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Key words: An important key?

Relying on key words appended to an article without reading at least the abstract, as Dr. Bruce Squires (*Can Med Assoc J* 1989; 141: 183) says some readers do, can be dangerous. I once wrote an article on the problems of the little man in a big society; one of the key words was "impotence". I got a lot of reprint requests from urologists!

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Electrical safety in patient care areas

Proposed revisions to the Canadian Standards Association (CSA) standard Z32.2, which deals with electrical safety in patient care areas, will soon be published.

This voluntary standard heretofore applied to hospitals. It is proposed that the standard be extended to apply to private physicians' offices and clinics as well. Under the revisions the CSA recommends differentiation of patient-care and non-patient-care areas, establishment of electrical maintenance programs and establishment of educational programs for personnel who operate electromedical equipment.

Further information about the standard and its implications for office practice will be published when it is made available by the CSA.

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on Health Care Technology

Ranking the MD degree

Writing about hospital management (*Can Med Assoc J* 1989; 140: 1203, 1205), Dr. Peter Richardson uttered some personal and highly subjective opinions about the relative scholastic ranking of the MD degree. Indeed, to emphasize the importance he attached to the opinion, he set the text off from the main article (in collaboration with the editor, I presume) in a coloured box.

I do not believe that his opinion warrants formal argument as to its correctness. What I do question is the usefulness of recording, in the journal that ostensibly represents Canadian physicians, a position of this sort. It is not subject to any rigorous proof and seems to be at least obsequious, if not deliberately demeaning in intent. I can imagine no useful purpose for it other than adding to a general chorus bent on publicly reducing the physician's stature.

I hope I speak for physicians at large (I certainly speak for the sample of physicians I've polled in my community) in asserting that we have never felt any necessity to hold up, for odious comparison, that body of learning we have mastered or to test its worth against that of other disciplines, professional or otherwise.

Without being smug we should feel perfectly content with our traditional and contemporary place in the academic firmament. Our concern should always be, as I believe it is, with consistently updating and improving our own curriculum and polishing the techniques used in teaching it and the methods of evaluating our students.

I believe we should leave the assessment of our academic ranking to master's students in, for instance, faculties of education and the publication of their results to pedagogic journals.

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[Dr. Richardson responds:]

I am delighted that Dr. Warner read my article. His response well illustrates a minor point of my contribution, which was that some colleagues have a rather strong opinion of their overall professional superiority. My view remains that our academic qualifying degrees are essentially at a bachelor's level and relate to our own discipline of human biology, pathology, and the diagnosis and treatment of human ill health and that these initial degrees do not automatically qualify us to be experts in all aspects of health care delivery and its place in society. Sweet reasonableness and appropriate humility about this still seem well warranted to me.

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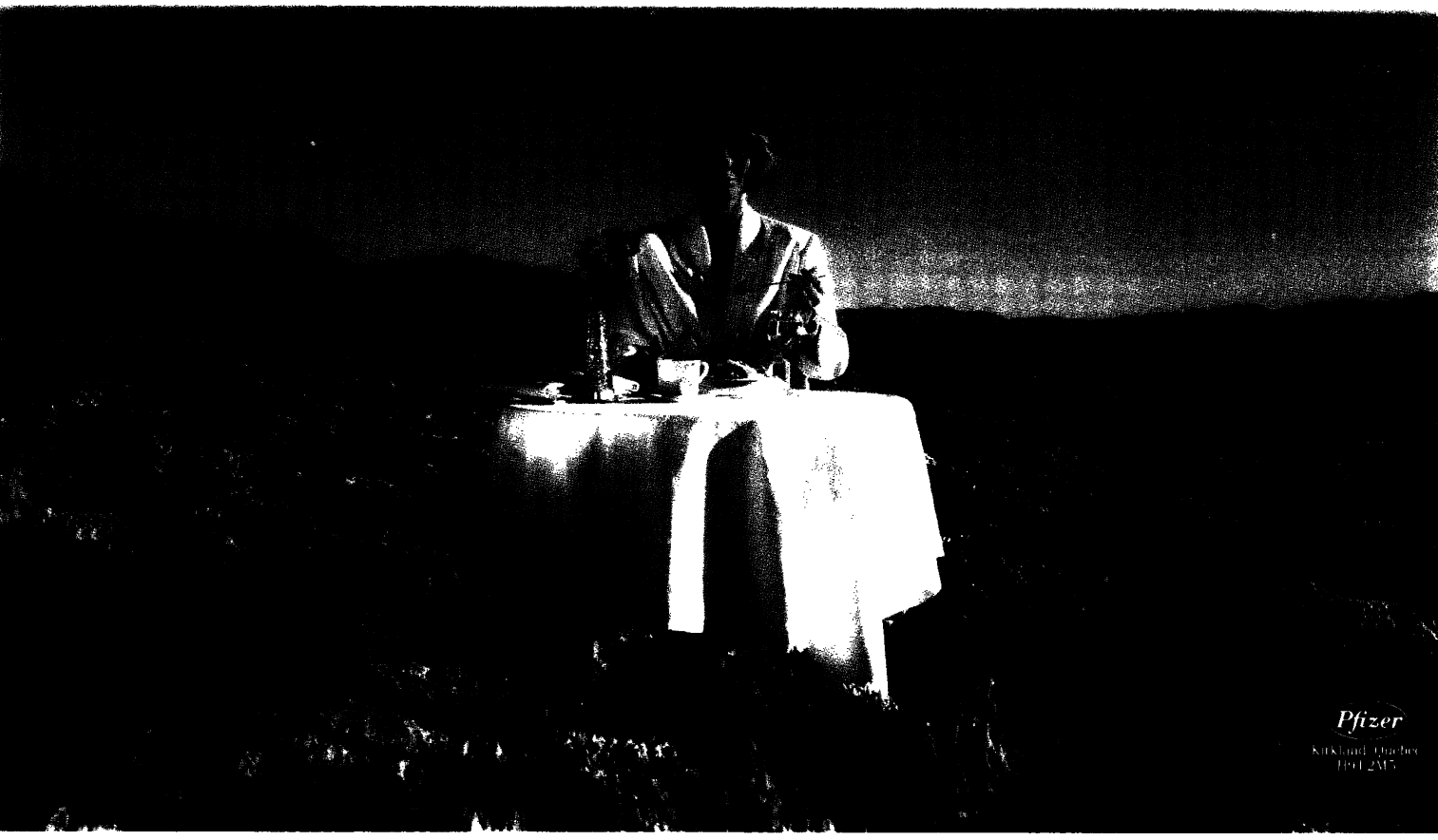
Good sense in medical science

