Response to anonymous referee #2

We thank the referee for the throughout review which has helped us to improve the manuscript. The review is repeated here in bold and starting with an arrow. Our comments are written below.

>The present manuscript analyzes the problem of radar imaging in 3D for incoherent scatter applications that will be implemented using the EISCAT 3D radar. It is mentioned that the proposed technique includes "near field" effects on the formulation of the radar imaging problem because EISCAT 3D applications will be in such regime. The analysis includes also the concept of MIMO radars in order to improve the resolution of radar images. The manuscript is well organized and the results are presented clearly. Although the analysis performed in this document introduces new ideas related to the radar imaging problem, I would recommend a careful revision of the document before its possible publication. As I will explain there are some important issues that have to be addressed first.

>1. In Equation 2 (page 5), it is assumed that the number of independent measurements per second is proportional to the number of lagged products in a longpulse experiment. This is definitely not the case. In a long pulse experiment, lag products are not independent, all of them are correlated. This is because, within the length of a pulse, signals from a common volume are mixed. Assuming that all lag products are equally informative, it is also an oversimplification that I think may lead to not necessarily correct conclusions, particularly, in this application in which the target fills the radar volumen. I would recommend the authors to review this section in order to analyze more carefully the relationship between the number of samples within a longpulse and the integration time needed to reduce statistical uncertainty. Notice that if you consider Np=1, there is a singularity in equation (7), I don't think this is correct. I would also recommend to review equation 11 since a radar volume can be modeled better as a spherical cone section rather than as a truncated cone. In this expression, if you consider "r" at the center of the radar volume, the expression becomes simplified.

In the lagged products, the signal is correlated, but white noise is not. As long as the noise power is much greater than the signal power, also clutter and other non-white noise effects can be neglected. The lagged products are therefore independent for low SNR. At zero lag, the product includes all the white noise from the receivers. We therefore ignore the zero lag. This is where the singularity in eq. (7) comes from. If including the zero lag, the denominator would be $F_m N_p(N_p+1)$ without singularity. When inserting Np = 1 into Eq. (7), and the zero lag is ignored, there are no measurements left and the variance is infinite.

In the E region, the decorrelation time is long in the VHF band which is due to heavy ions (O_2^+ and NO^+) and relatively low electron and ion temperature. While the pulse is 0.5 ms long, the decorrelation time is around 1 s.

We have investigated the difference between a truncated cone (conical frustum), a spherical cone section, and a cylinder when modeling the volume in the E-region using a radar beam corresponding to the solid angle of the EISCAT 3D beam. We found no significant differences between these three models in this case. This study is included in the referee response.

The radar volume in Eq. (11) is indeed better represented as a spherical cone than by a truncated cone. Changing the model to a spherical cone has the consequence that $\tan^2(\theta/2)$ is substituted with $2(1-\cos(\theta/2))$. For small θ, like θ = 1° as in the article, the difference is small. For significantly larger beam opening angles (more than 10 degrees), these models start to diverge. Considering r to be the range to the center of the radar volume simplifies the expression in the brackets to $3r^2+\Delta r^2/4$

and the volume shrinks about 1%. The equation in the manuscript will be changed to the spherical cone, but letting the range be to the lower boundary of the volume as before since this is closer to what was used in the calculations.

>2. In the introduction (line 22, page 4), it is mentioned that there is not much literature related to 3D imaging and the authors make reference to a recent work of one of the coauthors. This is not fully true, the works of Palmer et al(1998), Yu et al (2000), and Chau & Woodman (2001) (see references below) addressed the imaging problem in 3D in the same sense as the present manuscript does. Of course the difference is that the new approach is addressing the incoherent scatter problem while the previous work was mainly focussed on coherent scatter echoes. So proper references should be used.
We will add the references mentioned. Since the literature description in the article starts to become complex because similar literature is described two places in the text, we merge the literature on imaging on p.2. Here we will clarify that the novelty is the use on incoherent scatter.

>3. In line 29, page 7, the integration time for MIMO applications is analyzed and it is mentioned that the integration time will be longer in the MIMO case than in the SIMO case, but the authors indicate that the difference depends on cross-coupling between antennas. I don't think this conclusion is correct, at least not as a first approximation. There is plenty of literature related to soft-target radar equations that explain clearly that the received power is directly proportional to an effective antenna aperture area (which is also proportional to the true antena area). So, even if you use the same power on transmission, the received power will be less when using a small antenna. Then, the need for additional integrations in the MIMO case is directly related to the fact that smaller antennas will be used, less power will be detected and SNR will be smaller. Cross-coupling may have an additional role but that is definitely a second order effect. I would recommend to review Radar Principles by Toru Sato. https://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/19910017301.pdf I would also recommend to review the work of Woodman(1991) which is very related to the type of analysis performed in this work.

