The Effect of HMOs on the Inpatient Utilization of Medicare Beneficiaries

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Objective. To determine the effect of joining HMOs (health maintenance organizations) on the inpatient utilization of Medicare beneficiaries.

Data Sources. We linked enrollment data on Medicare beneficiaries to patient discharge data from the California Office of Statewide Health Planning and Development (OSHPD) for 1991–1995.

Design and Sample. A quasi-experimental design comparing inpatient utilization before and after switching from fee-for-service (FFS) to Medicare HMOs; with comparison groups of continuous FFS and HMO beneficiaries to adjust for aging and secular trends. The sample consisted of 124,111 Medicare beneficiaries who switched from FFS to HMOs in 1992 and 1993, and random samples of 108,966 continuous FFS beneficiaries and 18,276 continuous HMO enrollees yielding 1,227,105 person-year observations over five years.

Main Outcomes Measure. Total inpatient days per thousand per year.

Principal Findings. When beneficiaries joined a group/staff HMO, their total days per year were 18 percent lower (95 percent confidence interval, 15–22 percent) than if the beneficiaries had remained in FFS. Total days per year were reduced less for beneficiaries joining an IPA (independent practice association) HMO (11 percent; 95 percent confidence interval, 4–19 percent). Medicare group/staff and IPA-model HMO enrollees had roughly 60 percent of the inpatient days per thousand beneficiaries in 1995 as did FFS beneficiaries (976 and 928 versus 1,679 days per thousand, respectively). In the group/staff model HMOs, our analysis suggests that managed care practices accounted for 214 days of this difference, and the remaining 489 days (70 percent) were due to favorable selection. In IPA HMOs, managed care practices appear to account for only 115 days, with 636 days (85 percent) due to selection.

Conclusions. Through the mid-nineties, Medicare HMOs in California were able to reduce inpatient utilization beyond that attributable to the high level of favorable selection, but the reduction varied by type of HMO.

Key Words. Medicare, risk HMO, inpatient utilization, selection

Over the past two decades, Congress has directed the Medicare program to foster the growth of managed care by offering generous payments to health maintenance organizations (HMOs) (General Accounting Office 1999). Health maintenance organizations in the Medicare+Choice program now cover 14 percent (July 2003) of the Medicare population but the question of whether HMOs can successfully adapt their managed care practices to an older and sicker population remains unanswered.

Lower costs in Medicare HMOs have been attributed primarily to favorable selection (Miller and Luft 1994; Riley et al. 1996; Cox and Hogan 1997; Morgan et al. 1997; Hamilton 1999; Thiede Call et al. 1999; Riley, Lubitz, and Rabey 1991). Since Medicare HMOs receive capitated payments that vary minimally with enrollee health status, they have an incentive to enroll and retain the healthiest beneficiaries, a process known as risk selection (Newhouse, Buntin, and Chapman 1997). Health maintenance organizations can also lower costs through managed care practices that alter the delivery of care and by negotiating lower prices with providers.

Research on HMO practice patterns in younger populations has shown reductions of 0 to 35 percent in inpatient utilization between HMO enrollees and FFS (fee-for-service) beneficiaries (Miller and Luft 1994; Luft 1987; Manning et al. 1984; Miller and Luft. 1997; Weinick and Cohen 2000). However, the literature is not consistent in reporting whether HMOs achieve lower utilization by reducing admissions or length of stay (Miller and Luft 1994; Luft 1987; Manning et al. 1984; Miller and Luft. 1997; Congressional Budget Office 1995; Glied 2000.)

Studies on the effects of HMOs on the inpatient utilization of the Medicare population are limited (Kasper et al. 1988; Langwell and Hadley 1989; Hill et al. 1992; Congressional Budget Office 1997; Physician Payment Review Commission 1996b). The Medicare Tax Equity and Fiscal Responsibility Act (TEFRA) Evaluation (Hill et al. 1992) surveyed a national sample of 12,000 FFS and HMO Medicare beneficiaries, which was too small to estimate differences in inpatient utilization precisely. The estimated 17 percent reduction in hospital days observed in Medicare HMOs after controlling for selection was not statistically significant. A recent study using panel data from the Medicare Current Beneficiary Survey (MCBS) from 1993 to 1996 found

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much larger reductions in inpatient days after controlling for selection. Inpatient claims are filed selectively by HMOs, resulting in missing claims, which may inflate the differences between FFS and HMO use (Mello, Stearns, and Norton 2002).

A more developed body of literature has consistently reported that Medicare HMOs experience substantial favorable selection at enrollment (Miller and Luft 1994; Riley et al. 1996; Cox and Hogan 1997; Morgan et al. 1997; Hamilton 1999; Thiede Call et al. 1999; Riley, Lubitz, and Rabey 1991). These studies suggest Medicare HMO enrollees use approximately 20–40 percent fewer resources in the 6 to 12 months prior to enrollment. Studies on selection at disenrollment have found that disenrollees are generally sicker than continuous FFS beneficiaries or members who remain enrolled (Cox and Hogan 1997; Morgan et al. 1997; Riley, Lubitz, and Rabey 1991).

The dominance of research on HMO selection rather than the effect of HMOs on resource use is due largely to data availability. Medicare routinely collects data on the utilization of services by FFS beneficiaries for payment purposes. However, similar data on HMO enrollees have not been available, making it difficult to examine what happens to Medicare beneficiaries after they join an HMO.

We have generated a unique database on the utilization of inpatient services for all Medicare HMO and FFS beneficiaries in California from 1991 to 1995. These data on utilization before, during, and after enrollment into HMOs enable us to look into the "black box" of service utilization in HMOs.

