DISCUSSION PAPERS IN ECONOMICS

Working Paper No. 06-01

Making Babies Healthier by Providing a Managed Care Option to California's Poor

Tania Barham
U.C.Berkeley

Paul Gertler
U.C.Berkeley and NBER

Kristiana Raube
U.C.Berkeley

May 1, 2003

Center for Economic Analysis
Department of Economics

University of Colorado at Boulder
Boulder, Colorado 80309

MAKING BABIES HEALTHIER BY PROVIDING A MANAGED CARE OPTION TO CALIFORNIA'S POOR*

Tania Barham
U.C. Berkeley

Paul Gertler
U.C. Berkeley and NBER

and

Kristiana Raube
U.C. Berkeley

May 1, 2003

Abstract

The first chapter investigates whether mandating a managed care option for California Medicaid beneficiaries improves access to prenatal care and birth outcomes in a traditionally fee-for-service system. We compare two competing models: one that only offers a county-organized health system option (COHS), and the Two Plan Model (TPC) that provides mothers with a choice between the county system and a commercial managed care organization. The results show that while COHS improved access, only the TPC program led to reductions in low-birth weight. The superior health outcomes obtained with TPC might be explained by higher quality care induced by competition among health providers and/or mainstreaming Medi-Cal beneficiaries into commercial organizations that also serve higher income populations.

*The authors we like to thank Alain de Janvry, Claudio Ferraz, Frederico Finan, Guido Imbems, Karen Macours, Sebastian Martinez, Craig McIntosh, Xiangyi Meng, Elizabeth Sadoulet for their helpful comments and input. We would also like to thank the California Policy Research Center for their financial support as well as Marcine Craine and Jim Kleine from the California Department of Health Services for their help with the programmatic details about the manage care mandates and data. All mistakes are solely those of the authors and the usual disclaimers apply.

Contact information: Tania Barham Department of Agriculture Economics, University of California Berkeley 94720, email: barham@are.berkeley.edu. Paul Gertler, Haas School of Business, University of California Berkeley, 94720-1900, email: gertler@haas.berkeley.edu. Kristiana Raube, Haas School of Business, University of California Berkeley, 94720-1900, email: raube@haas.berkeley.edu.

1.0 Introduction

During the 1990s, California undertook a sweeping change in its Medicaid program by shifting over half of the beneficiaries from fee-for-service into managed care. In fact, by 2001, over 5.8 million Medi-Cal recipients (52 percent) were enrolled in managed care. California was not alone as 57 percent of all Medicaid recipients nation-wide were in managed care by 2002 (Figure 1).[1] States embraced managed care in part as a solution to control skyrocketing costs following private sector success with using managed care to control cost during the 1980s (Kaestner, 2002), and as a means of improving access to quality primary care for low-income groups (McCall et. al., 2000). Indeed, access to quality care has been a problem for fee-for-service Medi-Cal patients (Menges et. al., 2001; Coburn et. al., 1999). In fact, many pregnant Medi-Cal beneficiaries started prenatal care late and most used providers associated with the county public health care systems.

In this paper, we investigate the impact of California mandating that Medi-Cal beneficiaries be given a managed care option on their access to prenatal care and birth outcomes. We take advantage of the fact that California mandated that Medi-Cal recipients in 22 out of 58 counties be provided with a managed care option (Table 1). In addition, counties began implementing the mandate at different times between 1994 and 2000. This variation in time and geography provides us with a potential instrument for estimating the impact of managed care on the health outcomes of California's poor using a double difference approach.

We also examine the importance of offering an existing mainstream commercial managed care option as opposed a county organized managed care plan, which is a non-

[1] Presently, all states except for Alaska and Wyoming have some form of Medicaid managed care (Kaiser Commission on Medicaid and the Uninsured, 1999).

commercial managed care option that contracts with the network of mainly public providers traditionally used by Medi-Cal beneficiaries. A number of counties implemented County Organized Health Systems (COHS) that provide a single county organized managed care option, whereas others used the Two Plan Model (TPC) that offers choice between a mainstream commercial plan and the county organized option.

Using longitudinal birth record information from California's vital statistics from 1991 through 2001, and paying close attention to the choice of control group, we find that offering managed care reduced the number of low-birth weight and premature births in TPC counties, but not in COHS counties. These findings are especially remarkable given the overall rise in low-birth weight and premature babies during the 1990s. Moreover, that managed care had positive health benefits is important for public policy given that there do not appear to be any cost savings for Medi-Cal from managed care (Duggan, 2002).

These results also highlight the importance of providing a mainstream commercial managed care option to improve the health of babies in poor populations. The commercial groups effectively mainstream Medi-Cal beneficiaries into care equivalent to mothers with higher incomes. Moreover, the commercial option in the TPC model may have applied competitive pressure on the non-commercial managed care option to provide better quality of care.

