PROVIDER SELECTION, BARGAINING, AND UTILIZATION MANAGEMENT IN MANAGED CARE

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Managed care controls cost through a combination of provider selection, bargaining, and utilization management. Provider selection will reduce expenditures if patients are funneled to efficient providers. Bargaining will reduce expenditures through lower rates. Utilization management will reduce expenditures if providers reduce treatment intensity due to monitoring. We estimate that about 30% of the reduction in inpatient expenditures in a mental health carve-out was due to provider selection, 5% was due to bargaining, and the remaining 65% was due to utilization management. We find that both the provider selection and utilization management effects were likely to be welfare improving. (*JEL* I1)

I. INTRODUCTION

The managed care industry is extraordinarily dynamic. One of the earliest examples of managed care is the Kaiser prepaid group health plan, which contracted exclusively with the Permanente group to supply health care to a group of beneficiaries. The types of contractual arrangements used in managed care plans have since evolved into so many other forms that it is difficult to classify relationships using traditional typologies (e.g., staff versus group or network model health maintenance organization [HMO]). The managed care organization may own the hospital and/or physician group, the hospital may own the managed care organization, or the managed care organization may contract with hospitals and physician groups.[1] The large variety of institutional arrangements known as managed care make it necessary for health economists to look inside the black box of managed care and examine not only whether managed care reduces expenditures but also how expenditures are reduced.

What is relevant for this analysis is not whether a managed care organization calls itself an HMO, a preferred provider organization (PPO), independent physician association (IPA), or another acronym, but how managed care affects expenditures and quality of care. Managed care organizations generally use three of the tools to reduce expenditures: selecting low cost providers, bargaining for lower rates, and utilization management. The economic differences between

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1. See Robinson (1999) for a summary of the variety of institutional arrangements.

ABBREVIATIONS
BCBS: Blue Cross and Blue Shield
HMO: Health Maintenance Organization
PPO: Preferred Provider Organization
SSI: Supplemental Security Income

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managed care plans lie in which of these three tools are applied and to what degree. Thus, we can gain insight into modern managed health care by studying the way each of these three components affect cost and utilization. This study measures the relative contribution of each managed care tool to changes in aggregate expenditures. We study the effect of a managed care program in which there was a change from a fee-for-service type arrangement to a managed care environment. However, the concepts and methods in this article can be applied to any situation where the network, contractual terms, or utilization management rules have changed.

One way that managed care organizations control expenditures and quality of care is to restrict the number of providers in a network. We define the effect of selecting certain types of providers as the *provider selection effect*. In other words, the provider selection effect is the change in expenditures that can be attributed to the inclusion or exclusion of different providers into the network. For example, the managed care organization may select providers with a history of less intensive treatment patterns and avoid providers with a history of highly intensive treatment patterns. The change in future expenditures that are due to selection of low (or high) intensity providers is the provider selection effect *with respect to utilization*. Similarly, the difference in rates at providers before the new rates are bargained is defined as the provider selection effect *with respect to price*. In summary, the provider selection effect measures the effect of selecting *types* of providers on subsequent expenditures. It does not measure how rates or utilization patterns change at a given provider over time.

After providers are selected to be in the network, the managed care organization may try to achieve expenditure reductions by bargaining over prices and utilization management. Changes in reimbursement rates over time for a given provider are defined as the *bargaining effect*, and changes in utilization over time are defined as the *utilization management effect*. The bargaining effect is the change (usually reduction) in prices that is a result of bargaining with the managed care organization in order to be included in the network. Although it is possible that managed care organizations negotiate other aspects of the contract, our investigation of this industry has revealed that bargaining over prices is by far the most important in terms of changes in expenditure. Managed care organizations are usually able to leverage price discounts from providers because they can threaten to exclude providers from the network. Bargaining over prices will reduce expenditures if the managed care organization negotiates lower rates than the provider previously charged, ceteris paribus. The bargaining effect differs from the provider selection effect with respect to price because bargaining causes changes in rates at providers that win a contract, whereas provider selection measures differences in rates between network providers and other providers at baseline, before the new rates are bargained.

The *utilization management effect* is the change in expenditures due to utilization management. Managed care organizations may actively manage and monitor care to change the way providers provide care. Utilization management includes profiling providers, prior authorization of admissions, and concurrent utilization review. For example, a managed care organization may require that all admissions to a hospital be certified. Another type of utilization management is profiling providers (e.g., tracking the intensity of care given by providers). Profiling may change provider behavior when there is a credible threat to exclude the provider from future contracts, as shown in Ma and McGuire (2002). The distinction between the utilization management effect and the provider selection effect is that the utilization management effect measures how managed care leads to changes in utilization at the provider over time, whereas the provider selection effect measures differences in utilization between network and other providers at baseline.

Although we have presented the three effects as distinct, in some cases they are closely related. For example, managed care organizations may use financial incentives, such as capitated payments to providers, to shift the risk and benefit of utilization management to providers. If the providers bargained with the managed care organization over the level of financial incentives, then these financial incentives affect expenditures through both the bargaining and utilization management effects. In this case, the providers themselves often conduct the utilization management, rather than the managed care organization as in Kerr et al. (1995).

In this article, we show how to estimate the magnitude of the provider selection, bargaining, and utilization management effects. We use data from a mental health carve-out in Massachusetts where a managed care organization contracted with hospitals to set up a managed care network. Our approach for decomposing utilization into the effects consists of a series of regression and count data models. We also decompose changes in prices using weighted averages because prices were fixed per diem and did not vary by disease. Finally, we assess whether changes in quality can be linked to changes in utilization to gain insight into whether reductions in utilization were due to efficiency gains or lower quality.

Decomposing total expenditures into the three components provides insight into how managed care affects social welfare. Provider selection, bargaining, and utilization management affect social welfare in different ways. Provider selection improves social welfare when efficient providers are selected into the network. Utilization management can either increase or decrease social welfare depending on the economic appropriateness of treatment intensity without utilization management. Bargaining is usually thought of as a transfer between two parties, with no net effect on social welfare. However, bargaining can affect social welfare if the change in reimbursement rate affects quality of care or efficiency. For example, lower reimbursement can reduce social welfare if access and quality of care are compromised and providers are not profit maximizing (Pauly, 1988). In addition, if a managed care organization has and takes advantage of its monopsony power by bargaining artificially low rates, social welfare will decline, as in Pauly (1998). On the other hand, lower reimbursement may induce providers to lower costs of treatment, thereby improving efficiency and social welfare. Robinson (1991) and Zwanziger et al. (1994a) have analyzed the effect of price competition on lower costs and found that costs did decline. If price pressure causes cost to decline without detrimental effects on access and quality, the bargaining effect can be welfare improving. Although an interesting theoretical issue, measuring the long-term effect of bargaining on subsequent cost changes is beyond the scope of this article.

