Stephen T. Mennemeyer
Michael A. Morrisey
Leslie Z. Howard

Death and Reputation: How Consumers Acted Upon HCFA Mortality Information

From 1986 through 1992, the Health Care Financing Administration (HCFA) released information comparing patient death rates at individual hospitals. This was viewed widely as an effort to aid consumers in selecting hospitals. This study evaluates how the release of this information affected hospital utilization, as measured by discharges. It finds a very small, but statistically significant effect of the HCFA data release. A hospital with an actual death rate twice that expected by HCFA had fewer than one less discharge per week in the first year. However, press reports of single, unexpected deaths were associated with an average 9% reduction in hospital discharges within one year. HCFA was justified in eliminating its mortality report, not because it was being used by consumers to choose hospitals, but because it was not. Implications for report cards are discussed.

"The purest treasure mortal times afford
Is spotless reputation . . ."
William Shakespeare, *Richard III* 1:1

On March 10, 1986, the Health Care Financing Administration (HCFA) released a brief mimeograph report (HCFA 1986) that showed the actual and predicted Medicare inhospital mortality rates for short-term general hospitals which, in 1984, were "statistical outliers" in terms of their mortality experience. In subsequent years through 1992, HCFA released a report identifying all community hospitals' actual and expected mortality rates (HCFA 1987, 1988, 1989, 1991 and 1992). In 1993, Bruce Vladeck, the HCFA administrator, chose not to release the data, saying, "Regrettably, the publication increasingly has come to be regarded primarily as a consumer publication" (Vladeck 1993).

The rationale for releasing these data was that, while far from a perfect measure, the reports gave the public useful information on the quality of the hospitals in their communities. It was expected that because of the release of mortality information, patients, their physicians, and their insurers would avoid lower quality facilities and instead frequent higher quality hospitals. Further, the pressure of both public scrutiny and the potential shift of patients would lead hospitals to improve their health outcomes.

From the beginning, the HCFA mortality studies were reported in the popular press. The first year, the *New York Daily News* reportedly ran the story on page two under the headline: "City's 'Killer' Hospitals Listed" (Moskowitz 1994). In many cities, the release of the HCFA mortality report has been the subject of an annual story on the ranking of area hospitals. Indeed, by 1994 a *Consumers' Guide to Hospitals* was published listing the 1990 and 1991

***Stephen T. Mennemeyer, Ph.D.**, is an associate professor and scholar; **Michael A. Morrisey, Ph.D.**, is a professor and the director; and **Leslie Z. Howard, M.A.**, is a research assistant, all at the Lister Center for Health Policy, School of Public Health, University of Alabama at Birmingham.* *This study was funded by the Robert Wood Johnson Foundation under grant no. 021080.* *Address correspondence to Dr. Mennemeyer at the Department of Health Care Organization and Policy, School of Public Health, University of Alabama at Birmingham, Birmingham, AL 35294.*

Inquiry 34: 117–128 (Summer 1997) © 1997 Blue Cross and Blue Shield Association and Finger Lakes Blue Cross and Blue Shield.
0046-9580/97/3402–0117$1.25

HCFA mortality ratings for 5,500 acute care hospitals (Center for Study of Services 1994).

Since the first release of the HCFA data, researchers have been investigating the relative quality of mortality data as a measure of hospital quality. To our knowledge, however, no one has examined whether the release of hospital mortality data by HCFA affected the use of hospitals. The purpose of this paper is to examine whether the HCFA data releases had an impact on community hospital discharges over the period 1984 to 1992. The first section briefly describes and reviews the literature on the strengths and weaknesses of this measure. The second section develops our methodology and presents our empirical measures. The third section presents the findings, and the final section summarizes the paper and contains our conclusions.

To anticipate a bit, we found that the release of the HCFA mortality data, at best, had only a small impact on hospital discharges. Press reports on the HCFA findings failed to have measurable effects on discharges. However, press reports of easily understood, bad outcomes did influence hospital volume. A newspaper account of an unusual hospital death was associated with a 9% reduction in hospital use, for the average hospital.

Background

HCFA released information on the actual and predicted mortality rates of hospitals from 1986 through 1992. Through time, the reports used progressively more sophisticated methods to compute the actual and predicted mortality rates, and they presented a somewhat different breakdown of the mortality rates by disease or procedure categories. Table 1 summarizes the date of release, the vintage of the data analyzed, and the statistical method used to determine the predicted mortality.

The first release, in April 1986, reported inhospital Medicare beneficiary mortality in calendar year 1984, but only for the hospitals in the tails of the distribution. Later reports contained data and results for all hospitals and for multiple years; however, there was always a lag of approximately 18 months between the vintage of the data and the date of the release. No report was issued using 1985 data.

Table 1. The HCFA mortality reports

Date of release	Years covered	Year basis	Method of analysis
April 1986	1984	Calendar	Ordinary least squares
December 1987	1986	Calendar	Logit
December 1988	1986, 1987	Calendar	Logit
December 1989	1986, 1987, 1988	Calendar	Logit
October 1991	1987, 1988, 1989	Fiscal	Survival
June 1992	1988, 1989, 1990	Fiscal	Survival
Not released 1993	1990, 1991	Fiscal	Survival

The 1991 release is typical of the later releases. It offered statistics that were comparable for the years 1987, 1988, and 1989. The data for the HCFA analyses were obtained from the Medicare patient abstracts records (MEDPAR) file, which contains summary information on the hospitalizations of Medicare beneficiaries. These records were supplemented by Social Security Administration data on beneficiary deaths. Mortality rates were computed in terms of deaths occurring within 30, 90, and 180 days of admission. In cases where a patient was admitted to more than one hospital, the patient was attributed randomly to one of the hospitals. In earlier reports, death rates were sometimes based upon deaths occurring during hospitalization or upon 30 days post admission. Also in earlier reports, a patient admitted to multiple hospitals was assigned to the last hospital for analysis purposes.

