Letters

- Protecting children from lead in tap water
- Hospital standardized mortality ratios
- Smoking cessation trials

Protecting children from lead in tap water

Lead in drinking water is still an important health issue in Germany. There is concern about the association of lead exposure with neurologic and intellectual deficits as described by Mark Payne[1] and with hearing impairment in infants; higher levels of lead exposure have been associated with increased morbidity in adults and with cognitive decline in older people.[2,3] Although the maximum allowable lead concentration in tap water will be reduced from 0.025 mg/L to 0.01 mg/L by December 2013 in Germany, a 2005 test of 237 000 random samples of tap water showed that the lead concentration in more than 5% of the samples exceeded 0.025 mg/L in several regions.[4] In Germany it is recommended that pipes be flushed to reduce lead levels by running water for at least 5 minutes every morning, as also suggested by Payne.

Data from the German Federal Environment Agency suggest that even at a lead concentration of 0.01 mg/L in tap water, infants should not consume more than 0.4 L of tap water per day if the water comes from plumbing systems containing lead. As an interim solution until all lead is removed from plumbing, it has been proposed that infants should be given bottled water to avoid exposure to tap water during childhood because the threshold exposure level for lead toxicity has not yet been established.[5]

Most German municipalities are taking responsibility for removing lead from plumbing systems to protect infants and toddlers in particular from the health hazards associated with exposure to lead in drinking water. In Hamburg alone, all plumbing systems containing lead will be replaced for 28 000 households.

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Competing interests: None declared.

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DOI:10.1503/cmaj.1080107

Hospital standardized mortality ratios

We do not agree with Kaveh Shojania and Alan Forster's assessment of the value of reporting hospital standardized mortality ratios.[1] Our view is that the public reporting of hospital standardized mortality ratios in Canada provides a useful and much needed focus on the quality of health care.

The authors criticize the validity of hospital standardized mortality ratios on the basis that they "correlate weakly with other measures of quality of care" and cite as an example one of the findings from a 1987 US study by Dubois and colleagues.[2] However, Dubois and colleagues reported in the same study that "detailed reviews by physicians of the records of patients who died during hospitalization revealed a higher rate of preventable deaths in the high [outlier hospitals] than in the low [outlier hospitals]."

In some circumstances, process and outcome measures would be expected to be correlated, but in others they would not, for a number of valid reasons. When these 2 types of measures produce different results, we should not treat the process measures as the gold standard against which a "big dot" (i.e., broad-based) outcome measure like the hospital standardized mortality ratio should be assessed. Both types of measures have strengths and limitations and as such it is important that both be considered when examining the quality of health care within a hospital.

The authors also criticized the precision of the hospital standardized mortality ratio on the basis that "random variation likely accounts for much of the observed differences in mortality among institutions." In our report of hospital standardized mortality ratios,[3] we presented the hospital standardized mortality ratios results and confidence intervals only for large hospitals and regions to minimize the effect of random variation and to inform users of the level of precision associated with a given hospital standardized mortality ratio.

Producing hospital standardized mortality ratios for Canadian hospitals responds to the need for a "big dot" measure of the quality of health care. With an understanding of their limitations and in conjunction with other measures and information, hospital standardized mortality ratios can be used for their intended purposes. Within this context, the hospital standardized mortality ratio is both a valid and useful measure. Our work on developing more and improved quality measures is ongoing.

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Competing interests: The Canadian Institute for Health Information reports hospital standardized mortality ratios for Canadian hospitals and health regions.

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DOI:10.1503/cmaj.1080101

[The authors respond:]

The finding of higher rates of preventable deaths in hospitals with high mortality in the study by Dubois and colleagues[1] applied only to the analysis of deaths from pneumonia, for which the physician reviewers exhibited very poor agreement (kappa = 0.11). Moreover, in citing Dubois and colleagues in our commentary,[2] we did not presuppose that process problems constitute the gold standard for quality indicators. However, process change represents the major aspect of health care delivery under providers' control. If hospital standardized mortality ratios correlate poorly with the need for process changes (as in the study by Dubois and colleagues and a recent study from Ontario[3]), it remains unclear how hospital standardized mortality ratios can serve as a useful screen for quality problems.

Few would argue there are quality problems in the Canadian health care system. The Canadian Adverse Event Study found preventable events in every hospital studied.[4] Ideally, all hospitals would accept these results as fact and undertake vigorous efforts to look for quality problems rather than wait for the results of their hospital standardized mortality ratios analysis. Given that this does not occur, one might argue for the use of a screening test, to engage hospitals.

However, as we outlined in our commentary, the hospital standardized mortality ratio has both low sensitivity and poor specificity for quality problems.[2] This is not unheard of among screening tests. Despite terrible performance characteristics, the fecal occult blood test improves detection of colon cancer, presumably because the results of annual application of this test randomly scare sufficient numbers of patients into undergoing the test they should have agreed to undergo in the first place, namely colonoscopy.

Unfortunately, whereas colon cancer really does reside in the colon, most quality problems do not manifest themselves in the charts of deceased patients.[5] Thus, rather than engaging hospitals in vigorous and effective detection of quality problems, promotion of hospital standardized mortality ratios focuses hospitals' attention on chart reviews of in-hospital deaths, which has all the inconvenience of colonoscopy but not comparable benefits.

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Competing interests: None declared.

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DOI:10.1503/cmaj.1080102

Smoking cessation trials

I submit that the meta-analysis by Mark Eisenberg and colleagues on pharmacotherapies for smoking cessation[1] is grounded in a false premise, namely that researchers were somehow able to hide the onset of nicotine withdrawal symptoms from control group members, whose previous quitting history had taught them exactly how withdrawal felt (a rising tide of anxieties, anger, dysphoria, concentration difficulty and sleep fragmentation within 24 hours of quitting), and that researchers found a way to mask the reduction of withdrawal syndrome for intervention group members. Mooney and colleagues found that studies of nicotine replacement therapies are generally not blind in that participants correctly guess assignment at rates significantly above chance.[2] When this finding is combined with the meta-analytic finding by Eisenberg and colleagues that smoking cessation with pharmacologic treatment is nearly always more successful than cessation without pharmacologic treatment in clinical trials and the fact that cessation with pharmacological treatment has failed to be more successful than cessation without such treatment in nearly all of real-world surveys conducted to date,[3] it strongly suggests that the pharmacologic treatment of chemical dependency may be the only known research area in which blinding is impossible.

Mooney and colleagues warned that the validity of the results of clinical trials of nicotine replacement therapies could be questioned if future studies failed to assess the integrity of study blinding.[2] This warning has not been heeded. How badly can study blinding fail? Dar and colleagues found that control group members were 3.3 times more likely to correctly guess that they had received placebo than to incorrectly guess that they had received nicotine (54.5% v. 16.4%).[4]

In the era in which pharmacologic therapies are used for smoking cessation, the decline in smoking rates seen previously has come to a screeching halt.[5] Although excitement about varenicline should briefly improve cessation rates, Canadian policy-makers must realize that toying with chemicals that stimulate the dopamine pathway is not more effective than teaching those hooked on nicotine how to quickly and more comfortably adapt to natural stimulation.

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Competing interests: John Polito is the editor of WhyQuit, a forum on abrupt nicotine cessation. He was compensated by the State of South Carolina for presenting 63 prison programs on abrupt nicotine cessation in 2007 and 2008.

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