HEALTH QUALITY DATA

■ *To the Editor:* We read with interest the article, "Practice guidelines and physician scorecards: grading the graders," by Vogel and Topol,[1] and your accompanying editorial, "Health quality data: Are flawed data better than none?" (March/April 1996).[2] While we agree with your basic premise that the development of provider-specific "scorecards" is a complex process and is subject to potential bias, we take issue with several of the opinions that were expressed. In addition, we wish to clarify several inaccurate statements made with respect to the Cleveland Health Quality Choice Project.

First, most clinicians would agree that medicine is an imperfect science. The tests that we commonly use to diagnose disease are not accurate 100% of the time, the treatments we employ are not efficacious in all patients, and the advice we give patients with respect to prognosis is often inaccurate. Nevertheless, medicine has flourished, in spite of its dependence on such "flawed" data.

In judging the quality of scorecards, we feel it is important to use the same standards of evidence-based medicine[3] that apply for sound clinical practice. For example, evidence-based medicine would dictate that the methods used to risk-adjust outcomes across different providers be validated prior to implementation and that they be able to discriminate between patients experiencing bad and good outcomes. From this perspective, the risk-adjustment methods employed by Cleveland Health Quality Choice[4] exceed the discrimination of other well-established and widely used procedures[5–7] and prognostic systems.[8]

Second, while we agree that "gaming" scorecards is possible, we have found no evidence to suggest that hospitals participating in Cleveland Health Quality Choice are systematically altering data to enhance severity or are under-reporting patients who experience poor outcomes. Third, we have attempted to be as open as possible with respect to the "black box" of risk-adjustment. For example, variables and coefficients that are used to risk-adjust outcomes in medical and surgical patients are regularly released to hospitals for independent review. Fourth, as noted by Topol, evidence suggests that outcomes reporting may lead to improvements in quality over time. The 21% decline in mortality of patients undergoing coronary artery bypass surgery in New York State[9] following the reporting of provider-specific data is similar to declines in medical and surgical mortality in Cleveland hospitals since the implementation of Cleveland Health Quality Choice and similar to differences associated with therapeutic advances in many clinical trials.[10,11]

We also wish to clarify several inaccurate statements about Cleveland Health Quality Choice. The project is a collaboration of employers, physicians, and 27 hospitals in the Cleveland area (not 37 as referenced by Topol). In addition, employers in Cleveland have paid for the project. Since the project's implementation, $1.25 million has been directly contributed by business. Lastly, the use of nonspecific and pejorative descriptions of Cleveland Health Quality Choice are not supported by fact nor theory. The multivariable risk-adjustment models that are used to assess hospital performance are based on clinical variables that have both face and statistical validity as predictors of outcome. For example, the congestive heart failure model includes 25 factors, such as age, comorbidity, admission vital signs and neurological assessment, and results of admission laboratory testing (eg, arterial blood gas results)—factors that we suspect Topol might himself use in managing patients. The program continues to invite physicians to provide suggested enhancements to the models employed.

In sum, we agree that developing fair and valid health care scorecards entails several challenges and that current efforts are likely imperfect. Although methods used to compare providers will likely improve over time, we strongly believe that currently available methods do in fact provide useful information to both providers and purchasers. We look forward to continuing our collaboration with the Cleveland Clinic and other participating hospitals in this important area.

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■ *Reply:* We thank Morley and Harper for their thoughtful letter, although we take issue with some of the points they have raised. We apologize for citing the incorrect number of hospitals participating in the Cleveland Health Quality Choice program.

All physicians know that no diagnostic test is 100% accurate, but there clearly are degrees of inaccuracy beyond which a test becomes useless. As in the case of risk-adjustment technologies, there are well accepted measures of the accuracy of diagnostic tests, eg sensitivity, specificity, predictive value, and correlation coefficients. I am unaware of any useful laboratory test with a correlation coefficient (R^2) as low as 0.35 (equivalent to 35% "accuracy" in this context), as was the case with the Cleveland Health Quality Choice data. Such assays have gone the way of the thymol turbidity test and the basal metabolism test. They yield misleading data and have been appropriately discarded. The fact that the outcomes measures in the Cleveland Health Quality Choice project perform as well or better than those in similar projects elsewhere may only indicate that it is among the best in a group of methodologies that all perform at an unacceptable level.

It is reassuring that gaming has not been found in the Cleveland Health Quality Choice Project so far. However, we still believe that the potential for gaming exists, and that such gaming may not be easy to detect.

Morley and Harper state that Cleveland Health Quality Choice regularly releases risk-adjustment variables and coefficients to participating hospitals for independent review. While now true, this release of information was agreed to belatedly and reluctantly, and the hospitals are sworn to secrecy, thus retaining the "black box" characteristic for everyone but the hospitals.

Claims of great savings and improved outcomes resulting from the project cannot be substantiated, since similar savings occurred not only in Cleveland, but throughout the country. These savings began appearing before the Cleveland project started and have continued throughout its existence. Rather than these cost savings being the result of outcomes reporting, it is more likely the savings were the result of improving technology and increasing market pressure to contain costs.

Employers in Cleveland have paid only a small portion of the true costs of the project. Most of the project's true costs reside in additional personnel hired by hospitals to extract data retrospectively from clinical records. These expenses become part of the cost of doing business for hospitals and get passed to consumers as increased health care costs. Furthermore, Cleveland employers have recently withdrawn their financial support for the project.

Finally, we do not believe there is anything pejorative in the articles; the facts speak for themselves.

JOHN D. CLOUGH, MD
Editor-in-Chief

HANDGUNS

■ *To the Editor:* Please permit me to take violent exception to a statement that you made regarding the public health risk of the "ready availability of handguns" in your editorial "An ounce of prevention" (May/June 1996).[1] Please be advised that

handguns themselves are not the cause of damage they do. The responsibility in that area lies with our criminal justice system which has completely failed in addressing the root cause, which is crime in which a tool, eg, a handgun, is used.

Although I find the rest of the content of the *Journal* interesting for the most part, I regret to see such a prominent journal in effect openly advocating total disregard for the second amendment to the United States Constitution.

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SHARK CARTILAGE AND CANCER

■ *To the Editor:* I enjoyed reading Markman's review of the shark cartilage craze (May/June 1996).[1]

As he states, the fascination with shark cartilage stems partly from the notion that sharks do not get cancer.

Markman and your readers will be interested, but not surprised, to know that sharks do indeed get cancer. Stoskopf gives a detailed list of reported cancers in sharks in Chapter 101 of his book *Fish Medicine*.[2]

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THE INTERNET AND MEDICAL JOURNALS

■ *To the Editor:* I found your editorial (July/August 1996)[1] very interesting. I have been "curling up in bed with a laptop computer...tethered to a phone" for years. You may ask my wife to verify this! It has been easy to sell electronic media and notebook computers short, especially before today's powerful, colorful and relatively small notebook computers. I do prefer a magazine or journal, but I am limited to only a handful at any one time. Not so with a World Wide Web/Internet link. There, I can move about as the subject requires or as my own curiosity demands. For this advantage the notebook computer is only a minor inconvenience compared to the printed page. And it is indeed very portable.

In my case, I came to medicine with an undergraduate degree in engineering, so my experience reflects that natural to an early adopter of this technology. However, I see nothing that prevents its widespread adoption. Indeed the newness and gadgetry is attractive to many, although it is seen as an encumbrance to others.

Though the fundamental computer technology was here yesterday, it is now finally reaching a critical mass of adoption. You are to be congratulated in moving forward with this leading wave.

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