In 1991, the predicted probability of death for each patient within a given number of days of admission was estimated as a function of: age, sex, comorbidities of cancer, cardiovascular disease, liver disease or renal disease, admission source (e.g., referral by physician, transfer from a nursing home), and measures of previous numbers of hospitalizations. A survival model then was used to estimate the relationship with the predicted rate for a hospital obtained by summing the probabilities of death for each admitted patient. In 1989, a logistic regression model was used with the number of hospital admissions in the prior six months, the principal diagnosis of each admission, and other covariates similar to the 1991 analysis. Earlier reports were less sophisticated about using covariates to adjust for risk of death. The first report, released in 1986, used the hospital rather than the patient as the unit of analysis, and used an ordinary least squares regression approach.

A number of studies have examined death rates among hospitalized patients and used them to compare hospitals and to recommend policy. Examinations of the relationship between volumes of certain procedures and corresponding mortality rates are common (Adams, Fraser, and Abrams 1973; Williams 1979; Luft, Bunker, and Enthoven

1979; Maerki, Luft, and Hunt 1986; Luft et al. 1990).

There have been a number of studies critiquing the HCFA approach. Miller et al. (1994) provide a good summary. In general, there have been four methodological criticisms. First, the hospital data themselves are prone to error and/or differential diligence in coding. Second, the measures fail to adequately account for severity of patients. Third, the measures often fail to adequately distinguish comorbidity present at admission from that which occurred after admission, thereby making poorer quality hospitals appear to have more severe cases. Fourth, hospitals and their physicians have different criteria for the admission and care of patients; patients who may not be admitted by one physician may be admitted by another, leading to classic selectivity bias.

The most basic of the criticisms is the capability of administrative data to reflect clinical quality. Hannan et al. (1992) examined an administrative record system, somewhat similar to that used by HCFA, to see how closely it could predict patient mortality and identify "outlier" hospitals. The study focused on coronary artery bypass graft (CABG) surgery in New York state. It found that models based on the clinical record system were more accurate at predicting mortality and at identifying outlier hospitals which were likely to have quality problems than were models based on administrative data. The authors concluded that administrative databases were adequate for quality assurance purposes or for targeting hospitals for onsite visits by regulatory authorities. They noted, however, "if the information is used to inform consumers of relative quality of hospital care, the differences in hospital ratings between the two systems as well as the potential damage to a hospital's reputation are probably too great to risk using an administrative database."

In contrast, HCFA's own analysis (Krakauer et al. 1992) indicates substantial agreement between the Medicare claims data and clinical data in predicting hospital mortality. Using data on nearly 47,000 patients from 84 hospitals over the 1987–1990 period, the authors found that the ranking of hospitals based upon claims data was highly correlated with that using clinical data (correlation coefficient .91). If a hospital was not a mortality outlier under the claims model, then it was very unlikely to be one under the clinical model. However, positive predictive power was only modest; if a hospital was an outlier under the claims model, there was a better than one in three chance that it was not an outlier under the clinical model.

Luft and Romano (1993) examined all CABG patients in California from 1983 through 1989. They concluded: "Risk-adjusted outcomes for CABG patients derived from administrative data exhibit substantial patterns of consistency. Such patterns cannot be detected for low-risk patients but are evident for the top quartile of patients stratified by risk. Even with reporting lags and changes in hospital outcomes over time, a policy of channeling high-risk patients away from high-outlier hospitals and toward low-outlier hospitals could lower the overall risk-adjusted mortality rate by 54%."

Another problem is that hospitals may not be equally good at treating every condition. Rosenthal (1997) found a weak association between hospital mortality rates for individual diagnoses, using HCFA mortality data for discharges in fiscal year 1991. He argues that it may not be valid to generalize conclusions about hospital performance from a single diagnosis.

There also has been criticism of the statistical methods used to determine outliers. Localio and colleagues (1997) have examined how hospital and physician mortality rates are reported for CABG surgery in Pennsylvania, a state with a data reporting and analysis system somewhat more sophisticated than HCFA's. They found that the Pennsylvania system tends to over-report the number of outliers because it fails to adequately model the random variation among hospitals and physicians. The problem is analogous to the "fixed effects" that we subsequently discuss. They also find that the statistical power to identify outlier hospitals or physicians is often problematic due to the small number of cases treated by some of these providers.

For all of this debate there is no empirical evidence, to our knowledge, that the release of the HCFA data or other quality of care data has had an impact on the use of hospitals. On the one hand, Luft and colleagues (1990) have observed that there is no empirical evidence that measures such as the HCFA mortality data are related to decisions of where patients are hospitalized. Their study did not actually examine the effect of the release of data, however. It examined how the quality of care, as measured by unpublished mortality and other proxies, affected admissions on the assumption that quality was implicitly known by market participants. On the other hand, there have been a number of studies which imply that patients' perceptions of quality affect the use of hospitals. Bronstein and Morrisey (1991), for example, found that rural patients travel considerable distances, bypassing local rural hospitals, to obtain care at larger or more urban hospitals. Feldman and colleagues (1990) found that structural measures of quality were associated with the determination of which hospitals got managed care contracts. Hodgkin (1996) found that the presence of cardiac catheterization has a strong significant effect on choice of hospitals, but that the effect appeared to be limited to those patients with some probability of using the service. There was little evidence that the service was a general signal of quality.

