

# Bed Availability and Hospital Utilization: Estimates of the "Roemer Effect"

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*"Roemer's Law," the notion that an increase in the number of hospital beds per capita increases hospital utilization rates, is an important underpinning of efforts to control hospital construction through health planning. Attempts to measure the magnitude of the effect have yielded results ranging from no effect to a one-to-one relationship. The present study, by restricting its inquiry to Medicare patients and using a unique data base, avoids many of the shortcomings of earlier studies. This study concludes that an increase of 10 percent in hospital beds per capita would increase hospital utilization by Medicare enrollees by about 4 percent.*

## Introduction

Twenty years ago, Milton Roemer and Max Shain raised the possibility that hospital use increases along with the number of hospital beds available in an area (Roemer and Shain, 1959). This relationship, commonly referred to as "Roemer's law," became a major conceptual basis for health planning. If additions to bed capacity caused an increase in utilization, then market forces might not result in the optimal number of beds. Further, because excess beds would generate additional demand, their cost would be greater than the cost of maintaining empty facilities. Regulatory constraints on hospital construction could be justified by this relationship.

A decade later, Martin Feldstein pointed out that Roemer had in mind something different than the usual workings of markets—where an increase in supply raises the quantity sold via the price mechanism (Feldstein, 1971). Feldstein instead conceptualized the Roemer phenomenon as a shift in supply (the change in bed capacity) inducing a shift in the demand function. To test this hypothesis, he estimated a demand with both price and the number of beds *per capita* as independent variables. The elasticity of hospital days with respect to hospital beds was found to be 0.53, which was statistically significant. That is, an increase of 1 percent in hospital beds was found to be associated with a 0.53 percent increase in days of hospitalization.

A number of subsequent studies have also found evidence of a Roemer effect. These studies, however, have produced substantially different estimates of the size of the effect, with elasticities ranging from nearly zero to over one. May (1975), using survey data, found elasticities close to zero. He included time prices as independent variables

though. Many suspect that time prices are an important mechanism through which the Roemer effect works. When facilities increase, time prices fall, inducing greater use. Inclusion of time prices in the regression precludes measurement of this part of the Roemer effect.

Newhouse and Phelps (1976) obtained an elasticity of 0.46. Like May, they used survey data, but they omitted time prices from their equations and used estimation techniques more appropriate for an equation with a limited dependent variable.

Chiswick (1974) estimated an elasticity of total days with respect to beds of 0.85 using data aggregated to Standard Metropolitan Statistical Areas. The result was unchanged when two stage least squares estimation was employed.

Friedman (1978) aggregated Medicare data to the Census division. He found elasticities ranging from 0.75 for acute myocardial infarction to 1.82 for diabetes mellitus. Most elasticities were slightly greater than 1.

The existing research on the Roemer effect, however, has a number of weaknesses. Much of it was designed primarily to measure the response of utilization to changes in price. The assessment of the Roemer effect—the response of utilization to changes in bed supply—was generally incidental. As a result, the design of the studies was not always the best for a measurement of the Roemer effect.

This study avoids several major technical problems found in previous research. Four such problems are described below. Circumventing these problems has produced a more accurate estimate of the Roemer effect, producing results that are important in analyzing health planning and other aspects of health policy.

## Problems in Previous Estimates of the Roemer Effect

The standard method of assessing the Roemer effect has been to use multiple regression to predict use—usually

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days of hospitalization *per capita* — as a function of demographic variables, health status, the price of hospital services (net of insurance), and variables reflecting the availability of medical resources. Demographic variables typically have included age, sex, family status, and income. Physicians *per capita* and hospital beds *per capita* have served as measures of the availability of resources. In such an equation, the regression coefficient of the hospital beds variable provides the measure of the Roemer effect. Some studies have applied this method to data from individual survey respondents, while others have used aggregate data. This discussion will focus on problems inherent in the data used in these studies rather than on the specifics of the techniques used in them.

### **Incorrect Measurement of the Price of Services**

In a study of the Roemer effect, the appropriate measure of the price of services is the marginal price minus insurance reimbursement—that is, the net price to the patient of an extra day or an extra stay in the hospital. Previous studies typically lacked such a measure. Most often, they lacked data on gross prices. Studies such as May, Newhouse-Phelps, and Chiswick only have data on insurance coverage. With no data on hospital price variation from area to area, the Roemer effect cannot be separated from the effect of beds on utilization that works through the price mechanism. Such an omission is likely to cause an overestimate of the Roemer effect, because both effects of an increase in bed supply—the Roemer effect and the effect through the price mechanism—work in the same direction.