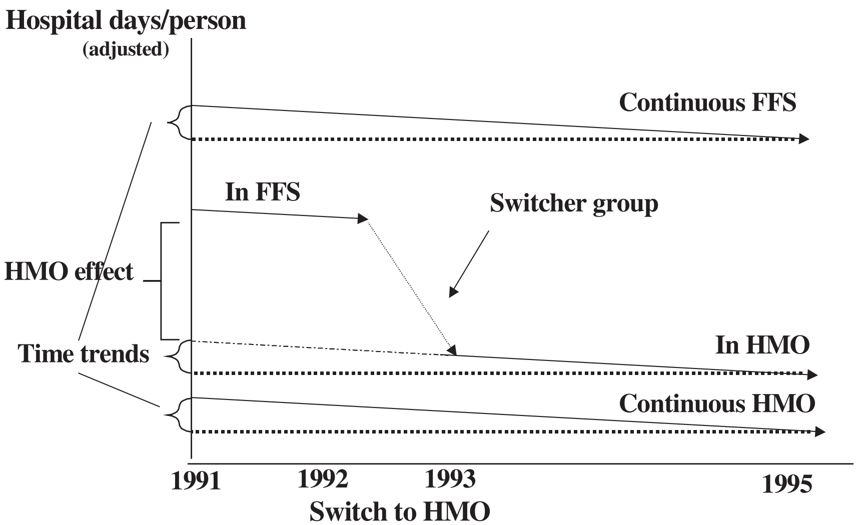
METHODS

Study Design Overview

We used a "before and after with a comparison group" design to evaluate the impact of Medicare HMOs on hospital inpatient utilization (Cook and Campbell 1979). To estimate the HMO effect on inpatient utilization, we examined beneficiaries who started in FFS and switched to an HMO. We compared use in FFS before switching to use in HMOs after switching to estimate the managed care or HMO effect.

Because changes in utilization in this switcher group could also be due to other factors (e.g., aging, death, or secular trends), we included two comparison groups in our study to adjust for these factors: (1) beneficiaries continuously enrolled in FFS and (2) beneficiaries continuously enrolled in HMOs. We compare the difference in utilization before and after HMO enrollment

Figure 1: Hospital Days before and after Switching to an HMO



for switchers, to the differences in utilization in the comparison groups in that time (see Figure 1). The differences-in-differences design meant we did not have to adjust for any selection differences directly. Nevertheless, we estimated an analytic model that includes sociodemographic characteristics and measures of health status for two reasons. First, there is a general interest in the effects of these measures on inpatient utilization. Second, our study design assumed that decisions to enroll and disenroll were not related to *changes* in health status and that the underlying health status of HMO enrollees changed at the same rate over time as that of people who stayed in FFS (Morgan et al. 1997; Physician Payment Review Commission 1996a). To control for the possibility that changes in health occur at different rates in these groups, we included time-varying measures of health in the model. We tested these model assumptions by estimating and comparing two models: (1) with (the "full" model) and (2) without (the "no predictors" model) the health and sociodemographic variables. Given that we made valid assumptions, the estimate of the HMO effect on utilization should be the same for each model.

After using our restricted analytic sample to determine the effects of being in an HMO on hospital days, we went to the larger California sample. The overall observed difference in hospital days per member was then split between that due to "managed care practices" (the estimated effect of being in an HMO), and the remainder due to selection.

Data Linkage

The data for this study were derived from linking Medicare enrollment data on all beneficiaries in California between 1991 and 1995 from the CMS (Centers for Medicare and Medicaid Services) Denominator files to inpatient discharge data for short-term stays from the California Office of Statewide Health Planning and Development (OSHPD). All nonfederal hospitals in California submit discharge records to the state agency irrespective of payer source. Records were linked using social security number, zip code of residence, date of birth, gender, and race. The Medicare enrollment file was the primary file used for the linkages. Discharges that were coded as Medicare in the OSPHD data without corresponding links in the enrollment files were excluded (<5 percent). These included beneficiaries who moved out of state during the study period. If they had discharges during the study period, we were able to link them based on the unique patient identifier in the OSPHD data, the Record Linkage Number (RLN). The linkages were performed by OSHPD under IRB (Internal Review Board) approvals from the California Department of Health and Welfare, CMS Data User Agreements between all parties with access to the confidential data, and the RAND Committee for Protection of Human Subjects. The linked data were returned to RAND after all potential identifiers were stripped. Medicare patients who had at least one admission during the study period had greater than a 90 percent probability of being matched. The CMS plan contract numbers from our data were linked to the Monthly Report on Medicare Coordinated Care Health Plans from CMS to obtain data on model type.

Sample Selection

We excluded from the study beneficiaries who met any of the following criteria: (1) death in 1991 or in 1992, (2) had ESRD (end-stage renal disease), (3) did not have both Part A and B coverage, (4) resided in counties with fewer than 500 HMO enrollees in 1991, (5) were enrolled in a cost-reimbursed HMO, or (6) were less than 65 years old. Because we differentiated HMOs into group/staff and independent practice association (IPA) model type, the 1.5 percent of HMO Medicare beneficiaries who could not be matched to a specific Medicare risk HMO were dropped (Gabel 1997). The analytic sample used to compute the HMO effect consisted of three groups: (1) The switcher group included all beneficiaries who were enrolled in FFS for all of 1991 and continued in FFS until they switched to an HMO during 1992 or 1993; (2) a random sample of beneficiaries who remained in FFS from 1991 through 1995 or until their death in 1994–1995; and (3) a random sample of HMO members who were continuously enrolled from 1991 through 1995 or until their death in 1994–1995.[1] To estimate the magnitude of selection, we used data on all California beneficiaries in 1995 after applying the same six exclusion criteria used to derive our analytic sample.

Model Specification

Our unit of analysis was the person-year. We used a two-part model to analyze differences in an individual's total inpatient days per year based on theory and statistics (Duan et al. 1983; Diehr et al. 1999). A technical appendix with the rationale and statistical tests underlying model specification, retransformation, validation, and computing standard errors of the combined model can be found at http://www.rand.org/publications/WR/WR138. The first part of the model was a logistic regression of the probability of at least one inpatient day in a year. The second part was an ordinary least squares regression of the natural logarithm of total hospital days per year given at least one day in the hospital. The focus on days meant that the 0.2 percent of admissions with zero length of stay were excluded.