Methodology

The Nature of Knowledge

Attempts to analyze the effects of the release of information on performance are immediately faced with the Watergate/Whitewater problem: to wit, what did you know and when did you know it? There are at least three alternative models: the "implicit knowledge" model, the "phone book" model, and the "media" model.

The implicit knowledge model asserts that the quality of alternative hospitals at any point in time is known by patients, or at least by their physician-agents, and that they act upon this knowledge. Through their own experience, or that of their friends and family, patients know the "good" and "bad" hospitals. In addition, or alternatively, through interchanges in the elevators, over coffee, and at medical gatherings, physicians have come to know the relative quality of local hospitals. In this model, the market reacts to the implicit knowledge, and the eventual, that is, lagged release of HCFA mortality data has no effect. Thus, if the implicit knowledge model is true, we expect to see that the release of, say, 1986 mortality information in 1988 will not affect a hospital's 1988 utilization. However, if the model is true, and if the HCFA mortality release is an unbiased measure of true (or at least perceived) quality, then the 1986 HCFA mortality information, applied to 1986 hospital utilization experience, will result in lower use in low-quality hospitals and higher use in high-quality hospitals, other things being equal. The released data, projected back in time, capture the implicit knowledge in the market in that earlier period.

A second view of knowledge is the phone book model. In this model, patients (and their physicians) are largely ignorant of hospital quality. The knowledge consists of a book of information on hospital mortality. With some effort an individual or a physician can obtain access to the book and identify those hospitals that are "good" and "bad." Under this theory, HCFA releases its most recently available measure of hospital mortality, that is, it releases its 1986 measures in 1988. These 1986 data will affect 1988 utilization in the expected directions.

A third view of knowledge is the media model. Consumers and physicians are essentially ignorant of hospital quality. HCFA releases its findings with a lag. However, the costs of processing this information are high. Only when the local newspapers, radio, and television report high- and low-mortality hospitals do decision makers adjust their utilization.[1] A variant of the media model involves advertising. In this view, not only do the media report news on hospital mortality, but individual hospitals spend resources to call attention to their governmentally announced high quality.

In the empirical sections that follow, we develop measures of implicit, phone book, and media information and apply them in alternative models of hospital discharges. We also incorporate additional media variables that capture non-HCFA quality of care stories reported in the local papers.

Econometric Issues

As often happens, the theory provides little insight as to the correct econometric specification of the model. We considered two major alternatives: a partial adjustment model and a changes model. Within each model type, we have explored fixed and random effects variants, and have estimated the implicit knowledge, phone book, and media models on the effect of the HCFA data release.

The partial adjustment assumes that there is an equilibrium level of hospital discharges, D^*, that results from a vector of hospital characteristics, X, and other variables, Z. That is:

$$D^*_{it} = a_0 + a_1X_{it} + a_2Z_{it} + v_{it}$$

where v is the random error term, and i and t denote hospital i in year t. The market is not able fully to adjust to the equilibrium level of discharges from one period to the next. The degree to which it can make the adjustment is represented by the parameter γ:

$$D_t - D_{t-1} = \gamma(D^*_t - D_{t-1})$$

where $0 < \gamma < 1$. Substituting terms produces an equation where discharges are regressed on their lagged value and the other explanatory variables.

$$D_{it} = a_0\gamma + a_1\gamma X_{it} + a_2\gamma Z_{it} + (1 - \gamma)D_{it-1} + \gamma v_t.$$

In this model, estimated regression coefficients on vectors X and Z are interpreted as long-run impact (the a's) modified by the adjustment parameter (γ). Hospital discharges thus are viewed as a function of hospital and market characteristics, including information on hospital quality. We hypothesize that favorable quality information increases hospital use; unfavorable information decreases use.

The lagged variable (discharges in year $t - 1$) is a key to the partial adjustment analysis. When information becomes available, physicians and their patients use it. However, everyone is not able to adjust immediately. Some people may find out about the newly released report only after some time. Some patients may arrive at the arguably higher quality facility after their physician has had an opportunity to obtain clinical privileges at that hospital. Other patients may have had to find new physicians. These information flows are controlled for in the lag term. The short-run impact of the new information is captured by the estimated coefficient on the HCFA mortality report, $a\gamma$ from above; the long-run impact is measured by $a\gamma/[1 - (1 - \gamma)]$, that is, the short-run coefficient divided by one minus the coefficient on the lag term.

The changes analysis assumes that the market moves to equilibrium in a single time period. Changes in independent variables affect changes in the dependent variable, here discharges. The model takes the general form:

$$(D_{it} - D_{it-1}) = b_0 + b_1 (X_{it} - X_{it-1}) + b_2(Z_{it} - Z_{it-1}) + u_{it}.$$

In this formulation, the release of a new HCFA report results in a (potential) change in one of the hospital characteristics, reported quality (an X). This change in information results in a direct change in discharges and is measured as the coefficient value b_1.

In the partial adjustment model, the set of hospital and market characteristics are captured by alternative measures of the HCFA data release, the lagged dependent variable, hospital costs per admission, and health maintenance organization (HMO) enrollment in the area. Other things being equal, costs per admission are a proxy for price and one would expect that higher prices lead to fewer admissions. However, the variable also may pick up differences in services and amenities, making the expected sign ambiguous. HMO enrollment also has an ambiguous effect. On the one hand, HMO enrollees have lower hospital admission rates, suggesting a negative coefficient. On the other hand, evidence suggests that HMOs selectively enter markets with higher utilization, implying a positive coefficient. In the changes model, these variables are included as their annual change.