We explicitly link the effect of provider selection to efficiency. Efficiency is defined as both the amount of health benefit derived from a bed day and treatment in a medically and economically appropriate setting. For example, treatment at an efficient provider yields more health benefit in one day than an inefficient provider. A shorter length of stay may indicate more efficiency (more health benefit per day), or it may be due to providers having an established infrastructure or protocol to reintroduce patients into the community sooner than other providers, as shown by Dickey et al. (1996), Bond et al. (1988), and Borland et al. (1989). Therefore, in either case, a patient treated in an efficient hospital will have a shorter length of stay. Newhouse (1996) advanced this definition as least cost treatment by a medical provider, holding quality constant.

In our study, it is possible that a shorter length of stay is due to withholding needed care (lower quality) rather than efficiency. We empirically test the link between efficiency and length of stay by testing whether shorter length of stay is correlated with measures of quality of care. We chose two measures of quality—the length of time between admissions and readmissions within 30 days—that are consistent with current mental health policy and available in the data. The measures are imperfect because changes in these measures will reflect utilization management in addition to quality. Therefore this analysis will only detect the most serious readmissions. Our analysis also considers the results of surveys of in-network providers conducted by Callahan et al. (1995) and Beinecke et al. (1997). These surveys included questions about access, utilization management, and quality. We integrate these results into our discussion of the effect of provider selection, bargaining, and utilization management on welfare.

II. PRIOR LITERATURE

The prior literature has identified either the effect of provider selection, bargaining, or utilization management in isolation. Our study extends this literature because we decompose aggregate changes in expenditures and study the relative share of the provider selection, bargaining, and utilization management effects. We show that linking the contractual design of the managed care program to changes in reimbursement affords deeper insight into the welfare implications of managed care and an understanding of which stakeholders (e.g., providers or patients) bear the brunt of expenditure reductions.

There have been relatively few studies that link managed care–related cost savings to the provider selection effect. Robinson and Phibbs (1989) was one of the first studies that found that selecting low-cost providers contributed to cost savings. Zwanziger et al. (1994b) analyzed the characteristics of network hospitals and found that less costly hospitals were more likely to be selected into a network under Medicare prospective payment. Studies of the effect of utilization management are more common than the provider selection effect. The empirical analysis in Wickizer et al. (1989) found that utilization review significantly reduced admissions and the total number of inpatient days but did not affect length of stay. Gotowka and Smith (1991) measured the effect of psychiatric utilization management using an experimental and control group that were drawn randomly. They found a significant increase in inpatient charges per member of the control group and a slight decrease in the experimental group. There are several other studies of the use of utilization management for psychiatric care and general health care. The results generally show at least some reduction in overall inpatient expenditures per enrollee at least in the short run. See Wickizer (1990) for a summary.

In mental health services research, several authors have documented how managed care reduces costs. For example, Sturm et al. (1995) found dramatic reductions in mental health visits in a prepaid (capitated) environment vis-à-vis fee-for-service. Dickey et al. (1996) and Frank and McGuire (1997) document the early experience of Massachusetts with the Medicaid mental health carve-out analyzed here. Goldman et al. (1998) in a study of a private-sector mental health plan found that much of the cost reduction was due to fewer outpatient sessions, lowered admission rates, reduced length of stay, and lower costs per unit of services. Ellis and McGuire (1996) found that a change in the reimbursement for psychiatric Medicaid beneficiaries in New Hampshire lead to a 14% decline in length of stay. The authors stressed that reductions in utilization could be traced to low-cost alternatives to hospitalizations and outpatient care. In their study, some these changes were introduced by the managed care organization through utilization management. In our study, we decompose these types of changes in utilization into the provider selection and utilization management effects. Sturm (1997) finds that one of the major reasons for cost saving in a study of 24 private carve-outs was not utilization management but contractually fixed reimbursement rates. They concluded that mental health parity could be affordable under managed care.

Ma and McGuire (1998a) also found reductions in expenditures in a mental health carve-out program for state employees. They trace the reductions in utilization to the incentives of the contract. A similar contract was used in the carve-out studied in this article. In the next section we discuss the terms of the contract used in the Massachusetts behavioral health carve-out. The contract is interesting because the incentives built into the contract help shape the relative contribution of provider selection, bargaining, and utilization management to changes in total expenditures.

III. THE MASSACHUSETTS BEHAVIORAL HEALTH CARVE-OUT

The primary data set used to decompose cost savings is from a Medicaid behavioral health carve-out. In 1992 the Commonwealth of Massachusetts contracted with a managed care organization to manage the mental health care of all Supplemental Security Income (SSI) disabled and non-disabled Medicaid enrollees. It was not possible for the enrollees to opt out of any managed care. However, there was a choice between a local HMO and the carve-out. Only 2% of the disabled enrollees opted to join the local HMO. The remaining 98% were in the carve-out plan that we study. Administrative expenses were reimbursed at a per beneficiary rate (i.e., capitated). The managed care organization was liable for any administrative cost overruns, but it was also able to keep any cost savings. Thus, there were strong incentives to keep administrative and management costs at a minimum.

Although Medicaid paid the managed care organization a capitated rate for medical expenses, there were cost-sharing provisions that lessened the amount of risk borne by the managed care organization. The cost-sharing provisions implemented after the first six months of the contract were a combination of \$1 million to \$2 million loss/profit caps and 8–25% cost-sharing bands. The contract also included a provision where the managed care organization could receive a lump-sum bonus if it reduced admissions in the first six months of the contract. In subsequent years they received additional money if admissions were held close to the initial reduction. See Frank and McGuire (1997) for more details of the contract.

