Core stability is a subset of motor control

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Love it or hate it the term 'core stability' is ubiquitous, and is a firm part of the lexicon of modern life. While preparing to write this editorial I asked many people, from therapists, to clients, to the non-injured and healthy 'man-in-the-gym' what they thought of the term and what it meant. "I don't use the term when I am talking to my patients," said Chris Dorgu, an elite football Osteopath, working in the UK, "I prefer to talk about the specifics of what I am working on with them. The term core stability is imprecise and open to interpretation." Suzy Barton, a London based Pilates Teacher said that, "The term core stability is used by everyday people and, to most, it means a strong centre while moving the arms and legs. People often have no idea about the science behind the concept." Phillip O’Callaghan, currently working hard with a Personal Trainer in a late stage rehab of an ACL reconstruction said, "It's all about the abdominals, isn't it?"

In this Prevention and Rehabilitation section, the JBMT is publishing an article by Eyal Lederman, entitled "The myth of core stability." It is likely to be an interesting, perhaps challenging, read for clinicians who use the concepts of core stability in their everyday practice.

Lederman reflects a current increase in the popular press, including the 'New York Times' (nytimes.com) and the United Kingdom's 'The Times' (timesonline.co.uk) newspapers, questioning aspects of core stability theory. Lederman’s approach to this article looks at identifying assumptions within core stability theory and applying research findings to see if the assumptions bear scrutiny.

In Lederman’s article the term core stability particularly relates to the mid 1990s work of Hodges and Richardson (1996), Richardson et al., 2004 from Australia’s Queensland University, and the identification of a timing delay in the firing of the transversus abdominis (TrA) during rapid shoulder movement in subjects with low back pain. In the notes given out at a course I attended in 1996, presented by Paul Hodges (Richardson et al., 1996), the term core stability was not used. 'Motor control' was consistently referred to, as was 'local joint stabilization.' The term core stability appears to have developed, concurrently or later and seems to have become the default term applied to all motor control training around the trunk, through to (possibly!) 'a clean and jerk then squatting a water filled Swiss ball’ (youtube.com).

There does seem to be consensus that there may be some differences in the meaning of core stability as the term core strengthening is also frequently found in the literature and on the web. Comerford and Mottram (2001), Comerford (2004) points out that some therapeutic exercises are aimed at strengthening weak muscles around the core, and others are designed to improve the recruitment of muscles that may be underactive and not fulfilling their role in the synergy of neuromuscular control about the trunk and girdles. The difference Comerford suggests is related to the threshold of the recruitment required for each type of exercise, with 'core strengthening' needing to bias the fast fatiguing, fast motor unit to effectively strengthen, and 'motor control core stability' training of smaller postural loads, aimed at improving the recruitment and endurance of the slow motor unit. The clinical problem is in providing the assessment of the clients’ faults so that the correct threshold of exercise is given (Mottram and Comerford, 2008).

Do we need a model of muscle function?

Regardless of how a model of muscle function is created, the construction of a model is important for clinicians to use to try and better understand the complexities of the brains control of muscle, both voluntary and sub-conscious.
For me local muscles such as ‘Hodges’ TrA, (Hodges and Richardson, 1996), or ‘Hides’ deep fibres of Multifidus (DM) (Hides et al., 1996, 2001) have helped me build a concept (however flawed?) of how the deeper muscles may work to control joint translation which, if uncontrolled, may lead to micro-trauma, wear and tear and possible injury (Panjabi, 1992). This led me, in my practice, to offer to those who I felt would benefit from having the deeper muscle structures brought to their attention, an ‘educational’ session identifying those muscles and a ‘muscle recruitment’ quiz to see if they can ‘turn them on.’

Muscle inhibition due to pain is identified in the literature (Hides et al., 1996), as is increased activation (van Dieen et al., 2003). If pain does cause DM inhibition and leads to eventual DM atrophy over an affected motion segment, I feel justified in teaching a client in the clinic how to recruit this muscle. I have read MacDonald et al.’s (2006) ‘The lumbar multifidus: does the evidence support clinical beliefs?’ in which a review of data about the DM and superficial multifidus (SM) is applied to the Queensland approach, and though some beliefs are supported by the data (for example: SM and DM are both segmental motion controllers, DM can work as a translational movement controller without having an antagonist), others are not (for example: SM is not solely a rotator or extensor, DM is not tonically active during static postures). The paper advises that the findings have implications in clinical practice. It appears, in my reading of the paper, that the DM remains an important component of the integrated musculature of the body, so I will continue to address its under-activation.

