# Medicare elective surgery outcomes and State prospective reimbursement programs

by Gary L. Gaumer, Eugene Poggio, and Cary Sennett

*This article examines the relationship between the introduction of State prospective reimbursement (PR) programs and mortality rates for elective surgery. We study 15 such programs using a sample of about 40 percent of U.S. hospitals. We examine mortality data for 1974 to 1983 for these hospitals, selecting a 20-percent sample of all Medicare admissions for eight elective procedures. Indirect standardization (age, sex, procedure) was used to define mortality outcomes, and regression procedures were used to estimate PR effects that controlled for hospital, community, and other regulatory influences. Introduction of PR is found to be occasionally and inconsistently associated with increases in relative mortality.*

## Introduction

In response to high rates of increase in hospital expenditures, prospective reimbursement (PR) programs were instituted by over 30 State, industry, and payer groups during the 1960's and 1970's. The objective of these revenue control programs was to provide incentives for prudent management by putting hospitals at some risk for the consequences of expenditure decisions. To a lesser extent, the programs have tried to make reimbursements more predictable and to unify reimbursement approaches across payers. As revenue-limiting programs, the extent to which expenditures are influenced is determined by the way that hospital managers respond to program incentives. It is the prerogative of the managers to decide how expenditure reductions (if any) are to be allocated within the institution; for example, whether nurse staffing, scope of services, or ancillary utilization are to be targeted as sources of savings; whether revenue restrictions will be met through asset consumption (deficits) or through expenditure cuts; and whether or not cuts will be made in ways that may affect the actual delivery of patient care services.

Insofar as PR programs motivate the reduction of expenditures on patient care services, there is concern that these programs might compromise the quality of hospital care. Recent research by Coelen et al. (1986) shows that at least eight State PR programs had reduced per-case expenditures by more than 8 percent by 1983, with several of these States achieving savings of 20 percent or more. No recent studies have been reported on the quality of care implications of the savings created by these State PR programs.

The impact of PR on the quality of care was examined in six major program-specific prospective reimbursement evaluations conducted in the mid-1970's (Geomet, 1976; Cromwell et al., 1976; Dowling, 1976; Thornberry and Zimmerman, 1975; Applied Management Sciences, 1975). In only one study, in downstate New York, was there any evidence that the quality of care may have been affected by PR. In this study, provisional accreditation of hospitals (a structural measure) was found to be sensitive to implementation of PR.

We have undertaken a multidimensional analysis of the impact of PR on hospital quality, using aggregate data for 15 States under PR between 1974 and 1983. Much of that work is presented elsewhere (Gaumer et al., 1987). We report here our analysis of the relationship between PR and patient care outcomes for a sample of Medicare patients hospitalized for elective surgical procedures between 1974 and 1983.

## Reimbursement and patient outcomes

Quality may be affected by changes in the hospital's volume and/or output mix in response to constraints on the total hospital budget. The magnitude of the effect on quality depends directly on the sensitivity of costs to changes in outcome quality. If reductions in quality yield only miniscule cost savings, then we can expect to observe little change in quality when budgets tighten. Similarly, if PR has little effect on hospital budgets, then we would not expect a large quality decline, even if the costs of increments in quality are large.

These hypothetical relationships between PR and quality presume a fixed-transformation relation between costs (resources) and quality (outcome). If administrators have been less than perfectly efficient in applying resources to achieve the observed level of patient outcomes, then the linkage between resource use and patient outcomes is loose. This implies that even large effects of PR on budgets may engender little or no change in observed patient outcomes as administrators tighten up the structure and process by which care is provided and, therefore, the efficiency with which patient outcomes are produced.

The extent to which quality might be expected to fall under PR is, of course, bounded at the lower end by accreditation and/or certification requirements and the fact that hospitals must maintain a staff of admitting physicians. For hospitals near the lower bound of quality, predicted quality deterioration can only occur until the point at which loss of accreditation and/or certification will take place.

Reprint requests: Gary L. Gaumer, Abt Associates Inc., 55 Wheeler St., Cambridge, Massachusetts 02138.

These postulated cost and reimbursement effects of PR on outcome quality lead to the expectation that outcome quality could fall under cost-containing regulations such as PR. The fall will be smaller:

- The smaller the PR effect is on budget reductions.
- The less sensitive costs are to quality changes.
- The more slack exists in the way that the hospital has been producing given quality levels in the past.
- The closer the hospital is to the minimal levels of quality needed to maintain accreditation and/or certification and retain medical staff.

# Methods

## Measures

Eight elective procedures are studied using three samples: the group of aggregated elective surgical cases (hemorrhoidectomy, cholecystectomy, inguinal herniorrhaphy, transurethral prostatectomy, open prostatectomy, hysterectomy, excision of bladder lesion, and mastectomy) and inguinal herniorrhaphy (IHR) and transurethral prostatectomy (TURP) cases separately. As an outcome measure of quality of care we use mortality rates for fixed time periods, each beginning with the hospital admission (e.g., the mortality rate in the 30 days following admission), without regard to whether deaths occur during or after the hospital stay. Mortality rates over periods of several different lengths are analyzed. With a shorter period, one runs the risk of not detecting the total effect of PR; with a longer period, it may be harder to detect an existing effect because of the increased likelihood of deaths from causes unrelated to the hospital stay. In the absence of *ex ante* knowledge about the impact of PR on mortality rates, we elected to estimate mortality rates over a range of intervals from 15 to 360 days after admission.

