

Letters to the Editor

Sir,

I write to comment on the article by Männchen (2001). This paper details what purports to be 'an objective evaluation of the pitchfork analysis (PFA)'. The cases used for the PFA showed more skeletal and less dental change than those for Björk's structural analysis (BSA), a difference that prompted the seemingly extreme conclusion that 'the PFA is not sufficiently sensitive to differentiate between dental and skeletal changes'.

In the PFA, all change, skeletal and dental, is measured parallel to the mean functional occlusal plane (MFOP). Given a sign convention appropriate to the malocclusion, the individual elements must add up to 'explain' the molar and overjet corrections. The author, however, compared a modified PFA (in which he apparently was forced by computerization and the use of existing records to use a 'nasal line' for the maxillary superimposition) with a version of the BSA in which skeletal change was measured parallel to Frankfort Horizontal (FH) and dental change along MFOP. As a result of this mixture of planes, the algebraic sum of the individual elements does not equal the molar and overjet corrections. Thus, the study was merely a comparison of two *different* analyses, one of which was a type of PFA.

FH was Björk's preference. If, however, one were to conduct a 'comparison', the use of different planes of reference, FH versus the MFOP, would seem guaranteed to produce exactly the differences Dr Männchen found in the superimposition of his three series. An extensive error study ($n = 73$; Johnston, 1996, Figure 10) argues that, even within a single method, comparable differences are inevitable, especially when $n = 3$. If relatively small differences are to be the basis of an indictment, then it is clear that both Dr Männchen's results and his harsh conclusions were inevitable. If it is assumed that the basic idea of the PFA is appealing, then it would seem more profitable to debate the technical details of superimposition.

As noted in Johnston (1996, p. 95), the technical details outlined are merely the preferences of this author.

A major departure from the European gold standard is the use of an occlusal plane orientation and a maxillary superimposition to generate estimates of skeletal change. When colleagues at the University of Michigan execute the PFA, they commonly infer skeletal change from a cranial base superimposition not unlike that employed by Björk. Use of a maxillary superimposition is, as stated by Dr Männchen, sensitive to mid-facial rotation, an effect also previously observed (Johnston, 1996, p. 98).

It should be noted that I do not use a 'nasal line' to effect maxillary superimposition. In any event, it is clear that I failed to reckon with rotations of the magnitude of those posited hypothetically in Figures 16 and 17. I had in mind clockwise rotations more in the region of 1–2 degrees. In retrospect, even such small rotations would probably explain the fact that my estimates of the skeletal effects of functional appliances are slightly on the high side compared with others in the literature.

Isolated rotations of the maxilla such as those invented by Dr Männchen, would have an effect not just on the analysis, but also on the actual occlusion. In the event, what would these changes be called? They would not be tooth movement; however, Dr Männchen implies (p. 11) that they would not be 'real biological effects' either.

Although the effects of maxillary and occlusal plane angulation/rotation on analyses such as the Wits appraisal are well known, Dr Männchen's discussion of the potential effects of *vertical* growth (Figure 18) is, to my knowledge, a novel contribution.

To this end, I would argue that PFA is what it is—an internally logical framework for the assessment of change. All that is required is the use of regional superimposition in conjunction with a sign convention and a *single* plane of

orientation (be it AP, vertical, or mediolateral). The best way of executing each type of PFA remains to be seen and thus is fair game for serious discussion. Dr Männchen's 'critical analysis', however, does not qualify as a participant. A study of two methods, one of which is to be accorded gold-standard status, should not be constructed in such a way that the results are a foregone conclusion. Given that the analyses he compared are very different, both in terms of measurements and the techniques employed to execute them, there is no way that the estimates of skeletal and, to a lesser extent, dental change could have turned out the same, especially given the probable impact of measurement error on what appears to be the third smallest sample ever examined.

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References

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Sir,

I would like to thank Professor Johnston for his comments.

The Pitchfork Analysis (PFA) has been used in a large number of studies published in recent years. The aim of my article was to investigate whether or not some of the conclusions obtained by the PFA could be misleading. It was not mainly intended to evaluate a standard error for the method—statistical analysis has already been performed by others (Hashim and Godfrey, 1990; Keeling *et al.*, 1993).

In this era of 'evidence-based medicine', statistics are crucial for the evaluation of scientific methods and procedures. However, statistics can only describe the reproducibility of a method or the probability that the differences found between

two or more samples are not just accidental. A method may be very reproducible, but still not appropriate to be applied in a specific situation.

For me the question was not: 'Is the PFA accurately reproducible?' or 'What is the difference between two treatment groups when examined according to the PFA?' The question was: 'Does it make sense to use such a procedure to analyse just any sagittal changes?' and 'Does this method lead to conclusions that reflect a reasonable orthodontic perception (as presented, for example, by Björk's analysis)?' or 'Is the method affected by extreme situations or treatment modalities?' These questions cannot be answered by statistics, but rather by using common sense. Thus, it is not the sample size that makes the investigation meaningful. It was pointed out in the article that two extreme cases were chosen for this investigation because I thought that the PFA was very much affected by rotations of the maxilla and the occlusal plane, which could be clearly shown and explained.

It would appear that Professor Johnston was thinking of rotations 'in the neighbourhood of 1–2 degrees'. With a posterior rotation of the maxilla of 1–2 degrees, an additional ABCH of about 1–1.5 mm is produced. In the case of a full Class II malocclusion treated by 7 mm to a Class I intercuspation, the partial contribution of, for example, 50 per cent skeletal and 50 per cent dental, is altered to about 70 per cent skeletal and 30 per cent dental. So even small rotations cause major effects.

Professor Johnston also admits in his letter that he was not aware of the sensitivity of the PFA to vertical changes, but still he prefers to use the occlusal plane instead of another, more horizontal (and less individual) line. The main reason why vertical changes affect the results this much is the choice of this line. If the chin of a patient (e.g. subject number 1) moves forward less than the maxilla and you, nevertheless, get a positive ABCH, you have a serious problem to explain why. That is the understanding behind the sentence 'the PFA does not reflect real biological effects'. 'Not reflecting' is probably not correctly phrased (*mea culpa*), as the PFA certainly reflects biological effects *somehow*. It would have been better to say

that the interpretations obtained by the PFA sometimes do not match the orthodox orthodontic terminology.

The PFA for subjects 1 and 2 look very similar, although these two patients represent extremely different patterns. An analysis that levels such differences is not useful. However, I did admit that, in the absence of any rotation of the maxilla, mandible, and occlusal plane, the PFA supplies you with information about the partial dental and skeletal contributions to the intercuspation change in just the one individual dimension of the occlusal plane. This rather restrictive indication is probably respected by few authors now using the PFA. On this basis investigators

have to make their own decision whether to use the PFA (which is impressive for its readability) or a more complex method instead.

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