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Accuracy of clinical diagnosis

To the editor: During the past decade the significance of autopsies has been downgraded by clinicians and pathologists alike, presumably because of the sophistry of today's investigative technology. Dr. William M. Thurlbeck's article "Accuracy of clinical diagnosis in a Canadian teaching hospital" (*Can Med Assoc J* 1981; 125: 443–447) shows the results of this presumption.

Indeed, if one rearranges Dr. Thurlbeck's data, almost one quarter of the clinical diagnoses were proved to be wrong by autopsy. Similarly, clinical opinions concerning the causes of death were incorrect in 36% of cases. Our own data from two community hospitals, collected for local purposes, are virtually identical.

However, neither set of data can be compared with the rate of incorrect diagnoses reported by Britton for Swedish clinicians (30%). This is because of a marked selection bias of which Dr. Thurlbeck is aware. The autopsy rate Britton reported was 96%; the rate in North America was 20% or less. Thus, it seems that autopsies here are performed mainly in the interesting cases of interested clinical colleagues. What would be our "batting average" in the 76% of deaths after which an autopsy was not done?

I am confused about the actual percentage of cases in which the outcome might have been better had the correct clinical diagnosis been made. In Dr. Thurlbeck's abstract and discussion the figure mentioned is 10%. In the abstract, the results and Table I he speaks of three cases. This would amount to 1.5% of the whole series and to 12% if only the group with "major disagreement" is taken into consideration. Table IV gives an additional group of 19 cases (when counted as listed). If this is added to the aforementioned three, the figure then is 11% of the series.

A closer look at Table V shows that the autopsies revealed 44 cases of lethal pneumonias, 27 or 61% of which were missed clinically. I agree that the patients with pancreatitis, bowel infarction and subdural hematomas could have been better off with a proper antemortem diagnosis. But what about those 27 with pneumonias? Could it be that all of them were beyond our therapeutic ability? Or could it be that our stethoscopes had been plugged by the woolly idea that a chest roentgenogram is better than auscultation and percussion?

As for cost efficiency and effect on subsequent patient care, the autopsy is neither better nor worse than clinicopathological conferences, workshops, seminars and other types of continuing medical education. One thing is certain, however. The autopsy rate in North America has fallen greatly. Another decade of the same would make it necessary to import medical knowledge as well as cars and textiles.

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[We showed Dr. Karnauchow's letter to Dr. Thurlbeck, whose reply follows.
—Ed.]

To the editor: Dr. Karnauchow is correct in his assessment of errors. In three patients the underlying disease was misdiagnosed in such a way that I considered a better outcome possible; there were 19 instances in which the cause of death was misdiagnosed and it appeared to me that a better outcome could have occurred if the diagnosis had been correct (Table IV). Thus, there are a total of 22 cases, or 11%. A problem arose with confusion between serious pulmonary infection and embolism; in my judgement this made little difference to the outcome of the patient's illness in a number of instances.

It would indeed be interesting to know the correctness of diagnosis in the cases in which an autopsy was not done. Britton's report indicated that there were substantial errors, even in cases in which the diagnosis was thought to be clinically certain.

I believe that autopsies are important and that autopsy rates should be high. The point I tried to make in my article was that if autopsies are done as a mechanism for quality assurance, then it is essential to show that what is learned in the autopsy room results in a modification of patient care and, subsequently, a better outcome for patients.

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Good English is good medicine

To the editor: As a confessed language watcher I enjoyed the Personal View by Mr. David Woods "Good English is good medicine" (*Can Med Assoc J* 1981; 125: 624–629). But after reading the article I wondered if the author ever considered his own prose as a standard to use in promoting his argument. Although I certainly agree that much medical communication is marred by jargon, what about the jargon in journalism and publishing? Does Mr. Woods expect physicians to grasp easily the meaning of this excerpt? "but in an egalitarian society *'foreign'* has racist overtones, so this estimable work will be remaindered while scalps all over the country are searched for elusive pediculosis". Since I used to have a patient in the book-selling business I happen to understand what he means. But 5 years ago I would have thought that to be remaindered might be a worse fate than to be Timbrelled.

While lamenting the misuse of the word "hopefully" in the sense "it is to be hoped that", Mr. Woods offers us "arguably, there's no place for fun in therapeutics". I say there is no place for "arguably" in the sense "it can be argued that". I wonder if Mr. Woods knows why those who are interested in English expression, while deploring the incorrect use of "hopefully", apparently overlook similar constructions. To illustrate:

- "Interestingly, the Ontario Party leader Michael Cassidy" (*Can Med Assoc J* 1981; 125: 372).
- " 'The closest thing I can be found guilty of is manslaughter' Abbot reportedly said" (*Newsweek*, Oct. 5, 1981: 31).
- "Curiously, researchers read different messages in the frozen bubbles" (*Newsweek*, Oct. 5, 1981: 74).

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To the editor: The call by Mr. David Woods for clarity in writing should be heeded. A parallel call for brevity, to which he himself might pay attention, would also not be out of place.

Twice in the course of perusal of the article I was discomfited. First, I was told that "prestigious" was nonexistent. Funk and Wagnall's "New Standard Dictionary" (1950 edition), which purports to deal with the English language, tells us that "prestigious" means "of or pertaining to sleight of hand; hence deceptive" being derived from the Latin "praestigium" meaning delusion. Admittedly this is hardly the use that is commonly made of the word, and many of those to whom the adjective is applied would feel offended if they consulted Funk and Wagnall's — but the word does exist.