



Are alternative funding plans a good idea?

Marshall Godwin and colleagues are to be congratulated for their survey of the effect of an alternative funding plan on physician referrals¹; however, we have several concerns.

First, the authors state that to interpret the “negative perceptions” of the respondents regarding the alternative funding plan one should “consider the confounding effect of other changes in the health care system in Ontario.” A control group should have been selected. Reductions in health care allocations in Ontario are not exclusive to the academic centre studied.

Second, a null hypothesis would be that physician referral would not be affected by changes in funding. However, 39% of the surveyed physicians sent fewer patients to the study’s tertiary care institution, and 37% sent more to the tertiary care (nonstudy) institutions. Although we realize the limitations of post-hoc analysis, this shift is probably nonrandom. Moreover, the consultant physicians at the nonstudy centres who were already dealing with cutbacks were now forced to cope with an increased caseload deferred from the study centre. Fortunately, some physicians within the study centre opted out of the alternative funding plan and assumed a major increase in referral work, according to the authors’ data.

Third, the authors infer support for the alternative funding plan among referring physicians by commenting that “only 39% ... indicated that they would not want to be part of an alternative funding plan.” This is a confusing interpretation. Their results show that only 35% of the referring physicians stated that they would want to participate in an alternative funding plan.

Finally, the alternative funding plan is a funding experiment, requiring more bureaucracy, implemented without evaluative plans. For a publicly funded enterprise, citizens have the right to ask about the “value-added” features of this new

bureaucracy. In the absence of unequivocal evidence of benefit, would it not be reasonable to terminate the experiment and restore the previous system?

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Reference

1. Godwin M, Shortt S, McIntosh L, Bolton C. Physicians’ perceptions of the effect on clinical services of an alternative funding plan at an academic health sciences centre. *CMAJ* 1999; 160(12):1710-4.

[Two of the authors respond:]

We commend Michael Jacka and Brian Milne for their thoughtful reading of our paper¹ and welcome the opportunity to clarify the 4 points they have raised.

First, comparison with physicians from nonacademic centres might be interesting but would represent a different study; our study was designed to document perceptions of immediate stakeholders in the alternative funding plan. One identified perception was that the alternative funding plan had less impact on service delivery than other contemporaneous changes in the broader health care system. Obviously, this does not imply that reductions in resources have been confined to academic centres.

Second, 71% of the referring physicians stated that they had not increased referrals to consultants participating in the alternative funding plan in Kingston or to consultants in other secondary care centres. However, 39% stated that they had decreased their referrals to consultants participating in the plan. This suggests that referring physicians themselves may be providing more care by eliminating marginally necessary referrals, a trend that must be considered as an improvement in appropriateness of care. Although not mentioned in our paper, the survey revealed that a similar proportion of consultants participating in the alternative

funding plan (14%) and of those not participating in the plan (16%) reported increased referrals following implementation of the plan. In aggregate, we do not view these changes as indicating a large workload shift.

Third, 39% of the referring physicians stated that they would not wish to be part of an alternative funding plan. However, 26% were uncertain, and 35% stated that they would like to participate. We agree that this may suggest an ambivalence on the part of the referring physicians, in contrast to alternative funding plan consultants (again not mentioned in our paper), of whom two-thirds were pleased to be in the alternative funding plan, while one-quarter were not.

Finally, we agree that “citizens have the right to ask about the ‘value-added’ features” of the alternative funding plan. Jacka and Milne will be pleased to learn that the South Eastern Ontario Academic Medical Association commissioned 19 evaluation studies, in addition to an interim and final evaluation. Relevant aspects have been and will continue to be shared with the citizens’ representatives, the Alternative Payment Branch of the Ontario Ministry of Health, to assist in crafting future iterations of the program.

Given the legitimate concern Jacka and Milne show for evaluation, we are puzzled by their suggestion that the South Eastern Ontario Academic Medical Association should “restore the previous system.” The fee-for-service system has never been subject to evaluation showing “value-added” for the major stakeholder: the tax-paying public. On the contrary, it has been shown to encourage high-volume practice, bearing an undetermined relationship to patient need.²

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References

1. Godwin M, Shortt S, McIntosh L, Bolton C. Physicians’ perceptions of the effect on clinical



services of an alternative funding plan at an academic health sciences centre. *CMAJ* 1999; 160(12):1710-4.

- Shortt SED. *The doctor dilemma, public policy and the changing role of physicians under Ontario medicare*. Montreal: McGill-Queen's University Press; 1998.

Radiofrequency radiation: What's safe?

