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Transversus abdominis: a different view of the elephant



EDITOR'S CHOICE

Paul Hodges

It is good to see that clinical and research hypotheses are debated in the literature. The purpose of science is to challenge ideas and to consider alternative interpretations of observations. Within this, the place for neurophysiological/biomechanical studies in clinical research is not to predict the potential efficacy of a clinical approach, but to try to understand the mechanisms that underlie it. This is helpful as it provides a means to refine, improve, and direct intervention and provides a platform to develop rationales for intervention, particularly when we are faced with complex patients who do not fit the clinical prediction rule or the narrow criteria adopted for inclusion in clinical trials. If we understand the mechanisms we have a powerful tool to rationalise and test interventions. The developing debate about the role of transversus abdominis is healthy for rational consideration of motor control interventions for back pain.

I welcome this opportunity to comment on the opinions and interpretations of Allison *et al*¹ and Cook.² As indicated by Allison *et al* in their paper published in JOSPT,³ it is not the data that are questioned; it is the interpretation. It seems that we have a recurrence of the issue of the six blind men and the elephant, where we see the same animal, but from different perspectives, and draw different conclusions. There are a number of assumptions that require consideration to challenge the interpretation of Allison *et al*¹ and the opinion of Cook.² A key issue is that to conclude that a single observation from a single task refutes the conclusion of a whole range of different methodologies/tasks seems unfounded.

CAN PHYSIOLOGICAL DATA INFLUENCE CLINICAL EFFECTIVENESS?

In response to the editorial by Cook,² the first thing to consider is that the results of physiological/biomechanical studies cannot be used to challenge the outcomes of

clinical trials and systematic reviews. The fact that the control of transversus abdominis may not be as simple as once thought does not challenge the positive clinical outcomes from interventions that include strategies to train this muscle. It suggests that we need to take a look at the potential mechanisms for efficacy of the approach. Although Cook argues that the efficacy of the approach is "disappointing", it is worth taking stock of the current status of the literature. Most systematic reviews^{4,5} suggest that motor control training that includes training of the deep trunk muscles has a large effect size when applied to specific populations, and a reduced effect size when applied to a generic non-specific low back pain group. This is not surprising. Our challenge is to identify those who benefit most from the interventions. This is the goal of several research groups around the world and applies equally to most therapeutic interventions.

DIFFERENT VIEWS OF THE ELEPHANT

Allison *et al*¹ make a number of assumptions that require further consideration. These authors appear to assume that: (1) a muscle can only do one thing at a time, and in unilateral arm movements transversus abdominis can only contribute to rotation; (2) if the muscle does not turn on at the same time on both sides it can do nothing (despite the fact that activity is present on both sides, albeit a little later on the ipsilateral side, at the time the arm starts to move); (3) observation of asymmetrical activity in an arm movement task refutes all other data of unique activation of transversus abdominis during tasks such as walking, trunk movements and trunk perturbations (this data was not mentioned by Allison *et al*¹); and (4) clinical approaches are restricted to teaching patients to activate transversus abdominis bilaterally.

TRANSVERSUS ABDOMINIS CAN ACT TO ACHIEVE TWO GOALS AT ONCE

In response to the first assumption that a muscle can only do one thing at a time, there is an abundant literature that shows

that muscles can be activated to achieve two or more goals concurrently. It has been well documented that transversus abdominis is active asymmetrically during trunk rotation.⁶ How transversus abdominis contributes to axial rotation is unclear for a number of reasons. First, transversus abdominis is active with both directions of rotation (but greater when rotating the thorax towards the side of the muscle), and second, the muscle has a trivial moment arm to generate rotation torque.⁷ The contribution of the muscle to rotation may relate to control of the linea alba, while the contralateral obliquus externus (OE) and ipsilateral obliquus internus (OI) contribute most to the torque.⁶ Thus, the finding by Allison *et al*¹ that transversus abdominis is an axial rotator is not new. Furthermore, in an earlier study that combined modelling of reactive trunk moments and measurement of trunk muscle EMG it was argued that the timing of transversus abdominis may be linked to the control of axial rotation.⁸ The problem is that Allison *et al*¹ have assumed that if transversus abdominis is active to control rotation then the muscle cannot do anything else. We have documented in a number of experiments that this is not the case and the nervous system can coordinate the activity of muscle to achieve multiple goals concurrently. In some simple examples we have shown that activation of transversus abdominis involves multiple components. For instance, transversus abdominis activity is tonic during gait (perhaps to contribute to some aspects of spinal control), but this activity is phasically modulated in association with breathing (to assist expiration), and has peaks of activity associated with heel strike events (which are coordinated with periods of peak reactive force from the foot contact and the time of change in direction of rotation of the trunk).⁹ Similarly, activity of the diaphragm¹⁰ and pelvic floor muscles,¹¹ during repetitive arm movements, includes tonic activation as well as phasic activation with movement and breathing. Recent data show that the diaphragm, like transversus abdominis, is involved in axial rotation of the trunk.¹² The fact that transversus abdominis is active earlier on the contralateral side (but bilaterally by the time the movement starts) may simply reflect the mechanical demand to both influence axial rotation (including the control of rotation, which is an aspect of stability) and contract bilaterally to contribute to

