Studying the Effects of the DRG-Based Prospective Payment System on Quality of Care: Design, Sampling, and Fieldwork

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We have conducted a nationally representative before-after study of the effects of the diagnosis related groups–based prospective payment system (PPS) on quality of in-hospital care for aged Medicare patients. We used a pre-post design with multiple time points in both the pre-PPS (calendar years 1981 and 1982) and post-PPS (July 1985 through June 1986) periods. We gathered clinically detailed data from medical records of patients with one of six diseases and supplemented these data with postdischarge information from Health Care Financing Administration files. We used a stratified multistage cluster sampling design with data gathered on 16,758 patients chosen from 297 hospitals in 30 areas in five states. Our hospital participation rate was 97%; we successfully accessed 96% of the medical records we requested; and our mean item-level reliability score was 0.80. Our sample matches the nation closely on hospital urbanicity, size, teaching status, ownership, and percentaged of Medicare and Medicaid patients, and patient demographics and mortality.

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IN 1983, THE Health Care Financing Administration (HCFA) changed the way hospitals were reimbursed for treating patients under the Medicare program. Prior to 1983, hospitals received payment for all services provided, subject to appropriateness review. Since 1983, under the prospective payment system (PPS), hospitals have been paid an amount based largely on flat rates per admission calculated for each of approximately 470 diagnosis related groups. Because the new payment system contains incentive to decrease length of stay and substitute lower-cost services and procedures, concern has arisen that the quality of health care may have declined.

Since 1985, we have been conducting a national study to investigate the effects of the PPS on quality of care for hospitalized Medicare patients. Other articles in this series present our findings. In this article, we summarize our design and sampling decisions, give details on the fieldwork involved in gathering our primary data, and present results on the composition and national representativeness of our final sample.

METHODS

Choice of Treatment, Control Groups, and Study Years

The PPS was not introduced in 1983 as a controlled experiment. Instead, before October 1983, hospitals were reimbursed for treating Medicare patients under the old retrospective payment system, and during the year from October 1983 to September 1984 nearly all acute care general hospitals were phased into prospective reimbursement. The exceptions were hospitals in the waiver states—Maryland, Massachusetts, New Jersey, and New York—where reimbursement alternatives to the PPS were used until 1986.

Because the PPS was introduced in...
this nonexperimental way, a prospective controlled trial evaluating its effects on quality of care was not possible. We instead designed a retrospective observational study, in which we contrasts data on Medicare patients prior to and subsequent to 1983. We considered supplementing such data with pre- and post-PFS information from a control group (eg, patients from the waiver states, non-Medicare patients aged 55 to 64 years in the PPS states, or patients from another country such as Canada), but funding limitations prevented this.

Because pre-post designs may be confounded by secular trends over time, we gathered data at multiple time points in both the pre- and post-PFS periods, so that such trends might be at least roughly estimated. We report only differences pre- and post-PFS in the middle five articles in this series and discuss issues of trend estimation and causality in the final article in the series. We chose as our pre-PFS sampling window the period from January 1, 1981, to December 31, 1982, and as our post-PFS window the period from July 1, 1985, to June 30, 1986. Our design concentrated 50% of the sampling in each of the pre- and post-PFS periods: 20% in 1981, 30% in 1982, and 25% in each of 1985 and 1986.

Our design was longitudinal at the hospital level and cross-sectional at the patient level; ie, we gathered data at each of our sampled hospitals in all study years, with different patient cohorts sampled in each time period within the chosen hospitals. The longitudinal nature of the hospital sampling increased the accuracy of the pre-post comparison by holding the hospital factor constant.

We have elsewhere advocated the measurement of quality of care in a disease-specific way. However, instrumentation costs limit the number of diseases that may be studied in detail. After consultation with an expert panel, we selected six diseases for study: congestive heart failure, acute myocardial infarction, pneumonia, cerebrovascular accident, hip fracture, and depression. We discuss the extent to which our findings from these six diseases generalize to other Medicare patients in another article in this series.

