

ABSTRACT

Title of the Dissertation: ESSAYS IN HEALTH ECONOMICS

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Health care expenditures have risen dramatically in the last several decades. Various agents have responded by reforming their practices in an effort to protect their budgets. My dissertation studies the implications of two of these changes on both quality and expenditure dimensions.

The first chapter introduces and briefly discusses these topics. The second chapter discusses the implications of hospital mergers. A large body of research has examined their financial consequences, while little has analyzed the effect on patient health and experiences. This chapter aims to fill this gap, utilizing 17 years of hospital discharge data to study the impact of 40 California hospital mergers on changes in treatment choices and health outcomes. I use an empirical strategy that is based on geography of residence to enable a market level analysis. My findings indicate that hospital mergers result in increased utilization of intensive treatments for heart disease, such as bypass surgery and angioplasty. This result could be driven by increased access to intensive procedures as well as a change in hospital treatment practices. I also find

evidence of a small increase in inpatient mortality which could be driven by an increase in average travel time to the nearest facility offering cardiac services.

In chapter three, co-authored with Mark Duggan, we analyze the implications of a widespread Medicaid reform: contracting out health care treatment of Medicaid recipients to managed care organizations. State governments rapidly shifted Medicaid enrollees into managed care during the 1990s, perhaps partly as a response to increasing Medicaid expenditures. This reform has not previously been studied at the national level. We use state-level aggregate administrative data for the years 1991-2003 in conjunction with a unique data set on mandatory managed care enrollment policies to estimate the average national impact. Results suggest that this policy may have increased the expense of the Medicaid program, particularly for HMO-style insurance plans. We extend our analysis to investigate the impact of these policies on enrollment decisions. Using CPS data, we find mixed responses to mandatory managed care policies, though all changes in take-up were small and did not appear to increase uninsurance rates.

ESSAYS IN HEALTH ECONOMICS

By

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Dedication

I would like to dedicate this dissertation to the memories of my grandfathers, Dr. Arthur Schiller and Robert Hayford. I will always cherish them both.

I would also like to dedicate this dissertation to Maria Hegstad, whose immeasurable love, support, and encouragement has made completing this project possible.

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I would also like to thank the California Office of Statewide Health Planning and Development for giving me access to the hospital and inpatient discharge data sets and to Bowen Garrett from the Urban Institute for giving me access to their data on local Medicaid managed care policies. I am also grateful to Patrick Healy for putting together the Medicaid expenditure data.

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Chapter 1: Introduction

Health care expenditures have been rising steadily and dramatically for decades. The trajectories of overall and per capita health expenditures are exhibited in Figure 1.1. Health expenditures comprise a rising share of overall spending as well. In 1980, national health expenditures comprised 9.1 percent of gross domestic product (GDP). By 2007, this share increased to a historical high of 16.2 percent of GDP. The trajectory of health expenditures as a share of GDP is displayed in Figure 1.2.

This increase in expenditures affects all levels of budgets, from household to firm to government. In February 2009, a tracking poll conducted by the Kaiser Family Foundation found that 19 percent of individuals faced one or more difficult consequences because of medical bills, such as being contacted by a collections agency or an inability to pay for necessities. The same poll found that 45 percent of individuals were 'very' concerned about the prospect of having to pay more for health insurance and health care. This figure is up from 40 percent in March 2007 (KFF 2007), suggesting that concern over health expenditures pre-dates the current recession. Despite pervasive difficulties in paying for health care, out-of-pocket expenditures accounted for a falling share of private consumer spending across time, as evidenced in Figure 1.3. Rising insurance costs necessitate rising premium rates, which in turn causes financial difficulties for employers that offer health insurance benefits to their employees. This may be one of the driving factors behind the falling rate in private insurance coverage found by Cutler and Gelber (2009).

State and federal budgets are also struggling under increasing health care costs. A large fraction of health expenditures are devoted to the Medicare and Medicaid programs, which comprise nearly 20 percent and 15 percent, respectively, of total national healthcare expenditures in 2007. Over one-fifth of state budgets are devoted to paying Medicaid expenditures, and Medicaid and Medicare account for a slightly larger share of federal expenditures at 22.9 percent (CMS 2009, CBO 2009). In addition to public insurance spending, federal, state, and local governments are large employers and offer substantive benefits packages to their employees, including health insurance. Thus they also face similar challenges as private employers with rising insurance premium costs.

There are a number of factors that have contributed to the soaring spending on health care. The Kaiser Family Foundation provides an overview of many of these factors in their recent primer on health care costs (2009b). Broadly speaking, many of these factors fall into the following categories: an ability to spend more on health care within wealthier countries, populations that are aging and more susceptible to a variety of chronic diseases, and increases in the share of expenditures paid by insurance rather than individuals. In addition, as new technology is developed, it is easy to utilize regardless of cost effectiveness compared to older alternatives. This statement applies to innovations in both pharmaceuticals and medical devices. In addition, as insurance pays a higher share of medical expenditures, there are fewer incentives for physicians and patients to consider the cost of a course of treatment.

In response to these pressures, a number of actors in the health care sector have adapted in ways to reduce expenditures and protect their bottom line.

Employers have responded to rising benefit costs by reducing cost sharing for premiums and offering less generous insurance plans (KFF 2009b). And the likelihood of small employers offering health benefits has fallen as well (GAO 2007). Another adaptation is the development of managed care insurance, which has been able to reduce expenditures without necessarily affecting utilization of services (Cutler, McClellan, and Newhouse 2000). Managed care insurers form networks of providers, and negotiate lower rates in exchange for allowing a provider to treat its enrollees. This, in turn, squeezes the financial position of providers.

