



## Choosing a first-line antihypertensive

In their systematic review of antihypertensive therapies, James M. Wright and colleagues conclude that “low-dose thiazide therapy can be prescribed as the first-line treatment of hypertension with confidence that the risk of death, coronary artery disease and stroke will be reduced. The same cannot be said for high-dose thiazide therapy,  $\beta$ -blockers, calcium-channel blockers or ACE [angiotensin-converting-enzyme] inhibitors.”<sup>1</sup> Although there may be good reasons for selecting thiazide therapy, such as low cost and low rate of withdrawal for adverse effects, the efficacy data in Table 4 do not support the authors’ conclusions that only low-dose thiazide therapy will prevent death and cardiovascular morbidity in patients with hypertension.

Table 4 shows that there was essentially no difference among low-dose thiazide, high-dose thiazide, and calcium-channel blocker therapy with respect to mortality (relative risks 0.89, 0.90 and 0.86 respectively) or total cardiovascular events (relative risks 0.68, 0.72 and 0.71 respectively). For total cardiovascular events, a Mantel-Haenszel analysis<sup>2</sup> finds no evidence of heterogeneity between these medications ( $\chi^2 = 3.6$  on 2 degrees of freedom,  $p = 0.16$ ). There was lower risk reduction for  $\beta$ -blockers than for the other medications, but there was no significant difference between the  $\beta$ -blockers and low-dose thiazide therapy for mortality (relative risk 1.01 and 0.89 respectively). For the  $\beta$ -blockers, the risk reduction for total cardiovascular events just failed to reach significance at the 5% level (relative risk 0.89, 95% confidence interval 0.78–1.02). There were no trials of ACE inhibitors against placebo, but the one trial comparing ACE inhibitors with calcium-channel blockers (Table 2) suggested that the ACE inhibitor was at least as good as the calcium-channel blockers in reducing mortality and cardiovascular events.

I conclude that the data presented by Wright and colleagues show that low- and high-dose thiazide therapy, calcium-channel blockers and ACE inhibitors are similarly efficacious in reducing mortality and cardiovascular events in patients with hypertension.

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### References

1. Wright JM, Lee C-H, Chambers GK. Systematic review of antihypertensive therapies: Does the evidence assist in choosing a first-line drug? *CMAJ* 1999;161(1):25-32.
2. Rothman KJ, Greenland S. *Modern epidemiology*. 2nd ed. Baltimore: Lippincott Williams & Wilkins; 1998.

### [The authors respond:]

We appreciate Murray Finkelstein’s comments about our systematic review; however, we disagree with his conclusion. We carefully chose the wording of our 2 concluding statements. Our first statement, that “low-dose thiazide therapy can be prescribed as the first-line treatment of hypertension with confidence that the risk of death, coronary artery disease and stroke will be reduced,” is substantiated by the statistical significance (95% confidence intervals) of the reduction of total mortality, coronary artery disease and stroke with low-dose thiazides, as presented in Table 4. Our second statement was that “the same cannot be said for high-dose thiazide therapy,  $\beta$ -blockers, calcium-channel blockers or ACE inhibitors.” A statistically significant reduction in all 3 measures has not been shown for high-dose thiazides,  $\beta$ -blockers or calcium-channel blockers (in Table 4 the confidence intervals include 1.00). Nor has it been shown for ACE inhibitors or any other class of drugs, as they have not been studied in trials meeting the criteria of this review.

We therefore cannot prescribe these other classes as first-line agents with confidence that they will reduce each of these 3 adverse outcomes. We did not conclude, as suggested by Finkelstein,

that only low-dose thiazides will prevent death and cardiovascular morbidity. Nor did we conclude, as Finkelstein has, anything about the relative effectiveness of low-dose thiazides and the other classes of drugs; the available head-to-head evidence is insufficient to comment on the relative effectiveness of the different classes of antihypertensive drugs.

We did demonstrate in this review that using thiazides as first-line therapy was associated with a greater reduction in systolic blood pressure and a lower rate of withdrawal for adverse drug effects than that associated with some of the other classes of antihypertensive drugs. We did not comment on the cost advantage of thiazides but are pleased that Finkelstein has made this point.

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## White-coat hypertension

In his recent *CMAJ* editorial on white-coat hypertension,<sup>1</sup> David Spence reviews the question of 24-hour ambulatory monitoring of blood pressure, which often demonstrates a lower blood pressure reading than that done in a medical centre. I agree with this phenomenon.

The patients I refer to a cardiologist for ambulatory monitoring are those whose blood pressure is uncontrolled by combinations of antihypertensive drugs. The cardiologist often measures a normal ambulatory reading, leaving me looking like a fool.