To control for the release of HCFA data on neighboring hospitals, early specifications included the straight-line distance to both the nearest hospital with a high and a low HCFA mortality outlier status. These variables were always small and without statistical significance, and were dropped from subsequent specifications.

To further control for hospital and market characteristics, we estimated fixed and random effects variants of the two basic models. Based upon the Hausman (1978) test, we reject the random effects models. In the results section, we report the fixed-effects results for the partial adjustment model with hospital and time components, hospital-only components, and no fixed effects. We also report the full fixed-effects specification for the change model.

Data and Measures

The data for this study were drawn from the HCFA mortality reports, the American Hospital Association (AHA) Annual Survey of Hospitals, the NEXIS news retrieval service, and the Area Resource File. The analysis is limited to community hospitals during the period 1984–1992. Data for 1983 were included to allow for the computation of lags and changes over the entire nine-year period. The sample consisted of all community hospitals with a standardized HCFA mortality rate of more than one standard deviation from the mean in any year. In addition, we included 50% of hospitals that were never outliers under this definition. Over the nine-year period of study, this yielded 23,564 observations. Table 2 presents the variables used in the analysis, their means, standard deviations, and sources.

We were able to obtain computer tapes of the mortality data from HCFA for the release years 1989, 1991, and 1992. However, HCFA was unable to find tapes for earlier releases. Based upon a literature search of people using the HCFA mortality data, we obtained a tape copy of the 1987 release.

We could find only a microfiche copy of the 1988 release and a paper copy of the 1986 report.[2] From these sources we extracted the total number of Medicare deaths, the predicted number of deaths and its associated standard error, the degrees of freedom of the estimate, and the time period to which the data relate.

HCFA used a sequence of methods to determine outlier hospitals. We experimented with three approaches for making the information in the data consistent across years: the standardized mortality rate, the *z*-score, and the area under the mortality curve. In practice, it makes little difference which measure was used. We report results using the standardized mortality rate, defined as the observed mortality divided by the predicted mortality. When multiple years of data were reported in a single release, we only use the most recent year.

The data were linked by hospital identifier to AHA Annual Survey data. From the annual survey we extracted hospital discharges, total expenditures per adjusted admission, and the reporting period. For missing values we interpolated from preceding and succeeding years. However, the time periods covered in the HCFA data releases, in general, do not correspond to AHA hospital reporting periods.

Table 2. Means, standard deviations, and sources

	Mean	Standard deviation	Source[a]
Partial adjustment models			
Discharges	5,813.5	6,621.0	A
Lagged discharges	5,905.2	6,633.1	A
Standardized mortality rate—implicit knowledge	1.04	.23	B
Standardized mortality rate—phone book	1.07	.22	B
Costs/discharges (in $1,000s)	5.8	4.1	A
HMO enrollment (in 1,000s)	138.0	554.5	C
Untoward death report	.0003	.0184	D
Changes model			
Δ Discharges	−2.3	30.8	A
Δ Standardized mortality rate—phone book	2.8	35.3	B
Δ Cost/discharge	8.9	22.5	A
Δ HMO enrollment	20.9	325.1	C

[a] A = American Hospital Association, Annual Survey, 1983–1992.
B = Health Care Financing Administration, Report on Hospital Mortality, 1986–1993.
C = Bureau of Health Professions, Area Resource File, 1995.
D = NEXIS full-text online newspaper retrieval service, 1984–1992.

Nor are the dates of release coincident with hospital or HCFA reporting periods. We dealt with this by computing the hospital's average HCFA standardized mortality rate weighted by the proportions of the reporting period subject to the old and new data release.

Newspaper reports of hospital quality were drawn from NEXIS, a full-text online newspaper retrieval service. We searched the database in each year 1984 through 1992 and constructed six dummy variables. Two dummies related to one or more press reports of the HCFA data release indicating that the hospital was either a high- or low-mortality outlier in at least one of the disease/procedure categories identified by HCFA. The other four dummy variables related to other press reports of a hospital's quality not associated with the HCFA data releases. These were: the presence of a favorable story; an unfavorable story; a government action, such as the announcement of a lawsuit; and an unusual death.[3]

These data suffer from three problems. First, the NEXIS database does not include all newspapers, and its universe of included papers expanded over the course of our study. Second, the NEXIS database does not include nonprint media. Third, except for the HCFA stories, the quality stories were rare. We ran the partial adjustment and change models using the "media model" of HCFA information diffusion and found only small and statistically insignificant results, often of the wrong signs. We also experimented with splitting our sample and estimating the early and late periods separately to deal with the growing NEXIS database. The variables had no effects, perhaps because of data limitations. We do not present these results here.

Results

Our findings with respect to alternative specifications of the phone book model of knowledge are shown in Table 3. It is immediately apparent that the specification matters. Column I reports the ordinary least squares (OLS) specification without any additional controls for hospital characteristics or time trend. The inference from the positive and statistically significant coefficient on the standardized mortality rate is that having more deaths than is expected results in more patient volume.

The problem, in part, is that the specification does not adequately take into account hospital and market specific factors. Column II is a fixed-effects model in which each hospital serves as its own control. In this specification, the ratio of actual to expected mortality (i.e., the standardized mortality rate) remains positive but is smaller and statistically insignificant. This suggests that the release of the HCFA data did not affect hospital discharges.