Summary of Sampling Plan

We used a stratified, multistage cluster sampling plan with four levels of sampling hierarchy: states, areas within states, hospitals within areas, and patients within hospitals. We oversampled hospitals treating Medicare patients and chose approximately the same number of patients from each hospital in a way that produced patient-level national representativeness. In our sampling design we chose five states, with four to eight areas per state for a total of 12 states nationwide; six to 18 hospitals per area for a total of approximately 60 hospitals per state and 300 hospitals overall; and about 17 patients per hospital for a total of approximately 17,000 patients.

Choice of States.—We selected our final states purposively, subject to eligibility criteria and stratification goals. Eligibility considerations excluded the waiver states and states with either too few hospitals or a mixture of hospitals that was either too urban or too rural. The main stratification goal in the choice of states was geographic diversity, with one state from each region of the country. Our final sample included California, Texas, Indiana, Pennsylvania, and Florida.

Choice of Areas.—Photocopying sampled medical records and sending the copies to a central location in the chosen states for abstraction would have been desirable on cost grounds, but the photocopy quality of microfilmed records, which made up a non-trivial portion of the pre-PFS sample, was too poor to permit this option. Given the resulting cost restrictions on data collector travel, the only feasible sampling plan involved dividing the sampled states into geographic areas and clustering the chosen hospitals in a sample of these areas.

We used geographic diversity within state and six hospital-level factors as stratification variables in our final area choice: urbanity, percentages of Medicare and Medicaid patients, size, teaching status, and hospital ownership (eg, proprietary vs nonprofit). We considered a large number of designs, each with a total of 20 to 30 areas, by conducting a computer-aided search among all possible choices of four to eight areas in each of our five states. Our final choice was purposive and had 30 areas, with four to eight areas per state.

Choice of Hospitals.—We based the hospital-level sampling frame on the 1984 HCFA Provider of Services file. We used three eligibility criteria at the hospital level. First, we restricted sampling to short-term acute care facilities and excluded veterans' military, and psychiatric hospitals. Second, we also restricted attention to hospitals that were in existence during the entire period from 1981 to 1986. Third, a small number of hospitals in our chosen states had 15 or fewer patients per year with one or more of our six study diseases (based on HCFA's 1984 MedPAR file) and were judged to be too small.

The final hospitals were chosen by defining a stratification grid indexed by size, urbanicity, and hospital poverty status. We defined "high-poverty hospitals" as those facilities whose percentage of Medicare patients was at or above the 90th percentile of the Medicaid distribution in the state in which the hospital was located. Our goal was representativeness with respect to size and urbanicity and an oversampling by a factor of 2 of both high-poverty hospitals and city-county facilities. Hospitals were then chosen by a restricted randomization procedure that maximized representativeness with respect to ownership, teaching intensity, and percentage of Medicare patients, while varying the number of hospitals per area from 6 to 18 in such a way that our stratification targets were achieved. In this manner, 30 hospitals were chosen.

Choice of Patients.—We based the patient-level sampling frame on lists, generated by the chosen hospitals, of all patients hospitalized in each study year with one of the study diseases, as indicated by International Classification of Diseases, Ninth Revision, Clinical Modification (ICD-9-CM) codes (Table 1). Twelve hospitals were unable to provide such lists; we built the patient sampling frames for those hospitals from HCFA's MedPAR file. We chose simple random samples of patients in the cells of a three-by-six stratification grid indexed by study period and disease. The goal was balance across all diseases, with nine or 10 patients chosen from each disease category in each hospital, distributed within each disease across study periods in approximate proportions of 20% (1981), 30% (1982), and 50% (1985 and 1986).

To meet our goal of 17,000 total patients, we examined a somewhat larger number of records because some records were unavailable for study and others did not meet our clinical inclusion criteria (see below). In total, 22,725 records were requested from the participating hospitals.

