

■ A New Model for Orthopaedic Manual Therapy Research: Description and Implications

Despite forays into editing, consulting, and teaching, I still consider myself first and foremost a clinician. And, at that, I like to think of myself as an evidence-based clinician. For me this appreciation of the importance of research in my day-to-day clinical decision-making did not come easy. My entry-level degree in physical therapy strongly emphasized authority-based knowledge and clinician expertise. I considered courses in research methodology and statistics as curriculum fillers and mere lip service to the supposed scientific foundation of my new profession. But who could blame me for this negative attitude some 20 years ago when the research basis for physical therapy was very meager indeed? And even if there was any relevant research, it certainly was not introduced to me during my early studies.

I continued on with graduate studies in the area of orthopaedic manual therapy (OMT) and by then—much to the credit of the physical therapy profession and, of course, of chiropractic, medicine, and osteopathy—the amount of relevant research had skyrocketed. However, the clinical value of research still continued to elude me. This was not because my professors were poor at conveying the central concepts and methods to me. And it also, although I would be the first to admit that I am certainly no statistical “wunderkind,” was not because I failed to understand research methodology or statistical procedures. It was the lack of concordance between what I saw in the clinic and what the research was telling me. Although I found that OMT was highly effective for a subset of patients, these admittedly anecdotal observations were not reflected in the randomized trials, systematic reviews and meta-analyses, or clinical practice guidelines I read.

With my by-now expanded knowledge of statistics and research methodology, I tried to make sense of this discrepancy between my clinical observations and the published research on OMT. Of course, there were the standard arguments of misrepresentation in the randomized trials of interventions called physical therapy and OMT that in no way reflected what we actually do in clinical practice. Then, with no one being quite sure what the effective component is in many OMT interventions, there was the use of inappropriate control group interventions that may have had unintended therapeutic effects, thereby washing out the effect of the experimental OMT interventions. Finally, there was the problem that the generally small sample sizes in OMT research predisposed these studies to type II error^{1,2}.

With regard to systematic reviews, I questioned the comprehensiveness of the literature search; after all, a fair amount of research in this area is not published in the more easily accessible databases commonly used in such reviews³. Then there were the conflicting results based on the use of different methodological quality assessment tools despite nearly similar literature search results, oversampling of data that was published multiple times (perhaps initially as a pilot and later as a larger study), and—perhaps more noticeable to a non-native English speaker like me—the bias of including only English-language articles. With regard to meta-analyses, there was the issue that statistical pooling is only possible if the included studies are sufficiently homogeneous. If the studies are heterogeneous with regard to clinical parameters (e.g., sample, intervention, outcome assessment) or methodology, then pooling is impossible. Yet, statistical procedures used to test for similarity of the studies included have limited power to detect heterogeneity when the reviews include only a few studies and these few studies again have only few subjects, a situation very common in OMT research⁴. And then there is the fact that systematic reviews and meta-analyses on intervention use randomized trials with their specific weaknesses as described above. No wonder there is such a high proportion of unreliable or poorly reported reviews being published⁵.

So I was perfectly capable of providing seemingly valid criticisms of the available OMT research but as a result, I also found myself even more without any research-based guidance for clinical practice.

True to my proverbial Dutch stubbornness I dug in my heels to find a solution: how could I be an evidence-based clinician if I had no good evidence to guide me? Willing or unwilling, readers of *JMMT* have had a front seat in my

personal quest: my critical enthusiasm for evidence-based practice in the editorials I have written over the last three years; my increased use of data on reliability, validity, and responsiveness as part of my decision-making in the case reports I have (co)authored; an increasing interest in, at first, narrative and then systematic reviews of diagnostic utility studies; and perhaps most telling, my attempts at combining the various recent clinical prediction rules relevant to OMT with the biomechanical and neurophysiological models that formed the basis of my OMT education.

If there is one thing that clinical prediction rules have made clear to me, it is that the impetus for OMT research needs to be the description and validation of patient subgroups based on a treatment-based classification system. Recently I read an excellent text by Donelson⁶, wherein he discussed the ADTO-research model first described by Spratt⁷. The acronym ADTO stands for Assessment-Diagnosis-Treatment-Outcome. The research model described three essential links:

1. Assessment-Diagnosis: This first link is the fundamental step of establishing intra- and interrater reliability of the identification of a proposed subgroup.
2. Diagnosis-Treatment: Once we have shown we can reliably identify a subgroup within a treatment-based classification system, we then need to determine if this subgroup indeed responds consistently and favorably to one or more proposed matched interventions.
3. Treatment-Outcome: In this final step, we determine which of the interventions shown efficacious in the second step is in fact the most efficacious.

