2nd review of
Subvisible cirrus clouds – a dynamical system approach; revised version
by E.J. Spreitzer et al.

Summary:
As stated in my first review, I appreciate the research presented in the paper. The revised version is definitely improved. In particular it benefits from, e.g., the shift of microphysical details and derivations to the appendices. And I want to higlight the presentation of the parameterization idea in the Conclusions.
Before publication, I suggest a straightforward amendment, including
to present the paper concisely ('Conclusions' are ok), to call the same thing everywhere by the same technical term, to arrange clearly the equs., to inspect the equs carefully, to avoid repetitions, and to skip material that is really basic knowledge or not absolutely necessary.
A list of specific examples (not complete) is given below.
After the 1st revision, this should not pose a severe problem anymore.

Specific comments

1. The terms equilibrium state, critical state, point attractor, singular point are used alternately, e.g. l. 81-85, l.410-412, Reply to Rev.2 p.2, and others. Please be more precise, although you find the mismatch in some textbooks. In your model, the thermodynamic equilibrium requires ice saturation, but this needs $w = 0$. For $w > 0$, you examine an open system and you look for steady states (which you call 'critical points', although this expression can be misleading). The term 'attractor' implies that the longterm behaviour of the system will end in a part of the phase space (e.g., point a., periodic a.); the opposite is a repellor. I have the impression that you use 'positive a.' for a stable node or focus.
   Please screen the manuscript for consistency and keep track with the wording.

2. For the final version, please focus the descriptions in Section 2 - in combination with App. A, B. Few examples: App. A does not contain anything else but given in Section 2 together with the very basic principle of total mass conservation. Shorten l.218-224 to one line. Shorten l. 241-246 to something like 'w, T, and p are assumed constant and treated as control parameters.' Para l.252-255 is mostly a repetition, same for l.270-272. l.247-249 Sentence 'If ...' is redundant.
   The transformation rates can be written in the same formal structure to simplify the reading and retracing. E.g., the growth term (13), (25b), (B9), and coefficient (B13d) should have the same structure and variables; do not switch between RH_i, S_i, p_v, and q_v as well as p_{si} and q_{si} etc. if not absolutely necessary.
   Please go through all equations.

3. l. 68/69. You 'exclude the possibility of liquid origin ice clouds'. Isn't this a contradiction to your response to reviewer 2, 2nd comment, that nucleation of ice particles is from the LIQUID phase? Please clarify.

4. l. 95. 'system variable' instead 'control v.'.

5. l.139. Skip 'dry'.

6. l. 161p. You write that f_a were normalized, but this neither coincides with the unit of f_a nor with e.g., Eq.(8). Please clarify.

7. Eq.(B13a) does not match with (9) and (10a). Please check.

8. l. 275, Eq.(21): The factor $\partial RH_i/\partial q_v$ is missing on the RHS.
   Please check in (B13e) whether a factor ε is missing in the first bracketed term.

9. l. 279pp and your reply to Rev.2. Mathematically, both variables RH and q_v can be used as prognostic variable, since both carry the information on the amount of water vapour. We should stop the discussion, although I do not follow your arguments.
   Pure ice clouds can indeed exist close to thermodynamic equilibrium (ice saturation), the single steady state in case of $w \to 0$.

10. Eq.(25b). The sedimentation term should be proportional to $N_c^{-\delta} q_c^{\delta+1}$ (instead of $N_c^{-\delta} q_c^{\delta-1}$). Please check.

11. p.25 Eq. (B13b,c): The variable c occurs in 2 different meanings, c and $c(T,p)$. Please distinguish. Eq.(16), (34), (B13). Please unify corr(T,p) and c(T,p).

12. After Eq.(25). The new presentation is definitely improved. Unfortunately, only much later (l.373pp) you interpret the terms as internal transformation rates and external sources/sinks.
    Please work out sedimentation as external sink, the w-term as external source, and the nucleation terms also as external sources (since ice particles form from an inexhaustable external reservoir of liquid droplets). Only the 'diffusional growth' is an internal transformation. This can be seen seen from Eq.(25) (and more easily if q_v is prognostic variable), hence please consider to place the discussion there or in the initial part of section 3.3.
    l. 332-334. I do not see this. Without sedimentation, only external SOURCES exist, i.e. nucleation and the w effect. For $RH_i = 100\%$, $dRH_i/dt > 0$, that is no steady state.

13. l. 376 'artificially produced vapour'. Please reformulate, since vapour enters your system as external source.

14. l. 380-407, 'dissipative system'. The previous text explains the externally forced system, not ('Thus'?) its dissipative character. The discussion of $\nabla \cdot \mathbf{F}$ (27) is on the contribution of each term, not on 'dissipative system'. What is the aim? A dissipative system is characterized by the negative value of the temporal average of $\nabla \cdot \mathbf{F}$ in its longterm behaviour, not necessarily at each instance. I wonder whether you need this passage.

15. l.438-444. This is an important point. If only a single steady state exists and if this is stable, ALL trajectories should end up there. Focus on this aspect and skip talking about trajectories starting in the neighborhood for conciseness.

16. l.461-471 Poincare-map. Although this is a nice calculation, I do not see any added value, since you have found and discussed the limit cycle before (figs. 2, 6). If it is for the purpose to calculate the period (l.534p), please tell so and skip the rest.

17. l.588 'analytical model', fig. 13,14 'simple model' and perhaps other locations. You talk about the model presented in this paper. It is not an anaytical model, since you solve most of the equs. numerically. And it is by no means a 'simple' model. Please choose a proper name and use this name throughout the paper.

18. l. 648. Which microphysical properties? Maybe you address number and mass concentrations, but these are no 'microphysical' properties.

19. l. 650. 'in the point attractor case'.

20. Figs. 13, 14 are a repetition of Figs. 1, 2, supplemented by other model results. Figs. 13, 14 can replace Figs. 1,2 for shortness.