Reply To Examiners

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Reply to Examiner No. 1

Name of Student:	Le Thanh Hai
Degree:	Doctor of Philosophy
Thesis Title:	Scalar & Homoskedastic Models for SAR & POLSAR data

First, I would like to express my appreciation towards Examiner No. 1 for his **over-all positive comments**. I also appreciate the fact that Examiner No 1 includes not only his comments in the examiner's report but also helpful suggestions inside the original thesis. There was no direct question in the report, and followings are my replies to his comments in the returned thesis.

In page 14 of the returned thesis, Examiner No. 1 wonders "Does the intagration of the PDF given for A_x, A, I sum up to 1?". First I would like to thank Examiner No. 2 for raising such a question. By going through his question, I realized that a negative sign were missed in the equation for $pdf(A_x)$. With that corrected, let me repeat the Equations here

$$pdf(A_x) = \frac{1}{\sqrt{\pi}\sigma} e^{-\frac{A_x^2}{\sigma^2}} \tag{1}$$

$$pdf(A) = \frac{2A}{\sigma^2} e^{\frac{-A^2}{\sigma^2}} \tag{2}$$

$$pdf(A_1) = 2A_1 e^{-A_1^2} \tag{3}$$

$$pdf(I) = \frac{1}{\sigma^2} e^{-\frac{I}{\sigma^2}} \tag{4}$$

The integration of these becomes

$$cdf(A_x) = \int_{-\infty}^{\infty} \frac{1}{\sqrt{\pi}\sigma} e^{-\frac{A_x^2}{\sigma^2}} \, \mathrm{d}A_x = \frac{1}{2} erf\left(\frac{A_x}{\sigma}\right)\Big|_{-\infty}^{\infty} = 1 \tag{5}$$

$$cdf(A) = \int_{0}^{\infty} \frac{2A}{\sigma^2} e^{\frac{-A^2}{\sigma^2}} \, \mathrm{d}A = -e^{\frac{-A^2}{\sigma^2}}\Big|_{0}^{\infty} = 1 \tag{6}$$

$$cdf(A_1) = \int_{0}^{\infty} 2A_1 e^{-A_1^2} \, \mathrm{d}A_1 = -e^{-A_1^2}\Big|_{0}^{\infty} = 1 \tag{7}$$

$$cdf(I) = \int_{0}^{\infty} \frac{1}{\sigma^2} e^{-\frac{I}{\sigma^2}} \, \mathrm{d}A_1 = -e^{-\frac{I}{\sigma^2}}\Big|_{0}^{\infty} = 1 \tag{8}$$

For the first equation, consider the standard PDF equation for normal distribution centering around μ and having variance σ given below

$$\textbf{Normal Distribution: } pdf(x) = \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{(x-\mu)^2}{2\sigma^2}} \tag{9}$$

$$\textbf{Chi-Squared Distribution (dof=2): } pdf(x) = \frac{e^{-x/2}}{2} \tag{10}$$

$$\textbf{Exponential Distribution: } pdf(x) = \lambda e^{-\lambda x} \tag{11}$$

$$\textbf{Normal Distribution: } cdf(x) = \frac{1}{2}\left[1 + erf\left(\frac{x-\mu}{\sqrt{2\sigma^2}}\right)\right] \tag{12}$$

$$\textbf{Chi-Squared Distribution (dof=2): } cdf(x) = 1 - e^{-x/2} \tag{13}$$

$$\textbf{Exponential Distribution: } cdf(x) = 1 - e^{-\lambda x} \tag{14}$$

Clearly, A_x follows the normal distribution with expectation $avg(A_x) = 0$ and variance $var(A_x) = \frac{\sigma}{\sqrt{2}}$.

Next consider the standard PDF equation for chi-square distribution with k degree of freedom given below:

$$pdf(x) = \frac{x^{\frac{k}{2}-1} e^{-x/2}}{\Gamma(k/2) 2^{k/2}} \tag{15}$$

Set $k = 2$, and thus $\Gamma(k/2) = \Gamma(1) = 1$, then:

$$pdf(x) = \frac{e^{-x/2}}{2} \tag{16}$$

Set $\frac{x}{2} = \frac{A^2}{\sigma^2}$ or $x = \frac{2A^2}{\sigma^2}$, thus $\frac{dx}{dA} = \frac{4A}{\sigma^2}$. Variable change theorem give us:

$$pdf(A) = \frac{e^{-A^2/\sigma^2}}{2} \frac{4A}{\sigma^2} = \frac{2A}{\sigma^2} e^{-\frac{A^2}{\sigma^2}} \tag{17}$$