Providers of many types have responded by consolidating, enabling them to both take advantage of scale economies and expand their negotiating power with managed care insurers. The last several decades have been a very dynamic period in the health care sector for all of these reasons, and it is important to analyze and assess how each of these changes affects the various dimensions of health care.

The second chapter of my dissertation addresses this secondary response to the growth in health care expenditures. In response to the many factors squeezing their budgets, many hospitals closed or merged with another facility. From the years 1989 to 1996, the number of independent general acute facilities fell by 561 (HHS 1999, Bazzoli, et al 2002). One-third of these facilities continue to operate as a satellite of another facility as a result of consolidation. This wave of consolidation began in the 1980s and continued through the early 2000s. While consolidation occurred at the ownership level with the growth of hospital systems, these statistics as well as the analysis that follows refer to facility-level consolidation. These

transactions involve a complete integration of two previously independent hospitals and will be referred to as mergers for the remainder of this dissertation.

A large literature has studied the financial consequences of hospital mergers, but few have investigated how patients were affected. This chapter aims to fill this gap, utilizing individual-level California hospital discharge data from the years 1990-2006 to study the effect of forty mergers completed during this time period. Previous research has found small increases in readmission rates and small, transitory increases in mortality rates in merging hospitals (Ho and Hamilton 2000, Capps 2005). Unlike previous research, I study the effect of mergers across all patients in affected markets. I use a geographic approach, in which outcomes are examined as a function of exposure to a merger. This approach has the advantage of capturing three types of effects: (1) changes in quality at the merging facilities, (2) changes in quality at hospitals differentially affected by the mergers, and (3) the impacts of patient reallocation in response to the mergers. Results of this kind may better inform policy because they capture the market-level effect of mergers. Hospital-level analyses such as those listed above would miss effects (2) and (3) listed above, and may also misestimate (1) if there is endogenous movement into or out of a merged hospital.

To examine the effect of hospital mergers, I focus on patients with a heart disease diagnosis. This enables me to analyze how mergers affect treatment patterns in addition to health outcomes. I find that hospital mergers induce small though statistically significant increases in mortality. These increases appear to be driven by an increase in average distance to care rather than differences in quality of care or changes in treatment patterns. I also find that mergers induced increases in the usage

of intensive surgical treatments, such as bypass surgery and angioplasty, in favor of medical management of heart disease. Patients who were more exposed to a merger through the smaller of the two hospitals often experienced an increase in access to these specialized procedures, and were thus more affected by the merger. However, patients whose exposure came from the larger of the two hospitals, and were thus much less likely to experience an increase in access, also experienced an increase in utilization of these intensive procedures. This result suggests that merged facilities and/or their competitors increased their treatment of surgical procedures for heart disease patients.

Public insurance programs also responded to increasing budget stress with program reforms intended, at least in part, to reduce the growth of expenditures. Chapter 3 studies the impact and effectiveness of one such reform¹. As stated above, Medicaid expenditures comprise a growing share of both state and federal budgets. One of the most widespread reforms enacted by states has been to shift enrollees from traditional fee-for-service (FFS) style Medicaid into Medicaid managed care plans. Capitated payments provide a financial incentive to the managed care plan to reduce utilization, potentially reducing expenditures. In 1991, 10.8 percent of Medicaid recipients were enrolled in a managed care program. By 1995, this fraction exceeded one-third, surpassing one-half in 1998, and reaching over 60 percent in 2005. This steady increase came from both increases in the number of states creating Medicaid managed care programs and from within-state expansions of eligibility across counties.

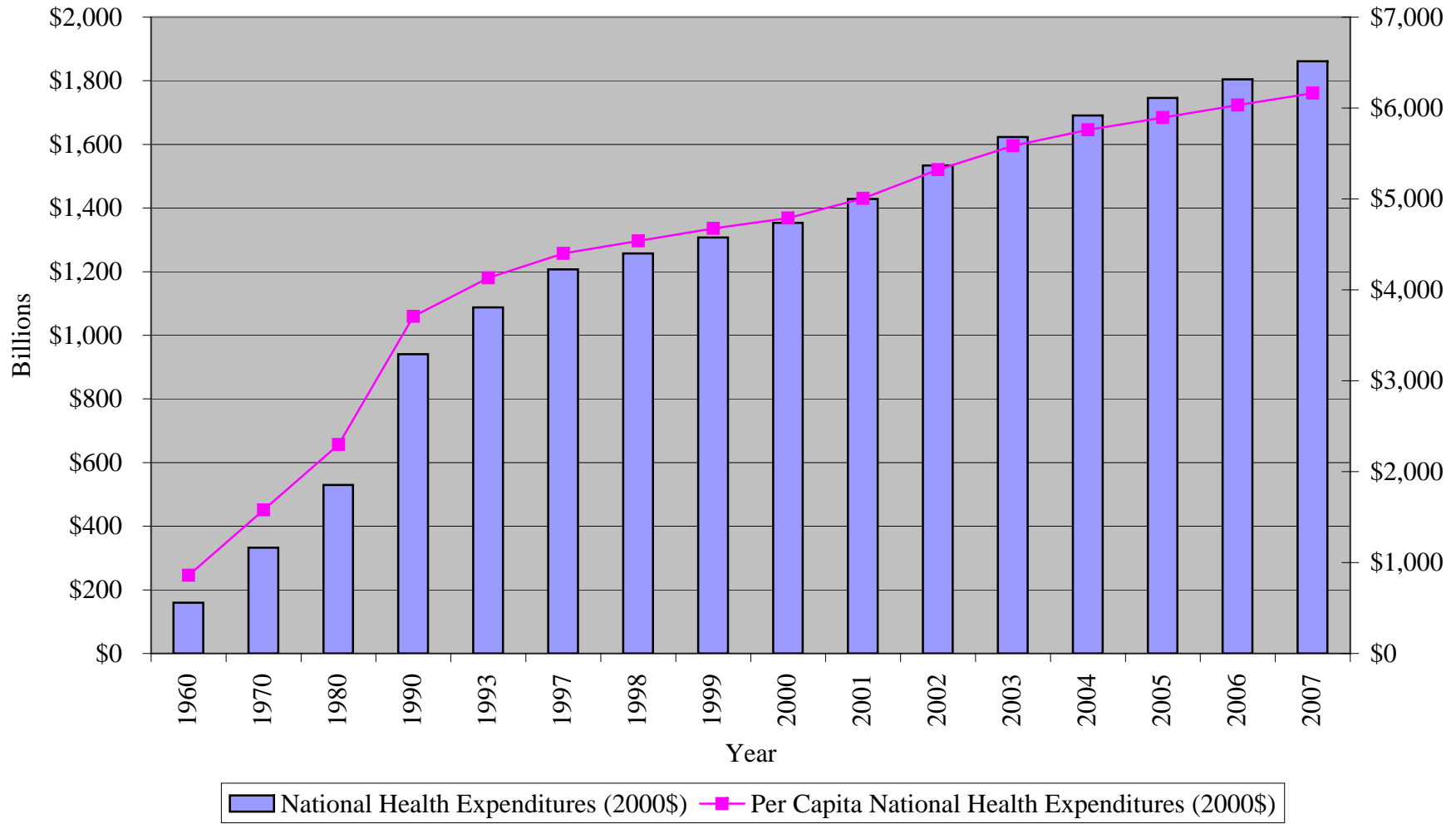
¹ This chapter is a co-authored work with Mark Duggan.