During the 1984–1992 period, the trend of national hospital discharges was downward, both on a per capita basis and in aggregate discharges. Column III accounts for this trend by employing a full fixed-effects model, which includes dummies for each hospital and each year. This specification finds a small, but statistically significant negative effect of the HCFA data release on hospital discharges.

Finally, column IV presents the changes model using the full fixed-effects framework. We also estimated the changes model with only hospital dummies and in a simple OLS context. None of these specifications provides any evidence that the HCFA data release affected the discharges at U.S. community hospitals.

Therefore, we use the partial adjustment model with full fixed effects (column III) to examine the impact of the HCFA release of mortality data on hospital volume. This specification allows the hospital to serve as its own control, allows for important time trends, and allows the impact of information shocks to be manifest over time.

Column V presents our principal findings for the implicit knowledge model. The implicit knowledge model holds that, through whatever mechanism, the market already knows the quality of its various hospitals. To measure this, we use the standardized HCFA mortality index merged back to the year to which the data applied. Column V shows that the mortality index had only a small and statistically insignificant effect on hospital discharges. Indeed, the point estimate suggests that more actual deaths relative to predicted deaths resulted in slightly more discharges. This result provides no support for the argument that patients and their physicians already knew which hospitals were particularly good and which were particularly bad, at least with respect to the dimensions of quality captured by the HCFA methodology.

In contrast, column III reports the results with respect to what we regard as the best specification of the phone book model. Under this approach, the market receives new information when HCFA releases its report. Patients and/or their physicians learn of the differential quality either directly from the HCFA reports, or perhaps through press reports of various kinds. The HCFA data release did reduce hospital admissions in the short run by a small but statistically significant amount. A hospital with two actual deaths for each HCFA predicted death had, within one year, 46 fewer discharges, the equivalent of less than one discharge per week. Over the long

Table 3. Alternative models and specifications of information flow

	Phone book specifications				Implicit knowledge
	I Ordinary least squares[a]	II Partial adjustment fixed effects (hospitals)[a]	III Partial adjustment fixed effects (full)[a]	IV Changes model fixed effects (full)[a]	V Partial adjustment fixed effects (full)[a]
Lagged discharges	.99 (.001)***	.59 (.005)***	.60 (.005)***	—	.60 (.005)***
Standardized mortality rate	69.32 (18.72)***	10.01 (19.16)	−46.40 (24.00)**	.002 (.007)	8.60 (22.22)
Cost/discharge ($1,000s)	.96 (1.01)	−17.59 (1.35)***	−18.77 (1.39)***	−500.26 (8.98)***	−18.58 (1.38)***
HMO enrollment (1,000s)	.06 (.007)***	.20 (.036)***	.16 (.037)***	.07 (.63)	.17 (.04)***
Constant	−137.19 (21.79)***	[b]	[c]	[c]	[c]
Adjusted R^2	.998	.993	.993	.162	.993
N	23,564	23,564	23,564	23,564	23,564

Note: Standard errors are in parentheses.
[a] In columns I, II, and III the dependent and independent variables are measured as levels. In column IV dependent and independent variables are measured as the change from the prior years.
[b] Column II includes separate dummy variables for each hospital.
[c] Columns III, IV and V include separate dummy variables for each hospital and year.
* Significant at the 90% confidence level.
** Significant at the 95% confidence level.
*** Significant at the 99% confidence level.

run, this would imply 116 fewer discharges $(-46.4/(1 - .6))$ over our nine-year "long run." Based upon this result, the HCFA data release had only a very small effect.

Our results suggest that the effects of mortality reports, to the extent that they occur, had slow impacts. The coefficient on the lagged discharge variable is .60 for each model. This implies that 40% of the effect of the information was felt in the first year, and the rest appeared subsequently.

Turning briefly to the other variables, we found that hospitals with higher inflation-adjusted costs per discharge had fewer discharges. This finding was significant at the 99% confidence level. Given the lagged dependent variable and the fixed-effects specification, this variable appears to serve as a proxy for price. The effect is very small, however. A 10% increase in price implies a 2% reduction in discharges in the short run, and a 4.5% reduction in the long run.

The number of HMO enrollees in the county also had a positive and statistically significant effect in both equations III and V. We interpret this as reflecting a tendency of managed care firms to enter markets where utilization is high, and therefore, where there is an opportunity to either achieve reductions in utilization or attract subscribers who are low utilizers.

Finally, we re-estimated these equations including the vector of media stories relating to the hospital's quality. In all of the alternative specifications, the media stories had no statistically significant effects. They also did not affect the coefficient estimates of the other parameters in the model. The exception was press reports of an untoward death in the facility. These are press reports of such events as a patient falling off a gurney and dying, or an aide inadvertently turning off a ventilator. In virtually all the specifications examined, this variable was large, robust, and statistically significant. It resulted in a reduction in discharges of approximately 9%, evaluated at the sample mean.

We did not include the untoward deaths variable in our reported specification; it applies to only five observations in our data set and the inclusion or exclusion of it did not affect the magnitudes of the other estimated parameters. The figure shows the reported discharges for these five hospitals over the 1985–1992 period. The year in which the press report occurred is circled. At each data point, we also indicate the value of the HCFA ratio of actual to predicted deaths using the figures most recently available to consumers, that is, the "phone book" approach. Only one of these observations was designated by HCFA as an outlier, specifically Hospital C in 1992 for high mortality.

