

THE SHORTCOMING AND DEFICIENCY IN “ATTEMPTING PRIMARY CLOSURE FOR ALL OPEN FRACTURES: THE EFFECTIVENESS OF AN INSTITUTIONAL PROTOCOL”

Moola and colleagues¹ have done a lot of work on attempting primary closure for all open fractures, and they have found that primary closure for all open fractures is a safe and efficient practice. However, we have some concerns regarding the paper and wish to share them.

First, there was an obvious mistake in the design of the study. As we know, the timing of wound closure in the management of open fractures is very clear both in the orthopedic traumatology textbook and literature. The open fracture, from Gustilo type I to Gustilo type IIIa, should be treated with primary wound closure. Delayed wound closure is mainly performed in patients with Gustilo types IIIb and IIIc wounds, which always require second-look débridement to assess gross contamination. Such complicated open fractures no doubt have higher rates of infection and nonunion.²⁻⁵ However, in this study the authors analyzed the following patients with open fractures: 152 type I (51.2%), 73 type II (24.6%), 46 type IIIa (15.5%), 13 type IIIb (4.4%) and 13 type IIIc (4.4%) injuries. Of these, types I, II and IIIa accounted for 91.3% of all open fractures. This means that most open fractures for the study should have been treated with primary wound closure. Therefore, the results comparing Gustilo type I, II and IIIa and Anderson type I and II, determining that they had the highest rates of definitive immediate closure, was meaningless, repetitive work. We suggest the authors analyze the attempting of primary closure for type IIIb and IIIc open fractures, which remains somewhat controversial in orthopedic traumatology.

Second, certain types of open fracture wound closure need to be

treated with delayed wound closure, which are not subject to Gustilo type restrictions (e.g., wounds with delayed presentation [> 12 h] or high-risk of anaerobic contamination). Even in the study by DeLong and colleagues² there were still some Gustilo I and II wounds treated with delayed closure.

Third, the authors claimed that the only published prospective study evaluating wound closure protocol for open fractures is by Rajasekaran and colleagues.³ However, we are aware of at least 2 published prospective articles in the literature.^{6,7}

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AUTHOR RESPONSE

We are happy to address your concerns with our paper.

You have the following 3 concerns:

1. You feel there is no timing debate.
2. Some wounds mandate delayed closure.
3. We did not quote the appropriate papers.

1. Timing is still controversial. We felt that there was enough evidence in the literature to start an institutional protocol. Reviewers still feel that we are too radical with our protocol. On average this is not a design flaw, but rather an attempt to answer a real question in North America: “Can you close open fractures?”. The inclusion of all grades is a review of a system protocol change, not a case-by-case dictation of whether to close or not. The paper is as much a review of a protocol implementation — whether it was successful and whether all surgeons followed — as it is a review of what happens with these patients.

2. Contaminated wounds and old wounds underwent the same protocol; excision of all contaminated areas converted the wound to a clean wound. The protocol did not forbid second looks, and patients were allowed to be taken back to the operating room for débridement. As long as the skin was closed initially, they fell in the primary closure group.

3. At the time of the initiation of the protocol, the quoted paper by Benson and colleagues¹ was the only prospective paper addressing this subject matter. This is the paper we quote in our design consideration.

Thank you so much for your letter; it is always great to have people read your work so keenly.

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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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Reference

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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 colleagues¹ 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-