I study the effect of this program on expenditures and on overall Medicaid enrollment. The impact of managed care on expenditures is theoretically unclear because, while private managed care insurers have had success in negotiating lower provider rates, Medicaid reimbursement rates are traditionally low and may be difficult to reduce further (Gruber 2004). While managed care insurers may have encouraged utilization of preventive care, there is little incentive to do so because of substantial churn in the Medicaid population. The spell of program eligibility is often short (Duggan 2004), reducing the benefits of improved health to the insurer. In addition, many states embedded a variety of protections for safety-net providers into their managed care programs.

The scope of state programs varies along several dimensions: managed care penetration rates, mandatory vs. voluntary enrollment, and type of managed care program. Thus the effect of Medicaid managed care (MMC) may vary across states. I use state-level aggregate administrative data in conjunction with a unique instrument for managed care enrollment to study the average effect of MMC on expenditures. This instrument is intended to bypass the selection bias that comes with voluntary managed care enrollment. Alternate specifications split penetration by program type: health maintenance organization (HMO) vs. primary care case management (PCCM). These specifications enable an analysis of one of the broader differences between state programs. Overall, I find that managed care does not reduce the fiscal burden of the Medicaid program. If anything, that burden has increased for states that utilize HMO-style managed care plans.

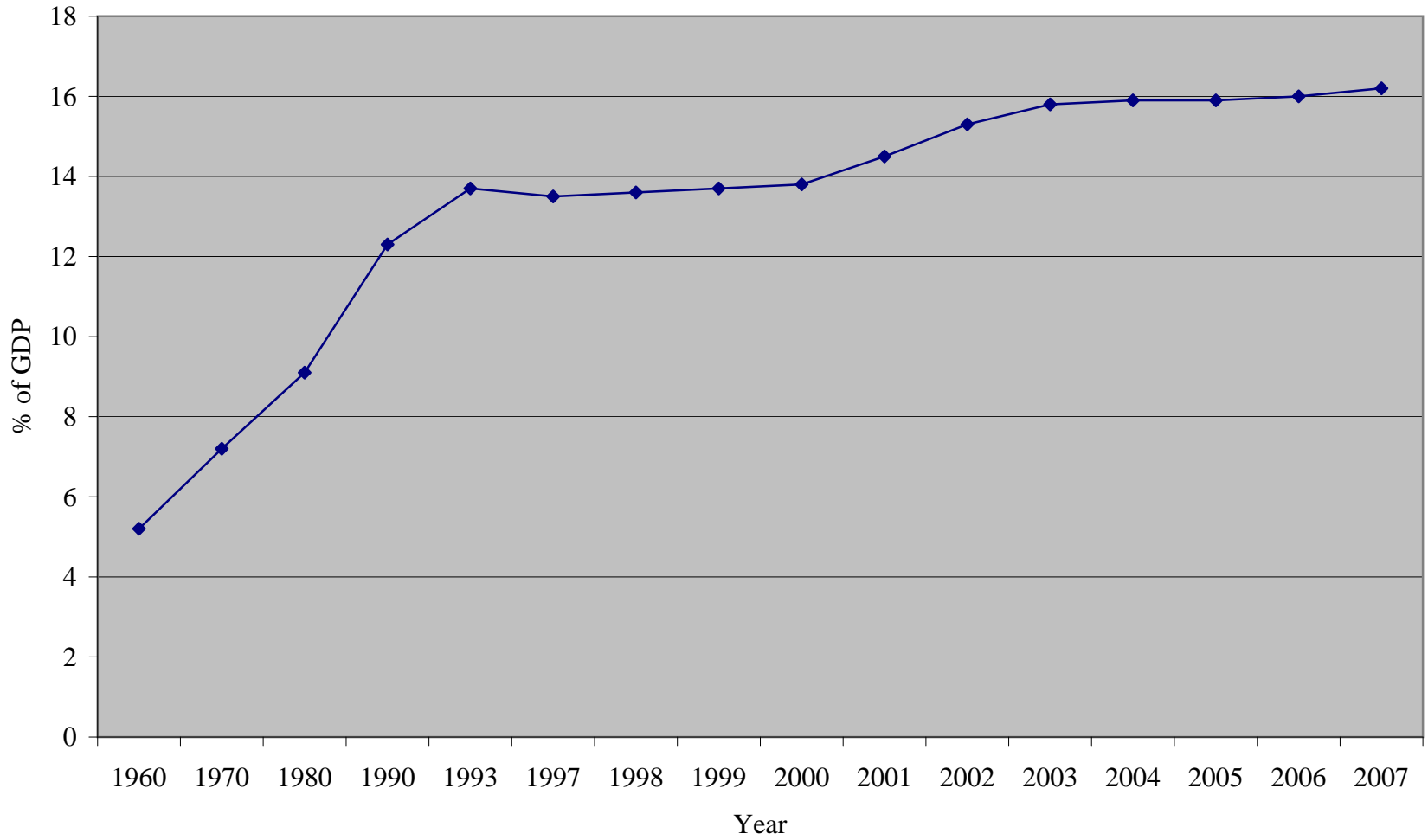
Another concern that has been raised with Medicaid managed care policies is that take-up of Medicaid benefits may fall if eligible individuals decide that managed care reduces the attractiveness of the program. These decisions could increase the vulnerability of an already vulnerable population. I use CPS data to address this question, and find little evidence to substantiate this concern. Mandatory participation in more restrictive forms of managed care induced small declines in Medicaid take-up, though most substituted into other insurance plans. The response to mandatory participation in less restrictive forms of managed care was mixed, though all changes in take-up were small and did not appear to increase uninsurance rates.