Data Collector Training, Abstraction, and Monitoring

Nurses and medical records personnel who were experienced with clinical data were selected by the professional review organizations in the chosen states to become data collectors. After demonstrating adequate skills, 52 data collectors participated in training that lasted 17 days, with at least 2 days focused exclusively on the study of each of our six diseases. During the period of data collection,
we monitored abstractors with a series of interrater reliability studies (see below) and gave feedback to data collectors who were identified as having problems. A project manager who was familiar with the hospital in which the abstraction took place supervised the work. In addition, each completed abstraction form was reviewed by both a physician and a nurse for an average of 30 minutes per record to assess internal consistency and to assure that coding was consistent with supporting clinical data. Certain types of clinical information from the medical record (eg, the exact words characterizing a patient's stroke if one occurred) were written verbatim into the abstraction form, and specified pieces of data (eg, chest roentgenogram reports, admission histories and physicals, and discharge summaries) were photocopied and attached to the form. Discrepancies that could not be resolved during the review process were returned for reabstraction. Physicians interpreted photocopies of roentgenogram reports and some electrocardiogram tracings and reports.

To maintain the confidentiality of hospitals, patients, and physicians, coded identifiers were assigned to hospitals and patients. No identifying information about physicians was obtained.

Each disease-specific abstraction form contained approximately 700 items grouped into five categories: inclusion criteria, demographics and sickness at admission, explicit information about the processes of care given to the patient, patient outcomes, and patient status at discharge. Examples include physician documentation of preadmission symptoms of myocardial infarction, frequency of nurses' blood pressure readings on hospital day 2 (congestive heart failure), prehospital mental status (pneumonia), use of coumarin at any time during hospitalization (cerebrovascular accident), and number of physical therapy sessions on postoperative day 1 (hip fracture). More details about data elements can be found in the other articles in this series.21 The abstraction forms took an average of 90 minutes per record to complete.

**Interrater Reliabilities**

We assessed interrater reliabilities for each disease using both records that the data collectors knew to be test cases and records that they did not know were being monitored. We used $\kappa$ scores to measure how much the agreement between different readings of the same medical record exceeded chance.

**Inclusion Criteria**

We used inclusion criteria to assure a homogeneous group of patients with the chosen diseases in the pre- and post-PPS periods. To be included in one of our disease-specific samples, patients had to be at least 65 years of age, admitted during one of our study years, and hospitalized for the indicated disease. Patients with the study disease as a complication of hospitalization, rather than as a reason for admission, were not eligible for the sample.

**Secondary Data Collection**

In addition to the clinical primary data previously described, we also collected secondary data of two types: (1) information about postdischarge outcomes that was merged with the records in our primary database in order to examine the effects of the PPS on outcomes during the year following hospitalization, and (2) national data on mortality trends in our study diseases from 1980 to 1986 with which to compare our sample data for validation purposes.

We compiled three kinds of postdischarge outcome information: mortality, hospital readmissions, and nursing home stays. To obtain postdischarge mortality data, we used patients' last names, first names, dates of birth, and health insurance claim numbers from the medical record to match our sampled patients with the corresponding records in HCFA's health insurance master file.22

To validate our sampling with respect to mortality, we obtained data from HCFA's MedPAR file on all patients hospitalized with our five nonpsychiatric diseases (n = 2,062,610) in the 27-quarter period from January 1986 to September 1986. Variables we extracted from the MedPAR file included admission status within 30 days of admission, gender, age, and diagnosis related group. We established a correspondence between our ICD-9-CM-based disease definitions (Table 1) and diagnosis related groups and used these data to calculate age- and sex-specific and age- and sex-adjusted 30-day death rates for each of our study diseases, and in the aggregate across our study diseases, in each of the 27 quarters.

We then used the postdischarge mortality data on our sampled patients to compute age- and sex-adjusted quarterly sample mortality rates in the 30-day period following admission, by disease and in the aggregate across diseases. To compare these data with the national MedPAR mortality data previously described, we plotted the national age- and sex-adjusted 30-day quarterly mortality series on the same graph with the quarterly 30-day mortality data from our study sample and marked off error bars of 2 SEs either way from the observed sample mortality series. We estimate that noncomparabilities between national and sample mortality values arising from our use of inclusion criteria were small.