In their study of radiofrequency radiation in Vancouver schools, Art-narong Thansandote and colleagues "... conclude[d] that the levels measured during [their] study posed no health risk to the students, school staff or the general public"¹ In fact, they did not measure health. They showed that levels of radiation in schools with antennae nearby are thousands of times higher than background radiation levels, which, granted, are lower than the mysterious safety code levels. I looked up the authors' references and I am unable to figure out how the safety limits were determined. To conclude that there is no health risk seems to be an inappropriate leap of faith far beyond what the data would warrant.

We know that x-radiation at a level substantially lower than that which causes immediate harm is still potentially lethal over time. Why should we think that radiofrequency radiation is any different? This study does not provide any reassurances to this pertinent concern.

Ronald G. Cridland, MD
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Reference

- Thansandote A, Gajda GB, Lecuyer DW. Radiofrequency radiation in five Vancouver schools: exposure standards not exceeded. *CMAJ* 1999;160(9):1311-2.

[The editorialist responds:]

Ronald Cridland questions the interpretation of health risk on the basis of radiofrequency exposure measurements and comparison to a permissible exposure guideline in the study by Thansandote and colleagues.¹ The results of risk assessment may change over time as additional studies become

available. In addition, various assumptions must be made, such as the shape of the dose-response curve at low doses. Given these uncertainties it is difficult from a scientific perspective to deny risk definitively even at very low levels of exposure, especially when dealing with stochastic effects such as cancer.

The permissible exposure limits for radiofrequency radiation questioned by Cridland are based on the threshold for subtle thermal effects in tissue.² The specific absorption rate depends on frequency, and therefore the permissible exposure levels, measured in power density units, vary with frequency. The thermal effects associated with radiofrequency radiation demonstrate a clear threshold phenomenon. The very low levels of exposure to radiofrequency radiation measured in 5 Vancouver schools by Thansandote and colleagues¹ were orders of magnitude below the permissible limits and should not be associated with any thermal effects. The other health outcomes associated with radiofrequency — nonthermal effects and cancer — are at present speculative.

The interpretation of risk from ionizing radiation mentioned by Cridland is different because there is definitive evidence of risk of carcinogenicity for ionizing radiation and there are good data regarding dose response. Even if these currently speculative outcomes for radiofrequency radiation were later shown to be present, the risk would be dependent on absorbed dose and hence low in areas of measured low exposure of this ubiquitous form of non-ionizing radia-

tion. Therefore, the results of the study by Thansandote and colleagues¹ should be reassuring after evaluation of the probability of any adverse health effects being associated with such exposure.

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References

- Thansandote A, Gajda GB, Lecuyer DW. Radiofrequency radiation in five Vancouver schools: exposure standards not exceeded. *CMAJ* 1999;160(9):1311-2.
- Elder JA. Radiofrequency radiation activities and issues: a 1986 perspective. *Health Phys* 1987;53:607-11.

[Two of the authors respond:]

Ronald Cridland is correct in stating that we did not measure health in our study of radiofrequency emissions at several Vancouver schools.¹ However, our conclusion that there is no apparent risk to human health remains valid. It is based on the fact that the measured radiofrequency power densities were thousands of times below the Safety Code 6 limits, which incorporate a 50-fold safety factor from the scientifically established thresholds for harmful effects. Although we acknowledge that there is a body of evidence of biological effects at levels near or slightly below the Safety Code 6 limits, the evidence of adverse health effects at these intensities is weak. If one then considers the low probability of adverse health effects occurring at radiofrequency radiation levels thousands of times below the lim-

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its in Safety Code 6 and other international standards (as were measured in the schools), then our conclusion seems justified.

To clarify another point, we made no attempt in this study to compare the measured levels of radiofrequency radiation to background radiation levels. Although it is true that the radiofrequency radiation levels measured in our study are much higher than naturally occurring (background) levels, this is necessary to make a radio system function properly.

Finally, with reference to Cridland's last remark, it has been known for some time that the physical properties and effects of x-radiation and radiofrequency radiation on matter are vastly different. As Cridland implies, ionizing radiation (x-rays) at low intensities possess sufficient energy to directly break chemical bonds in material such as DNA. This is not the case with the radiofrequency radiation investigated in this study, which possesses a photon energy at least 6 orders of magnitude lower than that of x-rays.

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Reference

1. Thansandote A, Gajda GB, Lecuyer DW. Radiofrequency radiation in five Vancouver schools: exposure standards not exceeded. *CMAJ* 1999;160(9):1311-2.

Imaging errors

I have read with interest the recent *CMAJ* series on tuberculosis. As a radiologist, however, I feel compelled to comment on the article on extrapulmonary tuberculosis.¹

Fig. 1 does not show left mid-ureteral narrowing and upper tract dilatation. It shows multifocal right ureteral disease and irregularity of the urinary bladder wall. There may be upper urinary tract dilatation, but this is mostly obscured.

