Comments on 'On the Impact of thunder on cloud ice crystals and droplets' by K. Kourtidis, S. Stathopoulos and V. Amiridis.

Summary

I have read with interest the manuscript. Although a bit long, it is a reminder of the possible effects that thunder may have on ice particles and droplets. However, for the reasons mentioned below I cannot recommend it for publication.

Main points (to be explained later)

Contrary to the article's title and conclusions, the article does not consider, by far, the impact of thunder on cloud particles. At best it is a parametric study of two of the effects they consider: Breakup in shock waves and agglomeration in an acoustic wave. But there are problems with both. The first is considered on the basis of an erroneous assessment of the air velocity in a shock; the second is parametrized on the basis of models and calculations that are not applicable to either clouds or thunder. Incidentally, the case they consider -sinusoidal acoustic wav, has been thoroughly studied in articles not cited by the authors.

Also important is the fact that the authors include, without explanation, certain results upon which they base their estimates. The main examples of this are two equations, both central to their arguments, which appear in Sec. 2.2. Because of that omission I include in my comments an extended discussion of the origin and limits of those equations.

Sec. 2.1 Particle breakup in the supersonic thunder shockwave front

The main point considered in this section is the breakup of droplets produced by shock waves. This effect is well-known and well-understood. The contribution of this section is Table 1, which shows their estimates for the minimum sizes of particles that will break up, using as a criterion the experimentally-obtained value for the Weber number that produces droplet breakup, We_{cr}, which they say is 12. This value is in the range of values that have been found (although the actual value depends on the type of breakup). The problem I have in this regard is that they take that value to apply to the breakup of solid particles. I know of no experimental research supporting that application.

In any event, the authors tabulate the minimum sizes of some types of particles that would breakup when exposed to shock waves whose fronts move at 60 km/s or 1 km/s. To find those sizes they use the definition of the Weber number for breakup, solving for the diameter, i.e,

$$d = \sigma We_{cr} / \rho_g u^2$$

Here u is the air velocity. This is fine, but in getting the values of u corresponding to those two shock waves, the authors make a fundamental error: According to them (line 80) 'the relative air velocity equals the front velocity.' This is incorrect. The front of a shockwave <u>always</u> moves faster than the gas behind it. This is particularly important for the shock speeds they have in mind. The issue here is not the numerical difference but the erroneous conception of what a shock wave is.

As for the shock speeds they use, 60 km/s and 1 km/s, I assume each may occur within a short distance from the lightning discharge, but I wonder about the effects on nm size ice particles of the high temperatures that exist behind the corresponding shocks, as well as on the molecular composition of air in those conditions. [Incidentally, a very good source of information about such matters is Zeldovich and Raizer, *Physics of Shock Waves and High Temperature Hydrodynamic Phenomena*, 2 Vols., Academic 1966.]

To conclude, there is nothing new in this section other than an erroneous estimate of the minimum droplet sizes that result in breakup and the questionable use of information obtained with water droplets for the case of solid particles.

<u>Sec. 2.2 Particle agglomeration in the thunder sonic field</u>

In this section the issue of agglomeration in 'the thunder sonic field' is parametrized in the basis of a procedure, called orthokinetic agglomeration by some authors. Although there are several variations, the technique refers to agglomeration of particles due to the relative velocities of small particles of different sizes responding to single-frequency sound wave . The literature is vast but a basic source is Mednikov, *Acoustic coagulation and precipitation of Aerosols*, Consultant Bureau, 1965. Most of the work in this area has been done with high-frequency sound waves, but some time ago Temkin (Droplet agglomeration in a shock wave flow field, *Phys. Fluids*, June1570, not cited) showed that agglomeration can be effectively produced with low frequency waves that have shock fronts.

As remarked later, much closer to the work in review is Temkin's study of agglomeration of polydisperse distributions by monochromatic sound waves (Temkin, Gasdynamic agglomeration of aerosols. I, Acoustic waves, *Phys. Fluids*, July 1994, not cited).

In this work, the authors use the term 'resonance ratio' for the ratio of the displacement (or velocity) of a particle in a sound field to the gas velocity in the wave, usually called the 'Entrainment Ratio' in the literature. In any event, the ratio as given by eq. 1, or

$$\eta = {}^{U_p}\!/_{U_0} = \left[1 + \left(\omega\tau_p\right)^2\right]^{-1/2} \qquad (1)$$

Note that the two velocities appearing in this equation are not defined, nor is it stated anywhere in the text that the equation applies only in very limited cases: spherical particles, single-frequency sound waves, very small gas velocities (small Reynolds number) and frequencies that are not large.

The authors define the quantity τ_p appearing in their Eq. 1 is defined as

$$\tau_p = {}^{\rho_p d^2}\!/_{0.0003286}$$

This 0.00032886 is not only unpleasant, but it also obscures the fact that it includes the value of the (dynamic) viscosity coefficient of the gas ($\bar{\mu}$) sustaining the motion, with which that the equation should be written differently, for example, with $18\bar{\mu}$ instead of that number.