It was our expectation that most, if not all, of any PR effect would occur within the first 45 days following admission. It seems probable that any deterioration in quality would result from poorer care delivered preoperatively (e.g., inadequate screening of those at risk for complications of anesthesia leading to excess deaths from anesthesia), intraoperatively, or immediately postoperatively (especially wound complications attributable to reduced skilled nursing and embolic complications attributable to reductions in ancillary nursing care).

Throughout most of the analysis, we focus on the impact of PR on age-, sex-, and diagnosis- (or procedure-) adjusted "standardized mortality ratios." The standardized mortality ratio (SMR) is the ratio of actual mortality rate to the rate expected based on the actual rates in a reference (standard) population. For the reference population, we have used the Medicare admissions in the States that were never under PR. Using this population, we calculated two sets of standard rates, one for the period 1974 through 1978 when the *Eighth Revision International Classification of Diseases, Adapted for Use in the United States* (ICDA-8) classification system was used and one for the period 1979 through 1983 after the changeover to *International Classification of Diseases, 9th Revision, Clinical Modification* (ICD-9-CM). Standard rates were computed for a classification of patients on the basis of sex, age, and procedure.

It is certainly true that PR may act to change the mortality rate within a cohort of patients identified by hospital admission by changing the mix of patients who comprise that cohort—that is, by changing hospital admission policies, and thereby altering the prior probability of death among those admitted. Because it is our intent to evaluate the effect of PR on the mortality rate faced by a patient of a given • type and given hospitalization, this potential effect of PR is less relevant to our analysis of quality change (and may in fact confound it). We have attempted to identify such an effect, and control for it, by standardizing crude mortality rates for these important demographic factors.

## Sample design

The hospital-year sample used for our analyses includes two components:

- All hospitals in 15 States with PR programs having a median length of stay of 15.0 days or less.
- A 25-percent simple random sample of all hospitals in the continental United States, outside of the 15 PR States, with a median length of stay of 15.0 days or less.

Patient outcome measures for each hospital year from 1974 through 1983 were formed by aggregating cases within each hospital year. The size of the sample of hospitals varies from year to year, but it is about 2,300 hospitals, of which slightly more than half are in PR States. The aggregated file across these hospital years includes about 270,000 elective surgery cases in the 15 PR States and about 130,000 cases in other States.

The principal source of our data is the Health Care Financing Administration (HCFA) MEDPAR 20-percent sample file. This file contains data on each hospital stay for all Medicare beneficiaries having social security numbers (Medicare identification numbers) ending in zero or five. This MEDPAR data set includes basic demographics (age, race, sex), principal diagnosis, principal procedure, indications of a second diagnosis or procedure, days of care, days of intensive care, dates of admission and discharge, status at discharge, and charges for routine services and a variety of ancillary services. The HCFA Medicare eligibility files were used to determine the date of death for each patient, if death occurred, for the calculation of mortality rates.

Prior to 1978, most deaths were recorded in the eligibility files as occurring on the last day of the month in which they occurred, since this is the date on which eligibility was terminated. A precise determination of the length of time between hospital admission and the occurrence of death was, therefore, impossible. To address this problem, we assigned all dates of death to the end of the month in which the death occurred. Assuming deaths are approximately uniformly distributed among the dates in each month, it is then possible to estimate mortality rates for any period of time following admission by the number of deaths occurring in that period plus 15 days (see technical note). This estimator permits unbiased comparisons to be made between mortality rates, over any interval, in hospitals that differ with respect to their PR status.

## Statistical methods

Multivariate least-squares regression analysis was employed to investigate the association between PR and standardized mortality. The statistical model expresses the dependent (mortality indicator) variable of interest ($Y$) as a linear function of a vector of exogenous variables including year variables interacted with regions ($DYR$), measurable characteristics of the hospital and the county in which the hospital is located (a vector $X$), and the presence or absence of PR ($DPR$):

$$Y_{ht} = b_0 + b_i DYR_t + b_2 X_{ht} + b_3 DPR_{ht}$$

Variation in $Y$, due to variation in time or in hospital catchment demography, is accounted for by the $DYR$ and $X_{ht}$ elements of the right hand side of the equation. The $DPR$ term, which represents the presence ($DPR = 1$) or absence ($DPR = 0$) of PR, captures the influence of PR, and $b_3$ represents the average difference in the value of $Y$ in year $t$ for hospital $h$, were its status with respect to PR changed. The $t$-statistic associated with $b_3$ is a measure of the confidence with which we can conclude that the difference is not zero. Several approaches are used to specify the influence of PR and are discussed in the following section.

### Practice pattern variables

Several of the exogenous variables used in our analysis represent influences on the cross-sectional or temporal variation in medical practice patterns. Consequently, admission and discharge policies, as well as length-of-stay customs, could be related to these measures. They include:

- NHBPOP: nursing home beds per 100,000 population (in county).
- PHYSPOP: practicing physicians per 100,000 population (in county).
- SPMD: percent of practicing physicians who are not engaged in general practice (in county).
- HMOPOP: percent of population (in county) enrolled in HMO's.
- MAPP: hospital with significant teaching responsibilities, indicated by membership in the Council of Teaching Hospitals (COTH).
- PROF: hospital is investor-owned.
- GOV: hospital is owned by a governmental entity.

