# Answer to JSTAR reviewers’ comment

ID: JSTARS-2014-00192

Title: Scalar and Representative Observables, and Their Associated Statistical Models, for POLSAR data.

## Reviewer 1:

Thank you for taking time to read and review this paper, and for giving us some useful suggestions and comments.

**Comment:**

**Overall it is an interesting paper but some elements are missing in order to ensure a good repeatability of the results.**

Answer:

The paper has been written with an aim to ensure good repeatability: The proposed PDF can be tested with both Monte Carlo simulations and real-life captured data. In fact, several repeatable and easily verifiable test experiments are demonstrated in the paper. In fact, readers are welcomed to retry these experiments on different datasets.

The authors would be delighted if the reviewer can be specific on what elements need to be added to increase the paper’s repeatability or if there was any specific difficulty in retrying these experiments.

**Comment:**

**I am 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 reference would be appreciated.**

Answer:

The PDF derivation was originally prepared in an appendix, but was removed to keep the paper concise. Please find the appendix attached. It should be noted however that the PDF derivation, as commented by the reviewer, is complex and was not done for unconstrained dimension (d) and look number (L). For simple cases, e.g. D=1, the PDFs are derived in the appendix and are presented in the paper in Eqns. (24) to (26).

**Comment:**

**The pdf[] notations could be removed on page 4, since we know that we are dealing with PDFs.**

Answer:

The pdf[] notation is used to differentiate the notation in Eqns. (21) to (23) also in page 4 where, as the Reviewer commented earlier, are not written in PDF format. For example, Eqn(16) illustrates an observable which behaves like a product of multiples random variables. We could remove it, but it does reduce the possibility for confusion, at only almost no cost to the un-confused reader. On balance we would argue that it is better to keep it.

**Comment:**

**P4 L55, define the notation in ln(Q)! Is it the Wilk’s lambda distribution?**

Answer:

The notation in this paper stands for our so-called log-chi-squared distribution. It is defined as where is a random variable following the chi-squared distribution. Given that the PDF of the chi-squared distribution is available , the PDF of the log-chi-squared distribution can be derived as: .

It is not the same as Wilk’s distribution. Even though they are related in the sense that:

1. If both random variables A&B follow Wishart distribution and can be considered as independent of each other, then

The derived observable follows Wilk’s distribution

1. We assert that (in this paper) and its log-transformed version (not presented here) follows fixed distributions.
2. Not shown in the submitted paper, but elsewhere, i.e. the first author’s thesis, it is shown that where stands for our log-chi-squared distribution.

To avoid confusion the equation for ln(Q) has been removed from the paper.

**Comment:**

**I am a 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 various pdfs are perfectly known (in particular L which is estimated here) and uncertainty about sample homogeneity are also absent.**

Answer:

Actually, many Monte Carlo simulations were carried out, under various conditions, during our experiments. Showing a plot specificaly to demonstrate that the stochastic Monte-Carlo simulation matches the theoretical statistics model is probably unnecessary for inclusion in what is already quite a long paper (but could be done very easily by the interested reader). So we simply state that the results match but do not include the graphs in the paper. We felt it is more important, and more interesting, that this paper demonstrates the validation of the model against real-life practical data.

**[HAI – Do you have such a graph ---- I mean Monte Carlo vs theoretical? I don't think it should go in the paper, but might be nice to include it here]**

**Comment:**

**The various histograms may be easier to display in a log-probability axis especially for the tail behaviour.**

Answer:

Our work does include log-transformed versions of the proposed models. They are found to be not only consistent, i.e. independent of the underlying signal, but also additive and homoscedastic. They were not included in the original paper due to the space constraint, but we have added thes to the Appendix (and will include it in the final paper if the reviewer/editor deem it is necessary).

**Comment:**

**The various sample sizes should be given as well as the estimated L values.**

Answer:

The sample sizes for homogeneous patches of the AIRSAR and RADARSAT2 datasets are 50x50 and 300x300 respectively. The computed L values are: 3.2752 and 3.4241 respectively. This information has now been added in the revised article.

**Comment:**

**Section IV, in the multi-dimensional case, it is not clear what analytical relation similar to (26) was used to compute the model PDFs for (22) and (23).**

Answer:

The paper did not use (26) to compute (22) and (23). Rather, it shows that Eqns. (24) to (26) are a special case of Eqns. (22) and (23), where d=1. Thus, the proposed models for POLSAR encompass the traditional models for SAR intensity as its special case.

## Reviewer 2:

Thank you for taking time to make some useful and extensive comments. Hopefully with these clarifications the paper has been enhanced.

