or fatal events in hypertensive patients.

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H-1B or not to be?

The article "Deportation proceedings against Canadian MDs may hold lesson for others heading south" (*Can Med Assoc J* 1997;157[7]: 934-5), by Milan Korcok, outlined the problems 2 Canadian physicians encountered after seeking to practise in the US. However, it omitted an important legal fact.

Under the North American Free Trade Agreement, Canadian physicians are not permitted to practise in the US "on TN visas"; these documents only allow them to teach or perform research. The proper temporary category for physicians wishing to practise in the US is H-1B, and to declare that documents for this category "are rarely granted" is a gross overstatement. My firm has obtained approval of well over 100 H-1B petitions on behalf of Canadian physicians.

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HIV and blood, circa 1982

After reading Dr. John Hoey's editorial "Human rights, ethics and the Krever inquiry" (*Can Med Assoc J* 1997;157[9]:1231), I would like to share my efforts to prevent the spread of HIV through Red Cross blood products in New Brunswick in the early 1980s.

At that time, I was a minor member of the [Red Cross's] provincial board and had no independent authority. During one of our meetings the question of testing donated blood for HIV was raised, and we debated the issue for half an hour. I was the only physician present, and I strongly recommended that such testing be done. The nonmedical board members were not really opposed to testing, but they were worried about the questions it might raise. They were concerned that people who were "healthy" but positive for HIV would, as a result of donating blood, learn from the Red Cross that they had a potentially fatal disease.

I said that testing should be done but was even more adamant that HIV-positive donors must be informed and must not be allowed to make further donations. After an argument, the topic was suddenly dropped without a vote being taken. The minutes of the meeting, distributed later, contained no mention of the discussion or the debate about the problems involved, and the topic was not raised again.

I resigned from the board shortly after. Hoey is correct in stating that nonmedical members of the Red Cross at that time were eager not to give any hint that the Red Cross was hostile to gay people. Because the whole political world seemed to be of the same opinion, I did not write letters to the editor or others — I was sure they would never be published.

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Coping with acronyms

The article "A place in the shade: reducing the risks of UV exposure" (*Can Med Assoc J* 1997;157[2]: 175-6), by Drs. Konia J. Trouton and Christina J. Mills, contains a total of 7 different acronyms. The acronyms themselves are easily identified because they appear in capital letters. But their definitions are hard to find because they are in lowercase letters.

Perhaps *CMAJ* could save its hapless readers some time by providing a glossary of the acronyms for each article.

W. Robert Harris, MD
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Primary prevention of heart disease and stroke

Dr. James P. McCormack and colleagues, in their article "Primary prevention of heart disease and stroke: a simplified approach to estimating risk of events and making drug treatment decisions" (*Can Med*

Assoc J 1997;157[4]:422-8) have published very practical nomograms for calculating the risk of cardiovascular and cerebrovascular events in individual patients. These tools are an example of how treatment decisions can be made rationally, if the physician has adequate information about risks and benefits. Unfortunately, such information is often not available at all, and even when it does exist, it is seldom readily accessible to clinicians at the point of care.

Given that such access to information is precisely what clinicians need, it is surprising that the authors view as cumbersome the use of a computer to calculate risk factors. On the contrary, computers are among the best tools for obtaining information. If every physician's office were equipped with a computer with connections to the Internet, CD-ROMs and other knowledge sources, the information available at the point of care would increase substantially, and there would be improvements in decision-making, patient satisfaction and quality of care.

I therefore strongly encourage the authors to supplement their nomograms by creating a Web site about the primary prevention of heart disease and stroke, and updating the site as new studies are published. Users would enter a patient's age, sex, lipid levels, blood pressure and other risk factors, and software at the Web site would calculate the patient's risk of cardiovascular and cerebrovascular events, as well as the risk reduction (relative, absolute and "number needed to treat") that could be expected with therapy to reduce blood pressure or cholesterol levels.

I also encourage all physicians in Canada to get access to the Internet, so that they will have more of the information they need to make informed treatment decisions.

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I would like to comment on the use of meta-analyses of short-term intervention trials to estimate the potential benefits of antihypertensive therapy.

