This study presents a synthetic data set for testing and developing algorithms and data processing change. The challenge rises from the needs of an atmospheric model consistent from the perspective of different instruments (i.e., lidar, radar, multi-spectral imager and three-view broad-band radiometer. From my point of view, this study achieved the quality stablished by the journal but only one of the two goals (presentation and discussion). My major concern is that the discussion suffers from weak qualitative comments. Therefore, I would recommend a major revision before accepting its publication. I would suggest authors to consider the following comments and correct the found typos.

Major comment:

Section 3 is addressed for presentation and discussion of the simulated L1 data for EarthCare test scenes. However, the discussion part suffers from weak qualitative comments. I would expect a deep quantitative comparison in this section. See below some examples:

- P24L436-438: *'It can be seen that, as expected, the lidar penetration into the clouds is limited, especially in the central part of the frame.' […] 'The aerosol fields in the southern segment of the frame are also well captured.'*
    - For sure this is expected, why does one make this huge effort (make this great simulator) to conclude what is already known? The reason is that with the simulator, one can provide numbers!, quantitative estimations. In this sense, I would suggest providing the lidar penetration in clouds in meters and compare this with other lidars.
    - It is good to know that the aerosol fields are well captured but, again, this was expected. The key question is how well captured they are? Which is the SNR? Could you provide any comparison to quantitatively assess the detection?
- P25L450-457: *'The strong 94-GHz attenuation by hydrometeors results to missed detections near the surface. This can be clearly seen by the depression of the surface echo radar reflectivity at 3700 km and the complete loss of the surface echo around 4100 km'. […] 'Despite their noisiness, the CPR Doppler velocities reproduce the main features of the GEM model Doppler velocities, namely, the transition from solid to liquid hydrometeors and the low sedimentation Doppler velocities in the upper cloud levels.'*
    - This is obvious. The real contribution would be to make a direct comparison between the surface echo radar reflectivity and the Integrated Liquid Water Content? This would provide a kind of threshold from which the reflectivity near the surface is not valid/trusted.
    - Regarding the CPR Doppler velocities comment, I expected a quantitative assessment of the conditions that will make CPR Doppler velocities valid. For example, under which conditions CPR Doppler velocities can be distinguish from noise? Could we define a reflectivity threshold to flag it? Even if not possible, why would it be not possible is of interest. Again, do we need a complex simulator to state just that '*CPR Doppler velocities reproduce the main features*'?

The following discussions in the manuscript have the same issue but I consider two examples are enough. I think that the length of the manuscript was the main reason for keeping the discussion short, but it weakens the way the ECSIM's potential is shown.

Minor comments:

- P3L57: consider explaining the acronym ECCC

- Section 2.1 focuses on 'scene constituents'. However, 2.1.2 is named VIS-UV-IR which is not a constituent but is about hydrometeors-radiation interaction in the VIS-UV-IR spectra range. Then 2.13 is named radar although section 3 is already named radar simulations, so I would say this section about hydrometeors-radiation interaction in the radio spectra range. I strongly suggest authors to check section organization. Also, I would suggest to extent titles: 2.1.7 SW -> 2.1.7 short wavelength radiative transfer | 3.1 Halifax -> 3.1 Halifax scene. Finally, I wonder if the section 2 and its subsections could be linked to the scheme shown in Figure 1.
- Table 1: Add "in GEM" to the figure label: "Cloud and precipitation microphysical parameters in GEM".
- P4L79-85: Which is the consequence of the lack of a backscatter peak of the aggregated solid-columns phase functions? In the same paragraph is stated that accounting for this peak produces more realistic values of lidar ratio and improves the agreement between lidar multiple-scattering coefficients derived using Calipso observations and theory. If this present a limitation of the simulator and there are other limitations, it would be needed to summarize them in a table.
- P13L243: "polarization elements" act perfectly in polarization state. I would say this a strong limitation of the simulator. Which are the consequences of this assumption. This could be added to the limitation table. Polarizing effects on the lidar depolarization products have been deeply studied and its influence is not negligible. This should be assessed.
- P13L253 Are the stars '*' needed in the equation?
- P14L273: Could the authors either shed light on the way dark current noise and ACCD readout noise are simulated or provide references to.
- P34L481: The assessment of the Doppler velocity is qualitative but not quantitative.

Typos:

- P4L70: extinction absorption -> extinction, absorption
- P4L78: observation(-> observation (
- P5L102: ad-Hoc -> ad hoc.
- P10L189: Tis -> T is
- P10L204: symbol μ is not in the equation
- P16L308:]is ->] is
- P23L424: Level-11 -> Level-L1 ?
- P24L444: in 13 -> in Figure 13
- P24L447: (12 -> (12
- P26L461 and P26L467: Figure 16 is mentioned before Fig. 15. Consider exchange order of figures.
- Figure 13: Consider exchange order of axis. It would be easier to compare the model and observed reflectivity is the axis are more near. The same for the Doppler velocity.
- Figure18: scene -> scene.
- P34L482: in 22 -> in Figure 22.
- P34L484: 23. -> 23).
- Figure 21: echo -> echo.