In the case of the last three measures, modal values across years are used for each hospital, so that cross-sectional differences are captured without introducing the endogeneity that might arise from any PR influence on hospital mission or ownership.

### Regulatory variables

In addition to PR, many hospitals have been exposed to regulatory constraints on capital expenditures (CON) and physician practice patterns (PSRO). The certificate of need (CON) measure takes a value of 1 for all hospital years following the implementation of a CON statute in the State. The PSRO variable is defined at the hospital level; it assumes a value of 1 for years following the implementation of binding PSRO review in that hospital. No distinction is made for whether the review was delegated by the PSRO to the hospital or not.

### Case-mix variables

Each model employs the standardized outcome as the dependent variable. However, preliminary analysis showed that interhospital differences in severity of case mix might not have been fully captured by this standardization procedure. Analysis of residuals using the ratio of actual to expected rates of outcome showed that there is a systematic influence of the number of admissions on outcome, indicating that hospitals that typically serve more cases may systematically serve more serious cases as well. We therefore included the number of cases in the hospital year of observation as an explanatory variable. A measure of influenza incidence was also included. It is defined as a State/year estimate that was formed using Medicare admissions for influenza and viral pneumonia, formed from the 20-percent MEDPAR file.

### Areawide market variables

A number of measures control for area-specific influences on both the demand for hospital services and the levels and mix of resources hospitals are able to supply. Demographic measures include standard metropolitan statistical area (SMSA) (or non-SMSA) location and county measures of educational attainment, racial composition, Aid to Families with Dependent Children (AFDC) and Medicare enrollments, unemployment rates, population size and density, the consumer price index, and birth rates.

### Temporal and cross-sectional variables

Temporal variation in the dependent variable was, in general, captured by including a dummy variable for each year (except the first), assigned the value 1 in the indicated year and in all subsequent years (0 otherwise). The coefficients of these variables indicate the marginal effect of each year on the dependent variable: that is, the effect in that year over and above the cumulative effects of time up to that year.

**Table 1**
**Features of prospective reimbursement programs of selected States, by State**

| State | Year begun or changed | Locus of authority | Payers included | Participation | Compliance | Negotiation with hospitals | Type of prospective limit |
|---|---|---|---|---|---|---|---|
| Arizona | 1973 | Dept. of Health Health Systems Agency | Blue Cross Commercial Self-pay | Mandatory | Voluntary | Yes | GPSR |
| Colorado | 1972 | Dept. of Health | Medicaid | Mandatory | Mandatory | Yes | Per diem rate |
| Connecticut | 1975 | Commission | Commercial Self-pay | Mandatory | Mandatory | Yes | NPSR |
| Indiana | 1960 | Blue Cross | Blue Cross Commercial Self-pay | Mandatory (for Blue Cross) | Mandatory (for Blue Cross) | Yes | GPSR |
| Kentucky | 1971 | Blue Cross | Blue Cross | Voluntary | Mandatory | Yes | GPSR |
| Maryland | 1974 | Commission | Blue Cross Charge payers | Mandatory | Mandatory | Yes | GPSR |
| | 1977 | | All payers after 1978 | Mandatory | Mandatory | Yes | Per case rates |
| | 1978 | | All payers | Mandatory | Mandatory | Yes | GPSR |
| Massachusetts | 1975 | Commission | Medicaid | Mandatory | Mandatory | No | Per diem rate |
| | 1976 | | Medicaid Commercial Self-pay | Mandatory | Mandatory | No for Medicaid | Medicaid per diem GPSR |
| Minnesota | 1975 | Hospital Association | Blue Cross Commercial Self-pay | Mandatory | Voluntary | Yes | GPSR |
| Nebraska | 1973 ended 1978 | Hospital Association | Blue Cross Commercial Self-pay | Voluntary | Voluntary | Yes | GPSR |
| New Jersey | 1969 | Private agency | Blue Cross Medicaid | Voluntary | Mandatory | Yes | Per diem rates |
| | 1975 | Dept. of Health | | Mandatory | Mandatory | Yes | |
| | 1977 | | | Mandatory | Mandatory | Yes | (tighter criteria) |
| | 1980 | Commission | All payers | Mandatory | Mandatory | No | Per case rates |
| New York | 1971 | Dept. of Health Blue Cross | Blue Cross Medicaid | Mandatory | Mandatory | No | Per diem rates |
| | 1976 | | | Mandatory | Mandatory | | (tighter criteria) |
| | 1978 | | Add self-pay Commercial | Mandatory | Mandatory | No | Per diem rates GPSR |
| Rhode Island | 1975 | Blue Cross State Budget Office | Blue Cross Medicaid Medicare before 1978 | Mandatory | Mandatory | Yes | Statewide GPSR Hospital GPSR |
| Washington | 1976 | Commission | Blue Cross Commercial Self-pay | Mandatory | Mandatory | Yes | GPSR |
| | 1978 ended 1981 | | All payers | Mandatory | Mandatory | Yes | GPSR (by payer for some hospitals) |
| Wisconsin | 1971 | Blue Cross | Blue Cross | Voluntary | Voluntary | Yes | GPSR |
| | 1977 | | Add all except Medicare | Mandatory | Mandatory | Yes | |
| Western Pennsylvania | 1971 | Blue Cross | Blue Cross | Voluntary | Mandatory | Yes | GPSR |
| | 1974 | | Add Medicare | Voluntary | Mandatory | | Per diem rates |
| | 1977 | | Add Medicaid | Voluntary | Mandatory | | |

NOTES: GPSR is gross patient service revenue. NPSR is net patient service revenue.
SOURCE: Abt Associates Inc.: *National Hospital Rate-Setting Study.* Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

Variation in the dependent variable resulting from stable and persistent regional differences in care, not specifically accounted for by any of the areawide variables described above, was captured by the inclusion of a series of dummy variables indicating geographic region (Northeast, South, or West), assigned the value 1 for all States in each region, and 0 otherwise. The coefficients of these variables indicate the marginal effect on the dependent variable or regional locale, relative to location in the North Central region.

