

# Re: “Upper Limb Neural Tension and Seated Slump Tests: The False Positive Rate Among Healthy Young Adults without Cervical or Lumbar Symptoms” Daves et al. *J Man Manip Ther* 2009;16:136–141

It was of great interest that I read the recent article by Davis et al<sup>1</sup> which questions the clinical validity of the seated slump test and upper limb neural tension test (median nerve), two commonly used clinical neurodynamic tests<sup>1</sup>. What ignited my interest was that this study employed a methodology which attempted to determine the ratio of false-positive test findings with definitions that do not adequately reflect the true intention of these neurodynamic tests.

Clinically neurodynamic tests assess the mechanosensitivity of neural tissue<sup>2</sup>. Neurodynamic tests utilize established sequences of movements to either stress or relieve the nervous system in such a way as to alter, albeit temporarily, the mechanics (i.e. ability of the nerve to withstand compression, glide, stretch) and/or physiology (i.e. localized ischaemia, alterations in intra-neural pressure) of that particular neural tissue<sup>2,3</sup>. Each test has a number of options of ‘sensitizing movements’ which are a “test component that preferably has no direct structural link with the symptomatic area except by means of the nervous system”<sup>4</sup>. These sensitizing movements therefore attempt to differentiate whether the symptoms that are reproduced during the test occur through provocation via alteration of the nervous system versus other, related and neighboring soft tissues<sup>3,9</sup>. This concept of neural sensitization, and therefore structural differentiation, has been widely explored in the literature.

It is important to note that although neurodynamic tests can provide information regarding mechanosensitivity and differentiation between neural and non-neural tissues, the definition of a *positive* neurodynamic test, clinically, should not be made on structural differentiation alone. Butler<sup>3</sup> defines a positive

neurodynamic test if “it reproduces symptoms, plus structural differentiation supports a neurogenic source, plus there are differences left to right and to known normal responses, plus there is support from other data such as history, area of symptoms, imaging tests”<sup>7</sup>. Shacklock<sup>8</sup> has developed a clinical algorithm to attempt to simplify and add clarity to the interpretation of neurodynamic tests. Integral to his algorithm is the distinction between *normal neurodynamic responses* and *abnormal neurodynamic responses*. As they deliberately load the neural tissue, it is to be expected that neurodynamic tests will evoke a neural response. In the absence of what Shacklock<sup>8</sup> refers to as *overt neurodynamic symptoms* (i.e. those symptoms that the patient complains of which are present on testing) any neural symptoms that are elicited in routine testing would be considered a *normal neurodynamic response*. These symptoms are often similar to that of the contralateral limb and as such should not be considered to be indicative of neurodynamic pathology and therefore should not be rated as a *positive* neurodynamic test. This is in support of the previous definition from Butler<sup>3</sup>.

Although Davis et al<sup>1</sup> have acknowledged the distinction that Shacklock<sup>8</sup> makes between an *overt abnormal neurodynamic response* and a *normal neurodynamic response*, they go onto define a *positive* test for their study “using structural differentiation as the criterion”<sup>1</sup>. Essentially the authors are happy to assign a positive finding to a neurodynamic test that shows structural differentiation. It is surprising that, based on this definition of a positive test and given the healthy subject population, the rate of false-positives was not 100% given that normal neurodynamic responses are to be ex-

pected when progressive load is imposed on the neural tissues, such as that with neurodynamic testing.

It is vital that the interpretation of neurodynamic testing must take into account the symptoms and presentation of the patient. Many experts in the field of neurodynamics have clearly stated the importance of the reproduction of a person’s symptoms, which implies the presence of pathology<sup>3,8,10,11</sup>. Therefore clinically, it would be flawed to suggest that a neurodynamic test is to be judged either as positive or negative based on structural differentiation. Unfortunately this is exactly what Davis et al<sup>1</sup> have done in defining a positive neural tension test, based solely on structural definition.

The other feature which is vital to the interpretation of any neuromusculoskeletal clinical measure is the comparison between sides (i.e. for neurodynamic testing, comparison between limbs). This study sought only to assess the left side. During neurodynamic assessment no inference can be made as to whether a clinical test is positive or negative unless bilateral comparison is made. This lack of comparison would surely increase the likelihood of a false-positive test for any clinical measure, particularly in light of the fact that healthy subjects were examined. Davis et al<sup>1</sup> do acknowledge that this situation is a limitation of the study. Further to this point, if claims are to be made about the clinical validity or usefulness of neurodynamic tests, then the fact that bilateral comparison was not made should have forced the methodology to be changed to incorporate this very important process. This being the case any claims regarding clinical validity must be debated.

