The study suggests a methodology to account for droplets inertia in the calculation of Doppler spectra broadening due to turbulent air motion. The paper is well-written and easy to follow. The manuscript presents one theoretical and one practical experiment to illustrate the rationale of the new methodology and its effectiveness in better representing the broadening of Doppler spectra due to air turbulence. The figures are of good quality and the conclusions follow the results obtained by the aforementioned experiments.

However, I am not completely sure about the formal correctness of the proposed method. It might be that the authors adopted some implicit assumptions that are just not clear to me. It is possible that those assumptions are explicit in Lhermitte (2002), whose framework the authors declared to follow. I, unfortunately, do not have access to the book at the moment, but, in any case, I recommend the authors to provide a concise, but exhaustive description of all the physical assumptions made in the development of their novel modeling technique.

I recommend the article to be published after a major revision. In doing so, I am aware that my concerns might arise from the mere misunderstanding of the assumptions made in the model formulation. If this is the case it is sufficient for the authors to better describe the physics behind the mathematical formula written. On the other hand, from what I was able to understand, I am not entirely sure that the physical model developed is correct. The better matching of the simulated and observed Doppler spectrum might result from compensating errors in the model formulation. In this case, I suggest the authors to take their time to thoroughly review their work and resubmit at a later stage.

In this detailed review I am addressing first the 2 major issues I have in understanding the approach proposed by the authors. I also provide a python code I used to check one of my concerns to allow the authors to better understand my doubts. Finally in the last (5th) page I list some minor points.

1 Major points

The authors introduce the dynamical system in Eq. 1. From this equation, it appears that the only force acting on the droplet is the drag force and therefore, in absence of vertical wind, the droplet would never move or change its velocity. It might be that the authors already assume that the droplet fall at its terminal fallspeed (approximated in Eq 8) and thus they only consider the fluctuations around this, however they do not explain why the non-linear terms that would result in the equation of motion are neglected. Moreover, in this case, the manuscript suggest that the Reynolds number and thus the drag coefficient are calculated taking into account only the velocity fluctuations, which would result in completely different values with respect to what is happening in reality (Re and Cd derive from the actual speed of the drop relative to air). Additionally, they do not provide evidence that Eq 8 is consistent with the assumptions made in section 3.1. I will try to explain my concerns regarding these two points in the following subsections.

1.1 Dynamical system

I am not an expert in turbulence, however I would have set up the problem as follows. A 1D reference frame is fixed with respect to the position of the radar and with an arbitrary z-axis oriented upwards. In this reference frame the equation of motion is

$$F - mg = m\frac{\mathrm{d}V_D}{\mathrm{d}t} \tag{1}$$

where F is the drag force, opposed to the gravitational force, mg and expressed as in the text as

$$F = \frac{C_d S (V_w - V_D)^2 \rho_a}{2} \tag{2}$$

Here V_D is the drop velocity with respect to the radar (it is negative when the drop falls) and V_w is the vertical wind speed (it is positive for updrafts and negative for downdrafts). In this framework one can clearly see that the gravitational force is always pointing downwards, while the drag force always points upwards. Because of that, the net force acting on the drop, can either point upwards or downwards depending on which of the two forces prevails. In the manuscript, the only acting force is drag, and, because of the squared velocity term, it is always positive, meaning that the net force would always push in the same direction (which is clearly not the case in reality).

A steady-state solution can be asymptotically reached when V_w (and also the other variables) do not depend on time t. In this condition the net force on the water drop is null and the drag force balance the gravitational force $F = mg$; thus it is possible to derive the terminal fallspeed of the drop relative to the vertical wind speed as

$$V_w - V_D = \sqrt{\frac{2mg}{C_d S \rho_a}} \tag{3}$$

up to here, I have assumed that all variables do not depend explicitly on time. Sure C_d depends on $V_w - V_D$, but still, they will reach a steady-state. ρ_a and g being a function of height should be considered time-dependent in a

Lagrangian sense, but I assume here that the integration time of the equation of motion is small enough to consider these variables constant.

