

# **Does Contracting Out Increase the Efficiency of Government Programs? Evidence from Medicaid HMOs**

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State governments contract with health maintenance organizations to coordinate medical care for nearly 20 million Medicaid recipients. Identifying the causal effect of HMO enrollment on government spending and health care quality is difficult if, as is often the case, recipients have the option to enroll in a plan. To estimate the average effect of HMO enrollment, this paper exploits county-level mandates introduced during the last several years in the state of California that required most Medicaid recipients to enroll in a managed care plan. The empirical results demonstrate that the resulting switch from fee-for-service to managed care was associated with a substantial increase in government spending but no observable improvement in health outcomes, thus apparently reducing the efficiency of this large government program. The findings cast doubt on the hypothesis that HMO contracting has reduced the strain on government budgets.

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## **1. Introduction**

A central issue in public finance concerns the desirability of contracting public services out to the private sector. Advocates of contracting out argue that private firms are more efficient and are more likely to offer a range of services that will allow each individual to get closer to her preferred bundle. Opponents assert that private firms reduce non-contractible quality and avoid unprofitable clients. As previous researchers have noted, the optimal public-private mix is likely to vary across government services, with garbage collection a strong candidate for contracting out and national defense located at the opposite end of the spectrum (Shleifer, 1998).

Though the extent of private sector involvement has been growing for a number of government services in the U.S., perhaps the most striking recent change has been the increasing role of private managed care organizations in the government's Medicare and Medicaid programs. Rather than directly reimbursing hospitals, physicians, and other health care providers, the federal and individual state governments now contract with HMOs and similar managed care organizations to coordinate medical care for more than one-third of the 70 million beneficiaries of these two programs. In most cases, the managed care plans are paid a fixed amount each month per enrollee. This capitated payment may vary with recipient characteristics but typically does not increase if the recipient's health deteriorates or if she receives intensive treatments.

This shift from fee-for-service to managed care was to some extent motivated by a desire to reduce both the level and the growth rate of expenditures in these two public programs. Taken together, Medicare and Medicaid now account for more than \$450 billion in government spending and represent one of the most rapidly growing components of federal and state budgets. While these two programs provide valuable insurance to two of society's most vulnerable groups – the elderly and the poor – the fee-for-service system of reimbursement introduces significant

distortions in medical care purchase decisions. Because program participants only face a fraction of the price of additional medical care (zero in Medicaid, 20% in Medicare), they are likely to consume some services that provide a benefit substantially below cost.

If managed care plans have information about the value that their Medicare and Medicaid enrollees are likely to place on particular treatments, they may be well positioned to reduce medical care utilization while still providing valuable insurance against adverse health shocks. In the limit, the set of marginal services - those covered by fee-for-service but not under managed care - would yield no health benefit and thus medical care spending could decline with no change in individuals' health outcomes (though presumably with some decline in their utility). Depending on the extent of moral hazard in medical care purchase decisions, the government could potentially lower public medical spending by contracting with organizations that could more accurately measure marginal benefits and costs.

There are two additional channels through which HMOs could lower health care spending. First, because their reimbursement does not increase in response to changes in the health of their enrollees, managed care organizations will have a financial incentive to keep their beneficiaries healthy. Thus plans may use their superior information about the benefits of health care treatments and lifestyle changes to encourage decisions that will lower future demand for medical care services. Additionally, plans may be able to negotiate lower input prices from medical care suppliers, with recent work showing that private managed care plans pay lower prices than their private fee-for-service counterparts for the same services (Cutler, McClellan, Newhouse, 2000). For these reasons, it is possible that managed care plans can lower public medical spending while holding constant or improving health outcomes for beneficiaries of the Medicare and Medicaid programs. Additionally, if the government contracts with multiple

managed care plans, then the resulting competition and the increase in the number of options that program participants have may lead to substantial improvements in medical care quality.

While the set of arguments in favor of contracting with HMOs is extensive, other theoretically appealing hypotheses suggest that shifting Medicare and Medicaid recipients to managed care will either increase government spending or adversely affect health outcomes and thus reduce program efficiency. Because each program insures approximately 40 million individuals, the federal and individual state governments are likely to have more market power than a typical HMO and may therefore be able to negotiate lower prices for medical care. Additionally, to the extent that there are increasing returns to scale in program administration, contracting with multiple plans will increase total administrative costs and reduce the fraction of spending devoted to medical care services. Third, the costs to the government will depend not just on the costs to the managed care organization but also on the extent to which plans markup their bids when competing for Medicaid business. If the HMO market is not particularly competitive, then this markup could be substantial. And finally, because HMOs are reimbursed a fixed amount per recipient that depends on a limited set of observable characteristics, they may have an incentive to provide a mix of services that discourages some individuals from enrolling.

It is therefore largely an empirical question whether the shift from fee-for-service to managed care has increased the efficiency of these two large government programs. Medicare is a federally funded and administered program in which recipients have the option to enroll in an HMO but are not required to do so. Identifying the effect of managed care enrollment in this program is difficult for two reasons. First, because virtually all Medicare recipients can enroll in a managed care plan, it is difficult to reliably estimate a causal effect, as those who choose not to enroll in a plan are likely to differ in unobservable ways from those who do not. Second, even if

one looks at changes in a particular recipient's spending or health outcomes following a switch into or out of managed care, it is plausible that a change in health and thus in the demand for medical care services caused the change. In contrast to Medicare, the Medicaid program is administered by state governments and there is substantial variation across states and localities in the rules that influence Medicaid managed care enrollment. Furthermore, many Medicaid recipients are required to enroll in an HMO. Thus both of the obstacles to identification are less of an issue for this program.

In this paper, I estimate the effect of HMO contracting on both government spending and health care quality by exploiting twenty county-level Medicaid mandates passed in the state of California during the 1990s. As a result of the mandates, millions of Medicaid recipients who had previously been enrolled in the fee-for-service Medicaid system were required to join an HMO, and the fraction of program participants in the state covered by a managed care plan rose from 14% to 51%. The number of plans varied across counties, but in all cases the state government paid HMOs a fixed amount per recipient-month and the plans then paid health care providers for medical care services. Recipients continued to bear no financial cost for any services covered by Medicaid – both the deductible and the co-pay remained at zero.<sup>1</sup>

My first set of empirical results, which uses county-level data from 1993 to 2000, strongly suggests that the shift from fee-for-service to managed care was associated with a significant *increase* in government spending. Obtaining a reliable point estimate of the effect of HMO enrollment from the county-level data is difficult, however, because the groups affected by the mandates accounted for a relatively small share of total Medicaid spending. In most counties, welfare recipients were required to enroll in an HMO while the aged, blind, and

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<sup>1</sup> See Ellis and McGuire (1993) and Newhouse (1996) for a discussion of the tradeoffs associated with different degrees of supply and demand-side cost-sharing.

disabled beneficiaries of the SSI program were not. While most Medicaid recipients are eligible through welfare (TANF / Cal Works), SSI recipients are several times more costly on average and account for the majority of program spending.

I therefore next use individual-level data to estimate the average expenditure effect of switching a fee-for-service Medicaid recipient into an HMO. Using a 5% sample of nearly 300,000 welfare recipients with at least one month of Medicaid eligibility between January of 1993 and December of 1999, my findings demonstrate that the average effect of the switch in enrollment induced by the mandates was to increase government spending by approximately 12%. The average expenditure effect of the mandate varies substantially across counties, with increases of more than 20% occurring in those counties that had relatively little pre-mandate managed care penetration and no increase in counties with one-third of their Medicaid recipients voluntarily enrolled in an HMO at the time of the mandate. This heterogeneous effect is consistent with the hypothesis that voluntary HMO enrollment also leads to more Medicaid spending than a pure fee-for-service system.

Whether this increased spending was associated with an improvement in health outcomes is the subject of the third empirical section. The findings there suggest that switching fee-for-service Medicaid recipients into HMOs reduced the avoidable hospitalization (i.e. pneumonia, asthma) rate among children by approximately 30%. But because similar declines were observed for other conditions unlikely to be affected by medical care quality (i.e. accidents), this finding appears not to reflect a real improvement in health. An analysis of infant health outcomes supports this conclusion, as infants born in affected counties following the passage of the mandates are no less likely to die or to be born at low birth weight.

Taken together, the results suggest that contracting with HMOs reduced the efficiency of California's Medicaid program. At a minimum, one can reject the hypothesis that the switch from fee-for-service to managed care reduced the state government's spending below what it otherwise would have been. It is obviously unclear whether these results do generalize to other categories of Medicaid recipients, to other state Medicaid programs, or to the federal Medicare program. But given that the mandates did, to a large extent, mitigate the selection problem, the fact that this switch was still unable to lower spending should give pause to those who believe that contracting with HMOs is an appropriate way to reduce current and future public medical spending.

The outline of the paper is as follows. The second section presents a simple theoretical framework for identifying the effect of contracting out a government program. Section three provides background on the Medicaid program and on the mandates introduced during the last several years that have required most Medicaid recipients to enroll in an HMO. The fourth section presents both county and individual-level analyses that examine the impact that switching Medicaid recipients from the fee-for-service system to HMOs had on government spending. In section five I explore whether the growth in managed care was associated with a significant change in health outcomes. The final section concludes and discusses whether the findings presented here are likely to generalize to other settings.

## **II. Theoretical Framework**

Consider a government program that provides medical insurance to  $N$  individuals by reimbursing two types of providers (i.e. hospitals and pharmacies) for the costs associated with each individual's medical care. Program participants do not bear any share of the costs for either

type of service. For simplicity assume that each unit of service provided by a provider of type k is reimbursed at a price  $P_{kG}$ . Under this program, total costs to the government will be:

$$(1) C_G = P_{1G} * \sum_{j=1}^N X_{j1}(P_{1G}, P_{2G}) + P_{2G} * \sum_{j=1}^N X_{j2}(P_{1G}, P_{2G}) + A_G$$

with  $X_{jk}$  equal to the units of medical care consumed by individual j at provider type k and  $A_G$  equal to the cost to the government of administering the program. As  $P_{kG}$  increases, the average quality of service type k is likely to increase because firms will compete to serve more program participants. This may also affect the equilibrium quality of the other service type and the total amount of medical care consumed by program beneficiaries. For any pair of prices the health of individual j will be  $H_j(P_{1G}, P_{2G})$ , with the effect of prices on health likely varying across individuals.

