Response to anonymous referee #1

We thank the referee for the careful review and useful comments, which have been helpful to us when improving our manuscript. The reviewer comment is indicated in bold and prefixed with a “>” symbol. Our answers are interleaved with the referee comments below.

>The paper has some interesting ideas and potential for the incoherent scatter radar (ISR) community. This paper is jumping to apply MIMO radar techniques to ISR and makes a good case that this can be applied to the E-region of the ionosphere. It argues that Eiscatt 3-D will be able to do some interesting high resolution imaging of the E-region ionosphere. The authors simulate the MIMO radar systems and then find that the SVD seemed to give the best results.

>The application of generalized inverse techniques to geospace sensors is something I generally support. I think this paper does a good job at taking specific problem in ISR and tries to apply general inverse theory and gets some promising results. Overall the authors did a good job of arguing that this sort of techniques will be useful for ISR. I think they need add some caveats to clarify the applicability of the technique.

>First off the the technique the authors show does not include fitting for ISR spectra/ACFs to get plasma parameters. This is fine if they specify that this for the E-region only and that is not clear from the title or the abstract. Yes, different techniques are required for different regions of the ionosphere with ISR but it would help the reader understand the current limits of the method you are applying!
You are correct in saying that a measurement of the full incoherent scatter radar spectrum is needed in order to fit for the plasma-parameters. However, this paper focuses on characterizing the imaging capabilities of EISCAT 3D, given the current interferometer design. In order to avoid making this study overly complicated. we felt the need to make a simplification of what parameter is to be estimated.

One of the primary purposes of the EISCAT 3D radar imaging will presumably be studying the structure of the ionosphere during auroral precipitation. One of the key parameters in this case is electron density. This needs to be measured with as high temporal resolution as possible. It is to first order proportional to returned power $P = n_e (1+T_e/T_i)^{-1}$. In the E-region the T_e/T_i ratio is somewhat constant, so we can to first order assume that a radar image of the returned power is related to the spatial distribution of electron density.

Fitting of the ISR spectrum or ACFs is not the main topic of the article. The ACF is indirectly mentioned when calculating the integration time where all time lags are used for analyzing the backscattered power. Implicitly the assumption of E region is repeated here. Clarifying the restriction to the E region we also regard as important. In the manuscript, we changed the beginning of the sentence on p. 5, l. 22-24:
“If we additionally can assume that the autocorrelation function is constant, the number of lagged product measurements per transmit pulse is $N_P (N_P - 1)/2$ because we also can use measurements with different time lags.”
to “In the E region, we can assume that the autocorrelation function is constant. Then the number of (...)”

>I want to also point out there has been work on imaging ISR in the past such as seen in [1], but this was a study that only applied interpolation techniques to ISR data. I think comparing this 2010 paper with the methods here would be helpful and show that this work is new.
Moving the radar beam to get a wider spatial coverage is somewhat different from the imaging this article discusses. However, this also is a method to see the horizontal distribution of ionospheric parameters, only in an other order of magnitude. For improving the survey over the literature, we move the last part of the paragraph on p.4, l.20-25 to p. 2, where literature on imaging is mentioned.

(The paragraph starts with "The application of aperture synthesis imaging for radar, i.e., aperture synthesis radar imaging (ASRI), has been used in space physics for observing high signal to noise ratio targets (Hysell et al., 2009; Chau et al., 2019)."
Here we will insert
"There is a good amount of literature on ASRI techniques in two dimensions (range and one transverse beam axis direction) for imaging field aligned irregularities (e.g., Hysell and Chau, 2012, and references therein). There have also been several researches on imaging of atmospheric and ionospheric features in three dimensions, eg. Urco et al. (2019), who applied it to observations of PMSE with the Middle atmosphere ALOMAR radar system (MAARSY), Palmer et al. (1998) on data from the middle and upper atmosphere radar in Japan, Yu et al. (2000) with a simulation study, and Chau and Woodman (2001) on the atmosphere over Jicamarca."
After this comes "The currently available horizontal resolution [insert "of ASRI"] is around 0.5° with Jicamarca, but down to 0.1° for strong backscatter (Hysell and Chau, 2012) in the case of field-aligned ionospheric irregularities; and 0.6° with MAARSY for polar mesospheric summer echoes (PMSE) (Urco et al., 2019)."

