Letter to the editor regarding "Application of principal component analysis in clinical gait research" by Federolf and colleagues.

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The recent paper by Federolf, Boyer, and Andriacchi (2013) proposes a very interesting approach to analysing gait biomechanics which aims to address a number of challenges, namely: the widespread use of discrete point analysis which utilises only an extremely small percentage of available data (<2 %), the coordination and interdependence of movement kinematics or/and kinetics within body and environmental constraints (Bernstein 1967), and the limitation of small sample sizes. The authors propose a new approach based around principal component analysis (PCA) which combines all of the kinematic and kinetic measures into one large vector, rather than (a) examining specific preselected discrete measures, or (b) examining all of the measures but only for one variable at a time (e.g. Harrison, Ryan, and Hayes 2007; Richter et al. 2013). Overall we highly commend the authors for attempting to address these major challenges, and we strongly support the use of PCA to analyse biomechanical data (Richter et al. 2013); however, we believe that the proposed approach contains a number limitations that could preclude its use as applied in the present study.

Federolf and colleagues use a discriminating vector, which incorporates characteristic differences between movement patterns, to identify statistically significant differences between healthy and pathological gait when assessed across *all* of the combined marker data (x, y and z) and force data (x, y and z); some 12,432 variables/data points. After a *single* positive statistical finding the authors go on to subjectively identify individual marker/force differences across very short phases of the gait cycle and infer that these are specific differences between the groups, without any further statistical analysis applied to the identified variable over the explicit phase, or without indicating *a priori* what magnitude of the discriminate vector represents a 'significant' difference.

In using this proposed subjective approach a researcher's opinion on which phase the difference is evident could be very biased by, for example, previous and possibly inaccurate findings in the literature, or findings 'needed to fit' an author's own theory. Even in the absence of bias, a result would be dependent upon a researcher's opinion of the differences in the visualised stick figures and force traces. For example, Federolf and colleagues suggest that there are differences in the loading rate of the vertical ground reaction force immediately after touchdown (label 'C', Figure 3); however, they do not suggest that the loading rate during the early phase of propulsion (approximately 55-75% of stance) is greater in the osteoarthritic gait even though the differences here appear to be larger than during the period immediately after touchdown. Similarly, in the sagittal view of the stick figures at touchdown (Figure 2) there appears to be greater ankle dorsiflexion in the swing leg of the osteoarthritic gait, but this is not mentioned by the authors. Examination of Figures 2 and 3 show numerous examples of where different researchers could

potentially arrive at different findings. This constitutes, in our opinion, a major shortcoming and runs counter to an objective scientific approach.

Federolf and colleagues chose not to apply their analysis to calculated secondary variables, such as joint angles, because they require "…..pre-selection of variables (through the decision which secondary variables are calculated) or definition of arbitrary axes to define the motion of the joint" (page 2177). This seems rather strange given that they ultimately infer changes at a joint angle based on the different position of markers. Similarly, we question how tri-planar actions can be separated into abduction-adduction and internal-external rotation components when only considering marker position projected onto global coordinate planes. The authors provide no evidence that the errors introduced here are smaller than the errors associated with the inaccurate identification of joint axes of rotation. In addition, identification of differences between healthy and osteoarthritic gait based on the stick figures (Figure 2) appears to be more challenging in the markers/joints that are further from the pelvis. This is because differences at the distal marker/joint (e.g. toe/ankle) are geometrically dependent upon differences at more proximal joints (e.g. knee and hip/pelvis), and the authors do not isolate them. Differences at the toe/ankle joint can be geometrically exaggerated because of small accumulative differences at the hip/pelvis and knee. This visualisation approach is also likely to be influenced by how the paired healthy and osteoarthritic stick figures are centrally aligned, which does not appear to be clear from the paper.

Finally, it would seem appropriate to us that a new data analysis method which claims to identify differences based on very small sample numbers should evaluate repeatability (e.g. jack knife, bootstrapping, cross validation using a withheld portion of the original sample), especially in this case where it is generally suggested that completing a PCA with low numbers is inappropriate (Comrey A. and Lee H. 1992). Although the appropriate number of samples is dependent upon the degree of correlation between the variables, with consistently high correlations indicating a need for smaller sample sizes (MacCallum R. et al. 1999), less than 50 samples is considered very poor as a rule of thumb (Comrey A. and Lee H. 1992). Equally, a low ratio of the number of samples to the number of variables increases the probability of errors and reduces the generalizability of the results (MacCallum R. et al. 1999). In addition, given that outliers have a large negative effect on the accuracy of the identified PC as it skews the subspace solution (Huber P. 1981), the proportional effect of an outlier (even a single outlier) is increased with low sample numbers. Within the presented study only 30 trials were used, two thirds of which were repeat trials, with an extremely low ratio of sample size to the number of variables (30:12,432 = 1: 414). Nunnally (1987) recommends a minimum ratio of 10:1.

There are two key stages in the work presented by Federolf and colleagues where repeatability could be examined: the generation of a discriminant vector (ultimately used in the statistical analysis and the visualisation of differences), and the subsequent identification of specific characteristic gait differences. Unfortunately, the authors do not report the repeatability of either, yet we would imagine that their original data set (Boyer et al. 2012) contains enough data to do this.

Conflict of Interest.

The authors have no financial or personal relationships with other people or organisations that could inappropriately influence (bias) our work

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