We used the same set of predictor variables in both parts of the model. Because our data covered a five-year period, we have up to five observations per person and each person-year was treated as an independent observation. For both parts of the model we estimated robust standard errors based on Huber clustering corrections by HMO to account for correlation among the results for the different HMOs (Huber 1967).

While hospital use involves both admission and length of stay decisions, we were ultimately concerned with the resulting effect of being in an HMO on total inpatient days. Overall days were the product of the predicted probability of at least one hospital day and the predicted total number of days given a stay of at least one day. Before these predictions could be combined, the second prediction had to be retransformed from the logarithmic scale to the original scale (log days to days). All statistical calculations were performed using *Stata 7.0* (StataCorp 2001).

Independent Variables

The key independent variable representing the effect of Medicare HMOs on inpatient utilization was defined as the proportion of time spent in an HMO each year. Because beneficiaries can enroll and disenroll from Medicare HMOs each month, our measure of the HMO effect captures partial-year enrollment. For example, a beneficiary switching from FFS to an HMO on July 1, 1992, and remaining in the HMO for the rest of the year would have a value of 0 for the HMO effect variable in 1991, a value of 6/12 or 0.5 in 1992 to reflect six months of enrollment, and a value of 1 for the subsequent years (if they remain alive).[2] For all years, the value of the HMO effect variable for enrollees in the continuous HMO comparison groups was set to 1, and to 0 for the beneficiaries in the continuous FFS comparison group. Because this variable changed only for those who switched, its estimated coefficient represented the effect of being in an HMO after controlling for selection and time trends in the model.

We grouped control variables associated with inpatient use into four categories: (1) enrollment history group, (2) health status, (3) sociodemographic characteristics, and (4) year.

To control for selection, we split HMO enrollees into two groups based on their enrollment history: (1) continuous HMO—those who were in a group/staff HMO or IPA HMO from the beginning to the end of the study period or until death; (2) switchers—those who switched from FFS into a group/staff HMO or IPA HMO in 1992 or 1993. The switchers were then split into those that remained in their respective HMO type until the end of the study period or death (switch and stay) and those who disenrolled (from either HMO type) back into FFS (switch and disenroll). Disenrollees who switched into another HMO were treated as switchers who stayed in an HMO. Switchers with more than one HMO enrollment separated by a spell in FFS during the study period were excluded. The third group in our study consisted of those who remained in FFS continuously from the beginning until the end of the study or their death. The characteristics of beneficiaries across each enrollment history group are presented in Table 1.

The enrollment history groups were dummy variables that did not change over time. For example, consider those who switched to an IPA HMO in 1992 or 1993 and stayed there. Their indicator variable "switch to IPA HMO and stay" was set equal to one in every year. The coefficients on these variables were meant to represent each group's average use relative to those that remained continuously enrolled in FFS during the study period after controlling for the HMO effect on inpatient use. Thus, these variables controlled for selection differences and were estimates of their relative magnitude among the enrollment history groups.

The second category of variables provided controls for differences in health status. The "time-to-future-death" measures indicated whether and when a beneficiary died during the study period. Given the steady increase in

Table 1: Descriptive Statistics of Model Variables in Selected Years for the Enrollment History Groups*

	Enrollment History Groups				
	1992–1993 HMO Switchers		*Continuous HMO*		
Model Variables	*Group/Staff*	*IPA*	*Group/Staff*	*IPA*	*Continuous FFS*
Dependent Variables (Mean)					
Admissions/beneficiary					
1991	.18	.14	.16	.14	.23
1995	.29	.25	.24	.22	.33
Total inpatient d/y					
1991	1.05	.82	.75	.71	1.49
1995	1.41	1.17	1.05	.92	1.96
Part I: Probability of one stay of at least one d/y					
1991	.12	.11	.11	.11	.15
1995	.18	.16	.15	.15	.20
Part II: Log (inpatient d/y given one stay of at least one day)					
1991	1.73	1.64	1.52	1.50	1.83
1995	1.62	1.54	1.50	1.37	1.80
Independent Variables (Mean)					
Proportion of year in HMO (HMO Effect)					
1991	0	0	1	1	0
1995	.92†	.94†	1	1	0
Other Independent Variables (Percentage in 1991)‡					
Male	43.2	42.3	43.7	41.8	39.5
African American	7.9	2.8	7.9	1.6	4.1
Medicaid-eligible	11.3	4.9	5.3	2.7	18.9
Disabled, >64 years	7.4	6.2	5.9	5.7	7.2
Age, years					
65–69	33.0	30.2	31.9	25.5	25.1
70–74	28.6	30.4	30.5	31.3	27.8
75–79	19.3	20.6	20.1	22.2	21.0
80–84	11.6	11.6	10.8	12.7	13.9
85+	7.5	7.2	6.7	8.3	12.2
Death from ’93 through ’96	18.5	16.5	17.5	17.4	24.7
No. of Beneficiaries in 1991	39,383	84,728	10,861	7,415	108,966

*HMO indicates health maintenance organization and FFS indicates fee-for-service.

†Less than 100 percent because of disenrollment to FFS. Over the course of the study 8.6 percent of the beneficiaries who joined either a group/staff or IPA HMO in 1992 or 1993 disenrolled to FFS. This variable is 0 for those who disenrolled before 1995 and between 0 and 1 for those disenrolling in 1995.

‡The number of beneficiaries will change over time due to deaths and therefore so will the percentage. All percentages refer to 1991 except death.

use of inpatient services near death, we categorized deaths by splitting the time they occurred relative to the current year into four periods: death in the first half or second half of the current year, and death in first half or second half of the following year (Lubitz, Beebe, and Baker 1995; Lubitz and Riley 1993; Lubitz and Prihoda 1984).

