



lated to the dreaded "length of stay" for each patient admitted in the previous year.

As a modestly successful family physician who saves the government more money than it can possibly know, I will be regarded askance if the mean length of stay for my patients is greater than that of others in my peer group. However, the data on which this appraisal of my services is based deserve closer study, as Marshall correctly points out. I admitted just 40 patients during the most recent year reviewed, their illnesses falling into 34 case management groups. There were only 5 diseases for which I cared for more than 1 patient, and none for which there were more than 3 patients. From these limited data, "they" calculate an "average" length of stay. I was taught that at least 3 data points were

needed to calculate a meaningful average, and that more than 3 values would be preferable. And what about the range? The mean for case management group 011 within my peer group was 9.90 days, but was the standard deviation 0.01 or 8.9 days? Silence on this elementary point. In addition, I found numerous statistical vagaries and even errors on my print-out.

A patient of mine who is employed in the medical records department of another local hospital knows someone who works in both that hospital and my own and who reports that the coding practices of the 2 hospitals are "quite different." We ought not to accept unknowing bean-counting.

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**[The author responds:]**

**D**r. Ford raises an excellent point about employing administrative databases for tracking health services utilization: for such purposes, it is necessary to compare apples and apples. Nonetheless, his letter indicates to me that his hospital's program is probably having the desired effect. From the information supplied each year, he can review the distribution of his patients according to disease; the data he receives are sufficiently detailed that he can validate them, and he can, if questioned on some overall average, mount a defence based on factual information. Awareness of utilization is achieved and data are fed back to the health care provider who, realizing their importance, will in turn report back to the medical records department on issues of accuracy. Presumably, members of the peer group have the opportunity to judge the process and will respond intelligently to Ford's point.

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**Studying workplace health**

**T**he review article "Wellness programs: a review of the evidence" (*CMAJ* 1998;158[2]:224-30), by Denise Watt and colleagues, addresses an important topic but has 2 important limitations. First, limiting the search to the MEDLINE database excluded many high-quality journals that often publish articles on this topic. Second, insisting that the studies for review had to have randomized controlled designs further excluded many high-quality studies.



In 1996 and 1997 the *American Journal of Health Promotion* published a series of 11 reviews, written by a team of 22 authors, on the health impact of workplace health promotion programs. A total of 365 articles met the review criteria, of which 29% had randomized controlled designs and 26% had comparison groups that were not randomly assigned. The series drew specific conclusions about the impact of programs in each of the intervention areas, such as fitness, nutrition and stress management. It revealed that these programs produce positive short-term changes in knowledge, attitudes, behaviours and health conditions. The long-term impact of the programs was not clear, both because of relapse and because too few studies measured this. The studies with randomized controlled designs had positive effects, although these were not as strong as for the studies as a whole. The quality of research in this area must continue to improve, but it is already comparable to or better than that of research used to support most nondrug medical interventions.

In addition, a recent article covered 40 studies that met the review criteria.<sup>1</sup> Overall, 88% of the studies showed cost savings because of re-

ductions in medical care costs or absenteeism, or both. These ratios were similar for the studies that had randomized controlled designs and those that did not. Among the 13 studies that calculated cost-benefit ratios, the returns were all positive and ranged from US\$2.50 to US\$6.00 for each dollar invested. The studies with randomized controlled designs showed the highest rates of return.

#### Michael O'Donnell, PhD

Editor-in-Chief

*American Journal of Health Promotion*

Lawrence, Kans.

#### Reference

1. Aldana S. Financial impact of workplace health promotion programs and methodological quality of the evidence. *Art Health Promot* 1998;2(1):1-8.

#### [Two of the authors respond:]

**D**r. O'Donnell makes 2 important points about the quality and extent of evidence that goes beyond what we covered in our review. We purposely limited our search to MEDLINE because it is usually the first database searched by primary care physicians and the only one to which some physicians have access. Other databases, including the social

science ones, were not searched because we wanted to focus on the resources typically used by primary care physicians.

We agree that studies of health promotion programs and of wellness in the workplace are important areas for further research. We specifically excluded them, pointing out that "studies of chronic psychiatric disorders and diseases, such as cancer and AIDS, were excluded, as were studies of health promotion programs in the workplace that used productivity as the outcome measure." We feel that these studies represent a body of evidence that stands on its own, with variables and outcome measures that are relevant and particular to the workplace and to occupational health medicine. That is not to imply that work in these areas does not represent valuable knowledge. However, we decided to exclude these special areas to determine if there was any evidence that applied to the general population.

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