Figure 1.1: Historical Health Expenditures



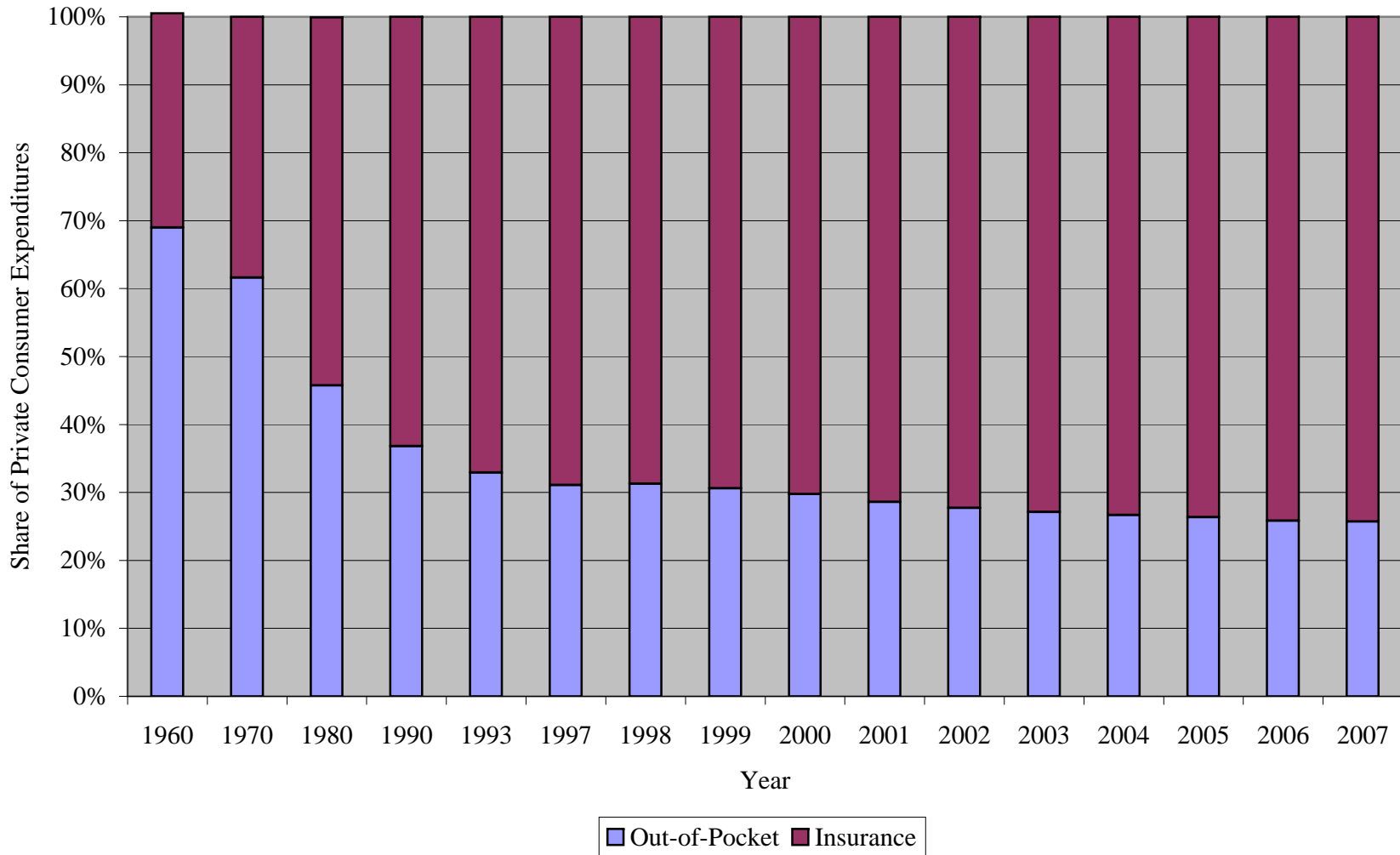
Source: CMS National Health Expenditure Web Tables