The paper is organized as follows. In section 1 we describe the pathways by which managed care might influence birth outcomes in poor populations and the existing evidence. We outline the main managed care models in California in Section 2. Section 3 describes the construction of the data, the treatment and comparison groups, and the dependent variables. The identification strategy is explained in Section 4 and Section 5 presents the validity of the

In addition to the incentives to invest in prevention, an additional mechanism by which managed care might improve health outcomes is through provider competition for patients. One-way providers can compete for a patient is by improving the quality of their services. Our analysis sheds light on the importance of competition in managed care on health outcomes. Our findings show that health outcomes improved for the Two Plan Model whereas there was little change in the model without competition, COHS. This suggests that in addition to the preventive care incentives imbedded in capitation, choice and competition are additional important mechanisms by which managed care improves health outcomes.

Past results on managed care's ability in improve quality and access for the commercial population are mixed. (Lufirst68,l5p rviIndn, choic205i on4l

Griffen et. al., 1999). Lastly, Kreiger et. al. (1992) use control groups, but lack pre-intervention data.

Two studies use a double difference approach. (Kaestner et. al., 2002; Duggan, 2002). On the national level, Kaestner et. al. (2002) find that Medicaid managed care has not significantly impacted infant health. The authors perform double difference analyses but have difficulties because they cannot clearly identify which births were insured by Medicaid in their data.

In California, Duggan (2002) uses 1993 to 2000 California hospital discharge data to examine the impacts of the managed care mandates on avoidable hospitalizations among children and infant death. He uses the FFS Medi-Cal counties as a control group and finds that child health status remained unchanged, and that infants were no less likely to die.

Duggan (2002) also evaluates the impact of the shift from fee-for-service to managed care on California state health expenditures. He finds a 15 to 20 percent increase in county Medi-Cal spending was associated with the switch from fee-for-service to managed care. However, he also finds almost no increase in government expenditures in those counties where 35 percent or more of their recipients were already enrolled in an HMO at the time of the mandate, suggesting the cost rise is likely to be temporary.

In these two papers the authors were unable to test if their control group is a valid counterfactual. Although a definitive test is impossible since we will never be able to know what would have happened to the treated had they not been treated, pre-intervention data can be used to test if the slope of the outcomes variables for the treatment and control groups are similar. We therefore cannot be sure if their results are due to the initiation of managed care for the Medicaid population or other factors, which may have caused the groups to have differing trends in the growth rate of the outcome variables. In our paper, we pay careful attention to choosing the correct control group. We identify three possible control groups—the Medi-Cal fee-for-service patients, the un-insured in Medi-Cal managed care counties, and the commercially insured in Medi-Cal managed care counties—and test their appropriateness as counterfactuals using pre-intervention data. We choose different control groups for the COHS and the TPC treatment groups.

3.0 Medi-Cal Managed Care

Medicaid managed care is not new to California, as Santa Barbara began such a program in 1983. However, starting in 1993, the state mandated that 22 out of the State's 58 counties to provide their Medi-Cal beneficiaries with a

The county operates it as an independent public entity that meets the insurance regulatory requirements for pre-paid health plans. This entity contracts with the network of providers who traditionally provided care to Medi-Cal beneficiaries, most of which are in the county public health care system. The California State Medi-Cal Assistance Commission pays the COHS a prepaid capitated rate each month for each Medi-Cal recipient (McCall et. al., 2001).

In 1995, 12 counties were designated to participate in the state's TPC managed care program. Under TPC, the county's Medi-Cal beneficiaries can choose between a commercial managed care plan and a county-developed plan similar to COHS called the local initiative that is intended to preserve the network of traditional safety net pr

4.0 The Data

Our empirical analysis is based on 1991 to 2001 California data from the Birth Statistical Masterfile that is available from the California Department of Health Services, Office of Health Information and Research. This database contains information on all live births reported on birth

Preterm delivery, the birth of an infant before 37 gestational weeks are completed, is the principal determinant of low birth weight and is the factor considered most responsible for the correlated with birth outcomes. For example, richer counties with more health care infrastructure and where birth outcomes were better may have been the ones that received the mandates. In this case, the correlation between managed care and birth outcomes would be confounded with the wealth effect. In principle, many of the types of (unobservable) characteristics that may confound identification are those that vary across counties, but are fixed over time. A common method of controlling for time invariant unobserved heterogeneity is to use panel data and estimate double difference models.

Therefore, without the benefit of a controlled randomized trail, we turn to a double difference approach, which compares the change in outcomes in the treatment group before and after the intervention to the change in outcomes in the control group.[3] By comparing changes, we control for observed and unobserved time-invariant county characteristics that might be correlated with the managed care decision as well as with birth outcomes. The change in the control group is an estimate of the true counterfactual, that is, what would have happened to the treatment group if there were no managed care mandates. Another way to state this is that the change in outcomes in treatment areas controls for fixed characteristics and the change in outcomes in the control areas controls for time varying factors that are common to both control and treatment areas.