Now, we introduce a turbulent vertical wind by separating a steady-state mean value from its fluctuations around the mean, similarly we will have a time-varying drop velocity

$$V_w(t) = \overline{V_w} + V'_w(t) \tag{4}$$

$$V_D(t) = \overline{V_D} + V'_D(t) \tag{5}$$

we substitute this in Equations 1 and 2

$$\frac{C_d S(\overline{V_w} + V'_w(t) - \overline{V_D} - V'_D(t))^2 \rho_a}{2} - mg = m\frac{\mathrm{d}(\overline{V_D} + V'_D(t))}{\mathrm{d}t} \tag{6}$$

we rearrange the terms and develop the square and recognize that $\overline{V_D}$ is not time dependent

$$\frac{C_d S \rho_a}{2}\left[\left(\overline{V_w} - \overline{V_D}\right)^2 + \left(V'_w(t) - V'_D(t)\right)^2 + 2\left(\overline{V_w} - \overline{V_D}\right)\left(V'_w(t) - V'_D(t)\right)\right] - mg = m\frac{\mathrm{d}V'_D(t)}{\mathrm{d}t} \tag{7}$$

If we assume that the fluctuations $V'_w(t)$ and $V'_D(t)$ do not cause a significant change in C_d one can recognize that the squared term of the difference of the mean velocity balances the gravitational force because it should be the steady state solution. What is left is the following

$$\frac{C_d S \rho_a}{2}\left[\left(V'_w(t) - V'_D(t)\right)^2 + 2\left(\overline{V_w} - \overline{V_D}\right)\left(V'_w(t) - V'_D(t)\right)\right] = m\frac{\mathrm{d}V'_D(t)}{\mathrm{d}t} \tag{8}$$

which differs from Eq 1 of the manuscript by 2 key elements:

1. The first term (squared difference of velocity fluctuations) resembles Eq.1 from the manuscript, but it explicitly address only the fluctuations with respect to the mean flow.

2. There is an additional term in the drag force that results from the non-linear dependency of such force with respect to the relative air velocity of the drop. Since drag depends on the square of the relative velocity, the same velocity fluctuation would result in different drag force for slow and fast falling particles. If one looks at the quantities involved it seems that this term should actually be the most relevant. Also, this is the term that clearly changes sign (direction) depending on whether the V_w fluctuation is raising or decreasing, allowing for faster or slower fallspeeds.

As I said, I am not an expert on this. It will be easier for me to understand what is the physical basis of the developed methodology if the authors provide a more complete physical description of the system and also point out which terms are neglected and why.

Additionally, in my view, the velocity term that should go into the calculation of the drag coefficient should be $\overline{V_w} - \overline{V_D} + V'_w(t) - V'_D(t)$, even though it is not specified I have the feeling that the authors only consider the fluctuations $V'_w(t) - V'_D(t)$ there, which would inevitably lead to a much higher C_d (see also the bottom row of my example figure 1 in the next subsection) because it would mean a much smaller value. In view of this, it might be that the incorrect calculation of C_d partially compensates for the missing of the non-linear term in the calculation of the time-varying drag force.

1.2 Calculate terminal fall velocity

If the equations in Sec 3.1 describe the motion of a drop in the fluid I expect that they hold also in absence of turbulent motion. Specifically I expect that the terminal fallspeed of a drop results from the equilibrium of the gravitational force with the drag force as described in equations 2 to 3 of the manuscript for internal consistency. In the following plot I calculate such terminal fallspeed and I compare it with the approximation of Eq.8 (a python code to reproduce these passages is also provided).

From this simple comparison it seems to me that the two relations are consistent up to a droplet size of 2 mm. After the assumption made in the calculation of the drag force is not consistent anymore. If we take the blue curve (Eq.8 Lhermitte 2002) as the most realistic one, it would mean that in reality these larger and faster drops experience a drag force that is higher than the one described in Sec 3.1.

Could you please provide an explanation for this inconsistency? Is it possible that the presented formulas to compute the aerodynamic drag are only an approximation valid for slower speeds and/or small spheres?

