**Reviewer #1 (Formal Review for Authors):**  
The reviewed manuscript entitled "Space time ambiguity function for electronically scanned ISR applications" presents a new formulation of the radar ambiguity function that can be used in the case of plasma structures traveling at constant velocity through the field of view of an incoherent scatter radar. This formulation accounts for the effects of the moving structures on the estimation of radar signal ACFs. The resultant effect is having "blurred" ACFs that may lead to the estimation of erroneous plasma parameters if this effect is not considered. Although, the new formulation might be useful in the proper estimation of plasma parameters, the manuscript does not show clearly the actual impact of using this new ambiguity function in modeling the actual measurements as it is discussed below.   
  
1. In the simulation case 1, we can see how a plasma enhancement moves through the radar beam at different scanning times. As expected the longer the integration time at a given position the more blurred the ACFs will be. However, is it possible to reconstruct accurately the actual densities from the estimated ACFs? As it is known, a blurred image results from a low pass filter process, therefore, there are some structures that will be filtered out and probably we won't be able to reconstruct them. Given that, what are the conditions that have to be met in order to be able to recover the real parameters from the blurred ACFs? For instance, let's imagine the following situation, a plasma enhancement traveling at the same velocity as the radar scan velocity. In this scenario we will probably measured the same ACF at all directions as if the structure is elongated across the radar field of view, is this a limit for the technique?   
On the other hand, if the radar can scan very fast (or faster in comparison with the structure velocity), there will be probably no need to use this formulation as at every scanning position we can consider the plasma to be stationary.   
A discussion about these issues (related to the time and space sampling) might be important in order to address the potential use of the technique.   
-In the first simulation we are sampling on a pulse-to-pulse basis, which is possible with phased array systems. In response to your question, “Is it possible to reconstruct the actual densities from the ACFs?” in the simulation the estimate of the ACFs will converge to the input ACFs given that the plasma parameters are uniform over the time and space covered by one measurement.

In the case of retrieving the parameters from the blurred ACFs it is possible but due to the non-linear nature of the parameter fitting there are a number of issues one has to contend with. Non-uniqueness is a major problem with ISR fitting so its import to pick good starting points and understand the composition of the plasma as well. In our simulation we use some simplifications such as having only one species of ions, O+, which is somewhat close to the F-region. The reason for these simplifications is to try to reduce the impact of other aspects of the fitting algorithms and show what happens when you put in these blurred ACFs. Now if you take two ACFs from plasma with different parameters and put them into fitting algorithm or

There is an inherent difference between pulse-to-pulse steering for phased arrays and scanning for dish antennas. As a dish is scanning through the space it has to integrate over a certain amount of time. In this case a single beam is integrated over a single time period. In a pulse to pulse sampling of the data multiple beams are integrating pulses.

We have added a paragraph and three figures to the discussion of space-time sampling. These are now figures 1-3.   
2. Although it is not clearly mentioned in the text, this formulation applies to the case in which the structures are not volume filling (or have partially filled the radar volume). It should be mentioned that if the structure is volume filling the derivation does not apply, as the ACF R(t,r) does not varies its position in time despite the fact that the plasma may have a drift. It might be important to discuss about the implications of applying this formulation to volume filling situations.   
Alternatively, it should be mentioned that the velocity considered is not the plasma drift but the velocity at which the plasma structure is moving (that are not necessarily the same).   
-This derivation will still apply because even if the target is volume filling its drift will still impact the ACFs in terms of Doppler shift. If a sheer is in the volume then there will be a mixture of different ACFs with different Doppler.   
  
3. In the simulation case 2, it is mentioned that the inverted parameters do not show an enhancement of plasma, however, it would have been better to first show the estimated ACF in order to see whether the enhancement is present or not in the ACF. Most likely, the ACF is so smooth such that the enhancement is also blurred explaining the reason for not having seen the plasma structure. On the other hand, the integration time consider in this case should also be mentioned.   
-If you look in what is now figure 20 you can see the blurred spectra along with what the spectra from the different plasma populations would be if the forward model of the radar is applied. I also added a reference to the integration time which was left out, which can be seen in line 323.  
  
4. In the abstract and in the introduction it is mentioned that the paper is going to explore through ways of improving the estimated parameters. However, the authors do not actually "explore" but shortly discuss about possible techniques to invert ionospheric parameters. It would have been interesting to see the actual performance of the formulation presented in this paper in the inversion of parameters applying the full model and a least-square fitting approach.   
-We are currently looking at techniques to take advantage of this new formulation, mainly from the image processing and image formation community. If we were to do this in one paper we fear this may make it very long. The other point of the discussion was to talk about the differences between full profile analysis techniques and lag-profile inversion techniques. Lastly we do use a least-squares fitting approach in case 2 to reconstruct the plasma parameters.

