Dear Mr. Hayashi,

Thank you for submitting your work to the IEEE Transactions on Biomedical Engineering (TBME). Your manuscript "A Variance Distribution Model of Surface EMG Signals Based on Inverse Gamma Distribution" (TBME-00661-2015) has been carefully reviewed by the TBME editorial review board. Based on the reviewers comments, I regret to communicate to you that we cannot accept your manuscript for publication at the current form.

If you think you can conduct significant additional work (experiments or computation) and make significant revision to address the major critiques of the reviewers, your resubmission may be reconsidered. Simply rewriting or revising the manuscript without substantial additional data is considered non-responding.

If you choose to resubmit, please submit a cover letter in the text box provided when you resubmit, referencing to this submission (manuscript number), and indicating clearly what **\*additional substantial data\*** you have obtained since the previous submission. Please also submit, as a supporting document, a detailed statement on how you have addressed each of the reviewers' critiques with changes marked in color in your revised manuscript.

Please be advised that the resubmission would be reviewed by the same reviewers if possible, although additional reviewers may be added as needed. Given that the current acceptance rate of TBME is about 20%, the resubmission will be rejected if the resubmission still does not satisfy the reviewers. So please consider carefully if you should resubmit before committing to such a major task. In order for us to be able to locate the same reviewers, please resubmit within 6 months if you choose to do so.

\*\*\*Please carefully consider if the title of your manuscript is appropriately worded so as to appeal to the broad BME readers with a concise and informed article title. Please make a structured abstract (objective, methods, results, conclusion, significance) of no more than 250 words, to highlight problem being addressed, innovations, critical findings and its significance to biomedical research or clinical applications. Please include a Conclusion section of no more than 300 words, to highlight the major findings and significance. Please reformat the reference list (list only the first author if there are 3 or more authors: A. Author et al, ....). You can see the new format requirements at the following: <http://tbme.embs.org/for-authors/>

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Thank you again for submitting your work to the IEEE Transactions on Biomedical Engineering.

Sincerely,

Bin He

Editor-in-Chief

IEEE Transactions on Biomedical Engineering

Associate Editor comments

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Comments to the Author:

The reviewers found merit in the study but all of them suggested anyway extensive revisions. The main concerns are related to a poor discussion on the assumptions and limitations, lack of clarity in some of the analytical derivations, and, especially, problems in relation to the experiments (not all results shown, relatively small number of subjects, etc.). The revisions requested are too extensive for proceeding but a resubmission may be considered, due to the general positive consideration of the work by the reviewers.

Reviewer's Comments

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Reviewer: 1

Comments to the Author

The manuscript proposes to model the variance distribution of surface EMG signals using inverse gamma distribution and Bayeisan estimation. The model allows the representation of noise superimposed onto variance therefore avoiding the constraint embedded in the conventional maximum likelihood (ML) method, which assumes that variance is constant. The simulation results illustrate that the proposed method outperforms the maximum likelihood method with respect to estimation of average of variance and variance of variance. Also, the experiment on real EMG signals reveals that can express noise superimposed onto variance depending on muscle force. Therefore, the manuscript includes several merits but needs to address the following comments before publication.

1. In lines 21-22 of column 2 in page 2, the authors describe that the P(x\_t) must be a constant. This makes the reviewer confused. According to equation (5), P(x\_t) should be a probability density function. The authors need to explain this point carefully.

2. The equation (12) and equation (17) seem to be conflict with each other. According to equation (12), when we have a segment of EMG signals, we obtain different estimation of average of variance at different time point t. That is, if the length of the EMG signals is L, we will have L estimations. However, according to equation (17), we only obtain a estimation for a segment of EMG signals due to the expectation operator, E〖[y\_t]〗^2. Maybe there is a mistake in equation (12), the authors should carefully check it.

3. The authors only show the experiment results based on the rectified and smoothed signals, i.e. based on the equation (17). Can the author also show the results based on raw EMG signals? i.e. based on the equation (12). We need to compare these two types of result to verify the necessity of rectification and smoothness of EMG signals. If we also can obtain excellent results with raw EMG signals, such preprocessing of EMG signals will be unnecessary.

4. In lines 11-12 of column 2 in page 3, why the scale parameter is tuned and the shape parameter is fixed to obtain the average percentage error? Why do not tune the shape parameter?

5. In Fig. 3, Fig. 4, Fig. 5 and Fig. 9, the meaning of the error bar in each figure is suggested to be added in the figure caption.

6. In the experiment of real EMG signals, the advantage of the proposed method comparing with the ML method should be illustrated. For example, does the proposed can outperform the ML method with respect to the muscle force estimation? In order to comprehensively demonstrate the effectiveness of the proposed method, the muscle force estimation should not be the future work because the experiment data were already collected. The author needed to present the advantage of the proposed model with the real data. As both models were to describe the relationship between force and EMG signals, this review suggests computing the variance using the MLE method and comparing the accuracy of distinguishing different force level of two methods.

7. In the simulation experiment, the raw signals were first rectified and smoothed, and then down sampled. This review did not understand why put the rectification and smooth process before down sampling. People usually set different sampling frequency to record the signals, and then do rectification. So this review suggested that the down sampling should put before the rectification and smooth.