**Comment:**

**The authors do not show that this parameter is any better than others, such as span, except in the sense that its pdf is defined completely, not just asymptotically.**

Reply:

There are several advantages of the proposed models that have been mentioned in the paper, although not specifically highlighted as such. To clarify, the paper suggests that its proposed models for multi-dimensional POLSAR also encompass the traditional model for SAR as a special case (i.e. d=1). Moreover, its scalar observable leads to consistent measures of distance, which is something lacking in many other common scalar observables, such as span. Even better are the properties of these proposed consistent measures of distance. First, compared to existing measures of distance (section II.B) the proposed pdf is, as the Reviewer noted, defined completely and not just asymptotically. Second, extending from the widely used intensity-ratio in SAR, the determinant-ratio can be considered as its natural extension in the multi-dimensional case!

We have now further emphasized the above advantages in a rewritten Conclusion section.

**Comment:**

**They also do not acknowledge that much of the useful information in a polarimetric image is in the relationship between the terms of the scattering matrix.**

Answer:

The paper does acknowledge that the proposed models are NOT lossless (P7 L27), since they are scalar representation of a multidimensional dataset. For example, what may be lost includes the intra-relationship among the terms of the multidimensional data. We believe, however, that the loss is minimal in the class of possible scalar observables for POLSAR. There is some evidence for that: Firstly, the proposed scalar observables are representative, i.e. they lead to consistent discrimination measures. Secondly, when the multidimensional dataset itself is collapsed into a single dimension, the proposed model degrades smoothly into the widely accepted model for single-channel SAR. The revised text is now rewritten to give further emphasis to the points above, including to further emphasise that it is not lossless.

**Comment:**

**Other parameters they present … are ill-defined in how to put them to practical use.**

Answer:

The paper suggests that the proposed determinant, determinant-ratio or change-ratio models for POLSAR also include the traditional models for SAR intensity, SAR intensity-ratio and SAR change-ratio. Thus their usage pattern follows the practical use of these existing SAR models. Still, to clarify further, a new paragraph has been included which illustrates some different uses of the proposed models.

**Comment:**

**No comparisons with established procedures are made.**

Answer:

The paper focuses on proposing new statistical models for several scalar and representative observables for POLSAR. The topic is important, we feel, as many established techniques have been shown to be derived from similar discrimination measures. Since the focus of this paper is not to propose new procedures/applications, but on the measures themselves, we believe it is more useful to verify this directly.

The paper thus focuses on comparing the proposed models with existing models for both SAR and POLSAR, and their advantages are shown. For practical applications, the paper also includes a portion to illustrate how the proposed models can be useful. Even for this purpose, instead of normal “comparison with established procedures”, a higher-level approach is pursued.

Since the proposed determinant, determinant-ratio or change-ratio models for POLSAR also include the traditional models for SAR intensity, SAR intensity-ratio and SAR change-ratio, the paper is really showing important support for adapting existing SAR data processing techniques for use with POLSAR data.

**Detail Comment:**

**P1/Col1/Para2: The authors state without citation that existing models are “complex and unintuitive”. This statement needs validation … justification.**

Answer

POLSAR is multidimensional and stochastic. There have already been several attempts to model all elements of the multidimensional POLSAR data [LopezMartinez\_2003\_TGRS, Lee\_1994\_TGRS]. In comparison with the equations in our proposed model, their mathematical equations are evidently complex. These citations were already mentioned in the 'related work' section but have now been added into the specified location in the revised paper to substantiate the comments.

**Detail Comment**

**P2/Col1/L36-52: text is misleading. It says that 1-7 are statistical models, not parameters for which models have been proposed. p,q,r,s needs to be defined.**

Answer:

1-7 list are different univariate POLSAR observables, for which statistical models have been proposed in the cited publications (i.e. they are not models themselves). Since the writing was probably a little unclear at that point, we have revised the explanation.

p,q,r,s are notations which indicate any of the commonly used polarization combinations (i.e. hh,vv,hv). The explanation for this has also been added in the revised text.

**Detail Comment:**

**P2/Col1/L56-60: have been shown by whom? Citation required, or is this the authors’ opinion?**

Answer:

The text reads: “… none of the underlying observables have been shown to meet the dual criteria of (i) resulting in statistically consistent discrimination measures and thus (ii) being representative of the complex POLSAR data”. This is actually shown in the very next section.

The next section reviews all, to the best of our knowledge, widely used discrimination measures for POLSAR. None of those commonly used discrimination measures are based on the reviewed observables. Most of them actually are based on the determinant of the covariance matrix, whose model is among those proposed in this paper.

We have changed the tense of the very in the sentence to clarify the meaning.

