Review of "University of Warsaw Lagrangian Cloud Model (UWLCM) 1.0: a modern Large-Eddy Simulation tool for warm cloud modeling with Lagrangian microphysics" by Dziekan et al. (doi:10.5194/gmd-2018-281)

The manuscript describes the University of Warsaw Lagrangian Cloud Model (UWLCM), a combination of Eulerian LES and Lagrangian cloud microphysics. Therefore, the manuscript can be seen as a continuation of Arabas et al. (2015, doi:10.5194/gmd-8-1677-2015) and Jaruga and Pawlowska (2018, doi:10.5194/gmd-2018-96), which describe earlier versions of the Lagrangian microphysics now applied in UWLCM. Additionally, the manuscript addresses different approaches to time sub-stepping and the number of simulated Lagrangian particles necessary for convergence, which are not only of value for UWLCM but also the larger Lagrangian cloud modeling community.

The manuscript fits the scope of Geoscientific Model Development. It is generally well written, but demands some language editing. Overall, there are a few slightly major comments and quite some minor issues that need to be addressed before advancing in the publication process.

Major Comments

Diffusion of superdroplets (p. 10, ll. 5 – 6; p. 5, l. 14; p. 13, l. 25). The motion of superdroplets in only determined by the resolved-scale LES air motion. Turbulent diffusion, which is considered in the LES implicitly due to numerical diffusion, is not considered for the superdroplets. This underestimates the diffusion of superdroplets and liquid water in all simulations, indicating that the fields of water vapor, temperature, and liquid water are not in physical agreement. I appreciate that the authors are candid about this issue, but they should address the implications of this discrepancy more clearly. Especially because there are methods available and to consider subgrid-scale motion of Lagrangian particles (e.g., Weil et al. 2004, doi:10.1175/JAS-3302.1), which are already in use in other Lagrangian cloud models (Sölch and Kärcher 2010, doi: 10.1002/qj.689; Hoffmann et al. 2017, doi: 10.1175/JAS-D-16-0220.1). One example where this neglect probably matters is the number of simulated cloud droplets N_C. The authors state that N_C is higher in UWLCM compared to other models (p. 13, l. 25). They explain this by the lack of numerical diffusion. This is right. However, the neglected turbulent diffusion of superdroplets also contributes to a higher N_C and needs to be mentioned.

Comparison of different time sub-stepping schemes. The comparison of the per-particle and per-cell sub-stepping approaches with a simulation without sub-stepping but a commensurately reduced timestep of 0.1 s is not very helpful due to the strong interaction of microphysics and dynamics. This becomes very clear for the three-dimensional simulations, in which the 0.1 s simulation enables a more detailed, and probably more adequate representation of this interaction. As a result, the entrainment rates vary significantly among the different model setups as seen in Fig. 5b, with commensurate effects on the liquid water path (decreases due to stronger entrainment), cloud base height (increases due to stronger entrainment), and indirectly precipitation (increases with liquid water path). To derive useful conclusions, it is necessary to untangle dynamical and microphysical effects. Therefore, I strongly suggest using either a kinematic driver providing each setup the identical dynamical forcing or to use the piggy-backing approach, which is actually part of UWLCM as stated on p. 22, ll. 12 – 13.

Minor Comments

P. 2, l. 9: Please clarify: Automated tests for what?

P. 2, l. 16 – 17: Focusing on precipitation is one aspect. Cloud cover might be an additional and very important second aspect to consider since precipitation might result in the transition from closed to open cells.

P. 3, l. 6 – 8: How does the auxiliary environmental state increase the *precision* of numerical calculations? Usually, these environmental states are necessary requirements to solve the system of

equations. Furthermore, the word precision usually refers to the number of significant digits of the solution. I do not believe that this is meant by authors.

P. 3, Eq. (3): It is explained later, but a brief description of what π is might be helpful at this point.

P. 4, Eq. (10): What are r and r_d?

P. 4, l. 12: What is so special about this definition of the relative humidity (the ratio of actual and saturation water vapor mixing ratio) to cite Lipps and Hamler (1982)? E.g., Clark (1973, doi:10.1175/1520-0469(1973)030<0857:NMOTDA>2.0.CO;2) defined the supersaturation (his Eq. (15)) also as the ratio between actual and saturation water vapor mixing ratio.

P. 4, l. 13: Consider replacing 0.622 by the ratio of the specific gas constant of dry air to the specific gas constant of water vapor (i.e., R_a/R_v).

P. 4, l. 14: Please comment if D and K include gas kinetic or ventilation effects.

P. 4, l. 19: For clarity, add "real" between "two" and "droplets".

P. 4, ll. 24 – 25: Superdroplets do not collide. Equation (12) states the probability that one real droplet of superdroplet j (or k) collects any real droplet of superdroplet k (or j).

P. 4, l. 27: Starting from (12), there are not necessarily ξ_j pairs of real droplets coalescing. The correct number is $\min(\xi_j, \xi_k)$.

P. 5, l. 14: The sedimentation velocity is explicitly considered in the motion of superdroplets. I believe this counts the (admittedly small) contribution of sedimentation twice since it is already considered in the LES velocity vector **u**, according to (3).

P. 5, ll. 23 – 24: Equation (14) is still "Eulerian" in the sense that is contains an advection term.

P. 6, ll. 3 – 5: State clearly that π is the pressure perturbation. Furthermore, I think the introduction "[that] it is characteristic for anelastic models that the pressure perturbation does not follow the ideal gas law" causes more confusion than clarification. I would omit it.

