

THE NEED TO KNOW

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It is the privilege of the newcomer to question the basic concepts of the new field, (and) I once wrote and tried to prove that the tensegrity model is no correct anatomical model. Then the only reaction in Rolf Lines was a letter consisting of one word: "Phew"

I am not a total newcomer anymore, but I am still trying to question areas where the basic concepts are questionable at least if not outright false. In the article on "Coxarthrosis", meaning arthritis of the hip joint, I used the scientific findings of X-ray studies to show my doubts about the "posterior pelvis". This time finally there was a real reaction, a long letter by Gary Horvitz in the last issue of Rolf Lines. He questions my questioning and the validity of my authorities.

In rebuttal let me use one of Gary's arguments to prove my point: "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" (angle of the posterior facet of S1 to the horizontal) from this illustration would seem to be wishful thinking."

Yes indeed, this is the problem. I too found in my practice that the coccyx can have all kind of angles in relationship to the sacrum. Often it is drawn in by an overtensed Pubococcygeus muscle (for those people the Kegel exercises Stanley Rosenberg recommends not would seem to be very helpful). And once I even found a coccyx that went out backwards from the sacrum. I could never find a consistent relationship between the position of the sacrum, the pelvic overall configuration, and the angle of the coccyx. Yet the horizontal line between tip of coccyx and superior rim of the pubic symphysis is all that Dr. Rolf has given us as a criterion for the horizontality of the pelvis - this is why I used that drawing.

I totally agree with you: Any inference about the horizontality or non- horizontality of the pelvis from this criterion "would seem to be wishful thinking"! But I have been taught, that horizontalizing the pelvis is our major goal of the basic series. How can I know that I have reached that goal? By no means with the help of Dr. Rolf's definition. But we need to know that. It is decisive for our everyday work. So we need to develop the work of Dr. Rolf further, we need to go beyond her.

John Cottingham has suggested using the anterior and posterior superior iliac spines and he has developed a cute technique for measuring it. But to come to a consensus that this could be a feasible way of determining the horizontality of the pelvis, we need a process in which the relevant sectors of our community are able to take a position on such questions and then form a consensus until new arguments come up.

Let me give you another example of our need to know: During the Board meeting in Boulder I had discussions about Roling theory with several Board members. I found out that we had totally different ideas of what fascia is. Most people I talked to, even eminent and senior Rolfers, thought that fascia includes the endomysium, the perimysium and the epimysium, the whole inside connective tissue of the muscle. Naturally this is all wrong. If you check your histology books you will find that myofascia are the enveloping layer of connective tissue the fibers of which generally run at a right angle to the course of the muscle fibers.

The fact that this misconception is so widespread - I did a little survey - in my view is not due to the ignorance or stupidity of Rolfers, but the result of a contradiction in many Rolfers minds and deeds. While verbally they try to stick to Dr. Rolf's dictum, that we work with fascia and fascial layers, in fact and deed they work with the whole connective tissue of the muscle and harmonize this contradiction by forgetting what the technical term "Fascia" originally means.

I also have my grave doubts whether we only work with fasciae. There are some instances where the sticking or shortening of these outermost envelopes can explain a lack of movement range or a displaced structural element. (I would very much like to give instances for that from my anatomy studies, but the article would get even longer!) But in most cases structural deviances cannot be explained by a shortening or sticking of fasciae alone. It is the whole connective tissue of the muscles. and the brittle, sandy, pebbly or other texture that we feel under our fingers in those areas can never be produced by the changes in those extremely thin envelopes. Obviously therefore one of the very basic concepts of our work nowadays is factually different from what Dr. Rolf proclaimed it to be. But still we stick to the old concept in our brochures and articles. Let's hope that nobody finds out that many of us don't even know what we are talking and writing about when we use the term "fasacia".

We need to know those things, if we want to stay a respectable profession - and if we want to do a good job for our clients. Therefore we need a procedure how we can develop both our knowledge and a consensus on what - for the time being - is common Roling knowledge. I suggest that install a Scientific Journal - that is, continue with the Notes of Structural Integration for example (I heard that you, Gary, were editor of the Bulletin, so maybe you are sympathetic) with a Board of Editors in which the Teachers, the Research Committee, the Board of Directors delegate one member besides the Managing Editor. No article may be published against the veto of any member. It has to be rewritten until all agree. This may take a long time until an issue can be published. But then it would contain articles which are not just private ideas, but have gained the support of relevant sectors of this community. This Scientific Journal then should be sent out to all members of the Institute as a selfevident free of extra charge service they are entitled to by paying their dues. It could also be used to represent ourselves to other professions.

I shall introduce a motion to that end in the next session of the Board of Directors and I would be most grateful for letters of support either to the Rolf Lines or to the President. I do need your support for in the last session of the Board of Directors I was a hopeless minority.

Now to your specific questions, Gary Horvitz:

It is not important who measures, but how. If the procedure is always the same, the result will always be the same. In this case in the x-rays the most posteriorly visible facet of S1 was taken as a line the inclination of which to the horizontal was measured.

The subjects were x-rayed with their shoes on in order to study their real life most common stance - the barefooted stance nowadays is rare. And naturally that does influence the angle "d" and tends to tilt the pelvis forward, which partly explains why they didn't have posterior pelvises in the study.

I don't know how it was determined that in people with strong lumbar lordosis the lumbo-pelvic musculature was "more strongly developed". But who are we to complain? Just remember the kind of talk after a Rolfing session in one of our workshops!

The subjects were asked to stand for one minute on the double scales before a reading was taken, and the surprising result was that in persons with a strong lumbar lordosis the scale never stood still while persons with an upright pelvis were able to create stability. Yes, coxarthrotic persons are in pain, and that explains some of their behaviour on the scales. But persons with a strong lumbar lordosis who had not - yet? -developed arthritis of the hip joints showed the same behaviour.

Yes, I agree with you, that probably the position of the arms in taking x-rays (They asked them to fold them in front of the breasts, because otherwise the side-view of the pelvis would have been disturbed by the bones of the forearm and hands) and the kyphotic countercurve of the persons with strong lumbar lordosis explains the disquieting findings, that the "bananas" were healthy, while those who corresponded to the "line", were in pain.

I do not agree with you on the easy loophole you construct for yourself on the issue of the "Line". You just call it merely an "image" and you move off into movement. While I do agree that movement is decisive and should never be disregarded as the more important criterion, I still insist that structure and therefore structural integration centers around the "line". The "line" should never be looked at only in the ear, the ankle and the pelvis - this is the major flaw of the study I reported on as far as its relevance for Rolfing is concerned. But the line through the whole body for me still is the decisive criterion for whether I have done good or bad work. I know that there are many more facets to this work, and for the clients most of these other facets may be much more important and they often couldn't care less whether they stand straight or not. But as long as I am a Rolfer I define myself as somebody who works structurally- and that and only that makes me special. As long as I take Structural Integration serious I take the view that all those other facets of my work have turned out fine when I get that structure organized around that central vertical line. If we give that up, we should give ourselves another name!

I tried to be as fierce and to the point in my rebuttal as possible. Don't let that lead you to the misunderstanding, that I am not extremely grateful for your response. I think the Rolf Lines Journal should be that: private thoughts on Rolfing publicly ventilated and controversially discussed, but without claim to institutional consensus which however a Scientific Journal on Rolfing should have.

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