Due to the high degree of cost sharing for medical services, the capitated rate with no cost sharing for administrative services, and the direct incentives to reduce admissions in the first year of the contract, we hypothesize that concurrent utilization management had little or no effect on length of stay and utilization management was focused on a one-time reduction in admissions. In practice, the strong incentives to reduce admissions were surprisingly effective and exceeded the goals of both Medicaid and the managed care organization. In fact, the incentives to reduce admissions were weakened in subsequent years due to the dramatic decline in the first year. Admissions declined primarily due to preadmission certification. After the first year, the admission rate stabilized at a level that was satisfactory to Medicaid and the managed care organization. The results presented here reflect the radical drop in admissions in the first year and the return to more stable levels in subsequent years. We do not expect the drop in admissions to reflect dumping because state officials monitored admissions at state hospitals closely to avoid the type of dumping of patients uncovered by Schlesinger et al. (1997). In fact, Callahan et al. (1995) concluded that dumping did not occur in the first year of the program.

We expect a significant provider selection effect because inclusion into the provider network was desirable and competitive. Inpatient admissions were reimbursed by the managed care organization at a comprehensive per diem rate in both the pre- and postperiod. Most of the hospitals that were chosen to join the network accepted a discount on the preperiod per diem rate. The number of hospitals fell from about 55 in the preperiod to 35 in the postperiod. However, access was believed to be unaffected by the decline because the network of winning hospitals was geographically disbursed (see Fisher et al. [1998]). In addition, we do not expect capacity constraints to affect the number of admissions in the postperiod. According to the American Hospital Association's *Annual Survey of Hospitals*, the capacity utilization at winning hospitals was about 72% in 1992. Only one winning hospital had a capacity utilization of over 90% and this hospital was in Boston where there were other winning hospitals with unfilled beds nearby. The ambulatory treatment capacity did shrink slightly under the managed care contract. The physicians that dropped out tended to be solo physicians; the vast majority of groups continued to treat patients under the managed care contract. Furthermore, Callahan et al. (1995) found that access to care versus the preperiod was rated 3.0 (1 = worse to 5 = better), which implies that access was unchanged by the introduction of managed care. This result is also supported in a follow-up survey by Beinecke et al. (1997).

IV. DECOMPOSITION OF EXPENDITURES

The provider selection, bargaining, and utilization management effects can be derived from changes in the aggregate inpatient expenditure between the pre- and postperiods. We start by defining aggregate expenditures in one period. The average cost of an episode of care, C, in period t is equal to the average length of stay weighted by the price per day:

$$\begin{aligned} C &= \sum_j \sum_i p_j \times los_{ji} \Big/ n \\ &= \left(\sum_j \sum_i p_j \times los_{ji} \Big/ \sum_j \sum_i los_{ji} \right) \\ &\quad \times \left(\sum_j \sum_i los_{ji} \Big/ n \right) \\ &= P \times LOS, \end{aligned}$$

where j indexes hospitals, i indexes individuals, p is the per diem rate, los represents length of stay, n is the number of inpatients, P is the average of per diem rates weighted by length of stay, and LOS is the average length of stay. The pre-post change in per episode expenditures, ΔC, can be written as

$$(1) \quad \Delta C = P^{post}LOS^{post} - P^{pre}LOS^{pre} = (P^{post} - P^{pre}) \times (LOS^{post} + LOS^{pre})/2 + (LOS^{post} - LOS^{pre}) \times (P^{post} + P^{pre})/2,$$

where the superscripts represent the period. There are two components of the change in per diem rates: the bargaining effect and the provider selection effect with respect to price. The bargaining effect is the difference in pre- and postperiod rates for providers that win a contract. The provider selection effect with respect to price is measured as the difference in preperiod rates at providers that win and do not win a contract. The first term on the right-hand side of equation (1) is therefore the combination of the bargaining and provider selection effect with respect to price, or

$$(2) \quad P^{post} - P^{pre} = (P^{post} - P^{pre}_{win}) + (P^{pre}_{win} - P^{pre})$$
$$\Delta P = Bargaining + Selection^{PRICE},$$

where $P^{post} - P^{pre}_{win}$ is the bargaining effect and $P^{pre}_{win} - P^{pre}$ is the provider selection effect with respect to prices. The subscript *win* indicates the subset of hospitals that won a contract. Similarly, the change in the length of stay can be broken down into the utilization management effect and the provider selection effect with respect to length of stay

$$(3a) \quad LOS^{post} - LOS^{pre} = (LOS^{post} - LOS^{pre}_{win}) + (LOS^{pre}_{win} - LOS^{pre})$$
$$\Delta LOS = Utilization\ Management + Selection^{LOS},$$

where $LOS^{post} - LOS^{pre}_{win}$ is the utilization management effect and $LOS^{pre}_{win} - LOS^{pre}$ is the provider selection effect with respect to length of stay. The utilization management effect is the within-patient and within-provider effect of managed care. The provider selection effect is the difference between the aggregate effect of the program and the utilization management effect.

Equation (3a) measures changes in length of stay conditional on admission. However, another component of utilization management is the change in admission criteria. For example, if the length of stay in days at one hospital increases from 10 to 12, but the number of admissions per period for each individual declined from 2 to 1, then assuming admissions and (conditional) length of stay are independent, the number of days per period would decrease from 20 to 12. Thus we measure changes in the probability of admission due to managed care and adjust equation (3a) to account for these changes to properly measure the effect of utilization management. The change in length of stay, adjusted for the change in the number of admissions, α, is

$$(3b) \quad \alpha^{post} * LOS^{post} - \alpha^{pre} * LOS^{pre} = (\alpha^{post} * LOS^{post} - \alpha^{pre} * LOS^{pre}_{win}) + \alpha^{pre} * (LOS^{pre}_{win} - LOS^{pre})$$
$$\Delta LOS_{\alpha} = Utilization\ Management_{\alpha} + Selection^{LOS}_{\alpha}$$

where α^{pre} and α^{post} are the number of admissions in the pre- and postperiods for an individual, the first term on the right-hand side is the utilization management effect, and the second term is the provider selection effect, both adjusted for the number of admissions. The subscript α is included to indicate that the estimates include the effect of changes in admissions. We attribute changes in the number of admissions to utilization management rather than access because there is no evidence that access was compromised (see Fisher, et al. [1998]; Beinecke et al. [1997]; Callahan et al. [1995]).