5. The 2D figures presented in the manuscript (Figures 8, 10, 11, 12, 14, 15) have an xy-plane cutting the image. Is this on purpose or is it an artifact of the plotting routine? If possible, it would be better to plot only the images in the yz-plane in order to observe better the features of the presented results.   
-We plot the xy plane to show that we are actually plotting a 3-d volume.

6. The derivation of the space-time ambiguity function (until expression 10) is a standard procedure presented in previous literature. The authors can refer for instance to the work of Woodman(1991) for this derivation. Since the derivation until this point is a standard procedure, it might not be necessary to include it in the manuscript but instead replace it for a proper reference.   
R. F. Woodman, "A general statistical instrument theory of atmospheric and ionospheric radars," Journal of Geophysical Research, vol. 96, pp. 7911-7928, May 1991.

-It’s the opinion of the corresponding author that having the derivation present is important because it helps the reader understand the notation in the rest of the paper. There are also numerous versions of this derivation and by jumping back and forth between a previous derivation might make it hard to follow for the reader. I have added a citation to Woodmen and others who have done similar derivations to the paragraph after equation 10.

7. There are typos here and there that need to be fixed, particularly in the equations. For instance from equation 12, the differential d\tau is not included in the integrals. Also review caption in Figure 13.

-Added d\tau to equations from 12 on when necessary.  
  
Given the above discussion, this work still requires some improvement before to be ready for publication.

**Reviewer #2 (Comments to Author):**  
This manuscript discusses a framework for interpreting the measurements of an electronically steerable array (ESA) for incoherent scatter (IS) measurements. It aims, in particular, to explicitly introduce the concepts of spatial inhomogeneities and temporal variability in an IS measurement environment. While these concepts are not unique to ESA measurements in that they are also present in dish-based IS data sets, they are both more tractable with rapidly-steered antennas and especially important in the proper interpretation of ESA images.   
  
Generally, the manuscript does a good job of describing the 'forward problem' of describing how the fundamental physical quantitates are hidden by the necessary integration of IS returns. It is not nearly as strong, however, when it comes to describing how to mitigate the effects. There is also the possibility for misinterpretation of the paradigm. When looking, for example, at the operator A() in equation 19, one can be tempted to assume that A can be properly accounted for in the analysis (as is done in standard IS parameter extraction). This is only the case, however, if R() is spatially uniform - which defeats the purpose of the model. It would help if this could be explicitly pointed out in the text.   
  
Here are a few specific suggestions:   
  
p.2, par. 1 - the abstract suggests that the dish-based antennas provide a two-dimensional slice. Isn't it just one dimension at any given time (range)?

-Often dish based antennas are scanned which creates a 2-d slice through space. In a sense we are stating at most dish based antennas can be a two-dimensional slice.  
  
p.6, eq. 1 - As far as I can tell, L() is never explicitly defined.   
-Definition added.  
p.7, par. 3 - K(r\_s,r) is described as a space-time ambiguity function but time is not a parameter.   
-Corrected typo, should have been L(tau\_s,r\_s,t\_s. tau,r,t.)  
p.15, par. 2 - the text states: 'Once the operator has been determined, standard processing techniques can be used as if the plasma is not moving, under the previous assumptions.' This is very misleading because it suggests that the plasma parameters do not vary spatially. Later in the text the limitations are shown, of course, but the implication here is not realistic.

- Replaced last sentence with “With this strategy the operator is acting purely as a spatial blurring function, thus reducing the dimensionality of the problem.”  
  
p. 19, end - Is there some reason that the electron density and temperature ratios are varied in a way to maintain constant variance? This sounds like an odd constraint.   
- We want to maintain constant variance through out simulation so all of the points will have the same expected error.  
p.20, par. 2 - The explanation with respect to the fit surface doesn't seem correct. Isn't the real problem that the plasma model used in the fitting is for a single set of parameters (Ne, Te, Ti, Vi) while the modelled situation is with different values in different volumes, which are then integrated together? This is mentioned very briefly on p. 21 with a reference to Knudsen et al. 1993.   
- Yes, the root reason is that there are two sets of parameters in one resolution element. The fit surface is used as a conformation that the fitter is incorrectly choosing the parameters. We reference Knudsen et al. 1993 because that publication observes similar phenomena but that experiment is using a multistatic radar configuration which shows how the spectrum can become distorted from different populations of plasma in one resolution element.   
p.22, par. 2 - The text states 'This would allow a statistically stationary ACF to be formed from plasma populations with the same physical state as they move through the field of view.' One detail that should be mentioned is that while the plasma state is uniform the geometry is not. Different beams will, thus, have different components of the vector drift velocity. Thus, they should not be simply integrated together.

