This paper presents forward and backward methods for tracking particles in SCALE-SDM, which employs the superdroplet method for cloud microphysics. This capability has strong potential to help address several fundamental open questions in cloud microphysics, including those highlighted by the authors, because particle-resolved tracking is one of the most promising ways to investigate such problems in a model that explicitly follows individual cloud droplets and raindrops. The manuscript presents the methods and algorithms in a generally coherent manner, together with a set of small test simulations.

Overall, I believe this work makes a useful effort to document tracking algorithms that will be of interest to the SDM community. However, I have several major concerns regarding the way the manuscript frames its scientific novelty. In particular, some of the scientific implications emphasized by the authors are already broadly known in the literature, but these prior studies are not adequately acknowledged. In addition, the currently proposed backward algorithm does not appear to be practically applicable to the most important simulation setting, namely, realistic 3D simulations, which are required to address many of the scientific questions the authors themselves propose. This limitation makes it difficult for me to view the present framework as a complete and practically ready method for broad community use. At this stage, it reads more as a promising conceptual capability that still requires further methodological development before it can be deployed for the more ambitious applications suggested by the authors.

If this latter interpretation is closer to the authors' intent, then I encourage them to state this purpose more clearly and to frame the paper accordingly, as an important step toward future development rather than as a method that is already practically ready for the full range of applications discussed. I therefore recommend that the authors address the concerns raised below and clarify the intended scope of the paper.

Specific comments

Comment 1:

For the purpose of documenting particle-tracking algorithms within an SDM-based model, I am generally satisfied with the presentation of the forward algorithm. However, I do not see strong algorithmic novelty in this part, as similar forward-tagging approaches have already been used more widely in the superdroplet community than the papers that the author cited, and are relatively straightforward to implement in existing models that employ the superdroplet method. In my view, the manuscript should therefore place greater emphasis on the backward algorithm, which appears to be the more distinctive methodological contribution.

Moreover, if the data size of forward sampling is about 76 MB for 400K particles, wouldn't it be more straightforward to simply employ more particles to track using the forward method?

Comment 2:

A fair comparison of computational cost for the two algorithms should be conducted under appropriately similar conditions (Sec.3.2.3). At present, the comparison is not fully convincing because the forward and backward algorithms are demonstrated under substantially different configurations, while the manuscript is framed as an algorithmic contribution.

More importantly, the backward algorithm, which seems to be the more novel component of the paper, is not currently demonstrated to be practical for 3D simulations because of the storage bottleneck associated with storing lineage pointers for all superdroplets. This is a substantial limitation. The scientific applications highlighted by the authors, including lucky-droplet growth, eddy hopping, and inhomogeneous mixing, fundamentally require realistic 3D simulations. In this sense, the current inability to apply the backward method efficiently in 3D limits the practical impact and generality of the proposed framework.

While the authors briefly note this in Sec. 4.2, and I appreciate the authors' effort to construct a method with efficient lookup and strong post-processing capability, I believe the manuscript would be significantly strengthened by a more explicit discussion of this 3D limitation and by a more controlled performance comparison under similar simulation settings. In a methodological paper, practical scalability is not a secondary detail but part of the central novelty claim.

Comment 3:

Furthermore, if the suggested future work for backward tracing is based on sampling, I would suggest exploring event-based particle backward tracking, as employed by Lim and Hoffmann (2023) and Lim and Hoffmann (2024). They tracked backward when the event of interest happened (entrainment and mixing in their paper). The authors could apply a similar approach for lucky droplets, triggering backward tracking when either a collision or mixing occurs. This might be a more appropriate and efficient solution for future work.

Comment 4:

When discussing the scientific implications, apart from algorithmic novelty, the authors do not sufficiently cite existing literature on lucky droplet and raindrop growth (e.g., Hoffmann et al., 2017; Dziekan & Pawlowska, 2017; Li et al., 2022), which has already been reported in the community using the superdroplet method. The 15-20 μm threshold and the lucky-droplet pathway are interesting, but they appear more as a demonstration of the framework's capability rather than broadly general new physics. Therefore, the authors need to cite foundational works done with superdroplets and the canonical papers from as early as the 1950s that established this theory, such as Telford (1955).

Comment 5:

In Sect. 4.1, the authors state that the algorithm provides "novel insights" into the microphysical pathway. However, the backward case is not used to analyze actual collision or coalescence events.

Rather, it is used to demonstrate condensational growth pathways that lead droplets into the size range favorable for coalescence. This distinction should be made more explicit. In its current form, the backward analysis supports pre-collisional growth diagnostics rather than reconstruction of actual collision histories.

More importantly, I do not find the scientific framing sufficiently justified. The results presented in this section appear to illustrate processes that are already broadly known, rather than providing genuinely new physical insight. For this reason, I encourage the authors to reconsider the use of phrases such as "novel insights" and to moderate the framing of this section accordingly.

Minor comment 1:

The manuscript would benefit from a clearer separation between the "framework description" and the "case-study science results." I am not certain if these test cases actually add new scientific knowledge beyond what is already known. While the framework itself shows potential, the results the authors presented here do not clearly demonstrate it.

Minor comment 2:

Several wording issues and typos currently hinder the readability of the paper. I encourage the authors to carefully proofread the manuscript and revise it accordingly.

Minor comment 3:

In Table 1, MP setup indicates cnd + coal for BKW_{cond}. Please fix this if this is an error.

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