5.1 Empirical Strategy

We compute the double difference estimate using a linear regression of the form:

[3] The double difference estimator is one of the most widely used in the evaluation literature (see, amongst others, Angrist, 1995; and Heckman et. al., 2000).

$$Y_{ijt} = \alpha_j + \beta_t t + \gamma MC_{jt} + \lambda t T_j + \sum_k \delta_k X_{itk} + u_{it}\,, \tag{1}$$

where:

Y_{ijt}	= birth outcome for birth *i* in zip code *j* in year t
α_j*'s*	= zip code fixed effects
β_t*'s*	= year fixed effects
t	= year
T_j	= 1 if the county in which zip code j is located will offer managed care at some point in time
MC_{jt}	= 1 if managed care was offered to Medi-Cal beneficiaries in zip code *j* in year *t*
X_{itk}*'s*	= socio-economic characteristics of parents
ε_{it}*'s*	= disturbance terms.

The coefficient γ is the double difference estimate of the average impact of making managed care available to Medi-Cal beneficiaries on birth outcomes. Since few women give birth in every year it is not feasible to have a mother fixed effect. Rather we include zip code fixed effects, which controls for zip code level factors that do not change over time. We do, however, include a set of birth specific socio-economic controls such as the age and education level of the mother and father. We also include year fixed effects, which control for time varying factors that are common to both control and treatment areas. Finally, we include a linear time trend interacted with whether a county will become a treatment county at some point in time to allow for different time trends.

The above specification assumes that the program had an immediate impact and that the level of the impact was constant over time. An alternative hypothesis is that the program took some time to implement, and time for beneficiaries and providers to learn how best to use it. In order to allow for this possibility we estimate a more general version of equation (1):

$$Y_{ijt} = \alpha_j + \beta_t t + \sum_s \gamma_s MC^s_{jt} + \sum_k \delta_k X_{itk} + u_{it}\,, \tag{2}$$

where s is the number of years that managed care has been available in a zip code, MC_{jt}^{s} equals 1 if managed care was available s years in zip code j in year t.

5.2 Threats to Validity of Identification

There are three critical assumptions for γ to be an unbiased estimate of the program impact. The first is the assumption that the change in birth outcomes in comparison areas is an unbiased estimate of the counterfactual—i.e. what would have happened to birth outcomes in the treatment areas if managed care had not been offered. While we cannot directly test this assumption, we can test whether the secular time trends in the control and treatment counties were the same in the pre-intervention periods (Heckman and Hotz, 1989). If the secular trends are the same in the pre-intervention periods, then it is likely that they would have been the same in the post intervention period if the treated counties had not offered managed care.

The second concern is that there may be unobserved characteristics that vary across time and space, which are correlated with both birth outcomes and the mandates. For example, it could be that the areas where managed care was mandated were also hit by positive economic shocks so that the socio-economic mix of Medi-Cal beneficiaries evolved differently over time. We address this concern by directly controlling for socio-economic characteristics of the parents.

The third concern is that the impact of treatment on the treated may not be homogenous across beneficiaries, but rather vary as a function of their characteristics. For example, the impact of managed care may matter for mothers who are uneducated. In this case, simple double difference estimates may suffer from two additional sources of bias (Heckman et. al., 1997;

treatment and control observations that do not have common support of observable characteristics, **x**. The second bias may arise from different distributions of the vector of observable variables that affect birth outcomes (**x**) within the treatment and comparison groups with a common support.[4]

Matching methods eliminate these last two potential sources of bias by pairing women in managed care counties (treatments) with women in non-managed care counties (controls) that have similar observed attributes. Using observations in the treatment and comparison groups over the region of common support in the distribution of **x** eliminates the first source of concern, while the bias due to different distributions of **x** between treated and untreated counties within this common support is eliminated by re-weighting the control group observations.

In general, conventional matching methods assume that, conditional on the observed variables **x**, the counterfactual outcome distribution of the treated units is the same as the observed outcome distribution of the units in the control group. This assumes that there is no selection into treatment on the basis of unobservables. To avoid the necessity of this assumption, Heckman et. al. (1998b) propose a generalized double difference matching estimator that extends conventional matching methods to longitudinal data. By conditioning on fixed-effects, the generalized double difference estimator identifies the parameter of interest without ruling out selection into treatment on the basis of time-invariant unobservables.

The objective, then, is to construct a control group by finding controls that have similar observed **x**'s as the treatments. Rosenbaum and Rubin (1983) show that to match treated and untreated units on the basis of **x** is equivalent to match them using a balancing score B(**x**). The

[4] Heckman et. al. (1997) suggests that, in practice, the firs

coarsest balancing score is the propensity score which gives the conditional probability of receiving treatment given the pre-treatment values of the vector **x,** i.e. $P(\mathbf{x}) = Pr(MC_{jt} = 1 \mid \mathbf{x})$. Then, the method of matching assumes that conditional on $P(\mathbf{x})$, the counterfactual outcome distribution of the treated units is the same as the observed outcome distribution of the controls. This result is very important in practice since it reduces the potential problem of matching on a high dimensional vector **x** to matching on a scalar.