**Effects of Sampling Plan on Analysis**

The sampling plan we employed had four features requiring special attention during the analysis:

1. Our oversampling of patients from hospitals serving an unusually large fraction of Medicaid patients would yield somewhat biased raw findings if the quality of care in these facilities differed substantially from that in other hospitals. To arrive at nationally representative findings, it was necessary to reweight our raw patient-level results, giving less weight to facilities serving an unusually large fraction of Medicaid patients. The weighted and unweighted results differed little in most of our major analyses. In what follows in this and other articles in this series, we present unweighted findings unless otherwise indicated.

2. The clustering of our sampled patients in only five states, 30 areas, and 297 hospitals has implications for the accuracy of our results. If there is more similarity on average between two patients in the same state, area, or hospital in the care they receive than between two patients in different states, areas, or hospitals, and no adjustment for this intraclass correlation is made, the result will be an overstatement of the precision of our findings. To
adjust for clustering and to produce SEs that accurately reflect the information content of our sample, we (1) calculated provisional SEs for all of our major estimates, as if we had gathered our data using simple random sampling, (2) computed "inflation factors" based on intracluster correlations that measured the amount of information in our sample relative to that obtained in a simple random sample of the same size, and (3) multiplied our provisional SEs by the inflation factors, thereby adjusting the significance of our results downward. The inflation factors ranged from 1.1 for outcomes such as in-hospital and 30-day death to 2.9 for our composite score, aggregating across diseases, that summarizes the quality of the processes of in-hospital care. The latter inflation factor, which is based on an unusually high intracluster correlation of .16, indicates a remarkable degree of homogeneity within hospitals in the processes of care rendered.

3. The longitudinal nature of the hospital sampling, in which we gathered data from all of our study hospitals in both the pre- and post-PPS periods, also had implications for the accuracy of our results. Holding the hospital factor constant acted on the precision of our findings in a manner opposite to that of the clustering; the latter decreased accuracy, whereas with independent simple random sampling in each of the pre- and post-PPS periods, the former increased accuracy because of positive correlations over time in the patterns of care given to patients within hospitals. We computed "deflation factors" to adjust simple random sampling SEs for the longitudinal hospital sampling and found that the clustering and longitudinal effects approximately canceled each other in our analyses. Thus, we were able to analyze the data essentially as if they had been gathered with simple random sampling in a cross-sectional fashion.

4. We present results in this series of articles, both at the disease-specific level and in the aggregate across diseases. Two issues arise in producing across-disease summaries: reweighting the diseases back to their actual frequencies in the Medicare population, and properly accounting for the degree to which pre-post differences themselves differ by disease (i.e., accounting for interactions between the pre-post and disease factors). Reweighting by disease prevalence is potentially necessary because we took samples of roughly equal size in each of our six study diseases, even though congestive heart failure is more than three times more frequent among Medicare patients than hip fracture, for example.

We computed weights necessary to reweight our raw findings back to the population of all Medicare patients with one of our six study diseases and did sensitivity analyses to see how much this reweighting affected our results. In all of the cases we examined involving our major study findings, the weighted and unweighted results differed little. We therefore report unweighted findings unless otherwise noted. Regarding pre-post by disease interactions, we tried not to emphasize across-disease summaries when the interactions were large (i.e., when there was serious disagreement among our diseases in the size of the difference pre- and post-PPS), and we tried to conservatively report the significance of the pre-post PPS effect, when the interactions were small to moderate, by adjusting the pre-post PPS significance in a manner consistent with the size of the interaction.20

RESULTS

Sampling

Almost all of the 300 selected hospitals agreed to participate in the study. Five hospitals refused and were replaced by hospitals in the same area with similar hospital and patient characteristics. Three other hospitals also refused to participate and were not replaced, resulting in a final sample of 297 hospitals and a hospital-level participation rate of 97% (297/305).

Our final sample had 51 to 62 patients per hospital (except for 12 small hospitals, where patients per hospital ranged from 17 to 50), with an average of approximately 57 patients per hospital and a total of 16,758 patients, slightly fewer than our target of 17,000.

