



More procedures, better quality of care? Is there a case for regionalization of pancreatic resection for neoplasm?

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Elsewhere in this issue (page 643), Marko Simunovic and colleagues¹ report data on in-hospital death (as case fatality rate) and mean length of stay for 842 patients who underwent pancreatic resection for neoplasm in Ontario over the 7-year period 1988/89 to 1994/95.

In most cases, resection for pancreatic carcinoma consists of a Whipple procedure or pancreaticoduodenectomy, an infrequently performed, technically demanding, high-risk operation. It consists of removal of the distal half of the stomach and the entire duodenum, including the distal common bile duct and the head of the pancreas, and subsequent re-anastomosis of the biliary tree, the distal pancreas and the stomach to the proximal jejunum. As treatment for a malignant tumour associated with only 5% to 20% survival 5 years after surgery among the 10% of patients in whom the procedure is technically feasible, the performance of this operation has been highly controversial, but it offers the only chance for cure. The complexity of the surgery and the rarity of its performance by the average surgeon have led to suggestions that it be offered in only a few regional centres.

To the best of my knowledge, the study reported here is the first of its type in Canada, where the health care system is publicly financed. The authors compare their results with those of similar studies in the US, which has a mixed public-private health care system and where financial or logistic barriers to care may lead to selective referral of "more healthy," lower-risk patients to private, high-profile centres (such as the Johns Hopkins Hospital² or Memorial Sloan-Kettering Cancer Center³) and better outcome results for the institutions in question.

Simunovic and colleagues conclude that the mean length of stay and the postoperative mortality rate were higher in hospitals performing a low or medium volume of procedures than in the high-volume hospitals. They also suggest that, at least for pancreatic resection for neoplasm, patients treated in high-volume hospitals may have better outcomes than those treated in low-volume hospitals and, by implication, that in the high-volume hospitals

better care is associated with the greater volume — an idea that will no doubt provide fodder for those who advocate regionalization of complicated, high-risk procedures.

I have a number of reservations about the methodology, results and conclusions of this study and therefore about the implications of this work for future policy related to health care delivery.

Surgical volumes were arbitrarily defined as low (fewer than 22 procedures over the study period), medium (22–42) or high (more than 42). These volume gradients were chosen post hoc on the basis of "the need for statistical stability and clinical relevance." In fact, the high-volume group consisted of only 2 of the 68 hospitals studied. One of these institutions accounted for almost 150 of the procedures and had a case fatality rate of about 2%; the other institution had nearly 60 cases and a case fatality rate of about 8%. The article is therefore more a testimony to the excellent results of a single institution than anything else. What would the data look like without the highest-volume hospital? The hospitals within the low- and medium-volume groups had widely differing mortality rates — and many had case fatality rates under 5%.

The authors found statistically significant differences in case fatality rate and length of stay when the low- and medium-volume groups were compared separately with the 2 high-volume centres, but the results of a comparison between the low- and medium-volume centres are not presented. In fact, mean case fatality rate and length of stay were *greater* for the medium-volume hospitals than the low-volume ones, although the differences were not significant.

Simunovic and colleagues discount the findings of Wade and associates,⁴ who did not find a positive volume-outcome relation in a similar study, on the basis that their sample lacked high-volume hospitals. However, in the Ontario study, as in those of Lieberman and colleagues³ and Gordon and collaborators,² the converse may well be true, that is, that because of a multitude of possible



factors other than simply hospital volume, a single institution with a huge number of procedures and exceptional results has skewed the data to favour "high volume." Indeed, Fig. 1 (on page 646) does not indicate a linear relation between increasing volume and outcome improvement for surgical volumes between 0 and 60, which puts into question the authors' statement that "sensitivity analyses showed that ... results were robust when volume gradients were changed."

