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.

Philip Barron
Surgeon
Ottawa Hospital — Civic Campus
Ottawa, Ont.

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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. Golledge 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 auxillary 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?

Peter Willard
General surgeon
Welland County General Hospital
Welland, Ont.

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.

Robert J. Fingerote
Gastroenterologist
Queensway-Carleton Hospital
Ottawa, Ont.

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.

Brian P. Higgins
Oncologist
Credit Valley Hospital
Mississauga, Ont.

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-

gent on access to such information.

Robert Myers, T.J. Muckle, Peter Willard and Brian Higgins would prefer that our results be attributed to differences in patient characteristics. We adjusted for the differences between the patient populations using statistical analysis, which increased the difference in survival from an overall relative risk of 0.67 (95% confidence interval [CI] 0.53–1.03) to an adjusted relative risk of 0.47 (95% CI 0.23–0.96) for women with tumours less than or equal to 20 mm in diameter. Unfortunately, the manner in which the tumour was detected was not routinely recorded by clinicians.

As Myers notes, information about tumour grade was more likely to be missing for women seen in community hospitals. These women experienced poorer survival than women with moderate-grade tumours. Conversely, information about estrogen receptor status was more likely to be missing for women seen at teaching hospitals, and these women experienced better survival than those with positive tumours. This could be viewed as a source of misclassification of patients or as an indicator of differences in the process of care. In either case, we believe it is important to examine the relationship with outcomes, as well as reasons for such potential differences in processes of care.

The study by Golledge and colleagues was a single-hospital study that looked at outcomes before and after introduction of specialization.[2] It did not, and could not by design, comment on impact of teaching status. Other British studies that have defined specialization in terms of teaching hospital status,[3] surgeon's workload[4] and local perception[5] have also found differences in survival. It is perhaps premature to conclude which aspect of specialization contributes to differences in outcome.

Robert Fingerote takes exception to wording in the introduction of the article and suggests that the study is biased. In fact, we were very careful to maintain a balanced approach in discussing possible interpretations of our results. It is our view that our article's wording is far more balanced than that of our correspondents.

Breast cancer treatment occurs within a complex system involving radiologists, surgeons, radiation and medical oncologists, pathologists and nurses, among others. We considered the initial treatment setting (a system of care) rather than the skills of individual clinicians. If the difference in survival that we observed can be attributed to differences in the process of care, we need to determine which element of the care provided at teaching hospitals is responsible for the differences and whether it can be applied to the community setting, particularly since the majority of women are initially seen at community hospitals.

Ruhee Chaudhry
Research associate
Women's Health Program
University Health Network
Toronto, Ont.
Vivek Goel
Chair
Department of Health Administration
University of Toronto
Toronto, Ont.
Carol Sawka
CEO
Toronto-Sunnybrook Regional Cancer Centre
Toronto, Ont.

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Raloxifene: handle with care

We have seen some women at high risk for breast cancer taking the selective estrogen receptor modulator raloxifene, in the belief that it is a breast cancer "preventive" with few of the risks or side effects of tamoxifen. Raloxifene is, in fact, not approved in North America for breast cancer prevention. Furthermore, perhaps because raloxifene has become available more recently, women, and sometimes physicians, do not seem to be aware that the risks of developing deep venous thrombosis, pulmonary emboli and hot flashes are similar to those seen with tamoxifen.[1]

We have also observed the frequent use of raloxifene by women who have completed the recommended 5-year course of adjuvant therapy with tamoxifen following a diagnosis of breast cancer. In randomized trials, there were more recurrences of breast cancer and more deaths in women who received adjuvant therapy with tamoxifen for 10 or more years than in those who received tamoxifen for 5 years.[2] This may be explained by the observation in animal and in vitro models that cells grown for long periods in the presence of tamoxifen can become dependent on it.[3]

Because raloxifene is very similar to tamoxifen, the prescription of raloxifene to a patient with residual tamoxifen-dependent breast cancer cells could promote the growth of such a cancer. In fact, it has recently been demonstrated in animal models that tamoxifen-dependent breast cancer cells can be stimulated by raloxifene.[4] Thus, physicians should be particularly concerned about the prescription of raloxifene in this situation.

If patients can develop tamoxifen-dependent breast cancers after protracted periods of therapy, perhaps women with a previous diagnosis of breast cancer who have not been treated with tamoxifen but who are treated with raloxifene for osteoporosis may develop raloxifene-dependent tumours. There are few safety data on the use of raloxifene in women with a previous diagnosis of breast cancer.[5,6]

In summary, raloxifene is not currently indicated for breast cancer prevention; it should not be prescribed as a substitute for tamoxifen as adjuvant therapy for breast cancer; it should not be prescribed to women who have completed 5 years of tamoxifen as adjuvant therapy for breast cancer; and the prescription of raloxifene to prevent or treat osteoporosis in women with a pre-