8. There were three different signals generated in the simulation experiment. Is there any difference for the performances of both models on these signals?

9. In fig 4, the error rate of the MLE method is lower than the proposed method when L was 1000 and 500. It seemed that the MLE method performed better than the proposed method when L was big. How about the results when L was bigger than 1000?

10. Five subjects were not enough for a journal paper. More subjects should be tested in the EMG Analysis part.

Reviewer: 2

Comments to the Author

I would like congratulate the authors for the clarity in presentation of their work. It made the manuscript very easy to read and understand.

I have a few suggestion to further improve your manuscript:

1. Eq. (5): Following the work of Hogan, you have modelled the EMG with a Gaussian function. Earlier work (e.g. <http://www.ncbi.nlm.nih.gov/pubmed/23047056> as well as Clancy's work [12]) has shown that the EMG at low contraction levels has a Laplacian distribution as as the force increases, presumably because more motor units fire, the distribution becomes a Gaussian. How does this finding affect your modelling. Please cite and discuss this in relation to model.

2. Please discuss your finding in terms of signal dependent noise in EMG in more details. You have [20] in your references but the discussion around it is very limited to two lines in the Intro. This is a major piece of work and your results must be discussed in that light, although you discuss [21] a bit too.

3. How does your new model track a changing force? Please include simulation (and ideally real) data showing your algorithm can track ramp and half-sine force levels. I think if the detrimental effect would be larger for large force levels.

4. Page 3, section 3 where you have all the %s. It is so difficult to follow and you have the same results in figure 3 and 4 anyway. Please update this section.

Reviewer: 3

Comments to the Author

General comments

This study presents a model to describe the distribution of the variance of the surface EMG signal. The EMG signal is represented as Gaussian signal, with noise additive to the variance of the signal. The paper follows the general model proposed by Hogan and Mann some time back. The performance of the new estimation method is examined for rectified, smoothed and down-sampled synthetic signals with -different window lengths and down-sampling rates. The results suggest that the proposed method can be used to estimate the signal variance, and performs better than the maximum likelihood method for small numbers of samples.

A main feature of the model is the addition of noise to the variance of the signal. What is the rationale for adding noise to the variance of the EMG signal in this way and what effect does the representation of noise within the model have? More usually noise, with a given variance, is considered to be added to the EMG signal itself, representing the contribution of the various noise sources to the voltage recorded.

If the magnitude of the noise is large, adding noise to the variance of the signal would presumably result in EMG signal variance with lower variance itself, when compared to a case where noise is added directly to the signal? The reasoning seems to be an underlying assumption that the noise is proportional to the force. This should be more fully explained, and the justification and consequences of the assumption outlined. What effect would this have on the estimated posterior variance presented in Fig. 9, for example?

The simulation results indicate superior performance of the proposed estimation method for small window lengths and low ‘down-sampling’ frequencies, when compared with the maximum likelihood method. These results are based on a phenomenological model, and upon assumptions regarding the distribution of the signal which are also inherent in the estimation method. The findings would be strengthened if similar findings were observed for the experimental EMG data. Though the ‘true’ value of the signal variance is not known for the experimental data, a divergence of the behaviour of the proposed estimation method and the maximum likelihood method for low window lengths and at lower frequencies of down-sampling would support the simulation results.

The structure of the paper would be simplified by moving all of the Results into one section, and similarly the Discussion into a single section, rather than splitting them across multiple sections. Related to this, a discussion of the main model assumptions and limitations is missing from the paper and should be included.

A discussion of the reasons for the better performance of the proposed variance estimation method when compared with the Maximum likelihood estimation method should be included.

Some of the figures are very small in size and difficult to read.

Specific comments

P. 1: For the force-EMG relationship, in addition to reference [12] see also classic references from Milner-Brown and Stein (1975), Woods and Bigland-Ritchie (1983), and Lawrence and De Luca (1983).

p. 2, line 38: How is the value of alpha set?

Units e.g. ms, Hz etc. should not be included in square brackets []

P. 3, line 7-8. Some further explanation of the ‘empirical’ method by which the hyperparameters are chosen should be included in the main text, even if the method details are included in the Appendix.

P. 3, Results: How was the ‘average error rate’ defined?

P. 4, line 60: ‘These results indicate... EMG signals’. As the performance of the method has been assessed here for simulated data based upon a specific model with certain assumptions, this sentence should be qualified to say for ‘simulated EMG signals generated using a model...’

P. 4, EMG Analysis: Some further details of the EMG recording methods are needed. What was the inter electrode distance, filter settings, electrode position etc. How was the weight attached?

What instructions were provided to the subjects? Were they instructed to lift their wrist with the weight above the desk? If so, by how much and how was this controlled?

P 5, line 22: a more thorough explanation of how the muscle force was estimated is required, with the corresponding equations.

P5. Results and Discussion: What window was used to smooth the data presented in Fig. 7?

A more comprehensive discussion of the method, assumptions, limitations and results is needed.

NOTE: Corresponding Author MUST check to see if additional comments were included as a downloadable file in the corresponding author's Author Center by:

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