The next sentences should be a new paragraph:
"However directly on incoherent scatter in three dimensions there is little literature, but some approaches have been made, like Schlatter et al. (2015), who used the EISCAT Aperture Synthesis Imaging array and the EISCAT Svalbard radar to image the horizontal structure of Naturally Enhanced Ion Acoustic Lines (NEIALs) and Semeter et al (2008) who interpolated sparse multio-beam PFISR-measurements to estimate the E-region electron density variation over a 65x60 km area during an auroral event.")

>The following is a list a technical corrections I've found will need to fixed:

>- Page 6 line 25: Do mean truncated cone? Conic section implies a 2-D plane and this is a 3-D volume.
Yes, we mean a truncated cone. The other referee recommended to switch to a spherical cone as this figure represents the radar volume better and this will be included in the revised manuscript.

>- Page 7 line 1: You state range, which range is this because we're not in a multistatic set up and the term range is ambiguous, transmit, receive, bistatic?
The imaging interferometer antennas for EISCAT 3D can be considered as approximately a monostatic system, because the largest separation between antennas is less than 2 km. In this case, the transmitter-target-receiver range is nearly constant for all antennas.

We have added the following statement in the paper to hopefully make this issue more clear to the reader: "by range, we mean the range from the center of the core array in Skibotn to the target." after mentioning the range for the first time on p. 7, l. 2.

>- Page 7 lines 9-10: O2 + is used for the calculation but the dominate species in the E-region is NO+. I don't think this will create a huge error in the calculations but it should be mentioned that this will not throw off the calculation by much.
You are right. Fortunately, the molecular masses are very similar. According to Brekke (2013), p. 222, the dominating ion species in the E region are NO^+ and O_2^+ where there is slightly more of O_2^+ around 120 km, else NO^+ dominates, but the difference is small. Since the species are approximately equally common, we can change the ion mass to 31 u corresponding to a mixture of NO^+ and O_2^+.

In the article, the ion mass is only used for calculating the thermal velocity, which is used for calculating the bandwidth of noise. The noise bandwidth is chosen by taking the largest of the thermal velocity and Since the inverse of the bit length exceeds twice the thermal velocity times the wave number by at least one order of magnitude, changing the ion mass slightly has no effect on the signal-to-noise-ratio.

In the paper, we will change "where m_i is the ion mass, which we set equal to 32 u corresponding to O_2^+" to "(…) to 31 u corresponding to a mixture of O_2^+ and NO^+. These are the two most dominant ion species in the E region (Brekke 2013)." (l. 9). At the end of the paragraph, we will add "For all bit lengths investigated here, τ_b^{-1} exceeds $2v_{th}k$ by at least one order of magnitude. The bandwidth is therefore independent on the ion composition as long as the measurements are restricted to the E region."

>- Page 8 Figure 2: I'm having trouble seeing the need for this figure since the assumption is for a monostatic system, and chemical composition is off too. I'm aware with MIMO that the resolution can be chosen but this link budget only accounts for a monostatic system. Plus in a monostatic system the gain from the antenna is directly tied to the resolution.

We feel this figure is of primary importance. It tells us if imaging is feasible or not from the point of view of signal to noise ratio. The figure shows the expected time resolution achievable with EISCAT 3D.

The caption for Figure 2 states the following: "Integration time of targets in the E-region observed using the E3D core for transmit and a single 91 antenna element module for receive".

A single 91 element antenna module is the antenna module used for interferometry. The integration time is the required integration time to estimate a cross-correlation function between antenna modules, which is the basic measurement that goes into imaging.

The gain of an antenna module is significantly less than it is for the full core array receiver. We therefore felt it to be important to investigate if it is even possible to make a measurement of incoherent scatter radar with reduced gain on the receiver.

Knowing how long it takes to estimate a cross-correlation between interferometer antennas with a certain accuracy is of primary importance when determining how long it takes to make a radar image with this system.

>- Page 13 lines 13-14: Please be more clear about what was done to the image from Figure 1. It seems you just took the magnitude of the image and mapped it from 0 to 1?
Yes, this is correct. We will add a sentence on this to p. 18, l. 14 where we mention the image: "(As original image, we use a part a part of Fig.1). A part of 97 x 97 pixels was cut out of the figure and the greyscale values were mapped to lay between 0 and 1. (From the...)"

>Overall, good paper. I think it should be accepted with the changes I’ve go over above.

>[1] J. Semeter, T. Butler, C. Heinselman, M. Nicolls, J. Kelly, and D. Hampton, “Volumetric imaging of the auroral ionosphere: Initial results from PFISR,” J. Atmos. Solar-Terrestrial Phys., vol. 71, no. 6–7, pp. 738–743, May 2009.

Thank you for your comments.