The decomposition can be summarized by rewriting equation (1) as follows:

$$\Delta C = (Selection^{PRICE} + Bargaining) \times (\overline{LOS}) + (utilization\ Management_{\alpha} + Selection_{\alpha}^{LOS})(\overline{P}),$$

where $\overline{LOS}$ is the pre–post average of length of stay and $\overline{P}$ is the pre–post average of price. In the remainder of the article we describe how we estimate the provider selection effect, bargaining effect, and utilization management effect. Estimation of *Selection*^{PRICE} and *Bargaining* are simple because the per diem price does not vary by diagnosis or length of stay. Thus we calculate these measures using equation 2 weighted by length of stay. However, length of stay does vary by diagnosis and severity of illness, and therefore we must control for variation in length of stay that is not due to *Selection*^{LOS} and *Utilization Management* using multivariate analysis. We use claims data from the Massachusetts behavioral health carve-out to decompose changes in utilization into the provider selection and utilization management effects. We describe the claims data and the comparison group in the next section.

V. DATA

Medicaid Data

The sample of inpatient claims data includes all SSI disabled Medicaid enrollees age 18–64 with at least one inpatient admission for schizophrenia, major affective disorders, or other psychoses (ICD-9 codes 295–299) from fiscal year 1991 to 1995. These data were obtained from the state Division of Medical Assistance. We limit our analysis to facilities eligible for psychiatric reimbursement (~5500 claims). The vast majority of these claims were at substance abuse facilities for treatment of substance abuse problems. Others were for facilities that were out of state. Claims for state mental hospitals are not included because they did not contract with the managed care organization (~1000 claims). We also eliminated claims reimbursed on a per episode (i.e., fixed rate for each admission) basis because Medicaid switched from per diem to per episode reimbursement at the end of 1992 (~7600 claims). Per episode reimbursement lasted from four to ten months depending on the hospital. Only four months of claims are excluded at hospitals that switched to per episode reimbursement relatively late, whereas up to ten months of claims are excluded at other hospitals. We exclude these claims because a shift from per episode to per diem represents a strong shift in provider incentives toward shorter length of stay and more admission and inclusion of these claims in the preperiod makes interpretation of the parameter estimates difficult.

After these adjustments, our sample consists of 21,875 claims by 8656 unique individuals. There were 8557 claims by 4173 individuals in the preperiod and 13,318 claims by 5600 individuals in the postperiod. About half of the individuals in the preperiod had at least one admission in the postperiod; 21% of those admitted in the preperiod only had one admission; and about 20% of those admitted in the postperiod had one admission. Fifty-five hospitals in the preperiod and 35 hospitals in the postperiod had more than 30 admissions. The sample includes admissions at several other hospitals that had less than 30 admissions. The postperiod started in early 1993 at some hospitals and by July 1993 (the beginning of FY93) all hospitals operated under the provisions of the carve-out. The postperiod is longer than the preperiod by at least one year.

The average length of stay fell by 2.8 days after the introduction of managed care. The average length of stay was 13.1 in the preperiod and 10.3 in the postperiod (see Table 1). The average age of the individuals in the preperiod was 38.1 years; after the program was initiated in the postperiod the average age fell to 37.1. Age is measured as deviations from 41 years in the regressions. The percentage of individuals diagnosed with schizophrenia increased from 28% in the preperiod to 33% in the postperiod. The percentage of individuals diagnosed with major affective disorders increased from 34% to 40%. The percent of individuals with reported comorbidities decreased from 50% to 41%; this drop is due to coding changes related to the link between coding and billing in the preperiod. All of the differences between the pre- and postperiod reported in this paragraph are significant at the 1% level.

TABLE 1
Descriptive Statistics

	All Observations	Medicaid Preperiod 7/90–~6/92	Medicaid Postperiod 7/92~–6/95	Medicaid Difference (SE)	Comparison Group Preperiod 7/90–12/91	Comparison Group Postperiod 7/92–12/94	Comparison Group Difference (SE)
Length of stay	12.31 (12.29)	13.112 (11.876)	10.328 (9.781)	−2.78* (0.15)	14.48 (12.59)	13.57 (15.13)	−0.91* (0.23)
Age	37.82 (11.32)	38.135 (12.305)	37.102 (10.944)	−1.03* (0.16)	38.73 (10.94)	38.06 (10.80)	−0.66* (0.19)
Schizophrenia	0.33 (0.47)	0.276 (0.447)	0.327 (0.469)	0.051* (0.01)	0.364 (0.481)	0.364 (0.481)	−0.0002 (0.01)
Major affective disorder	0.46 (0.50)	0.341 (0.474)	0.397 (0.489)	0.056* (0.01)	0.569 (0.495)	0.577 (0.494)	0.008 (0.01)
Medical comorbidities	0.50 (0.43)	0.503 (0.500)	0.414 (0.493)	−0.089* (0.01)	0.54 (0.28)	0.59 (0.28)	0.05* (0.01)
Per diem rates	N/A	$485.93 (308.40)	$478.67 (199.82)	−$7.27 (3.83)	N/A	N/A	N/A
Number of hospitals[a]	60	55	35	−20	28	31	3
Number of patients	N/A	4173	5600	1427	N/A	N/A	N/A
Number of observations	37,044	8557	13,318	4761	5037	10,132	5095

*Difference significant at the 1% level. SD in parenthesis unless otherwise indicated.
[a]Hospitals with more than 30 admissions in both periods.

Data on pre- and postperiod per diem rates were calculated from the claims data that included actual reimbursement rates in both periods. These rates are the actual rates negotiated with the managed care organization in the postperiod. Average per diem rates fell slightly from $485.93 to $478.67.

The Control Group

To control for underlying trends not captured in the pre–post design, we analyze data from a control group called the Health Care Cost and Utilization Project Nationwide Inpatient Sample released by the Agency for Health Care Policy and Research. The sample is limited to patients admitted to Massachusetts hospitals, aged 18–64 with a major mental illness. We limited the sample to patients with Medicare, fee-for-service insurers, self-pay, or other sources (e.g., CHAMPUS) as payers because we do not expect care for the patients to be directly affected by the Medicaid program. We exclude claims for patients whose visit is paid by Medicaid, HMOs, Blue Cross and Blue Shield (BCBS), and other alternative payers. We do not use claims from these patients because either Medicaid contracting or utilization management may affect the length of stay of these claims. For example, we eliminated BCBS claims because they form networks of preferred providers. We also exclude claims during the last half of 1992 so that the time period of the comparison group roughly matches that of the Medicaid group. There are 15,169 episode claims in the comparison group. Twenty-eight hospitals in the preperiod and 31 hospitals in the postperiod had more than 30 visits. There are about twice as many discharges in the postperiod in the comparison group.