Disability status as determined by the original reason for Medicare entitlement was also used to control for differences in health status in this age-65-and-over study sample.

Sociodemographic variables included race (African American or not), gender, age category, and Medicaid eligibility. Beneficiaries were deemed Medicaid-eligible if they were covered by Medicaid during any part of 1991. Lastly, we included dummy variables for each year to control for secular changes in utilization over the five-year study period.

Estimating Selection

Our study design estimated the differences in use between our enrollment history groups (due to selection) after adjusting for the HMO effect. However, because our analytic samples excluded some beneficiaries (e.g., those turning 65 after 1991, multiple switches between FFS and HMO), these estimates do not represent all Medicare beneficiaries. To derive an estimate of selection more representative of the entire population of California Medicare beneficiaries, we applied the same six general exclusions required of the analytic sample as described earlier to all beneficiaries in California.[3] In addition, we had to drop 1.5 percent of the HMO beneficiaries because we could not match them to a specific Medicare risk HMO. We then computed total inpatient days per thousand beneficiaries per year for this larger sample of California beneficiaries by group/staff HMO, IPA HMO, and FFS. Because Medicare beneficiaries may change entitlement (due to death), or enroll and disenroll from HMOs on a monthly basis, we calculated monthly utilization rates and then aggregated these measures over 12 months by calendar year. We then estimated the selection effect by subtracting our model estimate of the HMO effect from the difference between our calculation of the total HMO and the total FFS inpatient days per thousand beneficiaries, attributing any remaining difference between HMO and FFS to selection.

RESULTS

Our sample for estimating the HMO effect consisted of 251,353 Medicare beneficiaries yielding 1,227,105 person-year observations during 1991 to

1995. The initial sample sizes of all groups of interest are shown at the bottom of Table 1 along with descriptive statistics on utilization and the independent variables in the two-part model. In Table 1, the enrollment history group variables are shown as columns to allow readers to compare the different groups of beneficiaries (e.g., 1992–1993 IPA HMO switchers, and continuous FFS beneficiaries) with respect to important model variables.

Hospital utilization increased for each enrollment history group between 1991 and 1995 as expected given their aging, and was the greatest for the continuous FFS group in all years. For the 1992–1993 switcher group, the proportion of time in an HMO was zero in 1991, and 0.92 and 0.94 in 1995 for group/staff versus IPA HMO switchers, respectively. Among the other enrollment history groups, values of time in an HMO were always zero or one.

Approximately 24.7 percent of the continuous FFS group died between 1993 and 1996 compared to 16.5–18.5 percent of the 1992–1993 HMO switchers and 17.4–17.5 percent of the continuous HMO group. The other predictor variables (e.g., disabled status and age) observed in 1991 also indicated generally poorer health status among FFS beneficiaries compared to HMO enrollees. African American beneficiaries tended to join group/staff HMOs. A larger number of FFS beneficiaries were eligible for Medicaid at the start of the study compared to any HMO enrollees.

Total Inpatient Days per Year

Beneficiaries enrolled in group/staff HMOs used 82 percent of the inpatient days they would have used, had they remained in FFS (95 percent confidence interval, 79–86 percent), while enrollees in IPA HMOs used 89 percent (95 percent confidence interval, 82–97 percent) (Table 2, third column).

The enrollment history group variables revealed that enrollees in both types of HMOs were consistently healthier than the FFS beneficiaries. On average, enrollees in group/staff HMOs who stayed used 73 percent of the inpatient days of continuous FFS beneficiaries after adjusting for the HMO effect and all other covariates compared to the IPA HMO enrollees who used only 64 percent (Table 2, third column). These findings suggest that IPA HMOs experience greater favorable selection than group/staff models. The smaller group of HMO switchers who disenrolled to FFS before the end of the study period would have had 25 percent more inpatient days in FFS than those who remained in FFS. These measures provided estimates of selection while controlling for managed care effects.

Table 2: Determinants of Total Inpatient Days per Year, 1991–1995*

Independent Variables	*Reference Category*	*Full Model*	*No-Predictors Model*
		% Total Days per Year Relative to Reference (95% CI)†	
HMO Effect			
Proportion of year in group/staff HMO	Continuous FFS	82 (79, 86)	80 (77, 83)
Proportion of year in IPA HMO		89 (82, 97)	88 (82, 94)
Enrollment History Groups	Continuous FFS		
Group/Staff HMO		73 (71, 76)	63 (61, 66)
IPA HMO		64 (60, 67)	52 (49, 55)
Switch to HMO and Disenroll to FFS		125 (118, 133)	128 (117, 140)
Year	1991		
1992		97 (95, 98)	109 (107, 111)
1993		95 (91, 98)	137 (131, 144)
1994		90 (86, 95)	153 (147, 159)
1995		80 (76, 84)	138 (130, 146)
Male	Female	121 (118, 124)	—
African American	Non-African American	120 (112, 129)	—
Medicaid-eligible in 1991	Not eligible	151 (145, 157)	—
Disabled >64 years	Other>64years	168 (163, 172)	—
Age, years	Age 65–69 years		
70–74		119 (116, 122)	—
75–79		141 (138, 144)	—
80–84		162 (158, 167)	—
85+		161 (150, 173)	—
Death from 1993 through 1996	Alive		
Death in first half of current year		429 (388, 475)	—
Death in second half of current year		743 (698, 791)	—
Death in first half subsequent year		447 (420, 476)	—
Death in second half subsequent year		265 (251, 280)	—
Goodness of fit (Efron's R^2)		6.7	0.8
Number of Observations		1,227,105	1,227,105

*HMO indicates health maintenance organization and FFS indicates fee-for-service.

†Results are based on a two-part model where the first part is a logistic regression of whether the beneficiary had one or more days in the hospital in a year, and the second part is an ordinary least squares regression of the natural log of total hospital days per year given at least one day. Log days are retransformed to days using the smearing estimate. CI indicates confidence intervals, which are based on standard errors adjusted for clustering by HMO. Confidence intervals assume log normality.

