DOI: 10.1503/cjs.008114

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Regarding "Factors affecting the relative age effect in NHL athletes"

I read with interest your recent article in the *Canadian Journal of Surgery*[1] in which you report that a small effect was found in relative age effect (RAE) of birth month when the year was divided chronologically in 2 6-month blocks. As I read it, 2 questions arose.

The article reports the height and weight of players. Was this information characterizing players for that season part of the NHL roster? If so, I wonder if you considered using the height, weight and time of drafting and your opinion on their potential effect.

Second, do the other jurisdictions from which NHL players originate share the same birth month–related categorization policies as Canada does in the early years of play? I wonder if that could explain the lack of identified RAE effect you found.

Thank you for informing the discussion on this topic.

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Author response

Thank you for taking the time to share your questions.

Regarding the height and weight of players, the data for height and weight were obtained for the season itself. We used this data for 2 reasons: (1) the players are in the NHL because of their current fitness and physical attributes, not the attributes they were drafted with, and (2) this information was most readily available and verified.

Regarding other jurisdictions, although this information is not readily available, other jurisdictions probably do not have the same narrow and restrictive draft conditions that cause an RAE. We discuss in the article why the RAE happens in some sports and not others worldwide. Pavel Datsuk has stated publicly that if he had been in the Canadian system as a youth he would never have been drafted. That would have been a real loss!

Thank you for your questions. I hope this response answers your concerns.

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Medical student–run education: the next steps

The recently published paper by Li and colleagues[1] offers interesting insights into the potential for medical student–run medical education. The medical student–run provision was popular and the researchers were able to show significantly more interest statistically in surgical careers in the intervention group. However, the researchers are also correct that further qualitative analysis of their data should prove useful. The limited qualitative data that they have provided are tantalizing. The learners felt that the senior medical students were good role models and clearly felt more empowered to ask them questions. Conversely, the teaching staff was perceived as being more cutting-edge, albeit limited by staff time constraints. It would likely prove fruitful if further qualitative research could delve into these thoughts and reflections. Such qualitative research is unlikely to find that one form of education is better than another, but it might tease out the exact outcomes that are most effectively and efficiently achieved with student-delivered and staff-delivered learning. A learning package could then be put together, taking the best features of both forms of delivery. This package could then be evaluated.

Another point of note is that the researchers understandably concentrated on the learner outcomes; however, it would be interesting also to hear the feedback of the student educators. It would be interesting to know whether they felt positive about the experience, whether they consolidated their own knowledge and skills by teaching others, and whether they developed teaching skills themselves. This would be a secondary but still worthwhile outcome. As soon as students graduate and become doctors, they are automatically expected to begin teaching juniors, so any experience that they can obtain as undergraduates would likely prove useful. Many of the teaching skills that they develop are also transferable skills (e.g., communication and presentation skills). These are yet more reasons to encourage the involvement of students in the teaching process.

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A not-so-systematic review

In evaluating Ebrahim and colleagues' meta-analysis,[1] which compared low-

intensity pulsed ultrasound (LIPUS) to electrical stimulation for fracture healing, we were disappointed to find several glaring errors and omissions.

Fracture nonunion was improperly defined. Nonunion was defined to include both "possible unions (bridging at 3 cortices) and nonunion (bridging at 2 ≤ cortices),"[1] yet bridging at 3 cortices defined a healed fracture in 3 of the 7 LIPUS studies evaluated (references 32, 37 and 39).[2–4] Fractures that had been defined as healed in these 3 LIPUS studies were therefore arbitrarily reclassified by Ebrahim and colleagues[1] as treatment failures.

Reduced time to radiographic union was considered a "surrogate end point," yet a survey of 335 orthopedic surgeons concluded that "radiographic outcomes were more important than functional outcomes" in designing clinical trials.[5] Overall, 88.1% of surgeons accepted that nonunion is defined by radiographic and clinical criteria, whereas "return to function" was seen as important by just 29.9%.[5]

Seminal LIPUS papers were omitted. It is possible that simple error accounts for omission of the first randomized clinical trial (RCT) to evaluate LIPUS. The Heckman study[6] evaluated in preference to the RCT[7] is actually an econometric evaluation of tibial fracture. Simple error might also account for why a registry study[4] was analyzed instead of an RCT published by the same author in the same year.[8] Finally, the only RCT in which LIPUS was used to evaluate delayed union[9] was omitted for unspecified reasons.

The selection of studies for analysis was biased. Figure 2 in the meta-analysis[1] identified 6 biased studies (≥ 5 of 8 categories at high risk of bias). There were 3 biased LIPUS studies and 3 biased ESTIM studies, and these 6 studies should have been excluded. Yet the meta-analysis included all 3 biased LIPUS studies (references 37, 39 and 41),[3,4,10] while excluding all 3 biased ESTIM studies (references 46, 51 and 55).[11–13] A contrast is therefore drawn between the weakest LIPUS papers and the strongest ESTIM papers, whereas meta-analyses usually strive to avoid such imbalances.

Whether fractures were fresh or nonunion prior to treatment was ignored. Fresh fractures treated with LIPUS were evaluated, as compared to nonunion fractures treated with ESTIM. Of the LIPUS papers evaluated, 6 of 7 were about fresh fracture; of the ESTIM papers evaluated, 5 of 8 were about nonunions. Normal healing is expected in fresh fractures; no spontaneous healing at all is expected in nonunions. Thus, any impact of LIPUS would be hard to document, whereas even a minor impact of eStim should be quite obvious.

We believe these problems invalidate the effort of Ebrahim and colleagues to produce a clinically useful meta-analysis.

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