Asia-Pacific Chiropractic Journal

Regarding the 'Prevalence of abnormal findings in a cohort of 737 patients referred for MRI examination by Doctors of Chiropractic and potential neurological consequences associated with Vertebral Subluxation'.

Phillip Ebrall

Kent has produced a fine piece of work for which we must be grateful to McCoy Press for publishing. As readers will gather from Blum's commentary the paper is not without criticism.

The Journal has placed both the paper and an explanatory video clip on our landing page for this issue, such is the value we accord it. We also acknowledge that Blum's original Letter to the Editor was declined. We have no interest in the politics or otherwise for this decision, but I do pause to wonder whether I am categorised by others as being in a cartel, a term which usually refers to being part of the anti-subluxation stance of the WFC and the AmCA, or in some other group, perhaps rampantly pro-subluxation.

In my academic career which spans 30 years and counting, I have been categorised in both. It is the medical lobby, notably a troll called Sue Ieraci and her followers on the fringe of chiropractic who categorise me as a '*subluxationista*', and it is past students of my classes who categorise me as '*anti-subluxation*' if not the '*antiChrist*'.

To be blunt, I don't give a stuff how others see me, it is of no relevance to me if of some passing relevance to them. The matter we have to address it, what is Dr Christopher Kent and his co-author trying to tell us?

I read this paper with great fascination. I appreciate it will be criticised on so many levels and while regrettable, such criticism is also meaningless. Let me explain.

A current paper by Kaye et al concludes '*we could not associate the presence of Degenerative Spondylosis (DS) with increased baseline neck or arm pain. Instead, DS appears to be a relatively frequent (20% in this series) age-related condition reflecting radiographic, rather than necessarily clinical, disease*'. The authors took the medical perspective and used basic instruments such as the NDI to seek an association.

At face value this seems to suggest Kent's work is in error. I disagree and argue that it may be Kaye's work which is at fault. They '*evaluated the preoperative status of our patients undergoing anterior cervical decompression and fusion (ACDF) and compared cohorts of those with and without the presence of degenerative cervical spondylolisthesis (DCS) to more precisely examine the association of DCS with clinical findings*'. In other words, they measured patients pre-selected for surgical correction of a presumed disease state and who proceeded to surgical intervention and its consequences.

If we step back we can see that in each report (Kent and Kaye) we have a sampling from clinicians who are treating patients who presented with a problem. In Kaye's case the patient saw an orthopaedic surgeon where the paradigm of correction is surgical intervention complete with complications which are a quantum more serious than with conservative chiropractic care.

Similarly, Kent's very much larger (n=737 compared no n=242) sample is of patients who consulted a chiropractor where the paradigm of correction is manual correction of subluxation, a remarkably safe intervention.

In other words, both cohorts were filtered by clinical assessment and proceeded to the paradigm of care offered by each. We can accept that pragmatically, each patient had sufficient signs and symptoms of the problem for which they sought care. In the case of chiropractic patients attending a chiropractor, these indicators were evidence of vertebral subluxation.

Let's back up a moment: Blum's concern is really whether or not the MRI findings indicate subluxation. I share that concern and recall a textbook a decade or more ago which purported to show radiographic findings, in cadavers, of subluxation, an impossibility.

I read Kent as NOT showing evidence on MRI of subluxation, rather I strongly argue he has shown a fascinating finding that 90% of patients seeking subluxation-based chiropractic care demonstrated abnormal MRI findings.

To me, it does not matter whether or not the MRI showed a physical finding at, let's say T5/T6, in a patient who has attended to me for my care. If we were generous we could presume the patient had some indicators of subluxation about T5/T6, but I would rather argue they had a clinical presentation outside such a localised finding; perhaps dyspepsia or some other indicator of dysfunction in the thoracic spine.

I would also not particularly care if I chose to adjust perhaps T1/T2 in such a patient with findings at T5/T6. Why? Well, this is where it gets complicated with concepts of primary and secondary subluxations and so on.

I argue that the actually physical location has little to do with the exceedingly complex clinical construct of subluxation and am completely happy on adjusting whatever the evidence at that time indicates I should pay attention to.

The rather remarkable finding of Kent and Costello is that the greater majority of patients seeking chiropractic care have observable findings of things about the spine that we reasonable expect may contribute to a form of small dysfunction. And it is this idea which creates our wonderful thing we call 'subluxation'.

The conventional chiropractor will proceed to provide their preferred form of correction by their preferred technique, and heaven help us should we ever dare to suggest that '*this MRI finding* …' indicates a '*Gonstead thrust with counter-clockwise torque at spinal segment T5* …' or, in another paradigm, an '*Activator impulse at Atlas Left*.'

Chiropractic is a clinical art, not a prescribed recipe of care. It is reliant on every individual practitioner's reading of the patient before them which will in turn direct their specific form of care.

This notion that every chiropractor will provide the same type of care is an abject nonsense, yet the patient still gets better. This suggests to me that the subluxation is something beyond a collection of physical findings seen on MRI and something beyond what one chiropractor may take as prompting their style of intervention compared to another.

To conclude, I could not under any circumstance consider my colleague Dr Blum to be part of an anti-subluxation cartel, just as I can not consider Kent to represent a pro-subluxation mob. I know each to be deeply thoughtful on what it is that chiropractors do, and regrettably I suggest that none of us are yet close to finding out.

I think Kent's work is deeply impressive with a lot of meaning. I think Blum's questions are valid. I believe that all chiropractors must think more deeply about that it is that they do, even while going ahead and doing it, many times a day, with great success.

Phillip Ebrall
Editor
pebrall@me.com

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