Reply to McClelland et al.: EMG rectification and coherence analysis

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REPLY: We thank McClelland et al. (2014) for their interest in our paper. They call their letter relating to our paper “EMG rectification has inconsistent effects on coherence analysis even in single motor unit studies.” This title implies that one would use electromyogram (EMG) rectification in a single motor unit study. We are sure this is not the intended meaning; however, such a title in itself is a source of confusion. The study that they are critiquing simultaneously recorded single motor units with surface EMG and used partial coherence analysis to establish whether the coherence between single motor unit pairs was best represented by the raw or the rectified EMG (Ward et al. 2013). In defense of our approach, we are forced to comment robustly on the individual points raised as “problems” with our work by McClelland et al. (2014); we also comment on the conclusions that they have drawn from our paper and other papers that are important in the field.

First, as careful reading of our article makes clear, we concentrate on low-force contractions and do not claim any generality or extrapolation of our results. We will, however, return to the issue of motor unit recruitment later.

Second, we suggest that the selective reading of papers, i.e., ours and that of Farina et al. (2013), by McClelland et al. (2014) is unwise and scientifically unhelpful. Ward et al. (2013) represents, to the best of our knowledge, the first attempt to compare directly using actual EMG and motor unit data the effects of rectification on frequency domain synchronization detection. In the paper, we were careful to point out that this is a low-load situation. In the paper, we report that with increasing load the differences between rectification and non-rectification lessened; importantly, at no point was rectification inferior or less robust nor was there inconsistency as implied by McClelland et al. (2014).

As indicated by McClelland et al. (2014), the modeling section of the paper of Farina et al. (2013) is important. In this paper, the effects on signal transmission of phase cancellation of simulated motor units are studied. Motor unit phase cancellation increases with force-dependent increased motor unit recruitment. In their paper, Farina et al. (2013) state that the “no-cancellation” condition represents the optimal state for detecting oscillatory drive to motor unit pools (see their Fig. 2). They then explore for different common input frequencies (10, 20, and 30 Hz) the signal-to-noise ratio (SNR) of the transmission of the spectral peaks within the neural drive to those within the EMG where the EMG is no-cancellation, rectified, or raw. Their Figs. 3 and 4 illustrate two very important features of the model. First, as can be seen from Fig. 3, the SNR for low numbers of motor units (<10) is nearly 100% for rectified and no-cancellation EMG. In contrast, the SNR of the raw EMG is 20–40%. Interestingly, the SNR of the rectified and the no-cancellation condition both decline with increased motor unit number, whereas that of the raw EMG condition is by comparison flatter. At input frequencies of 10 Hz, the SNR always favors rectification over raw. At input frequencies of 20 and 30 Hz, the crossover point where the SNR favors the raw signal over the rectified is approximately 60 and 30 motor units, respectively. Figure 4B of Farina et al. (2013) is of particular importance to the EMG rectification debate because in this the input-output coherence is calculated. In this figure is shown the coherence between the input signal and the three EMG conditions, and the coherence magnitude is expressed as a function of the number of motor units recruited. In such a situation, a good consistency of coherence between presynaptic input and the EMG output with changes in motor unit recruitment will reflect the most stable and therefore the most reliable estimate of coupling between the input oscillation, 20 Hz in this instance, and the EMG. As can be seen, in the range 5–260 motor units, the 20-Hz coherence between the input and rectified EMG is in the range 25–43% and attains a very stable value of \( \sim 35\% \) for >50 motor units. In contrast, the raw signal shows a large (20–65%) range of coherence with increasing motor unit recruitment and does not stabilize until >100 motor units are recruited. The conclusion that McClelland et al. (2014) seem to have drawn from this simulation (see their point l and the title of their letter) is that raw EMG, because it shows increased coherence levels with more motor units, is preferable. This increase they seem to associate implicitly with the stability of the estimate when they state correctly that in an experimental situation the preferred measure should be the one with the most stable estimate as muscle contraction increases. However, a proper and less selective reading of Farina et al. (2013) and especially of Fig. 4B should have led McClelland et al. (2014) to conclude that it is rectification that produces the most stable estimate of coherence with changes in motor unit recruitment. Furthermore, the data from Boonstra and Breakspear (2012), in which a heterogeneous and therefore more realistic shape distribution of the motor unit action potentials has been modeled, show clearly that rectification is favored over raw EMG for the recovery of oscillatory drive. This paper is highly relevant to the debate and yet is ignored in the letter of McClelland et al. (2014).