The average length of stay of the comparison group in the preperiod was 14.5 days, and in the postperiod it fell to 13.6. The difference between the pre- and postperiod length of stay is significant at the 1% level. The average length of stay is 1.5–3 days longer than that of the SSI disabled population. The average age in the comparison group decreased from 38.7 in the preperiod to 38.1 in the postperiod. This difference is significant at the 1% level. The average person in the comparison group is less than one year older than the average age in the SSI disabled group. In both samples about a third are diagnosed with schizophrenia. There is no change between the pre- and postperiod in the comparison group. There are almost 20% more individuals with major affective disorders in the comparison group than the treatment group. Medical comorbidities are also more frequent in the comparison group—54% in the preperiod and 58% in the postperiod. This difference is significant at the 1% level. It is possible that the increase in diagnoses of schizophrenia and major affective disorders found in the managed care sample is due to diagnosis creep, where doctors upgrade diagnoses to ensure that patients receive adequate care. We redid the analysis with and without controls for principal diagnosis, and the results were unaffected.

VI. ECONOMETRIC METHODS

Our empirical method decomposes the aggregate change in expenditures into three effects. The method used to estimate the provider selection and utilization management effects with respect to length of stay consists of a main equation with length of stay as the dependent variable and a series of adjustments. The first step is to estimate the aggregate effect of managed care on length of stay (equation [4] without hospital and patient fixed effects). Next, we estimate the effect of utilization management on length of stay (equation [4]). The decomposition is further complicated because we need to control for contemporaneous variation in treatment practices that affect length of stay. We adjust for contemporaneous trends using predictions from equation [5]. We then estimate zero-inflated negative binomial count data model to obtain the predicted the number of admissions. The adjustments are tied to the length of stay estimates when we calculate expected number of days in equations (6) and (7). The method for calculating the provider selection, bargaining, and utilization management effects with respect to price and utilization using equations (2) and (3b) is described after we present the econometric results in section VIII.

The first step in the decomposition is to estimate the effect of managed care on length of stay. The square root of length of stay is modeled as a function of individual and disease characteristics, individual fixed effects, hospital fixed effects, and year/managed care status,

$$(4)\quad \sqrt{los_{ijt}} = P_{it}\beta + FY92_{ijt}\gamma_1 + FY93_{ijt}\gamma_2 + FY94_{ijt}\gamma_3 + FY95_{ijt}\gamma_4 + \lambda_i + \theta_j + \varepsilon_{ijt},$$

where P_{it} is a row vector of individual and disease characteristics that vary over individuals and time, β is a vector of parameters associated individual and disease characteristics, $FY92$–$FY95$ are dummy variables indicating whether individual i at hospital j was in the year/program at the time of discharge, γ measures the effect of the year/program, λ_i represents fixed individual characteristics that affect length of stay, θ_j is an error component representing fixed hospital characteristics that affect length of stay, and t is time. The estimates of γ when individual and hospital fixed effects are excluded represent total effect of managed care, and the estimates of γ with hospital and individual fixed effects represents the conditional effect of utilization management on length of stay. We use the square root of length of stay to adjust for the skewness of the data because the square root transformation is less sensitive to heteroskedasticity than a log transformation, as noted in Manning (1998). We describe the square root retransformation process that we use to adjust for smearing below.

Control Group

To control for contemporaneous trends in length of stay we estimated the following model on the control group data pooled with the Medicaid sample:

$$(5)\quad \sqrt{los_{ijt}} = P_{it}\beta + \sum_{y=2}^{5} FY9y_{ijt} \times \tau_y + \sum_{y=2}^{5} FY9y_{ijt} \times Medicaid_i\gamma_y + Medicaid_i\phi + \theta_j + \varepsilon_{ijt},$$

where the estimate of τ_2, τ_3, τ_4, and τ_5 represent changes in length of stay over time that affect all inpatients, and the estimates of $\gamma_2, \gamma_3, \gamma_4$, and γ_5 represent changes that are unique to the Medicaid population. All estimates of length of stay are retransformed using the smearing estimator and calculated using predictions from the sample of Medicaid patients. Thus the estimates represent the length of stay of Medicaid patients as though they were treated the same way as patients in the comparison group.

A. Expected Number of Admissions

The estimated change in length of stay due to managed care must be adjusted for the change in the probability of admission, as discussed in section III. We estimate the number of admissions at each hospital using an equation similar to the length of stay equation without fixed effects. This equation includes all patients who had at least some mental health care and so is unconditional, like the first part of a two-part expenditure model. We measure changes in the number of admissions using a zero-inflated negative binomial count data model. To estimate the count data model we first aggregate the inpatient claims by year and include observations of those patients that did not receive inpatient care in a given year. We do not exclude the claims that were reimbursed on a per episode basis in this step so that we measure a full year of claims for each patient. We use the zero-inflated model rather than an unadjusted negative binomial process because 77% of the individuals do not have any admissions in a given year, and thus there are more zeros than would imply a conventional negative binomial. Another reason we use the zero-inflated specification is that we cannot observe whether the individual was sick and a decision not to admit was made or if the individual was simply healthy.

The number of admissions per year is modeled as a function of age, primary diagnosis, comorbidities, and year. The predicted number of admissions for each year represents α^t in equation (3b).

Estimated Number of Days per Year

We retransform the square root of length of stay using a smearing estimator to obtain unbiased estimates of the effect of managed care. White, Park, and Glejser specification tests all rejected the null hypothesis of homoskedasticity by year. However, these tests also implied that there was no heteroskedasticity by age, number of visits, provider, or disease. Therefore, we computed the smearing estimator separately for each year/health plan, as suggested by Manning (1998).

The estimated number of inpatient days are calculated using a transformation that is similar to the standard two-part model commonly used in health economics. Unfortunately, it is not possible to observe the actual admission decision at the episode level in our data, thus we are unable to use a standard probit first stage. Instead, we observe whether there was an admission and the number of admissions in each year and therefore use a zero-inflated negative binomial model in the first stage. Thus, the difference is that the first part is estimated using a zero-inflated negative binomial model rather than a probit model.