We estimate propensity scores using a logit model of the probability that a birth was in a managed care county as a function of the socio-economic characteristics found in Table 2. These models are then used to predict the propensity (probability) that a birth is in a managed care county. The logit equation is adjusted by including the interactions between different characteristics to find the best fit possible for each of the different comparison groups. We check that the estimated propensity score does a good job of matching by verifying that the difference in means of the observable characteristics is not significantly different between the treatment and comparison groups within 20 subgroups created based on intervals of the propensity score.

We identify control and treatment observations on a common support as follows. We exclude all control observations whose propensity scores are less than the propensity score of the treatment birth at the first percentile of the treatment propensity score distribution, and exclude all treatment observations whose propensity score is greater than the propensity score of the control observation at the 99^{th} percentile of the control distribution. We then estimate the double difference model on the sample with common support which deals with the first form of bias from heterogeneous response. To deal with the second source of bias, we perform separate double difference analyses on each the observations within the same deciles of the propensity score. However, the estimated effect sizes did not very across deciles and we therefore only report the results for the analysis on the whole overlapping support.

5.3 Comparison Groups

The key to implementing double difference analysis is to find valid comparison groups. We have two treatment groups, COHS and TPC, and consider three possible comparison groups that are summarized in Table 3:

1. *Comparison 1*: Births to mothers covered by Medi-Cal that occurred in counties that did not switch to manage care but stayed on fee-for-service (FFS).

2. *Comparison* 2: Births to uninsured mothers or “self-pay” mother’s that are in the same counties as the TPC or COHS mandates.

3. *Comparison 3*: Births to mothers covered by commercial insurance that occurred in the same counties as the TPC or COHS mandates.

The advantage of comparison group 1, Medi-Cal FFS group, is that both types of patients are enrolled in Medi-Cal and therefore have similar socio-economic characteristics. However, the comparison groups are located in different counties, and these counties tend to be more rural, as they are less populated and have fewer large cities than the treatment counties. Environmental factors such as the availability and quality of medical care might evolve differently over time in these counties implying that the change in this comparison group might be a biased estimate of the counterfactual. Comparison groups 2 and 3 are located in the same counties and therefore do not suffer from this bias. However, self-pay and commercial insurance beneficiaries may look very different from Medi-Cal recipients. While the double difference approach controls for differences between the treatment and comparison group that are fixed over time, the self-pay and commercially insured may be subject to different environmental impacts even if located in the same county as the Medi-Cal beneficiaries.

In total there are 1,307,725 treatment observations (Table 4). Approximately 88 percent of these observations are in the TPC Medi-Cal treatment group, leaving 159,204 in the COHS Medi-Cal treatment group TDalfoupem.Dalfo where T_j = 1 if the county in which zip code j is located will eventually offer managed care to Medi-Cal beneficiaries. If the secular trends are the same then the λ_t*'s* will not be significantly different from zero.

We run separate models for both treatment groups with each of the comparison groups using the pre-intervention data. For comparison group 1, we use all Medi-Cal observations from

erD02(nainsurll 0.0002 ,c4trun separate)6.1(n)1.180.8(ty p0.0en2 Tct)-335 6 T.37 To.335 r.37 TJ/TTy-0.29Cals

non-man evenn

part of the managed care mandates, so have not experienced any major health system changes. Changes in LBW in this case must be a result of other factors. Using equation 3 we test statistically that the pre-mandate trends of the birth outcomes for the self-pay are the same whether the birth is in a Medi-Cal managed care county (comparison group 2) or in a non-

non-managed care counties. This amounts to subtracting the difference in the secular time trends between comparison group 2 and 4 from the standard double difference analysis using comparison group 1 (Table 3).   Similarly, we co groups are the same, adjusting for differences in the trends between the intervention and non-intervention counties.

Our hope then is that the Medi-Cal patients in non-intervention counties (comparison group 1) will be a valid comparison group if we use the self-pay (comparison groups 2 and 4) or the commercial (comparison groups 3 and 5) to adjust for differences in secular trends between intervention and non-intervention counties. We estimate equation five and report the F-statistics for the joint significance of the κ_t in Table 6. The results show that if we use the self-pay (comparison groups 2 and 4) to account for differences in spatial trends, high school and that are teen mom, as well as a larger proportion of Hispanics than in the self-pay comparison group.

Comparing the TPC Medi-Cal managed care and comparison group 1, the mean age and education levels of the parents differ by less than one year, though there are a larger portion of mothers who have not finished high school, and slightly fewer teen moms in the treatment group. The racial composition between these groups is also different. There are fewer Caucasian parents and more Hispanic parents in the Medi-Cal managed care population than the comparison group, the Medi-Cal FFS population.

The difference in means for many of the birth outcomes, during the pre- and post-mandate periods are significantly different between the Medi-Cal managed care counties and the comparison groups (Table 7). Many of the differences are actually quite small, yet are significant due to the large sample sizes.