The use of the term *false-positive* would imply that a clinical test is found to

be positive, thus implicating the presence of a condition or diagnosis, where in fact the condition does not exist. To conduct a study to specifically assess the ratio of false-positive findings for a clinical test in a population of healthy subjects appears to be an unfair witch-hunt. Surely a study conducted to try to establish true-positive results and therefore attest to the strength of clinical validity in a symptomatic group (compared even to a healthy population) would seem a much more robust methodology. With this type of design, the ratio of false-positive rates to true-positive findings could still be assessed.

I think the negative comments that Davis et al<sup>1</sup> make in respect to the clinical validity and usefulness of neurodynamic tests require further debate, especially when the working definition that they have used to judge a positive or negative test is not complete. As a newly emerging field of neuromusculoskeletal therapy, neurodynamics has been associated with many different terms and definitions. Leading authorities, like David Butler and Michael Shacklock actively try to promote clear terms and definitions to avoid confusion for clinicians. This study has the potential to undermine this effort. It is vital that there is a universal adoption of clear and concise terms and definitions within neurodynamics, particularly in respect to interpretation of neurodynamic tests. There is no *gold standard* measurement

or clinical test for neurodynamic dysfunction. In respect to clinical validity or neurodynamic tests, measurement of construct and content validity is perhaps the best assessment available. Clearly more research needs to concentrate on the true underlying physiological and biomechanical underpinnings of neurodynamic pathology before a gold standard measurement exists. While we are waiting, Shacklock's<sup>8</sup> clinical algorithm presents the most simple and user-friendly method of interpretation or neurodynamic tests.

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## AUTHOR RESPONSE

**W**e appreciate the opportunity to reply to the comments provided by Mr. Ellis regarding our investigation, which examined the false positive rate of the upper limb neural tension test (ULNTT) and seated slump test (SST) among healthy young adults.<sup>1</sup> In his letter, Mr. Ellis calls for further debate regarding the validity of these neurodynamic tests. We support his desire for not only debate but more importantly additional research in this area. However, the debate should be based on science and conducted with professional decorum.

Mr. Ellis identified three primary concerns with the methodology used in our investigation. Each of these concerns relate to the operational definition of a positive test. Mr. Ellis stated that “neurodynamic testing must take into account the symptoms and presentation of the patient.” He also stated “a positive neurodynamic test, clinically, should not be made on structural differentiation alone.” He suggests that additional information is needed from the “history, area of symptoms, and imaging tests.” Mr. Ellis also stated that “no inference can be made as to whether a clinical test is positive or negative unless bilateral comparison is made.”

It should be noted that our investigation was purposefully conducted on a sample of individuals who were without pathology, thus these subjects did not have any neural mediated symptoms that could be used for comparison. While a composite examination may offer greater diagnostic validity, we sought to examine the stand alone validity of these tests. Ad-

ditionally, we clearly stated that the tests were conducted on the left upper and lower extremities and identified this as a limitation of the study.

Complete examination of diagnostic validity requires the testing of subjects with and without the condition or disease. Our investigation only examined the false positive rate among subjects without the condition or disease. Using a clearly defined and reproducible operational definition of a positive test, we found a high false positive rate among these tests. Our investigation made no attempt to offer data relative to sensitivity, positive predictive value, false negatives, prevalence, or post-test odds. We welcome future investigations that examine the full spectrum of diagnostic validity of the ULNTT and SST.

While a debate regarding the operational definition of these tests is welcomed, it should be conducted with professional discord. To suggest that our investigation was an “unfair witch-hunt” is presumptuous and portends a superficial review of the article. In addition to offering data regarding the false positive rate, we suggested possible cutoff values that may enhance the diagnostic validity of these tests.

The tone of Mr. Ellis’ letter does not foster collegial dialogue and offers little evidence to advance our understanding of the diagnostic validity of these neurodynamic tests. If we are to move forward as evidence based practitioners we must be willing to critically examine evidence in an unbiased manner and be willing to recognize potential limitations of our

clinical tests and measures. In an eloquent editorial, the late Jules Rothstein<sup>2</sup>, Editor-In-Chief of *Physical Therapy*, wrote . . . “All evidence has limitations, but whatever those limitations may be, data are far better than debates that are more about theology than they are about health care.”

We invite Mr. Ellis and other researchers to replicate our investigation and improve upon the methodology where it is deemed necessary. We have come a long way toward Dr. Rothstein’s dream of becoming an evidence based profession. Rather than becoming marred by dogma and rhetoric, let us instead add to the body of evidence and learn from our limitations so that we can become better health care providers.

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