Dr. Judith A. Leech is to be congratulated on her editorial "Cancer cluster investigation: toward a more rational approach" (*Can Med Assoc J* 1989; 141: 105–106). She makes an admirable advocate for good sense in medical science.

Several points would further



fortify her argument. Both she and Dr. Murray M. Finkelstein, author of "Mortality rates among employees potentially exposed to chrysotile asbestos at two automotive parts factories" (*ibid:* 125–130), mention two studies whose results are stated to be conflicting; however, a simple explanation for the apparent conflict is available. Berry and Newhouse,[1] in their study of workers in the Ferodo Company, found no increased risk of lung cancer. In contrast, McDonald and colleagues,[2] in their study of Connecticut workers, found a slightly increased standardized mortality ratio (SMR) for lung cancer. Moreover, in the latter study the SMR for respiratory cancer was found to be 167 for the employees who had worked for less than a year but 137 for those who had worked for over a year, which might suggest that the shorter the exposure the greater the risk!

Before attributing a small but statistically significant increase in the SMR for lung cancer to asbestos exposure it is essential to know the smoking habits of the cohort under investigation and how they compare with the habits of the reference group from which the SMR was derived. This problem has been recognized for some time by the Registrar General of Britain, whose *Decennial Supplement on Occupational Mortality* lists mortality not only by social class but also by smoking habits in the various occupations. Other publications describe data on smoking habits according to social class[3] and the risk of lung cancer with various levels of smoking.[4]

From a consideration of these data it can be shown that manual labourers (blue-collar workers in North America) have a lung cancer risk 20% higher than administrative and clerical workers. Moreover, when it is borne in mind that around 40% of all deaths from cancer in men are due to lung cancer, the difference in smoking habits between the two groups mentioned above increases the total number of cancer deaths in male blue-collar workers by about 10%. This factor alone would easily explain the differences between the two cited studies.

Finkelstein acknowledges that there were no smoking histories available and gratuitously adds that limited telephone enquiries were made into the smoking histories of some of the deceased workers. This is thoroughly inadequate. Retrospective smoking histories are notoriously unreliable, as any shoe-leather epidemiologist knows, and this is especially true in matters related to compensation. A study by Berry, Newhouse and Antonis[5] of asbestos workers compared smoking histories obtained during life with those obtained after death. No less than 33% of those categorized as never having smoked showed a completed about-face.

There is a regrettable tendency at present to attribute all disease to environmental or occupational exposures. It is always much easier to tell others to clean up their act than to modify one's own habits. If improvements in life expectancy are to be made they have to come about from changing behaviour, not chasing will-o'-the-wisp whims.

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[Dr. Finkelstein replies:]

Readers who are familiar with the literature of occupational medicine will know that not only has Dr. Morgan coauthored a fine textbook on occupational lung diseases but also he is a habitual writer of acid-penned letters to the editor. Regrettably, his letters are sometimes ill-conceived owing to a lack of understanding of the original publications. For example, Dr. Michael Jacobsen, of the Institute of Occupational Medicine in Edinburgh, has recently responded to one of these letters by writing that "Dr. Morgan has misread, misunderstood, and misrepresented our account of what we did, what we found, and what we concluded."[1] These remarks are appropriate in the present instance as well.

I wrote that "fragmentary data about smoking habits were collected in a 1981 telephone survey in which responses were obtained from 426 (26%) of the study subjects". Morgan, however, sets up his paper tiger by writing that "Finkelstein acknowledges that there were no smoking histories available and gratuitously adds that limited telephone enquiries were made into the smoking histories of some of the deceased workers", then he proceeds to vanquish it: "This is thoroughly inadequate. Retrospective smoking histories are notoriously unreliable, as any shoe-leather epidemiologist knows". In fact, the survey was of living workers. Since Tables II and III indicate a total of 124 deaths in the study population, it is hard to imagine how Morgan could conclude that the responses from 426 individuals came from a survey of next of kin of some of the deceased workers. A survey