In the no-predictors model, the use of the group/staff HMO enrollees and IPA HMO enrollees was even lower at 63 percent and 52 percent of FFS days, respectively. The no-predictors model estimated larger differences in use (due to selection) between the HMO and FFS groups because it did not control for other risk factors such as age and death rates, which were higher among FFS beneficiaries. In the no-predictors model, aging and death in the cohort cause utilization to rise as shown by the increases (up to 53 percent in 1994) for later years relative to 1991. However, after controlling for age and death in the full model, utilization fell steadily over the years of the study.

The effects of sociodemographic characteristics on inpatient utilization were consistent with our expectations and the literature. Men used 21 percent more inpatient days per year than women, and African American beneficiaries used 20 percent more total inpatient days per year than non-African American beneficiaries. Medicare beneficiaries eligible for Medicaid used 51 percent more inpatient days per year compared to those not eligible. Disabled beneficiaries used 68 percent more total days per year than nondisabled beneficiaries of the same age. Finally, inpatient use increased steadily with age over 65, but leveled off for the very oldest (85 years and older). Death during the study period had a considerable effect on utilization. For example, beneficiaries who died in the first half of a given year used 429 percent more days in the same year than beneficiaries who remained alive during the entire study period. Those who died in the second half of a given year had even greater utilization in that year. Utilization in a given year for those who would die in the subsequent year was much higher than that of beneficiaries who remained alive through the end of that year.

All the results in Table 2, and in particular, the 18 percent and 11 percent reduction by group/staff versus IPA model type, came from combining the statistical results of each part of the two-part model. These detailed regression results are shown in Table 3. The coefficients in the top row show that the reductions were largely the result of a decrease in length of stay.[4]

We validated the decrease in days per year from our regression model by computing differences in length of stay per admission between HMO and FFS. We computed the average length of stay (ALOS) by DRG (diagnosis related groups) for HMO switchers in 1994 and standardized it to the distribution of DRG admissions in FFS. The adjusted HMO ALOS (5.9 days) in our sample in 1994 was 16 percent lower than the actual ALOS (7.0 days) for FFS in 1994.

Table 3: Two-Part Full and No-Predictors Models of Inpatient Utilization, California Medicare Beneficiaries 1991–1995*

		Full Model		*No-Predictors Model*	
		Part I: Probability Any Days	*Part II: Log Days per Year*	*Part I: Probability Any Days*	*Part II: Log Days per Year*
Independent Variables	*Reference Group*	*Odds Ratio (95% CI)*	*Coefficient (95% CI)*	*Odds Ratio (95% CI)*	*Coefficient (95% CI)*
HMO Effect	FFS				
Proportion of year in group/staff HMO		1.00 (.960, 1.05)	−.157 (−.186, −.128)	.977 (.946, 1.01)	−.161 (−.183, −.139)
Proportion of year in IPA HMO		1.07 (.984, 1.16)	−.124 (−.176, −.072)	1.04 (.981, 1.11)	−.123 (−.166, −.080)
Enrollment History Groups	Continuous FFS				
Group/staff HMO		.804 (.772, .838)	−.104 (−.116, −.091)	.729 (.695, .764)	−.141 (−.161, −.121)
IPA HMO		.714 (.670, .762)	−.148 (−.167, −.130)	.628 (.593, .666)	−.208 (−.233, −.182)
Switch to HMO and Disenroll		1.19 (1.13, 1.26)	.068 (.022, .114)	1.21 (1.11, 1.31)	.068 (−.004, .132)
Year	1991				
1992		.990 (.975, 1.01)	−.037 (−.046, −.027)	1.08 (1.05, 1.10)	.004 (−.005, .012)
1993		.894 (.853, .938)	−.026 (−.038, −.013)	1.18 (1.11, 1.25)	.108 (.096, .119)
1994		.982 (.935, 1.03)	.109 (−.141, −.077)	1.44 (1.38, 1.51)	.066 (.048, .083)
1995		.987 (.942, 1.03)	.195 (−.232, −.158)	1.50 (1.41, 1.59)	−.011 (−.042, .020)
Male	Female	1.28 (1.25, 1.31)	−.012 (−.035, −.011)	—	—
African American	Non-African American	1.04 (.970, 1.11)	.118 (.070, .166)	—	—
Medicaid-eligible in 1991	Not eligible	1.378 (1.33, 1.42)	.146 (.114, .178)	—	—
Disabled > 64 years	Other > 64 years	1.66 (1.63, 1.69)	.123 (.103, .144)	—	—
Age, years	Age 65–69				
70–74		1.19 (1.16, 1.121)	.060 (.038, .083)	—	—
75–79		1.43 (1.40, 1.45)	.106 (.090, .121)	—	—
80–84		1.70 (1.66, 1.73)	.138 (.116, .160)	—	—
85+		1.78 (1.62, 1.96)	.117 (.081, .153)	—	—

continued

Table 3. *Continued*

		Full Model		No-Predictors Model	
		Part I: Probability Any Days	*Part II: Log Days per Year*	*Part I: Probability Any Days*	*Part II: Log Days per Year*
Independent Variables	*Reference Group*	*Odds Ratio (95% CI)*	*Coefficient (95% CI)*	*Odds Ratio (95% CI)*	*Coefficient (95% CI)*
Death from 1993 through 1996	Alive				
Death in first half of current year		5.78 (4.81, 6.96)	.360 (.307, .412)	—	—
Death in second half of current year		10.47 (8.81, 12.42)	.617 (.599, .635)	—	—
Death in first half of next year		3.97 (3.57, 4.41)	.552 (.529, .575)	—	—
Death in second half of next year		2.58 (2.44, 2.68)	.314 (.281, .347)	—	—
Goodness of fit		Pseudo $R^2 = .069$	$R^2 = .077$	Pseudo $R^2 = .011$	$R^2 = .025$
Number of Observations		1,227,105	188,979	1,227,105	188,979

*CI indicates confidence intervals, which are based on standard errors adjusted for clustering. HMO indicates health maintenance organization and FFS indicates fee-for-service.

