Review of "Data reduction of incoherent scatter plasma line parameters" by Gupta and Guio

Summary: This paper presents new methods for detecting and analyzing plasma lines with the EISCAT radar. One method involves supervision and hand tuning of parameters but accurately extracts any plasma lines present. The second method is fully automated and can be applied unsupervised to a large data set, but suffers from failed detections. The authors apply these new algorithms to 5 days of plasma lines observed by the EISCAT UHF radar. The extracted plasma line frequency and intensity are then analyzed, showing the dependence of the radar pointing direction and ionospheric density.

This is a well written paper that showcases a new method for plasma line extraction and further shows how plasma lines are influenced by radar and ionospheric parameters. I recommend this paper for publication with some revisions.

Review Comments:

1. In Section 3.3, it is mentioned that the plasma lines are fit to Gaussian functions. Can the authors comment on why a Gaussian is chosen instead of a Lorentzian? From *Perkins and Salpeter* (1965), the frequency spectrum of a plasma line is expected to be a Lorentzian shape.
2. Figure 4 shows the fitted plasma lines, but this is hard to see as the red curves are thick and cover most of the data. This figure would be more clear if modified so that both curves are visible. Suggestions are to plot the red curve underneath the black curve, reduce the thickness of the red curve, or make the red curve a dashed line.
3. In Section 3.4, the phase energy is given by equation 16. First, it is not necessary to approximate the Bragg wavelength as $2f_{radar} + f_{offset} \rightarrow 2f_{radar}$. Second, as shown in *Longley et al.* 2021, the phase energy has a strong dependence on aspect angle. This dependence is given in equation 2 of *Longley et al.* 2021 (ignoring the gyro terms):
$$E_\phi = \frac{1}{2} m_e \left(\frac{\omega}{k cos^2\theta}\right)^2$$
It is recommended the authors correct equation 16 for both of these changes, while also commenting on the aspect angle changing what electron energies are resonant with the plasma line. Note that this effect is small for the experiments in this paper, but may change how Figures 10 and 11 look.
4. In line 235, it is mentioned that plasma lines are only detected in the 1.7-3.4 eV range. Can the authors comment of where the ionospheric densities were high enough to observe plasma lines at the upper limit of the filter band (7.65 MHz)? Also, it should be noted that the plasma lines with phase energies below 1.7 eV correspond to a lower density plasma, and therefore lower SNR.
5. The interpretation of plasma line enhancement given in lines 286-293 is not consistent with the derivations of *Longley et al.* 2021. There is a distinction between the total damping of Langmuir waves and the individual contributions: collisional

damping, thermal Landau damping, and photoelectron Landau damping. Plasma lines are enhanced above thermal levels because the photoelectron Landau damping term flips signs at photoelectron peaks and generates waves through inverse Landau damping. It is suggested that the authors revise this paragraph.

6. A similar issue of interpretation comes in the paragraph around line 295. Yngvesson and Perkins 1968 assumed suprathermal electrons would be unmagnetized, and therefore their claims about changes with aspect angle are not valid. This was corrected with a fully magnetized derivation in *Longley et al.* 2021, which has some plots showing that plasma line intensity is roughly constant from 0 to 10 degrees aspect angle, then increases from 10 to 20 degrees. However, the full understanding of the presented EISCAT UHF observations is difficult as a good photoelectron model is needed for plasma line power calculations. The authors should revise the discussion in this paragraph to avoid repeating the incorrect conclusions of Yngvesson and Perkins.
7. Lines 306-307 state "we aim to further develop an aspect angle dependent plasma line intensity model and apply it to the current dataset." Can the authors clarify how such a model would be different from either *Enger* 2020 (https://hdl.handle.net/10037/19542) or *Longley et al.* 2021?