For the $pdf(I)$, if we set $\lambda = \frac{1}{\sigma^2}$ then the equation turns out to be the standard PDF equation for the Exponential Distribution

$$pdf(x) = \lambda e^{-\lambda x} \tag{18}$$

Reply to Examiner No. 2

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Examiner No. 2 suggests to further emphasize the significance of the research motivation in **Section 1.1 and Section 1.2**. In a similar fashion, the conclusion chapter should also include the research objectives. These have been updated in the revised thesis. Even though I would wish to note that the research objectives have been spelled out in the Introduction chapter. The examiner also suggested to change the word "theory" to "model". That has also been applied through out the thesis.

The second main comment is to further highlight the novelty claim, which can be splitted into two parts.

In the first part, the examiner appears to discount the novelty claim by suggesting that: "the proposed model is derived based on the existing statistical models for SAR and extended to POLSAR". The main approach, in actuality, differs in a subtle but important way: The proposed model is derived based on generic mathematical results for multi-dimensional random-walk [Goodman, 1976, Goodman, 1975]. These results are well known to be applicable to POLSAR. Thus, the derived models by nature are multi-dimensional model. However, when collapsed into single-dimension, they match perfectly with existing SAR models. As such, the proposed models can be considered as an extension of existing statistical models for SAR.

In the second part, the Examiner expresses his doubt in "the use of MSE (mean squared error) as the single unified evaluation criteria". His main concern: "the proposed log-transform model will introduce an inevitable bias error". It is no doubt tricky to evaluate evaluation criteria. And I can empathize with his feelings. Still, I wish to point out that MSE evaluation inherently includes bias evaluation. In fact it has two components: 1. bias evaluation and 2. variance evaluation, which for SAR speckle filter evaluation translates into 1. radiometric preservation and 2. noise suppression respectively.

The Examiner also suggestion the inclusion of several particular references into **the literature review**. I really appreciate the examiner effort in pointing out specific references [Argenti et al., 2013, Lee et al., 1994, Cetin et al., 2000, White, 1994, Sattar et al., 1997, Wang et al., 2004, Nielsen, 2012]. They, and some other articles, have been included in the thesis.

To conclude, I appreciate the fact that the Examiner is able to a summarize the long thesis into a single sentence. Also, portions of this thesis has been extracted out as scientific articles and submitted for peer review. Various errata exposed by Examiner No. 2 and have also been updated in the thesis. Last but not least, I wish to express my appreciation towards Examiner No.2 for his careful and detailed review.

References

[Argenti et al., 2013] Argenti, F., Lapini, A., Bianchi, T., and Alparone, L. (2013). A tutorial on speckle reduction in synthetic aperture radar images. *Geoscience and Remote Sensing Magazine, IEEE*, 1(3):6–35.

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[Nielsen, 2012] Nielsen, F. (2012). K-mle: A fast algorithm for learning statistical mixture models. In *Acoustics, Speech and Signal Processing (ICASSP), 2012 IEEE International Conference on*, pages 869–872.

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Reply to Examiner No. 3

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Since Examiner No. 3 presented his comments and suggestions in the order of the thesis' chapters, I would follow his suite.

1. Overall Organization and presentation

Examiner No. 3 noted: "The overall organization of the thesis is appropriate". Still minor editions have been included to address his other concerns:

- **Page ii of the Contents section is missing:** This were probably an administrative mistake and oversight from us. Please accept our apologies!
- **Usually, a Glossary is put at the end of the document:** Updated (now the Glossary section stays at the end of the thesis)
- **Some page number missing:** Updated.
- **List of source codes:** Updated (now "List of Pseudocodes" as suggested.)

2. Abstract

The abstract has been updated so that:

a. Each of the proposed models have their own name and
b. Not only the benefits of the proposed models are listed, but so are their shortcomings.

3. Chapter 1

The examiner suggests that "Table captions appears at the top of a table, rather than below it". This suggestion has been updated in this chapter, as well as through out the whole thesis. Also references are included to increase the persuasion power of the claims. The list of publication is also now included in this chapter, in addition to the existing one found in the final chapter.

