Letters to the Editor

To the Editor:

Chiropractors at McMaster University: The formation and direction of a university-based multidisciplinary chiropractic working group. J Can Chiropr Assoc. 2010;54:10–12.

There is an inaccuracy in the otherwise interesting and informative paper by Passmore, Riva and Goldsmith.[1] In their paper, which discusses the creation of a chiropractic working group at McMaster University, the authors make the following statement: "Within chiropractic educational facilities there are no formal programs cultivating chiropractic clinician researcher development" and they cite a 2006 paper by Haas and colleagues.[2] This is factually incorrect. For the past 5 years Palmer College of Chiropractic has offered a master's of science degree in clinical research through the Palmer Center for Chiropractic Research. This program, which was initially funded as part of K30 award from the National Center for Complementary and Alternative Medicine at the US National Institutes of Health, provides training in clinical research to the graduate fellows who matriculate into the program. Those fellows are, without exception, chiropractors who wish to obtain additional training in order to develop a career that involves the conduct of clinical research. Our fellows have included both new graduate chiropractors and those with many years of practice experience, as well as two dual degree participants (MD/DC and DC/PhD), as well as several faculty clinicians at Palmer College of Chiropractic. Information about the program can be seen at the program website: http://www.palmer.edu/pcc_current2.aspx?id=5290. I simply wish to set the record straight on this issue, but commend the authors for their informative work, which bodes well for the future of chiropractic research and education.

References

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To the Editor in reply:

We would like to thank Dr. Lawrence and Palmer Chiropractic College for making us aware of their clinical research program.[1]

In our article we simply made an attempt to cite the most recent peer reviewed literature pertaining to the topic of chiropractic clinician researcher development which is why we cited the Haas, Bronfort, and Evans (2006) paper.[2,3] In the generation of our article it was not our intention to complete a systematic analysis of programs existing at chiropractic colleges worldwide, although in light of Dr. Lawrence's comments and exciting program such a paper could be timely.

Another chiropractic college (New York Chiropractic College), has had a "Fellowship" program in place since 2002. Their Fellowship program aids in the training of clinician researchers at mainstream research intensive academic institutions.[4]

Both of these programs (and other similar programs that may exist) should be applauded for their innovation in encouraging and supporting those who have already completed health professional degrees to explore research training.

Unfortunately to this author's knowledge there are presently no entry level programs from an undergraduate degree leading to combined terminal clinical and research (DC/PhD) credentials, where all tuition fees are reimbursed or waived, and the student is provided with a stipend on which to live for the duration of both degrees. The debt load accrued by tuition payments for the clinical student is seen as a disincentive for the development of clinician scientists.[5] Programs have emerged in other disciplines to decrease financial barriers in developing the clinical research leaders of the next generation.[6] Funding this type of opportunity seems like a logical future goal for the chiropractic profession.

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To the Editor:

Chiropractic care of patients with asthma: a systematic review of the literature. J Can Chiropr Assoc. 2010;54(1):24–32.

We read with great interest the manuscript by Kaminskyj et al.[1] Their use of the Downs and Black[2] scoring system provides another perspective to assessing the methodological quality and evidence of studies on SMT and asthma. Hondras et al[3] and Ernst[4] also performed a similar undertaking using the Jadad Scoring.[5] All determined the clinical trials on asthma of high methodological quality and reflective of the level of evidence from their findings. However, as pointed out by Brouwers et al,[6] incorporating quality assessment into systematic reviews finds both favor and dissent in the scientific literature. In this context, Kaminskyj et al[1] failed to consider the basic research design issues in designing placebo-controlled trials on SMT and hence the potentially misleading results of their findings.[7] None of the sham SMTs employed in the clinical trials on asthma have been validated as appropriate for a placebo-controlled trial. We found no support from the studies cited[8,9] by Nielsen et al[10] and Bronfort et al[11] to justify the appropriateness of their sham SMT. The study by Balon et al,[12] considered the clinical trial of highest methodological quality, describe a "simulated therapy" that is arguably another type of manual therapy. Additionally, the incorporation of massage into their simulated protocol negates any assumed inert effect on asthma[13] and the assumption on the part of Balon et al[12] that the audible release following SMT is the differentiating factor for active versus sham SMT was ill conceived.[14] Kaminskyj et al[1] also failed to address the limitations of spirometry in their examination of the evidence. Wifhaber et al[15] demonstrated that spirometry variables (i.e., FEV1, FCV, etc.) do not sufficiently correlate with asthma severity or control as determined via symptom scoring. Schneider et al[16] found the diagnostic accuracy of spirometry on asthma as questionable with sensitivity at 29% (95% CI 21–39); specificity at 90% (95% CI 81–95), positive predictive value at 77% (95% CI 60–88) and negative predictive value at 53% (95% CI 45–61). As such, any interpretations based on the spirometry in the clinical trials on asthma must be examined with caution.