Selection Effect

We estimated the selection effect based on the utilization of all Medicare FFS and HMO beneficiaries in 1995 after applying the six general exclusions outlined above plus a small percentage of HMO beneficiaries that were unable to be matched to a specific HMO ($N = 2{,}616{,}942$). Medicare FFS beneficiaries in California used 1,679 days per thousand beneficiaries, while group/staff HMO enrollees used 976 days per thousand enrollees, and IPA HMO enrollees used 928 per thousand enrollees yielding differences of 703 and 751 days per thousand between FFS and group/staff and IPA HMOs, respectively. Based on our estimate of the HMO effect for group/staff enrollees, HMO enrollment accounted for 214 days of this difference ($[\{1/0.82\}-1] \times [976\ \text{days}/1000]$). In other words, switchers would have used 214 additional days had they remained in FFS. Selection accounted for the remaining 489 days per thousand (703–214 days) or 70 percent (489/703) of the lower use with 30 percent attributed to managed care practice. Similarly, 115 days of the difference between IPA HMO enrollees and FFS beneficiaries ($[\{1/0.89\} - 1] \times [928\ \text{days}/1000]$) can be attributed to HMO enrollment, with selection accounting for the remaining 636 days per thousand (751–115 days). In the case of IPA HMOs, more of the difference in use is attributed to selection (85 percent [636/751]).

COMMENT

This study examined the effect of being in a Medicare HMO on inpatient utilization. A unique database constructed by linking Medicare records with California hospital discharge data over five years allowed us to measure inpatient utilization after people switched to HMOs. We also calculated the difference in use between all California Medicare FFS and HMO beneficiaries and apportioned it to an HMO effect and a selection effect. Previously, researchers had no way of estimating the effects of being in a Medicare HMO and had to estimate selection in HMOs based on data before enrollment and after disenrollment. Our data on enrollees before, during, and after they enrolled in Medicare HMOs, provide new evidence on the extent of selection and the impact of joining an HMO on inpatient use.

Our results confirm prior studies showing substantial favorable selection in HMOs, which accounts for most of the difference in inpatient use but it varies by type of HMO. Beneficiaries in group/staff HMOs were less healthy than beneficiaries in IPA HMOs. We also report new estimates on the effect of enrolling in HMOs on inpatient use. Our findings also suggest that after adjustment for population differences, Medicare beneficiaries in HMOs used significantly fewer inpatient days than they would have used had they remained in FFS. Our analytic sample included over a million observations allowing us to calculate fairly precise and robust estimates of the reduction in hospital days due to joining an HMO with the reduction ranging from 11 percent for IPAs to 18 percent for group/staff HMOs. The differences in impact by type of HMO are consistent with the expectation that HMOs with greater utilization control protocols such as group/staff HMOs can be more successful in reducing utilization than IPA HMOs (Welch, Hillman, and Pauly 1990; Hillman, Welch, and Pauly 1992; Miller and Luft 1993).

Surprisingly, the reduction in inpatient days is due entirely to reduced length of stay. Medicare HMOs' large effect on length of stay is unexpected for many reasons. First, the Medicare program has paid hospitals a fixed amount per admission using DRGs since 1983, which has contributed to substantially reduced lengths of stay (Carter and Melnick 1990). This payment method contains a strong incentive for hospitals to lower length of stay, since any savings accrue directly to the hospital. Second, the consensus in the literature on favorable selection in Medicare HMOs challenges the idea that these HMOs can reduce hospital days even further among a healthier population. Finally, California with its history of Medicare and non-Medicare managed care, has long stood out as having the lowest hospital utilization rates in the country (Zwanziger, Melnick, and Bamezai 2000). Thus, in an already lean system, Medicare HMOs might not have been successful at extracting additional reductions in inpatient utilization. However, because many California HMOs pay hospitals on a per diem basis, they do have incentives to reduce days per year.

The Medicare program and CMS have struggled with a wide range of implementation issues surrounding HMOs including payment methods, payment rates, and HMO withdrawals from the program that have forced Medicare beneficiaries to involuntarily switch plans or return to FFS. These problems have made Medicare HMOs increasingly controversial and have reduced support for Medicare HMOs, particularly since most believe that Medicare HMOs increase rather than decrease total Medicare program costs each year (General Accounting Office 1997, 1999, 2000). The implementation of risk-adjusted capitation payments by CMS to Medicare HMOs addresses the issue of favorable selection, and could potentially reduce funds available to subsidize the additional benefits that Medicare HMOs have used to attract members. Thus, the ability of HMOs to maintain and expand membership may depend largely on their ability to generate cost savings.

Although the reduction in inpatient days is substantial, the actual net cost savings may be somewhat less. Because the reduction in inpatient days is due to reduced length of stay, the net inpatient hospital cost-savings to Medicare HMOs are likely to be smaller than the estimated reduction in utilization. The marginal cost of an additional day in the hospital is less than the average cost (Carter and Melnick 1990). It is also plausible that managed care practices involve more intensive treatment per day resulting in additional limits on cost-savings. Moreover, shorter lengths of stay may lead to higher use of post-acute care services.