Those attempting to measure the Roemer effect have disagreed about the appropriateness of including time prices in the analysis. While economic theory calls for their inclusion, the policy question of how an increase in hospital beds affects the use of service requires that time prices not be held constant. If time prices were a major mechanism by which bed supply affected use, the possibility of the hospital market not being self-regulating would remain.

### **Incorrect Measurement of the Utilization Rate**

Studies using aggregate data face a problem in specifying utilization rates, in that the numerator and denominator of the utilization rate should refer to the same market area. Patients, however, migrate from rural counties to metropolitan areas, from small metropolitan areas to larger ones, and from one State to another to obtain hospital services. Days of hospital care (the numerator) reflect services delivered by area hospitals to both residents and nonresidents alike, but population (the denominator) reflects only the residents of the area. This error in the dependent variable is correlated with the bed availability, in that areas with many beds *per capita* tend to have overstated utilization rates. This would cause an upward bias in the estimate of the Roemer effect (Kelegian and Oates, 1974).

### **Omitted Variables**

If important determinants of utilization that are correlated with bed availability are omitted from the analysis, the esti-

mate of the Roemer effect could be biased. The omission of information on health status from all but the May and Newhouse-Phelps studies is a troubling example. If poor health status in an area tends to produce both higher utilization and a larger supply of beds, the omission of information on health status would bias estimates of the Roemer effect upward.

A particularly serious form of this problem appears in studies using survey data on individuals. The adequacy of the supply of beds in an area depends on a variety of characteristics of the population of that area. For example, four beds per thousand population, the current standard of the U.S. Department of Health and Human Services, may be ample in the average community, but it might be a tight standard in communities with an elderly population or where health status is poor. Omission of these aggregate characteristics of the communities can therefore seriously bias estimates of the Roemer effect.

The expected bias would depend on the specific study. If the omitted characteristics were not correlated with bed availability across the entire sample, the omission would constitute random error in the measure of bed availability. The result would be a downward bias in estimates of the Roemer effect (Pindyck and Rubinfeld, 1976). If the omitted variables were correlated with bed availability, the measurement error would be systematic. The direction of bias would then depend on the relationships between the omitted variables, utilization, and bed availability. Interestingly, those studies employing survey data—which cannot adequately control for aggregate variables such as average health status—do yield smaller estimates of the Roemer effect.

### **Simultaneous Equation Bias**

While bed supply may determine utilization, theory also suggests that over the long run, utilization should determine bed supply. Beds *per capita* is fully exogenous only in the short run. Nevertheless, with the exception of Chiswick, none of the studies treat bed availability as endogenous. Ordinary least squares estimation could result in an upward bias of the estimate of the Roemer effect.

## **New Estimates of the Roemer Effect**

By using a Medicare data base, this study avoids—in whole or in part—some of the shortcomings of previous work discussed above. Incorrect measurement of the price variable is avoided because hospital prices faced by Medicare beneficiaries—a deductible equal to the national average *per diem* Medicare reimbursement—do not vary.<sup>1</sup> Incorrect measurement of utilization rates is avoided by comparing the utilization in Professional Standards Review Organization (PSRO) areas to the Medicare population in

<sup>1</sup>Supplementary private insurance coverage (for example "Medi-gap" policies) introduces a small amount of price variation that is not accounted for in this analysis. Also, prices of a complementary input—physician services—vary across areas. Nevertheless, the specification employed reduces substantially the chance of bias resulting from this omission.