Thus the estimated medical expenditure in the base year (1991) for each patient i can be written as

$$\text{(6)}\quad \mathit{Inpatient\ Days}_{i,FY91} = \hat{\alpha}_{i,FY91}[(P_i\hat{\beta})^2 + \hat{\phi}_{FY91}]$$

and for each postyear

$$\text{(7)}\quad \mathit{Inpatient\ Days}_{it} = \hat{\alpha}_{it}[(P_i\ \hat{\beta} + \mathit{Year} \times \hat{\gamma}_t)^2 + \hat{\phi}_t - \mathit{contemporaneous\ trend}_t]$$

where $\hat{\alpha}_{it}$ is the estimated number of admissions in each year, $(P_i\hat{\beta} + \mathit{year} \times \hat{\gamma}_t)$ is the fitted value from equation (4), $\hat{\phi}$ is the additive smearing factor, and *contemporaneous trend* is the adjustment for contemporaneous changes in length of stay estimated using equation (5). The difference between the average estimate in equations (6) and (7) reflects the aggregate effect of the program on inpatient utilization for each year:

$$\text{(8)}\quad \overline{\mathit{Inpatient\ Days}_t^{OLS}} - \overline{\mathit{Inpatient\ Days}_{FY91}^{OLS}} = LOS_\alpha^t - LOS_\alpha^{pre}.$$

When γ_i is estimated using equation (5) with providers and individual fixed effects, then

$$\text{(9)}\quad \overline{\mathit{Inpatient\ Days}_t^{FE}} - \overline{\mathit{Inpatient\ Days}_{FY91}^{FE}} = \mathit{Utilization\ Management\ Effect}$$

for each year, where the superscript *FE* denotes fixed effect estimates. We estimated

TABLE 2
Regression Analysis of Length of Stay

Variable	Ordinary Least Squares ($n=21{,}875$)	Hospital Fixed Effects ($n=21{,}875$)	Hospital and Individual Fixed Effects ($n=21{,}875$)
Constant	3.078** (0.042)	2.420** (0.182)	0.700 (0.615)
FY1992	−0.124** (0.039)	−0.077* (0.031)	−0.100 (0.053)
FY1993	−0.189** (0.041)	−0.097** (0.036)	0.013 (0.091)
FY1994	−0.437** (0.037)	−0.327** (0.029)	−0.211 (.0109)
FY1995	−0.541** (0.044)	−0.369** (0.033)	−0.168 (0.139)
Schizophrenia	0.569** (0.036)	0.652** (0.027)	0.197** (0.051)
Major affective disorders	0.358** (0.033)	0.448** (0.025)	0.164** (0.042)
Comorbidities	0.103** (0.027)	0.169** (0.020)	0.171** (0.033)
Age minus 41	−0.010** (0.001)	−0.006** (0.001)	0.050 (0.031)
Visit number (First, Second, . . .)	−0.006 (0.007)	−0.028** (0.001)	−0.007 (0.007)
Estimates of length of stay adjusted for smearing		$(P_i\hat{\beta} + Year \times \hat{\gamma}_t)$	
FY1991	13.34	12.71	11.64
FY1993	12.03	12.05	11.73
FY1994	10.53	10.64	10.34
FY1995	9.97	10.40	10.60
R^2	0.06	0.16	0.32

Notes: Huber-White SEs at the individual level. Dependent variable is square root of the length of stay. Sample includes only the Medicaid population.
**Significant at the 1% level, *significant at the 5% level.

95% confidence intervals around these estimates by using an empirical bootstrap of the entire system of equations using 1000 repetitions.

VII. RESULTS

Estimates of equation (4) without hospital of individual fixed effects measure the aggregate effect of managed care on conditional length of stay. The estimated length of stay in the base year, FY91, was 13.3 days, 12.0 days in FY93, 10.5 days in FY94, and 10.0 days in FY95 after controlling for individual and disease characteristics (see Table 2, first column). When we include only hospital fixed effects the estimated decline in length of stay during the carve-out is lower (see Table 2, second column). The difference between the two estimates reveals the provider selection effect without considering the effect of funneling all patients to winning hospitals.

Estimates using equation (4) with both hospital and individual fixed effects are used to calculate the utilization management effect. The length of stay in this specification increases by about 0.1 days in FY93, decreases by 1.3 days in FY94, and decreases by 1 day in FY95 (see Table 2, third column). Recall from the discussion that there was a dramatic reduction in number of admissions in FY93. We expect the average severity of illness to be higher in FY93 due to the dramatic decrease in admissions. It appears that the increase in length of stay in FY93 is due to the fact that admission criteria were much more stringent and severity of illness, conditional on admission, was higher. A higher

TABLE 3

Decomposition of Changes in Inpatient Utilization by Year

Column	Length of Stay (Fitted Value from Equation [4] without Fixed Effects, ($P_i\hat{\beta} + Year \times \hat{\gamma}_t$) (1)	Concurrent Reduction in Length of Stay (Fitted Value from Equation [5]: FY91 Fitted Value–Post Year Fitted Value) (2)	Estimated Number of Admissions per Year ($\hat{\alpha}_{it}$ from Zero-Inflated Negative Binomial Count Data Model) (3)	Estimated Number of Days per Year (Equation [6] and [7]: [Column (1) + Column (2)] * Column [3]) (4)	Utilization Management Effect (Equation [9]) (5)	Selection Effect (Column (7) Minus Column (5)) (6)	Aggregate Effect of Managed Care on Utilization (Equation [8]: Year in Column (4) Minus FY91 Value) (7)
FY91	13.34 (13.05, 13.65)	N/A	0.41 (0.39, 0.43)	5.52 (5.21, 5.82)			
FY93	12.03 (11.67, 12.42)	1.19 (0.71, 1.72)	0.13 (0.12, 0.13)	1.67 (1.56, 1.79)	−3.19 (−3.54, −2.82)	−0.66 (−0.91, −0.46)	−3.85 (−4.18, −3.51)
FY94	10.53 (10.34, 10.70)	1.95 (1.52, 2.43)	0.32 (0.30, 0.33)	3.98 (3.74, 4.18)	−0.90 (−1.42, −0.41)	−0.64 (−1.01, −0.23)	−1.54 (−1.93, −1.17)
FY95	9.97 (9.76, 10.16)	2.64 (2.17, 3.25)	0.28 (0.27, 0.30)	3.56 (3.34, 3.79)	−1.08 (−1.62, −0.54)	−0.88 (−1.27, −0.41)	−1.96 (−2.32, −1.55)
Postperiod[a]	10.50	1.99	0.27	3.47	−1.31	−0.74	−2.05

Note: 95% confidence intervals in parentheses.
[a]Weighted average of FY93–FY95.

severity of illness of inpatients in the postperiod has been confirmed by postperiod surveys of physicians (see Callahan et al. [1995] and Beinecke et al. [1997]).

