# Comments on Journal Submissions

## JSTARS-2014-00192

### Associate Editor Comments:

Associate Editor  
Comments to the Author:  
Dear Dr Le:  
Your manuscript entitled " Scalar and Representative Observables, and Their Associated Statistical  
Model, for Polarimetric SAR Data", which you submitted to IEEE JSTARS, has been reviewed. The referee  
comments are included at the bottom of this letter.  
The referee(s) think that the manuscript cannot be published as it is. Therefore, I invite you to revise  
your paper according to the referee(s)' comments and resubmit it once all the referee concerns have  
been properly addressed.

### Reviewer's Comments: Reviewer 1

Comments to the Author  
this paper investigates various statistical models for the ratio of the determinant of covariance matrices. Overall it is an interesting paper but some elements are missing in order to ensure a good repeatability of the results.  
  
I'm a little bit sceptical about the derivation of the pdf for the ratio (21) in order to get (23), are you just showing a ratio of chi-squared distributed random variables without actually computing the ratio pdf? usually the derivation of the pdf of the ratio of independent random variables implies an integration using a Jacobian, therefore some details or a reference would be appreciated.  
  
The pdf[] notations could be removed on page 4, we know that we are dealing with pdfs.  
  
p4L55, second column: please define the notation \Lambda in ln(Q), is it the  Wilks's lambda distribution?  
  
Section VI: I'm little surprised that the authors chose to look immediately at real data, I would have expected some Monte-Carlo simulations where the parameter values for the various pdfs are perfectly known (in particular L which is estimated here) and uncertainties about sample homogeneity are also absent. The various histograms are maybe easier to display in a log-probability axis especially for the tail behavior. Also, the various sample sizes should be given as well as the estimated L values.  
  
In Section VI.B, in the multi-dimensional case, it is not clear what analytical relation (if any) similar to (26) was used for computing the model pdfs for R\_\Sigma (22) and R\_C (23) (related to my first comment).

## RSN-2014-0174

### Reviewer 2:

Comments to the Author  
This is a well-written and well-organized paper, with a good theoretical introduction and a good statement of the topic it tackles. The use of the log transformation for transforming the multiplicative speckle noise into additive noise is well known. The statistical study is interesting and correctly presented, but the final contribution is not clear.  
Although many studies are presented, a more thorough and wide study focused on the SAR images is strongly suggested. The paper uses many simulated images and ground truth images, but only one SAR image. The parameters of the image are not provided (acquisition mode, resolution, geographical área,...). A study of the impact different areas of SAR images (urban, water, land, forest, etc.) or image products would have on the parameters presented in the paper would be not only interesting but in my opinion also necessary to validate the results already achieved.

## TGRS-2013-00497

### Associate Editor Comments:

The major problem of the submitted paper lies in that the scope of the context does not reply to what the title implies. Authors claimed that the proposed statistical models for POLSAR data are new beyond what are commonly known, but there are no evidence to support it. Quite lengthy equation derivations was presented at best. Reviewers clearly question the weakness of the presented paper, but suggest the authors redo their work and resubmit.

### Reviewer 1:

This manuscript deals with a new scalar and representative models for polarimetric SAR data. The idea, introducing the scalar and representative models, is very interesting. However, I have a problem and several suggestions.  
  
1.      What do v1 and v2 mean in defining contrast of equation (35), and how does Figure 1 figure out?  
2.      Propose the authors to move Section III-A into Section II. The introduction of POLSAR discrimination measures should be after the introduction of POLSAR covariance matrix.  
3.      It should give the definitions before arg and avg are used.  
4.      It is better to adjust the order Equation (4) and Equation (5). Equation (4) is the symmetric refined Wishart distance, and Equation (5) is the asymmetric refined Wishart distance.

### Reviewer 3:

This paper discussed the statistical modeling of PolSAR data. It generalized the some commonly used one-dimensional SAR statistical models for multi-dimensional POLSAR data.  
Although the authors repeatedly claimed their proposals are novel and highly representative, there are lack of proofs in both theory and application. This is the main weakness and the current development is not complete.  
Also, there is only one example for only visually evaluating the Boxcar speckle filter. It is not sufficient and convincing to believe the superiority of your proposal. The authors are encouraged to intensively redevelop the technique, clearly demonstrate and validate the superiority of the proposal with sufficient comparison studies.  
There are several major revision suggestions to be considered and some confusing statements or judgments need to be clarified or proved. All of them are listed as follows  
1.      In Abstract, “Compared to other scalar statistical models for POLSAR, the proposed models are highly representative of the multi-dimensional data and enable useful discrimination measures to be easily determined.”  
Please explain why the proposed models are highly representative?  
2.      In pp. 3, “While many scalar observables for POLSAR were presented, their corresponding statistical models and classifiers were not available.”  
Please provide the references and detail exactly what scalar observables you meant here.  
3.      In pp. 3, “…, none of these observables have been shown to meet the dual criteria of 1) resulting in statistically consistent discrimination measures and 2) being representative of the complex POLSAR data.”  
Can you explain the advantage of the dual criteria? And please explain clearly how to judge one measure is representative or not?  
4.      In pp.5, “The first point is that the POLSAR data is multiplicative and heteroskedastic in its original domain”.  
What is your meaning in this statement? “multiplicative” and “heteroskedastic” are assumptions or facts? Why?  
5.      In pp. 23, “Similar to the way that other measures of distance can be used to derive POLSAR classifiers [8], change detectors [1], edge detectors [22] or other clustering and speckle filtering techniques [15] [23], new detection, classification, clustering or speckle filtering algorithms can be derived using the models presented in this paper.”  
This is exactly what I expected the authors should have done. You can directly compare your developed measures with others (for example, Conradsen’s measure [1]) in one or several of these applications. And you can make a conclusion which proposal is more advantage. This is a more convincing way to validate the potential superiority of your proposal.  
6.      Finally, as far as I understood, you have proposed a number of scalar statistical models for PolSAR data. However, the current version is hard to understand. Please well investigate and summarize their inter-relationships. Please also discuss and demonstrate the performances of the independent measures. These investigations will be more helpful for readers.