those areas, after adjustment for migration of patients across PSRO area boundaries (U.S.D.H.H.S., 1979). Possible bias from omitted variables is reduced in several ways: by using aggregate (PSRO area) data, by entering bed availability into the equation as the change over a two year period, and by the inclusion in the equation of both regional dummy variables and a three-year lagged value of the dependent variable (Medicare days of care per 1,000 enrollees). Since the lagged utilization variable is a strong predictor of the dependent variable, it serves as an excellent proxy for unmeasured variables that might affect utilization. The regional dummies serve a similar but less crucial role as a proxy for omitted variables. Finally, potential simultaneous equation bias is reduced by entering beds per capita as a first difference. Given long lags in bed construction, it is unlikely that the change in beds is influenced by the change in utilization for the same time period. Indeed, the likelihood is reduced still further by lagging the change of beds by one year. In this specification, simultaneous equation bias would remain only if changes in utilization rates from 1974 to 1977 influenced changes in bed supply between 1974 and 1976. Given long lags in bed construction, this could occur only if relative trends in Medicare utilization rates have been quite stable over long periods of time, and if hospital administrators are aware of those trends and use them in making decisions about bed closures or construction. It should be reiterated that the use rates employed in this study are based on denominators that are adjusted for patient migration between PSRO areas—information of a sort that most hospital administrators during that period most likely did not have. The problem is also reduced as the utilization variable is only for Medicare enrollees.

The study does have a major shortcoming, however, in being limited to the Medicare population. The Roemer effect for Medicare enrollees could be either larger or smaller than for the rest of the population. Nevertheless, the Medicare population accounts for roughly one-third of days of hospital care, so the estimate is important in its own right. Further, generalization to the entire population may involve less error than the problems affecting other studies discussed above.

### Data

The analysis was based on the hospital use of all Medicare beneficiaries in 1977. These data were obtained from the 100 Percent Medicare Claims File.

The unit of analysis, however, was the PSRO area. All areas in the United States were included, regardless of whether they had a functioning PSRO at the time. To obtain a utilization rate, the number of Medicare enrollees (obtained from the Medicare Master Enrollment File) was adjusted to take into account patient migration across PSRO area boundaries. The Master Facility Inventory of the National Center for Health Statistics and the Area Resource File of the Bureau of Health Manpower were used to obtain measure of the demographic and health-care system characteristics of each area.

### Method

Ordinary least squares regression was used to analyze the data. PSRO areas, rather than individuals, were the unit of analysis, so the regression had relatively few (189) degrees of freedom. Definitions of the variables used in the analysis are provided in Table 1, and their intercorrelations are displayed in Table 2.

TABLE 1

Variables in Regression Analysis of the Roemer Effect

Variable	Form
1977 total days of care per 1,000 Medicare enrollees <sup>1</sup> (dependent variable)	static, 1977
1974 total days of care per 1,000 Medicare enrollees <sup>1</sup> (1974 Medicare utilization)	static, 1974
Change in proportion of population age 64 or older ('proportion aged')	first difference, 1976-1974
Certified long-term care beds per 1,000 Medicare enrollees ('l.t.c. beds')	first difference, 1976-1973
Change in physicians per 1,000 population ('physician supply')	first difference, 1976-1974
Population per square mile ('population density')	static, 1976
Proportion of hospital days attributable to Medicare patients ('Medicare proportion')	static, 1976
Hospital occupancy rate (weighted average of within-hospital rates)	static, 1976
Proportion of families with incomes below \$5,000 ('poverty')	static 1977
Months of PSRO review	static, 1977
Four-way regional contrast: Northeast, Northcentral, South, West	3 dummy variables
Proportion of Medicare-certified short-stay beds in teaching facilities (proportion teaching)	static, 1977
Change in short-stay hospital beds per 1,000 population ('bed supply')	first difference, 1976-1974

<sup>1</sup>Medicare population base (denominator) adjusted for patient migration between PSRO areas.

TABLE 2

## Correlation Matrix of Variables in the Regression Analysis

	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1977 Medicare Utilization	.93	-.07	-.23	.09	.30	.23	.56	.06	-.15	.35	.27	-.72	.22	.22
1974 Medicare Utilization	1.00	-.13	-.31	.02	.22	.24	.37	.14	-.14	.22	.39	-.65	.14	.11
Proportion Aged		1.00	-.06	.35	-.12	.27	.16	-.13	-.16	.04	-.13	-.16	.07	.45
L.T.C. Beds			1.00	-.07	.13	-.07	.19	-.37	.24	.32	-.13	.32	.12	-.29
Physician Supply				1.00	.27	-.14	.22	.11	.07	.05	-.13	-.11	.55	.23
Population Density					1.00	.01	.25	.23	.13	.29	-.11	-.07	.36	-.06
Medicare Proportion						1.00	.02	.24	-.10	.29	.00	-.28	-.24	.08
Hospital Occupancy Rate							1.00	-.33	.01	.48	.05	-.51	.38	.10
Poverty								1.00	-.06	-.21	-.03	-.04	-.02	.03
Months of PSRO Review									1.00	.14	-.19	.26	.13	-.15
Northeast vs. West										1.00	-.34	-.27	.18	-.06
Northcentral vs. West											1.00	-.29	.02	.01
South vs. West												1.00	-.15	-.38
Proportion teaching													1.00	.14
Bed Supply														1.00