6.2 Double and Triple Difference Results

Given that double difference analysis requires a comparison group, we use the propensity score to ensure that the observables of the treatment and comparison groups are in the same range. The impact is reported both before and after this common support is created to examine if it causes any important changes in the results. We also report the results of the impact of the entire post-intervention, as well as, with pooling some years together. Specifically, we pool the first two years of the mandates to examine the impact in the initial stages of the managed care mandates, and then pool the rest of the years. We test if the impact in the first two years is different from the impact in the remaining years.

The results for the COHS treatment group show that managed care has lead to improvements in access to prenatal care but has had almost no impact on birth outcomes (Table 8). There is little difference in the average treatment affect between the outcomes with and without the propensity score common support. We find that the month that a Medi-Cal beneficiary begins prenatal care decreases by about 5 percent, and 6 percent more women started prenatal care in the first trimester. There is also a significant 30% decrease in extremely low birth weight when the common propensity score support is used. There are no other significant reductions in the percent of low birth weight or premature babies.

Furthermore, the managed care mandates had an immediate and growing effect on access to pre-natal care. The month treated woman began prenatal care reduced by almost 4 percent in the first 2 years, but by over 7 percent over the next 4 years of managed care. Likewise, 4 percent more treated woman began prenatal care in the first trimester during the first two years, and this percentage doubled to 8 percent for the remaining 4 years of the program. Interestingly, the reductions in extremely low birth weight occurred during the first two years of the mandates and were not sustained for the following years.

The results on prenatal care access are less profound for the TPC treatment group than for the COHS, but we find important reductions in both the number of low birth weight and premature babies for this group (Table 9). This time there are differences in both the impacts and the significance levels if a common propensity score support is used. Without the propensity score common support, there is a significant reduction in the month prenatal care began of almost 1 percent and a similar increase in Medi-Cal mom starting prenatal care in the first trimester. With the common propensity score support the impacts are the same but they are no longer significant. There is also a more than 8 and 7 percent decline in the in incidence of premature and low birth weight babies respectively for those who have a common propensity score.

Turning to the pooled impacts, we find the redu

(Duggan, 2002). Despite the lack of cost savings, our results provide a rationale for Medicaid turning to Managed Care. Medicaid programs can make healthier babies by providing commercial managed care options that mainstreams beneficiaries.

References

Billings, J., G. Anderson, L. Newman. 1996. "Recent Findings on Preventable Hospitalizations." *Health Affairs*, 15(3), 239-249.

Bindman, A., K.Grumbach, D. Osmond, M. Komaromy, K. Vranizan, N. Lurie, J. Billings, A. Stewart. 1995. "Preventable Hospitalizations and Access to Care." *JAMA*, 274(4), 305-311.

Carey, T., K. Weis, C. Homer. 1991. "Prepaid versus Traditional Medicaid Plans: Lack of Effect on Pregnancy Outcomes and Prenatal Care," *Health Services Research* 26(2), 165-181.

Chay, K., D. Almond, D. Lee. 2002. *Does Low Birth Weight Matter? Evidence for the U.S. Population of Twin Birth*. Mimeo, Department of Economics, University of California at Berkeley, August 2002.

Chiu, H. 1997. "Health Insurance and the Welfare of Health Consumers," *The Journal of Public Economics*, 64(1),125-133.

Coburn A., S. Long, and M. Marquis. 1999. "Effects of Changing Medicaid Fees on Physician Participation and Enrollee Access," *Inquiry*, 36, 265-279.

Currie, J. and J. Gruber. 1996. "Health Insuranc

Duggan, M. 2002. *Does Contracting Out Increase the Efficiency of Government Programs? Evidence from Medicaid HMOs.* NBER working paper 9091.

Elgen I., K. Sommerfelt, T. Markestad. 2002. “Population Based, Controlled Study of Behavioural Problems and Psychiatric Disorders in Low Birth Weight Children at 11 Years of Age”, *Arch Dis Child Fetal Neonatal Ed*, 87(2):28-32.

Feldstein, P. 1999. *Health Care Economics*, Delmar: Boston.

Gavin, N., M. Farrelly, J.B. Simpson. 1998. “Children’s Use of Primary and Preventive Care Under Medicaid Managed Care,” *Health Care Financing Review*, 19(4), 45-68.

Goldfarb, N., A. Hillman, J. Eisenberg, M. Kelley, A. Cohen, M. Dellheim. 1991. “Impact of a Mandatory Medicaid Case Management Program on Prenatal Care and Birth Outcomes: A Retrospective Analysis,” *Medical Care*, 29(1), 64-71.

Griffin, J., J. Hogan, J. Buechner, T. Leddy. 1999. “The Effect of a Medicaid Managed Care Program on the Adequacy of Prenatal Care Utilization in Rhode Island,” *American Journal of Public Health*, 89(4), 497-501.

Health Care Financing Administration (HCFA). 2000. *A Profile of Medicaid: Chart Book 2000*. US Department of Health and Human Services, Washington D.C.

Heckman J. and V. Holtz. 1989. "Choosing Among Alternatives Non-Experimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training", *Journal of the American Statistical Association*, 84(408), 862-72.