Now suppose that the government contracts out the care for n of the N program participants to a managed care organization. Assume for simplicity that current health status does not vary within this group and therefore that each individual's health equals the average health  $H_n = H_n(P_{1G}, P_{2G})$  when all are in the fee-for-service program with prices  $P_{1G}$  and  $P_{2G}$ . If health is perfectly observable then the government could specify in its contract that health for each managed care enrollee be greater than or equal to  $H_n$ . Subject to this constraint, managed care organization M will accept a contract to treat all n patients for an average reimbursement per program participant R that is greater than or equal to  $R_M$ .

Because the number of fee-for-service recipients has fallen and their average characteristics have changed, the government may choose to adjust the fee-for-service prices to  $P_{1g}$  and  $P_{2g}$ . Total costs for the government would then be:

$$(2) C_G = nR + P_{1g} * \sum_{j=n+1}^N X_{j1}(P_{1g}, P_{2g}) + P_{2g} * \sum_{j=n+1}^N X_{j2}(P_{1g}, P_{2g}) + A_g + A_R$$



with  $A_g$  equal to the costs of administering the smaller fee-for-service program and  $A_R$  the corresponding cost for the managed care program. This equation demonstrates that the change in costs following the shift of  $n$  program participants to a managed care organization will depend on three factors – the change in costs for the  $n$  managed care enrollees, the corresponding change for the  $N-n$  remaining fee-for-service recipients, and the change in total administrative costs  $A_g + A_R - A_G$ . If the government does not change its fee-for-service prices and the supply of medical care services to the  $N-n$  fee-for-service participants is unaffected by the managed care contract, then only the first and third factors will matter for the cost calculation.

The cost to the managed care organization of coordinating medical care for the  $n$  program participants and ensuring that their average health status remains at or above  $H_n$  will depend on the prices that they pay to both types of providers and the number of units of each type of service that enrollees consume. In contrast to the government’s role in the fee-for-service system, the managed care organization can directly affect the quantity of medical care consumed by choosing not to authorize services that are not considered cost-effective. Thus costs for the managed care organization will be equal to:

$$(3) C_M = P_{1M} * \sum_{j=1}^n Z_{j1}(P_{1M}, P_{2M}) + P_{2M} * \sum_{j=1}^n Z_{j2}(P_{1M}, P_{2M}) + A_M$$

with  $P_{1M}$  and  $P_{2M}$  equaling the price paid to each type of provider,  $Z_{j1}$  and  $Z_{j2}$  representing the number of units of each type of good consumed by individual  $j$ , and  $A_M$  the costs of administering the program.

For at least three reasons, the costs to a managed care organization of coordinating medical care for the  $n$  program participants may differ from the corresponding cost for the government. The most obvious reason is that the managed care plan may reduce medical care utilization by discouraging services that are not cost-effective and encouraging those that are.

Additionally, plans may end up negotiating prices  $P_{1M}$  and  $P_{2M}$  that are quite different from the government's fee-for-service prices of  $P_{1G}$  and  $P_{2G}$ . And finally, the costs of administering the program may also differ across the two types of plans.

One final factor that will influence the expenditure effect of contracting with managed care organizations is the difference between the contractual amount  $R$  and the average cost  $C_M$ . If plans mark up their bids substantially above their expected average cost, then it may prove to be more costly to contract with a managed care organization, even if their costs are equal to or lower than the government's.

This discussion assumed that quality (i.e. health status of recipients) is perfectly observable and that the government writes the contract holding health care quality constant. An alternative and equally plausible view would be that the government sets spending and then allows plans to submit bids based on quality. In reality, both spending and quality are jointly set in a typical contract, and some elements of quality may not be contractible. Thus one must examine the change in both government spending and health outcomes when assessing the desirability of contracting with managed care organizations.

Ideally, one would trace out the relationship between spending and health outcomes for both fee-for-service and managed care. The goal of the subsequent empirical work is considerably more modest. In it, I estimate the changes in government spending and health outcomes associated with the switch from fee-for-service to managed care.

### **3. Medicaid Managed Care Background and Empirical Framework**

The Medicaid program currently provides health insurance to nearly 6.5 million individuals living in the state of California. Program participants are a diverse group, including newborn infants, the institutionalized elderly, and low-income individuals in dozens of other

eligibility categories. Until recently, the majority of Medicaid recipients were enrolled in a fee-for-service program, with the state of California directly reimbursing hospitals, physicians, pharmacies, and other health care providers for the costs associated with their medical care. Perhaps because of dissatisfaction with the fee-for-service system, the state of California recently shifted to contracting with HMOs to coordinate care for more than half of the state's Medicaid recipients. From 1991 to 1999, the share of Medicaid recipients enrolled in a managed care plan increased from less than 12% to more than 51%. As Table 1 demonstrates, this increase tracked the national increase quite closely, with the corresponding figures for the U.S. as a whole standing at 10% in 1991 and 56% in 1999. Under this system, HMOs that contract with the state of California to coordinate medical care for Medicaid recipients are paid a fixed amount per recipient-month that varies across Medicaid eligibility categories.

While the growth at the state level proceeded quite smoothly during this time period, there was substantial variation across local market areas in the pace of managed care penetration. This variation was primarily driven by differences across counties in the timing of Medicaid managed care legislation. Beginning with Sacramento in April of 1994 and most recently with Monterey in October of 1999, Medicaid recipients in the state of California were required to enroll in an HMO. The groups affected by the mandates and the number of plans from which recipients could choose varied substantially across counties.

#### **A. The Three Models of Medicaid Managed Care**

There are three main strategies that California has employed to shift its Medicaid recipients into managed care plans. In the first, which is referred to as Geographic Managed Care (GMC), the state government contracts with several commercial HMOs to coordinate care for Medicaid recipients. Plans apply by submitting a menu of prices at which they would be

willing to insure each type of Medicaid recipient. The government then awards contracts to the six or seven plans most likely to deliver high quality medical care at a low price, though the weight placed on quality and expenditure are not specified. The first county to switch its Medicaid recipients to managed care under this model was Sacramento, where welfare recipients and several other categories of Medicaid recipients were required to enroll in a plan beginning in April of 1994. Those affected by the mandate accounted for nearly 70% of Medicaid recipients but a smaller percentage of program spending. Aged, blind, and disabled Medicaid recipients were not required to join an HMO but had the option to do so, while a small share of recipients (i.e. non-resident aliens) were not allowed to enroll in a plan.

At the time of the mandate, approximately 8% of welfare recipients were voluntarily enrolled in a managed care organization. This share increased steadily during the next several months, but never reached 100% (see Figure 1). There were three reasons for this. First, it typically took a few months for newly eligible Medicaid recipients to become enrolled in a plan. Second, foster children receiving welfare payments were not required to join a plan. And third, a small percentage of recipients were given waivers from the mandates and allowed to remain in the fee-for-service system. Despite this, the increase from less than 10% to more than 80% in just six months was quite a significant change.

An even more sharp change occurred in the county of Solano, the first county to switch its recipients into a county-organized health system (COHS) during the time period of interest,<sup>2</sup> as is shown in Figure 2. Under this model, the not-for-profit, community-based HMO is reimbursed a fixed amount per recipient-month that varies by eligibility category. In contrast to

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<sup>2</sup> Medicaid recipients in Santa Barbara and San Mateo had been required to join a county-organized health system beginning in 1983 and 1987, respectively. I focus on Solano, though, because my data do not begin until 1993.

Sacramento, recipients do not have a choice of plan in COHS counties. In addition to welfare recipients, the aged, blind, and disabled are required to enroll in the managed care plan.<sup>3</sup>

The final type of Medicaid managed care that was utilized in California involved competition between one commercial plan and one not-for-profit, Medicaid-only HMO. Alameda was the first county to switch its recipients into managed care according to this “Two-Plan” model. As displayed in Figure 3, the resulting increase in the fraction of recipients enrolled in an HMO was significant, though a bit less rapid than the corresponding increase in Sacramento. Welfare recipients were required to enroll in one of the two plans, though the aged, blind, and disabled had the option to join. Table 3A summarizes how each category of Medicaid recipient was affected by the three types of managed care mandates.

Table 3B lists the counties that introduced Medicaid managed care mandates during the 1990s and the dates when each of the mandates was imposed. The final column gives the fraction of Medicaid recipients that were voluntarily enrolled in a managed care plan immediately before the mandate took effect. If these program participants did systematically differ from those still in fee-for-service at the time of the mandates, then the average effect of the mandate may vary across counties even if the average effect of HMO enrollment does not.

Three additional points are worth emphasizing. First, the set of counties chosen for the mandates is by no means a representative sample of California’s counties. Eighteen of the twenty largest counties in the state required Medicaid recipients to join an HMO while only three of the smallest thirty-eight disproportionately rural counties did. Part of the reason for this may be that HMOs already had a substantial private market presence in the larger counties but virtually none in the rural areas of the state. Second, the type of managed care chosen in each county was not random. Sacramento and San Diego were designated GMC counties by the state

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<sup>3</sup> In most COHS counties, long-term care services are “carved out” of the managed care contracts.

at least partly because the “safety net” providers there were not as reliant on Medicaid business, and especially Disproportionate Share funds, as their counterparts in counties like Los Angeles and Riverside. And finally, the timing of the mandates is unlikely to have been strictly exogenous. It is therefore possible that policymakers passed the mandates precisely when health care spending was projected to accelerate or the average Medicaid recipient was becoming less healthy. All three of these issues should be kept in mind when interpreting the subsequent empirical results.