To avoid problems associated with the use of change variables as dependent variables, the analysis employed a static dependent variable (1977 Medicare days of care) (Cohen and Cohen, 1975; Cronbach and Furby, 1970). Accordingly, a lagged value of the dependent variable (1974 Medicare days of care) was used as a covariate to control for previous utilization rates. This variable also serves as a proxy to control for a variety of unmeasured differences between PSRO areas. The effectiveness of using a lagged value of the dependent variable as a proxy for omitted independent variables depends on existence of stable relationships between the dependent variable and the omitted variables, as well as on stability in the omitted variables themselves. Neither of these can be directly assessed with data employed in this study. The relationships between the dependent variable and the measured independent variables, however, are reasonably stable, as indicated by the first two rows of Table 2. Moreover, the fact that the lagged dependent variable shows not only a strong zero-order relationship with the dependent variable, but also a strong partial relationship (a standardized regression coefficient of 0.78) suggests that is an effective proxy for some important omitted variables.

Three independent variables were used to control for demographic differences between PSRO areas. One was the change from 1974 to 1976 in the proportion of the population aged 65 and over. This variable is important in theory, since changes in it would lead directly to changes in the Medicare utilization rate which could bias estimates of the Roemer effect. (It turned out empirically to be an important variable in the regression as well.) Population density and the proportion of families in poverty were entered as static (1976) variables.

Six variables represented the health-system characteristics of each area. One was the independent variable of interest — the change from 1974 to 1976 in short-stay hospital beds per 1,000 population. Since bed supply was entered only as a change variable, a static occupancy rate

measure was added as a control for the adequacy of hospital capacity. The use of both bed supply and occupancy variables in a regression with days of care per 1,000 as the dependent variable did not create an identity, because of the use of lags and first differences. The remaining health-system variables employed were the number of Medicare-certified long-term care beds per 1,000 enrollees, the proportion of days of care attributable to Medicare patients, the proportion of hospital days attributable to Medicare patients, and the change (1974-1976) in the number of physicians per 1,000 population.

A four-way regional contrast (Northeast, Northcentral, South, and West) was represented by three dummy variables. These were included because of the well-known difference between regions in patterns of hospital use. In the absence of a full explanation of those regional differences, the region variables can be seen as a proxy for unmeasured demographic or health-system characteristics.

Finally, the number of months that PSROs had been conducting review was included. The use of this variable was necessitated by the finding that PSROs have had an effect on Medicare hospital use (CBO, 1979; 1981).

## Results

The regression analysis indicated a large and highly significant Roemer effect ( $t = 5.6$ ,  $p < .0005$ ; see Table 3). The regression coefficient indicated that a 1 percent change in bed supply, all else being constant, produces a 0.42 percent increase in Medicare days of hospitalization per 1,000 enrollees.

Because of the specification used, the Roemer effect expressed in percentage terms will vary with the level of the bed supply and utilization variables. The values above were obtained by evaluating the results at the baseline (1974) level of bed supply and the 1977 level of utilization (the dependent variable). Using the baseline (1974) level of utilization would not materially affect the results.

**TABLE 3**  
**Results of the Regression Analysis**

Variable	Mean	b	t
1974 Medicare utilization	3640.5	.840	26.26****
Proportion aged	3.280	-15.773	2.98***
I.t.c. beds	2.05	-.119	0.11
Physician supply	.104	111.257	0.54
Population density	1625.1	.006	2.68**
Medicare proportion	.342	63.6	0.25
Hospital occupancy rate	.737	1950.5	6.98****
Poverty	.110	-205.6	0.40
Months of PSRO review	7.70	-3.178	2.39*
Northeast vs. West	.243	87.095	2.03*
Northcentral vs. West	.270	-58.884	1.65
South vs. West	.185	-62.746	1.18
Proportion teaching	31.115	-.496	0.72
Bed supply	0.0581	373.919	5.55****
.....			
Intercept	-725.4		
R <sup>2</sup> = .94			
R <sup>2</sup> adjusted = .93			