Simunovic and colleagues also examined early readmission data, to capture additional deaths and length of stay that might be attributable to the original procedure, but the essential raw data are not provided. Such readmissions included transfers to a convalescent hospital. Older patients and patients in rural areas with fewer home care resources (a more common situation for low- and medium-volume hospitals) are perhaps more likely to be transferred to a convalescent institution where a 2-, 3- or 4-week stay may be arbitrary and standard, which would artificially increase length of stay. The authors found that the odds of death for patients undergoing the procedure in teaching hospitals was double that for patients in nonteaching hospitals, but hospital teaching status is then discounted as a significant predictor of death on the basis of the readmission data, which are not provided for readers' scrutiny. Other potentially important correlations between teaching and nonteaching status, hospital size, and low, medium and high volume are not given.

The data source for this retrospective study was coded discharge abstracts collected by the Canadian Institute for Health Information (CIHI), which uses the *International Classification of Diseases*, 9th revision, clinical modification⁵ (for diagnosis) and the *Canadian Classification of Diagnostic, Therapeutic and Surgical Procedures*⁶ (for surgical procedures). The limitations of this source relative to direct chart inspection were demonstrated by Taylor⁷ in a study of laparoscopic common bile duct injuries in Ontario. Further, although Simunovic and colleagues account for comorbidity, it is not clear whether comorbidity in the CIHI database included all secondary diagnoses or tumour staging and postoperative complications. It is important to control for both coexisting morbidity and advanced disease. Lieberman and colleagues³ included tumour staging and all postoperative complications as an indirect control for severity of illness. In the Ontario study, length of stay was considered a principal marker of quality of care, on the assumption that patients experiencing postoperative complications would have a greater length of stay. Although this may be true, length of stay is also determined by the patient's age, the surgeon's aggressiveness and attitudes, hospital and peer pressure for early discharge, bed availability, and availability of home care and convalescent services and resources, all of which may

differ according to geographic location, hospital teaching status and hospital size. The younger age of the patients treated and the greater proportion of nonurgent procedures (perhaps reflecting referral after preliminary work-up and treatment elsewhere) at the highest-volume hospital may be significant enough to explain some of the difference in length of stay. Clearly, better measures of quality of care are needed.

The authors address hospital procedure volume but provide no data on the number of surgeons or surgeon procedure volume. The number of surgeons at each institution, in particular the highest-volume institution, might be important. Could these excellent results be due to the surgical prowess of 1 or 2 highly skilled individuals? Perhaps of equal importance might be their experienced judgement in optimal patient selection. The study from New York, which involved a risk-adjusted perioperative mortality analysis,³ suggested that both hospital and surgeon volume are important determinants of postoperative death, but a hospital's experience, as opposed to individual surgeons' experience, was critical. The observation that many inexperienced surgeons work in low-volume hospitals confounds the issue. Of the 748 surgeons performing pancreaticoduodenectomy in that review,³ only 4 were considered to have performed a high volume of procedures; likewise, only 2 hospitals were classified as "high volume." More data are needed to differentiate individual surgeon volume from hospital volume.

In the Maryland study,² which reviewed 501 cases of pancreaticoduodenectomy over the 5½-year period 1988–1993, 54% of the procedures were done at a single centre — the Johns Hopkins Hospital, which averaged 50–60 cases/year. The remaining 46% of procedures were done at 38 different hospitals, and the maximum case load was only 20 cases over the entire study period. The superior outcomes of the Johns Hopkins Hospital were similar to those of the highest-volume institution in the Ontario study. Gordon and collaborators² suggested that this regional provider had a unique group of health care professionals with special expertise, including surgeons, anesthesiologists, intensivists, infectious disease specialists and nutrition advisors. Other possible explanations include early, experience-driven detection and treatment of complications.

The outcome of some surgical procedures depends to some extent on hospital or surgeon volume. However, the precise reasons — undoubtedly multifactorial — remain obscure. Which procedures should be regionalized and at what volumes are controversial questions with inconclusive answers, as recently reported elsewhere by Simunovic.⁸ As a surgeon who performs pancreatic resection, I believe that the authors' premise is correct. However, complications, readmissions, functional status, qual-



ity of life and survival after discharge must also be measured. Until that is done, I would be reluctant to advocate centralization of such procedures in designated centres, which would require a major commitment of both human and monetary resources to those centres. Instead I would insist that all institutions meet designated standards of performance. This is an important and provocative study. What the statistics reveal is interesting, but what they conceal is critical!

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