Forecasting the economic benefit of reducing non-operative time

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To the Editor,

With reference to Head *et al.*'s "swing room" model based on regional anesthesia, they showed that patients undergoing hand and wrist surgery with regional anesthesia can have a briefer non-operative time period and more cases can be scheduled within an eight-hour workday.¹ They state: "These findings contrast with those of Dexter *et al.*² who found that anesthesia-controlled time (or the time from surgical closure on one patient until anesthesia is ready on the following patient) for most types of surgeries would have to be decreased by more than 100% in order to be able to book one additional 30-min case each day."

First, in response to their statement, I defined anesthesia-controlled time as "the sum of 1) the time starting when the patient enters an OR to when positioning or skin preparation can begin plus 2) the time starting when the surgical dressing is completed to when the patient leaves the OR."² Head *et al.*¹ misstate my definition.

Second, the mean surgical time for hand cases was 40 min in Table 1 of both Head *et al.*'s and my articles.^{1,2} Also, the mean non-operative time in Table 1 of my article was 35 min, 12 min being anesthesia-controlled time.² According to Table 3 in my article, the "decrease in anesthesia time needed to predictably finish earlier" was 110%,² or 13 min. Since Table 3 of Head *et al.*'s article shows a 39-min reduction,¹ my calculations² also predict additional case(s) in the eight-hour period.

Third, the cases studied by Head *et al.* were considered completed within an eight-hour period¹ not rationally scheduled into an eight-hour period.² Cases have predictive

variability (e.g., a case scheduled for 45 min could take one hour).³ A rational manager whose goal is an eight-hour workday schedules fewer than eight hours' worth of cases (e.g., 7.3 hr).^{2,4,5} The header in my Table 3 reads "predictably finish earlier", since my analysis takes this into account.^{2,4,5} Head *et al.*'s formula is economically irrational.^{4,5} The issue is simply their analysis, not their conclusions, as their approach to use anesthesia assistants to monitor patients after regional anesthesia has excellent potential to increase productivity.⁶ The benefit of their approach will simply be slightly less than they predict because of the issues of "reliably" scheduling another case^{2,4,5} and the likelihood that an entire room of appropriate cases can be scheduled daily.^{1,6}

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Competing interests None declared.

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Reply

We thank Dr. Dexter for his interest in our study and appreciate the detailed clarification of the specific definition of “anesthesia-controlled time” used in his article.¹ We are grateful for the opportunity to respond to his comments and to highlight some of the important differences between his 1995 work and our study.

First, Dr. Dexter’s statement, “Head *et al.* found that patients undergoing hand and wrist surgery with regional anesthesia have briefer non-operative times than patients having general anesthesia”, is inaccurate. Rather than compare regional with general anesthesia, we compared two different “care models”. Compared with a traditional single-operating room model based on general anesthesia, we found that the implementation of a two-theatre “swing room” model based on ultrasound-guided regional anesthesia (not used routinely at the time of Dexter *et al.*’s study) performed in a third “block” room (i.e., utilizing two forms of parallel processing) significantly reduced operating room turnover times and increased case throughput. The swing room model markedly reduced the frequency of phase I postanesthesia care unit admission, postoperative interventions for pain and nausea, and patient home discharge times. As we pointed out in the Discussion section of our article, it is important to recognize this difference as the various components of our new care model likely affected the various study endpoints in a differential manner. Specifically, we would attribute the effects on turnover and throughput mainly to the parallel processing component, and we would ascribe the effects on postoperative status mainly to the ultrasound-guided regional anesthesia component.

Second, the primary endpoint in our study was operating room turnover time, defined as the time from the completion of surgical closure on one case until surgical incision on the following case.² Unlike Dexter *et al.*, we did not attempt to measure “anesthesia-controlled time” or “between-case time” specifically. Since our care model is based on parallel processing, “anesthesia-controlled time” was not a suitable endpoint with which to examine case turnover and throughput seeing as regional anesthesia is administered while the previous case is underway. Also, because surgical anesthesia is usually present when the patient enters and leaves the operating room, a definition of anesthesia-controlled time as “the sum of 1) the time starting when the patient enters an OR to when positioning

or skin preparation can begin plus 2) the time starting when the surgical dressing is completed to when the patient leaves the OR” is not reflective of anesthesia-controlled time in the context of our regional anesthesia-based swing-room care model.

Third, regarding the statement, “The mean surgical time for hand cases was 40 min in Table 1 of both Head *et al.*’s and my study”, and the statement, “Since Table 3 of Head *et al.*’s article shows a 39-min reduction, my calculations also predict additional case(s) in the eight-hour period”, we are uncertain whether Dexter *et al.*’s study would permit valid inferences regarding possible additional cases because no comparison was made between a control group and a treatment group for the times reported. Furthermore, the surgical procedures were not identical as Dr. Dexter suggests; whereas our study included a wide range of hand and wrist procedures, Dexter *et al.*’s “hand cases” were comprised of carpal tunnel releases exclusively (which, at our institution, are not routinely performed under brachial plexus blockade).

Fourth, regarding Dr. Dexter’s final point, we neither studied cases “completed within an eight-hour period” nor cases “rationally scheduled into an eight-hour period”. We selected a period of eight hours arbitrarily for convenience; the formula can be modified easily to suit any other period of interest. In the *post hoc* comparison of throughput between two study groups, our formula represents a simple and straightforward tool based on abstracted real-life data. As we explained in our article, this approach was necessary to determine the number of “comparable cases” possible per day for both groups, since the daily lists frequently included cases that did not meet inclusion criteria. Regarding the suggestion that the benefit of our new care model “will simply be less than they predict”, our results are based on conservative premises and reflect data from actual matched cases as opposed to mathematical modeling – we predict, based on the conservative approach to analysis as well as our ongoing daily experience with the swing room care model, that the increase in throughput will be greater than the 56% reported in our article. We recognize that case length variability is always a consideration for the purpose of scheduling, and adjustments in the number of cases assumed to be reliably completed in a day might be necessary. However, we did not study the validity of scheduling premises, and as we clearly stated in our article, we did not intend to provide a detailed economic analysis. As Dr. Dexter would be aware, analyses of the economical impact of health care interventions are complex and cannot be conducted without consideration of the specific contextual health care system,³ and it is well-known that the health care system in Canada differs significantly from that in the United States. In our publicly funded universal not-for-profit Canadian system, surgical

wait list reduction has been and remains a priority.^{4,5} In our view, our data demonstrate that anesthesiology-driven innovation in the approach to perioperative care delivery represents an effective tool to help achieve this goal.

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Conflicts of interest None declared.

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