Heckman, J., H. Ichimura and P. Todd. 1997. “Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Program”, *Review of Economic Studies* 64 (4), pp. 605-54.

Heckman, J. H. Ichimura, J. Smith and P. Todd. 1998a. “Characterizing Selection Bias using Experimental Data”, *Econometrica* 66 (5), pp. 1017-98.

Heckman, J., H. Ichimura and P. Todd. 1998b. “Matching as an Econometric Evaluation Estimator”, *Review of Economic Studies* 65 (2), pp. 261-94.

Kaestner, R., L. Dubay, G. Kenney. 2002. “Medicaid Managed Care and Infant Health: A National Evaluation,” *NBER working paper* 8936.

Kotch, J. and R. Barber-Madden. 1992. “Chapter 15: The Case for Universal Maternity Care,” in *A Pound of Prevention: The Case for University Maternity Care in the US*, American Public Health Association, Washington D.C.

Kotelchuck, M. 1994. “An Evaluation of the Kessner Adequacy of Prenatal Care Index and a Proposed Adequacy of Prenatal Care Index,” *American Journal of Public Health*, 84(9), 1414-1420.

Kreiger, J., F. Connell, J. LoGerfo. 1992. “Medicaid Prenatal Care. A Comparison of Use and Outcomes in Fee-for-Services and Managed Care,” *American Journal of Public Health*, 82(2), 185-190.

Leibowitz, A., J. Buchanan, J. Mann. 1992. “A Randomized Comparisonled Trial to Evaluate the Effectiveness of a Medicaid HMO,” *Journal of Health Economics*. 11, 235-257.

Levinson, A. and F. Ullman. 1998. “Medicaid Managed Care and Infant Health,” *Journal of Health Economics*, 17(3), 351-368.

Luft, H. 1981. *Health Maintenance Organizations: Dimensions of Performance*, John Wiley and Sons, New York.

Manning, W., H. Leibowitz, G. Goldberg, W. Rogers, and J. Newhouse. 1984. “A Controlled Trial of the Effect of a Prepaid Practise on the Use of Service” *The New England Journal of Medicine*. 310(23):1505-1510.

Mattison, D., K. Damus, E.Fiore, J.Petrini, C. Atler. 2001. “Preterm Delivery : A Public Health Perspective,” *Paediatric and Perinatal Epidemiology*,15 (Suppl. 2), 7-16.

Mauldon J., A. Leibowitz, J. Buchanan, C. Damberge, K. McGuigan. 1984. “Rationing or Rationalizing Children’s Medical Care: Comparison of a Medicaid HMO with Fee-for-Service Care,” *American Journal of Public Health*, 6: 899-904.

McCall, N., J. Korb, P. Turner, A. Peterson. 2000. *The Medi-Cal Managed Care Market 1996-1998*. Prepared for the Medi-Cal Policy Institute by Laguna Research Associates.

Kaiser Commission on Medicaid and the Uninsured. 1999. *Medicaid and Managed Care,* Working paper, Washington, DC.

Medi-Cal Policy Institute. 1998. *Medi-Cal Managed Caer: Medi-Cal Facts 1*, Oakland, CA., 1998.

_____1998. *Understanding Medi-Cal: The Basics*, Oakland, CA.

_____2000. *Speaking Out…What Beneficiaries Say About the Medi-Cal Program,* Oakland, CA.

_____March 2000. *Medi-Cal Facts Number 8* , Oakland, CA.

_____2002. *County Data* [on-line], [Accessed on September 18, 2002], Available on the World Wide Web: < http://www.medi-cal.org>

Menges J., C. Park, J. Babcock, C. Chimento, R. Haught, and S. Ho. June 2001. *Comparin Physician and Dentist Fees Among Medicaid Programs*. Prepared for the Medi-Cal Policy Institute by The Lewin Group.

Miller, R. and H. Luft. 1994. “Managed-Care Plan Performance Since 1980: A Literature Analysis,” *JAMA*, 271:1512-1519.

Miller, R. and Luft H. 2002. “HMO Plan Performance Update: An Analysis of the Literature 1997-2001,” *Health Affairs*, 21(4): 63-87.

Millman, M. 1993. *Access to Health Care in America*, National Academy Press, Washington, D.C.

Oleske, D., M. Branca, J. Schmidt, F. Ferguson, E. Linn. 1998. “A Comparison of Capitated and Fee-for-Service Medicaid Reimbursement Methods on Pregnancy Outcomes,” *Health Services Research*, 33(1), 55-73.

Paneth, N. 1995. “The Problem of Low Birth Weight,” *The Future of Children*, 5(1), 19-34.

Ray, W., J. Gigante, E. Mitchel, G. Hickson. 1998. “Perinatal Outcomes Following Implementation of TennCare” *JAMA*, 279(4), 314-316.

Rosenbaum, P. and D. Rubin. 1983. “The Central Role of the Propensity Score in Observational Studies of Causal Effect,” *Biometrika*, 70(1), 41-55.

