reductase inhibitors. Partly to make a teaching point regarding balanced inhibition and partly because of clinical interest in the drug, I tried to moderate these concerns with atorvastatin. In this case, there is a 1:1 relation between the active parent drug and the active hydroxy metabolites; the metabolites are as potent as or slightly more potent than atorvastatin itself; the differences in lipophilicity are much less than those with simvastatin; and the changes in concentration (a 3.3-fold increase in atorvastatin acid, a 1.6-fold increase in bioactivity) are within the usual dosing ranges for the drug.[5] One could not make the same concessions for simvastatin or lovastatin.

The issue regarding the interaction of calcium-channel blockers and statins has been addressed by others.[6] There is a major interaction (3.5–6.2 fold elevations in statin concentration) between diltiazem or verapamil and lovastatin or simvastatin.[7,8] The change in drug levels is about the same order of magnitude as the interaction of these drugs with erythromycin. Prégent would like us to believe that a recently published meta-analysis[9] adequately addresses concerns regarding concomitant use of these drugs with the statins and that their interactions are without clinical significance. However, these data came from studies designed to assess clinical efficacy and not adverse events, least of which would be drug interactions. There were no controls of the number or types of potential inhibitors used by patients (they reported aggregate data for calcium-channel blockers) and the numbers of events were far below those that would be required to show a difference, if any existed. In other words, the data are poor and are vastly underpowered to answer the question.

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[The editorialists respond:]

We thank Ernest Prégent for his comments about our editorial.[1] He is correct to point out that not all reports of significant rates of myalgia in patients receiving combination therapy with a statin and certain other agents have involved simvastatin or lovastatin. However, the reports with these particular HMG–CoA reductase inhibitors are often based mechanistically on their inhibition by a CYP3A4 inhibitor. The concept of differential susceptibility of the statins in terms of CYP3A4 inhibition still holds true.

The myopathy reported in patients receiving combination therapy with pravastatin and cyclosporine clearly is not based on inhibition of CYP3A4 metabolism. We all continue to learn as these drugs are used, and therefore interactions are often not recognized until years after clinical trials are completed. Adverse reports of large trials such as those discussed by Gruer and colleagues[2] are reassuring. However, the data do have limitations. This study was conducted when our understanding of cytochrome-mediated drug metabolism was in the early stages. Therefore, drug interactions may have been underrecognized. While the mechanism of cyclosporine–pravastatin interactions is not known, it could relate to interference with transport mediated by P-glycoprotein.[3]

We know that a few drugs, such as niacin, fibrates and cyclosporine, increase the likelihood of myopathy with *some*, not *all* statins as Prégent states. In the end, we all agree that the potential for myopathy increases when the most potent CYP3A4 inhibitors are given with statins metabolized by CYP3A4.

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Degrees of difficulty in ascertaining credentials

I am disgruntled to see the names of *CMAJ* authors published without the authors' degrees. I have always rapidly screened credentials to decide if, when, and in how detailed a fashion I would peruse an article. I know I can get used to this jarring change in the *CMAJ* but I disapprove of it.

If the purpose of the omission is to take the focus off the author and put it on the article, then the policy is having the reverse effect. I am now compelled first to turn to the end of the article to see who really is the author. Is it a clinical medical colleague? A basic scientist? A priest? A social worker? The head of an institute of alternative medicine? A freelance writer? (I'm not suggesting that these categories are mutually exclusive, nor that I wouldn't possibly be interested in articles by all such authors.)

Omitting degrees is a friendly, equi-

table gesture but it does not work for this reader.

John Stoffman
Pediatrician
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[Editor's note:]

As John Stoffman correctly discerned, *CMAJ* wishes to emphasize the content of what we publish and not the qualifications of the authors. The variety of letters after people's names has been growing; while some of these may be as familiar as the MD degree, in other cases it was becoming difficult to determine whether they were in fact academic degrees and what they meant. Most degrees do not describe the subject matter of the degree, only the degree level (undergraduate, masters and doctoral). Our preference is to describe the current position or occupation of the author, not their level of qualification.

Medicare

The information on health expenditures in the Feb. 8, 2000, editorial[1] is dated, although this fact does not necessarily alter the main points made. The national health expenditure database, which is the basis of the Health Canada publication on national health expenditures to which you referred,[2] was transferred from Health Canada to the Canadian Institute for Health Information (CIHI). CIHI has produced 3 annual health expenditure reports since the Health Canada document was released in early 1997. The latest CIHI report was released on Dec. 16, 1999.[3]

According to the latest estimates, Quebec, not Alberta, has the lowest health expenditure per capita among the provinces: $2453 per person in 1999. Alberta has the 5th lowest expenditure, at $2832 per person. Nevertheless, Albertans continued to spend a lower proportion of their provincial gross domestic product on health in 1999 (7.6%) than citizens of any other jurisdiction in Canada.

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The statement in your editorial in the Feb. 8, 2000, issue that "private health care is always more expensive"[1] mixes opinion with fact. The reference you gave[2] provides no statistically valid data to support such a statement. In British Columbia, private surgical clinics offered to provide contract surgical services for medicare patients at 60% of the government-calculated cost in public hospitals. We did not need a study, or a health policy analyst, or a health economist or any other redundant bureaucrat to back up our calculation that we could achieve this and still make a (nasty word) profit. Why *CMAJ*'s editors continue to blindly trust the ability of a state-controlled monopoly to deliver efficient, effective and excellent health care services is mind-boggling to many of us.

The bottom line is very simple. There is nothing morally wrong with spending one's own money on the health of oneself or a loved one. The hogwash being spouted by self-serving lobbyists and unions is being matched by the editors of *CMAJ*.

Brian Day
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2. Rough seas in US managed care [editor's preface]. *CMAJ* 1999;161(6):669.

[The editor-in-chief responds:]

Perhaps Brian Day, in criticizing our editorial,[1] should reread the referenced paper.[2] Although that piece focused on the debacle of for-profit managed care in the US we could also draw Day's attention to studies specific to the question of hospital ownership. These show, for example, that US medicare spending in 1995 in for-profit markets resulted in $5.9 billion in excess costs when compared with spending in not-for-profit markets.[3] Privately financed care costs considerably more than equivalent publicly financed care.

John Hoey

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Letters may be submitted by mail, courier, email or fax. They must be signed by all authors and limited to 300 words in length. Letters that refer to articles must be received within 2 months of the publication of the article. *CMAJ* corresponds only with the authors of accepted letters. Letters are subject to editing and abridgement.

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