**Detail Comment:**

**P3/Col1/L10-15: While it may be nice for mathematical purity to have an exact distribution instead of an asymptotic one, it should be demonstrated that the asymptotic assumption is invalid for POLSAR data. Ultimately, it needs to be shown that better separation of regions may be obtained using the proposed distribution than with existing methods.**

Answer:

First, we feel that having an exact distribution instead of an asymptotic one is an obvious theoretical contribution made by this paper. Also, while we show that the exact distribution is valid, that definitely does not necessarily lead to the conclusion that the asymptotic assumption is invalid for POLSAR data.

Second, while we believe that better separation can be achieved with the proposed model, we feel that it this a different topic. Due to space constraints, the paper tries to focus on a single topic – even with that focus there is ample potential for confusion. Furthermore, this theoretical results need to be established (published) first, before we or others can propose the techniques to make use of it.

Third, it should also be noted that the paper does include a section (comment: which one?) discussing the application and the advantages of the proposed models. One advantage, in terms of more easily classifying POLSAR data is, for example, the use of the change ratio, which evidently is much cheaper in computation than existing measures, such as the Bhattacharyya ratio.

**Detail Comment:**

**P3/Col1/L25: Incorrect nomenclature. Single pol transmit, dual-pol receive is “compact polarimetry”. Partially polarized signals contain both polarized and unpolarised power.**

Answer:

The term we used is “partial polarimetry” and it is totally different from “partially polarized signals”. We are aware of the term “compact polarimetry” which was used by Souyris [Souyris\_2005\_TGRS]. In the cited paper, the proposed mode is 45 degrees in transmit. This fact is important because there is also another proposal by Raney [Raney\_2006\_TGRS], termed “hybrid polarimetry”, where circularly polarized signals are transmitted. By “partial polarimetry”, we try to indicate that our model works not only on “full polarimetric SAR” or “traditional SAR”, but also covers both the above mentioned cases, and more. In fact, our paper validates the case where either the horizontally or the vertically polarized signal is transmitted.

**Detail Comment:**

**P3/Col2/Eqns 16, 17: Confusing notation or a typo? These are distributions. Is the exponent missing in the equations?**

Answer:

Yes, these are distributions. The exponent notation was used in the original paper to denote the dimensionality number (d), and we agree that this looked confusion. In the revised paper, the squared-exponent notation is re-used for the chi-squared distribution, to avoid this confusion.

**Detail Comment:**

**P3/Col2/L43-48: This paragraph is a circular argument. SAR speckle noise is multiplicative. You use Goodman’s result to capture this, and Eqn 19 … also does. It is not an implication. Note the Eqns 18 and 19 break down for L<d, i.e. for single look imagery.**

Answer:

To clarify, our intention is to show that the determinant of the POLSAR covariance matrix is not only multiplicative but also heteroskedastic. Subsequently we show that these properties are also similar to SAR intensity. It seems that the idea has not been fully conveyed by the original text. As such, in the revised paper, the text has been rewritten to emphasize and clarify the point.

Concerning Eqns 18, 19, it is true that they are broken when L<d. It should be noted that L stands for Number-of-Looks and d is the dimension number. Thus when L<d, the determinant is also ill-defined, as is the Complex Wishart distribution (Eqn. 13).

**Detail Comment:**

**P3/Col2/Eqn 20: why would the underlying covariance ever be known a priori? It is what we are trying to estimate.**

Answer:

The underlying covariance matrix may not be known a priori with absolute accuracy, but may be considered known to some degree of confidence. For example, detection of a ship in a “homogeneous” background of sea water. Hence, in that sense Eqn 20 may be useful.

From another perspective, this can also be considered as a purely logical advancement. In fact, in the very next sentence, the case of unknown underlying covariance matrix is covered.

**Detail Comment:**

**P4/Col1/Eqn 21: for this equation to work, regions must be known to be homogeneous. Almost no natural regions are …. Consequently, this parameter, while formally satisfying, may be useless in practice.**

Answer:

The paper shows that when the two underlying covariances are the same, Eqn. 21 is theoretically satisfying. Regarding its use in practice, however, we agree with the reviewer that it is dependent on our imagination.

However that does not mean it is useless. For example, given two observable covariance matrices, Eqn. 21 can be used to test the null hypothesis of the same underlying covariance. With some minor calibration, this technique can be applied in, for example, change detection applications. In essence, it means that the comment of: “for Eqn. 21 to work, regions must be known to be homogeneous” is not strictly true in all possible cases, although may well be so for the particular application interest of the reviewer.

**Detail Comment:**

**P4/Col1/Eqn 23: is wrong. With numerator and denominator the same, it has fixed values of Rc=1,2,6 for d=1,2,3**

Answer:

It should be noted that the Eqn. 23 indicates a stochastic process. The division of two stochastic variables having the same underlying distribution, in the general case, does not lead to a fixed number. It only leads to a fixed distribution.