This τ_p, called the relaxation time in the literature, is a simple time scale for small-amplitude motions. In any event, the issue is not the way τ_p is written or why is it called that way, but whether Eq. 1 is at least approximately valid for a parametric study of effects that supposedly take place in a cloud as a result of thunder.

In any case, Eq. 1 is apparently used in the article to obtain the basic relation that the authors use to estimate the 'Effective Agglomeration Length' (L_{eff}), for a number of particle sizes. That equation appears on line 221. From the text one gathers that η_{12} is proportional to the difference in the velocities of two particles, but the equation ignores the fact that those velocities have different phases in the oscillatory motion of the gas. A result that incorporates those phase differences was given by Temkin some time ago (Temkin, *Phys. Fluids*, 1994).

$$u_v - u_w = \frac{\omega\left(\tau_v - \tau_w\right)}{\sqrt{1+\left(\omega\tau_v\right)^2}\sqrt{1+\left(\omega\tau_w\right)^2}}\sin\left(wt' - \phi_v - \phi_w\right)$$

These velocities are scaled with the maximum air velocity in the wave (the same U_0 that appears without definition in the author's Eq. 1). Comparing this with the equation given by the authors we see that η_{12} refers to the *magnitude* of the velocity (or displacement) difference for two small spheres in a monochromatic sound wave. In any case, using absolute values is fine in a parametric study.

The issue here is that the authors parametrize the effect using particle size ranges and number concentrations that may or may not apply to clouds. It should be remembered that while a given size distribution specifies a size range, the opposite is not true. Particle size distributions in actual clouds are of considerable importance in the assessment of agglomeration effects produced in actual clouds by any effect. Incidentally, the theoretical and numerical work by Temkin cited above uses the coagulation equations to evaluate the agglomeration effects produced by a monochromatic sound wave on a specified particle-size distribution.

Returning to the article in review. One more parameter is needed before Eq. 1 can be used, and that is the gas velocity which the authors now call U_g in the equation they give (without explanation) on line 225, which I re-write as follows:

$$\rho_g c^* U_g = 10^{\left[(SPL-94)/20\right]}$$

I am sure that at least some of your readers will, like myself, be mystified by this equation. So please forgive me for including here elementary material that de-mystifies it and that also tells us what is the meaning of its rhs, and what are the limitations imposed on the values of U_g found this way.

To start, I assume that c^* is the sound speed in the gas in ambient conditions. If so, the product on the lhs has the dimensions of pressure, which in turns shows that the quantity on the rhs must be a pressure, or more specifically an acoustic pressure because the SPL appears there. Let's call that pressure p'. Its value follows from the definition of the SPL, namely, $SPL = 20\log_{10}\left(p'/P_{ref}\right)$, where P_{ref} is the reference pressure, whose value is 2x10^{-5} N m^{-2}. Solving for p' and using the fact that $20\log_{10}\left(P_{ref}\right) = 94$, we find that the rhs is equal to p' so that the above equation can be written as

$$p' = \rho_g c^* U_g$$

This is, of course, a limited form of a general theoretical result from acoustics that specifies the pressure fluctuation in a plane (unidirectional) acoustic wave, moving adiabatically with speed c* and inducing the fluid (a gas in this case) to move with speed U_g. The limitation here arises because p' was derived from a SPL so that both p' and U_g are time-independent. In any case, the derivation shows that the gas velocity obtained from value of the SPL is not arbitrary but is constrained by whatever conditions apply to the equation above.

In addition, it should also be remembered that the equation shown on line 221, which uses that velocity, has its own set of requirements. That is, in addition to be limited by plane, isentropic sound waves, that equation is limited to waves in which the time variations of pressure and air velocity are sinusoidal. So far as I know the sounds generally associated with thunder do not fit that category.

In this context, the authors use some reported measurements of SPLs that are associated with the occurrence of 'thunder'. I have read some of the papers the authors mention and it is evident that there is no consensus as to what the measurements mean. Nor do we know whether they refer to a single, direct wave or to reflections or to some other effect. That is, a microphone will pick up whatever air vibrations exist in its immediate vicinity and will interpret them as an acoustic pressure. This means that the SPLs it reports cannot generally be tied to any specific motion. In other words, an SPL value cannot by itself tell us what were the air motions that produced it.

<u>Sec. 3 Conclusions</u>

Among the conclusions one finds the following statement (lines 14 and 15): "The results presented here demonstrate that thunder has the potential to alter the size distribution of cloud droplets in thunderclouds." For the reasons stated here this statement is not supported by the estimates of the effective agglomeration lengths presented in the article.

At the end of the article the authors bring up charge separation, obviously the most important issue as far as lightning is concerned. Their short discussion is hypothetical and I am not sure it belongs in the article. But charge separation due to droplet breakup is relevant and was considered some years ago by Dreyfuss and Temkin (Charge separation during rupture of small water droplets in transient flows: Shock tube measurements and applications to lightning, *J. Geophys. Res*. 88, C15. 1983