RSN-2014-0174
Evaluation of SAR Speckle Filters Using Mean-Squared-Error Criteria on Log-Transformed Data
Comments

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1 Comments

Rather than a "Review" paper, as stated in the "Manuscript Type" field, this manuscript is presented as an original contribution. The authors should correct this.

The Introduction makes a nice presentation of the problem at hand. The authors may find useful these additional references:

- The works by Vandewalle et al. (2009); Koenker & Zeileis (2009); Yale Law School Roundtable on Data and Code Sharing (2010); Sandve et al. (2013) regarding Reproducible Research, and

- Moschetti et al. (2006), for the discussion and proposal of a protocol based on Monte Carlo simulation, i.e., for the difference between repeatedly-run simulations with different simulated input data vs. single-run results.

As a general recommendation, the authors should state clearly their contribution once, and then compare it, stressing its advantages and disadvantages, with classical models and techniques. In its current version, one reads too many times that the proposal consists of using the homomorphic domain (a technical and precise denomination which appears too late in the manuscript) and the Mean Square Error. The same holds for the discussion of the validity of the Gauss-Markov theorem.

The assertion "should reduce the variation of the additive noise (i.e. speckle suppression power)" may be misleading, since speckle, in its natural domain, is not additive.

The notation deserves clarification. One infers that X_{est} and X_{obs} are, respectively, the estimated (filtered) and observed data, but then in the same page the authors discuss the use of the equivalent number of looks, and they define it as the ratio of the (undefined) mean to the (also undefined) variance of the (most markedly undefined) intensity. This is confusing. Amplitude and Intensity formats should be defined beforehand, and since both observed and filtered data are everywhere in the manuscript, the authors must define a notation for them and stick to it.

When discussing homogeneity, the authors should be more specific. Are they referring to texture? If so, Gao (2010) provides a comprehensive survey on distributions for SAR data.

The authors' criticism on the use of features for filter evaluation are pertinent, but it should incorporate the fact that most of the cited references employ a single image for such purpose. Such weakness could be removed using the approach presented by Moschetti et al. (2006).

Explicitly state the parametric space and the support whenever presenting probability density functions, as in equations (1), (2) and (3).

What is the difference between the multiplicative and the product model? This reviewer understands that there is no difference at all, so "in various SAR models including the multiplicative model [12] and product model [21]." is misleading. In fact, the multiplicative model explains all levels of texture (from textureless to extremely textured data); cf. Frery et al. (1997) and the numerous more recent articles that cite this work. In fact, this a major weakness of the manuscript: an insufficient, outdated and somewhat shallow discussion of the underlying model. This shortcoming becomes evident when one reaches Table 1; these results are trivial, and should not belong to a research paper. Also, equations (6) and (8) add nothing to the manuscript; the same holds for equations (10) and (12). Prolonging the discussion of both the standard and the scaled models renders an excessive and confusing text.

What are “normal images”?

This reviewer does not agree whit the assertion that “by definition, heteroskedasticity leads to inconsistent measures of distance.” Distance measures between distributions form a consolidated research area with strong intersection with Information Theory. Among the many results for SAR data, Nascimento et al. (2010) present a class of distances between models able to incorporate extreme texture, i.e., heterokedastic. That work also presents a classification procedure based on those distances. Also, the connection of least square regression models with the discussion is unclear, as is the validity of the assertion that heteroskedaticity impairs such regression analysis.

The authors omit that the core Amplitude and Intensity models they discuss are valid only for the single-look case; this should be clearly stated, and the implications of such hypothesis discussed. Multilook processing appears after Table 3, but without a proper definition and an adequate connection with the models. At this point surfaces again a discussion involving regression and, now, wavelets and neural networks.

The authors claim that the log-transformed means are biased; this is not correct, unless they refer to the sample mean as estimator of the expected value. Imprecise assertions like this one impair the technical quality of the manuscript.

The presentation of Figure 1 just before Section 3.4 is not adequate for the following reasons:

- No information is given about the image and the samples. In particular, sample size, equivalent number of looks (and how it was estimated), analysis of spatial correlation (and decorrelation technique, if needed) are essential.

- Which are the estimated parameters used for the density? How were they obtained (maximum likelihood? moments?) Was estimation performed solely in the original data domain?

- A visual comparison is not enough to attest to the correspondence between model and data; there are numerous statistical tests that can be used for that purpose, being the Kolmogorov-Smirnov and χ^2 some of the most popular ones.

- Even if a visual agreement would be enough, there is not such agreement in this figure; the difference between the histogram and the density is noticeable and, quite likely, significant at any sensible level of confidence.

- Asserting that "An excellent correspondence is evident between the distributions in the graph, confirming the log-transformed model." may be misleading unless estimation was performed in the transformed domain.

Equations (14) to (17) are unclear, as one does not know the meaning of the *avg* operator. It is possible to suspect from the context that this is the expected value, although it is different from the average (in finite samples).

Consistency has a precise meaning for the statistical community. Please check, for instance, the book by Wassermann (2005). The authors should adhere to it, and look for another wording for their discussion.

The variable C_{L^I} is the logarithm (base 2) of the ratio of two intensities, as defined in (23). It, therefore, spans the whole real line. According to (25), its probability density function is $2^t(1+2^t)^{-1}\ln 2$. This is a logistic function and, therefore, it is not a density on the real line.

2 Recommendation

I stopped reading the manuscript at the end of Section 3.4. I found too many errors to keep analyzing the text.

The title and abstract left me enthusiastic about this contribution, but the authors failed at conveying it in an adequate manner.

3 Minor observations

Many citations are old conference articles. The literature on this subject has evolved dramatically, and most, if not all, of them could be replaced by more updated material published in peer-reviewed Journals.

References must be carefully checked and edited to remove problems such as "sar" instead of "SAR".

Use `\ln` in LaTeX to produce $\ln(I)$ instead of $ln(I)$. Also, avoid unnecessary parentheses, so prefer $\ln I$ to $\ln(I)$.

Correct "Then logarithmic transformation is then shown".

References

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