

Correspondance

Physician, know thy limits

I enjoyed the first article by Donald Redelmeier and colleagues on problems for clinical judgement.¹ I was particularly interested to note that overconfidence ranks high as a source of clinical errors (3 of the 9 causes of fallibility relate to overconfidence, if one considers unquestioning self-approval and unawareness of limits of judgement as aspects of overconfidence). I suspect that a key reason for this lies not in physicians' lack of knowledge of cognitive psychology but in the fact that hubris is actively encouraged and rewarded during medical training.

As someone who went to medical school after several other careers, I was often appalled by the way arrogance and overconfidence were encouraged during medical training. Indeed, during clinical training I was frequently criticized for expressing uncertainty and humility to patients or teachers. It struck me as ironic that awareness of the limits of one's knowledge or data is encouraged in graduate school (I have a PhD in biology), where the degree of uncertainty is far less than in clinical practice. Perhaps the level of certainty in professional discourse is inversely proportional to a profession's scientific rigour?

Will Rogers is reputed to have said, "The problem is not what you don't know but what you know that ain't so." I believe that medical educators should take this to heart and reform their approach accordingly.

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Reference

1. Redelmeier DA, Ferris LE, Tu JV, Hux JE, Schull MJ. Problems for clinical judgement: introducing cognitive psychology as one more basic science [commentary]. *CMAJ* 2001;164(3):358-60.

In their introductory article on problems for clinical judgement, Donald Redelmeier and colleagues stated that "examples of clinical judgement range from the monumental (such as whether to discontinue life-support for a patient

on dialysis) to the banal (such as whether to discontinue a telephone call when on hold with nephrology)."¹ The authors' example of a situation requiring monumental clinical judgement is unclear. If a patient is competent, he or she should make the decision to stop treatment. If the patient is not competent, then family members should decide. Physicians may, of course, need to determine if life-support is in fact only prolonging the dying process. I suspect it is this decision that the authors felt requires monumental clinical judgement.

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Reference

1. Redelmeier DA, Ferris LE, Tu JV, Hux JE, Schull MJ. Problems for clinical judgement: introducing cognitive psychology as one more basic science [commentary]. *CMAJ* 2001;164(3):358-60.

Do women treated for breast cancer at teaching hospitals really fare better?

Breast cancer is a disease that is notoriously heterogeneous. Virtually every week a new factor is identified to help predict which patients will have a superior chance of survival. Ruhee Chaudhry and colleagues provide another: whether the surgery is performed at a teaching or nonteaching institution.¹ Although this is an interesting factor to consider, their study is potentially damaging to community hospitals, particularly when the majority of breast cancer surgeries in Ontario are performed in nonteaching hospitals.

Tumour grading was not done in almost half of the cases in the community hospitals, whereas estrogen receptor status was not known in 21% of the teaching hospital cases. These differences in tumour characteristics, along with differences in Her-2/*neu* oncogene status, would likely account for the differences in

survival outcomes. The factors the authors suggested to try to explain the differences, including the use of multidisciplinary teams, closer follow-up and improved supportive care, are important in management, but they have never been shown to make any difference to survival.

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Reference

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.

Ruhee Chaudhry and colleagues suggest that treatment at teaching hospitals rather than community hospitals may be advantageous for women with small breast tumours.¹ I suggest that the reason women treated at teaching centres live longer than those treated at community hospitals has more to do with differences in the patient populations than with differences in the quality of treatment at the 2 types of hospitals.

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Reference

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.

In many jurisdictions there is increasing pressure on oncology services to specialize. Ruhee Chaudhry and colleagues provide evidence that survival following breast cancer treatments is better when care is provided at teaching hospitals rather than at community hospitals.¹ This is not supported by Golledge and colleagues, who found that specialization of breast cancer treatments, not the teaching status of the treating institution, affected outcomes.² From 1990 to 1992, care of breast cancer patients in a community hospital in England was managed by all 5 local surgeons. From 1993 onward, care of breast cancer patients was concentrated in the hands of 2

of those surgeons. Disease-free survival improved and the local recurrence rate decreased following specialization of services. The results were attributed to an increase in axillary dissection and more frequent use of tamoxifen and chemotherapy. Gillis and Hole reported similar post-specialization results in the west of Scotland.³ Although the teaching status of the treating hospitals was not reported in this study, it is likely that specialization occurred in both teaching and nonteaching hospitals, given the demographics of this region.

