ers that all surgeons involved in our study were affiliated with regional cancer centres, and thus waits among their patients may not be representative of waiting times for all patients across Ontario.

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Bedside rationing

To implement bedside rationing as described in Peter Ubel's *Pricing Life: Why It's Time for Health Care Rationing*[1] would most certainly set medical ethics back 2500 years by ignoring the issue of patient trust, which gave rise to the traditional Hippocratic oath. The fundamental unit of health care is the physician–patient relationship. For physicians to knowingly withhold beneficial services from patients to promote the financial interests of others (or of themselves) would introduce suspicion into that relationship, further subjectivize the practice of medicine, and increase the power disparity between physician and patient. What patient wouldn't question the physician's commitment under such circumstances?

Should rationing ultimately become necessary, then bureaucrats must impose it broadly, at the system level, for the sake of maintaining consistency across the population and of minimizing physician conflict of interest. Patients must also have the option of obtaining services privately. Before Hippocrates, the sick could never be certain of their physicians' motives or competing interests, but generations since have enjoyed the peace of mind that comes from the physician's pledge to do no harm. Bedside rationing would undermine this precious gift that has protected us all.

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Reporting the clinical importance of randomized controlled trials

Karen Chan and colleagues address how well reports of randomized controlled trials discuss the issue of clinical importance.[1] We agree with these authors and others[2,3] that clinical importance needs to be discussed in the report of any randomized controlled trial.

Chan and colleagues defined clinical importance using 10 dimensions, such as an explicit statement of the primary outcome. We are surprised that they state early in their article that the CONSORT statement "failed to recommend specifically that authors discuss the clinical importance of their results." Perhaps they have not completely read the CONSORT statement[4] and its accompanying explanation and elaboration paper,[5] which definitely draw attention to this important issue. For example, item 6 of the CONSORT checklist explicitly recommends that authors of randomized controlled trials report "clearly defined primary and secondary outcome measures."[4] Moreover, the explanatory paper is clear about the relevance of clinical importance: "The difference between statistical significance and clinical importance should always be borne in mind. Authors should particularly avoid the common error of interpreting a nonsignificant result as indicating equivalence of interventions. The confidence interval (item 17 of the checklist) provides valuable insight into whether the trial result is compatible with a clinically important effect, regardless of the *P* value."[5]

The CONSORT statement is an

ever-evolving tool and the CONSORT group welcomes suggestions to further improve the quality of reports of randomized controlled trials. Unfortunately, in this instance, Chan and colleagues have apparently overlooked the existing CONSORT documents. Nevertheless, we congratulate them for their excellent study and for highlighting the issue of clinical importance.

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Considering their study's objectives, I was surprised that Karen Chan and colleagues did not explain why they evaluated only 10% (27/266) of available randomized controlled trials.[1] This is especially interesting as both previous studies they referenced[2,3] evaluated more studies, 102 and 45 respectively, and therefore were more precise.

Further, why weren't the proportions in Table 3 accompanied by 95% confidence intervals, particularly when the reporting of confidence intervals was one of the criteria Chan and colleagues used to evaluate randomized controlled trials?

When one refers to Diem and Lentner's *Scientific Tables*,[4] it is troubling to note the imprecision of the proportions reported by Chan and colleagues[1] (e.g., 22/27 = 81%, confidence interval [CI] 62–94%; 20/27 = 74%, CI 54–89%; 18/20 = 90%, CI 68–99%; 2/18 = 11%, CI 1–35%; 13/18 = 72%, CI 47–90%; 17/27 = 63%, CI 42–81%; 11/27 = 41%, CI 22–61%; 10/20 = 50%, CI 27–73%; 15/20 = 75%, CI 51–91%). Apparently, the upper and lower limits of many of these confidence intervals could lead to differing conclusions. For example, although Chan and colleagues found that 74% of investigators (20/27) discussed the clinical significance of their findings,[1] this estimate is also consistent with values as low as 54% and as high as 89%.

In closing, I would argue that the determination of study precision should be part of the planning process for all studies, not just randomized controlled trials. Such as step would strengthen both the statistical and clinical integrity of any planned study.

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[The authors respond:]

Upon rereading the revised CONSORT statement,[1] we still do not see an explicit recommendation that authors should discuss the clinical importance of their study results. We do not believe that the discussion of such an important component of the reporting of randomized controlled trials should have been relegated to the accompanying explanation and elaboration paper.[2] We are delighted that the CONSORT statement is an ever-evolving tool and suggest that in the next version the checklist explicitly state that authors should (1) report and justify the magnitude of the minimal clinically important difference and (2) discuss and justify their interpretation of the clinical importance of the study result in relation to that difference.

We agree with Bart Harvey's comment. However, the goal for our study[3] was to highlight an important shortcoming in the reporting of randomized controlled trials rather than to document the precise frequency of this phenomenon. We believe that we were able to accomplish this goal with our relatively small sample size.

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