Figure 1.2: National Health Expenditures as a Percent of GDP



Source: CMS National Health Expenditure Web Tables

Figure 1.3: Distribution of Private Consumer Expenditures



Source: CMS National Health Expenditure Web Tables

Chapter 2: The Impact of Hospital Mergers on Treatment Intensity and Health Outcomes

Abstract: A large body of previous research has examined the effect of hospital mergers. Virtually all of this research has focused on financial outcomes, such as hospital prices and profitability, rather than health care treatments and health outcomes. This paper aims to fill this gap, utilizing 17 years of hospital discharge data from the most populous state to study the impact of hospital mergers on changes in treatment choices and health outcomes. This study period is long enough to analyze 40 hospital mergers and how their impact changes over time. I use an empirical strategy that is based on geography of residence rather than choice of hospital to detect the effects of a merger across all patients in affected markets. My findings indicate that hospital mergers result in an increase in utilization of intensive treatments for heart disease, such as bypass surgery and angioplasty. This result could be driven by an increase in access to intensive procedures as well as an increase in the likelihood of merged hospitals prescribing these procedures. I also find evidence of a small increase in inpatient mortality which could be driven by an increase in travel time to the nearest facility offering cardiac services.

2.1 Introduction

Consolidation is a common response to financial and competitive pressures in many markets. In most of these markets, such as gasoline, airlines, and telecommunications, the primary concern is the financial implication: will the merger result in a significant price increase? Markets related to healthcare, however, such as hospitals, involve additional complications. For example, both dimensions of demand for hospital services are affected by third parties: treatments are prescribed by physicians and consumers are shielded from the full costs by insurance companies. In these cases, the implications of price changes on consumers may be more difficult

to disentangle. In addition, an increasing percentage of individuals is insured by government programs, for which hospitals cannot affect prices. This trend is likely to continue with the aging of the baby boomer population into Medicare eligibility. Hospitals also provide a service whose quality matters a great deal for both survival and quality of life. Thus, the impact of consolidation in hospital markets on prices may be increasingly less relevant while the importance of the impact on other dimensions, such as quality of care, may grow.

Recent decades have witnessed a large number of hospital consolidations. Nationally, 74 mergers were completed between 1983 and 1988, at an average rate of 12 mergers per year. This pace doubled into the 1990s: an additional 190 mergers were completed between 1989 and 1996 (Bazzoli, et al 2002). For the purposes of this paper, a hospital merger is defined as two legally independent facilities consolidating under a single license². This transaction involves unifying previously separate hospital boards and staffs into single organizations. Legally and financially, the merged hospital is a single entity operating out of multiple physical campuses.

Similarly to research conducted on mergers in other markets, most of the hospital merger analysis has focused on their financial consequences: price and cost efficiency. Much less attention has been paid to the implications for quality and patient experiences. Previous research has found that hospital mergers are likely to improve the financial position of a hospital through price increases and economies of

² Hospitals can also consolidate at the ownership level into systems. This transaction involves far less integration than a facility-level merger, and is thus likely to have different consequences.

scale³. These financial benefits should enable a facility to invest in additional human and physical capital. Patients are likely to benefit from these investments. In addition to creating a single legal entity out of two, hospital mergers often involve some level of service consolidation between the two campuses. This results in an increase in patients and procedures seen and performed by a particular physician or team. A lengthy literature suggests that this increase in volume should result in improved quality and health outcomes⁴. While both of these mechanisms suggest mergers should improve quality, several researchers have found that hospitals in more competitive environments have higher quality and better outcomes⁵. If this is a causal relationship, it suggests mergers may reduce quality through reducing competition. Thus, the effect of hospital mergers on quality is an empirical question.

To date, two papers investigate the direct impact of hospital mergers on measures of quality (Ho and Hamilton 2000; Capps 2005). These studies use similar methodology in that both analyze the difference in quality measures before and after a merger as compared to non-merging hospitals. Both only study 10-11 mergers across a five-year time period, which could reduce the power of their analyses. Neither finds mergers to have a significant effect on quality. One concern with this approach is that patient choice may respond to hospital mergers if there is a perception that mergers affect quality. Thus, hospital level analyses could be subject to bias caused by changes in patient composition. Changes in patient choice could also result in a reallocation of individuals to hospitals that are a better (or worse) fit, which could

³ See, for example, Dranove and Lindrooth 2003 and Conner, et al 1997 on cost efficiencies and Devers, et al 2003; Capps, Dranove, and Satterthwaite 2003; Dafny 2008; and Antwi, Gaynor, and Vogt 2008 on price increases.

⁴ See Gaynor 2006 for a summary of this literature.

⁵ Gaynor 2006 provides a summary of this literature as well.

affect outcomes as well. Another potential concern is that comparing merging to non-merging hospitals requires assuming that non-merging hospitals do not respond to the merger. For example, Dafny (2008) found that non-merging hospitals raise prices following a merger between nearby hospitals. Non-merging hospitals may respond to the reduction in competition that comes with a merger along the quality dimension as well.

The current paper builds on this previous literature in a number of ways. To address many of the concerns detailed above, I use a geographic approach. I measure health outcomes at the zip code level to assess how all patients are affected by a hospital merger. This approach has the benefit of including all potentially affected patients in the analysis. I exploit variation across zip codes in exposure to a merger to analyze its average effect. Exposure is measured as the share of patients in the zip code using the merged facility. Patients of non-merging hospitals are also potentially affected by the merger, and they are more likely to be affected when merging hospitals are closer and have a larger share of the market.

