prognostic value independent of other predictive tests, including EF, its clinical utility will not be clear.

Also, as pointed out by Dr. Chan and colleagues, future studies of MTWA should consider all cause and cardiac mortality in addition to arrhythmic events as primary end points. As stated in our Discussion section, this is a limitation of prior studies and likely due to their relatively small size, thus resulting in a lack of hard end points. As highlighted by the recently published Defibrillator in Acute Myocardial Infarction Trial (DINAMIT) (2), a decrease in arrhythmic mortality does not necessarily translate to a decrease in cardiac or overall mortality.

Additionally, it is important to emphasize an issue not raised by Dr. Chan and colleagues. That is, a strategy must be developed for proceeding when MTWA testing is non-diagnostic, which occurs on average in one-third of patients. Although a non-diagnostic test seems to carry the same prognosis as a positive test in previous studies, fundamentally it seems illogical to consider a non-diagnostic test to be relevant to the prediction of arrhythmic events. For example, events in these patients may have nothing to do with MTWA but to other factors such as heart failure.

Thus, future clinical studies of MTWA need to be carefully designed. Given the current guidelines for implantable cardioverter-defibrillator (ICD) implantation, it would be unethical to randomize patients with severe left ventricular (LV) dysfunction to ICD implantation, dependent on the results of MTWA testing. The most prudent approach may be the development of a sudden-death registry. Patients with structural heart disease, regardless of EF, could be enrolled in a registry after performing a variety of prognostic tests on them such as EF, signal-averaged ECG, and heart rate variability in addition to MTWA. By following these patients prospectively, clinicians could better understand how to incorporate MTWA as well as other established predictors into the decision for ICD implantation. In fact, we have proposed the development of a scoring system for risk stratification that incorporates all such variables to guide ICD implantation for primary prophylaxis (3). In addition, such an approach may help to utilize our health care resources more efficiently.

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REFERENCES


The Big Chill: Adverse Effects of Public Reporting on Access to Health Care

When asked by a national news organization to review and comment on the study published in JACC by Moscucci et al. (1), I had occasion to read the accompanying editorial by Turi (2). Dr. Turi, wrapping up an otherwise lucid and well-reasoned editorial, devotes a paragraph highlighting his own heroism in managing high-risk patients, then makes the following statement: “The public, even the well-informed public, frequently ignores published statistics, perhaps best demonstrated by former President Clinton’s recent cardiac catheterization and coronary artery bypass graft surgery (CABG).” These procedures were performed in the only two hospitals in New York State that had risk-adjusted death rates significantly higher than the statewide average.

Risk-adjusted CABG mortality at Columbia Presbyterian has never, ever been “significantly” higher than the statewide average. Because we are referring to an average, let us agree not to be surprised that roughly one-half the hospitals in New York State have risk-adjusted CABG mortality that is “higher than the statewide average,” and about one-half the hospitals are lower than the statewide average. Individual hospitals commonly fluctuate above and below the average from one reporting cycle to the next, which has been true for Columbia Presbyterian. In the most recent reporting cycle, three hospitals had risk-adjusted CABG mortality that was “significantly” higher than the statewide average, defined as falling outside the 95% confidence limits. I reiterate, Columbia Presbyterian was not one of these statistical outliers, and has never been one of them. Dr. Turi and the editorial board of JACC are certainly sophisticated enough not to be excused for tossing around the term “significantly” when not referring to a statistically significant difference. Never mind the questionable taste employed when dragging an individual patient into print to score points, just as Larry Altman did in the New York Times article that Dr. Turi cites (inaccurately) to support his argument.

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REFERENCES


REPLY

Dr. Smith’s kind categorization of my editorial (1) as generally both “lucid and well-reasoned” is tempered by what appears to be lack of awareness of New York State’s report card for his own
institution. “Two hospitals (Columbia Presbyterian-NYP and Westchester Medical Center) had risk-adjusted mortality rates that were significantly higher than the statewide rate” was clearly stated in the New York State Department of Health report of April 2004, Adult Cardiac Surgery in New York State (2), based on the data (2001) available at the time of President Clinton’s bypass surgery. Thus, Dr. Smith’s testimonial that “Risk-adjusted CABG mortality at Columbia Presbyterian has never, ever been ‘significantly’ higher than the statewide average” and “I reiterate, Columbia Presbyterian was not one of these statistical outliers, and has never been one of them” is inaccurate. The risk-adjusted mortality rate (RAMR) for Columbia Presbyterian in that report was 3.93 (95% confidence interval [CI] 2.33 to 6.21). The statewide RAMR was 2.18, placing Columbia’s CI range entirely above the statewide mean, the definition for “significantly higher” explicitly stated in the report. Thus, despite his invitation to the reader, namely “let us agree . . . that roughly half the hospitals . . . have risk-adjusted CABG mortality that is ‘higher’ . . . and about half the hospitals are lower,” only two hospitals had CIs that put them entirely above the mean.

Mr. Altman’s New York Times article stated the following (3): “The hospital where former President Bill Clinton awaits bypass surgery has the highest death rate for the operation in New York State, according to the state’s Health Department. While the death rate is quite low—fewer than 4% of all bypass operations—it is still nearly double the average for hospitals in the state that perform bypasses . . . Columbia Presbyterian and Westchester Medical Center were the only two hospitals in the state that had risk-adjusted death rates that were significantly higher than the statewide rate.” My only reference was to the last sentence, included almost verbatim (discreetly omitting hospital names). I am puzzled why Dr. Smith states the citation was inaccurate.

As to the question of taste, “heroism” was neither stated nor implied, and Dr. Smith’s use of that word is hyperbole. “Courage” was the term used; it did not originate with me, and it was a quote rather than a self-congratulatory description of a rather routine procedure. Neither “heroism” nor “courage” is implied by the actions of 62% of surgeons in New York State who admitted not performing cardiac surgery because of fear of public reporting (4) or by the opinion of 83% of interventional cardiologists that high-risk patients are similarly denied percutaneous coronary intervention (PCI) (5). As to the ad hominem attack on “dragging an individual patient into print” (the patient was neither dragged nor identified), public policy ultimately has important effects on individual patients. Anecdotal examples are an appropriate editorial tool to highlight the human consequences of risk-averse heart surgeons and interventional cardiologists. “A single death is a tragedy, a million deaths is a statistic” (Joseph Stalin’s comment to Winston Churchill at the Potsdam Conference) argues for looking beyond arcane discussions of risk-adjustment techniques to this policy’s effect on individuals.

Figure 2 in the study by Moscucci et al. (6) shows that the hospital in New York with the highest percentage of cardiogenic-shock patients still does fewer such cases than the hospital with the lowest rate in the Michigan consortium. Given the 41% reduction in mortality suggested by the SHould we emergently revascularize Occluded Coronaries for cardiogenic shocK (SHOCK) trial registry (7), the fact that PCI for cardiogenic shock is performed one-quarter as often in New York State as in Michigan suggests that this is a statistic with considerable tragedy attached.

Further, as to the question of taste, I was careful not to mention Columbia Presbyterian or Westchester Medical Center by name anywhere in my editorial. I do not believe these numbers reflect quality of care at either hospital. Dr. Smith’s letter misses one of my major points: individuals sophisticated enough to understand the limitations of public reporting recognize that the risk-adjusted mortality rate is so flawed as to be potentially useless. At the same time, as my editorial (1) maintained, the net effect of public reporting does significant harm to the public, both because patients may be misled in their decision making, and because physicians, as I believe is illustrated by Dr. Smith’s letter, are driven to great lengths by discomfort with their own or their institution’s reported outcomes.

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REFERENCES