At this juncture, I would also wish to address one of Examiner No. 3's main concern. He wonders: "What makes you think that this model is appropriate?". Before the answer is laid out , I wish to make a few points. First, I thank the Examiner to raise this question, which no doubt is an important one. Second, while it is important to judge the end result, I am not quite sure that should be done at the very first chapter. In fact, a large portion of the final chapter is devoted towards such a discussion.

To directly address the Examiner's concern, let me iterate a few points that has been noted in the thesis. First the generic model proposed in this thesis is applicable to multi-dimensional POLSAR data. Second, when the multi-dimensional data is collapsed into one dimensional, the specific case of the proposed model matches perfectly with the model proposed for SAR by Arsenault [Arsenault and April, 1976]. Thus, I believe, the proposed generic model is at least as appropriate as the widely-accepted model cited above.

4. Chapter 2

For Chapter 2, Examiner No. 3's suggests: "Perhaps the theory section can be moved to a separate chapter, so that more space can be dedicated to the discussion of related work". In a bid of not confusing other examiners, however, I have decided not to move the theory section into a separate chapter. Still, this chapter is heavily updated so that:

a. Section 2.1 is shortened considerably and
b. Section 2.2 is extended significantly.

This I believe leads to a better balance between describing the nature of the data and reviewing the related work.

5. Chapter 3

There are apparently two main concerns from Examiner No. 3's comments. The first is: "how can the statement on page 44 line 1 ... be true?". And second: "why homoskedasticity is valid, and what are the assumption in this analysis?"

First, the statement in page 44 line 1 should be intepreted strictly in the context of what preceeds it. That is mathematically speaking σ is just a mathematical constant, which makes the statement true. Any resemblence of it with the SAR signal is only established in the next paragraph onwards. In fact, the third line of the second paragraph in page 44 clearly indicated where σ can be considered as constant (i.e. spatial homogeneous area or at each resolution cell). This is the assumption used in this analysis. The assumption is acknowledged in the very next sentence, as the Examiner No. 3 pointed out precisely: "Over heterogeneous area, ... σ varies significantly ...". If σ is not a constant, then heteroskedastic ensues. The next section (i.e. Section 3.2) then outlines the effects of heteroskedasticity, which explains the needs for logarithmic transformation.

The assumption described above continues to hold in Section 3.3 and, of course, table 3.3. Under this condition, the variances of the log-transformed variables are then independent of σ. The point being made in the table is that: while variances are dependent on σ in the original domain, in the log transformed domain, they are independent of this value.

The assumption actually is not very restrictive. It should be noted that at the level of each radar physical resolution cell, the value measured in SAR is not deterministic. For all practical purposes, the single measured value is to be considered as the result of a stochastic process, with one "true" signal σ. This model actually is applicable to both homogeneous and heterogeneous areas.

Now, if the area is heterogeneous, then σ is no longer a constant. In fact, as reviewed in Chapter 2, there are many different ways to model σ which in turns leads to many different models for the observable magnitude. This leads to heteroskedasticity, which the Examiner correctly pointed out "$var(L_A)$ and $var(L_I)$ cannot be homoskedastic", should we consider "$var(L_A)$ and $var(L_I)$" as the variation of the observables in an area. However, if we consider "$var(L_A)$ and $var(L_I)$" as the deviation of the observables from corresponding its "true" signal at each physical resolution cell level, then they will be independent of σ (as described in Table 3.3). In fact they are constant, even as $sigma$ changes. This leads to homoskedasticity!

The second point concerns the models and their names. This chapter has been updated and model names are given for each to clarify the concepts. The confusion is understandable, and partially my fault, in not highlighting the following points. First there are different models being proposed, in Section 3.4. And second, they are all however derived based on ONE basic model: the one based on "the base-2 log-transformation of the SAR data." Finally, there are differences in each of the models, even though, whether these differences are significant or not is, to be honest, rather subjective at my current state of understanding.