Figure 2.1 demonstrates the heterogeneity in the exposure measure for the 1999 merger between Fresno Community Hospital and Valley Medical Center of Fresno. Darker zip codes are ones that are more exposed to the merger. While I cannot disentangle the effect of changes in the merged facility from changes in local non-merging facilities or patient sorting, I am able to analyze the market-level effect of a merger, including ripple effects to other facilities. This latter effect might be more informative to policymakers and antitrust analysts because it captures the full market-level impact.

My analysis includes the 40 mergers that are completed during the years 1990-2005 throughout the state of California. This longer time period enables me to analyze how the impact of a merger changes with time. I focus on patients admitted with a heart disease diagnosis. Heart disease is a serious condition that is often treated with surgery in conjunction with pharmaceuticals to prevent future heart attack and heart failure events. Targeting this group enables me to investigate whether hospital mergers induce different treatment patterns for intensive procedures such as bypass surgery and angioplasty. While inpatient mortality is a concrete measure of quality, it does not capture the mechanism through which any effect occurs. Expanding the analysis to treatment intensity may offer additional insight into how patient experiences are affected by a merger. Using inpatient discharge data through 2006, I find that hospital mergers induce more intensive heart disease treatment and larger mortality rates.

In a sample zip code with 20 percent of patients discharged from the merged facility, intensive treatment utilization increases by 2.9 to 3.5 percent and inpatient mortality increases by 1.6 to 2.7 percent. These effects are not transitory and continue to exist five years after a merger and beyond. The increase in mortality appears to be driven by patients whose distance to care increased because of the merger, while increases in treatment intensity appear to be partially driven by increases in the likelihood of prescribing these procedures. Increasing usage of intensive procedures may be another way for merged facilities to improve their financial position because intensive heart surgeries are often one of the more profitable services offered by hospitals (Horwitz 2005). And while the rise in

inpatient mortality does not appear to be driven by the additional risk from performing additional intensive procedures, the data do not enable me to discern whether the additional surgeries lead to longer term benefits.

The remainder of this document is organized as follows: Section 2.2 will provide a background on hospital mergers and related literature. Section 2.3 describes the data used in this project, and section 2.4 provides an overview of California hospital mergers during this time period. Section 2.5 details the empirical methodology and provides the first set of results. Alternate specifications are detailed in section 2.6, and section 2.7 concludes with a discussion of results and future work.

2.2 Background on Hospital Mergers

While hospital consolidation existed in the 1980s, its pace nearly doubled during the 1990s. For example, there were 190 hospital facility mergers between 1989 and 1996, while there were only 74 between 1983 and 1988 (Bazzoli et al, 2002). This represents an increase in the average annual number of mergers from 12 to 24. These mergers are defined as two facilities consolidating under a single facility license. The facility whose license is retained is considered the parent facility while the facility whose license is relinquished is considered the satellite facility. There were a variety of factors that contributed to this wave of mergers, though there is no consensus on the relative importance of each factor. Vogt and Town (2006) suggests that technological advances both shifted many inpatient procedures to an outpatient setting and reduced the length of stay necessary for other procedures that remained in the inpatient setting. Through these mechanisms, technological advances had the

consequence of creating excess capacity in hospitals across the board. Mergers provide an avenue for hospitals to consolidate services and reduce capacity without being subject to state regulations because the reorganization is taking place within a single facility (Dranove and Lindrooth 2003).

In addition to reduced demand, hospitals also reduced payments for hospital services. These reductions came from government-sponsored insurance and private insurers alike. Medicare began restricting hospital payments with the institution of the Prospective Payment System (PPS) in 1984, and tightened the purse strings in 1988 by strengthening the requirements for elevated payments. This policy change resulted in significant negative margins on Medicare patients (Coulam and Gaumer 1991). The 1990s also saw the advent of managed care insurance. Enrollment in all types of managed care increased over the 1990s. HMO penetration in Metropolitan Statistical Areas rose from just over 10 percent in 1990 to roughly 30 percent in 1999; it fell slightly to approximately 28 percent by 2000 (Vogt and Town 2006). Even though managed care enrollment is shifting from HMOs to less stringent varieties of managed care, such as PPOs, these more flexible plans still create networks of preferred providers and negotiate fixed reimbursements to hospitals in exchange for being included in the network⁶. Out of three studies of the relationship between managed care penetration and hospital consolidation, only one finds a positive correlation between the two⁷. Based on the prevalence of qualitative anecdotes on the

⁶ The structure of these reimbursements vary by plan and include payment by DRG and per-diem payments for care that vary by the service type, such as medical/surgical, ICU, acute care, etc. (Capps and Dranove 2004).

⁷ Dranove, Simon, and White (2002) find a positive relationship between managed care penetration and hospital consolidation; Sloan, Ostermann, and Conover (2003) and Town, et al (2007) fail to find a connection.

relationship between managed care and consolidation, Vogt and Town (2006) suggests that the ‘threat’ of managed care, if not actual managed care penetration, could have influenced the decision to merge.

As explained in section one, hospital mergers are likely to impact quality of care through three mechanisms: their ability to improve their financial standing through achieving cost efficiencies and through negotiating higher prices with insurance firms; the implications of reduced competition in the hospital market; and improvements in physician skill due to the volume-outcome relationship. I expand on each of the three mechanisms below and demonstrate the potential each has to impact hospital quality.