## **B. Medicaid Spending and Health Outcomes**

Those enrolled in a managed care plan typically have no paid fee-for-service claims. It is therefore not surprising that the increase in managed care enrollment has coincided with a decline in the relative importance of fee-for-service payments in the Medicaid program. In 1989, the \$6.8 billion in fee-for-service expenditures accounted for 73% of total Medicaid spending (see Table 2). This fraction declined to 53% by 1999 (64% if hospital disproportionate share payments are included), with the corresponding shares for capitation payments increasing from 4% to 15%. The first aim of this paper is to determine whether part of the slowdown in Medicaid spending during the last several years (from 16% annually in the 1989-93 period to less than 5% per year during the sample period) was caused by the increase in HMO enrollment.

I begin by using annual, county-level data for the 1993 to 2000 time period to investigate whether expenditures changed significantly following the shift of Medicaid recipients from fee-for-service to managed care plans. I utilize the passage of a county managed care mandate as an instrumental variable to estimate the causal effect of switching Medicaid recipients into an HMO. The identifying assumption of this empirical approach is that the mandate is related to

government spending only through its effect on plan enrollment. I discuss potential problems with this assumption below. The baseline empirical specifications are as follows:

$$(4) \text{ManCare}_{kt} = \alpha_1 + \gamma_1 * \text{Mandate}_{kt} + \mu_1 X_{kt} + \theta_{1k} + \lambda_{1t} + \varepsilon_{1kt}$$

$$(5) \text{Spending}_{kt} = \alpha_2 + \gamma_2 * \text{Mandate}_{kt} + \mu_2 X_{kt} + \theta_{2k} + \lambda_{2t} + \varepsilon_{2kt}$$

with k and t indexing counties and years, respectively. The IV estimate of the effect of managed care enrollment on government spending is simply the ratio  $\gamma_2 / \gamma_1$ .

The expenditure data in equation (5) includes county-specific fee-for-service, capitation, and disproportionate share hospital payments but exclude the other expenditure categories listed in Table 2, which together account for approximately 20% of California’s Medicaid spending. Two of these categories – miscellaneous non fee-for-service and Medicare buy-in – are paid out almost exclusively on behalf of aged, blind, and disabled SSI recipients and thus are unlikely to be affected by mandates that differentially impacted welfare recipients. More importantly, these services are “carved out” of most managed care contracts for SSI recipients. Similarly, dental and community mental health services (covered by the Short-Doyle program) are also not included in the state’s managed care contracts. As long as managed care coverage is not affecting payments to these other two types of providers, the absence of this data will not be problematic for the county level analysis.

The lack of data for the costs to the state of administering Medicaid for each county’s Medicaid recipients is a cause for some concern. It is likely that the switch from fee-for-service to managed care affected the total cost to the state government of administering the program. Two opposing forces make the sign of this effect ambiguous. The state no longer disburses fee-for-service payments for most Medicaid recipients, but must now contract with and regulate a new set of organizations. The fact that administrative expenses continued to rise by 7% per year

from 1993 to 1999 despite a 40% reduction in the number of individuals enrolled in the fee-for-service program suggests that the switch to managed care did not significantly reduce administrative costs for the state and may actually have increased them. Given these opposing forces and the fact that administrative costs account for less than 5% of total program spending, it seems unlikely that the absence of this data will have a significant impact on my estimates of the expenditure consequences of contracting with managed care organizations.

I then use a five percent sample of Medicaid recipients to estimate an analogous set of individual-level regressions. The California Department of Health Services (DHS) maintains a database that contains all fee-for-service payments made on behalf of each Medicaid recipient. These claims include payments to hospitals, long-term care facilities, pharmacies, physicians, and many other types of health care providers. In a typical year, there are more than 150 million fee-for-service claims. Additionally, the DHS tracks each recipient's eligibility status in every month, and its eligibility file provides data on whether a recipient is enrolled in a managed care plan or is instead in the fee-for-service system. This eligibility data can be matched to another DHS data source that contains plan-specific reimbursement rates by month and Medicaid eligibility category. I use this information together with the fee-for-service claims to calculate individual-specific Medicaid spending for medical services received by the recipient in each year. My sample includes information for 634,284 individuals with at least one month of Medicaid eligibility between January of 1993 and December of 1999.<sup>4</sup>

There are two important advantages to using this individual-level managed care enrollment and expenditure data. First, it allows me to focus primarily on welfare recipients - the group that was differentially affected by the mandates - who are much less expensive than are

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<sup>4</sup> This implies that nearly 12.7 million Californians were eligible for Medicaid at some point during this seven year time period.



recipients in other Medicaid eligibility categories. This is shown in the following table, which compares expenditures for three different categories of Medicaid recipients in the five percent sample in 1993. In all of the counties that introduced a Medicaid managed care mandate during the time period of interest, individuals eligible for welfare payments were required to enroll in an HMO. Recipients in the second largest major eligibility category - aged, blind, and disabled beneficiaries of the SSI program - had the option to join a plan but were not required to do so in most counties. Average costs per month for this group are approximately six times greater than for welfare recipients.<sup>5</sup>

Eligibility Category	# Eligible 1993	Avg. Paid 1993	Avg. Months 1993
All Recipients	332,302	1613	9.4
Welfare	197,698	718	9.6
Aged, Blind, Disabled	65,228	4538	10.6
All Others	69,376	1416	7.5

**Table A: 1993 Medicaid Expenditures by Eligibility Category**

A second advantage of the individual-level data is that the county-level expenditure information is calculated based on the year of payment rather than the year that the service was received.<sup>6</sup> A substantial share of spending in a year is made for services provided in previous years. For both reasons the individual-level data will allow me to more reliably isolate the effect of the increase in HMO enrollment induced by the mandate.<sup>7</sup> To the extent that the mandates may have affected spending for other eligibility categories (i.e. the aged, blind, and disabled), the focus on welfare recipients will not be complete and thus the county-level specifications will have an advantage.

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<sup>5</sup> The two most common group in the “all others” category are OBRA aliens and medically indigent children. The first group was not allowed to enroll in a managed care plan while the second group typically was. I intend to include the medically indigent in subsequent individual-level analyses.

<sup>6</sup> The county data is from administrative sources. I intend to calculate county-level spending from my 5% sample in a future version of this paper – thus this second reason will no longer be a factor.

<sup>7</sup> One limitation of the individual-level data is that I do not have person-specific disproportionate share hospital payments. Using the county level data, I test whether the absence of this information is likely to bias my individual-level estimates of the expenditure effects of HMO contracting and find that it does not.

Once an individual enters a managed care plan, the Medicaid claims data is no longer informative about the individual's health status. It is therefore not possible to identify the effects of HMO enrollment on health care quality without a different source of data. In my analysis of health outcomes, I utilize hospital discharge data. Though the available data does not allow me to link a specific hospital patient with a particular Medicaid recipient, I can investigate whether average health outcomes changed in an area following the passage of a managed care mandate. The specific outcomes used are described in section five below.

#### **4. The Effect of HMO Enrollment on Medicaid Spending**

In this section, I utilize both county and individual level Medicaid expenditure data to investigate whether government spending changed substantially following the shift of Medicaid recipients into HMOs. From January of 1993 to December of 1999, the fraction of Medicaid recipients enrolled in an HMO increased from 13% to 51%. As Figure 4 demonstrates, the increase was even greater for welfare recipients, rising from 18% to 76% during the same time period. Virtually all of this increase was driven by the managed care mandates described above.

There are at least three channels through which HMOs could reduce medical care spending. First, managed care plans may reduce resource utilization by authorizing only those services that provide an expected benefit that is at or above the expected cost. Second, the plans may negotiate lower prices from medical care providers, with the potential savings depending on the Medicaid fee schedule that was in place at the time of the mandates. And third, the HMO has a financial incentive to keep individuals healthy and may therefore encourage cost-effective preventive care and lifestyle changes that will reduce the future demand for health care services.

But there are other reasons to think that contracting with HMOs will lead to an increase in government expenditures. Most managed care organizations have fewer beneficiaries than the

Medicaid program and thus may have less market power to negotiate low rates from suppliers of medical care.<sup>8</sup> Additionally, the total costs of administering the program may increase as a result of the “partial privatization” of the program, with the government still administering a fee-for-service program and now having a new set of institutions to regulate. The HMOs must also pay to administer their Medicaid managed care programs and thus the fraction of Medicaid costs devoted to program administration could rise substantially. And finally, the costs to the government will depend not just on the costs to the managed care organization but also on the extent to which plans markup their bids when competing for Medicaid business. If the HMO market is not particularly competitive, then this markup could be substantial.

#### **A. County-Level Expenditure Effects of the Increase in HMO Enrollment**

To assess whether the introduction of the managed care mandates coincided with a significant change in government spending, I begin by running regressions analogous to (4) and (5) at the county level. The specifications in the county level regressions use observations for all 58 California counties in each year from 1993 to 2000 and include county and year fixed effects in all cases. Summary statistics for the variables used in this section are included in Table 4C.

The dependent variable in Table 4A is equal to the fraction of Medicaid eligible months in county  $j$  in year  $t$  in which recipients were enrolled in an HMO. The explanatory variable  $\% Mandated$  equals the fraction of Medicaid eligible months that were affected by the mandate.<sup>9</sup> This variable will differ across counties for three reasons. First, at a point in time certain counties will have a mandate in effect while others will not. Second, the Medicaid eligibility categories required to join an HMO vary across counties. For example in COHS counties the

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<sup>8</sup> A number of models predict that price discounts will increase with the market share of the buyer. See Stoles and Zwiebel (1996) for the case of a monopoly supplier and Snyder (1998) for an example with competing suppliers.

<sup>9</sup> Thus if there are 100 Medicaid recipients eligible for the entire year and 70 are required to join an HMO as of October 1, the value for  $\% Months Mandated$  will be  $0.7 * 0.25 = 0.175$ .

aged, blind, and disabled are required to join a plan whereas in Two-Plan and GMC counties they are not. And finally, the distribution of Medicaid recipients across eligibility categories is not identical across counties. For example, a Two-Plan county with relatively many SSI recipients as a fraction of its Medicaid population will, all else equal, have a lower value for % *Mandated* than will another Two-Plan county with relatively few SSI recipients.

