

data to project future needs ... implies that the way we do things now is optimal." Wrong. There is no such implication in our analysis. In our calculations, we simply keep fixed the most recent utilization rates (whatever they are) and allow only population to change.

Charles Low feels that apparent shortages of both family doctors and specialists make our future projections "difficult to evaluate." Again, what we were projecting was not changes in requirements for physicians from all causes, but changes resulting *only* from population aging and growth. We make no judgement about what utilization rates *should* be but take them as they are.

Michael Borrie and associates assert that we underestimated the "provision and need for services for elderly patients" because we failed to give explicit treatment to geriatricians. We dealt with an exhaustive set of 19 categories of physicians, the maximum for which age-sex rates of utilization are available. The underlying patient utilization data were compiled from OHIP records (the only source), as provided by the Canadian Institute for Health Information (CIHI). Geriatrics is included in the CIHI category "internal medicine" (along with 10 other specialties). Given what Borrie and associates recognize as the low numbers of physicians who have been trained in geriatrics, it should be clear that any separate treatment would have had only a negligible effect on the overall projection results.

Through our analysis we found that demographic effects on overall physician requirements are likely to be smaller than might have been supposed in light of popular discussion of the "aging crisis." A helpful response to that finding would be something like the following: Good — and now that we have that out of the way, let's focus on other factors that are likely to be more important, including those mentioned by the letter writers. Population aging cannot be ignored, but it should not be at the top of the list of things to worry about in physician hu-

man resource planning at the aggregate level.

Frank T. Denton

Department of Economics

Amiram Gafni

Department of Clinical Epidemiology
and Biostatistics

Byron G. Spencer

Department of Economics

McMaster University

Hamilton, Ont.

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The whiplash debate

In a review published in *The Left Atrium*, Walter Rosser¹ lauds as a "remarkable book" Andrew Malleison's *Whiplash and Other Useful Illnesses*.² Rosser writes that Malleison "challenges many different groups ... for their self-interest and their failure to critically assess the medical case for whiplash." Yet in Malleison's book, no study that found evidence of a valid whiplash syndrome is accurately presented, whereas those against are highly praised.

As just one example, Malleison promotes a Norwegian-Lithuanian paper that claimed that 202 drivers involved in rear-end collisions resembled control subjects at the end of 2 years.³ He writes, "Schrader and his Norwegian colleagues ... had cut too close to the quick. Like frightful Vikings from the past, they had threatened to wreak havoc with the profitable whiplash industry." This paper was evaluated by the Norwegian Centre for Health Technology Assessment, a group established by the Department of Health and Social Affairs for Norway and operating as a unit within SINTEF Unimed, a non-profit independent research organization. The expert group who wrote the Centre's report⁴ concluded that more than 4000 individuals in each group would be needed to discover with 80%

probability a statistically significant difference in the occurrence of chronic neck complaints between subjects who had and had not been involved in a collision, and the Schrader study was denied validity.

Harold Merskey

Professor Emeritus of Psychiatry

University of Western Ontario

London, Ont.

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[The author responds:]

There are no studies that confirm a "valid whiplash syndrome." Perhaps what annoys Harold Merskey about my book¹ is not that I presented inaccurately the studies that do exist but that I presented them too accurately, leaving the studies, their authors and their advocates bereft of scientific credibility. Merskey cites a report of the reputable Norwegian Centre for Health Technology Assessment to condemn my book. Ironically, the authors of that report,² after commenting on the lack of science in the whiplash literature, confirm the very thesis of my book with the following conclusions:

- "There is no documented evidence supporting a causal relationship between type or grade of injury and specific symptoms or symptom constellations.
- "Evidence-based documentation has not been found to support the contention that chronic complaints following a whiplash injury mechanism are specific or are directly related to the actual injury mechanism."