Hospitals were able to find the medi-
cal records we requested in almost all cases. Of the 22,786 medical records requested, 570 (3.8% of the requested records) were not available for review, leaving 21,216 total records reviewed.

The number that were not available was higher in 1981 (6.6% of those requested) and 1982 (5.0%) than in 1986 and 1987 (1.7%). The numbers of records unavailable were sufficiently small that any bias arising from their unavailability would be too small to significantly change the study's major findings.

Of the 21,216 records reviewed, 5,107 (24%) were excluded (Table 2). The fraction of records excluded in the post-PPS period (21%) was significantly lower than the fraction excluded in the pre-PPS period (26%; P<.01). Table 3 gives the most frequent causes of exclusion. The most common reason was that clinically detailed review of the medical record showed the patient did not have symptoms or signs of the disease suggested by the hospital's assignment of ICD-9-CM codes. For example, 17% of the patients with a congestive heart failure code on their medical records did not have signs of congestive heart failure at admission, ie, they did not have either chest roentgenogram evidence for heart failure or leg edema at admission.

We were able to obtain accurately merged postdischarge mortality information from the HCFA's files on 92% of our sample (2,821 of 14,012 patients).

Interrater Reliabilities

For 10 reliability records known by the data collectors to be test cases, each rated by an average of 47 different data collectors, we found across-disease item-level χ scores averaging 0.66. For 192 different records rated by two different data collectors who did not know they were being monitored, we obtained across-disease item-level χ scores averaging 0.78. Overall, the 10th percentile of the distribution of our χ scores was 0.63, the 90th percentile of our χ scores was 1.0; 5.6% of our χ scores were below 0.4 (a level generally recognized as signifying poor reliability), and 88% were above 0.75 (a level implying excellent reliability).

Effects Detectable With the Study's Sampling Resources

Table 4 gives examples of the differences from pre-to post-PPS that were

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Table 4.—Detectable Pre-Post Differences: Process* and Outcome

<table>
<thead>
<tr>
<th>Disease</th>
<th>Pre-PPS† Rate, %</th>
<th>Pre-Post Change Detectable With 80% Power, Percentage Points</th>
<th>SE for Estimated Pre-Post Difference, Percentage Points</th>
</tr>
</thead>
<tbody>
<tr>
<td>Congestive heart failure</td>
<td>14.7</td>
<td>3.4</td>
<td>1.3</td>
</tr>
<tr>
<td>Acute myocardial infarction</td>
<td>24.4</td>
<td>4.2</td>
<td>1.5</td>
</tr>
<tr>
<td>Pneumonia</td>
<td>15.9</td>
<td>3.6</td>
<td>1.3</td>
</tr>
<tr>
<td>Cerebrovascular accident</td>
<td>21.3</td>
<td>4.0</td>
<td>1.4</td>
</tr>
<tr>
<td>Hip fracture</td>
<td>5.1</td>
<td>2.1</td>
<td>0.8</td>
</tr>
<tr>
<td>Aggregating across diseases</td>
<td>16.7</td>
<td>1.9</td>
<td>0.6</td>
</tr>
</tbody>
</table>

*Process: the study's power to find 1.1-point or greater disease-specific pre-post differences on a 100-point process scale with a patient-level SD of 10 was at least 80%. In the aggregate across diseases, 0.5-point or larger differences on such a scale were detectable with at least 80% power.
†PPS indicates prospective payment system.

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Table 5.—Characteristics of the Study Sample by Disease and Time Period*