2.2.1 Cost Efficiencies

If hospitals can achieve economies of scale through merging, improvements in quality of care may be possible because hospitals have more resources to devote towards quality provision or because quality becomes less expensive to provide. The general consensus of the literature is that hospital mergers lead, at worst, to slower cost growth and possibly induce significant cost savings. Most of the research does not explicitly distinguish between consolidation at the ownership level (into systems) and at the facility level; this research is summarized in Dranove and Lindrooth (2003). One exception is the Conner et al (1997) study which investigates the impact of consolidation on cost changes over a 9-year period. Unsurprisingly, the authors find much larger cost savings for non-system hospitals and for hospital pairs that are closer in size and have a larger overlap of service provision before the merger.

Dranove and Lindrooth (2003) expands upon this literature by explicitly studying system acquisitions and facility mergers separately and constructing a control group using propensity score matching. Each consolidation is matched to 10 non-merging hospitals based on their propensity to merge. Data from the American Hospital Association and from Medicare cost reports for the year before the merger and four years following the merger are used to estimate initial cost savings and the persistence of these savings over the next several years. The authors find that system acquisitions result in small and statistically insignificant cost savings. However, facility mergers result in approximately 14 percent cost savings that persist for at least 4 years after the merger.

2.2.2 Price Increases

Hospitals also have the potential to improve their financial position through merging if consolidation allows them to negotiate larger reimbursements from insurance providers. The benefit of this strengthened negotiating position will vary across service offerings because Medicare prices are regulated and certain diagnoses are disproportionately experienced by Medicare beneficiaries⁸. But, previous research suggests that hospitals do not compete at the diagnosis level, suggesting that the financial benefits of price increases will be spread throughout the hospital⁹ (Dafny 2005).

⁸ For example, approximately 60% of heart attacks and 75% of strokes in California from 1992-1995 were experienced by Medicare beneficiaries (Ho and Hamilton 2000).

⁹It is possible that hospitals will use these increases in revenue to compete for private payer patients rather than for overall increases in quality, but it is also possible that quality increases targeted at private payer patients would spillover into quality improvements across the board.

The experiences and opinions of health care workers are suggestive of a causal relationship between hospital consolidation and price increases. Devers, et al (2003) uses an extensive set of surveys of various types of health care workers in 12 major healthcare markets to identify whether hospital consolidation leads to increased bargaining power with insurance firms. The authors find that the ‘on-the-ground’ opinions of health care workers is that consolidation has improved hospital bargaining power, leading to an increase in the negotiated prices between hospitals and insurance firms.

Recent empirical research strengthens this finding. Capps, Dranove and Satterthwaite (2003) use a structural approach to estimate willingness-to-pay for including each of the hospitals in the San Diego metropolitan area in a managed care insurance plan network. This measure is utilized to estimate hospital profits and prices as well as predicted prices in the event of a merger. The authors find that a merger between two of the three hospitals has the potential to increase prices by over 10 percent. Dafny (2008) finds a similar result in a national analysis. She uses a combination of rival analysis and a distance-based instrument for hospital mergers and finds that a merger between two rival hospitals increases price at a nearby non-merging hospital by approximately 40 percent. Given that the non-merging hospital was unlikely to experience a demand or supply shock outside of the rival merger, this finding suggests that merging hospitals increase their prices after a merger. Antwi, Gaynor, and Vogt (2008) provides some evidence to the contrary, finding that the large increase in California hospital prices from 1999-2005 is not correlated to increases in concentration from 1992-2003. However, there are a large number of

hospital closures during this time period that would increase concentration, but may have a different effect on prices, potentially convoluting the results.

2.2.3 Competition and Quality

There are several theories that suggest hospital quality should increase with competition. In general, the mechanism behind this relationship is that consumers face little, if any, difference in out-of-pocket expenditures across hospitals, leading hospitals to compete for patients on a quality dimension instead. These theories include the medical arms race theory and one that models the Medicare market and utilizes a regulated price. Both of these theories are explained in depth in Gaynor (2006).

Most of the recent empirical research on hospital competition and quality suggests that competition increases quality. One of the seminal papers on this topic, Kessler and McClellan (2000), uses a patient choice model to create a plausibly exogenous predicted Herfindahl Index (HHI) measure. The authors use this measure to estimate the impact of competition on Medicare heart attack outcomes in 1985, 1988, 1991, and 1995. They find that more competitive markets are associated with greater treatment intensity in the 1980s and with lower treatment intensity in the 1990s. HMO penetration is identified as the reason for this difference.

Two more recent papers corroborate these findings. Kessler and Geppert (2005) follow a similar methodology, but allow treatment and outcome coefficients to differ for high versus low risk Medicare heart attack patients. They find that high risk patients are treated more intensively and experience better health outcomes in more competitive markets, while low risk patients are treated less intensively and

experience similar outcomes. Sari (2002) expands beyond the Medicare subset and estimates the impact of county-level market shares and competition on a set of quality indicators constructed by the Agency for Healthcare Quality and Research (AHRQ) using the AHRQ National Inpatient Sample data set for the years 1992-1997. Sari instruments for shares with patients' average incomes and for HMO penetration with the unemployment rate and the percent of large firms in the market. Sari finds that increases in market share and overall concentration worsen quality of care for 3 out of 7 sets of quality indicators. There is no effect on the remaining four indicators¹⁰.

Not all research in this area finds competition to be quality-improving. Gowrisankaran and Town (2003) uses a similar methodology as Kessler and McClellan (2000) to analyze the impact of competition within different payer types in Los Angeles and find that increases in competition for Medicare patients are associated with increased mortality rates for both heart attack and pneumonia patients. This difference in findings could stem from differences between Los Angeles and national averages. In general, this literature investigates how hospitals respond to the level of competition they face while ignoring how the competitiveness of the environment developed. To the extent that this relationship is causal, these findings suggest that hospital mergers could reduce quality of care through this mechanism.

