

## COXARTHROSIS RESEARCH DISCUSSED

by Gary Horvitz, M.A., P.T.  
Certified Rolfer, NAR

I read with interest your summary and analysis of the German booklet, "Standing Upright," but I confess to being disturbed and puzzled by a number of questions about its content. I have decided to respond in Rolf Lines, since this avenue is likely to have the most benefit.

To begin, I am not familiar with the term "coxarthrosis" (See LETTERS TO THE EDITOR) and was frustrated throughout my reading by the fact that it was never defined. Since we haven't been informed as to what coxarthrosis is, we don't know anything about the "healthy" subjects either. We also don't know who determined the coxarthrotic subjects to be such, or by what means. It wouldn't surprise me if I got six different definitions were I to poll any half-dozen readers of the of the study. Nevertheless, there are quite a few features of your summary that raise substantial questions regarding the significance of this investigation.

As for the measurement of the angle "d", who did the measuring By what means? If both authors measured, did they always agree exactly? By how much did they disagree? And why were the subjects x-rayed with their shoes on? Wouldn't differences in heel height and asymmetries in heel wear be reflected in the angle "d" as well as elsewhere?

In those persons found to have a low angle "d", how was it determined that their lumbo-pelvic musculature was "more strongly developed"? By girth? Strength tests? When asking the subjects of this study to stand on the double scales, how long did they stand before the reading was taken? How long did the scales have to be still before taking the reading?

As far as EMG studies are concerned, it is not surprising that subjects with a lower angle "d" had a greater activity in the erector spinae groups. Muscles which chronically reside in a shortened range develop greater sensitivity to disturbance. The gamma motor system becomes more reactive to passive stretch than in a muscle which has a more normal resting length. On the other hand, were any of the coxarthrotic patients in pain? If so, it must have played some part in the level of activity of these muscles. And even if

they weren't in pain on the day of measurement, a chronic condition will always facilitate associated muscle activity.

As for Ida Rolf's presumed definition of the horizontal pelvis as depicted on page 102 of her book, it appears to me that this definition is dependent on the relationship between the superior rim of the pubic symphysis and the tip of the coccyx. Since in real life there is considerable variation not only in the configuration of the coccyx but also to some extent in the precise declination of the sacral promontory, any inference about the angle "d" from this illustration would seem to be wishful thinking.

Now, why do 90% of the coxarthrotic subjects appear to be in better alignment than the majority of the so-called healthy subjects (By the way, what is the age distribution of this sample? Are we talking about early coxarthrosis? Advanced?) Could it be for the simple mechanical reason that an increased lordosis implied by a sharp angle "d" would normally be accompanied by a corresponding degree of kyphosis, thrusting the head forward enough to decrease the horizontal distance of external ear from the acetabulum?

What the authors of this booklet and Wolf likewise seem to overlook is that structure is a continuum. Developing rigidities in one area of the body, no matter how fixed they may appear to be, do not constitute a fixed reality for the whole body. Change continues elsewhere no matter what. The reason there appear to be conflicting results of this investigation is precisely because of this fact. I believe Wolf is correct in pointing out the crucial flaw in this study to be the position assumed by the subjects for the x-ray. It is not a functional position for real life. Hence, these results can barely shed any light on a general understanding of structure, let alone a comprehension of Rolfing.

Nevertheless, on several occasions Wolf refers to the Rolf "line" as a theoretical construct to be confirmed or invalidated by this study. At the risk of sounding heretical, the image of the "line" is only that. It tells us very little about the range of motion available throughout a given body for functional activity. After all, life is movement, is it not? Wolf places unwarranted value on the notion of "stability" (a pitfall of chronic contemplation of a static image presumed to represent organized structure), losing sight of the fact that there is a great difference between the form of repose demonstrated in certain aspects of this study and a dynamic balance which is in the long run far more supportive of proper function.

Despite the obvious difference I have with this study and with your analysis of it, your consistent and thoughtful efforts are important and appreciated. I felt that they deserved an equally thoughtful response. ☺