The interaction of time and locale was captured by the inclusion of dummy variables assigned the value 1 for all observations occurring in the specified region in the specified year (and 0 otherwise). The coefficients of these variables indicate the marginal effect of time and space, over and above the effect of either alone, on the value of the dependent variable.

**Specification of PR variables**

The impact of PR was evaluated in three ways. The "average" effect of PR was investigated in regressions using a single dummy variable: 1 if a State had a PR program operative in a given observation year, no matter how long it had been operative, 0 otherwise. The coefficient of this variable measures the effect of PR averaged over all years of observation.

In addition to the aggregate PR variable, State-program-specific dummy variables, set equal to 1 for particular States in all years in which PR programs existed (and 0 otherwise), were used to determine the average effect of each State program on outcome. The 15 State programs are summarized in Table 1. In instances when State programs are modified, a separate PR dummy variable is specified for each program variant.

The final specification replaces dummy variables with a continuous variable, representing the estimated percentage reduction in hospital expense due to PR, excluding any indirect effect of PR on expense by means of changes in the volume and composition of hospital services.[1] These estimates were made for each of the State program variants. The variable is expressed as the cumulated percentage saving in expense per case. For example, by the year 1983, the estimated PR-related savings in expense per case across the States were as shown in Table 2.

**Table 2**

**Estimated prospective reimbursement-related cumulated percentage saving in expense per case by the year 1983, for selected States**

| State | Percentage saving |
|---|---|
| Arizona | 8 |
| Colorado | (7) |
| Connecticut | 18 |
| Indiana | 0 |
| Kentucky | 2 |
| Maryland | 24 |
| Massachusetts | 15 |
| Minnesota | 9 |
| Nebraska | (5) |
| New Jersey | 12 |
| New York | 26 |
| Rhode Island | 19 |
| Washington | 3 |
| Wisconsin | 5 |
| Western Pennsylvania | (6) |

NOTE: Numbers in parentheses are negative savings.

SOURCE: Coelen, C., Mennemeyer, S., and Kidder, D.: *Effects of Prospective Reimbursement Programs on Hospital Revenue, Expense and Financial Status.* Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass. Abt Associates, Inc. Dec. 1986.

## Estimation

Weighted least-squares regression was used to estimate the basic models. This was done to remove heteroskedasticity in residuals across hospital-year observations that, if uncorrected, make all parameter estimates inefficient (though unbiased). In our data set, heteroskedasticity results from differences in the numbers of patients in each hospital year of observation.

## Significance levels

In all multivariate analyses, we report coefficients that are significant at least at the $p = .10$ level. We elect to report at this lower-than-usual level of significance for two reasons. First, we are concerned that measurement errors in the MEDPAR file may elevate the standard errors in the model, causing significant program effects to be overlooked if significance levels are too stringent. Second, we believe that the policy applications of this work require that we allow less than the usual chance of committing false negative errors (that is, failing to accept evidence that an adverse program effect exists, even though it truly does, because our threshold for acceptance is too high).

# Descriptive results

Actual and standardized mortality rates for 1983 are summarized in Table 3. Statistics are weighted mean values for all States with and without prospective reimbursement programs. It is clear from Table 3 that absolute mortality rates among patients admitted for elective surgery are relatively low; at 30 days, mortality is about 1 percent, and at 1 year only 9 percent. While these are not very small numbers, they are not very different from baseline mortality rates among the Medicare population. Furthermore, the nearly proportional rise in mortality rate with length of followup suggests that we are in fact observing baseline mortality, and that the mortality associated with the episode of illness motivating elective surgery is, itself, relatively small. In no case is

[1]A discussion of the derivation of these estimated savings by PR program by year is found in Chapter 4 in Coelen, Mennemeyer, and Kidder (1986). The estimates are intended as measures of savings due to improvements in the economic and technical efficiency of hospitals and exclude any augmentation or offsetting of these savings due to changes in length of stay and admissions.

**Table 3**

**Actual mortality rates and standardized mortality ratios for elective surgery, by postoperative period: Selected States, 1983**