This study has several limitations. First, our state database does not include federal hospitals, or hospitalizations occurring outside the state. However, because it relies on within-person comparisons, this gap will bias the results only to the extent that use of Veteran's Administration or out-of-state hospitals is systematically affected by joining an HMO. Second, it is based on data from a single state, California. Since almost 40 percent of all Medicare risk enrollment was in California by 1995, our findings are significant for policymakers, but it is unclear whether the experience of California Medicare beneficiaries generalizes to the rest of the country (Zarabozo, Taylor, and Hicks 1996). Third, our findings do not cover noninpatient use. High-quality outpatient data for most HMO and FFS beneficiaries that is needed to quantify the reduction in overall costs is not yet available. However, HMOs have historically achieved their savings primarily through reductions in inpatient use (Miller and Luft 2002). Moreover, results of the Medicare TEFRA evaluation showed no increase in home health services and only slightly higher outpatient use for HMOs (Hill et al. 1992). Finally, this study does not address the extent to which reduced inpatient use from Medicare HMOs affects health outcomes, and in particular outcomes for vulnerable populations (e.g., those age 85 and over, African Americans, those with chronic disease).

The last two limitations point to important areas for further research. First, data on outpatient, pharmacy, and home health use would provide a more complete picture of the potential cost savings by Medicare HMOs. While the impact of Medicare HMOs on total costs is itself important, a fair judgment of the program must also consider the impact on patient outcomes, especially those patients who may be less desirable to Medicare HMOs. Investigating use and outcomes for such patients and determining whether reduced hospital days are the result of increased efficiency or decreased quality is a critical next step.

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Notes

1. We eliminated persons dying before January 1994 for comparability to the HMO switchers who had to remain alive until the end of the HMO enrollment period in the study.
2. Because the models are nonlinear, using the average time of enrollment for the transition years could lead to bias. As a sensitivity analysis, we omitted all transition years to make the "in HMO" variable dichotomous. The estimated reduction was 1 percent greater with a standard error that was 10 percent larger than when transition years were included.
3. In this case, beneficiaries residing in counties with fewer than 500 HMO enrollees in 1995 (as opposed to 1991) were excluded.
4. Because the mean number of admissions per year for beneficiaries with an admission is very similar across the enrollment groups (1.5 in 1991, falling to 1.4 in 1995), differences in days per year with an admission are proportional to differences in length of stay, and we will use the more familiar term "length of stay" in discussing the results.

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TECHNICAL APPENDIX

In this Appendix, we explain and justify our analytic approach to estimating the pure managed care effect of Medicare HMOs.

We used a "before and after with a comparison group" design to evaluate the impact of Medicare HMOs on hospital utilization.[1] To take advantage of our enormous data set with up to five years of inpatient data (1991-1995) on millions of Medicare beneficiaries in which the HMO people spent considerable time in and out of an HMO, we were somewhat restrictive in selecting a clean HMO sample. (See Study Design Overview and Sample Selection portions of paper).

After estimating the reduction in utilization due to being in an HMO in our selected sample, we assume that this managed care effect is proportionally the same for everyone, both in and out of this sample. We then calculate the selection effect indirectly as the difference between FFS use and what we hypothesize the people in HMOs would use if they were in FFS.

Other assumptions and choices made:

The unit of analysis is the person/year. We descriptively evaluated utilization and deaths by quarter, and even by month in smaller samples, and found no cyclical patterns or short-run differences in these variables around the time of changes from FFS to HMO or changes from HMO to FFS. Therefore, we defined the "in HMO" variable for each year by the ratio of the months people are in an HMO over the months that people are alive in that year.

Using the person/year in a two-part model means that the first part is whether a person has any hospital days in the year, and the second part is how many days, conditional on having one or more. This specification is a slight departure from standard admission/length of stay models. The focus on days means that admissions with zero length of stay are excluded. Such admissions were rare in these data (0.2% of people in HMOs had an admission but 0 total days each year as did 0.14% in FFS).

Assume each person/year is an independent observation. Originally we had planned to use panel data methods to exploit the switch from FFS to HMO in our selected analytic sample (xtgee in Stata[2]). After some preliminary diagnostics, we estimated general linear models (with log link and gamma family errors, as suggested by the patterns in residuals discussed below, which is the same as a one-part exponential regression). These models fit reasonably well despite the large number of zeros. The runs showed that the correlation of residuals of days across years is very low (for random coefficients models it was 0.075 between years). Correlation of health care utilization over time is low in general, but this correlation is particularly low because we are only modeling inpatient use and we are controlling for current and future year death. Ignoring this correlation reduces the precision of our estimates, but does not lead to bias in estimated means, so instead of panel data methods we used simpler models that treat each person/year as a separate observation. The before and after with the always in FFS comparison group aspect of the data is captured using cohort indicator variables that represent switching to HMO and staying or always remaining in FFS, which average the behavior and other unobserved characteristics of individuals in these cohorts.

The correlation of the up to five years of data for each individual does affect the estimated error in estimates, so we estimated robust standard errors to control for clustering of residuals within people.[3] The robust confidence intervals of the variable coefficients are up to 30% wider for the logistic regression of any use and 20% wider for the regression of log days given any use compared to the unadjusted confidence intervals. Increases are greatest for variables that are constant over time, and strong predictors of use (e.g. disabled >65); for most constant variables, cluster corrections are around 15% and for variables that change from year to year such as "in HMO" or die they are almost the same as unadjusted confidence intervals.

We tested the impact of assuming independence of years indirectly by evaluating the sensitivity of the results from the first part of the model using conditional logit, a method that does not assume independence. We performed conditional logit regressions of the probability that a beneficiary had any days in different years with the varying HMO membership variables and the varying death variables. All constant variables drop out of conditional logit, which studies the impact of changes in predictor variables over time on people who sometimes have hospitalizations and sometimes do not. The method predicts for these people the years they have events and is the equivalent of a fixed effects model for logistic regression. The estimated effect of being in an HMO on years with use in these conditional logistic models was identical (odds ratio =1.01) to results from our standard logistic regression when each year was considered independent.