It is easy to see where the various OMT research efforts and study methodologies fall within this model. Whereas the first step requires reliability research, the second step would be best served with observational cohort studies, and the last step is where the randomized trial has its place⁶. This model also focuses our research efforts. If a particular school of thought within OMT has a hypothetical model that defines treatment-based groups, their first effort should be to validate those subgroups with research into the reliability of this proposed classification system. If no reliable subgroup can be identified based on the existing consensus-derived criteria, then these criteria need to be re-evaluated and adjusted. This may mean that we need to let go of some of our most prized diagnostic tools, such as motion palpation, or perhaps decrease the emphasis we place on its findings. Ultimately, failure to show reliable subgroup classification would lead us to question the theoretical premises behind the approach. After reliable subgroups are established, the diagnosis-treatment step will then allow for cohort studies comparing the outcome of interventions matched to the specific subgroup with, for instance, the same intervention for patients not identified as belonging to that subgroup. Cohort studies are more representative of what we do in actual clinical practice and should, therefore, be feasible within one or multiple centers. Consistent findings in those cohort studies in support of the proposed treatment-based classification can then be tested in the randomized trial, the most labor-intensive and costly design, in the final step. The resultant homogeneous populations in these trials will most likely show the results that we clinicians have been observing in practice but which used to be “washed out” in the older trials described above with their massive heterogeneity with regard to study subjects.

In a previous editorial⁸, I expressed excitement that recent research had provided me—as an active clinician—with evidence-based guidelines with regard to diagnosis and management of at least some of the patients who come to see me for back and neck pain. Having been introduced by Donelson⁶ to this ADTO-model, I find another reason to be excited about these prediction rules, as they so obviously fit within this model that I believe will drive future research within our field. However, true to my ever-critical nature, I would also dampen my own excitement with a caveat. Only one of the prediction rules has as of yet been validated^{9,10}. At that, validation occurred in a population and setting and with therapists that were not very different from the original study, leading us to question the more general applicability of this rule to a more varied population of patients, settings, and therapists. We also need to consider that the suggested treatment-based classification systems upon which these rules are based are derived by way of expert opinion and subsequent consensus. The categories currently included do not represent all categories proposed with the various OMT schools, most notably a specific manipulation and a myofascial pain subgroup. I am not saying that such groups truly exist but I am saying that current research into clinical prediction rules cannot be used to exclude the existence of such groups and various other proposed groups with different treatment implications. It is up to proponents of such subgroups to start producing the research to prove their existence, and the ADTO-model may provide the framework to do such research.

Talking to one of my former students recently, I learned of a company-wide initiative to educate therapists in the use of the recent prediction rules with the intent to have these rules dictate clinical practice. I am quite certain that such “evidence-driven totalitarianism” was never the intent of these rules. After all, as Beattie and Nelson¹¹ have already pointed out, clinical prediction rules are not clinical decision rules. Prediction rules have the potential to significantly contribute to the clinical

decision-making process but in the spirit of true evidence-based practice, they are always to be used in combination with clinical expertise and patient preference.

On a different note, this is my last editorial as the editor of *JMMT*. After three years in this position, I find that I need to spend some more time with my young family. I would like to take this opportunity to thank our Executive Editor and former Editor-in-Chief, Dr. John Medeiros, for giving me this great opportunity to contribute to my chosen profession. I would also like to thank our Copy Editor, Dr. Jill Kelly, for correcting my all too frequent infractions against the English language. I would like to thank the Associate Editors with whom I have had the pleasure to work over these last three years; not one ever grumbled when I dumped another load of unpaid labor on their already overfilled desks, and their work in getting articles ready for publication has been invaluable. Typesetting first by Dean Wiest and now by Anne Landgraf has provided us with a high-quality, beautiful layout. Without Jim Doree, our website could not possibly have been as successful. Thanks also to the authors. We would not have had a *Journal* without your hard work and dedication needed to do all the rewrites required. Also, thanks to all the readers, with many of whom I have had the pleasure to interact via email. Finally, most thanks go to my wife and children, who have allowed me the time to do this. I look forward to seeing upcoming issues of the *Journal* under the capable and highly energetic leadership of the new Editor-in-Chief, Dr. Chad Cook.

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