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Interactive comment on "On the CCN [de]activation nonlinearities" *by* S. Arabas and S. Shima

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The authors discuss cloud droplet activation and deactivation in terms of saddle-node bifurcation. By doing so they can employ a nonlinear dynamic approach to study the properties of such processes. Specifically, they approximate the drop growth by diffusion equation to a normal form of a saddle-node bifurcation (df/dt = a + fËȨ2) and therefore they could use the properties of such form to study hysteresis and catastrophe behavior. The approach and the mathematical insights are very interesting but in order to make this paper accessible to readers from cloud physics, the authors should be much more generous in the details they provide in the mathematical derivations, and invest efforts in translating the mathematical insights to physical ones.

We thank the reviewer for the evaluation of the paper and the provided comments which we address in detail below. We enclose a revised version of the paper.

> A reader that is not fully updated with the jargon of nonlinear dynamics will find this paper hard to follow. The explanations in some sections are fully based on such jargon (see part 4 - "... the coalescence of the fixed points is associated with a passage throu

We have rewritten and significantly extended the section on saddle-node bifurcation in which the jargon is first used. The rewritten section contains explanations of the basic nomenclature.

We have not tried to refrain from using the jargon, though. The journal choice was motivated by the aim of addressing the nonlinear dynamics community as well.

> On the same note, throughout the paper the explanations are very slim. It starts from the overview in the introduction in which the Kohler theory is hardly mentioned (although it is central in the paper).

A paragraph introducing and pointing out the role of the Köhler theory in the presented mathematical model was added. While we fully agree that it is central in context of atmospheric CCN, the presented analysis – in principle – applies to activation phenomena in a wider context. This is now also highlighted in the introducing as well as in the paragraph where the Köhler theory is first mentioned.

> Moreover, many of the numbers provided there are not so accurate. One can have coarse mode aerosols larger than a micron. Concentration can vary between 10’s to 10000’s etc.

We have extended the given ranges following the suggestion of the reviewer, at the same time changing the numbers into more approximate textual representation (“from tens to thousands”, “from fractions of to multiple micrometres”) – in an attempt to underline the roughness of the estimation.

> In the next chapters where they develop the mathematical framework, they should add guidelines and physical insights in each of the main steps. What does it mean Saddle-node bifurcation at Köhler curve maximum? They should explain in not from a mathematical point of view (“ ... when the fixed points coalesce into a half-stable fixed point ... ”) but from a physical point of view. What is the meaning of this point. What can we learn about it from the Köhler theory? This is true to all mathematical steps in the paper.

We have added several sentences addressing this point. In particular, it is now clearly stated how stability of the fixed points relates to CCN growth and activation.

We have added a new figure (Fig. 1 in the revised manuscript) aimed at depicting the consecutive steps taken in the fixed point analysis as well as depicting how the phase portrait of the system can be recognised in the flipped Köhler curve.

> While the mathematical derivations look right (as much as I checked) the math derivations details are slim and there is hardly no physical interpretation to the shown insights (which could make this paper much more relevant).

While we hope that the introduced changes improved the text, let us also point out that throughout the paper we have in fact openly acknowledged that the discussed mathematical nuances of the studied system likely have limited relevance to cloud phenomena, most notably due to the monodisperse assumption (“limitations certainly restrain the relevance of the presented calculations to real-world problems", "[close-to-equilibrium hysteresis] of no foreseeable relevance to the macroscopic behaviour of the large-scale cloud systems"). Yet, as underlined in the abstract and conclusions, the employed approximations and the depicted hysteretic behaviour are of relevance in construction of numerical schemes for solving drop growth equations.

Nevertheless, we consider the derived analytical estimate of the activation timescale readily applicable in studies dealing with cloud microphysics. This has been clearly pointed out in the abstract and conclusions.

> The above comments are applicable to all sections in the paper – readers that are not fully updated in the nonlinear dynamic math jargon will not be able to follow parts of this paper.

In a new last paragraph of the introduction, we have acknowledged appearance of the jargon throughout the text, and referred the reader to selected introductory chapters in the book of Strogatz for reference. Nevertheless, we do hope the rewritten section on fixed point analysis, supplemented with the new figure, makes the paper much more approachable.

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Discussion paper