The statistically significant first-stage estimate of 0.563 demonstrates that the mandates induced a substantial increase in the share of Medicaid recipients enrolled in an HMO, though the relationship is not one-for-one for the reasons mentioned in the third section above. Including county-specific time trends in this regression does not affect the estimate substantially, which falls to 0.507 but remains statistically significant at the 1% level. A comparison of the estimates for the % *Aged, Blind, and Disabled* coefficient in the first two columns suggests that the specification with county time trends may be the more reliable one, as SSI recipients are much less likely to be in a managed care plan but the first specification suggests the opposite.

In the third and fourth specifications I interact the % *Mandated* variable with the fraction of Medicaid recipients that were voluntarily enrolled in an HMO in the month before the mandate. If a fraction  $\alpha$  of recipients are in a managed care plan at this time, then the maximum effect of the mandate on HMO enrollment is to increase this fraction by  $1-\alpha$ . As expected, the estimate for the % *Mandated* \* *Initial* coefficient is significantly negative and its inclusion leads to an increase in the estimate for the % *Mandated* coefficient, which rises to 0.78.

Using these two variables as instrumental variables for managed care enrollment, I then investigate the relationship between the HMO enrollment induced by the mandates and county-level Medicaid spending in Table 4B. The first four columns use total spending per eligible as the dependent variable, while the latter four instead use the log of this value. The first two

*Spending* specifications in columns (1) and (2) use only the *% Mandated* variable as an instrument and yield conflicting estimates for the *% Managed Care* coefficient. The difference between the two specifications is that the first one does not include county time trends while the second one does. The inclusion of these trends causes the estimate to change from small in magnitude and insignificantly negative to significantly positive. A similar pattern is observed for the *Log (Spending)* specifications in the fifth and sixth columns. Thus even if one accepts the assumption that the mandates provide plausibly exogenous variation in managed care enrollment, it is clearly difficult from this first set of specifications to determine the magnitude or even the sign of the expenditure effect.

In the next set of specifications, I use both the *% Mandated* variable and its interaction *% Mandated \* Initial* to instrument for HMO enrollment. The inclusion of this variable allows the average effect of the mandates to vary across counties depending on what share of recipients were initially in a managed care plan. If the expenditure effect of HMO enrollment is similar across individuals then one would expect to observe a smaller change in spending in counties with relatively many already enrolled when the mandate takes effect. By adding this second instrument, the estimates for the specifications with and without county time trends are much closer in magnitude and both are significantly positive. A similar pattern is observed for the analogous *Log (Spending)* specifications, though only one of the two is significant.

It therefore appears from this first set of results that the mandates were associated with a significant increase in government spending, though the results are not fully conclusive. It is worth noting that this effect, if properly estimated, could be driven by a number of different factors. For example, the state may be paying HMOs more for the groups that are required to enroll in an HMO. Alternatively, plans may be selecting relatively healthy individuals among

the groups that are allowed, but not required, to join an HMO and thus expenditures may increase due to imperfect risk adjustment. It is also possible that the state increased provider reimbursement rates to ensure continued access for fee-for-service participants, who would now represent a smaller share of the market. And finally, fee-for-service providers may increase the intensity with which they treat those remaining in the system if managed care plans contract with a different network and the fee-for-service providers try to make up their lost income. I focus on the first of these channels in the next section but all four are potentially relevant for a reliable estimate of the full expenditure effect of the HMO mandates.

One potential concern with this set of specifications is that it includes California counties that have never required their Medicaid recipients to enroll in managed care plans. These areas are smaller, less densely populated places that may not represent appropriate controls for the larger urban counties that introduced managed care. In a separate set of specifications not reported here I show that excluding the 37 counties without a mandate does not have an important effect on my estimates. Additionally, because my subsequent individual-level analyses do not include hospital disproportionate share payments, I run the identical set of county-level specifications excluding these expenditures from the analysis. The results are quite similar to the ones reported here.

In Table 5A I replicate the specifications summarized in Table 4A but differentiate between the three types of HMO mandates that were introduced in California. The first specification suggests that the mandates had the sharpest effect on HMO enrollment in those counties that shifted recipients into county-organized health systems. This is consistent with the graphs of managed care enrollment in the three “pioneer” counties described in section three. While Solano’s HMO enrollment (in the COHS) increased from 1% to 100% in just one month,

the increase was slower in both Sacramento (GMC) and Alameda (two-plan) counties. The negative coefficient estimate for the fraction of Medicaid recipients who are aged, blind, and disabled has the expected sign and is statistically significant at the 10 percent level. The estimates are not significantly affected by the inclusion of county-specific time trends but all three estimates increase when I control for the pre-mandate managed care enrollment.

In Tables 5B and 5C I attempt to estimate the difference in the expenditure effect across the three different types of counties. Theoretically, one might expect to observe a heterogeneous effect if there are increasing returns to scale in program administration, if the number of enrollees affects the plans' bargaining power with providers, or if the possibility that an enrollee will switch to another plan weakens the incentive to reduce expected future health care utilization. Of course given that the choice of plan type is not exogenous one would worry whether any difference is caused by this omitted variable rather than by the number of plans. But given the limited precision of both the reduced form and the IV estimates reported in these two tables, it is not possible to accurately measure this difference. This is perhaps not surprising given that there are relatively few counties in each plan type (2 GMC, 7 COHS, 12 Two-Plan) but I return to this issue below using the individual-level data.

Because the groups required to join an HMO in most counties accounted for a relatively small share of Medicaid expenditures, filtering out the signal from the noise in the county-level regressions is clearly difficult. In the section that follows, I use individual-level data and focus on welfare recipients to estimate the average effect of HMO enrollment on Medicaid expenditures. Focusing on the group with the mandate will obviously not provide a complete picture of the effect of the mandates as there may be spillover effects and groups allowed but not required to join may experience a change in spending because of imperfect risk adjustment or

some related factor. Nevertheless, this will allow me to more reliably estimate the effect of HMO enrollment on government spending.<sup>10</sup>

## **B. Individual-Level Expenditure Effects of the Increase in HMO Enrollment**

In each of the 21 counties with a managed care mandate currently in place, welfare recipients are required to join an HMO.<sup>11</sup> Table 6A demonstrates that the increase in HMO enrollment among welfare recipients in COHS, GMC, and 2-plan counties from 1993 to 1999 has been substantial. While just 20% of welfare recipients in these 21 counties were enrolled in an HMO in January of 1993, more than 85% were by December of 1999. During this same time period, HMO enrollment among welfare recipients in the other 37 counties remained at just 1%.

In this section, I focus on a sample of individuals who are eligible for Medicaid in at least one month between January of 1993 and December of 1999.<sup>12</sup> Additionally, I include in the sample only those individuals who are eligible for Medicaid through current or recent welfare receipt (i.e. not SSI, Medically Indigent, OBRA alien, etc.) in *all* of their eligible months during this period. This sample will therefore consist of the group of Medicaid eligible individuals whose HMO enrollment is most likely to have been affected by the managed care mandates. Of the 634,284 individuals with at least one month of Medicaid eligibility between 1993 and 1999, my sample includes 302,285 individuals. Of the remaining 331,999 people in the five percent sample, 243,074 have exactly zero months of eligibility through TANF / Cal Works while

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<sup>10</sup> If, for example, there are spillover effects that cause expenditures for groups supposedly unaffected by the mandates to experience significant increases following the passage of the mandates. See Baker (1998) for an example of the spillover effects of Medicare HMOs on treatment and expenditures for fee-for-service recipients.

<sup>11</sup> This group of Medicaid recipients includes 33 Medicaid eligibility categories. Some of the individuals are not receiving welfare payments in each eligibility month. For example, as a result of the stricter welfare rules enacted during the 1990s, the state extended Medicaid benefits for many individuals whose welfare benefits were terminated. Foster children on welfare (approximately 2% of the welfare sample) are only required to join an HMO in COHS counties.

<sup>12</sup> Individuals are included in or excluded from the sample based on the last two digits of their social security number. For the 12% of recipients without a valid SSN, the last two digits of the unique Medicaid identifier are used.



88,925 have some – but not all – of their Medicaid eligibility months due to welfare receipt. Finally, I exclude 1907 individuals with data inconsistencies across years (i.e. male in 1993 but female in 1995) and drop the 21,393 individuals who live in San Diego during one or more months of their Medicaid eligibility because I do not have plan reimbursement rates for this county. I am then left with a sample of 278,985 Medicaid recipients.

An important feature of the Medicaid program is that spell lengths are often quite short and vary substantially across individuals. Figure 5 shows the distribution of the number of Medicaid eligible months in my five percent sample. More than 25% of the welfare recipients in the sample have 10 or fewer months of Medicaid eligibility and more than half have less than 30 months. There is a “spike” at 84 months of eligibility – for most of these individuals their spell is both right and left-censored. Additionally, some individuals in the sample have a break in eligibility, with 26% having two or more distinct spells during the 84 month time period.

In the empirical work that follows, I use individual-level data and take a recipient-year as the unit of observation to estimate the effect of the managed care mandates on government spending, weighting each observation by the number of months that the individual is eligible for Medicaid during the year. Because of heterogeneity in the number and duration of Medicaid eligibility spells, the number of observations varies across individuals. This distribution is shown in Table 6B, and my sample has more than 1.04 million person-year observations.

The key identifying assumption of my empirical approach is that the mandates affect Medicaid spending only through an impact on HMO enrollment. This assumption would be violated if, for example, the timing of the mandate in county  $j$  was systematically related to changes in spending that would have occurred in this county, relative to the rest of the state, in the absence of the mandate. It is not possible to rule out this type of legislative endogeneity,

though because the date of each mandate varied substantially across counties and was typically chosen far in advance by the state government, this seems like a reasonable assumption. Furthermore, fee-for-service reimbursement rates for many providers, including physicians and pharmacies, were set at the state, and not at the provider level. This uniformity across market areas will to some extent limit the potential for policy-makers in the state government to forecast when expenditures would increase in a specific county relative to the rest of California.