Curiously, in four of the five cases, the number of discharges declined in the year in which the untoward event was reported. Sometimes the decline was as much as 12%. Notice too, that coincident values of the HCFA mortality ratio are generally consistent with our finding that a small effect occurred in the expected direction for the mortality information. For example, Hospital A had an increase in discharges from 1985 to 1986 coincident with a mortality ratio fall (i.e., improvement) from 1.0 to .78. After the untoward event, discharges fell, despite a tiny further improvement in the mortality ratio. Thereafter, it had no change from this lower discharge level (albeit with three years of missing data) coincident with a movement of its mortality ratio back toward the mean. For Hospital C, discharges continued downward after the untoward event, coincident with a mortality ratio that bordered and eventually achieved official high outlier status. For Hospital D, the untoward death was coincident with a mortality ratio that rose about one standard deviation above the mean; thereafter discharges rose while the mortality ratio moved back toward the mean. For Hospital E, the untoward event occurred coincident with a mortality ratio of 1.0; a modest rise in discharges occurred the next year when the mortality ratio fell to a level just inside the two standard deviation range; thereafter, discharges were essentially flat with unexceptional values of the mortality ratio.

Summary and Conclusions

This study is the first to evaluate the effects of the release of HCFA mortality reports on hospital utilization. We examined discharges of U.S. community hospitals over the period 1984 through 1992 using a variety of alternative econometric approaches. The result is simply stated. Using the specification that is most sympathetic to the HCFA information effort, there was only a small effect of the release on hospital discharges. A hospital that had two actual deaths for each expected death reported by HCFA would have observed less than one fewer discharge per week in the first year, and only 116 fewer discharges over the nine-year long run.

We found no evidence to support an "implicit knowledge" view of hospital quality. The HCFA mortality index, projected back to the period in which it was collected, did not affect hospital discharges. We also found no support for the view that press reports of the HCFA release, rather than the information release itself, affected discharges. However, limited data availability keeps us from making too much of this finding. Indeed, our only statistically meaningful findings related to the actual release of the HCFA mortality report, our so-called "phone book" model in which the public treats the report as new information.

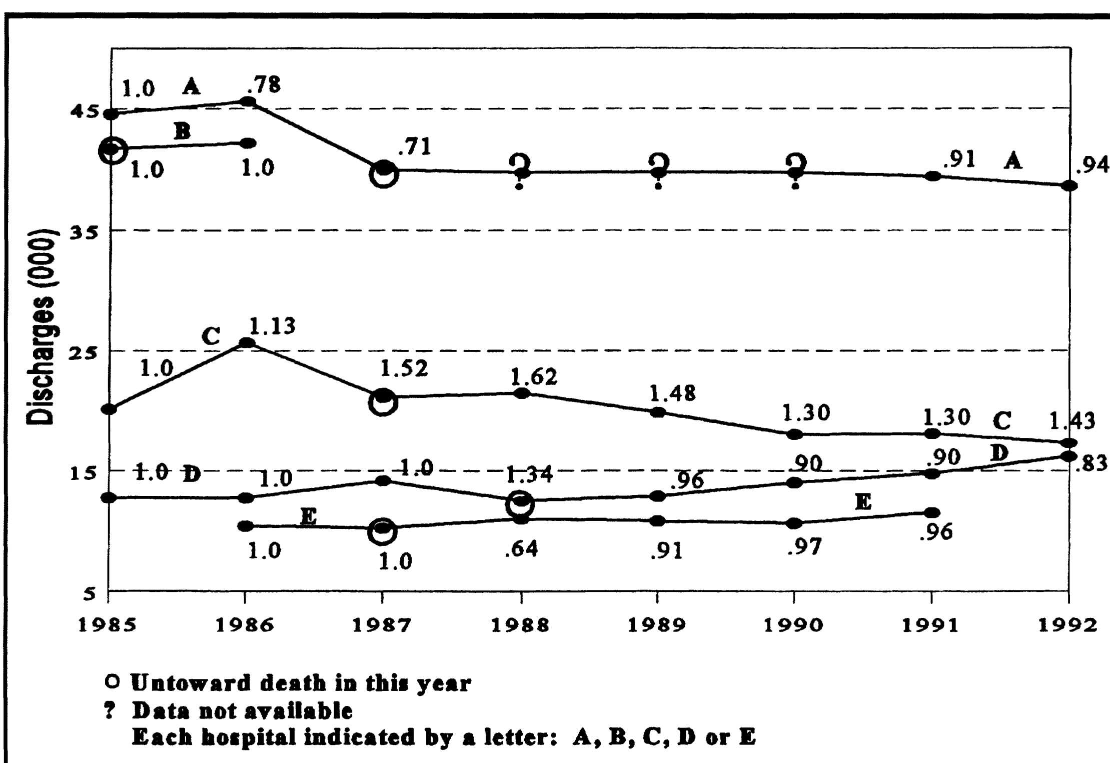
However, we also found evidence of large and significant effects of press reports of untoward deaths in hospitals. These reports, characterized by stories of an unfortunate patient who falls from a gurney and dies, resulted in reductions in discharges of approximately 9%.

The principal conciusion of this study is that HCFA was justified in ending the release of the mortality data—not because it had turned into a consumer information vehicle, but because it did not.

These findings raise two questions. First, why was the HCFA data release essentially ineffective and, second, what implications do these findings raise for the managed care report card movement that is currently gathering steam?

