Utility of EMG to Quantify Activity Levels in Small Muscles

To the Editor: The article “EMG Characterization of Embouchure Muscle Activity,” by Iltis and Givens,1 published in the March 2005 issue of Medical Problems of Performing Artists, adds significantly to the body of knowledge attempting to relate muscle activity to pathologies that appear to arise from it. In their study of embouchure dystonia, the authors focused on small/tiny muscles in the face, a goal that challenges the limits of EMG as a reliable tool. In this regard, we would appreciate the authors addressing two concerns that arise from use of a surface EMG technique.

First, the study made no mention of “cross-talk” that typically occurs during EMG data collection on small muscles. Published results of this study would be considered extremely good, even for big muscles (such as the larger ones in the legs and arms). Based on the method description, electrode placement required at least 11 mm of separation. Due to the relative movement of skin and muscle tissue during activity, targeting of the electrodes is very difficult in the selected test area, and results uninfluenced by “cross-talk” would be surprising. This is problematic because cross-talk from nearby muscles contaminates EMG signals and may mislead the interpretation of the information generated.

Current knowledge indicates that there is only one way to reduce and possibly eliminate cross-talk in EMG data detected using a surface electrodes—i.e., a double differential technique. This technique consists of using surface electrodes having three equally spaced contact points. By examination of adjacent electrode pairs, two signals are obtained. A differential signal can be calculated subsequently and used to rectify results. In the current study, not only do investigators have to deal with muscles in close proximity to one another, but they must also deal with ones that overlap. Anatomic factors such as anisotropy and nonhomogeneity compound the difficulty of reliably repeating the measurements. Without engaging in discussion of cross-talk, one needs to be particularly cautious in interpreting results from EMG measurements.

Theoretically, one cannot avoid this issue, even when results are of the high quality observed in the current study. Perhaps a saving grace can be found in the high level of competency of the study subjects. It appears reasonable to hypothesize that, due to long-term training, the muscles in proximity to the test location operate synchronously. In this case, cross-talk could reinforce the reliability of qualitative interpretations based on the measurements. One way to examine this possibility would be to repeat the study using novice players—ones in whom asynchronous activity presumably will be present due to an absence of effects associated with long-term training—and compare results from both studies.

Second, extreme caution is needed in presenting EMG as a quantitative measure. Detected EMG signals will vary significantly among different subjects with about equal muscle strength. To enable comparison between and among subjects, the practice of normalizing EMG data is commonplace. Even after normalization, results must be considered quasi-quantitative. The normalization process expresses EMG measurements as a percentage of a chosen reference value. Commonly used references include 1) the maximal signal level during a trial and 2) a maximum voluntary contraction (MVC) determined in a separate trial under high-load conditions. The first of these references provides results of questionable quantitative utility, because the “reference” can vary from trial to trial even with the same subject. The second

SFF and one diphthong duration measure. These results confirm earlier cross-sectional studies that compared singers with nonsingers, in that singing training mostly affects the singing voice and rarely the speaking voice.


Music therapy has been used to treat instrumentalists with performance anxiety; the most commonly used modality is systematic desensitization, a form of behavioral therapy. Virtual reality may be considered among these types of behavioral therapy. This study examined the effect of computer-generated virtual reality graded exposure on the physiologic and psychological responses of three university saxophonists. The subjects, 2 women and 1 man, performed while immersed in 4 virtual environments, each designed to gradually increase the expected anxiety level of the performer. Heart rates and subjective measurements were taken throughout the exposure period.

Results indicated that this exposure under these conditions did elicit both physiologic and psychological indications of increased anxiety. Findings also corroborated previous research that found gender to be a predictor of heart rate during performance. In this study, the women’s heart rates increased dramatically when compared with that of the man. Especially considering the small test population of this study, the author indicates the need for additional research into this topic.
approach roughly establishes the physiologic limits of the muscle (MVC), a reference value that is more stable and may be deemed reasonable. Using the latter tool, loads and intensities may be expressed as a percentage of the physiologic limits of the individual. Such values permit quasi-quantitative comparison among trials and between subjects.

Results of the current study underscore the promise of EMG as a tool in examining and identifying factors related to injuries in industries where fine motor control dominates the activity. Engaging in dialogue regarding the method’s strengths and weaknesses can add to its utility. We hope the authors can elaborate further on the details of their processes and interpretations related to the two concerns identified here.

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In reply: I would like to thank Dr. Shan and Mr. Visentin for their thoughtful comments on our recent paper. First, with regard to the issue of “cross-talk,” although I have a high degree of confidence that the very small (1.5 mm inside diameter) electrodes were actually over the desired muscles (isolation movement trials specific to the targeted muscles confirmed that we had at least to some degree isolated the target muscle), there is always the possibility of “cross-talk.” However, in determining reliability, the critical issue is that the signal obtained be reproducible. This was enhanced by several factors in our study, including consistency in electrode placement between sessions (specific landmarks were used) and consistency in muscular activity (the same notes were played at a controlled level of effort by highly skilled French horn players). Even if some “cross-talk” were present, it was evidently consistently present and failed to substantially reduce the reliability of the obtained data. Furthermore, the problem of skin movement as a source of signal noise is also very minimal, given the highly static nature of the muscle contraction from trial to trial.

Dr. Shan and Mr. Visentin are correct in their observation that the reliability obtained in our study may indeed be population specific and that less skilled performers may not show the same degree of reliability. We currently are expanding our study to include players of lesser ability as well as to include other facial muscles involved in embouchure control. The obvious final extension of this possible difficulty comes when the idea of studying dystonia patients is brought into play. This awaits further study.

Concerning the use of a third contact point (electrode) in order to utilize a double differential technique to account for “cross-talk,” this would have been impossible given the very small muscles being examined and the 11-mm outside diameter of the miniature electrodes. I have been in contact with corporations that manufacture multiple bar electrodes for overcoming this and other problems, but none are manufactured small enough yet for our particular application.

Shan and Visentin also comment on the well-known process of normalization of EMG signals when attempting to cautiously employ quantitative techniques. We certainly considered this issue, but normalizing did not seem appropriate for two reasons. First, normalizing to MVC is obviously a problem when studying the muscles of the embouchure. As a horn player myself, I cannot conceive of how one might execute a maximal embouchure contraction. We could have normalized the signals to a standard performance task, but the second consideration that kept us from doing this concerned the nature of the study itself. We were not concerned with the actual values of amplitude-related variables at all; rather, we were concerned with the reliability of these signals between testing sessions within subjects on a standardized task. Any quantitative data that relates to amplitude is unquestionably of limited value when comparing individuals or populations, and we do not in any way advocate using it for that purpose. However, even amplitude data can be very useful in assessing between-session reliability. Finally, the quantitative spectral data probably do not suffer from this limitation and may actually be of some use in comparative studies in the future.

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