I certainly do not believe that any one muscle is any more important than another in the so-called ‘core.’ ‘All muscles are created equal’ but this does not mean we are more important than another in the so-called ‘core.’ ‘All important component of the integrated musculature of the body noting that the client back to a healthier footing than they were before their presentation. What I think I am doing, may produce the desired result, however, it may conceivably be by an entirely different process! Furthermore, in the clinic we don’t just do ‘one’ thing to the client, we provide multi-modal treatment regimes which result in the clients eventual outcome. We may reflect and discard for an individual, or all our clients, some approaches that may not have appeared to work, but it is difficult to know with certainty, that we are discarding the ineffective technique. Often, through good luck, good management or the natural history of a condition, the client improves to a greater or lesser extent. The problem for the Researcher is to identify which modality, or which combination of modalities had what effect, which bears up to the rigour of evidence based practice and which do not. This is clearly exceptionally difficult. Being a clinician seems to be the easier choice. For me, the choice to use ‘core stability’ techniques remains valid under the current body of evidence.

Core robustness exercises

Is ‘core stability’ new? Is it a re-badge of other concepts? Wallden (2009), My Co-Editor of this section, in his recent ‘Neutral spine principle’ Wallden (2009) discusses ‘neutral’ which is an integral component of ‘core stability’ yet there is not a mention of core stability in his piece. Why is this? Perhaps its because neutral concepts aren’t exclusive to core stability? Siff (2009b) suggests that at one time we had kin-aesthetic, proprioceptive, or motor skill training, but now it is core stability training, he suggests that this is not a suitable modern substitute. He opines that the core, in most instances, operates in a world where peripheral contact with a surface is important and peripheral stabilization is more important than the stabilization of the core. In relation to knee injury risk, however, Zazulak et al. (2007a,b, 2008) show that deficits in neuromuscular control of the trunk predicts knee injury, in female, but not male athletes. Forces affecting the body from foot contact up the kinetic chain are clearly important, but, so too, it appears, are centre down forces.

‘Stability’ itself is poorly understood (Reeves et al., 2007), differentiating between static and dynamic systems is important. Reeves states that ‘static stability
explanations’ account for the findings ‘that there is a potential for injury under low level loading’ (Cholewicki and McGill, 1996) and ‘that a lack of stiffness was associated with injury’ and that this led to the development of concepts of core stability. Reeves suggests that this was taken from a static understanding of spinal stability where increasing stiffness does increase stability. In a dynamic model of stability, however, there are times when less stiffness is desirable to help in the precision of motor controlled activities such as standing, balancing or gait by providing a more supple spine. The ‘central controller’ needs to exhibit a variable control and efficient feedback, and this continues in a loop. Reeves goes on to say that a system is either stable or unstable, but it is the robustness (how well the system copes with uncertainties and disturbances) of the system that is important. Reeves comments that stability is often confused with robustness, ‘Core stabilizing exercises do not make the spine more stable, they make it more robust, thus reducing risk of injury.’ Perhaps the next new hot exercise fad to take over from core stability training will be ‘core robustness exercise!’

Canada Vs. Australia

Within the world of core stability research there appears to be two schools of thought perhaps best personalised by Stuart McGill from Canada and Paul Hodges from Australia. Their work frequently references each other as it appears they both produce good science with interesting results that can be applied to the theoretical models that they propose. Both are passionate (Chaitow, 2005), and if you have to identify the key area of research for each you may possibly choose spinal biomechanics for McGill and spinal motor control for Hodges, though the distinctions between them would be blurred. Both advocate exercise regimes for the prevention or treatment of spinal pain. Some of Hodges exercises are regarded as isolationist (Siff, 2009a), this could be interpreted, and has been, according to Lederman, in that the TrA and Mf are to be exercised and strengthened in isolation. This is reminiscent of the fitness (gym) worlds isolated exercise of the ‘biceps curl’ (not truly just a biceps brachii exercise, it is the synergy of elbow flexion/extension that is actually exercised). Admittedly the TrA and Mf muscles are identified and recruited in isolation, (not strengthened!) but this immediately precedes integrating those muscles into function, so it may be fair to say ‘isolationist’ but only until functional movement is added to the regime.