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control of multiple mechanical demands on the trunk.

ASYMMETRICAL ACTIVITY OF TRANSVERSUS ABDOMINIS IS STILL MECHANICALLY USEFUL

The second assumption is that if the contraction is not symmetrical it cannot do anything for the spine. But, as mentioned above, Allison *et al*'s¹ data show that, while the onset was not simultaneous, the muscle was active on both sides in a feedforward manner (i.e. active before any feedback could be available to induce activation of the muscle (~50 ms after the onset of deltoid EMG))¹³ and both sides were active at the time of onset of deltoid EMG (well before the movement started and well before any reactive forces would affect the spine). Although Allison *et al*¹ argue that the lack of activity of transversus abdominis on the other side or activity of the rectus abdominis (RA) would limit the potential for transversus abdominis to generate force, this is not what their data show, as transversus abdominis on the other side is active by the time the movement starts and may well contribute to spinal control in spite of a later onset. Furthermore, direct measurement of intra-abdominal pressure (IAP) clearly demonstrates that there is a mechanical output of the muscle contraction before the arm moves.⁸⁻¹⁴ In-vivo¹⁵⁻¹⁶ and modelling¹⁷ studies show that IAP contributes to spinal control. Other studies show that transversus abdominis is the abdominal muscle most closely correlated with IAP changes.¹⁸ Modelling studies that suggest that transversus abdominis does very little for spinal stability¹⁹ consider only the role of transversus abdominis as a flexor (for which it has a trivial moment arm). This ignores the biomechanical data that show that transversus abdominis can contribute to spinal control via IAP or fascial tension.²⁰⁻²¹ Both human and animal studies show that activation of transversus abdominis has a mechanical effect on the spine and the pattern of activation of the muscle in Allison's data is not inconsistent with that assumption. What that data shows is that, in addition to the bilateral activation at the time the movement starts, it also has activity that could be consistent with an additional role in control of axial rotation.

An issue that is accurately indicated by Allison *et al*¹ is that there are no data to show that the spine is less optimally controlled when activation of transversus abdominis is changed. This is challenging to test in humans (because it is difficult to

take the muscle away) and cannot be tested in current biomechanical models (as few include the contributions of IAP and fascial tension). Animal studies are underway to test the effect of reduced deep muscle activation on spine biomechanics and it is hoped that this will shed light on this issue.

IT'S NOT JUST ABOUT ARM MOVEMENTS; DATA FROM OTHER METHODS SUPPORT THE ROLE OF TRANSVERSUS ABDOMINIS IN SPINAL CONTROL

The third issue relates to Allison *et al*'s¹ and Cook's² failure to consider the wealth of data from numerous groups using other experimental designs. Arm movement tasks provide a window of opportunity to study the system, but there are many other models which have provided insight into the function of transversus abdominis. In trunk movements,²² isometric trunk tasks,²² trunk perturbations in sitting²³ and lying,²⁴ transversus abdominis is active in a manner that is unique amongst the trunk muscles, that is, it is active with forces and movements in opposite directions in the sagittal plane. These tasks do not include an axial rotation component and that may make the interpretation easier (as it does not involve the functional requirement to combine activity for rotation with other aspects). These observations and the interpretation of unilateral flexion and extension movements raise the question: why does the nervous system use a muscle in a similar manner with two opposite directions of movement? I concur with Allison *et al*¹ that this does not necessarily mean that it contributes to spinal control. That is simply a hypothesis that we have gone on to test in a number of biomechanical studies, with results that show the activation can control spinal motion.¹⁵⁻²¹ To conclude that a single observation from a single task refutes the conclusion of a whole range of different methodologies seems unfounded.

IS THE DEBATE ABOUT TRANSVERSUS ABDOMINIS MISSING A CRITICAL ISSUE?