The teaching status of the initial treating hospital is unlikely to serve as a useful proxy for surgical specialization and use of adjuvant therapies. Breast cancer management is a multidisciplinary process; whether the initial surgery is done in Ottawa or Owen Sound is probably not relevant.

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References

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.
2. Gollidge J, Wiggins JE, Callam MJ. Effect of surgical subspecialization on breast cancer outcome. *Br J Surg* 2000;87:1420-5.
3. Gillis CR, Hole DJ. Survival outcome of care by specialist surgeons in breast cancer: a study of 3786 patients in the West of Scotland. *BMJ* 1996;312:145-8.

The Jan. 23, 2001, issue of *CMAJ* made a real attempt to bring together several articles on breast cancer, a topic of considerable importance. However, I found the paper by Ruhee Chaudhry and colleagues to be seriously flawed.¹

In this retrospective study, the women seen in community hospitals were markedly different from those seen in teaching hospitals. This could result in lead-time bias in favour of teaching hospital patients. There is indeed some evidence of this in the paper, as the tumours of women presenting to teaching hospitals tended to be smaller and less malignant tumours (ductal carcinoma in situ) than those of women presenting to community hospitals.

Thus, they would have had better outcomes irrespective of location.

In addition, the authors failed to describe the manner in which breast cancer was detected. There is a better outcome for breast cancer detected through screening mammography than for breast cancer detected clinically.

Lastly, we don't know the proportions of women who had axillary node dissections in each group. This procedure is used less often in community hospitals than in teaching hospitals, and thus there may be a greater potential for misclassification of the stage of disease in the community setting. Do the authors have any information on this important variable?

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Reference

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.

I am concerned by the conclusion reached by Ruhee Chaudhry and colleagues that patients who underwent surgery for breast cancer tumours smaller than 20 mm in diameter experienced better survival if they were initially seen in teaching hospitals rather than community hospitals.¹ I could not help but detect a degree of bias in this study against physicians in nonteaching hospitals. Statements such as "teaching status may affect patient outcomes directly because of better knowledge and skills" imply that surgeons in teaching hospitals are superior to those in community hospitals; this has no foundation in fact.

I agree with the authors that differences in patient outcomes between the 2 types of hospital need to be analyzed. If there is a factor that differentiates patient survival in the nonteaching versus teaching centres, it needs to be detected and addressed. If differences in outcome are "artifact[s] of misclassification," this study needs to be expanded to confirm or refute this point. In the meantime, however, let us not fall into the trap of publishing articles such as this that are

biased and will have a limited role in improving health care for Canadians.

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Reference

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.

Oncology is a difficult enough specialty to practise at the best of times; it has now become even more challenging as a result of the article by Ruhee Chaudhry and colleagues.¹ I can't believe this type of research was published, let alone placed as the lead article in *CMAJ*.

The teaching centre cases tended to have more favourable characteristics (smaller tumours, more favourable tumour grades and greater proportions of estrogen-receptor-positive tumours) than the community hospital cases. It should be noted that in fact more women were treated with adjuvant systemic therapy in the community hospitals than in the teaching hospitals (38% v. 30%). It is distressing that the authors draw conclusions with such far-reaching clinical implications from this study.

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Reference

1. Chaudhry R, Goel V, Sawka C. Breast cancer survival by teaching status of the initial treating hospital. *CMAJ* 2001;164(2):183-8.

[The authors respond:]

The purpose of our study was to describe the relationship between settings for initial treatment and outcomes from breast cancer on the basis of available data.¹ In our paper we acknowledged the limitations of these data. Nevertheless, we believe that it is important to publish such results to promote discussion. Improvement and accountability in our health care system are contin-