The results from specifications that explain the effect of the mandates on managed care enrollment are presented in Table 7A. In each specification, both year and 278,985 *individual-specific* fixed effects are included. In this set of specifications, the *% Mandated* variable is equal to the fraction of the *person's* eligible months in which they are required to be in a managed care plan. As was true in the county level regressions, the mandates are associated with a significant increase in HMO enrollment, and the estimated effect increases after controlling for the share of the county's welfare recipients that were enrolled in a managed care plan immediately before the mandate took effect. Specifications (2) and (4) control for the year that the person first appears in the data and interacts this variable with year dummies. Individuals initially appearing in 1993 and still eligible six years later may differ substantially from those appearing only in 1993 or who are first eligible in 1999. The inclusion of this set of interactions has virtually no impact on the coefficient estimates of interest.

The dependent variable in Table 7B is equal to total Medicaid expenditures for medical care services received by the recipient in the year. The reduced form specifications presented in the first and second columns suggest that the mandates were associated with an increase in government spending. According to the estimates from column (2), the average effect of the mandate was significantly lower in counties with a large share of recipients already enrolled in a

plan.<sup>13</sup> This pre-mandate cost difference may have been driven by selection – HMOs attracted individuals healthier than the average and yet were reimbursed based on average costs – or simply because the counties initially paid rates to HMOs that were above average costs.

The results summarized in columns 3 and 4 utilize the *% Mandated* variable to instrument for the share of eligible months that the person is enrolled in an HMO. The point estimates are positive and statistically significant using conventional standard errors, though insignificant after correcting for serial correlation in the error term. Because average spending per year has a mean of \$766 in 1999 dollars in this sample, the point estimates suggest that the HMO enrollment induced by the mandate is associated with a 12-14% increase in government spending.

In the final two specifications I allow the average expenditure effect of the mandate to vary across counties by using both the *% Mandated* and the *% Mandated \* Initial* variables as instruments for HMO enrollment. If the difference between HMO and fee-for-service costs does not vary significantly across counties, then one would expect the average effect of a mandate to differ if some counties initially had just 5% of their recipients in a managed care plan while others had 75%, because the predicted increase in HMO enrollment in the latter county would be much smaller.<sup>14</sup> The point estimates for the *% Managed Care* variable increase substantially and suggest that switching a Medicaid recipient from fee-for-service to managed care was associated with more than a 20% increase in government spending.

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<sup>13</sup> The statistically significant point estimates of 138.1 and -399.6 on the *% Mandated* and *% Mandated \* Initial* variables suggest that the average expenditure effect in counties with 35% of their recipients enrolled in an HMO at the time of the mandate was close to zero. Appendix Table 2 shows that Medicaid expenditure increases were largest in those counties that were closest to a pure fee-for-service system before the mandate.

<sup>14</sup> An alternative is to control for whether the person was voluntarily enrolled at the time of the mandate – I plan to do this in the next version of the paper.

In Table 7C I investigate whether the magnitude of the average expenditure effect did vary systematically across the different types of managed care mandates. Because I am missing data for one of the two GMC counties (San Diego) I group the other one (Sacramento) with the Two-Plan counties. The final specification summarized in this table suggests that the expenditure effect was not very different in COHS and Two-Plan counties, though the point estimate is somewhat larger for the former.

It therefore appears that the mandates passed during the last several years in the state of California have not been responsible for the slowdown in Medicaid spending. The observed expenditure increase could have been driven by a number of different factors. Perhaps the most plausible one is that managed care plans are now paying providers more than they received under Medicaid's fee-for-service systems. One of the largest COHS plans pointed out in a recent annual report that its physicians "receive 140 percent of the Medicaid fee schedule" (Cal Optima, 2000). Presumably plans must also expect to make a profit in order to willingly participate, and thus some of the additional spending may be accounted for by HMO net income.<sup>15</sup> The magnitude of this likely depends on the competitiveness of the HMO market, which may partially explain why counties with relatively little pre-mandate managed care penetration saw the largest increases in government spending. And finally, if there are economies of scale in program administration, then the state government may have a substantial per-recipient cost advantage as it can spread these costs over nearly seven million recipients each year.

While the results presented in this section cast considerable doubt on the hypothesis that managed care is responsible for the slowdown in Medicaid expenditure growth in the nation's most populous state, it says relatively little about the efficiency of the program. In the next

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<sup>15</sup> This same COHS plan reported net income of more than \$35 million from capitation revenues of \$570 million.

section, I explore whether the increase in HMO enrollment has been associated with an observable improvement in health care quality, and then assess the efficiency effects of Medicaid HMO contracting.

## **5. The Effect of HMO Enrollment on Health Outcomes**

There are a number of mechanisms through which HMOs could improve medical care quality for the poor. For example, plans will have a financial incentive to keep their enrollees healthy to avoid future increases in expenditures. Additionally, HMOs may have better information about the quality of local medical care providers than low-income Medicaid recipients do. Third, competition between multiple plans will encourage HMOs to provide high quality medical care to attract potential customers. And finally, plans may use the additional revenues that they receive to give recipients access to better providers than they would have in the fee-for-service system. Alternatively, short spell lengths on the program, incentives to select low cost patients while avoiding expensive ones, and plan dropout could reduce the benefits of contracting with HMOs.

In this section, I investigate whether the growth in HMO enrollment caused by the Medicaid managed care mandates led to a net improvement in health outcomes for the poor. The claims data described above have detailed information about each recipient's health status while in the fee-for-service system, but do not have comparable information once an individual enrolls in an HMO. I thus use a different data source that provides similar data for Medicaid recipients whether they are in a managed care plan or not. Specifically, I use hospital discharge data and administrative data on infant health to explore whether health outcomes for Medicaid recipients have improved as a result of the increase in HMO enrollment.

The first measure of health outcomes that I use is the number of avoidable hospitalizations among Medicaid-insured children. Previous studies have defined a set of conditions for which hospital admissions among children would fall if the quality of primary care increased (Parker and Schoendorf, 2000). If managed care organizations provide better medical care than individuals obtain under a fee-for-service system, then it is plausible that health would improve as a result of this. Though I am unable to measure the amount of primary care that individuals receive, hospital discharge data does allow me to test whether an increase in HMO enrollment among Medicaid recipients is associated with a significant reduction in the hospitalization rate for avoidable conditions.

The regression results summarized in Table 8A provide some insight into the importance of this effect. In these regressions, the unit of observation is a zipcode-year. I am unfortunately unable to link the discharge data with individual-level data on HMO plan data, but I do know the zipcode of residence for each Medicaid patient admitted to the hospital and for each Medicaid recipient. I therefore test whether the HMO enrollment increases induced by the mandates are associated with a reduction in the avoidable hospitalization rate at the zipcode level, and account for the fact that the mandates are varying only at the county level when calculating the standard errors. The regressions include zipcode fixed effects to control for time-invariant differences across areas in health status or medical care practice patterns and year effects to control for average improvements in health among Medicaid recipients.

The dependent variable in the first column is equal to the log of the number of avoidable hospitalizations among Medicaid insured children in the zipcode-year. This set of conditions includes asthma, pneumonia, other upper airway conditions, gastroenteritis, dehydration, cellulitis, and seizures. The IV estimate for the *% Managed Care* variable implies that the

switch from fee-for-service to managed care induced a 30% reduction in the avoidable hospitalization rate among Medicaid-insured children. Alternative measures of the dependent variable (i.e. the number of hospitalizations or hospitalizations per Medicaid recipient under the age of 15) yield similar results.

This first result suggests that HMOs have improved the health of Medicaid recipients, perhaps by investing in preventive care and steering their enrollees to high quality health care providers when the return to doing so is substantial. But it is also consistent with the hypothesis that HMOs choose to hospitalize fewer individuals with a particular condition. I therefore test whether there is a similar decline in the hospitalization rate for external causes of injury (i.e. accidents, poisoning, etc.), a set of conditions that is unlikely to be affected by managed care organizations. The significantly negative estimate for the % *Managed Care* coefficient in the third column of Table 8A demonstrates that the decline for this other set of conditions has been similarly large, suggesting that the fall in medical care utilization is likely attributable to a change in treatment patterns rather than to a real improvement in health.

In Table 8B I examine whether infant health outcomes appear to have changed following the passage of the HMO mandates by estimating an analogous set of specifications for the fraction of infants born at low birth weight and the infant mortality rate. The advantage of this outcome measure is that all births occurring in California show up and thus it is easier to separately identify the health effect from the change in practice patterns. One disadvantage with this set of specifications is that I am unable to differentiate between Medicaid-insured infants and those with private insurance or without health insurance. If outcomes for these other groups are unaffected by the mandates then this should not systematically bias my findings, though it is important to keep in mind when interpreting the results.

The point estimates for the *% Managed Care* variable in the odd-numbered columns suggest virtually no relationship between the increase in managed care enrollment and the change in infant outcomes. If anything, average infant outcomes have gotten worse following the passage of the mandates, as all three point estimates are insignificantly positive.

The even-numbered columns investigate whether the average change in health outcomes appears to vary across the three types of mandates. The positive and statistically significant point estimates in all three specifications for the *% Managed Care \* GMC County* coefficient indicate that infants born in GMC counties have worse outcomes now than they did prior to the mandates. The corresponding estimates for Two-Plan counties are much smaller in magnitude but insignificantly positive, while all three estimates for the COHS counties are negative.

