The manuscript presents really interesting simulation results and clearly demonstrates the efficiency of constructed EISCAT_3D facility in fine-structure detection and resolution using in-beam imaging. The authors performed a thorough analysis of simulated incoherent scatter spectra employing the model of ionospheric parameters and synthesized noise to retrieve the ionospheric signature of Kelvin-Helmholtz instability. The authors used well-proven theoretical methods for data analysis and provided in-depth description and discussion. As a whole, the manuscript contains many important outputs that are useful for many scientists and other stakeholders. The results obtained can be developed further and will be useful for testing future experimental results and arranging new promising experiments. I hope, the EISCAT_3D facility will be put into operation very soon and provide new insights into the high-latitude ionospheric plasma.

The manuscript is worth to be published after some revisions. My specific comments are below.

1. As for me, the title of the manuscript reflects the main idea slightly incorrectly. The reader can wrongly assume that the authors dealt with real experimental data. I suggest adding something like “synthetized EISCAT_3D data”, “simulated imaging”, “Theoretisation and simulation” or something else – it is up to the authors.

2. Line 169: “atomic oxygen is the dominant ion…”. This is unclear, if the authors consider pure atomic oxygen ions (oxygen approximation)? If not, what other heavy or light ion fractions they analyzed?

3. Line 317: “…data for the 25 sampled frequencies (± 12 kHz at 1 kHz resolution)…”. As for me, the used spectrum is sampled really roughly. I might be wrong but can suppose that it can result in the distortions both the incoherent scatter signal and the autocorrelation function. I had some experience in incoherent scatter signal synthesis (but for multiple ion species), where the sampling of the order of 10 Hz should be applied. I realize that the laptop productivity is limited. It would be good to refer some publications or justify adequacy of the used resolution.

4. If I am right, the authors also simulated the noise to add it to the incoherent scatter signal. Often, the real noise characteristics are far from the synthesized ones. It would be better, if the authors used the characteristics of really measured noise in the same location (of course, if possible).

5. I have noticed that the manuscript suffers from the lack of quantitative estimations, while the authors prefer the qualitative assessments. For instance, “Selecting the optimal regularization based on the SNR is therefore an essential factor…” (lines 334 – 335), “…incoherent scatter spectra are also resolvable depending on the regularization used and SNR of the measurements” (page 17, bottom), “…assuming there is sufficient signal to noise standard deviation ratio (SNR)” (line 366). Fortunately, Figures 8 – 10 can shed some light but the reader need to guess what values the authors meant as “optimal” or “sufficient”. Taking into account, that the reported method is supposed to be

used further, it would be useful to emphasize on the specific values, where this method is appropriate. I suggest adding the table with SNR and regularization coefficients (maybe, something else) to quantitatively specify the results.

6. Line 371: “Consistent sub-km measurements…”. I think that this paragraph should be omitted or moved to Introduction. This shifts the focus from the results to the advantages of the EISCAT_3D facility on the whole.