Having highlighted some of the assumptions made by Allison *et al*¹ and Cook² it is also worth considering some issues in the clinical literature as a whole that have spawned some of this current debate. In my view the whole debate around transversus abdominis is missing a critical issue. Back pain is not an issue of a single muscle, it is associated with complex changes across a whole system.

Although the early studies focussed on this muscle,²⁵⁻²⁶ an abundant literature has evolved that shows that the changes in back pain are complex and involve many muscles and many control properties. One of the factors that have perpetuated the confusion is that, although the changes in transversus abdominis appear to be relatively consistent,²⁵⁻²⁶ the changes in the other muscles are variable,²⁷⁻²⁸ and therefore harder to find in a non-specific pain population. Recent work even suggests that many people with back pain may have increased stability rather than decreased stability,²⁹ potentially as a result of increased activity of the more superficial trunk muscles, and this puts a whole new perspective on the meaning of optimal spinal control; not simply to increase stability, but to find a balance between too much and too little.³⁰ It is increasingly clear that rehabilitation should not target a single muscle, but instead should involve careful evaluation of a whole system. While changes in transversus abdominis (and other muscles such as multifidus) can be a useful marker of dysfunction in the system (and recent data show that patients with delayed transversus abdominis do better with a motor control training approach than people without a delay) (unpublished data), to limit treatment to this muscle is unlikely to be beneficial. The days of contracting transversus abdominis as the primary exercise and then sending the patient away are over. Instead, training of transversus abdominis should be part of the intervention, when appropriate for the patient and the changes in their control system.

Cook² argues that training transversus abdominis bilaterally may be redundant as that may not be the way the muscle functions. Although this may not always be the case, it is likely to be so in some tasks. But evidence that the muscle is not symmetrical (although bilateral) cannot be used to say that bilateral training is not effective or appropriate. There is a developing literature that shows that training muscles in this way changes the control of the muscle in other tasks. Not only does it change the timing of activation of transversus abdominis in arm movement and gait tasks³¹⁻³²; it also changes the organisation of the motor cortex³³ and this change is related to the change in timing during an arm movement task. Notably, this was only achieved by cognitive bilateral activation, and not by simple activation as part of a sit-up³¹ or other abdominal bracing manoeuvre.³⁴ Furthermore, focussed attention on activation of the deeper muscles

can also change the activation of many muscles of the trunk.³⁵ These data suggest that training of bilateral activation is an effective training stimulus to change the way the muscle is activated in function, despite the fact that this may not be the only way it is active in function. This principle of a training stimulus that does not reflect every function is true for many exercise approaches. For instance, eccentric loading is effective in management of tendinopathy, but this is not the only way those muscles function.

WHY ARE MOTOR CONTROL INTERVENTIONS SUCCESSFUL IN TREATING BACK PAIN?

Finally, I agree with Cook² and Allison *et al*¹ when they argue that we do not know why motor control interventions are effective. We don't know that the effect is explained by increased stability of the spine due to activation of transversus abdominis and other deep muscles. Core work by our group has focussed on this very issue over recent years. In a series of studies we aimed to evaluate the potential mechanisms for efficacy of a motor control approach to the management of neck pain. These studies showed that the motor intervention not only changed the control of the deep neck flexor muscles (Jull *et al*, unpublished data), but was also associated with improvements in posture³⁶ and neck proprioception.³⁷ In terms of the back, recent data in a small clinical trial suggest that the intervention can reduce the muscular stabilisation of the trunk by reducing activity of more superficial muscles.³⁵ This could suggest that the approach leads to more optimal control. This is being followed up in a large randomised controlled clinical trial. There are many candidate mechanisms and we need to keep an open mind to make sure that we do not miss the wood for the trees. While we have investigated the potential role of optimisation of control of the spine by changing the activation of the system of trunk muscles, including transversus abdominis, the truth is likely to be more complex. That is the joy of science, to hypothesise and then challenge new ideas. The ultimate goal is to understand so that we can identify better treatments.

THE CHALLENGE FOR THE FUTURE

In summary, the data provided by Allison *et al*¹ add richness to our understanding of the control of the deep muscles and ultimately the control of the trunk. As highlighted above, the data

do not refute the original hypotheses of the role of transversus abdominis in trunk control; in fact, they are very congruent with the evolution of our understanding of the function of the deep muscles. The basic observations from the early studies that were conducted 15 years ago provided a starting point.²⁵ The subsequent data have shaped and evolved the interpretation. We all agree that clinical practice often adopts research findings in a simplified and hardline approach. Allison *et al*'s¹ data do not refute the viability and potential efficacy of the approach. Current literature suggests that the clinical application of the findings is beneficial. In an ideal world the experimental testing of an idea would be completed and all issues resolved and understood before implementation into practice, but this is not practical as nothing would ever be implemented. And, after all, research that has a clinical application must be done in an iterative manner with communication back and forth between clinicians and researchers. In that way clinical practice can inform research and research can be accurately implemented into practice. The challenge for us all is to keep our blinkers off and keep an open mind when looking at our data and looking at patients so that we have a chance to move forward.