One possible – but by no means the only - explanation for this pattern of results is that Medicaid recipients in GMC counties are less likely to be consistently enrolled in the same plan. Thus their medical care may frequently be disrupted as they switch from one plan to another. For example, 12% of Medicaid recipients in GMC counties eligible throughout calendar year 1999 who were enrolled in a managed care plan in January had switched plans by December. The corresponding percentages for Two-Plan and COHS counties were 6% and 0.4%, respectively. Having a choice between several different plans obviously has many potential advantages, but the costs may be greater if it substantially increases the ease with which HMOs can encourage unprofitable patients to leave their plan.

While this is by no means an exhaustive set of health outcomes, the results presented in this section strongly suggest that HMO contracting has not led to a substantial improvement in the quality of medical care received by California's Medicaid recipients.



## 6. Discussion

During the 1990s, the fraction of Medicaid recipients in the state of California enrolled in HMOs increased from less than 12% to more than 51%. The results presented in this paper demonstrate that the increased reliance on HMOs led to a substantial increase in government spending and suggest that health outcomes for the poor did not improve. It therefore appears that requiring millions of California's Medicaid recipients to switch out of the fee-for-service system and enroll in HMOs did not lead to an improvement in the efficiency of this government program.

Do the results for welfare recipients generalize to other Medicaid beneficiaries and to Medicaid programs in other states? Because the mandates differentially affected low cost Medicaid recipients, it is certainly possible that the impact for more expensive program participants with a different set of health conditions would not be similar. Additionally, fee-for-service Medicaid reimbursement rates in California are lower than those in the typical state. Thus it is possible that there is more scope for reducing government spending while simultaneously improving quality by contracting with HMOs in other states.

But if policy makers chose not to require other Medicaid recipients to enroll in a plan because they accurately forecasted that the efficiency effects for these other groups would be less favorable, then the average efficiency effects if all recipients had been required to enroll in an HMO would potentially have been even worse. Furthermore, because California's HMO market is the most mature in the nation, the managed care organizations in this state are presumably more efficient than the typical HMO. It is therefore ambiguous whether Medicaid HMO contracting would be more or less efficient in other states.

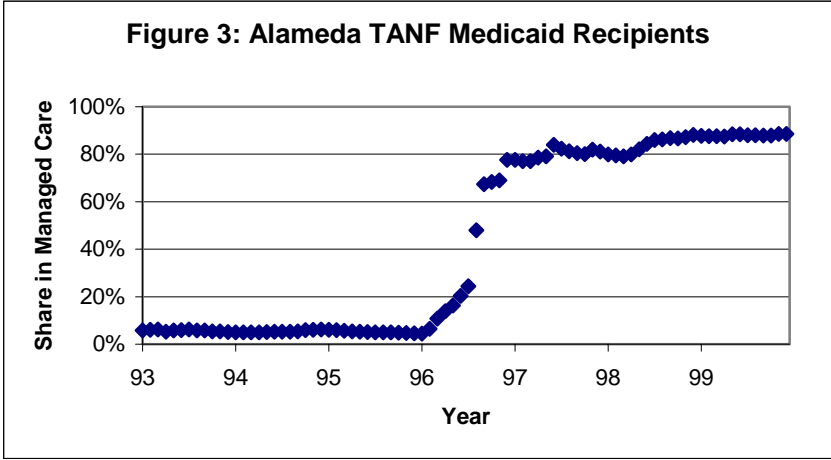
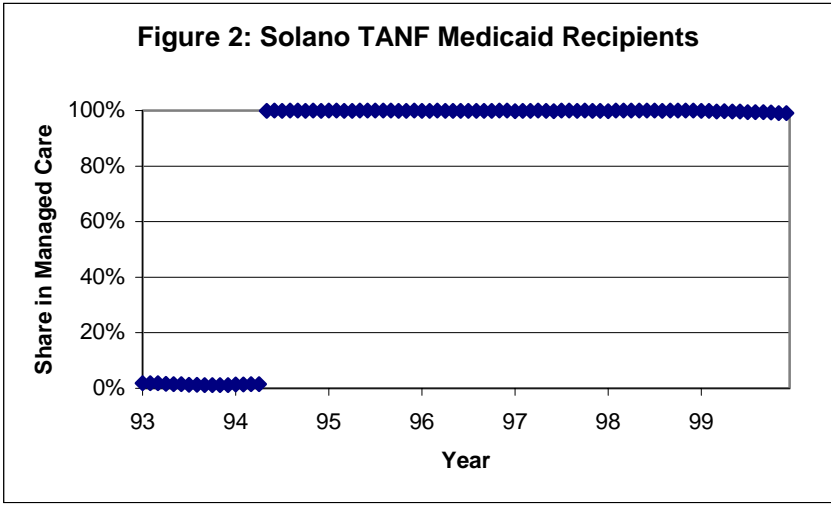
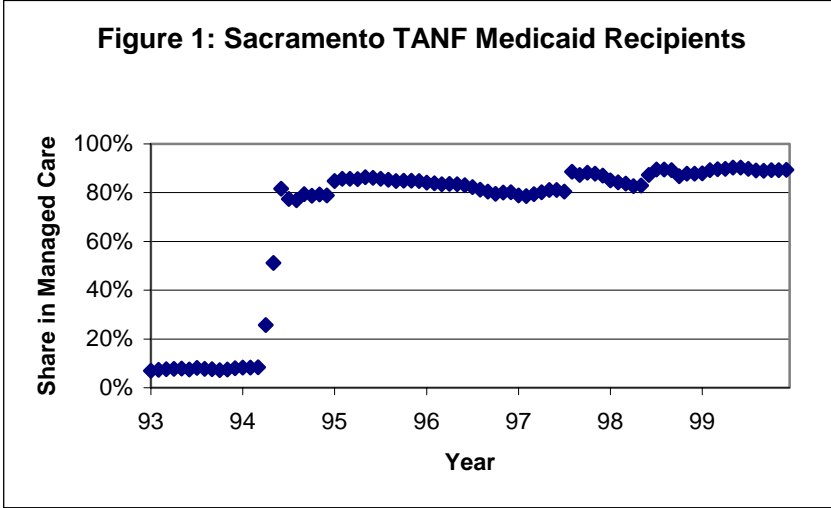
For two reasons, one may expect HMO contracting to be even less effective in the Medicare program. First, because recipients of this program have the option both to enroll in and to drop out of an HMO, managed care organizations may find it more profitable to select healthy enrollees (conditional on observable characteristics) rather than to improve health. Second, the extent of demand-side cost sharing is much greater for fee-for-service recipients of this program than for their counterparts in Medicaid. Thus the fraction of expenditures that have relatively little value to the beneficiary is likely to be lower in Medicare.

The projected increases in public medical spending along with the deteriorating health of federal and state budgets may increase policy maker interest in contracting with HMOs to reduce the strain on government budgets. But the results presented in this paper suggest that managed care organizations are unlikely to deliver cost savings to the government without reductions in medical care quality.

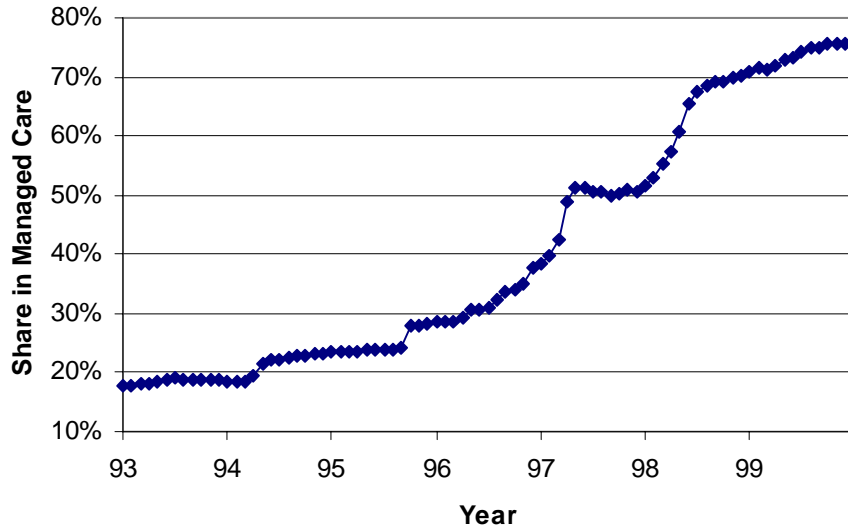
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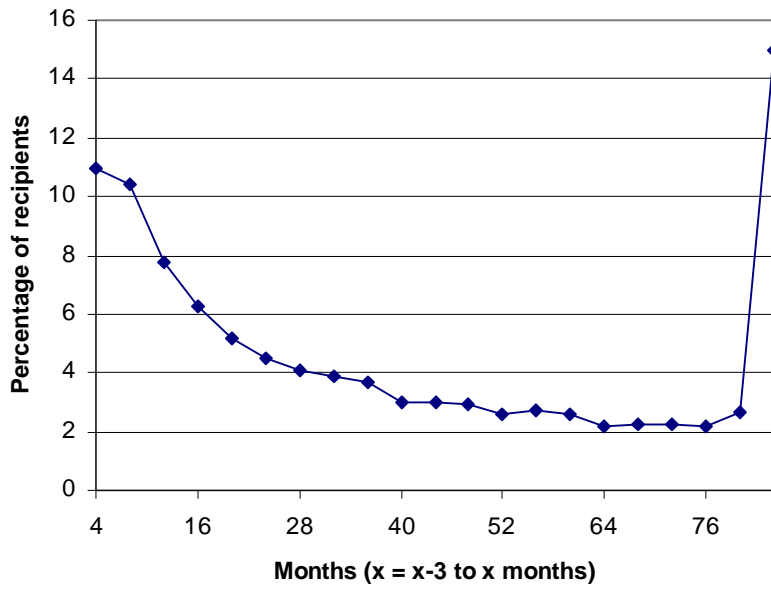
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**Figure 4: California TANF Medicaid Recipients**



**Figure 5: Months of Medicaid Eligibility 1993-99**



**Table 1: Medicaid Managed Care Penetration Rates 1991-2000**

Year	U.S. Medicaid		California Medicaid	
	# Recipients	% Man. Care	# Recipients	% Man. Care
1991	28.3	9.5%	4.02	11.7%
1992	30.9	11.8%	4.49	12.5%
1993	33.4	14.4%	4.83	14.1%
1994	33.6	23.2%	5.01	16.3%
1995	33.4	29.4%	5.02	23.5%
1996	33.2	40.1%	5.11	23.1%
1997	32.1	47.8%	4.79	38.7%
1998	30.9	53.6%	4.90	45.8%
1999	31.9	55.6%	4.97	51.1%
2000	33.7	55.8%	5.04	50.1%

Data were obtained from HCFA publications and from [www.hcfa.gov](http://www.hcfa.gov)

**Table 2: California Medicaid Expenditures in 1989, 1993, and 1999**

Category of Spending	1988-89	1992-93	1998-1999
FFS Provider Payments	6820	10195	10526
Capitation Payments	477	667	2962
Disproportionate Share Hospital	0	2054	2154
Miscellaneous non-FFS	94	280	1108
Administration	297	633	971
Medicare Buy-In	273	414	827
Dental	147*	479	689
Short-Doyle / Medi-Cal	119	244	614
Other and Recoveries	-19	9	-80
Total	\$8208	\$14973	\$19769
Per California State Resident	\$281	\$480	\$601

Values are in millions of 1999 dollars.