This is similar to the subtraction of two independent random variables having the same, say, Gaussian distribution. The latter case does not lead to a fixed value of zero, but a fixed distribution with expected value of zero. What the reviewer probably meant is: when d=1,2,3, Rc follows fixed distribution with expected values.

**Detail Comment:**

**P4/Col1/L28-38: 1D SAR is not 3D SAR collapsed. Single pol is a single component of compact or full pol SAR.**

Answer:

We do not wish to say that 3D SAR can be physically collapsed into 1D SAR and we did intend that meaning. Instead, what the paper meant is that the proposed generic mathematical models for multidimensional SAR (i.e. d=3 for full-pol SAR, d=2 for part-pol SAR), when collapsed into single-dimension (i.e. setting d=1 into these mathematical formula), result in the traditional model for SAR intensity!

It means that the multidimensional models proposed in this paper are generic, and subsume the traditional SAR intensity models as a special case. This is one of the relationships that we feel lends support to the proposed models.

**Detail Comment:**

**P4/Col1/last Eqn: you did not set d=1 here**

Answer:

Thank you for pointing out the glaring mistake. It has been rectified in the revised paper.

**Detail Comment:**

**P4/Col2/top Eqn: Without clarification, I cannot figure out how the succeeding results are calculated.**

Answer:

Thanks for pointing out that. A separate appendix has now been attached to show the mathematical derivation.

**Detail Comment:**

**P5/Col1/L40: A Radarsat2 image of what?**

Answer:

This has been updated in the revised paper. It is a Radarsat2 image of Muda Merbok (Malaysia)

**Detail Comment:**

**Anfinsen’s ENL is, in general, non-integer. How did you deal with that? Why did you not use the formal L values from the imagery (1,4 or 9).**

Answer:

It is true that our computed ENL did result in non-integer results. There is no problem with that since Matlab can simulate Chi-Squared distributions with non-integer degrees of freedom so that the model distribution can be plotted using Matlab simulations. The paper shows that the Matlab simulation matches nicely with the observed data.

We did not use the formal L values from the imagery because the estimated ENL gives a better match. We decided not to discuss too much about why the estimated ENL gives a better match, or why the two differ since the paper is already too long. This is highly interesting but slightly off topic. Interested readers could refer to my thesis for further details – it will be online and freely downloadable by the end of the year.

**Detail Comment:**

**How did you determine that the regions you choose were truly homogeneous? How did you determine the theoretical determinant for the ratio test? Which other region was selected for the change test?**

Answer:

The determination of homogeneity is admittedly by experience. We choose regions that are calm water surfaces as homogeneous. These, for example, are large lakes or known calm sea areas.

Assuming the areas are homogeneous, the theoretical determinant is the determinant of the region’s ensemble average. The change test makes use of the same assumed homogeneous region, shifted by a few pixels.

**Detail Comment:**

**P6, 7/Section VII: You used Anfinsen’s ENL to compute the L value required to make the data appear homogeneous. It should result in an ENL image that is instructive. The ENL value should vary around the image, although the actual number of looks does not. Consequently, your noise images show no structure, because all the variance structure has been extracted sin the ENL computation.**

Answer:

There are a few points to clarify, we believe. First, ENL estimation is not applied in this section’s experiment. Second, we do not wish to show that the noise images show no structure. In fact, quite the contrary, we wish to show that the residual image of 5x5 boxcar filter shows more recognizable structures than the 3x3 boxcar filter. The point in this section is to show, as an example, that existing techniques in SAR can be readily extended to POLSAR using our proposed models.

## References

[LopezMartinez\_2003\_TGRS] C. Lopez-Martinez and X. Fabregas. **Polarimetric SAR speckle noise model**. *Geoscience and Remote Sensing, IEEE Transactions on*, 41(10):2232–2242, 2003.

[Lee\_1994\_TGRS] Jong-Sen Lee, K.W. Hoppel, S.A. Mango, and A.R. Miller. **Intensity and phase statistics of multilook polarimetric and interferometric SAR imagery**. *IEEE Transactions on Geoscience and Remote Sensing*, 32(5):1017–1028, Sep 1994.

[Souyris\_2005\_TGRS] J.C. Souyris, P. Imbo, R. Fjortoft, Sandra Mingot, and Jong-Sen Lee. **Compact polarimetry based on symmetry properties of geophysical media: the pi/4 mode**. *Geoscience and Remote Sensing, IEEE Transactions on*, 43(3):634 – 646, Mar. 2005.

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