Rosenbaum, P. and D. Rubin. 1984. “Reducing Bias in Observational Studies Using Subclassification on the Propensity Scores,” *The Journal of the American Statistical Association*, 79, 516-524.

Schauffler, H. and E. Brown. 2000. *The State of Health Insurance in California, 1999*, Regents of the University of California, Berkeley, CA.

Strauss, J., and D. Thomas. 1997. "Health and Wages: Evidence on Men and Women in Urban Brazil," *Journal of Econometrics*, 77(1), 159-185.

Strauss, R. 2000. “Adult Functional Outcome of These Born Small for Gestational Age: Twenty-six-year Follow Up on the 1970 British Birth Cohort”, *JAMA*, 283(5):625-32.

Szilagyi, P. 1998. “Managed Care for Children: Effect on Access to Care and Utilization of Health Services,” *The Future of Children*, 8(2):39-59.

Taylor, H., N. Klein, N. Minish M. Hack. 2000. Middle-School-Age Outcomes in Children with Very Low Birthweight, *Child Development*, 71(6):1495-511.

8.0 Tables and Figures

Figure 1: Medicaid Managed Care (MMC) Penetration Rates, 1991-2002.

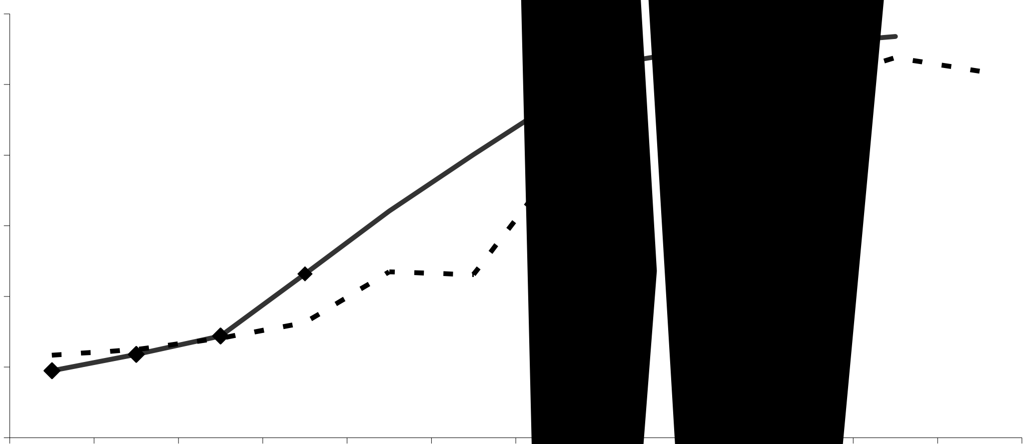

Table 1: Medi-Cal Managed-Care Plan Types and Beneficiary Information by County.

County				Total	in TPC, COHS Plans	(%)		
California				5,840,000	2,642,865	45%		
By Type of Managed-Care Plan								
Total in TPC counties			21,321,872	4,097,040	2,176,136	53%		
Total in COHS counties			5,427,796	575,361	466,729	81		
By County								
Monterey	COHS	Oct-99	409,511	63,953	48,190	75		
Napa	COHS	Mar-98	129,130	10,492	8,345	80		
Orange	COHS	Oct-95	2,872,632	301,928	241,333	80		
Santa Cruz	COHS	Jan-96	264,525	27,248	23,032	85		
Solano	COHS	May-94	408,095	45,106	41,140	91		
San Mateo	COHS	Dec-87	759,313	47,741	38,659	81		
Santa Barbara	COHS	Sep-83	417,331	54,486	44,862	82		
Yolo	COHS	May-01	167,259	24,407	21,168	87		
Alameda	TPC	Jan-96	1,492,004	186,533	96,037	51		
Contra Costa	TPC	Feb-97	942,662	89,468	45,667	51		
Fresno	TPC	Nov-96	825,365	235,991	149,999	64		
Kern	TPC	Jul-96	694,749	162,118	94,880	59		
Los Angeles	TPC	Apr-97	9,925,(2,)-186.2(r)-19(-)1,91,2-1.2-3204.6[-5.7(2-3TD-0.01)-24				928	24-(

Table 2: Means of Parent Characteristics.

Parent's Characteristic	Treatment	Control Group	Difference in Means	
Age of father	28.1	32.2	-4.2	***
Education of father	9.6	12.4	-2.8	***
Age of mother	25.1	28.5	-3.4	***
Education of mother	9.5	12.1	-2.6	***
Mother's age <18 =1	6.4%	2.6%	3.80%	***
Mother's Age >35 =1	6.0%	12.6%	-6.66%	***
Mother had no highschool =1	32.5%	14.8%	17.67%	***
Mother had some highschool =1	29.6%	14.3%	15.29%	***
Mother had some college =1				

Table 3: Potential Treatment and Comparison Groups.