Fig. 2 does not show narrowing of the L3-L4 disk, nor does it show a filling defect in the intrathecal contrast. The narrowing is at L4-L5, where there are changes associated with discitis. The intrathecal contrast shows extrinsic compression on the thecal sac at this level; a filling defect implies an intrathecal abnormality.

Fig. 3 does not show miliary nodules. These may be present on the original film but are not evident on this poorly reproduced image. A magnified view of one portion of the lung, carefully reproduced, would be necessary to show miliary nodules.

Fig. 4 shows a destructive process within the bone rather than inflammation of the meninges. The meninges are not seen on bone-windowed CT images. Inflamed meninges can generally be seen only on contrast-enhanced MRI scans.

Given the importance of imaging to modern diagnosis, and the ease with

which high-quality images can be made and reproduced in the electronic era, there is no excuse for poor reproductions and errors such as these. The title page affirms that this article has been peer reviewed; I assume that none of these peers has expertise in imaging. Perhaps *CMAJ* would be better served by ensuring review of diagnostic images by a radiologist before publication.

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Reference

1. Fanning A. Tuberculosis: 6. Extrapulmonary disease. *CMAJ* 1999;160(11):1597-603.

[The author responds:]

John Clark is correct that the imaging of the tuberculous lesions would have been much more accurately described had an expert in imaging been involved at the point of peer review.

In Fig. 1, the narrowing at mid ureter is indeed obscured. The changes in the right ureter are in fact present, but they were less obvious in the initial illustration than the obstructed left ureter. In Fig. 2, the error in calling the lumbar lesion 3-4 instead of 4-5 was mine. In Fig. 3 the miliary lesions were apparent in the film but lost definition in the printing process. In Fig. 4 the bone lesion is indeed the most obvious one.

Clark points out the critical importance of imaging in the diagnosis of tuberculosis. Without daily interaction with radiologists I would be unable to function. Would that I had consulted them in the final drafting of the paper.

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Dialysis patients with tuberculosis

Pour écrire à la rédaction

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We noted with interest the prevalence projections by Douglas Schaubel and colleagues for end-stage renal disease in Canada to 2005 and their impact on health care utilization.¹ We would like to highlight an additional comorbid condition that will likely affect the management of these patients.

We recently established, for the first time on a population basis, the risk of tuberculosis (TB) among dialysis patients in British Columbia.² All cases of TB in British Columbia are reported to Tuberculosis Control, and specific risk factors for TB including dialysis are identified. Likewise, all individuals receiving dialysis are registered in a central registry. We compared the dialysis and TB registries for the study period and identified all confirmed active cases of TB. We determined that the risk of TB among the dialysis population is 25.3 (95% confidence interval 22.86–31.49, $p < 0.001$) times greater than that for a similar age-matched

population.

This marked increase in the risk of active TB brings with it 2 important messages. Dialysis patients should be screened for the presence of tuberculous infection; although a significant proportion of patients may be anergic, many retain their ability to mount a response.³ In the presence of a positive purified protein derivative (PPD) response, isoniazid chemoprophylaxis should be strongly considered. We recently reported the therapeutic option of twice weekly, directly observed chemoprophylaxis⁴ and in the context of hemodialysis this represents an ideal way to ensure completion of therapy. Our finding should also alert physicians to the importance of considering TB in the presence of fever in their dialysis patients.

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References

1. Schaubel DE, Morrison HI, Desmeules M, Parsons DA, Fenton SSA. End-stage renal disease in Canada: prevalence projections to 2005. *CMAJ* 1999;160(11):1557-63.
2. Chia S, Karim M, Elwood RK, FitzGerald JM. Risk of tuberculosis in dialysis patients: a population based study. *Int J Tuberc Lung Dis* 1998;2:989-91.
3. Adler JJ, Patt C, Seckler Smirnoff M. Tuberculin and anergy skin-testing of chronic hemodialysis patients [abstract]. *Am J Respir Crit Care Med* 1997;155(Suppl):A22.
4. Heal G, Elwood RK, FitzGerald JM. Acceptance and safety of directly observed versus self-administered isoniazid preventive therapy in aboriginal peoples in British Columbia. *Int J Tuberc Lung Dis* 1998;2:979-83.

Correction

In Table 2 of the recent article by Tej Sheth and colleagues,¹ the death rate for men of European origin in the category "other cardiovascular disease" should have been 105.9, not 05.9. We regret this error.



Reference

1. Sheth T, Nair C, Nargundkar M, Anand S, Yusuf S. Cardiovascular and cancer mortality among Canadians of European, south Asian and Chinese origin from 1979 to 1993: an analysis of 1.2 million deaths. *CMAJ* 1999;161(2):132-8.