6. Chapter 4

Examiner No.3 expresses his doubt on two points. First, that "the determinant $|\Sigma|$ will be very small, leading to a very narrow PDF in Equation 4.1". And second that Σ^{-1} is ill-defined. I wish to highlight that Equation 4.1 is the PDF for the circular complex Gaussian distribution, which is widely used in POLSAR. Its form, as repeated from equation 4.1, is written as:

$$pdf(s;\Sigma) = \frac{1}{\pi^d|\Sigma|} e^{-s^{*T}\Sigma^{-1}s}$$

This equation is also ill-defined where $|\Sigma| = 0$, which is also the only time that Σ^{-1} is "ill-defined". In other words, the models proposed has the same assumption and validity with the widely-accepted circular complex Gaussian distribution model. In POLSAR, $|\Sigma| = 0$ most commonly happens when the dataset is in Single-Look format. This restriction is clearly stated in the sentence that follows Equation 4.1: "the covariance matrix is only defined on multiple data-points".

It is partially for the second concern that log-transformation is proposed. Since the original domain is multiplicative, the range of small values is, as also observed by Examiner No. 3, commonly found but extremely limited ($|\Sigma|$ ranges from 0+ to 1). Log transformed domain not only changes the nature from multiplicative to additive, but also give this "small" range $(0, 1)$ a much widely space $(-\infty, 0)$. In other words, it helps to expand the "narrow" distribution depicted in Equation 4.1 (when $|\Sigma|$ is small) to become another distribution whose shape does not dependent on $|\Sigma|$, as depicted by the Equation below.

$$pdf(\ln|\langle ss^{*T}\rangle|, \Sigma) = \ln|\Sigma| - \ln(2L)d + \sum_{i=0}^{d-1}\Lambda(2L-2i)$$

The Examiner also expresses his concern about the quality of the "visual match" on page 66. For this, I had updated the section to include a quantitative, and hence objective, measure of match instead of the current subjective evaluation. I believe, such a change should completely addresses his concerns. I also made other small updates, to addresses his other various minor concerns.

7. Chapter 5

Page 94, section 5.2.2.2 is updated to indicate MSE as "Mean Squared Error". The reference for computation in the section is the "true signal" σ.

Fig. 5.11 shows two curves that are essentially the same. One of them is "simulated result" and the other is "analysis formula". The difference is that the former is computed through a Monte-Carlo simulation and the other is a simple plotting of the mathematical calculated values. The purpose is to show that the heuristic formula given as

$$MSE = \frac{1}{(ENL - 0.5)\ln^2(2)}$$

closely tracks observable values. Readers should also note that this argument is presented in a much more detailed manner in Section 5.3.2.1.

Examiner No 3 also expresses his curiosity to see "how the f-MLE filter will compare with other state-of-the-art-filters". While, I am also eager to see, and to some extend to prove, the good results of my proposed f-MLE filter, I have decided not to include it in the section 5.3, where the performance of many different filters are reviewed. There are a few reasons for such a decision. First, in the section I am proposing a new way to evaluate speckle filters. And instead of at the same time discussing the performance of f-MLE filter, I wish to focus on that topic exclusively. Second, assuming the performance of f-MLE filter is included and found to be superior than others, such a result can be easily refuted. That is because I was proposing both new speckle filters and a new approach to evaluate speckle filters, which I wish to, as much as possible, keeping them independently of each other. Last but not least, as noted by all Examiners and myself included, speckle filtering is not the main topic of the thesis. It is only the avenue to demonstrate the benefits of the proposed models for SAR & POLSAR data of the thesis.

8. Chapter 6

In this chapter, the Examiner wonder: "if the proposed models are far from complete, how can it be an accurate representation of the data". To clarify, the proposed models in this thesis do not aim to become accurate representation, or fully representative, of the data. Rather, they are proposed as highly representative, probably the most representative scalar models, for the multi-dimensional data. They are, however, useful as scalar models are needed quite often, when scalar decision are to be taken, for example: what is the best speckle filter for the given data set?, or what type of surface does the region of interest belong to? ...

The Examiner's next question center around this very point: ".. definitely not full representative of the data". He went on to ask: "What kind of eror will it cause?". To this the answer is that the model inherently suffers from loss of dimension where the full data is multi-dimensional and the proposed models are scalar.

Besides these main points, the chapter is updated to increase its pursuasive power. For example: the sentence in line 1, page 133, section 6.3 is removed. Or a list of achievements has been included in this chapter.

9. Appendix

While the conference papers have been published and the journal paper submitted, their copies have been removed from this section.

References

[Arsenault and April, 1976] Arsenault, H. H. and April, G. (1976). Properties of speckle integrated with a finite aperture and logarithmically transformed. *Journal of the Optical Society of America*, 66(11):1160–1163.