Arguably, the SMT clinical trials on asthma are randomized comparison trials (rather than RCTs) with subjects in both SMT groups experiencing decreased asthma symptoms, decreased medication use and improved overall quality of life. More research is needed to determine the specific versus non-specific effects of a particular SMT prior to use in RCTs.

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To The Editor in Reply:

Thank you for the opportunity to respond to the Letter to the Editor by Drs. Joel Alcantara, Joey Alcantara and Junjoe Alcantara to our systematic review.[1] As we read their letter, the Drs. Alcantara seem to have three primary concerns: (i) what the 'C' in RCTs stands for ('clinical', 'controlled', 'comparative' or something else altogether); (ii) the appropriateness of the sham procedure used in some of the studies we described, such as the one by Balon *et al*[2] and; (iii) the use of spirometry to measure pulmonary function for patients with asthma.

(i) RCTs

In the peer-reviewed literature, RCTs may refer to Randomized Controlled Trials, Randomized Clinical Trials (which are either controlled or comparative), and Randomized Comparative Trials. Conventionally, albeit perhaps unfortunately, all of these are referred to in the peer-reviewed literature by the "RCT" acronym and the only way to know which type of trial was used in this or that study is to review the study's 'methods' section. Thus, our usage of the acronym RCT is appropriate.

Moreover, we must remind the Drs. Alcantara that the studies described in our systematic review survived the peer review and editorial process of several prestigious journals (Manual Medicine, JMPT, New England Journal of Medicine). These journals were satisfied to characterize these trials as RCTs. If the Drs. Alcantara take umbrage with the decision to characterize these trials as RCTs, we respectfully submit those concerns should be directed towards the respective publishing journal.

(ii) Sham 'manipulation' used in many RCTs

Drs. Alcantara cite an article from 1994[3] that they purport supports their assertion that the sham SMT used in many RCTs have not been validated. We again remind these authors that the aforementioned journals were satisfied with the sham manipulation procedure used in each study we cited in our systematic review. Again, any concerns they have with respect to the methodologies used in any particular study should be raised with either the authors of each study or the editorial board of the journal that published it.

More importantly, we can do no better than to defer to a recently published comprehensive and extensive review of the literature by Drs. Bronfort, Haas, Evans, Leiniger and Triano.[4] Each of these reviewers has extensive experience with conducting systematic reviews and we are content to bow our collective heads to their expertise. Their review article assessed the effectiveness of manual therapies for musculoskeletal and nonmusculoskeletal conditions. In their review, they concluded:

> *"There is moderate quality evidence that spinal manipulation is not effective (similar to sham manipulation) for the treatment of asthma in children and adults on lung function and system severity".*[4p53]

In other words, these experienced reviewers were not only satisfied to characterize the study by Balon[2] and others as RCTs, they mentioned the 'sham manipulations' used in each study without any commentary and their conclusions were much more critical than others with respect to the effectiveness of SMT for patients with asthma.

(iii) Use of spirometry for patients with asthma

With respect to the article by Schneider et al[5] cited, the positive predictive value (number of positive spirometry results that are defined via the gold standard as true, divided by the total number of times the spirometry result was positive, or the percentage of time a positive spirometry result is correct) is 77%. That is a relatively high value and since it is common practice to use spirometry in general practice the study reflects general practice.

In summary, while we thank the Drs. Alcantara for their views, we contend that our use of the term RCT was appropriate, the sham manipulations used in the studies we cited have been assessed and critiqued by others before us to their satisfaction and the use of spirometry to measure pulmonary function for patients with asthma seems to be justified.

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