Insurance Type	Medi-Cal Managed Care Counties	Medi-Cal FFS Counties
Medi-Cal	Treatment (COHS or TPC)	Comparison 1
Self-Pay	Comparison 2	Comparison 4
Commercial	Comparison 3	Comparison 5

Table 4: Number of Births.

Medi-Cal TPC Treatment	639,172	408,234	1,047,406	101,115	1,148,521
Medi-Cal COHS Treatment	63,091	81,350	144,441	14,763	159,204
Medi-Cal FFS Control for TPC[1]					

Table 5: Test for Secular Trends in Pre-Intervention Years for the COHS Group.

Without Propensity Score Common Support (NO PS) and with Propensity Score Common Support (PS)

	NO PS	**PS**	**NO PS**	**PS**	**NO PS**	**PS**	**NO PS**	**PS**	**NO PS**	**PS**	**NO PS**	**PS**	**NO PS**	**PS**
COHS Treatment and Comparison 1														
Observations	303,750	229,089	303,750	229,089	303,750	229,089	303,750	229,089	303,750	229,089	303,750	229,089	303,750	229,089
F-value														

Table 8: COHS Group Year and Zip Code Fixed Effects, Double Difference Regressions.

Without Propensity Score Common Support (NO PS) and with Propensity Score Common Support (PS)

	NO PS	PS	NO PS	PS	NO PS	PS	NO PS	PS	PS	NO PS	PS	NO PS	PS	NO PS
COHS Treatment and Comparison 2 (self-pay) - Average Impact of Mandate														
Mandate	-0.168***	-0.148***	0.037***	0.034***	0.06*	0.056	-0.002	-0.001	-0.001	-0.001*	-0.002	-0.002	-0.001	-0.001
	[0.017]	[0.018]	[0.004]	[0.005]	[0.036]	[0.039]	[0.002]	[0.002]	[0.001]	[0.001]	[0.003]	[0.003]	[0.001]	[0.001]
% Δ DV	-5.05%	-4.46%	5.94%	5.45%	0.59%		-4.38%	-2.19%	-29.53%	-29.53%	-2.42%	-2.42%	-7.10%	-7.10%
COHS Treatment and Comparison 2 (self-pay) - Pooled Year Impacts														
Year 1 and 2 of Mandate	-0.125***	-0.101***	0.027***	0.024***	0.006	-0.002	-0.003	-0.002	-0.001*	-0.001**	-0.003	-0.002	-0.001	-0.002
	[0.018]	[0.019]	[0.005]	[0.005]	[0.039]	[0.042]	[0.002]	[0.002]	[0.001]	[0.001]	[0.003]	[0.003]	[0.001]	[0.001]
Years 3 + of Mandate	-0.121***	-0.132***	0.027***	0.029***	0.152***	0.162***	0.002	0.003	0	0.001	0.001	0	0.002*	0.003**
	[0.018]	[0.019]	[0.005]	[0.005]	[0.039]	[0.042]	[0.002]	[0.002]	[0.001]	[0.001]	[0.003]	[0.003]	[0.001]	[0.001]
% Δ DV Year 1 and 2	-3.75%	-3.04%	4.33%	3.85%	0.06%	-0.02%	-6.57%	-4.38%	-29.53%	-29.53%	-3.62%	-2.42%	-7.10%	-14.21%
% Δ DV Years 3 +	-7.39%	-14.08%	8.67%	8.49%	1.49%	1.59%	-2.19%	2.19%	0.00%	0.00%	-2.42%	-3.62%	7.10%	7.10%
COHS Treatment and Comparison 2 (self-pay) - Per Year Impact of Mandate														
Mandate 1 yr	-0.084	-0.054**	0.016	0.009	0.005	-0.017	0	0	-0.001	-0.001*	-0.002	-0.002	-0.002	-0.002
	[0.023]***	[0.025]	[0.006]***	[0.006]	[0.049]	[0.054]	[0.003]	[0.003]	[0.001]	[0.001]	[0.004]	[0.004]	[0.002]	[0.002]
Mandate 2 yr	-0.16	-0.148***	0.035	0.035***	0.127***	0.138***	-0.005	-0.004	-0.002	-0.002**	-0.002	-0.001	-0.001	-0.001
	[0.020]***	[0.022]	[0.005]***	[0.006]	[0.044]	[0.049]	[0.002]**	[0.003]	[0.001]**	[0.001]	[0.003]	[0.004]	[0.001]	[0.001]
Mandate 3 yr	-0.201	-0.199***	0.044	0.044***	0.136***	0.128**	-0.002	0.001	-0.001	f%5p0.j9.4231(0)-3302 Tc0.00r(0.1)2a1(.330(.) 7.2(.)7.6(001)]TJ13.8601				

Table 9: TPC Group Year and Zip Code Fixed Effects, Triple Difference Regressions.
Without Propensity Score Common Support (NO PS) and with Propensity Score Common Support (PS)

	NO PS	PS	NO PS	PS	NO PS	PS	NO PS	PS	NO PS	PS	NO PS	PS
TPC Treatment and Comp												