We also note in relation to the SNR that in an experimental situation, for example EMG recordings from first dorsal interosseous muscle (IDI), in which many experimental studies of single motor unit, EMG-EMG, and corticomuscular coherence have been conducted, assuming motor units fire at between 5 and 20 Hz, one can estimate from the data of Zhou and Rymer (2004) that, for up to 30% maximum voluntary contraction (MVC), <50 out the 250 available motor units are recruited. For the vast majority of experimental studies in which nonfatiguing 10% MVC are used (see Farmer et al. 2007), number of recruited motor units will be <50. Therefore,
point 1 of McClelland et al. (2014) reflects a highly selective reading of the Farina et al. (2013) work and a highly selective reading of the literature as a whole (see their reference list). The most important omission in our view is the paper of Boonstra and Breakspear (2012), which represents the most comprehensive attempt to understand the EMG rectification effects on coherence estimates to date.

Third, although we agree with point 2 of McClelland et al. (2014) that there does appear to be a trend in our Table 2, we do demonstrate a statistically significant improvement \((P = 0.005)\), at the population level, of rectified EMG over raw EMG in predicting motor unit coherence for postural contractions. This difference is less marked but not significant \((P = 0.19)\) in low-force isometric contractions. The implication by McClelland et al. (2014) that this narrowing of the difference of partial coherence is sufficient to reject rectification is not supported by the data.

Fourth, unfortunately, point 3 of their letter reflects a serious misunderstanding about the relationship between coherence and spectral power, and for them to suggest that our consideration of spectral densities of processes involved in coherence analysis is “irrelevant” is simply wrong. Coherence as a frequency domain analog of the product moment correlation requires the ratio of covariance squared to the product of the variances to be evaluated. To calculate a ratio without examining the denominator, i.e., in this case, the EMG spectra, is a recipe for trouble. In addition, the spectra we examine are the predictors in the partial regression analysis (partial coherence), and it is important to ensure that the predictors have significant power over the frequency ranges they are being asked to predict (Gersch 1972). We note in our paper that the rectified EMG spectrum is closest to that of the single motor unit point process that represents the timing of motor unit firing.

Fifth, the point we wished to make regarding the 50-Hz mains artifact was that we did not feel that this gave any useful insight into how a surface EMG signal represented the timing of individual motor unit action potentials. This remark only arose because of the McClelland et al. (2012) paper. This is not the forum to comment on the manifest flaws in this paper, but the conflation of a mains artifact with measures of synchronization is a glaring example.

Finally, what appears to be absent from the discussion in the letter of McClelland et al. (2014) is an understanding that EMG coherence analysis and studies of motor unit synchronization originated in the point process paradigm in which it is timing information that is to be recovered from the signal (Farmer et al. 1993). With surface EMG data, the closest approach to a point process analysis of single motor unit spike trains is to threshold highly filtered and therefore “spikey” surface EMG (see Gibbs et al. 1997). Rectification is not dissimilar in this respect, i.e., the EMG wave form is disrupted deliberately to facilitate detection of time-varying increases and decreases in the signal resulting from changes in drive to the motoneuron pool. We note also that, in the future, high-density surface EMG recordings are likely to contribute to improvements in recovering timing-sensitive single motor unit signals and coherence from the surface EMG record (C. Van de Steeg, A. Daffertshofer, D. F. Stegeman, and T. W. Boonstra, unpublished observations).

To conclude, the continuing debate on the merits of rectification now has quantitative data in the experimental domain (Ward et al. 2013). This will help experimenters to make an informed choice for their data.

DISCLOSURES

No conflicts of interest, financial or otherwise, are declared by the author(s).

AUTHOR CONTRIBUTIONS

S.F.F. and D.M.H. drafted manuscript; S.F.F. and D.M.H. edited and revised manuscript; S.F.F. and D.M.H. approved final version of manuscript.

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