We were unaware of the Toru Sato and Woodman papers. We have studied them and they both seem like useful references on how atmospheric radars operate.

When using MIMO, the transmit antenna is divided into N separate transmit sections. These different regions need to transmit different waveforms in order for us to be able to separate the different transmitter sections on receive.

If we divide the antenna into N parts (and thus N independent transmitters), each transmit section will have an aperture of A/N and a power of P/N. Here A is the total area of the full array and P is the total power of the full array. This is the ideal case.

In discussions with EISCAT staff, we have been told that two neighbouring regions of the antenna should not be transmitting simultaneously with different codes, as the mutual coupling of two different transmit signals might be problematic. This mutual coupling may in the worst case cause power amplifiers to overload and break.

It was suggested that in order to reduce mutual coupling of different regions of the antenna when dividing it into multiple transmitters, buffer zones could be made around each section of the antenna array. This would reduce the amount of area and power for each section, making it slightly less than A/N and P/N.

We have tried to carefully reword this in our manuscript to make this point more clear.
Now the last part of the paragraph says:
"The transmit gain must be divided by the number of transmitters. It could be that because of cross-coupling between antennas, there must be buffer zones between transmitters. Then the gain decreases furthermore. On the other hand, the radar will illuminate a larger volume that contains more scatterers and so increase the received power again. In conclusion, the integration time for MIMO will be longer than for SIMO. How long is mostly dependent on the possible cross-coupling between antennas."
We will change it to:
"Because of the smaller antenna area, also the transmit gain must be divided by the number of transmitters. Additionally, there could be cross-coupling between antennas, which force buffer zones between transmitters. Then the antenna area and gain decrease furthermore. In conclusion, the integration time for MIMO will at least be the number of transmitters times the integration time for SIMO."

>4. In the discussion about the baseline cross-correlation, it is not clear why equations 20 and 23 (pages 10 and 11) should give different results. Both expressions come from taking the Fourier transform of a gaussian blow. It seems the difference comes from a different interpretation of the geometry. So, if the same interpretation is given both results (the far field and near field expressions) should be the same.
Equation (23) truly represents the farfield, and Eq. (20) was derivated mostly in nearfield. However to be integrateable, between Eq. (16) and (17) there was done an approximation to make the exponential linear. The approximation can be interpreted as assuming plane waves. Therefore, Eq. (20) is not exact anymore.

> Then, let me ask what the "near field" effects are.
The nearfield effects are blurring of the image as can be seen in the image reconstructions with matched filter. Palmer et al. (1998) call the method "Fourier-based imaging" because it uses the Fourier transform for reconstruction, which implies the farfield approximation.

>In fact, let me mention the following. In the work of Woodman (1997), it is argued that the near field effect can be modeled as a phase correction in the visibility domain, however, in the present manuscript the near field effect is not presented as a phase correction but as a change of the magnitude of the visibility (correlation) function. Given the different interpretation of the near field effects, I should ask again if the there is actually a "near field" effect that has to be considered in radar imaging problems.
The phase correction in Woodman (1997) can probably be used for imaging with EISCAT 3D. In the study we however followed an other approach where we do the simulations completely in the nearfield. As we say in the introduction, the computation becomes more complex, but is accessible with modern computers.

>Let me add one more detail. Woodman(1991) derives an expression for the cross-correlation between the voltages of two different antennas showing that the cross-correlation is equal to the Fourier transform of a Brightness function to a second order approximation. In this derivation, there was no need to match the Fraunhofer condition, it was enough that the radar range should be much greater than the separation between the antennas (R»D). This result was actually a generalization of an earlier result presented by Kudeki(1990).

>This is a very important issue that needs to be reviewed more carefully in this manuscript. Since it is argued that "near field" effects are considered, the authors should show clearly what these effects are. However, based on previous literature, it seems that the Fourier transform approximation is good enough for the EISCAT 3D scenario. If that is the case, the

problem presented in the manuscript gets simplified and the results presented can be obtained without a complicated framework.
It seems that Woodman (1991) assumes plane waves in a similar form as the linearization mentioned above. With the convention Toru Sato refers to, everything closer than ~1000 km is in the nearfield, if including the EISCAT 3D outrigger subarrays. The Fourier transform with correction as described by Woodman (1997) might be good enough for EISCAT 3D, but it is possible to calculate the theory matrices and do the simulations in the nearfield taking into account the spherical nature of the backscattered wavefronts and the antenna geometry. In general, when solving inverse problems accurate theory matrices are important.

In practice, when we have some imaging measurements from EISCAT 3D and we want to reconstruct the image with SVD, only the theory matrix A is needed. Regardless of near- or farfield, the SVD itself requires the most computational power. However, after having been computed once, it can be saved and reused.

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Thank you for your comments.

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