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Response to Manual Magic: The Method Is Not the Trick

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Manual Magic: The Method Is Not the Trick

The connection between manual care and magical performance is something that has interested us for many years, and we appreciate the allusion to their association by Mintken and colleagues.[8] However, very rapidly in the body of the guest editorial, the analogy falls apart when understood from the perspective of those who are familiar with manipulative practice, the relevant literature, and actual magical performance.

Unfortunately, the editorial's authors quickly abandon their magical allusion except to say at the end that "Manipulation is much more than sleight of hand. It is separate and distinct from a mobilization in both delivery and effect."

One of the world's premier practitioners of sleight-of-hand is Jamy Ian Swiss. He emphasizes that "the method is not the trick" and makes this point repeatedly in regard to both Three Card Monte and the Ponzi Scheme.[10] Neither would be successful without the surrounding drama carefully orchestrated and controlled by the operator. In short, the maneuver as revealed or studied in isolation is of no significant consequence without the plethora of distractions created within the "play" the target enters. In magic, this is the "secret" known by practitioners to make their effects "work."

Perhaps it is the surrounding drama that seems to make manipulation effective, and careful study would seek to eliminate the very thing that leads to this. However, a true understanding of the deep model of pain, best exemplified by Melzack's neuromatrix model,[7] would explain how these studies may lead to confusion and disagreement. When we don't study or consider what is happening within structures we cannot see, our best guess is no better than that of an audience member at any magical performance. Until all of us in the manual therapy community embrace today's neuroscience, this will be the case. However, we can do better.

The success of a sleight depends on many things, most of them far less obvious than the method itself. Similarly, the evident success of manual care for pain depends on aspects of the therapeutic milieu that may prove impossible to see clearly or eliminate entirely.[2,9] It is only when we understand the mechanism of effect that we can say with some measure of certainty why a method did or did not produce the expected outcome. We hope that the search for that mechanism remains at the forefront of our efforts.[4] If it does, methods concurrent with that understanding will naturally emerge.

In their guest editorial, the authors argue that some conclusions drawn regarding the effectiveness of manual therapy are erroneous for 2 reasons: imprecise terminology and the absence of clarification when describing "mobilization" and "manipulation." They state that "the rate of force application provides the necessary means to distinguish between the 2 techniques." We would ask, aside from speed, what additional distinctions would you cite? If, in fact, the effect of manual care (manipulation included) can be assigned to the consequent neurophysiologic change,[2] what is the significance of speed aside from its drama?

Mobilization is a term that can include techniques used with varying degrees of vigor and at different locations in the resistance range. Certainly, many can be performed at the end of the available range of motion and can involve forces similar to manipulation, without the higher rate of force application or thrust component. We have to ask, if speed alone is the issue, what theoretical rationale or base of evidence exists for that?

We already have evidence that factors other than the speed of movement might influence the outcome of manipulative care,[3] findings perfectly congruent with modern neuroscience and current neurophysiological explanatory models. Further confounding the study of manual care is the use of marked differences in patient-therapist positioning and context when directly comparing mobilization to manipulation,[5] especially when performed on a group of patients thought to benefit from manipulation. Regarding the relative strength of evidence between mobilization and manipulation, the authors mention that there is evidence that "a mix of low-velocity and high-velocity techniques, chosen pragmatically by the provider, is not effective." We would like to point out that there is also contrary evidence supporting just such a pragmatic selection of techniques[6,10,11] indicating that a distinct effect based on rate of force application may not exist—or at least that the current evidence is mixed.

Despite the mixed evidence, the literature seems clear that while we do not fully understand the mechanisms of action of these interventions, there are many important factors that are part of the therapeutic encounter besides the speed of movement of 1 or more techniques applied. Additionally, neither clinical trial results nor the current mechanistic explanatory models appear to provide support for the concept that speed of movement or rate of force development is the key to outcomes in manual therapy. We feel that the complexity of the encounter, the nature of pain, and the state of the evidence should not be lost in the (certainly laudable) desire to standardize our terminology.

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RESPONSE

We appreciate Dorko and Silvernail's final comment noting the importance of the need to standardize our terminology. That was certainly the focus of the editorial.[4] A controlling assumption of this editorial and our previous clinical guidelines on the same subject matter[3] is that, in developing the precise language needed, it is important to avoid theoretical assumptions regarding mechanism or intentions, so as to remain useful and timeless as theory and science evolve. One only has to look at our recent professional history regarding an almost exclusive use of the biomechanical model to explain mechanism of action to understand the potential pitfalls with such an approach. While a neurophysiological mechanism is certainly enticing,[1,2] the effects of high-velocity manipulation are more likely multifactorial, as Dorko and Silvernail note.