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Why glucocorticoids should be removed from the World Antidoping Agency's list of banned products

John W Orchard

Sports medicine clinicians and researchers should all be familiar with the concepts of false positives and false negatives. Research to test a hypothesis about a link between, say, a risk factor and a disease can potentially be wrong in either of two ways. The findings might falsely show a link when in reality one does not exist (a type I or α error), or they might fail to show a link when there really is one (a type II or β error).^{1,2} Hopefully most of the time, if studies are well conducted, the likelihood of both of these errors is reduced.

Similar errors in both directions can potentially occur in drug testing in sport, although the nature of false positives and false negatives is somewhat different from that in other clinical testing. The rigorous methods of collection and the use of "A" and "B" samples mean that many sources of potential laboratory error are minimised. The false positive and false negative phenomena in doping may be better referenced to the athlete's *intent* to cheat using performance-enhancing drugs. An athlete who takes a so-called "undetectable" anabolic steroid is a true "drug cheat", but one who might produce a "false-negative" drug test because the structure of the undetectable drug is not yet known by the testing authorities. By comparison, the athlete who inadvertently takes a banned drug (particularly one with minimal performance-enhancing potential) not to cheat, but to treat a legitimate medical condition, may test

positive in a doping test. Such a result may be considered a "false positive" with respect to intent to cheat using a performance-enhancing drug, even though the testing process was accurate in finding the drug in the athlete's system.

Intent to cheat is so difficult to prove or disprove that WADA takes a pragmatic approach and enforces strict liability for all positive tests.³ Strict liability means that denying intention to cheat is not relevant if the results of a drug test are positive. If the excuse of lack of intention was generally accepted, most true cheats would deny intent and many would escape prosecution as a result. History suggests, though, that there are some drug suspensions which were probably false positives with respect to intent to cheat. Perhaps the first such case was Rick DeMont of the USA, who lost a swimming gold medal after apparently being prescribed ephedrine by his team doctor to treat asthma at the Munich Olympics in 1972. A similar case saw Andrea Raducan of Romania stripped of her Olympic gold medal in gymnastics in 2000 after a positive test for pseudoephedrine, apparently taken as a medication to treat a cold.

Babette Pluim has recently highlighted cases of suspected false positives from the sport of tennis.⁴ Alarming, it was calculated that as many as 68% of the doping charges in tennis over the previous 5 years were false positives.⁴ Glucocorticoids were one of the major classes responsible for the suspected false positive cases. Because glucocorticoids are extremely commonly used in general medicine and have not been shown to

enhance performance in humans,⁴ it can be strongly argued, using Bayes' theorem, that this drug class is particularly likely to produce false positive results.

Bayes' theorem, developed centuries ago by an English clergyman, is a formula to calculate "positive predictive value" (the likelihood that a positive test actually represents a true positive).¹ The numerator is the number of true positive cases with the denominator being the sum of both true positive and false positive cases. One of the principles of Bayes' theorem is that the likelihood of a positive result being a true positive is proportional to the prevalence of the condition being tested for in the sample population. The principles of Bayes' theorem are used for screening in other areas of medicine, like cancer detection.⁵ Mammograms, for example, are recommended for postmenopausal women, but not for younger women. This is because breast lesions detected by mammogram in older women are somewhat likely to be malignant (because the prevalence of breast cancer is relatively high). By comparison, breast lesions in younger women are extremely likely to be benign (because the prevalence of breast cancer is very low). It is generally calculated by screening experts that a mammogram performed in a young woman is much more likely to cause harm (by falsely identifying a suspicious lesion which is, in fact, benign) than it is to lead to benefit (by identifying a suspicious lesion which is a true malignancy).⁵ As women get older and breast cancer becomes more likely, then the value of screening tests, such as mammograms, increases. In sports medicine, Bayes' theorem has been used to argue against routine ECG screening of asymptomatic young athletes.⁶

Bayes' theorem as it applies to drug testing can be demonstrated by considering a theoretical population of 10 000 elite athletes and two theoretical drugs which we can call "G" and "A". Drug "G" is a glucocorticoid and is commonly used to treat medical conditions such as asthma and sinus congestion. Therapeutic use exemption (TUE) is available for athletes

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