## TGRS-2012-00949

### Associate Editor Comments:

Taking log operation does not change the inherent property of multiplicative SAR noise. Hence it is doubtful the effectiveness of the log transformation. Indeed the idea is not new and is generally not popular simply because it does not offer advantages from filtering point of view. To evaluate a SAR filter's performance, several figures of merit must be applied, such as edge preservation, strong target isolation, ENL, statistical distribution conservation, etc.; MSE is only one of them. In addition, a useful filter must be generalized enough to copy with various applications, by meeting some baseline requirements. It is too trivial to develop a filter for one particular application.

### Reviewer 2:

Several weeks ago, I reached the end of this never-ending manuscript without reaching a decision on it. The paper is pleasantly written in fluent English, seems well reasoned and documented, and is sparkled by heavy math, all rigorously correct. However, there was something that was unconvincing for me, who has been working on SAR and despeckling over almost twenty years, but I did not find explicit clues of that. Few days ago, eventually I found enough time and started reading the paper again, this time from a different perspective: what is proposed is true and mostly agreeable, but is it also new and useful? Strangely, I found that the term “2nd order statistics” or equivalently “autocorrelation” is never mentioned concerning speckle. This lack was the clue I was looking for. The development of theory and all simulations are made with white speckle, which is a totally unrealistic model. The equivalent number of (independent) looks (ENL), which is always lower than or equal to the nominal number of looks L (the number of observations that are averaged to yield a multilooked SAR image) strongly depends on speckle autocorrelation, which is due to several causes, including slight over-sampling and focusing of raw data with frequency windows. ENL = L iff the looks that are averaged are independent of one another, that is if speckle is uncorrelated and the signal is not oversampled. ENL is traditionally calculated on homogeneous patched by taking sample mean and variance, for either intensity or amplitude images. When the authors try to estimate ENL, which is actually L, they pursue a task that makes no sense, because L is always known, while ENL, which is generally non-integer depends on autocorrelation, which is never considered to be nonzero.  
  
To come to the essence of paper, I do not see the need for using logarithm. Just take the ratio of noisy to denoised images and calculate its mean and variance, possibly on homogeneous patches. From the difference between the mean of ratio and one we can see if filtering is biased. From the difference between the variance of ratio and its nominal value, which depends on ENL (not L) and on the image format (intensity or amplitude) we can see if filtering is effective, over-smoothing or under-smoothing. Visual comparisons are also useful, but the authors have chosen dated filters all working with a 3 x 3 sliding window and poorly display the noisy/denoised images (automatic display parameters must be disabled). An explanation of that is because they are not concerned with a ranking of filters, unlike what the state in the paper title. Practically all simulations are so little realistic and inconclusive that box filtering turns out to be the best one, as otherwise expected from theory on a perfectly homogeneous area.  
  
What I remark is that the same result of the authors’ procedure can be achieved by analysing the ratio image, as almost everyone does when true SAR images are concerned, because what is qualifying is taking the ratio, not the logarithm, and the ratio is implicitly taken when MSE is calculated in the log-transformed domain. It seems that logarithm is introduced just to fill pages of math to derive the first order distributions of its first-order statistics. Unfortunately, second order statistics are also relevant in this context.  
  
I find also unbecoming that recent state-of-the-art methods have been thoroughly disregarded. The authors' effort would be at least more exciting if its results were presented to compare more developed and up-to-date filters that are representative of the two most promising approaches to despeckling, nonlocal mean despeckling and wavelet domain MAP despeckling. For the following two filters  
  
1) C. Deledalle, L. Denis, and F. Tupin, “Iterative weighted maximum likelihood denoising with probabilistic patch-based weights,” IEEE Trans. Image Process., vol. 18, no. 12, pp. 2661–2672, Dec. 2009.  
  
2) T. Bianchi, F. Argenti, and L. Alparone, “Segmentation-based MAP despeckling of SAR images in the undecimated wavelet domain,” IEEE  Trans. Geosci. Remote Sens., vol. 46, no. 9, pp. 2728–2742, Sep. 2008.  
  
I checked that it is possible either to download a Matlab code or to remotely run an implementation made by the authors. Furthermore, the authors have developed and published (and reference as [23][24]) two despeckling filters presumably relying on the concepts they are proposing now. How do they compare with the state-of-the-art? Why they have not been included in simulations.  
  
To come to a conclusion, what the authors state is mostly true. But what is true is also already known (take the ratio, not the difference, to assess filters; never use logarithm to perform homomorphic despeckling). What the authors do not consider is that speckle is autocorrelated and hence calculating ENL from the MSE of logarithm, as they propose, is not correct.  
Furthermore the manuscript is over-overlong and still lacking essential results (MSE of logarithm is preferable to mean and variance of plain ratio). The choice of filters is unfortunate, not to say irritating (filters dating back 30 years ago running with improper parameters are of no interest to anyone).  
  
My recommendation is that if the proposed approach were able to characterize two comparably performing, yet substantially different filters, better than the traditional statistics of the ratio, then the present manuscript, amended of all unnecessary theory and unclear/unfitting simulations (simulations on pattern target recognition are obscure and misleading, also because figures are not properly explained) might become a TGRS paper. Otherwise the authors should be aware of the intrinsic limitation of their otherwise theoretically elegant approach and condense the length of their manuscript proportionally to the significance of their proposal, possibly changing the target journal to GRSL.