While it is tempting to jump into the discussion on mechanisms of action, to do so would be to distract the reader from the main point of our editorial. The issue we raised was related solely to the description of techniques. Although examples of studies that investigated the effects of mobilization and manipulation were cited, this was only to point out the consequences of using imprecise language. The fact remains (and the intent of the editorial) that ultimately being able to truly clarify the mechanisms of action of a specific manual therapy intervention will remain elusive without a precise language to describe the application of the intervention. Without such language, we will remain affixed to explanations centered on biological plausibility instead of actual scientific discovery.

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Moving Past Sleight of Hand

We would like to comment on Mintken and colleagues recent editorial "Moving past sleight of hand."[7] The central theme in the editorial was the problems caused by the lack of precision in language used to describe manual therapy techniques. We support the need for clear descriptions of manual therapy

techniques used in trials. Our papers were held up as examples of sleight of hand where language was deliberately used to obscure reality. However, the criticisms of our papers relied upon misquotation.

Our Lancet trial[5] was criticized for using the term "spinal manipulation," obscuring the reality that the trial used both high- and low-velocity techniques. This would be a great basis for an argument, if it were true. In fact, we used the term "spinal manipulative therapy" in the Lancet paper,[5] not spinal manipulation, and we made very clear in the trial report,[5] trial protocol,[4] pilot study,[2] and trial registry that practitioners could use both low- and high-velocity techniques. The only sleight of hand here is in the editorial.

Our use of the term "spinal manipulative therapy" to include high-velocity and low-velocity techniques is common practice. It is used by the Cochrane Back Review Group,[1] for example. It was also used by the authors themselves in their 2008 paper entitled "A Model for Standardizing Manipulation Terminology in Physical Therapy Practice."[7]

We are also told that a specific high-velocity technique is effective for the management of back pain but not a pragmatic application of a mix of low-velocity and high-velocity techniques. We are told the evidence to support this is "high level," but, surprisingly, the authors only cite 6 studies. More worrying is that the high-level evidence cited in support of the specific high-velocity technique comes down to 3 discrete studies: 1 randomized controlled trial, 1 cohort study, and 1 case series. Total number of patients in the 3 studies was 214. That does not seem like high-level evidence as most scientists would use the words.

More typically, people use the term *high-level evidence* to refer to a systematic review of randomized controlled trials,[6] with Cochrane reviews[9] being usually of higher quality. The Cochrane review[1] of spinal manipulative therapy for low back pain summarized 39 randomized controlled trials studying 5486 patients. The authors found no evidence for the superiority of specific high-velocity techniques over other forms of spinal manipulative therapy.

The second sentence of the editorial states, "If you are not careful, or if you blindly believe without questioning, the result is that you will be taken in..." Those seem like very wise words.

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RESPONSE

We thank Drs Maher and Hancock for their comments and appreciate the opportunity to respond. We wish to clarify one important aspect. At no time was the intention to disparage their previously published papers, nor to suggest that the authors deliberately obscured reality. Instead, we proposed that the lack of precision in language describing the techniques in these studies[2,3] ultimately contributes to the misinterpretation of the results, or the interpretation of the results had the potential to vary greatly among readers.[6] We stand by this.

In reality, neither the phrase "spinal manipulative therapy," nor "spinal manipulation" is sufficiently accurate to describe precisely what occurred in the trial. The authors clearly state that the use of the phrase "spinal manipulative therapy" was meant to include high-velocity and low-velocity techniques.[2,3] And we would certainly agree that the use of terminology in this way is occurring. This is precisely the reason for the editorial,[6] and our original clinical guidelines aimed at standardizing our

terminology in this area.[5] Whether use of terminology in this way is common practice or not was not the issue that was raised. The larger question raised is whether we can afford to continue to be so general in an era in which evidence is highly sought and interpreted by referral sources and policymakers, and, perhaps most importantly, by clinicians, to allow interventions for patient care to be duplicated with reasonable precision to lead to their incorporation into clinical practice. Clearly standardizing the medication intervention while leaving the "spinal manipulative therapy" up to the whim of the therapist[2,3] makes it difficult to make conclusions about the outcomes of the patients receiving "spinal manipulative therapy." In many ways, the real driver for this editorial was to raise the point that, unless the language is more precise and universally understood regarding interventions such as high-velocity manipulation, it will never be possible to reproduce or retest clinical trials. The methodology will always be unique for the individual study, which makes it nearly impossible to duplicate in subsequent studies or generalize to the larger patient population.

Finally, Maher and Hancock correctly use the term "high-level of evidence" on a hierarchy of evidence as developed within the concept of evidence-based medicine (EBM). The systematic review is the highest level of evidence.[7] However, recent developments in the identification of subgroups responsive to treatment in management of low back pain and in back pain research has been acknowledged as vital work.[4,8] Evans[1] states that "the risk with available hierarchies is that, because of their single focus on effectiveness, research methods that generate valid information on the appropriateness or feasibility of an intervention may be seen to produce lower-level evidence." He goes on to argue that multicenter randomized controlled trials may "provide the best evidence for the effectiveness of an intervention, because the results have been generated from a range of different populations, settings, and circumstances."[1] That said, we agree with Maher and Hancock that much work still needs to be done in this area.