| Type of surgery and mortality rate period | Actual mortality rates: Under prospective reimbursement | Actual mortality rates: Not under prospective reimbursement | Standardized mortality ratios: Under prospective reimbursement | Standardized mortality ratios: Not under prospective reimbursement |
|---|---|---|---|---|
| **All elective surgery** | | | | |
| 15 days | .0060 | .0051 | .8572 | .7667 |
| 30 days | .0110 | .0113 | .8523 | .8693 |
| 45 days | .0166 | .0164 | .9028 | .8971 |
| 90 days | .0306 | .0293 | .9547 | .9262 |
| 180 days | .0539 | .0505 | .9832 | .9337 |
| 360 days | .0926 | .0924 | .9638 | .9789 |
| **Inguinal hernia repair** | | | | |
| 15 days | .0037 | .0046 | .7261 | 1.0191 |
| 30 days | .0061 | .0086 | .6845 | 1.1485 |
| 45 days | .0083 | .0117 | .6766 | 1.1647 |
| 90 days | .0166 | .0204 | .8158 | 1.0214 |
| 180 days | .0302 | .0346 | .8735 | 1.0049 |
| 360 days | .0588 | .0660 | .9273 | 1.0263 |
| **Transurethral prostatectomy** | | | | |
| 15 days | .0053 | .0051 | .9873 | .9468 |
| 30 days | .0107 | .0119 | .8460 | .9742 |
| 45 days | .0168 | .0175 | .9196 | 1.0065 |
| 90 days | .0322 | .0334 | .9027 | .9675 |
| 180 days | *.0582 | .0579 | .9322 | .9402 |
| 360 days | .1072 | .1069 | .9317 | .9516 |

*$p < .10$ (two-sided test).

SOURCE: Abt Associates, Inc.: *National Rate-Setting Study.* Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

the difference between standardized mortality rates significant ($p < .10$). In only one instance is the difference in actual mortality rates between PR and non-PR mortality adverse and statistically significant (all cases, 180-day rate).

The trends in actual and standardized mortality for the elective surgery cases are shown in Figure 1. There have been small improvements in actual mortality rate at 30 days between 1974 and 1983. Rates of improvement are similar in PR States and non-PR States. Standardized mortality trends are similar. The plots of standardized mortality are confused somewhat by the apparent discontinuity between 1978 and 1979, almost certainly the effect of changes in diagnostic coding that occurred as a result of the change from ICDA-8 to ICD-9-CM.

## Regression estimates

Multivariate regression analyses were performed using outcome data for all patients admitted for any of the eight elective surgical procedures and for those patients admitted for IHR or TURP. The model was estimated using a single dummy variable for PR, to permit assessment of the average effect of all years of PR, and using State program dummies as described above to assess the average effect of each PR program. For convenience of interpretation, the effects reported in the tables are the products of the coefficients of the PR dummy variable and the ratio of the actual mortality rate among non-PR States in 1983 to the standardized mortality ratio among those States in 1983. These thus represent an estimate of effect of PR on actual mortality rate in 1983.

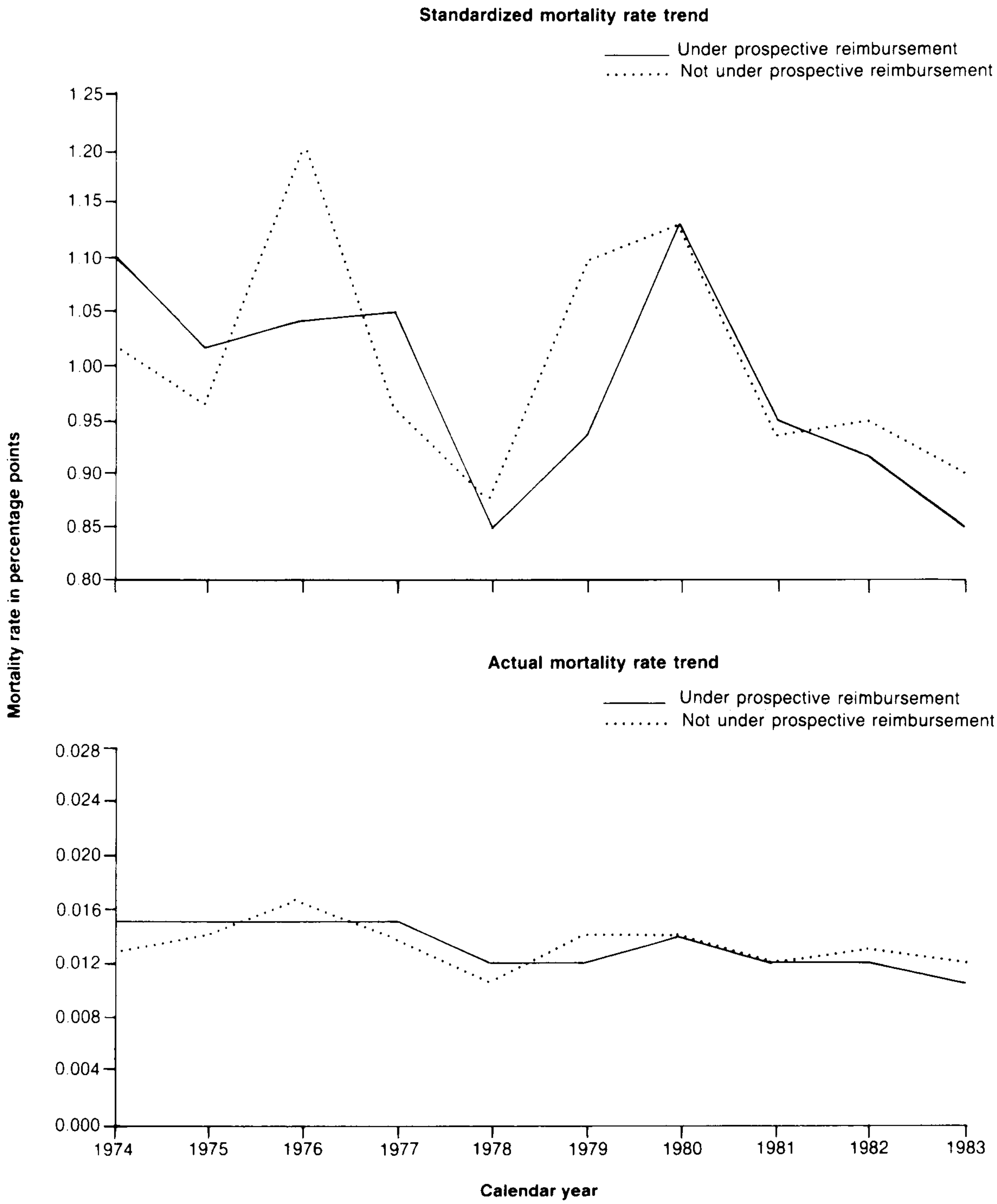
In Table 4, we report our estimates of the average effects of PR. These results suggest a small but significant effect of PR on mortality among all patients admitted for elective surgery. The magnitude of the effect appears to be between 1 and 5 premature deaths per 1,000 hospitalizations; surprisingly, the magnitude of the effect appears to increase over observational time (suggesting an effect of PR on postoperative mortality, manifest increasingly over longer postoperative periods). The physical basis for such an effect is not immediately obvious.

