the positive association between pentoxifylline treatment and nausea ($p < 0.001$, $\chi^2 = 20.57$, my calculation). Finally, the raw data were transformed once for the variance analysis (log of treatment walking distance divided by control walking distance) and a second time in preparing the final figures (geometric mean of treatment walking distance divided by control walking distance, -1×100). By choosing such a data transformation for their figures, the authors failed to reveal that, at week 16, the patients in the control group fared better than those in the pentoxifylline group, as could easily be calculated from the table presenting the initial claudication distance (Fig. 1).

These results do not inspire much confidence in their validity. If the other 11 trials summarized by Hood and associates have not been scrutinized for major biases, what is the use of compiling and presenting their results?

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Reference

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[One of the authors responds:]

Biron raises 2 important points concerning the lack of descriptive data reported in 1 of the clinical trials included in our meta-analysis and the less-than-optimal quality of reporting of clinical trials in general. As we indicated in the Discussion of our meta-analysis, "although we attempted to minimize bias, one limitation that persisted was the extraction of data from published trials. There seemed to be little consistency in the type of data reported." In the case of the trial by Porter and associates, we corresponded with the principal author, who provided us with the appropriate data to allow us to make the necessary calculations. Had Biron been systematic in his review of all of the trials we included in the meta-analysis, he would have observed, as we pointed out, that "in more than 25% of the eligible trials standard descriptive statistics were not presented and insufficient data were provided to calculate them."

The quality of reporting of randomized controlled trials (RCTs) has been a concern for some time. Several guidelines that describe what needs to be included when reporting a trial have been published.[1,2] In addition, some journals[3] have published checklists of items for assessing RCTs, to be used by authors, reviewers and readers. Other journals[4] have published their policy on the statistical assessment of trials. In general, these efforts have not had their intended effect of improving the quality of reporting of clinical research. Perhaps one of the reasons for this disappointment is that these efforts were not evidence-based.

More recent evidence-based approaches appear to be having a positive effect. *CMAJ*,[5] along with approximately 30 other journals, have either adopted or are seriously considering adopting the consolidated standards of reporting trials — the CONSORT statement[6] — which includes a checklist and flow diagram. The checklist consists of 21 items that pertain mainly to the methods, results and discussion of an RCT report and identify key pieces of information necessary to evaluate its internal and external validity. The long-term effects of CONSORT still require evaluation.

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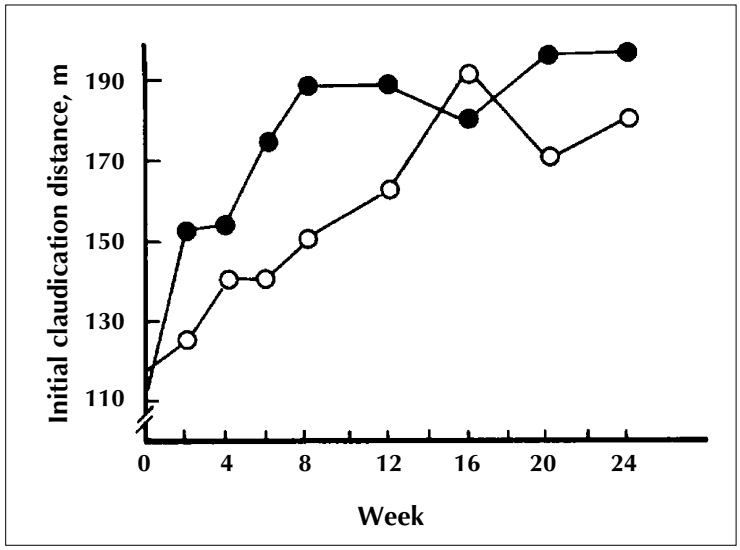
4. Gore SM, Jones G, Thompson SG. The *Lancet*'s statistical review process: areas for



Results of the trial by Porter and associates (taken from Table 3 of the article by Hood and associates), showing that patients taking a placebo (white circles, n = 40) were able to walk farther than those taking pentoxifylline (black circles, n = 42) at 16 weeks.

improvement by authors. *Lancet* 1993; 340:100-2.
5. Huston P, Hoey J. *CMAJ* endorses the CONSORT statement. *Can Med Assoc J* 199;155:1277-9.
6. Begg C, Cho M, Eastwood S, Horton R, Moher D, Olkin I, et al. Improving the quality of reporting of randomized controlled trials: the CONSORT statement. *JAMA* 1996;276;637-9.

Advance care planning

The article "Bioethics for clinicians: 6. Advance care planning" (*Can Med Assoc J* 1996;155:1689-92), by Dr. Peter A. Singer, Gerald Robertson and Dr. David J. Roy, sounds as if they were dealing with computers that can make yes/no, on/off digital decisions and not with frail, fallible, ambivalent humans. Surely we know enough about humans to remember that decision-making is an incredibly complex affair, especially in regard to future health care. People are ambivalent about everything almost all of the time, and especially about matters of life and death. People are susceptible to pressures in and around them, including subtle suggestions and innuendos that they do not even acknowledge to themselves, let alone voice to anybody else. Dying is one of life's most important occasions. Time is necessary to give blessings, make reconciliation and say good-byes. Most people have second thoughts about any decision they have made in the past, especially when confronted with the reality of what is just about to happen to them. Assessing mental capability is extremely difficult. Capability varies from one day to another; people may become lucid in episodes, after times when they are obviously incapable. Few people can make a completely rational decision. Those who have been abused expect to die young, and survivors often feel they deserve to die.

Because of these factors, ancient medicine included oaths that provided immutable guidelines, so that they were not susceptible to family squabbles, politically correct morality, economic pressures or even the whims of patient choice. (Although the ancients were without technology, they were wiser than we are.) Unless physicians, individually and collectively, adhere to an oath that commits them always to treat, how can they ever be trusted by anybody? It is not hard for patients to realize that their physicians are human and can be pressured by demands for beds, desires to inherit part of the patient's estate, selfish guardians, lazy trustees, powerful people who need a donor organ or the feeling that "this is taking too much time and effort when I could be golfing."