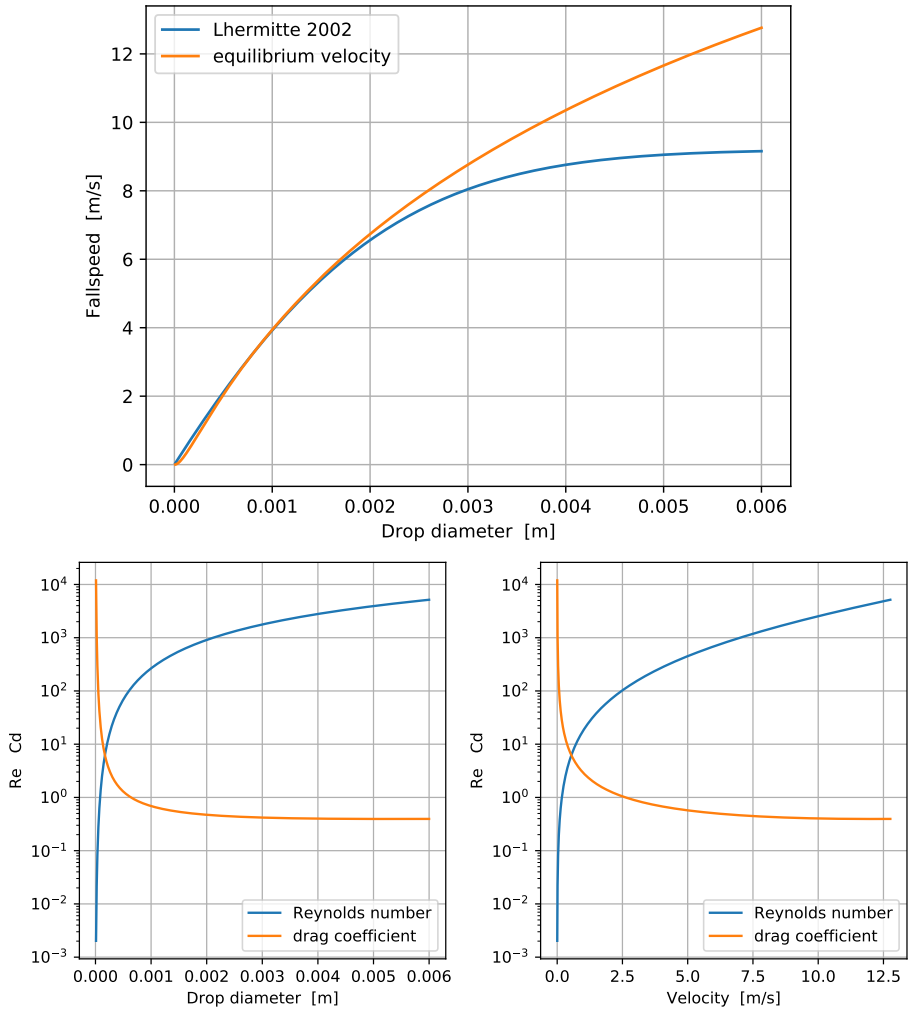

Figure 1: Top:Drop terminal fallspeed as a function of size as calculated by Eq.8 (Lhermitte 2022) and as resulted from the set of dynamic equations of Section 3.1. The code to reproduce this figure follows. Bottom: Reynolds number and drag coefficient as a function of the drop diameter (left) and steady-state relative fallspeed (right).

```
import numpy as np
import matplotlib.pyplot as plt
plt.close('all')

g = 9.81 # m s**-2
rho_a = 1.22 # kg m**-3
mu = 1.81e-5 # kg m**-1 s**-1

def VtD(D): # Lhermitte 2002 approximation for fallspeed
    d = D*100.0 # m to cm
    v = 920.0*(1.0- np.exp(-6.8*d*d-4.88*d))
    return v/100.0 # cm/s to m/s

def reynolds(v, D):
    return v*D*rho_a/mu

#def drag_coeff(r):
#    logr = np.log(r)
#    return np.exp(1.445 + logr*(-0.8796 +logr*(0.0642+logr*0.0104)))
# Using natural logarithms does not converge to good results... let's try base 10
```

```
def drag_coeff(r):
    logr = np.log10(r)
    return 10.0**(1.445 + logr*(-0.8796 +logr*(0.0642+logr*0.0104)))


def Vdyn(D): # equilibrium fallspeed
    S = np.pi*D*D*0.25
    m = D*D*D*np.pi*1000/6
    v0 = VtD(D) # first estimate

    Re = reynolds(v0, D)
    Cd = drag_coeff(Re)
    v1 = np.sqrt(2.0*m*g/(Cd*S*rho_a))

    while(np.abs(v0-v1)/v0 > 0.01): # use simple iterative method to try to reach convergence
        v0 = v1
        Re = reynolds(v0, D)
        Cd = drag_coeff(Re)
        v1 = np.sqrt(2.0*m*g/(Cd*S*rho_a))
    return v1

D = np.linspace(0.01e-3, 6e-3, 1000)
plt.plot(D, VtD(D), label='Lhermitte 2002')
Vd = np.array([Vdyn(d) for d in D])
plt.plot(D, Vd, label='equilibrium velocity')
plt.xlabel('Drop diameter  [m]')
plt.ylabel('Fallspeed  [m/s]')
plt.legend()
plt.grid()
plt.savefig('terminalVSlhermitte.pdf')

fig, (ax0, ax1) = plt.subplots(1,2,figsize=(8, 4), constrained_layout=True)
ReD = reynolds(Vd, D)
ax0.semilogy(D, ReD, label='Reynolds number')
CdD = drag_coeff(ReD)
ax0.semilogy(D, CdD, label='drag coefficient')
ax0.grid()
ax0.set_ylabel('Re   Cd')
ax0.set_xlabel('Drop diameter  [m]')
ax0.legend()
ax1.semilogy(Vd, ReD, label='Reynolds number')
ax1.semilogy(Vd, CdD, label='drag coefficient')
ax1.grid()
ax1.set_ylabel('Re   Cd')
ax1.set_xlabel('Velocity  [m/s]')
ax1.legend()
fig.savefig('Reynolds_Cd.pdf')
```