We appreciate the opportunity to promote and facilitate this much needed discussion. An intervention used as frequently as spinal manipulation should have a much higher degree of descriptive clarity if we are to truly interpret the results of its use.

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Core Muscle Activation During Swiss Ball and Traditional Abdominal Exercises

In response to the article published in May 2010 issue of the *JOSPT*,[1] titled "Core Muscle Activation During Swiss Ball and Traditional Abdominal Exercises," we have concerns with the use of the term "core" and thus question the validity of this particular study with regard to the use of this term. The area of the body studied and, therefore, the exercises themselves are ill-defined. There is no clear data as to functional relevance.

The term "core" is commonly thrown around by fitness enthusiasts and medical professionals. But with all this talk about the "core," "core stability," and "core strength," no one has yet provided a solid definition of what constitutes the core or why the core is so critically important. We owe it to ourselves as specialists in biomechanics to come to a consensus agreement with regard to discussions of the core musculature.

As the authors stated, "core" is often defined as the "lumbopelvic-hip complex." We are left to assume that the authors are using the 2 terms interchangeably, because there is no more-specific description of how "core" will be applied within the context of their study. However, both "core" and "lumbopelvic-hip complex" are relatively ambiguous terms. For example,

within physical therapy and medical literature, we have accepted definitions for what constitutes the shoulder complex and trunk musculature. In contrast and dependent upon the source, the muscles included in descriptions of the lumbopelvic-hip complex vary. The authors of this article never provide the reader with a definition of the specific body area to be studied, a reason why that area is of particular interest, or the reason for including or excluding particular muscles within the region of the hip, lumbar spine, and pelvis. We know that the iliacus attaches to both the ilium and the femur, although this muscle is not included in the authors' description of core or lumbopelvic-hip complex. While we understand that the iliacus is not a muscle easily accessible by the surface electrodes employed here, we are still left without a clear definition of what core specifically references in this study.

In discussing the reasoning behind this current study, the authors add to the above confusion by drawing conclusions about the core from articles that clearly reference the trunk. The current use of the terms "core" and "lumbopelvic-hip complex" are not necessarily interchangeable with our accepted definition of the trunk. While it appears that the hip has been included, the thoracic spine and cervical spine have been excluded. There appears to be evidence provided for improved function of the trunk, but not necessarily of the core.

Further, the exercises listed engage far more muscles than those tested and are less specific than we are led to believe. We are quite sure that if we examined the EMG readings for other muscles, such as the trapezius, serratus anterior, or even gastrocnemius/soleus, during many of these exercises we would get high activity readings as well. Thus, we believe that these exercises are less accurately labeled as core exercises and more appropriately described as exercises for static and dynamic trunk stabilization, coordination, and abdominal strengthening.

To summarize, we believe that we must be clear not only amongst ourselves, but across disciplines in our use of the term "core." This term is thrown about too freely in both scholarly literature and articles for the general population, but, as yet, there is no clear consensus agreement on what constitutes core musculature. The authors in this study could have greatly helped their cause by defining the specific muscles to be studied. Without a more refined and consistent definition of what constitutes the core, we perpetuate the ambiguity of this term and place in question the relevance of research purportedly completed to study a specific region of the trunk and associated musculature. At a time in our profession when we need to direct our efforts towards greater effectiveness and functional outcomes, we owe ourselves clearer communication regarding the understanding of biomechanical function/dysfunction. This goal is helped considerably by speaking in terms that are well defined and that are not based on phraseology taken from the popular press.

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RESPONSE

The purpose of our paper[3] was to assess core muscle activity during Swiss ball and traditional abdominal exercises. As a representation of what constitutes core muscles, we referenced a common definition from the scientific literature, which defined the core as the lumbopelvic-hip complex.[1,12] In our paper we provided specific examples of some of the primary muscles that comprise the core, which include both superficial and deep muscles that span the lumbopelvic-hip complex, such as the external and internal obliques, transverse abdominis, rectus abdominis, transversospinalis muscles, erector spinae muscles, quadratus lumborum, gluteus maximus and medius, hamstrings, iliopsoas, rectus femoris, etc. It was not the intent of our paper to study each and every one of the more than 30 muscles that comprise the lumbopelvic-hip complex, but rather to assess muscle activity from a sample of these core muscles commonly used during traditional abdominal exercises (ie, sit-up and crunch), and compare these EMG patterns to the EMG patterns during select Swiss ball exercises. This is why we specifically chose trunk and hip flexor muscles, such as the rectus abdominis, external and internal obliques, and rectus femoris, as these muscles have been shown to be active during traditional abdominal exercises such as the sit-up.[4] We did not choose the transverse abdominis and many other deeper core muscles because in this initial study we selected only core muscles for which we could measure muscle activity with surface electrodes.