- The statement has been changed due to another comment but we have added “assuming one corrects for geometry differences that arise in Doppler offset.”  
p.24, par. 3 - '... we can start with the linear array pattern...' should probably be '...we can start with the rectangular array pattern...'.   
-Changed to “the pattern from the first array can be represented as”  
  
p. 25, par. 1 - the shift of the second array is in both the x and y directions, not just the x direction (thus the n-1/2 in eq. A2).   
-changed to “$x$ and $y$ directions”  
p.25, eq. A3 - the term after the second = appears to be E1(), not E2(), I think.   
-Fixed, added exponential term to make it correct.

**Reviewer #3 (Formal Review for Authors):**   
  
Review of "Space-Time Ambiguity Functions for Electronically Scanned ISR Applications" by Swoboda et al. (paper #2014RS005620)   
  
This paper presents the space-time ambiguity function of relevance to IS autocorrelation function estimates when plasma is moving, resulting in a blurring effect related to the plasma motion and antenna beamwidth. The paper is interesting and sets up the ambiguity function in a logical manner. The paper does not actually show deconvolution of the space-time ambiguity function but shows its impact on parameter estimates.   
  
I have many comments below, mostly minor, and my recommendation is publication after addressing the comments.   
  
Why is the space-time ambiguity function only relevant to phased array ISRs? It would seem applicable to any ISR measurement.   
- Agree that it is applicable to other ISR measurement systems. Added stement to 2nd paragraph on page 6 “This paradigm can also be applied to other types ISR systems as well, but much of the utility of using this new formalism will likely be seen with ESA based systems.”  
Abstract: general comment on the abstract, it is too long and includes some introductory material.   
-Removed sentence “This concept is similar to the range ambiguity function that is used in traditional ISR for scanning antenna systems, but we have extended the concept to all spatial dimensions along with time as well. ” and combined paragraphs.  
Line 6-7: "This is in direct contrast to dish based antennas, where ISR acquisi7tion is limited at any one time to observations in a two dimensional slice." I'm not really sure what this means. Why couldn't a dish scan to form a 3D image?   
-With a dish antenna pulse-to-pulse steering of the antenna beam would not be feasible or a large angular area like is done with phased arrays. With out pulse-to-pulse steering the dish would have to scan through a through on a track. This would have each integration time be sequential as opposed to simultaneous with a pulse-to-pulse steering method. The ambiguity function formed for each beam would have no time overlap. This may be problematic when using different reconstruction algorithms.  
Line 16-17: "introducing potential error and impacting the reconstructions of the plasma parameters." The sentence leaves it unclear what is causing the error. Is it the spherical to Cartesian transformation or the interpolation process?   
-The interpolation procedure is looking at a single set of data points at one point in time. This can yield errors such as when plasma structure moves in between beams and the structure seems to “flicker.”  
Lien 22: the use of the term "space-time" ambiguity is a bit confusing before definition as the traditional 2D ambiguity function also includes time (lag). This is really the space-slow-time ambiguity function, right?

-True, this is a space slow-time ambiguity function.   
  
Line 23: "allow" -> "allows"   
-Fixed it  
Line 25-26: isn't the goal the measurement of the plasma parameters, not the ACF? It would seem much more reasonable to pose physical constraints on the plasma parameters for this problem.

-One could possibly apply constraints to the plasma parameters. The issue is that there is a non-linear relationship between the ACF and the plasma parameters thus making the initial application of the linear inverse theory to only the ACF. It is correct though that the final goal of ISR is to measure the plasma parameters. In order to reflect that we changed the original sentence to ”The use of this new measurement ambiguity function allow us to pose the ISR observational problem in terms of a linear inverse problem whose goal is the estimate of the time domain lags of the intrinsic plasma autocorrelation function used for parameter fitting.”  
  