The results using the Medicaid data pooled with the comparison group reveal that there was a contemporaneous downward trend in length of stay in each year. Estimated length of stay fell 1.2 days in FY93, 1.9 days in FY94, and 2.6 days in FY95 (see Table 3, second column). All of these estimates are significantly different from zero at a 5% level. In the underlying regressions used to calculate the contemporaneous trends, the length of stay for Medicaid patients fell by an additional one to three days (results not shown). If we were to choose FY92 as the base year, there would be no significant contemporaneous trends in later years. This implies that the contemporaneous changes between FY92 until FY95 were not as great as those relative to FY91. We use FY91 as the base year for pre–post comparisons throughout the rest of the article because all of the hospitals are on per diem reimbursement in all periods, giving us a stable preperiod. The results using only the Medicaid group (equation [4]) are robust to choosing FY92 as a base year; in fact, the choice of base year only affects the significance of contemporaneous trends.

The results of the zero-inflated negative binomial count data model reveal that the expected number of admissions in the five-year period is FY91 is 0.41. In FY93 the expected number of admissions increased to 0.13 (see Table 3, third column). Expected admissions subsequently increased to about 0.32 in FY94 and 0.28 in FY95. These results are consistent with the incentives of large lump-sum bonuses in the first year of the program. However, the low admission rate was not maintained in subsequent years.

The estimated number of days is calculated using equations (6) and (7). In FY91 the estimated number of days is $5.52 = 13.34 \times 0.41$ (see Table 3, fourth column). The estimated length of stay in FY93 for patients in the managed care program is 12.03. There was a contemporaneous decline in length of stay over this period of 1.19 days. Thus, in FY91 terms the estimated length of stay is 13.22 days ($13.22 = 12.03 + 1.29$). In the count data model, the expected number of admissions in FY93 is 0.13, thus the estimated number of days in FY93 which is $1.67 = 13.22 \times 0.13$. Similarly the estimated number of days is 3.98 in FY94 and 3.56 in FY95. The difference in number of days is −3.85 between FY91–FY93, −1.54 between FY91–FY94, and −1.96 between FY91–FY95 (see Table 3, Seventh column). These declines represent the aggregate effect of managed care on utilization.

We repeat this exercise to estimate the utilization management effect. First, we estimate length of stay from the regression with hospital/individual fixed effects (equation [4]). Using the same adjustments for contemporaneous trends and changes in admission criteria, there was a difference of −3.19 days between FY91–FY93, −0.90 days between FY91–FY94, and −1.08 days between FY91–FY95 (see Table 3, fifth column). These are the estimates of the utilization management effect. The provider selection effect is a residual estimate. It is the difference between the aggregate difference in days and the utilization management effect. The provider selection effect is relatively stable around −0.66 to −0.88 days (see Table 3, sixth column).

VIII. RESULTS OF THE DECOMPOSITION

Price

Next we estimate the bargaining effect and the provider selection effect with respect to price using equation (2). The overall increase in price, calculated using length of stay as weights, is \$1.44 (Table 4). However, this does not mean hospitals did not grant discounts. The managed care organization selected hospitals with relatively high preperiod per diem prices leading to a positive provider selection effect of \$11.66. Though higher-priced hospitals won contracts, prices declined at those hospitals as evidenced by the bargaining effect that accounted for about a \$10.23 per day reduction in price. Both of these effects are significant at the 5% level, but the lower end of the 95% confidence interval for the bargaining effect is only a decline of about \$1.

Number of Days

The aggregate difference in length of stay between the pre- and the postperiod is −2.05 days (see Table 4). This figure is taken from

TABLE 4
Decomposition of Cost Savings

	Preperiod	Postperiod	Difference	Contracting	Bargaining	Utilization Management
Price	$470.59 ($466, $475)	$472.03 ($470, $474)	$1.44 (−$3.43, $6.81)	$11.66 ($6.22, $16.65)	−$10.23 (−$17.43, −$1.17)	0
Annual number of Days (Table 3)	5.52 (5.12, 5.82)	3.47 (3.32, 3.62)	−2.05 (−2.39, −1.72)	−0.74 (−1.10, −0.33)	N/A	−1.30 (−1.80, −0.85)
Implied increase in outpatient and pharmaceutical care	N/A	N/A	$105.33 ($79.46, $152.70)	$38.13 ($15.64, $65.04)	N/A	$67.20 ($43.30, $107.87)
Direct service cost per admission	$2,597.30 ($2,452, $2,738)	$1,638.88 ($1,565, $1,713)	−$853.08 (−$1,004, −$704)	−$258.72 (−$397.44, $86.29)	−$45.98 (−$78.98, $7.65)	−$548.38 (−$755.65, −$353.24)

Note: 95% confidence interval in parentheses.

the last row of Table 3 and reflects the average of the estimates in three postperiod years weighted by total bed days. We adjust the estimate of changes in inpatient expenditures for potentially offsetting increases in outpatient and pharmaceutical care using the results from a patient fixed-effects regression of postperiod outpatient and total pharmaceutical expenditures on the postperiod mean annual inpatient days of the patient. We find that a one inpatient day reduction is associated with a $51.42 increase in outpatient and pharmaceutical care and is significant at the 5% level (results not reported here). Thus aggregate difference in expenditures is $853.08 after adjusting for substitution into outpatient and pharmaceutical care.

Utilization management accounted for a 1.30-day drop in number of days, leading to an adjusted fall of approximately $548.38 per year per individual. The provider selection effect is therefore −0.74 days and an adjusted $258.72 drop in annual inpatient costs. Overall about 30% of the cost savings are attributable to provider selection, bargaining accounts for about 5% of the cost savings, and utilization management accounts for the remaining 65%. Utilization management in the form of preadmission certification dominates the utilization management effect.