In their letter, Wilk et al argue that no one has yet been able to provide a solid definition of what constitutes the core or

why the core is so important; but these statements are not supported in the scientific literature. Several papers have demonstrated that deficits in neuromuscular and proprioception control of the body's core (defined to include the same muscles and segments as we provided in our paper) can lead to uncontrolled trunk displacement during athletic movement, which in turn positions the lower extremity in a valgus position, increases knee abduction motion and torque, and results in high knee ligament strain and anterior cruciate ligament injury risk.[5,13,14] Control of the lumbar spine is very important in controlling this trunk displacement, because a large percentage of trunk flexion, extension, and lateral flexion comes from the lumbar spine. Moreover, the lumbar spine is intrinsically associated with pelvis and hip movement, which again illustrates why we chose the lumbopelvic-hip complex to define the core. For example, several researchers have reported that neuromuscular control of the hip is required to control coronal plane trunk and pelvis motion, and a deficit in hip control can lead to an increase risk of anterior cruciate ligament injuries.[5,13,14] Moreover, Powers[10] reported that there exists a growing body of literature that demonstrates that a lack of hip control is associated with knee injuries. Both biomechanical and clinical studies have demonstrated that impaired muscular control of the hip, pelvis, and trunk can affect tibiofemoral and patellofemoral joint kinematics and kinetics in multiple planes, and that motion impairments at the hip are associated with multiple knee injuries, such as anterior cruciate ligament tears, iliotibial band syndrome, and patellofemoral joint pain.[2,5,8-10,12-14] Data from these papers provide a strong biomechanical argument for the incorporation of pelvis and trunk stability, as well as dynamic hip joint control, into the design of knee rehabilitation programs.[2,10] Stability and muscle recruitment patterns of the "core" have also been demonstrated to be associated with low back pain. For example, core stability exercises have been demonstrated to be effective in chronic low back pain,[6] and abnormal recruitment patterns from lumbopelvic-hip musculature have been shown to be associated with sacroiliac joint pain and low back pain.[7,11]

In their letter, Wilk et al also state that "the exercises listed engage far more muscles than those tested and are less specific than we are led to believe," and then stated that if we would have tested the EMG for other muscles, such as the trapezius, serratus anterior, and gastrocnemius-soleus complex, we would have likely found high EMG readings. While this is likely true, the purpose of the paper was not to assess the EMG from scapular or calf muscles, but rather from core muscles. Undoubtedly the intrinsic muscles of the hands and feet are also active during many of the exercises used in the study; but, as they are not core muscles, they were outside the stated purpose of the study. In other words, the purpose of the study was not to assess muscle activity from every muscle that we thought could possibly be active during the exercises we performed. Nor were we trying to answer all questions related to the core in our paper. We specifically assessed a few select core muscles in terms of muscle activity between traditional abdominal exercises and select Swiss ball exercises.

Wilk et al argue that there is no clear data as to functional relevance of our study. However, a key point of functional emphasis in the paper is that the biomechanical mechanism of abdominal recruitment was quite different between Swiss ball exercises and the sit-up and crunch. The sit-up and crunch recruit abdominal musculature by flexing the trunk against gravity (concentric muscle action), while most of the Swiss ball exercises recruited abdominal musculature by resisting trunk extension due to the effects of gravity, while maintaining a neutral spine (isometric muscle action). These Swiss ball exercises provide alternatives to the traditional trunk flexion exercises such as the sit-up and crunch, which are contraindicated in some patient populations, such as those with osteoporosis and certain lumbar disc herniations. In these circumstances, performing appropriate Swiss ball exercises while maintaining a neutral spine may be desired. Also, in addition to producing high abdominal muscle activity, several Swiss ball exercises also produced minimal hip flexor activity from the rectus femoris, which may be helpful in those with low back pain, because increased hip flexor activity (such as during a sit-up) may increase lumbar lordosis and pain in those with lumbar pathology. Moreover, unlike the traditional sit-up and crunch, several of the Swiss ball exercises produced moderate amounts of latissimus dorsi activity, which, via its insertion into the thoracolumbar fascia, helps stabilize the core. We also demonstrated in the paper that there are numerous Swiss ball exercises that recruit the abdominal musculature as or more effectively than the more traditional crunch and sit-up. Several other clinically relevant findings were reported in our paper. For example, the Swiss ball prone hip extension exercises, which are commonly used to develop important core muscles such as the gluteus maximus and hamstrings, also produced as much or more abdominal muscle activity compared to the crunch and sit-up. A clinician can use this and other information from this study as a way to use nontraditional Swiss ball exercises to strengthen select core muscles.

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