We considered the following hypothesis to explain this result: suppose that PR has two relatively independent and distinct effects on outcome following elective surgery. In particular, suppose that PR has an effect on background mortality rate (perhaps the result of changes in hospital case mix not adequately accounted for by our standardization procedure) as well as on perioperative mortality rate (the results of changes in care occurring around the time of surgery). Conceivably, an effect on the former could obscure a small effect on the latter, and account for larger effects of PR over longer periods of followup.

Preliminary investigation of this hypothesis suggests that it is tenable. We estimate on the basis of this investigation (not shown) that PR affects "episode-

## Figure 1

**Standardized and actual mortality rate trends for elective surgery, 30 days from admission, in States under prospective reimbursement and States not under prospective reimbursement: 1974–83**




SOURCE: Abt Associates, Inc.: *National Hospital Rate-Setting Study*. Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

**Table 4**

**Estimated increases in actual mortality rates for elective surgery due to prospective reimbursement, based on regression models, by mortality rate period**

| Type of surgery and mortality rate period | Average effects |
|---|---|
| **All elective surgery** | |
| 15 days | *.0009 |
| 30 days | *.0010 |
| 45 days | *.0014 |
| 90 days | *.0020 |
| 180 days | **.0040 |
| 360 days | **.0055 |
| **Inguinal hernia repair** | |
| 15 days | -.0003 |
| 30 days | -.0006 |
| 45 days | -.0006 |
| 90 days | -.0007 |
| 180 days | -.0019 |
| 360 days | -.0015 |
| **Transurethral prostatectomy** | |
| 15 days | *.0011 |
| 30 days | .0006 |
| 45 days | .0011 |
| 90 days | .0008 |
| 180 days | .0026 |
| 360 days | .0030 |

*$p < .10$ (two-sided test).
**$p < .05$ (two-sided test).

SOURCE: Abt Associates Inc.: *National Hospital Rate-Setting Study.* Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

related mortality" (as opposed to background mortality) to the extent of approximately 1 excess death per 1,000 admissions for elective surgery; and that that effect is manifest at 15 days, and stable thereafter. This suggests that the effect reported in Table 4, at 15 days, is accurate, but that effects at longer followup intervals are overestimates.

Hernia repair (second panel of Table 4) and prostatectomy (third panel) differ somewhat with respect to the extent to which PR is associated with outcome. Mortality rates following hernia repair in a hospital appear to be no higher, in an average year after the implementation of PR, than would be expected had the hospital not been facing those years of reimbursement control. Mortality rates following TURP, however, appear to be significantly higher at 15 days of observation—consistent with an effect of PR on the quality of perioperative care offered to these patients. This effect is no longer significant at longer follow-up periods—it may be that the relatively small effect of PR does not persist over time.

## Effects of PR in the various States

We next examined the State-program-specific effects of PR on elective surgical outcome using the series of State-program-specific PR dummy variables described earlier. Results for all cases are presented for 15-day, 90-day, and 180-day standardized mortality rates; because we felt that the effect at 15 days more accurately reflected the effect of PR on the quality of perioperative care (see above), we report only the effect at 15 days for IHR and TURP cases. These results are in Table 5.

There is no consistent evidence of increased mortality as a result of PR in Arizona, Connecticut, Indiana, Massachusetts, Minnesota, Nebraska, Rhode Island, Maryland[2], New York, Washington, and Wisconsin. Statistically significant increases in mortality rates are associated with the implementation of PR programs in Colorado, Kentucky, New Jersey, Western Pennsylvania (1977 program). The magnitude of this effect, at 15 days of followup, was as large as 3.1 premature deaths per 1,000 patients (Western Pennsylvania, 1977).

Programs appeared to have differentiable effects on care of IHR and TURP cases: that is, in some PR States, outcome of one but not the other group was affected by PR.

## Relationship to cost saving

As can be seen in Table 4 there appears to be no simple association between cost saving and excess mortality; consider the programs in Rhode Island (costs 20 percent less than expected, with an insignificant estimate of 0.5 excess deaths per 1,000), Western Pennsylvania (costs 2 percent greater, with excess mortality highly significant), and Connecticut (costs 8 percent less, with no excess mortality).