2 Minor points

1. Title and Line 13 - the generic term "particle" suggest that the model is applicable to any hydrometeor. However it seems clear to me that the proposed methodology is applicable only to liquid drops. Perhaps it is better to specifically address only liquid precipitation.

2. Line 53-56: I believe that there are some additional contributors to the spectral broadening. For example the finite beamwidth allows for some of the horizontal wind component as well as the vertical shear of the horizontal wind to cause some spectral broadening.

3. Line 89 - The data section seems a little misplaced here, it makes a sudden interruption to the introductory argument which focuses on the methodology and the methodology itself which is presented in Sec 3. Sec. 2 is very short and the data are used only in section 5 which is again quite short. Since the method is the central focus of the paper I suggest to make Section 2 a subsection of the current Section 5.

4. Line 102 - "turbulence" - turbulent

5. Line 109 - The title of this subsection explicitly mention turbulence. However there is no effect of turbulence explicitly taken into account. The subsection merely list the equations used to define the dynamics of spherical objects in a fluid regardless of its laminar or turbulent status.

6. Figure 3 - y-label velcoity - velocity

7. Sec 4.2 (and partially also Fig 1) it is not clear to me how the equation of motion is resolved. Is a numerical method for the solution of ordinary differential equations used? What is the time resolution of the method? Is the power spectrum of turbulent air motion truncated at a certain frequency? what is the expected uncertainty in the determination of the drop speed?

8. Line 370 - I am not sure how the DSD shape might shift the location of the scattering notch. To me the notch occurs at a specific size and provided that there is a well-defined velocity-size relation it would occur at a specific velocity regardless of the DSD. DSD discrepancies might only move the notch up or down in the spectral power. At lines 229-230 it is stated that Mie scattering theory is used for the scattering computation which would imply perfectly spherical raindrops, However, I think that such big raindrops are not spherical but rather slightly oblate. This means that their length along the vertical (which is the one relevant for the Mie resonances considering the vertical propagation direction) is smaller. Thus, a larger oblate raindrop is needed to produce a Mie resonance effect along the vertical direction than a spherical one. I suggest the authors to try using a spheroidal approximation of raindrops for scattering.

9. Code/Data availability - the authors include reference to a github repository owned by a person which is not listed among the co-authors. It is fine but I would suggest to include not the github repository, which is subject to modifications, but a more permanent link. Luckily, the repository offers also a packaged version that got a DOI on zenodo. It is ok to keep the reference in the data availability section, but zenodo offers the option to properly give author attribution, have it in the list of references, and to pin the citation to a permanent link of a specific version of the software.
   I take the opportunity to also invite the authors to publish their code openly which would be of great benefit for the radar community and for the repeatability of their results. The AMT journal invites all authors to publish their data and codes, and in this particular case it would have greatly helped in the understanding of what has been done in the study