- \* p<.05
- \*\* p<.01
- \*\*\* p<.005
- \*\*\*\* p<.0005

Note that because the beds variable is a first difference, the elasticity cannot be obtained directly from the usual formula:

$$\frac{dy}{dx} \cdot \frac{\bar{x}}{\bar{y}}$$

This formula would yield the desired elasticity — the percent change in utilization per percent change in bed supply — only if the bed supply in raw form were used as the independent variable.

Since bed supply was entered as a first difference, the calculation of the elasticity is a bit more indirect. A value of the first difference variable is selected to represent a given percent change in bed supply. This value is then multiplied by the regression coefficient to yield a resulting change in utilization rates, and this change in rates is divided by a baseline utilization rate to transform it into a percent change. The ratio of the chosen percent change in bed supply to this derived percent change in utilization rates is the elasticity.

The baseline (1974) bed supply was 4.1782 beds per 1,000 Medicare enrollees. A 1 percent change would therefore be 0.041782 beds per 1,000. Multiplying this change by the regression coefficient of 373.919 yields a utilization change of 15.623 days of hospitalization per 1,000 Medicare enrollees. This corresponds to 0.42 percent of the 1977 average rate of 3711.263 days per 1,000.

The specification provided a good fit, suggesting that the problem of omitted variables may have been largely avoided. To assess the degree of fit, however, it is necessary to distinguish between what could be called cross-

sectional fit and time-series fit — that is, between the model's ability to predict levels of utilization rates and its ability to predict changes in utilization rates. The model is necessarily much more successful in predicting levels than in predicting changes.

The cross-sectional fit of the model — that is, its ability to predict levels of utilization — is excellent: an R<sup>2</sup> of .93 after downward adjustment for sample size (see Table 3). The lagged dependent variable was the most important factor contributing to this good fit; its standardized regression coefficient was .78, with a t of 26. The regional dummy variables, which were also expected to play a role as proxies for omitted variables, were found to have moderate predictive power; all were statistically either significant (p<.05) or marginal (p<.10).

The model's ability to predict change in utilization rates, however, is necessarily much lower. This is because of the large proportion of variance accounted for by the lagged dependent variable. When predicting change in a dependent variable, R<sup>2</sup> is at best an ambiguous measure, because it is highly sensitive to methods (such as first differencing) used to remove the effects of prior values of the dependent variable. In such cases, it is more meaningful to measure the model's success in predicting innovation variance — that is, the variance of the dependent variable that is not predicted by prior values of the dependent variable (Pierce, 1979). The specification used here accounted for about 49 percent of this innovation variance (after downward adjustment for sample size).

## Implications

The observed relationship between hospital beds and rates of use in the Medicare population suggests that a similar relationship might exist for the general population. Medicare hospital coverage is similar to that for the general population, so that there is no *a priori* reason for changes in use by the non-Medicare population to offset changes in the Medicare population. The magnitude of the effect could differ, however, because the Medicare population has different medical conditions that are treated in hospitals and different time prices. There are no predictions of whether the magnitude of the Roemer effect for the entire population is larger or smaller than the 0.42 elasticity estimated for the Medicare population.

Confirmation of the Roemer effect supports the notion that health planning has the *potential* to significantly lower hospital costs. For example, if health planning were successful in reducing the stock of beds by 10 percent, resources would be saved not only from the reduction in excess capacity but also from the 4 percent reduction in days of care (Joskow and Schwartz, 1980). While not analyzed in this study, similar relationships may exist between major pieces of capital equipment, such as Computed Axial Tomography (CAT) scanners, and use of ancillary services.

Nevertheless, presence of the Roemer effect does not imply that health planning is the policy of choice to contain health care costs. Some have criticized planning on the basis of potential distortions from regulating one input, and the technical and political obstacles to making good project decisions. In addition, studies of the early health planning

activities by the States have not been encouraging about effectiveness (CBO, 1982). While the absence of demonstrated effects does not imply that planning cannot work, it is likely that realizing the cost-saving potential of planning will be difficult.

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