**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.   
  
  
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).   
  
  
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.   
  
  
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.   
  
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.   
  
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.   
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.   
  
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)?   
  
p.6, eq. 1 - As far as I can tell, L() is never explicitly defined.   
  
p.7, par. 3 - K(r\_s,r) is described as a space-time ambiguity function but time is not a parameter.   
  
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.   
  
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.   
  
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.   
  
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.   
  
p.24, par. 3 - '... we can start with the linear array pattern...' should probably be '...we can start with the rectangular array pattern...'.   
  
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).   
  
p.25, eq. A3 - the term after the second = appears to be E1(), not E2(), I think.

**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.   
  
Abstract: general comment on the abstract, it is too long and includes some introductory material.   
  
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?   
  
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?   
  
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?   
  
Line 23: "allow" -> "allows"   
  
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.   
  
Line 35: Most ISR models are formulated in the spectral domain   
  
Line 40-41: I would specify "incoherently averaging". And you are not averaging pulses, but the spectra or lag product estimates.   
  
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.   
  
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.   
  
Line 76: I'm not sure that "different plasmas" is the right terminology here.   
  
Line 89-90: Provide a reference here   
  
Eq1: The parameters in the equation need to be defined!   
  
Line 97-98: Also discontinuous sampling   
  
Line 119-121: Also highly dependent on frequency.   
  
Line 129: at a specific wavenumber, k   
  
Section 2.2: need references throughout derivation   
  
Eq 6: dr should be a vector integral?   
  
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.   
  
Figure 3-4: Why are the figures "binary" and not color coded? The ambiguity function should have an amplitude.   
  
Line 185: PRI time period == IPP   
  
Line 201-207: This is certainly not always true. Features can rapidly evolve in the polar cap, especially in response to solar wind variations.   
  
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.   
  
Figure 8: Why does the phantom look "cross"-like if it is a sphere?   
  
Figures 10-12: Are the reconstructions being interpolated between beams? If so, please state this explicitly