Review of

Subvisible cirrus clouds – a dynamical system approach

by E.J. Spreitzer et al.

Summary:

The authors present a conceptual model to investigate evolution characteristics by the methods of Nonlinear Dynamics. I appreciate the research presented here and I will appreciate more publications on similar topics. Yet, before publication, I suggest some points of straightforward amendment, primarily a stronger recurrent theme, i.e. a nonlinear model from cloud physics investigated by the methods of nonlinear dynamics, and a careful inspection of the model equations and their derivation. The major comments are given below. I have also compiled a list of minor comments, including hints to wording and to errors in the presented eqs. For shortness, I send this list directly to the authors.

Major comments

1. The presented research is devoted to the investigation of a cloud physical problem in the context of the theory of dynamic systems. This should be the red line in the presentation. Some suggestions:
   (a) Introduction: Reduce the discussion of SVCs, and put mor emphasis on the major point of the paper, i.e. a conceptual model using the ideas of Nonlinear Dynamics, which is mentioned only casually in l. 60-64 and in the last but one para of the conclusions. Other papers devoted to Nonlinear Dynamics and Cloud Physics are published by Graham Feingold, see e.g. Nonlin. Processes Geophys., 20, 1011-1021, 2013: 'A model of coupled oscillators applied to the aerosol-cloud-precipitation system' by G. Feingold and I. Koren.
   (b) The central dynamic eqs.(40) can be written in a clearer way by introducing few coefficients, something like $dN_c/dt = aRH_i - bN_c^{1-\delta/\delta} q_c^\delta$ etc. Why is RH_i not changed by nucleation? Why don't you use q_v as prognostic variable instead of RH_i? Then one can see the condition of mass conservation immediately and get rid of a lot of calculations in Ch. 2. Another important advantage is that you can use the set of (N_c, q_c, q_v)-eqs. to characterize immediately the steady state by 2 conditions: (i) Nucleation and sedimentation of N_c balance. (ii) Sedimentation of q_c and increase of q_v due ascent balance. A steady state is reached only for $RH_{i,steady} > 1$ and requires the parameter $w > 0$.
   (c) The basic set of eqs. (40) comprises many terms. To discuss this set of ODEs in the frame of nonlinear dynamics, please discuss which of the terms represent the external sources and sinks, and which represent internal transformations.
   (d) Discuss earlier Section 3.2 Mathematical Analysis as the heart of your method, and please give more explanations on Eq.(42). Once the set of prognostic equations is formulated, discuss the steady states and their (linear) stability (e.g. as in Section 3.2).

2. I have the impression that you use the wording 'critical points' instead of 'steady states'. Please correct and skip the sentence 'since the system...' in l. 371. A limit cycle is also an attractor, namely a periodic one. l. 392: What is a 'positive attractor'? A stable focus?

3. The authors assume dry adiabatic processes (Section 2.5) despite deposition. This requires clarification. Maybe, one can assume moist saturated adiabatic processes and use the value of the lapse rate at the prescribed temperature. For selected T = 233K or less, the lapse rate should be near 0.9 K/100m or even closer to the dry adiabatic one.

4. In Chapter 2, the basic equations are derived from first principles at length, while much has been derived earlier. The calculations can be reproduced at the utmost by lengthy derivations - which the reviewer did not do everywhere.
   Please check whether the central equations and prerequisites of the final Eqs. (40) can be compiled in an appendix, accompanied by a considerable shortening of Chapter 2 and another contribution in favor of the recurrent theme. Also the reader would be happy to find a collection of relationships between the moments of the size distribution function and the used variables like N_c and q_c, as well as a reduction of symbols to an absolute minimum number.

5. Chapter 2 carries inconsistencies in dimension. The chapter is devoted to the description of the spectral budget equation and the budget equation for the moments of the particle size distribution. This is well known from literature, also stated by the authors. However, the presentation does not agree with e.g., Beheng (2010). The authors start with a particle size distribution f in dimension mass^{-1} and a budget equation in terms of ρf with dimension volume^{-1}. How do you interpret fdm and $\rho f dm$? The moments μ_k according to Eq.(4) have units mass^k. The zeroth and the first moments have dimensions of 1 and mass, resp., and cannot be identified as number concentration N_c and mass concentration q_c (l. 114) with units stated in l. 122. The dimension of μ_k is mass^k, not as in l. 102. $f(m)$ in eq.(8) would have the dimension mass^{-2} with N_c from l. 122. The outline of Beheng results in the correct properties.
   Nucleation terms in (40a) and (17a) disagree, why?
   l. 247: $\rho d\Phi/dt$.
   Please clarify.

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