P. 6, 13 – 14, Shima et al. (2009) were not the first to advocate the integration of the squared wet radius. See, e.g., J.-P. Chen (1992): *Numerical simulations of the redistribution of atmospheric trace chemicals through cloud processes* (Doctoral dissertation, Pennsylvania State University), especially his Eq. (3.81).

P. 6, ll. 18 – 19: In what sense is condensation a fast process here? I think you need to be more specific. Arnason and Brown (1971, doi:10.1175/1520-0469(1971)028<0072:GOCDBC>2.0.CO;2) showed that for condensation a timestep corresponding to the phase relaxation timescale is sufficient, i.e., about 1 s or even longer for clean clouds. The requirement for a 0.1 s timestep arises, in my eyes, from the rapid change in droplet radius during growth at small radii. This is a well-known feature of stiff differential equations, as it is the case for the diffusional growth equation for droplets. Furthermore, how do you know that a sub-stepping timestep of 0.1 s is sufficient? In similar simulations of Grabowski et al. (2011, doi:10.1016/j.atmosres.2010.10.020) an initial timestep of 10^{-6} s that might increase to 0.1 s is used to integrate the diffusional growth equation (see their Appendix). Of course, they integrated the linear growth equation (dr/dt) and not the quadratic (dr^2/dt) as done here. But additional stand-alone integrations of superdroplets with different aerosol masses and a prescribed supersaturation using different timestep lengths are necessary to verify if a 0.1 s sub-timestep is actually sufficient.

P. 8, l. 8: Consider changing “a pair” to “the same pair” for clarity.

P. 8, l. 26: RHS of what?

P. 8, ll. 30 – 31: These sentences contradict each other since the UWLCM contains an LCM and an LES. Therefore, specify “[all] of the model dependent variables” more precisely.

P. 9, ll. 9 – 10, Fig. 2: Figure 2 confuses me. If only the shaded part is used as a coalescence cell, certain volumes filled with superdroplets are neglected in the collection process. However, I do not believe that this is what the authors are doing. Could it be the case that the lowest line of grid point always equals the first, and that the right-most column of grid point equals the left-most? In other words, how do the authors implement so-called ghost layers of grid points to facilitate a cyclic model domain?

P. 9, l. 18 – 19: Important for the formation of drizzle is the is the microphysical model, and usually not the LES dynamical core.

P. 10, ll. 25 – 26: Please comment on these options if they are essential for the conducted simulations. If they are not essential, I would omit this sentence for clarity.

P. 10, ll. 30 – 32: State that turbulence in two dimensions behaves fundamentally different from turbulence in three dimensions.

P. 10, l. 33 – p. 11, l. 1: Small random perturbations are not the reason for the variability, it is a fundamental property of a chaotic system, reacting to small changes in the initial values.

Figs. 3 – 6: For the final version of this manuscript, please make sure that the location of the figures matches the text.

P. 12, ll. 1 – 2: The entrainment is usually not calculated from the increase of the inversion height alone. Commonly, the subsidence velocity at cloud top height is subtracted.

P. 12, ll. 23 – 24: I suggest rewriting this sentence to: “[…] where the autoconversion efficiency increases with N_{SD}.”

P. 12, ll. 26 – 28: Since the characteristics of turbulence in two dimensions are fundamentally different from three dimensions, the better agreement with observations must be seen as purely coincidental.

P. 13, l. 29: Why is the iLES approach responsible for the simulated behavior of the third moment of the vertical velocity?

P. 13, l. 33 – p. 15, l. 2: Spurious cloud edge supersaturations are known to result in the artificial activation of cloud droplets at the top of stratocumulus (e.g., Stevens et al. 1996, doi:10.1175/ 1520-0493(1996)124,1034:TSPOCE.2.0.CO;2). Physical activations are largely impossible there since the top of stratocumulus is not dominated by strong, long-lasting updrafts resulting in physical supersaturations.

P. 17, ll. 1 – 2: Maybe it is worthwhile to add references to the models DHARMA and RAMS.

P. 17, sec. 4.5: How is activation determined? I assume a droplet is considered activated when it exceeds a critical radius. This is a valid assumption if the aerosol is small, and diffusional growth is not kinetically limited. However, for aerosols smaller than 0.1 µm, the typical timescale for activation is usually similar or even smaller than the timestep of the applied model, making the treatment of

activation in UWLCM, DHARMA, and RAMS practically identical. The activation timescale becomes only important if the aerosol is large, typically larger than 0.1 μm in radius, for which the critical radius exceeds a couple of micrometers. However, once located in a saturated environment, these *inactivated* particles exhibit behavior very similar to regularly activated droplets once their wet radius exceeds one micrometer, beyond which curvature and solute effects are usually negligible. Accordingly, the reduced susceptibility of aerosol activation on the cloud-base supersaturation maximum might also be just a result of the applied criterion for activation, which is not appropriate for the entire aerosol spectrum.

P. 17, ll.. 26 – 27: Please clarify: The cloud-base supersaturation maximum still causes activation in UWLCM, but it might not have an as immediate effect as in other cloud models because of the (presumably) applied criterion for activation (see last comment).

P. 19, ll. 10 – 11: I agree, that the number of superdroplets has no impact on domain-averaged quantities. However, it might be worthwhile to refer to the study of Schwenkel et al. (2018, doi:10.5194/gmd-11-3929-2018) in which small-scale effects of the superdroplets concentration are addressed.

Technical Comments

P. 5, l. 4: Change format of citation: “[…] in Gillespie (1972), […]”, not “[…] in (Gillespie, 1972), […]”

P. 10, l. 16: Change format of citation: “[…] in Ackerman et al. (2009), […]”, not “[…] in (Ackerman et al., 2009), […]”