<table>
<thead>
<tr>
<th>Study Disease†</th>
<th>CHF Pre</th>
<th>CHF Post</th>
<th>AMI Pre</th>
<th>AMI Post</th>
<th>PNE Pre</th>
<th>PNE Post</th>
<th>CVA Pre</th>
<th>CVA Post</th>
<th>HIP Pre</th>
<th>HIP Post</th>
<th>Total Pre</th>
<th>Total Post</th>
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<tbody>
<tr>
<td>≥80 y, %</td>
<td>41</td>
<td>41</td>
<td>45</td>
<td>45</td>
<td>53</td>
<td>53</td>
<td>51</td>
<td>51</td>
<td>45</td>
<td>44</td>
<td>41</td>
<td>41</td>
</tr>
<tr>
<td>Male, %</td>
<td>43</td>
<td>43</td>
<td>45</td>
<td>45</td>
<td>53</td>
<td>53</td>
<td>51</td>
<td>51</td>
<td>45</td>
<td>44</td>
<td>41</td>
<td>41</td>
</tr>
<tr>
<td>Nonwhite, %</td>
<td>23</td>
<td>23</td>
<td>22</td>
<td>22</td>
<td>18</td>
<td>18</td>
<td>19</td>
<td>19</td>
<td>19</td>
<td>19</td>
<td>14</td>
<td>14</td>
</tr>
<tr>
<td>Medicaid, %</td>
<td>16</td>
<td>15</td>
<td>12</td>
<td>12</td>
<td>12</td>
<td>12</td>
<td>19</td>
<td>19</td>
<td>14</td>
<td>14</td>
<td>13</td>
<td>13</td>
</tr>
<tr>
<td>Preadmission residence at nursing home, %</td>
<td>8</td>
<td>9</td>
<td>4</td>
<td>5</td>
<td>22</td>
<td>22</td>
<td>20</td>
<td>20</td>
<td>22</td>
<td>21</td>
<td>12</td>
<td>12</td>
</tr>
<tr>
<td>Sample size</td>
<td>1359</td>
<td>1465</td>
<td>1416</td>
<td>1437</td>
<td>1341</td>
<td>1408</td>
<td>1382</td>
<td>1442</td>
<td>1358</td>
<td>1404</td>
<td>6856</td>
<td>7156</td>
</tr>
</tbody>
</table>

*The results were reweighted to achieve national representativeness.
†CHF indicates congestive heart failure; AMI, acute myocardial infarction; PNE, pneumonia; CVA, cerebrovascular accident; and HIP, hip fracture.
‡Difference pre- vs post-PPS is significant (P<.01).

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detectable with the study's resources. It can be seen from this table that small disease-specific and aggregate differences in the average quality of processes of care, and a modest difference in aggregate mortality across all five diseases, stood a high chance of being found, while disease-specific differences in mortality would have had to have been fairly large to be detectable with high likelihood. The estimates in the table are typical of the power conclusions to be drawn about the many quantitative and qualitative response variables in our study; our data resources were sufficient to find small disease-specific differences for continuous outcomes and moderate differences aggregating across diseases for dichotomous outcomes.

Effects smaller than those referred to in Table 4 as detectable with 80% power may actually be found to be significant by the study, as can be seen from the column giving SEs for the estimated differences pre- and post-PPS in 30-day mortality, adjusted for sickness at admission.

Representatives of Final Sample

After reweighting, our sample matched the nation closely with respect to hospital size, urbanicity, percent of Medicare and Medicaid patients, teaching intensity, and ownership. With each of these variables divided into three to six categories, the largest discrepancy between national and reweighted sample prevalences in any category was less than 2 percentage points, and national and reweighted sample means and SDs for these variables (when relevant) agreed to within less than 1%. The Figure compares our sampled age- and sex-adjusted 30-day mortality values with known national values on a quarterly basis, in the aggregate across diseases; the results were similar at the disease-specific level. The national values fell within the sample 95% confidence limits in 11 of the 12 quarters, and the pattern of positive and negative deviations of the sampled values from the national mortality figures revealed no bias. Both of these observations are consistent with the hypothesis that our sample is representative of the nation with respect to 30-day mortality.

Composition of Final Patient Sample

Table 5 gives various characteristics of the study's patient sample, both before and after the PPS, by disease and in the aggregate across diseases. Demographics remained stable from pre- to post-PPS, but there was some change in the fraction of patients admitted from a nursing home; this percentage rose from 22% to 26% pre- to post-PPS for pneumonia patients but fell by the same amount (from 24% to 20%) for hip fracture patients (P<.01). In sickness at admission following the introduction of the prospective payment system. JAMA. 1990; 264:1962-1968.