Regressions of standardized mortality ratios on a cost-saving variable confirm the lack of association between cost savings and mortality effects for elective surgery as well. These results appear in Table 6. In no case do we find an association between the level of savings produced by PR and the standarized mortality for elective surgery for 15- to 180-day periods. Only in one case (TURP) do we find a significant relationship, which occurs at the 360-day period, but not earlier. Although this general absence of effects remains a surprising finding, it reinforces the need for further investigation into the program elements that are common in those States in which outcome deteriorated after PR was introduced.

# Discussion

Examination of postadmission mortality rates for patients admitted for elective surgery does not yield consistent conclusions on the issue of quality impacts of State PR programs. On average, hospitals in PR States were unable to achieve the same mortality reductions as were observed in hospitals not covered by PR: at 15 days after admission we find an increase in relative mortality of about 1 death per 1,000 admissions. This effect is also observed for TURP but not for hernia repair. A few of the single State programs provide some evidence of consistent increases in relative 15-day mortality rates across the

[2]The early PR program in Maryland (1975-77) was not reliably tested due to data problems in 1974 and 1975 data.

## Table 5

**Estimated program-specific increases in mortality rates for elective surgery due to prospective reimbursement, based on regression analyses, by mortality rate period and State**

| State program | Estimated cost savings as the result of prospective reimbursement[1] | All elective procedures | | | Inguinal herniorrhaphy | Transurethral prostatectomy |
|---|---|---|---|---|---|---|
| | | 15 days | 90 days | 180 days | 15 days | 15 days |
| Arizona | 8 | .0008 | .0027 | .0036 | .0003 | *.0035 |
| Colorado | -7 | .0018 | *.0064 | *.0083 | -.0014 | *.0034 |
| Connecticut | 18 | .0000 | -.0034 | -.0048 | .0005 | **-.0029 |
| Indiana | 0 | .0011 | .0007 | .0014 | **.0037 | -.0002 |
| Kentucky | 2 | *.0022 | *.0054 | **.0110 | -.0018 | .0023 |
| Maryland | 24 | | | | | |
| 1978 | | .0012 | .0017 | .0003 | .0001 | .0023 |
| Massachusetts | 15 | | | | | |
| 1975 | — | -.0011 | .0071 | .0092 | -.0027 | .0002 |
| 1976 | — | -.0001 | -.0007 | -.0007 | .0005 | -.0018 |
| 1983 | — | .0028 | -.0027 | -.0043 | -.0006 | -.0038 |
| Minnesota | 9 | -.0009 | .0006 | .0010 | **-.0041 | .0018 |
| Nebraska | -5 | .0032 | .0035 | .0044 | -.0055 | -.0024 |
| New Jersey | 12 | | | | | |
| 1975 | — | .0013 | .0016 | -.0008 | -.0005 | -.0018 |
| 1977 | — | .0003 | .0011 | .0029 | -.0012 | -.0010 |
| 1980 | — | .0007 | .0023 | *.0066 | *.0027 | .0003 |
| New York | 26 | | | | | |
| 1971 | — | -.0001 | -.0014 | .0016 | -.0035 | -.0004 |
| 1976 | — | -.0007 | -.0025 | -.0008 | -.0003 | -.0026 |
| 1978 | — | .0011 | .0001 | .0019 | *.0028 | -.0013 |
| 1983 | — | .0004 | -.0011 | .0003 | -.0002 | -.0005 |
| Pennsylvania (Western) | -6 | | | | | |
| 1974 | — | .0014 | .0036 | .0072 | .0005 | .0042 |
| 1977 | — | **.0025 | .0022 | *.0054 | **.0039 | .0016 |
| Rhode Island | 19 | .0005 | .0008 | .0053 | .0030 | .0007 |
| Washington | 3 | | | | | |
| 1976 | — | .0002 | .0005 | -.0005 | -.0012 | .0019 |
| 1978 | — | -.0004 | .0028 | *.0087 | **-.0076 | *.0043 |
| Wisconsin | 5 | | | | | |
| 1971 | — | -.0006 | .0009 | -.0038 | -.0013 | -.0020 |
| 1977 | — | .0014 | .0015 | .0005 | .0009 | .0010 |

*$p < .10$ (two-sided test)
**$p < .05$ (two-sided test)
[1]Cumulative savings (expressed as a percent) in expense per hospital admission, because of changes in efficiency, as a result of prospective reimbursement. See Coelen, Mennemeyer, and Kidder (1986), Table 5.5.

SOURCE: Abt Associates Inc.: *National Hospital Rate-Setting Study.* Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

**Table 6**

**Regression coefficients relating standardized mortality to estimated prospective reimbursement cost savings, by mortality rate period**

| Mortality rate period | Regression coefficient on cost-saving prospective reimbursement specification | | |
|---|---|---|---|
| | All elective surgery | Transurethral prostatectomy | Inguinal herniorrhaphy |
| 15 days | −.2098 | −.7892 | −.5319 |
| 30 days | −.0207 | −.0906 | −.7428 |
| 45 days | .0245 | .1575 | −4315 |
| 90 days | −.2000 | −.2962 | .1071 |
| 180 days | −.0989 | .1449 | −.3922 |
| 360 days | −.0955 | *.3549 | −.4728 |

*$p < .10$ (two-sided test).

