed before publishing.

Both the DSD parameters field and wind field are estimated at the 5s model resolution. And the Doppler spectra deduced from both the DSD and the wind field are averaged to be compared to the observed spectra at a 2min resolution.
Consequently, if the 5s winds oscillate inside the 2min observation window, it will result in shifting the 5s corresponding 24 spectra alternatively towards small and large Doppler velocities. Once averaged over the 2min time period, these 24 shifted spectra will result in a final 2min spectrum with a spectral width larger than all the 5s spectral widths, whatever the original DSDs. This is the way used by the algorithm to mimic the turbulence.

Let's suppose a DSD histogram at 5s constant over a 2min observation window. Then let's convert these DSD into a "theoretical" spectrum (Eq.13). We get 5s spectra also constant over the 2min time period. Fig. A1, blue line shows an example of such spectra at 24GHz.
Let's suppose that for this 2min time period, we have observed a spectrum much larger than this theoretical spectrum (see a simulated example on Fig. A1, black line).
Now let's suppose that the assimilated wind (at 5s) over this 2min observation window shows Gaussian fluctuations, as on Fig. A2.
If we add the effect of these 5s winds on the 5s (constant) theoretical spectrum, we will get final spectra (corresponding to Eq.14 without attenuation) shifted right and left according to the wind (see Fig. A1, dotted lines). When accumulated (averaged) over the 2min. observation window, we will get a 2min final spectrum much larger (broadened) than the theoretical one and possibly close to the observed one (Fig. A3, red line). The broadening here comes only from wind fluctuations at 5s, and not from any broadening on the DSD parameters.

Note that until now, absolutely nothing in our algorithm ensures that the wind fluctuations inside the observation windows are Gaussian. Moreover, the results (intensity of the fluctuations) will highly depend on the observations resolution. So this model is not a formal numerical implementation of the Gaussian convolution of Eq.17 but only an easy way to use the degrees of freedom of the model to mimic the turbulence.

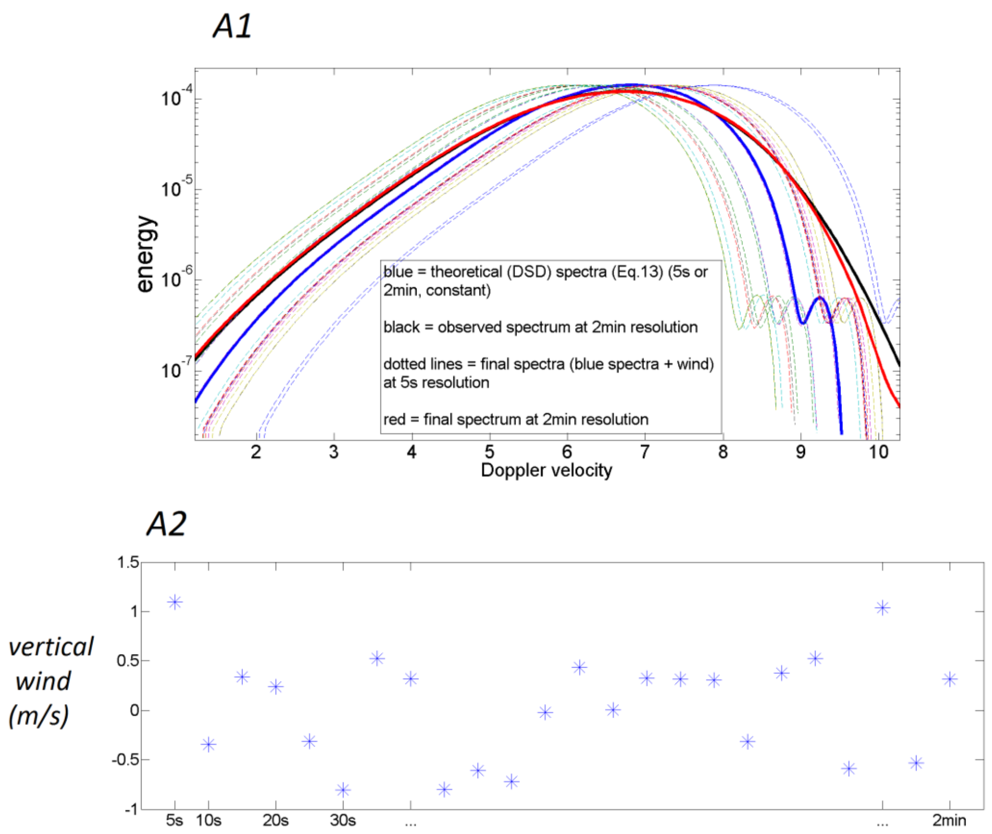

Figure 1 : Illustration for the answer to comment 10.

I tried to make it clearer by modifying a part of section 4.4.2. (page 120406, line 3). I hope now it is understandable now:

"We have seen that all the observations are integrated over a $2min$ observation window (Sect. 3.1), while the wind is retrieved at the $5s$ model resolution~~, resulting in $2min$ averagings consistent with the turbulence broadening~~. Consequently, if the $5s$ winds oscillate inside the $2min$ observation window, it will result in shifting the $5s$ corresponding spectra alternatively towards small and large Doppler velocities. Once averaged over the $2min$ time period, these shifted spectra will result in a final $2min$ spectrum with a spectral width larger than all the $5s$ spectral widths, whatever the original DSDs. This is the way used by the algorithm to mimic the turbulence."

The following three pieces of text are also impacted by the 5 s to 2 minute window effects:
a. Page 12407, lines 22-24. The sentence starting with "We have seen that a 2 min : : :" needs to be consistent with the modified text.
b. Page 12408, lines 3-7. This text describes nicely how the 3 DSD parameters are adjusted to account for observed broadened Doppler velocity spectra.
c. Page 12413, lines 18-20. This concluding text needs to be modified.

The following points should be answered by the clarification of section 4.4.2