There are at least five explanations for our findings with respect to the HCFA effort. First, it may be that the HCFA measure of quality, the adjusted mortality rate, was inconsistent with information valued by physicians and patients. Iezzoni and her colleagues (1996) have shown recently, for example, that 10 alternative measures of severity-adjusted mortality rates for myocardial infarction yielded significantly different implications. They conclude that "whether individual hospitals were identified as especially good or bad frequently depended on the particular severity measure" (p. 1384). It simply may be that physicians and patients implicitly use a different metric to judge hospital quality.

Second, it may be that patients and physicians discounted the HCFA report of a poor quality hospital. Newspaper reports of the HCFA data release routinely carried an interview with a hospital leader in which the results were discounted due to alleged faulty analysis by HCFA or appeals to particularly



Discharges for five hospitals with untoward deaths (The HCFA ratio of actual/predicted deaths is displayed at each data point. Source: AHA Annual Survey, 1985–1992) severe cases or special circumstances. These reports may have blunted much of the impact that the data releases otherwise would have had. The recent Localio et al. (1997) paper offers statistical support to this native skepticism.

Third, it may be that general quality reports on a hospital are too general to carry much weight. Rosenthal's (1997) finding of low correlation among different types of mortality rates speaks to this point. Hodgkin (1996), furthermore, examined the presence of specialized services on the use of a hospital. He found that the availability of cardiac catheterization had a strong effect on the choice of hospitals among those likely to demand heart services. However, patients with no "option demand" for the specialized services did not appear to use the presence of the service as a quality signal. By analogy, it may be that patients value information on specific services that they soon may use, but otherwise ignore quality information. One might examine this by looking at the disease/procedure specific mortality rates that HCFA released. Under this hypothesis, heart-related mortality would affect discharges for heart-related patients. However, our finding of the effects of press reports of untoward deaths, as a generalized quality measure, would seem to undercut this argument.

Fourth, it may be that members of the non-Medicare population ignored the results because they did not believe that outcomes of the elderly were applicable to them. This is certainly possible. However, Medicare beneficiaries account for about 40% to 50% of hospital admissions. If the young ignored the findings but the elderly did not, it then would imply no impact on the young, but nearly twice the impact on the elderly. Even so, this effect would remain very small, less than two fewer Medicare admissions per week when the death rate was twice what HCFA's models would suggest.

A fifth explanation is cultural. The population may not have been ready to absorb the information. From this perspective, the 1984–1992 period was still largely one of fee-for-service medicine with many incentives still in place to do everything potentially beneficial for the patient. Patients had personal experience with this sort of care. They also may have held deeply ingrained assumptions that the quality of care essentially was uniformly "good," although "better" in some locations than others. The view that care could be "poor" or actually cause harm may only have been dimly perceived.

Our own view stands with some combination of cultural trust, mixed messages in the media, and the complexity of the quality issue. The current media concerns about the quality of managed care providers probably is changing the culture and building acceptance for the view that there is a wider range of quality than people once believed. If the mixed message is a component of the problem, it will diminish in importance over time with the continued release of quality indicators of various kinds. Either the providers will lose credibility, or the quality measures will.

The complexity issue is more difficult. Our results suggest that the public essentially ignored a sophisticated attempt to measure quality. However, they appear to have clearly heeded very simple, unsophisticated, almost certainly nongeneralizable stories about untoward deaths. Together these findings suggest that any quality measure designed for public use must be easily understood.

What does this imply for the current efforts at developing and implementing report cards of managed care quality? We see three implications. First, the message must be simple and easily understood. From that perspective, efforts by the American Association of Retired Persons and the National Committee on Quality Assurance, among others, to "certify" providers of quality may be a useful approach. A simple message from a credible and respected entity may convey important information. Second, if the literature on the value of information on the quality of specific procedures is to be believed, it implies that lying behind the single quality summary is a set of simply understood quality reports on major procedures.

However, we are skeptical that such approaches only directed at individual consumers will be successful. Our third implication is that quality information also should be directed at decision makers who have stronger economic incentives to understand and use it. Some more sophisticated version of provider quality reports should be directed toward use by managed care firms and employers who may contract with them. More detailed information on managed care firm quality should be developed for employers and other group purchasers. This is not to say that the release of information to consumers should be abandoned. Far from it. Rather, the level of sophistication in the reporting to individuals and to organizations needs to be very different.

Notes

The authors thank Henry Krakauer, Alfred A. Rimm, Peter Kralovec, and Jerry Lanning for assistance in obtaining various data elements. Earlier versions of this paper were presented at the annual meetings of the Association for Health Services Research in Chicago in June 1995, and the American Public Health Association in San Diego in November 1995. We thank Henry Krakauer, Robert Ohsfeldt, Mahmud Hassan, Katherine Swartz and anonymous reviewers for comments on earlier drafts. Opinions expressed are those of the authors who are responsible for any remaining errors.

1 Within this structure of knowledge and the nature of the HCFA data releases, there are at least two other variants of knowledge. One is the "broad phone book" model in which the newly released information is for several years. Suppose in 1988, HCFA releases mortality data for each hospital in 1986, 1987, and 1988. Here the consumer internally weights all currently available data and makes utilization decisions based upon it. A second version is the "full information" model, in which consumers never discard old data and every data release, somehow weighted, enters into their decision calculus. We view these as second-order models, particularly given our findings.

2 The nature of these data was problematic. In the 1986 release (1984 data), HCFA only reported the actual and predicted values for hospitals in the two outlier tails of the distribution. We coded these and imputed the expected standardized mortality rate of 1.0 to the other hospitals. In the 1988 release (1987 data), we only had available microfiche data. For 1987 data, we substituted the 1989 release (of the 1987 data); it used the same logit model of mortality, was based upon calendar year data, and had the significant advantage of being available on tape. The correlation between the 1989 release and an urban sample of 1,488 hospitals for the 1988 release was .97.