To test whether the results were sensitive to the choice of control group, we expanded the control group to all insurers besides Medicaid. This control includes BCBS PPO patients and other managed care patients. Using this control group the adjustment for contemporaneous changes in length of stay is larger, leading to a slightly smaller aggregate difference in expenditures of $817.23. In addition, the contracting effect falls to $183.65 or 22% of the cost savings.

IX. WERE EFFICIENT HOSPITALS SELECTED

Although the provider selection effect contributes to almost one-third of the reduction in inpatient expenditures, it is still unclear whether efficient providers were selected. An alternative hypothesis is that low-quality providers were selected because of their lower cost. To test whether winning hospitals were efficient, we calculated two measures of efficiency from the claims data—the probability that a patient is readmitted at any hospital (including substance abuse and out-of-network providers) within 30 days and the length of time between admissions. If winning hospitals were efficient, then patients discharged from winning hospitals would have a lower probability of rapid readmission and a longer period of time between admissions. The results of this analysis are promising but inconclusive. We found no significant differences between winners and losers in rapid readmissions and length of time between admissions, but the lack of significance may reflect low power rather than differences in efficiency. When we examined the effect of managed care on the two measures we found a significant decline in the probability of rapid readmission in the postperiod when we included patient and hospital fixed effects. However, this result may reflect utilization management rather than efficiency.

X. DISCUSSION

Now that we have decomposed the overall change in annual inpatient expenditures into three effects, the next step is to link these results to social welfare. The provider selection effect, which accounted for 30% of the overall drop in expenditures and almost 40% of the changes in inpatient utilization, has clear social welfare implications if more efficient providers were selected. We believe that more efficient providers were selected, for several reasons. The managed care organization did have an incentive to select efficient providers because utilization management is costly after entering a contract (see Conrad et al. [1996] or Lindrooth [2000]). It is more costly to create efficient providers than to find them. We cannot reject the hypothesis that there was no difference between the winners and losers in terms of rapid readmission and the length of time between admissions. Though the power of this analysis is somewhat weak, it appears to imply that there were no quality differences between winning and losing hospitals, and hospitals with a shorter average length of stay in the preperiod were more likely to be selected.

However, we do not base our conclusions on this evidence alone. Fisher et al. (1998) studied the network and found that hospitals with more experience treating patients with psychiatric disorders and Medicaid patients were more likely to be selected. In addition, teaching hospitals were more likely to win a contract than other hospitals. These characteristics of winning hospitals may be correlated in quality. In addition, one of the aspects of the provider selection effect that was mentioned in the introduction was the existence of a protocol for reintroducing patients into the community. Callahan et al. (1995) found that there was greater availability of diversionary beds and services in the postperiod. These results and the results of other studies on the effect of the Massachusetts program imply that the provider selection effect is likely to be welfare improving (see Beinecke et al. [1997] and Fisher et al. [1998]).

The effect of bargaining—which accounted for about a 5% of the reduction in expenditures—on social welfare is ambiguous. As discussed in the introduction, bargaining with risk-neutrality implies that changes in rates represent a transfer that affects the distribution of welfare, not the size of the pie. If there is cost shifting or managed care monopsony power, social welfare can decline. We do not have strong evidence whether changes related to bargaining increased or decreased social welfare in Massachusetts, thus we conclude the effect of bargaining is ambiguous and small.

The utilization management effect, which accounted for 65% of the reduction in expenditures, may be welfare improving. A survey conducted by Callahan et al. (1995) yields insight into the effect of utilization management on quality of care during the first-year of the managed care program. On a scale of 1 (worse than before) to 5 (better than before), providers rated length of stay decisions versus the preperiod 3.5 and overall utilization management decisions versus other managed care plans 3.45. Access to care versus the preperiod was rated 3.0, virtually at the midpoint. Thus providers rated the utilization management decisions with regard to access and length of stay in the postperiod similar to those that were made by providers in the preperiod. This relatively high score was due to the fact that utilization management was flexible (fexibility was rated at 3.3) and the management team considered physician input. This favorable assessment of utilization management by physicians continued in FY94 according to a follow up study by Beinecke et al. (1997). Based on the results presented here, we predict that this favorable assessment continued into FY95 because we find that there is little evidence of large changes in utilization management over the time period of this study. Thus even though utilization was lowered, the utilization management decisions were acceptable to the physicians.

Ma and McGuire (1998b) did not find a significant utilization management effect in their study of psychiatric patients. In their model of network incentives, they find that providers have incentives to change the way care is given due to the managed care organization's ability to credibly threaten to switch providers in areas with provider competition. Thus utilization management (as they define it) does not drive changes in treatment intensity as much as the threat of switching providers. There is an interesting difference between Ma and McGuire's approach and the one used here. We assert that managed care organizations choose (and reward) providers that are already efficient without explaining why certain providers are efficient. We assert that utilization management changes the way providers treat the patients, whether by rules or monitoring and profiling for future contracts. Ma and McGuire explicitly measure and identify the "rules" part of utilization management in addition to the effect of network incentives (which require profiling and monitoring) due to the desirability of the contract.

A caveat of this analysis is that we do not measure the effect of a change in per diem rates on utilization. A relatively large portion of hospital costs is incurred during the first day or two of the stay, and therefore shorter lengths of stay can be associated with a higher cost per day. Thus, it is more profitable to keep patients in the hospital longer when providers are reimbursed at a per diem rate. This frontloading of costs is a much greater problem for procedures, such as surgery, where the intervention takes place the first day and subsequent days are used for recovery. We study psychiatric patients, where the intervention and costs are more likely to be spread over the entire length of stay. Even so, given this potential bias, our estimates reflect a lower bound of the effect of managed care and the utilization management effect would be slightly underestimated. This type of bias would not affect the contracting effect.

This study has three implications for economic research on managed care. First, our framework decomposes managed care into three separate effects related to the underlying contracts and incentives. "Managed care" is not a unique descriptive phrase, and our article digs beneath the label to understand how managed care affects utilization, expenditures, and quality of care. Our framework can be applied to any managed care setting. Second, we show how to identify each effect separately in empirical work. Empirical work on managed care must go beyond regressing outcomes on a dummy variable for managed care. Third, we relate the results to social welfare. The promise of managed care is high-quality health care and more efficient providers, but most research focuses on expenditures. By relating the results to social welfare, we focus on how managed care affects society, not just each stakeholder. In summary, we hope that by providing a framework for thinking about how managed care affects utilization, expenditures, and quality of care that future research will be able to explain how managed care affects social welfare.

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