When these patients return to me, do I proceed to ignore the readings over 150/100 mm Hg in my office because their ambulatory numbers were normal? No, I treat on the basis of the higher readings I see in my office. If I am charged with overtreatment, Spence will back me up, as he correctly states



that our current success in preventing heart attacks and strokes comes from treating on the basis of office blood pressure readings.

There are now blood pressure testing machines in many drugstores, and many patients also take their own readings at home with equipment we recommend. In virtually every case, the systolic numbers are at least 20 mm Hg lower than what I find at the office.

I advise patients that the office readings, taken in a more stressful situation than most ambulatory settings, demand attention. This may be contrary to current teaching that physicians should base treatment on the lower levels of blood pressure, but life is a compromise. So we may split the difference, shaving a few mm Hg from the top readings. This leaves everybody happy.

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#### Reference

1. Spence JD. Withholding treatment in white-coat hypertension: wishful thinking. *CMAJ* 1999;161(3):275-6.

## Foreign graduates deserve better

The acute shortage of physicians we face is due not only to government shortsightedness but also to parochialism within the medical profession. For instance, specialists with many years' experience in another province and possessing Royal College certification are not, as a rule, considered fit to practise in Ontario. Are Ontarians more delicate than others or are we more equal than others?

Forty years ago the Malayan Medical Association used excuses such as maintenance of the standard of care to keep people out of practice. My medical degree from Taiwan was a useless piece of paper back in 1959. I was forced to teach high school at a time when the physician-population ratio in my own country was 1 in 10 000. My hometown and its 30 000 people did not have a single practising doctor, yet my wife and I

had no opportunity to return to practice because of our foreign qualifications — a plight faced by many foreign medical graduates in Ontario today.

The concern that foreign-trained physicians are inferior is prevalent. To limit foreign medical graduates to a minuscule number of training positions is in itself a form of institutionalized discrimination.

I would like to present a cohort's experience to illustrate a point. Seventy-two students entered the premed class at the National Taiwan University in 1952, and eventually 26 of them completed postgraduate training in North America. One graduate became professor and chief of the Department of Microbiology at Uniformed Services University of Health Sciences in Washington, DC, while another is the chair of cardiovascular surgery at McGill University. Two others became professors at George Washington University in Washington, and another is a professor at the University of Missouri. Only 6 of us ended up doing general practice in North America — everyone else is a board-certified specialist.

If we had arrived in Canada today, we would be consigned to a life of servitude washing bottles and dissecting rats in laboratories. I think the experience of this cohort is proof that foreign medical graduates deserve a fair chance to serve the public.

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## Ticklish distinctions

The *CMAJ* case report of concurrent babesiosis and Lyme disease in Ontario by Claudia C. dos Santos and Kevin C. Kain<sup>1</sup> contains some incorrect information.

First, the authors state that "205 cases of Lyme disease were reported in this country between 1984 and 1994." In fact, the "205 cases" were in Ontario, not all of Canada.<sup>2</sup>

Second, they state that *Ixodes scapu-*

*laris* and *I. pacificus* ticks "have been identified in about 250 locations in Canada." The "250 locations" refer to the distribution of the blacklegged tick, *I. scapularis*, not *I. pacificus*. In the original reference<sup>2</sup>, there is no mention of the western blacklegged tick, *I. pacificus*. This tick has only been documented in British Columbia.

Third, Long Point peninsula is not in Point Pelee National Park. These 2 locations are approximately 200 km apart — a 3-hour car drive.

Finally, the common name of *I. scapularis* is blacklegged tick, not deer tick.<sup>3</sup>

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#### References

1. dos Santos CC, Kain KC. Two tick-borne diseases in one: a case report of concurrent babesiosis and Lyme disease in Ontario. *CMAJ* 1999;160(13):1851-3.
2. Banerjee SN, Christensen CI, Scott JD. Isolation of *Borrelia burgdorferi* on mainland Ontario. *Can Commun Dis Rep* 1995;21:85-6.
3. Keirans JE, Hutcheson HJ, Durden LA, Klompen JSH. *Ixodes (Ixodes) scapularis* (Acari: Ixodidae): redescription of all active stages, distribution, hosts, geographical variation, and medical and veterinary importance. *J Med Entomol* 1996;33:297-318.

#### [The authors respond:]

We thank John Scott for identifying a typographical error in this report: the sentence that mentions Long Point peninsula should have read "... Long Point peninsula and [not in] Point Pelee National Park."

We did not claim that there were exactly, or only, 250 locations where either *I. scapularis* or *I. pacificus* has been found, nor is this relevant. The point is simply that although ticks capable of transmitting *Borrelia burgdorferi* or *Babesia* spp. or both have been found in a number of locations in the country, they are only established in a few.

Scott is incorrect that blacklegged tick is the only commonly used name for *I. scapularis*. The term deer tick remains in common use in the medical literature and lay press (in fact, the Centers for Disease Control refer to them as deer ticks in their public infor-