Although randomized clinical trials are the gold standard for evaluating the efficacy of most interventions, recent data from the Framingham Study[1] suggest that the hypertension trials may underestimate the long-term benefits of antihypertensive therapy. These trials were designed to test the effect of short-term interventions (3–6 years) on end-points such as stroke, myocardial infarction and death. In young or middle-aged patients, the risk of these end-points occurring within 5 years (the time frame of most of the studies) is low, and the goal of antihypertensive therapy "is not to prevent an unlikely hypertension-related event . . . but rather to prevent or retard the development of cardiovascular lesions and help the subject attain their full life span."[2] These trials may also have systematically underestimated the degree of any short-term treatment effects as a result of administration of active treatment to high-risk individuals in the placebo arms of the trials, recruitment of low-risk subjects into the trials, greater loss to follow-up of high-risk subjects and lack of statistical power in many of these trials.[3] Finally, the relative risk reductions published in the meta-analyses represent the average treatment benefits for modest decreases in blood pressure (5–6 mm Hg diastolic, 10 mm Hg systolic) and, as such, probably underestimate the potential benefits for the individual in whom greater reductions in blood pressure may be achieved.

These limitations raise the question of how best to assess the potential long-term benefits of antihyper-

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tensive therapy. Although randomized clinical trials with long-term follow-up (on the order of 20–30 years) would be ideal, it is highly unlikely that these will be carried out. The use of actuarial data[2] and observational outcomes research[4] have recently been advocated, but these techniques are imperfect. Although the nomograms presented in the paper by McCormack and colleagues are a useful first step in estimating risk, we must now focus our attention on developing better estimates of the long-term benefits of preventive therapies.

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[The authors respond:]

We agree with Dr. Strasberg that computers have the potential to be useful clinical tools. Although many physicians have computers in their offices for billing and scheduling purposes, not many computers are available at the point of care. Our method of estimating a patient's risk is simple enough that a computer is not required. Furthermore, a computer program that provides "black box" estimates of risk

and benefit may not allow an appreciation of the basis for the estimates or an understanding of the concepts of relative and absolute risk reduction.

That said, we have become aware of a Web site (**www.hbroussais.fr/scientific/fram.html**) where the clinician can enter a patient's risk factors and obtain an estimate of the absolute risk for heart disease and stroke based on the Framingham data. The site offers no discussion of the limitations and assumptions of this process, and the clinician and patient are therefore unable to appreciate the impact and interplay of the different risk factors or the benefits of treatment regimens.

Dr. McAlister points to longitudinal data suggesting that the benefit of hypertension treatment (reduction of death from cardiovascular disease) extends beyond the 3- to 6-year period typical of randomized controlled trials.[1] However, these data are derived from uncontrolled, nonrandomized observational studies and address only the issue of death, not morbidity from cardiovascular disease. The magnitude of that long-term benefit remains uncertain and may never be evaluated in a randomized controlled trial.

McAlister also notes a number of reasons why the design of most 3- to 6-year randomized controlled trials has led to a systematic underestimation of the benefit.

The recruitment of low-risk patients is not an issue because these individuals represent a large proportion of patients who receive therapy for hypertension and elevated cholesterol. This factor is taken into account by our nomograms, which help the clinician to estimate the baseline risk.

The argument that many trials lack the statistical power to show a difference is not an issue because the trials and meta-analyses we included all had statistically significant results.

Loss of patients to follow-up would be an issue only if the loss were greater in the placebo group than in the treatment group. We are unaware of any studies affected by this phenomenon.

The suggestion that the benefit was achieved despite only modest reductions (5 mm Hg diastolic and 10 mm Hg systolic) in blood pressure may not be entirely correct. Although the difference between the drug and placebo groups was 10/5 mm Hg, the actual reduction in the drug group (from baseline) was greater, approximately 20/10 mm Hg; this magnitude of change may be important, particularly if one believes that the reduction in the placebo group was due primarily to nonspecific effects rather than to the placebo effect.[2]

The overall effect of switching some patients in the placebo group to active treatment at some point during a trial is difficult to estimate, because it would depend on when and to what degree it occurred. We estimate that if 20% of the placebo group received active treatment for half of the trial, there would be a 3% to 4% decrease in the overall reduction of relative risk. This decrease would have only a small impact on the estimates of absolute reduction and number needed to treat.

Although long-term information would be useful, a patient's decision to adhere to treatment for 5 years is, in our belief, an important commitment. We therefore continue to feel that treatment decisions based on a 5-year risk estimate and the relative risk reduction from randomized controlled clinical trials is the most reasonable approach when deciding on drug therapy.

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Correction

In the article "Costs and benefits of routine follow-up after curative treatment for endometrial cancer" (*Can Med Assoc J* 1997;157[7]:879-86) the authors' affiliation information was given incorrectly. The affiliations should have read as follows: Drs. Olu Agboola, Eva Grunfeld and Gad Perry are with the Ottawa Regional Cancer Centre, Faculty of Medicine, University of Ottawa, Ottawa, Ont. Dr. Grunfeld and Mr. Douglas Coyle are with the Clinical Epidemiology Unit of the Loeb Medical Research Institute, Ottawa Civic Hospital, Ottawa, Ont. We apologize for the errors. — Ed.