**Table 3A: Mandates by Plan Type and Medicaid Eligibility Category**

Aid Category	COHS	2-plan	GMC
AFDC / Cal-Works	Mandatory	Mandatory*	Mandatory*
Aged, Blind, Disabled	Mandatory	Voluntary**	Voluntary**
Medically Indigent Child	Mandatory	Mandatory	Mandatory
OBRA Alien	Varies	Not eligible	Not eligible
All Others	Varies	Varies	Varies

\* Excludes foster children

\*\* Excludes long-term care

**Table 3B: Type and Date of Managed Care Mandate by County**

County	Type of Mandate	Date of Mandate	Pre-Mandate % MC
Santa Barbara	COHS	9/83	-
San Mateo	COHS	12/87	-
Solano	COHS	5/94	1.0%
Orange	COHS	10/95	13.5%
Santa Cruz	COHS	1/96	0.0%
Napa	COHS	3/98	0.0%
Monterey	COHS	10/99	0.0%
Sacramento	GMC	4/94	6.6%
San Diego	GMC	7/98	39.3%
Alameda	Two-Plan	1/96	3.3%
San Joaquin	Two-Plan	2/96	0.7%
Kern	Two-Plan	7/96	0.0%
San Francisco	Two-Plan	7/96	6.7%
Riverside	Two-Plan	9/96	21.3%
San Bernardino	Two-Plan	9/96	23.9%
Santa Clara	Two-Plan	10/96	2.9%
Fresno	Two-Plan	11/96	3.3%
Contra Costa	Two-Plan	2/97	15.4%
Stanislaus	Two-Plan	2/97	0.0%
Los Angeles	Two-Plan	4/97	26.8%
Tulare	Two-Plan	2/99	0.0%



**Table 4A: The Effect of Mandates on Medicaid Managed Care Enrollment**

	% Eligible Months in Managed Care			
	(1)	(2)	(3)	(4)
% Mandated	.563 (.049) [.080]	.507 (.071) [.097]	.775 (.046) [.077]	.778 (.081) [.110]
% Mandated * Initial			-1.392 (.161) [.203]	-1.961 (.340) [.350]
% Aged, Blind, Disabled	.136 (.422) [.774]	-1.439 (.994) [1.039]	.190 (.337) [.609]	-.879 (.640) [.688]
# Observations	464	464	464	464
R-squared	.922	.956	.953	.970
County Trends?	No	Yes	No	Yes

Sample includes county-year observations for California from 1993 to 2000. Dependent variable is equal to the share of eligible months in which Medicaid recipients are enrolled in a managed care plan. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Table 4B: IV Estimates of Effect of Managed Care Enrollment on Medicaid Expenditures**

	Spending per Eligible				Log (Spending per Eligible)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% Managed Care	-39 (106) [183]	296 (108) [140]	180 (90) [136]	258 (92) [111]	-.038 (.043) [.070]	.099 (.045) [.046]	.040 (.034) [.047]	.084 (.040) [.038]
% ABD	8760 (1081) [1873]	9388 (802) [1233]	8732 (1086) [1905]	9340 (785) [1195]	2.281 (.428) [.756]	2.873 (.284) [.424]	2.271 (.431) [.776]	2.854 (.275) [.401]
# Observations	464	464	464	464	464	464	464	464
County Trends?	No	Yes	No	Yes	No	Yes	No	Yes
DSH \$ included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
IV used	1	1	1 & 2	1 & 2	1	1	1 & 2	1 & 2

Sample includes county-year observations for California from 1993 to 2000. Dependent variable in the first four (last four) specifications is equal to (the log of) Medicaid spending (in 2000 dollars) per eligible. Specifications in columns 1,2,5,6 use % Mandated as an IV, while those in the remaining four columns use both % Mandated and % Mandated \* Initial. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Table 4C: Summary Statistics**

	Mean	Std. Dev.
% Managed care	.311	.247
% Mandated	.305	.326
% Mandated * Initial	.047	.067
% Aged, Blind, Disabled	.245	.056
Spending per Eligible	2967	640
Spending per Eligible (no DSH)	2528	534
Log (Spending per Eligible)	7.97	.215
Log (Spending per Eligible (no DSH))	7.82	.198
% COHS Mandated	.059	.223
% GMC Mandated	.036	.146
% 2-Plan Mandated	.210	.282
% Managed Care * COHS County	.059	.214
% Managed Care * GMC County	.046	.146
% Managed Care * 2-Plan County	.206	.208

**Table 5A: The Effect of Mandates on Managed Care Enrollment by Type of Plan**

	% Eligible Months in Managed Care			
	(1)	(2)	(3)	(4)
% COHS Mandated	.818 [.037]	.866 [.041]	.936 [.044]	1.042 [.071]
% GMC Mandated	.555 [.173]	.565 [.293]	.878 [.060]	.934 [.096]
% 2-Plan Mandated	.449 [.064]	.313 [.053]	.665 [.052]	.569 [.077]
% Mandated * Initial			-1.290 [.184]	-1.863 [.257]
% Aged, Blind, Disabled	-.729 [.417]	-1.657 [.912]	-.619 [.483]	-1.269 [.581]
R-squared	.941	.968	.964	.980
County Trends?	No	Yes	No	Yes

Sample includes county-year observations for California from 1993 to 2000. Dependent variable is equal to the share of eligible months in which Medicaid recipients are enrolled in a managed care plan. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Table 5B: Reduced Form Estimates of Effect of Mandates on Medicaid Expenditures**

	Spending per Eligible			
	(1)	(2)	(3)	(4)
% COHS Mandated	118 [114]	114 [162]	254 [97]	140 [154]
% GMC Mandated	118 [125]	160 [54]	488 [168]	215 [101]
% 2-Plan Mandated	-103 [131]	163 [71]	144 [128]	201 [102]
% Mandated * Initial			-1480 [474]	-275 [394]
% Aged, Blind, Disabled	8044 [1678]	8959 [1075]	8171 [1597]	9016 [1110]
R-squared	.955	.984	.960	.984
County Trends?	No	Yes	No	Yes

Sample includes county-year observations for California from 1993 to 2000. Dependent variable is equal to Medicaid spending (in 2000 dollars) per eligible. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Table 5C: IV Estimates of Effect of Managed Care Enrollment on Medicaid Expenditures**

	Spending per Eligible			
	(1)	(2)	(3)	(4)
% Managed Care in COHS Counties	138 [133]	102 [209]	233 [140]	109 [206]
% Managed Care in GMC Counties	160 [195]	282 [282]	460 [227]	254 [222]
% Managed Care in 2-Plan Counties	-203 [283]	519 [217]	144 [197]	369 [197]
% Aged, Blind, Disabled	7969 [1596]	9821 [1292]	8390 [1835]	9556 [1228]
IV Used	1	1	1 & 2	1 & 2
County Trends?	No	Yes	No	Yes

Sample includes county-year observations for California from 1993 to 2000. Dependent variable is equal to Medicaid spending (in 2000 dollars) per eligible. Specifications in columns 1 and 2 use % *Mandated \* Type of Plan* as instrumental variables, while those in columns 3 and 4 also use % *Mandated \* Initial*. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Table 6A: Change in Managed Care Penetration Resulting from Mandates**

Type of Plan	AFDC		Aged, Blind, Disabled	
	1/93	12/99	1/93	12/99
COHS	29% (n=13425)	100% (n=9744)	26% (n=5357)	94% (n=6600)
Two-Plan	19% (n=107706)	83% (n=99448)	3% (n=38364)	7% (n=44657)
GMC	20% (n=18749)	88% (n=15849)	4% (n=6439)	12% (n=7605)
Other 37	1% (n=18045)	1% (n=14811)	1% (n=7172)	1% (n=7855)

AFDC category includes recipients who are receiving welfare (AFDC / TANF / CalWorks, depending on the year) payments or were discontinued from welfare but are still eligible because of the Edwards versus Kizer court order. Some welfare recipients are eligible only through the state (and not the federal) program. Aged, Blind, and Disabled group primarily consists of beneficiaries of the SSI program. Detailed data on eligibility categories are available from the California DHS publication, "Aid Codes Documentation."

**Table 6B: # of Observations for Welfare Recipients in the Sample**

# Years	# Recipients	% of Total
1	60540	21.7%
2	51356	18.4%
3	35312	12.7%
4	26657	9.6%
5	23951	8.6%
6	19452	7.0%
7	61717	22.1%

Number of observations for individuals in the AFDC sample. #Years=t row provides the number of individuals who have t years with at least one month of Medicaid eligibility as a welfare recipient from 1993-1999.