Two-Part Model. We chose the two-part model for hospital days based on theory and statistics**.** In theory, the decision to hospitalize is often a separate decision from the extent of use (i.e., length of stay) once hospitalized. While the provider and patient may know an expected length of stay, the patient's ultimate condition and the practice pattern of the physician as well as the system of care (i.e., HMO or FFS) will impact the actual length of stay. Many managed care organizations separate the management of inpatient services into pre-admission certification (whether to hospitalize or not) and concurrent review (length of stay and discharge disposition) highlighting the two-step nature of hospitalization decisions. Statistically, more than 80% of the sample per year uses zero days and among those who are hospitalized, length of stay has a long right tail. Both these reasons support the two-part model. We did the diagnostic tests recommended for specifying a model in Manning and Mullahy.[4] We found the two-part model, with the second part a log transformation, fit the data very well. First the variance of raw scale residuals is quadratic with predictions on the raw scale suggesting gamma and log transformation. Also the log scale residuals are homoscedastic with length of stay, close to normal (skewness = 0.08, kurtosis = 2.77), and the smearing factor is almost exactly $\exp(\sigma^2/2)$.

Because of concern about the undue influence of possible data errors or unusual cases, we looked at the distribution of log residuals from the length of stay regression. They were not quite normal, with skewness = .16. However, we tested winsorizing days greater than 91 days (the top 1/4% of annual days) to 91 days, finding it only changed the effect of the HMO membership coefficients in at most the 4^{th} significant figure. Because we defined year by month of admission, and considered length of stay to be the days since admission, a few people had lengths of stay over 365 days in a particular year and these were excluded from our sample.

Retransformation. After studying log (days) in the second part of the two-part model it is necessary to retransform the results back to days. Effects on total days for each variable were calculated by multiplying three parts: the effect on the probability of a year with at least one day, the mean effect on days conditional on any days, and the smearing factor. To calculate the effect of a variable on the probability of a year with at least one day, take for example, "continuous HMO." First, we recycled the entire sample to calculate the predicted probability p1 of a year with at least one day if all people were always in the HMO, and the probability p0 if all people in the sample were in FFS. The impact p1/p0 = 0.855. This result is closer to 1 than the odds ratio of 0.78 in Table 3a in the paper because some people are quite likely to have a hospitalization, based on other variables such as death, so changing one value for a predictor does not reduce their chance of hospitalization much.

If the residuals in the log scale of days truly have a normal distribution then mean days are the exponential (exp) of mean log days multiplied by the retransformation factor, which is $\exp(\sigma^2/2)$ for σ the standard deviation of the residuals. An alternative nonparametric retransformation factor is called the smearing estimate, which is the average of $\exp(e_i)$ for the residuals e_i of the regression on the log scale.[5,6] We report results using the smearing factor to retransform the data, but the median difference between the two retransformation factors on all our contrasts was less than 1%.

Prediction standard errors in retransformed total days. Effects on total days are obtained by multiplying three effects, those from the logistic regression of any use, the exp(coefficient of log days), and the smearing factor. The standard deviation of the estimated logistic coefficients is almost exactly represented by the first order Taylor series expression (the proof of this is given below), and therefore we can simply evaluate the impact of small changes in the coefficients on predicted probabilities to estimate the factor needed to transform the standard deviation of the coefficient in the original equation to the standard deviation of the impact on population probabilities. For example, a listed standard error of 0.0098 on the logit is 0.0075 in terms of increased log (years with a hospitalization). Because residuals were close to log normal and the smearing factor was so close to the log normal factor of $\exp(\sigma^2/2)$, we assumed the error in the smearing estimate was equal to the error in $\exp(\sigma^2/2)$. Assuming log normality, we can use the standard result that the variance in the estimated variance of σ^2 is $2\sigma^4/(N\text{-}p)$ to compute that error. We assume that errors in the three factors that are multiplied together to estimate effects on total days are independent, so we can add the variance of the log of those factors to get the variance of the log of the estimated effects on total days. Residuals and predictions for log(days) are independent by construction.

Proof that the first order Taylor series approximation to the confidence intervals is a good assumption for the logistic reduction in years with a hospitalization.

The log of the ratio of probability (with x=1)/probability (with x=0) can be written out as a function f of β where β is the estimated coefficient of x, the variable of interest. Let K_i represent the rest of the index for person I, and let g be the inverse logit function, $g(x) = (1+\exp(-x))^{-1}$.

Then $f(\beta) = \log [\Sigma\, g(\beta+K_I) /\Sigma\, g(K_I)] = \log[\Sigma\, g(\beta+K_I)] - \log[\Sigma\, g(K_I)]$. We are interested in $f(\beta + d)$ for small changes d.

The Taylor series is $f(\beta + d) = f(\beta) + d\, f' + d^2 f''/2$.

The second term of f does not depend on β, so drops out of the derivative. Also we will suppress the argument $\beta+K_I$ in what follows.

Now $f' = \Sigma\, g'/\Sigma\, g$ where $g'= \exp(-x)\, g^2$,

and $f'' = \Sigma g''/\Sigma g - \Sigma g'\, \Sigma g'/(\Sigma g)^2$, where $g'' = g'(x)\,(2g\exp(-x) - 1)$.

Now $h = g \exp(-x) = (1+\exp(x))^{-1}$ is between 0 and 1 so $g'=gh < g$, and the second term of f'' is less than 1. When h is small, $g'' = g'(2h-1)$ can be negative but in absolute value it is always less than g' and hence less than g. So the first part of f'' also has absolute value less than 1, and f'' has absolute value less than 2. Empirically, the absolute value of f'' is largest for small values of x, where g' is close to g, and the expression is negative. At $\beta= -0.287$, which is the smallest it gets, we compute that $f'' = (0.072/0.142) - [(0.111)^2/(0.142)^2] = -0.104$.

The largest standard error in estimating any coefficient is 0.018 for "die in that year", so 2 standard errors = 0.036. So the second term of the Taylor series with d = 2 standard errors satisfies $| f'd^2/2 | < 0.104(0.036)^2/2 = 0.00007$, which is negligible, so we will use the first order Taylor series values in the calculation of standard error of f.

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