McGill’s exercises reflect his opinion that “the relative contribution from every muscle source is dynamically changing” (McGill, 2007). His exercises, such as bird dog (see a description of this exercise in Liebenson’s ‘The missing link in protecting against back pain’ later in this editions Prevention and Rehabilitation section), and side bridge could be called co-contraction exercises as they recruit all muscles in the trunk, though will bias different muscle groups, and are aimed at promoting control of spinal posture in positions that are bio-mechanically sound. Liebenson (2007) reports on Koumantakis et al. (2005) study that demonstrated the “general” approach (McGill’s) was superior to the Australian “deep” local stabilization. Liebenson therefore advocates an abdominal co-contraction (bracing) exercise regime in this paper.

Tsao and Hodges (2008) in their study on subjects with low back pain, reported validating a motor control training strategy that improved TrA timing (feedforward) and maintained it for the follow up at six months. It was the first study to show such a finding. The authors took care to emphasise that the study, “does not advocate that repeated isolated voluntary contractions is sufficient to treat low back pain (LBP). Rather, the study indicates that one impairment often identified in LBP (i.e., delayed activity of TrA) can be changed with training.”

A similar study by Hall et al. (2009) (including in the authorship Tsao and Hodges), looked at the co-contraction exercises favoured by McGill and showed that a single session of this type of training did not change the feedforward timing of the TrA in low back pain subjects.

Admittedly both these studies had a low number of subjects but the findings are very interesting and may show there still is life in the theory of core stability.

It is very possible that in the future we will look back and say that McGill and Hodges’ theories both had merit and were looking at the same problem; that their views and exercises were not mutually exclusive, as the CNS has many strategies to deal with movement control. In the clinic, where distinctions are blurred, I am happy to use both commentators’ ideas at different times. I contend that McGill and Hodges, in the field of low back biomechanics and motor control, may have more common ground they agree on than ground over which they do not.

Future research

Chronic low back pain (CLBP) is a complex subject to investigate, not least because of the many various influences that may cause the pain. Hebert et al. (2008) attempted to subgroup patients with non-specific low back pain (LBP) to place them into various treatment categories including specific exercise, stabilization exercise, manipulation and traction. It appears that subgrouping LBP may help in future experimental design and provide cleaner results for interpretation.

Where is core stability research going? In fact is that the real question we should be asking? If Lederman is effective in his argument the term core stability will be phased out and research funding placed elsewhere. McGill, Hodges and others are perhaps less likely to think they are specifically undertaking core stability research but would refer to their work as research in motor control.

Core stability has really only ever been a subset of the broader church of ‘motor control’.

So where is motor control research going? It seems that the assessment of movement control is starting to show interesting results Luomajoki et al. (2008) looked at 6 motor control tests of the lumbar spine with a study size involving 210 subjects, half with LBP and the control half without. The study showed a significant difference in the ability between the groups to actively control the movements of the low back. The LBP groups control was poorer.

Roussel et al. (2009) has shown in dancers that two lumbo-pelvic movement control tests (standing bow and
a crook lying single knee lift) are predictive of injury risk to the low back or lower limb. Roussel’s findings suggest that motor control or strengthening interventions may reduce the chance of an injury happening at all.

**Conclusion**

Is the term ‘core stability’ limiting? I believe it is. Lederman’s article shows how some ideas around core stability have become part of the problem and not part of the solution, and it is definitely time to move on from there.

If this topic and this edition provokes a response from you, please email me as I would like to report back to the readership in my next editorial what your views are.

The last word comes from the Pilates Teacher Suzy Barton we met earlier. "If the public are asking for core stability exercises, don’t send them away saying we don’t do that any more, take their request and give them exercises to fit their individual issues!"

**References**


**Web sources**

http://www.youtube.com/results?search_query=clean+jerk+Swiss+Ball&search_type=f.


http://women.timesonline.co.uk/tol/life_and_style/women/diet_and_fitness/article6068862.ece.