**Table 7A: The Effect of Mandates on Managed Care Enrollment Using Individual-Level Data**

	% Eligible Months in Managed Care			
	(1)	(2)	(3)	(4)
% Mandated	.495 (.053) [.076]	.498 (.053) [.074]	.716 (.057) [.060]	.719 (.058) [.060]
% Mandated * Initial			-1.028 (.151) [.118]	-1.028 (.159) [.118]
Eligible Months	.018 (.002) [.003]	.014 (.002) [.002]	.019 (.002) [.003]	.015 (.002) [.002]
# Observations	1,044,302	1,044,302	1,044,302	1,044,302
R-squared	.758	.762	.775	.779
Person & Year Effects?	Yes	Yes	Yes	Yes
Year Entry * Year Interactions?	No	Yes	No	Yes

Dependent variable is equal to the fraction of Medicaid eligible months in which the recipient is enrolled in a managed care plan. Standard errors in parentheses are clustered by county\*year, while those reported in brackets are clustered by county to account for serial correlation in the error term. Regressions are weighted by the number of eligible months for the individual in the year.

**Table 7B: Effect of Managed Care Enrollment on Medicaid Spending Using Individual-Level Data**

	Medicaid Spending for Individual j in Year t					
	(1)	(2)	(3)	(4)	(5)	(6)
% Managed Care			105.6 (53.2) [74.4]	90.3 (52.4) [79.5]	182.9 (34.7) [39.2]	171.3 (33.7) [39.6]
% Mandated	52.2 (26.4) [36.8]	138.1 (22.7) [26.7]				
% Mandated * Initial		-399.6 (74.7) [48.5]				
Eligible Months	72.7 (1.4) [1.3]	73.2 (1.4) [1.3]	70.9 (1.9) [1.6]	78.4 (2.3) [2.3]	69.4 (1.5) [1.3]	77.0 (1.9) [1.6]
# Observations	1,044,302	1,044,302	1,044,302	1,044,302	1,044,302	1,044,302
R-squared	.497	.497	-	-	-	-
Person & Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Yr Entry * Yr Interact?	No	No	No	Yes	No	Yes
IV Used	-	-	1	1	1 & 2	1 & 2

Dependent variable is equal to total Medicaid expenditures for the individual in the year. Standard errors in parentheses are clustered by county\*year, while those reported in brackets are clustered by county to account for serial correlation in the error term. Regressions are weighted by the number of eligible months for the individual in the year.

**Table 7C: Effect of Mandates on Managed Care Enrollment and Individual Medicaid Spending**

	% Managed Care		Medicaid Spending for Individual j in Year t			
	(1)	(2)	(3)	(4)	(5)	(6)
% Managed Care in COHS Counties					155.6 (36.0) [48.2]	192.2 (30.4) [35.8]
% Managed Care in 2-Plan, GMC Counties					75.1 (56.3) [84.5]	165.6 (36.7) [42.5]
% COHS Mandated	.795 (.042) [.039]	.931 (.047) [.039]	130.2 (29.4) [39.1]	185.0 (30.6) [28.6]		
% 2-Plan, GMC Mandated	.458 (.052) [.070]	.674 (.058) [.054]	34.8 (26.0) [39.0]	121.2 (21.8) [26.2]		
% Mandated * Initial		-.968 (.163) [.137]		-387.4 (66.7) [48.4]		
Eligible Months	.014 (.002) [.002]	.015 (.002) [.002]	79.7 (1.9) [1.6]	80.1 (1.9) [1.6]	78.6 (2.4) [2.4]	77.2 (2.0) [1.7]
# Observations	1,044,302	1,044,302	1,044,302	1,044,302	1,044,302	1,044,302
R-squared	.767	.782	.497	.497	-	-
Person & Year Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Yr Entry * Yr Interact?	Yes	Yes	Yes	Yes	Yes	Yes
IV Used	-	-	-	-	1	1 & 2

Dependent variable is equal to Medicaid expenditures for the individual in the year. Standard errors in parentheses are clustered by county\*year, while those reported in brackets are clustered by county to account for serial correlation in the error term. Regressions are weighted by the number of eligible months for the individual in the year.

**Table 7D: Summary Statistics for Individual-Level Regressions**

	Mean	Std. Dev.
Medicaid Spending	766	2534
% Managed Care	.360	.444
% Mandated	.392	.468
% Mandated * Initial	.085	.141
Eligible Months	9.8	3.5
% COHS Mandated	.053	.299
% Two-Plan, GMC Mandated	.339	.455

**Table 8A: IV Estimates of Effect of Managed Care Enrollment on Child Hospitalizations**

	Log (Avoidable Hospitalizations)		Log (Accidents & Adverse Effects)	
	Medicaid	All	Medicaid	All
% Managed Care	-.332 [.148]	-.166 [.112]	-.296 [.144]	-.218 [.112]
Log(Eligible Months)	.481 [.082]	.376 [.088]	.412 [.052]	.347 [.072]
# Observations	8662	10588	7794	10057
Year and Zipcode Effects?	Yes	Yes	Yes	Yes

Sample consists of zipcode-year observations for 1993-1999. Dependent variable in the first specification is equal to the log of the number of Medicaid insured children admitted to the hospital with an avoidable condition. The dependent variable in the third specification is equal to the log of the number of hospitalizations among children with external causes of injury. Standard errors are reported in brackets and are clustered by county to account for serial correlation in the error term. Regressions include zipcode and year fixed effects and are weighted by the number of Medicaid eligible children.

**Table 8B: IV Estimates of Effect of Managed Care Enrollment on Infant Health Outcomes**

	Fraction LBW		Fraction Very LBW		Infant Mortality Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
% Managed Care	.0012 [.0022]		.0014 [.0011]		.00017 [.00067]	
% Managed Care * COHS County		-.0005 [.0022]		-.0004 [.0004]		-.00055 [.00046]
% Managed Care * Two-Plan County		.0014 [.0028]		.0014 [.0015]		.00018 [.00081]
% Managed Care * GMC County		.0031 [.0017]		.0048 [.0008]		.00157 [.00089]
% Hispanic	-.0076 [.0069]	-.0076 [.0068]	.0021 [.0024]	.0021 [.0024]	-.0036 [.0041]	-.0036 [.0041]
% Black	.1609 [.0816]	.1613 [.0818]	.0580 [.0258]	.0583 [.0261]	.0088 [.0039]	.0089 [.0039]
# Observations	11833	11833	11833	11833	11833	11833
Year and Zip Effects?	Yes	Yes	Yes	Yes	Yes	Yes

Sample consists of zipcode-year observations for 1993-1999. Standard errors are reported in brackets and are clustered by county to account for serial correlation in the error term. Regressions include zipcode and year fixed effects and are weighted by the number of Medicaid eligible children.

**Table 8C: Summary Statistics**

Table 8A	Mean	Std. Dev.	% Infants Hispanic	.515	.256
Log (MC Avoidable)	3.50	1.16	% Man Care	.313	.256
Log (All Avoidable)	4.13	1.01	% Man Care * GMC	.051	.163
Log (MC External)	2.76	1.04	% Man Care * COHS	.048	.180
Log (All External)	3.45	0.90	% Man Care * Two-Plan	.212	.234
Log (MC Eligible)	7.65	1.10	% LBW	.064	.020
Table 8B	Mean	Std. Dev.	% VLBW	.012	.008
% Infants Black	.093	.121	Infant Mortality	.0066	.0049

**Appendix Table 1:  
IV Estimates of Effect of Managed Care Enrollment on Medicaid Expenditures**

	Spending per Eligible				Log(Spending per Eligible)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
% Managed Care	47 (85) [168]	180 (101) [171]	127 (75) [142]	140 (87) [139]	.012 (.036) [.072]	.048 (.048) [.080]	.046 (.031) [.061]	.033 (.042) [.069]
% ABD	5602 (781) [1294]	8363 (898) [1561]	5592 (786) [1310]	8312 (865) [1498]	1.435 (.348) [.599]	2.726 (.333) [.602]	1.431 (.350) [.609]	2.707 (.318) [.572]
# Observations	464	464	464	464	464	464	464	464
County Trends?	No	Yes	No	Yes	No	Yes	No	Yes
DSH \$ included?	No	No	No	No	No	No	No	No
IV used	1	1	1 & 2	1 & 2	1	1	1 & 2	1 & 2

Sample includes county-year observations for California from 1993 to 2000. Dependent variable in the first four (last four) specifications is equal to (the log of) Medicaid spending (in 2000 dollars) per eligible. Specifications in columns 1,2,5,6 use % *Mandated* as an IV, while those in the remaining four columns use both % *Mandated* and % *Mandated \* Initial*. Robust standard errors are included in parentheses, and those reported in brackets are clustered by county to control for serial correlation in the error term. Regressions include county and year fixed effects and are weighted by the share of Medicaid eligible months in a year accounted for by each county.

**Appendix Table 2: Rates Paid to Commercial Plans in Two-Plan Counties**

County	% HMO Pre	Avg. Paid 95	Rate 1999	$\Delta P$
Los Angeles	40.2%	81.21	77.75	-4%
San Bernardino	31.0%	75.78	74.04	-2%
Riverside	30.5%	67.49	75.91	+12%
Contra Costa	22.1%	74.20	78.77	+6%
San Francisco	13.2%	68.65	90.56	+32%
Fresno	5.2%	60.41	75.14	+24%
Alameda	4.7%	60.37	80.30	+33%
Santa Clara	4.4%	61.00	89.09	+46%
San Joaquin	0.9%	51.48	71.14	+38%
Kern	0.0%	64.50	89.09	+23%
Stanislaus	0.0%	53.50	69.95	+31%

Table includes information for two-plan counties. Second column is the percentage of Medicaid recipients eligible through welfare receipt enrolled in a managed care organization in the month before the mandate. Third column is equal to average monthly expenditures for Medicaid recipients on welfare in 1995 in 1999 dollars. Fourth column is the monthly rate paid to the commercial HMO in January of 1999. Final column equals the percentage increase from the average paid amount in 1995 to the commercial rate paid in 1999.