SOURCE: Abt Associates Inc.: *National Hospital Rate-Setting Study*. Contract No. 500-78-0036. Prepared for the Health Care Financing Administration. Cambridge, Mass.

samples we study; these PR programs included Colorado, Kentucky, New Jersey, and Western Pennsylvania (1977-83). While hospitals in PR States have, despite significant cost savings, been able to make improvements in outcomes, these findings about relative increases in mortality are important, though not definitive indications of quality of care effects.

There are two aspects of the statistical findings that fail to corroborate a relationship between PR and mortality that is indicative of a quality of care effect. First, as seen in Table 4, the "average" PR effect sizes tend to increase over longer follow-up periods. However, as seen in Table 5, these patterns also occasionally appear as significant effects in State PR models for longer follow-up periods. It is not at all clear what sorts of quality changes could be occurring during, before, and shortly after surgery to generate such a pattern of results. We suspect that this pattern could result from differential trends in severity (prognosis) between PR and non-PR admissions that were not captured by our standardization technique. We did use other types of statistical models, and the pattern of average PR effects remained about the same in terms of direction, magnitude, and significance of PR effects. (Models tested included log forms, inclusion of a base year dependent variable as a covariate, various maturation approaches, and models that included expected mortality as a covariate.) Our inability to resolve this anomaly leads us to question whether the observed effects are related to quality or to alterations in surgical practice patterns that may be affected by PR.

The second issue relates to the pattern of results that relate PR program stringency to mortality. We are unable to confirm any association between the stringency of hospital cost containment and elective surgical mortality. Our direct statistical tests show no association between cost savings and 15- to 180-day mortality; only in one model (TURP, 360-day mortality) was there any signficant positive relationship.

The pattern of State programs effects on mortality is also inconsistent with expectations regarding stringency. Several programs that have achieved considerable savings are not associated with higher relative mortality (Connecticut, Maryland, Massachusetts, Minnesota, New York, and Rhode Island); and some programs that have not reduced hospital spending are associated with adverse mortality effects (Western Pennsylvania, Colorado, and Kentucky). These patterns suggest that if PR programs are having detrimental effects on elective surgical mortality, such effects are not to be considered as automatic consequences of cost containment; hospital administrators in some States have achieved considerable savings in resource use without increasing relative mortality.

Based on these findings, we conclude that PR programs may be increasing elective surgical mortality. However, patterns of effects across States and across follow-up periods fail to provide evidence of plausible mechanisms that might link PR programs and their cost-containment objectives to quality of care compromises. Because of the policy significance of this issue, these findings should prompt more research on the relationships and linkages between hospital cost containment, administrative actions, practice patterns, and patient outcomes.

# Technical note

## Estimation of mortality rates

As noted in the article, there are systematic differences in the recording of the date of death in HCFA's eligibility file between the years before 1978 and those after. In particular, date of death was generally recorded before 1978 as of the last day of the month in which the death occurred. Subsequently, deaths were, for the most part, recorded at the actual date of occurrence, although recording at the end of the month still occurred. This made accurate determination of the time elapsed between hospitalization (date obtained from the MEDPAR file) and the date of death (obtained from the eligibility file) impossible. Further, it introduced potential bias in estimating PR effects, since PR tends to be associated with later years, in which date of death would usually be recorded accurately rather than at the (later) end-of-month date.

To address this problem, we assigned all deaths, both before and after 1978, to the end of the month. This standardized the assignment procedure both within and across years, and eliminated the possible PR versus non-PR bias. So that the mortality rates more correctly reflect the true survival times, we estimated the death rate for "$x$" days from hospitalization as the observed death rate for "$x+15$" days from hospitalization, where the latter is based on dates of death recorded as of the end of the month.

The rationale for this is as follows:

- Assume month "*m*" has 30 days, and that the actual date of death for patient "*i*" is day "*d*" of month "*m*." Assign that death to the last day of month "*m*" (since *m* has 30 days, that assignment will be the 30th of "*m*"). The difference between the actual date of death "*d*" and the assigned date of death ("the 30th") will be, therefore, "30-*d*" days.
- Assuming deaths are randomly distributed across days of the month, the average value of "30-*d*" over all days "*d*" will be:

$$\frac{1}{30}\sum_{d=1}^{30}(30\text{-}d) = 14.5$$

  If "*m*" has 31 days, that average will be 15.0; if "*m*" has 28 days, the average is 13.5.
- The average value of (30-*d*) across all months of the year is the weighted average of these 3 values, the weights being based on the number of months with 30, 31, and 28 days. Thus, the average value is given by:

$$(4/12)(14.5) + (7/12)(15.0) + (1/12)(13.5) = 14.71 \cong 15$$

  (In leap years, the average is 14.75.)
- Recording date of death at the end of month thus adds an average of about 15 days to each person's survival time. To estimate the mortality rate at "$x$" days from admission, we consequently use the observed rate at "$x + 15$" days from admission.

The estimated rates will tend to underestimate the true rate slightly. This results from the estimated rates being based, not on the observed rate for a single time period, but on observed rates for a range of time periods, symmetrically distributed about the indicated period. For example, the estimated 90-day rate is based on rates ranging from 75 to 105 days ($90 \pm 15$), for months having 30 days. Because the cumulative mortality curve is concave downward, the average rates for a range of time periods is less than the rate for the average of the length of the time periods.

This underestimation of true rates applies to PR and non-PR hospitals. If PR results in higher mortality rates during the hospitalization, the cumulative mortality curve would be less concave, and the underestimation of true mortality rates thus would be less for PR hospitals, resulting in an upward bias in the estimate of the PR effect.

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