Line 35: Most ISR models are formulated in the spectral domain   
-Changed sentence to “These parameters are measured by fitting a nonlinear first-principles, physics based model of the power spectrum of the signal scattered from the random electron density fluctuations to that estimated by the radar. Alternatively, this fitting can be done in the lag domain buy using the intrinsic autocorrelation function (ACF) of the plasma, which can be determined by taking an inverse Fourier Transform of the power spectrum ”  
Line 40-41: I would specify "incoherently averaging". And you are not averaging pulses, but the spectra or lag product estimates.   
-Changed sentence to “In order to get an estimate of the ACF with reasonable statistical properties, an ensemble average must be performed by averaging power spectra or autocorrelations together from different pulses.”  
Line 63-65: I don't think that this definition of reconstructions is always true. For example, there are published results (e.g., Butler et al., RS, 2010; Nicolls et al., RS, 2014) that do not boil down to direct interpolation between measured points.   
- Added “Others have reconstructed full vector parameters using estimates of the ion velocity which can be determined using a simple Doppler shift of spectra” along with citations for those two articles.  
Line 74-74: What about evolution of the plasma? Just because you know where it's going doesn't mean that stationarity can be achieved by transforming to the moving frame.   
-Added the statement “or plasmas that are evolving or changing their shape on time scales longer then the integration time”  
Line 76: I'm not sure that "different plasmas" is the right terminology here.   
-Changed sentence to “Such a technique is very difficult to accomplish with dish antenna based systems having significant pointing inertia, and typically in these situations the returns from multiple plasma populations with different parameter sets are unavoidably and improperly averaged together.”  
Line 89-90: Provide a reference here   
-In this case we present the final solution of the derivation to give the reader an idea of where we’re going with the derivation. We do reference a number of other papers that show the 1-D range ambiguity function derivation in the paragraph after equation 10.

Eq1: The parameters in the equation need to be defined!

-Fixed added explanation for L  
  
Line 97-98: Also discontinuous sampling   
- The same holds for the ISR measurement problem, except that the pixels are no longer square or continuous is Cartesian space and instead are determined by the beam shape and pulse pattern.  
Line 119-121: Also highly dependent on frequency.   
- Changed sentence to “Generally, for incoherent scatter applications in the E-region of the ionosphere (~100 km altitude) and above, with systems that have a center frequency in the UHF band, the decorrelation time is less than a PRI, and thus ACFs must be formed over fast-time.”  
Line 129: at a specific wavenumber, k   
-added at a specific wavenumber $\mathbf{k}$ to the end of the sentence.  
Section 2.2: need references throughout derivation   
-We have a few references through out and at the end state a number of references in the paragraph after equation 10.  
Eq 6: dr should be a vector integral?   
-No, fixed it.  
Eq 11: I don't see why including this equation or the appendix in is relevant, as the simulation uses a very approximate beam definition. Also, without taking into account the element geometry, the correct grating lobe pattern won't be achieved. I suggest removing this equation and the appendix.

- If necessary we can redo the simulations with using these beam pattern expressions. Also, the corresponding author has yet to see any expression, even idealized, of the AMISR pattern in the literature. This could help the community by giving an expression to it even though it is simplified.  
  
Figure 3-4: Why are the figures "binary" and not color coded? The ambiguity function should have an amplitude.   
-This is a simplification but we try to reduce this almost to a 3-dB surface function to show the basic spatial extent of the ambiguity. It in a way it simplifies things to a 3db point in this high dimensional space. Adding a color to a 3-d plot could possibly be confusing.  
Line 185: PRI time period == IPP   
-Fixed  
Line 201-207: This is certainly not always true. Features can rapidly evolve in the polar cap, especially in response to solar wind variations.   
-We agree that this is not always true and we are using this an example of how the new expression of this ambiguity function can be used. We simply are saying this is a scenario.  
Line 219-220: The estimation of the vector velocity seems critical. How would errors in the velocity estimation fold into the analysis? In addition, the velocity field itself may have structure not captured by the measurements. It seems like the velocity should form part of the estimation procedure.   
-We have not done the calculation of the impact of errors from the velocity field. We are assuming we can derive the velocity field using a technique seen in Butler 2010.

Figure 8: Why does the phantom look "cross"-like if it is a sphere?   
-To generate a sphere we add an enhancement to the plasma within a spherical volume. So some of the corner cases are outside of this due to the nature of the input grid sampling.  
Figures 10-12: Are the reconstructions being interpolated between beams? If so, please state this explicitly

-Added “Lastly to plot the three dimensional structures after fitting we use a natural neighbors interpolation \citep{Semeter2009738}.” To page 18 paragraph 3.