Reviewer:

The quality of the EISCAT electron density was enhanced through adjustments in the integration technique. This led to decreased noise and enhanced reliability of the calculated collision frequencies. The authors' acceptance of suggestion in my previous comment is appreciated. However, the distinction between the observational findings and inferences remains ambiguous, despite this issue being raised in the previous comment. Consequently, certain sections of the study present inferred information as if it were observationally confirmed. Additionally, some conclusions are drawn based solely on assumptions or hypotheses applied in this study, disregarding other possibilities. Many of these issues were highlighted in the previous comments. It is crucial to carefully revisit comment 3 (Feature of the vertical motion in the lower thermosphere) and comments in the second circulation (starting with "Additional issues ..."). Despite the authors' agreement in their response, there are concerns that the latest version may not have been adequately revised. Therefore, the main points are repeated below as a supplement to the comment 3.

Authors:

We appreciate that the reviewer accepts the presented mitigation strategy to reduce the impact of 'noisy' EISCAT data. We have revised the wording to emphasis the underlying assumptions. The main narrative of the manuscript is the analysis of EISCAT observations during particle precipitation and to outline potential differences and impacts compared to other studies that are already published targeting other geophysical situations. The main idea is to show the differences of the observations and applied analysis depending on geophysically different processes, which provides a pathway to more observational studies with a suit of instruments providing a more detailed and comprehensive data set or modeling studies.

Reviewer:

[Observational evidence and estimation]
This research found that particle precipitation correlated with an increase in the collision frequency. Given that collision frequency is directly related to neutral density, this finding implies a rise in density. However, it is important to note that the study does not provide concrete evidence for the underlying physical mechanism. This aspect remains speculative. The text should be revised to clearly distinguish between observed facts and hypotheses. While increased atmospheric density due to upwelling is one potential explanation for the higher neutral density, it is not the only possibility. Comment 3 introduces an isobar-related mechanism. Advection could also play a role. It is crucial to recognize that this study lacks direct evidence of atmospheric dynamics such as upwelling or uplift. Neutral density could increase through various means, including advection effects. Please consider multiple angles and have a fair scientific discussion.

Authors:

We added Section 5.5 to discuss how winds along tilted isobars can cause local changes of thermospheric density. Our discussion is based on Oyama et al., 2008 “Generation of the lower-thermospheric vertical wind estimated with the EISCAT KST radar at high latitudes during periods of moderate geomagnetic disturbance”.

Reviewer:

[Conclusion under an assumption/hypothesis and its inconsistency] Generally, conclusions derived from arguments or calculations based on an assumption or hypothesis are only valid when the assumption/hypothesis itself is correct. It is crucial to assess the validity or explore alternative possibilities. Please consider this principle when revising the text. Some examples are:

- The GUISDAP analysis does not fit the ion temperature at altitudes below 100 km, leading to the assumption that particle or Joule heating does not cause a temperature enhancement. Clarify how this supposition aligns with the discussion that presumes heating occurs.

Authors:

The statement that Joule heating does not have a major impact below 100 km altitude is not based on EISCAT ion temperature measurements. The previous studies by Baloukidis et al., 2023 and Günzkofer et al., 2024 applied EISCAT ion velocity measurements to calculate electric fields and consequently Joule heating under the assumption of model conductivity profiles. The assessment (hypothesis) that at and below 100 km altitude, Joule heating rates are lower than particle precipitation heating rates is based on the estimated heating rate in Section 5.3 and the profiles shown in the mentioned references. The Pedersen conductivity reduces rapidly below the altitude of its maximum (normally around 120 km) and therefore so does the Joule heating rate. The GUISDAP ion temperature analysis and its assumptions do therefore not affect this hypothesis. We tried to clarify this in the manuscript.

Reviewer:

- Equation 3 represents the scenario where all thermal energy contributes to vertical expansion, resulting in the maximum vertical wind. The upwelling duration is determined using this calculation and assuming a distance of 6-19 km. It is important to assess the validity of these assumptions. The calculated duration should be consistent with the particle precipitation duration if the assumption is acceptable. In general, if the errors are so large that the validity of the results cannot be determined, then the analysis and discussion should be inconclusive. The text should be revised with this consideration in mind.

Authors:

We added the assessment of typical precipitation event duration from Grandin et al., 2024 which is slightly lower than the minimum estimated reaction time. We stated explicitly that the large uncertainties make it difficult to pin particle precipitation as the origin of the observed collision frequency changes despite the observed profile changes with estimated particle precipitation strength. We also added, that even if precipitation is the main driver, other processes might contribute as well. Lastly, we added a statement that the vertical wind in that Section is only an estimate and not an observation.

Reviewer:

- The discussion in Section 5.6 regarding Joule heating effects falls short of being satisfactory. The authors acknowledge that the Kp index is not an accurate indicator of localized geomagnetic activity. Furthermore, its 3-hour temporal resolution is inadequate for capturing precise temporal fluctuations. The text merely suggests that Joule heating might or might not influence the observed increase in neutral density, which is essentially a self-evident statement that offers no new insights. In essence, it remains unclear whether particle heating alone can account for the rise in neutral density. The study should conclude that the exact cause of the density increase remains undetermined.

Authors:

Calculation of Joule heating rates as done in Baloukidis et al., 2023 and Günzkofer et al., 2024 requires a different EISCAT measurement mode than applied in this study, therefore we cannot do

more than compare to these recent assessments of Joule heating in the EISCAT region. We point out in the manuscript that under regular conditions, Joule heating should be considerably lower than particle precipitation heating at and below 100 km altitude. However, we acknowledge that these studies based on Kp index might not be best suited to approximate the conditions found during the measurements presented in this paper. Hence, our conclusion is that according to common literature, Joule heating should not influence the observed changes of ion-neutral collision frequencies but we cannot conclusively determine that this holds for the present conditions. We elaborated this further in Section 5.6 and added a respective statement to the conclusion section.