3 We also were concerned that a hospital may market its services based upon a favorable report from HCFA. To our knowledge, no database exists to track advertisements. We attempted to measure this effort by using two dummies in place of the HCFA standardized mortality. These took the value "one" if a hospital was in the high or low tail of the mortality distribution. Hospitals in the low tail may be more likely to advertise their superior quality.

References

Adams, D. F., D. B. Fraser, and H. L. Abrams. 1973. The Complications of Coronary Arteriography. *Circulation* 48(3):609–618.

Bronstein, J., and M. A. Morrisey. 1991. Bypassing Rural Hospitals for Obstetrics Care. *Journal of Health Politics, Policy and Law* 16(1):87–118.

Center for Study of Services. 1994. *Consumers' Guide to Hospitals.* ISBN 0-9611432-5-8. Washington, D.C.

Feldman, R., H. Chan, J. Kralewski, B. Dowd, and J. Shapiro. 1990. Effects of Health Maintenance Organizations (HMOs) on the Creation of Competitive Markets for Health Services. *Journal of Health Economics* 9(2):207–222.

Hannon, E. L., H. Kilburn, M. L. Lindsey, and R. Lewis. 1992. Clinical Versus Administrative Data Bases for CABG Surgery: Does It Matter? *Medical Care* 30(10): 892–907.

Hausman, J. A. 1978. Specification Tests in Econometrics. *Econometrica* 46:1251–1271.

Hodgkin, D. 1996. Specialized Service Offerings and Patients' Choice of Hospital: The Case of Cardiac Catheterization. *Journal of Health Economics* 15(3):305–332.

Iezzoni, L. I., A. S. Ash, M. Shwartz, J. Daley, J. S. Hughes, and Y. D. Mackiernan. 1996. Judging Hospitals by Severity-Adjusted Mortality Rates: The Influence of the Severity-Adjustment Method. *American Journal of Public Health* 86(10):1379–1387.

Krakauer, H., R. C. Bailey, K. J. Skellan, J. D. Stewart, A. J. Hartz, E. M. Kuhn, and A. A. Rimm. 1992. Evaluation of the HCFA Model for the Analysis of Mortality Following Hospitalization. *Health Care Financing Review* 27(3):317–335.

Localio, A. R., B. H. Hamory, A. C. Fisher, and T. R. TenHave. 1997. The Public Release of Hospital and Physician Mortality Data in Pennsylvania: A Case Study. *Medical Care* 35(3):272–286.

Luft, H. S., J. P. Bunker, and A. C. Enthoven. 1979. Evaluating Individual Hospital Quality Through Outcomes Statistics. *Journal of the American Medical Association* 301(25):1364–1369.

Luft, H. S., D. W. Garnick, D. H. Mark, D. J. Peltzman, C. S. Phibbs, E. Lichenberg, and S. J. McPhee. 1990. Does Quality Influence Choice of Hospital? *Journal of the American Medical Association* 263(21):2899–2906.

Luft, H. S., and P. S. Romano. 1993. Chance, Continuity, and Change in Hospital Mortality Rates. *Journal of the American Medical Association* 270(3):331–337.

Maerki, S., H. S. Luft, and S. S. Hunt. 1986. Selecting Categories of Patients for Regionalization: Implications of the Relationship between Volume and Outcome. *Medical Care* 24(2):1505–1510.

Miller, G. M., L. S. Miller, B. Fireman, and S. B. Black. 1994. Variation in Practice for Discretionary Admissions: Impact on Estimates of Quality of Hospital Care. *Journal of the American Medical Association* 271(19):1493–1498.

Moskowitz, D. B. 1994. HCFA Mortality Data: What Do They Say About Quality? *Medicine and Health Perspectives* (October 24):1–4.

Rosenthal, G. E. 1997. Weak Association Between Hospital Mortality Rates for Individual Diagnoses: Implications for Profiling Hospital Quality. *American Journal of Public Health* 87(3):429–433.

U.S. Health Care Financing Administration (HCFA). 1986. No title, with attachments on prediction of statistical outliers for inpatient hospital mortality rates. Baltimore, Md.

———. 1987. *Medicare Hospital Mortality Information, 1986.* Washington, D.C.

———. 1988. *Medicare Hospital Mortality Information,*

1987. HCFA Publication 643, Superintendent of Documents Number AG22.34:987. Washington, D.C.
———. 1989. *Medicare Hospital Mortality Information, 1986, 1987, 1988.* Vols. 1–14. Pub. No. 00713, ICN No. 001744. Washington, D.C.
———. 1991. *Medicare Hospital Mortality Information, 1987, 1988, 1989.* Vols. 1–25. Pub. No. 00744. Washington, D.C.
———. 1992. *Medicare Hospital Mortality Information, 1988, 1989, 1990.* Vols. 1–55. ISBN 0-16-037885-0, Washington, D.C.: U.S. Government Printing Office.
———. 1993. *Medicare Hospital Information System, Fiscal Year 1991.* Data tape. Washington, D.C.
Vladeck, B. C. 1993. Letter to hospital administrators accompanying *Fiscal Year 1991 Medicare Mortality Information* (August 17). Health Care Financing Administration, Baltimore, Md.
Williams, R. L. 1979. Measuring the Effectiveness of Perinatal Medical Care. *Medical Care* 17(2):95–109.