¹⁰ Negative and significant coefficients are for adverse/iatrogenic results, wound infection, and inappropriate surgical utilization; no effect is found for mortality results, obstetrical results, major surgery complication results, and cesarean section delivery results.

2.2.4 Volume-Outcome Relationship

Hospital mergers often incorporate a consolidation of service offerings. At the very least, physician and nursing staffs are unified between the two facilities. If gained experience and shared expertise have the impact of improving outcomes, then hospital mergers have the potential to improve health outcomes and quality of care through this channel as well. Gaynor (2006) provides a review of the recent volume-outcome studies within the economics literature. Two studies instrument for bypass surgery volume with the number of area bypass surgery patients and the number of area hospitals that provide the surgery. Both found a significant causal effect between volume and inpatient mortality, and one of the studies identified that this impact comes from current volume rather than from built up experience (Seider, Gaynor, and Vogt 2004 and Gaynor, Seider, and Vogt 2005). Ho (2002) reports similar results, finding small improvements in inpatient mortality and in the need for consequent bypass surgery when a hospital performs an angioplasty procedure, but finds no effect of cumulative volume¹¹. Gowrisankaran, Ho, and Town (2006) uses a different methodology and finds that three different surgical procedures all exhibit positive volume-outcome correlations, though the extent of experience lasting after a volume shock varies by how routine the surgery is to perform. Overall, the literature is suggestive of a positive causal relationship between volume and outcomes.

¹¹ Same-stay bypass surgery in addition to angioplasty reflects that the angioplasty failed in clearing the blocked artery.

2.2.5 Consolidation and Quality

The impact of hospital mergers on quality is theoretically ambiguous. The research discussed above suggests that mergers have the potential to induce improvements in quality through strengthening their financial position. Mergers may also improve outcomes through increasing the volume of procedures performed by physicians. On the other hand, if mergers reduce competition sufficiently, there may be downward pressures on quality provision. However, the research on competition and quality is more suggestive of association than causation because outside factors could be responsible for both competitive markets and better outcomes. For example, urban hospitals face more competition but may also benefit from more resources and shorter travel distances for patients. Therefore, changes in the degree of competition a hospital faces may not automatically reduce quality as previous research suggests. Ultimately, the impact of hospital mergers on quality of care is an empirical question. It is important to note, however, that welfare is not necessarily positively tied to quality, particularly if there is supra-optimal provision of quality in the hospital market.

Two papers analyze the impact of hospital mergers on health outcomes. Ho and Hamilton (2000) study the impact of hospital mergers, system acquisitions and system transfers of California hospitals between 1991 and 1996. They find an increase in readmission rates for heart attack patients treated in a merged hospital, but no statistically significant effect on inpatient mortality for heart attack or stroke patients. However, these coefficients are not precisely estimated. Their empirical framework utilizes indicator variables for whether a hospital underwent each of the

three merger and acquisition events, and thus assumes that consolidation has a single level effect, rather than one that varies with time. Considering the likely initial upheaval from a merger and the learning curve of working under new leadership or running a new facility, this approach may cancel out opposing impacts over time. More importantly, by comparing merging to non-merging hospitals, they assume patient choice and the behavior of non-merging hospitals do not respond to the merger. Because patients may alter their hospital choices due to perceived changes in quality, the composition of patients may differ before and after the event. And because the effects could differ across patients, average changes may mask important changes in distribution of outcomes. In addition, if the resulting reallocation of patients results in better (or worse) matching between patients and hospitals, these changes would be missed by this type of comparison. Furthermore, any changes at merger hospitals will be masked if there are also changes at non-merging hospitals. Previous research has found that non-merging hospitals raise their prices after a merger between nearby hospitals (Dafny 2008). It is plausible that they could respond along quality dimensions as well.

Capps (2005) studies 11 hospital mergers in New York during the years 1995 to 2000. He uses the AHRQ quality measures and compares outcomes from merging hospitals to two sets of control groups: hospitals that are similar based on observable characteristics and those that have similar probabilities of merging as each merging hospital. Capps' methodology also relies on a hospital level analysis and thus faces the same set of concerns as the Ho and Hamilton (2000) paper above. He does not find mergers to have an impact on quality measures, with the exception of a small and

transitory negative impact on two heart attack outcomes using the first control group. Gaynor (2006) posits that the lack of significant results from these two papers could be either due to mergers not impacting quality or due to the small number of mergers studied and thus insufficient power in the analysis.

My analysis should build on previous research in several ways. It covers a longer time period (1990-2006) and a larger number of mergers (40). Also, instead of controlling for the endogeneity of hospital mergers by using hospital fixed effects (Ho and Hamilton 2000) or by constructing a control group of similar hospitals (Capps 2005), I use an instrumental variables approach based on the varying exposure to a merger across zip codes, where exposure to a merged facility is instrumented with predicted exposure. One benefit of this approach is that it captures the average effect of the merger across all patients, even if hospital choice and the behavior of non-merging hospitals responded to the merger. This methodology will be further explained below.

2.3 Data

The data for this project comes from two sources: an annual hospital financial data set and an annual inpatient discharge data set. Both data sets were obtained from California's Office of Statewide Health Planning and Development (OSHPD). The annual financial data set includes a vast set of information for each hospital in the state by fiscal year. It includes information on the characteristics of hospitals such as its owner, profit-type, location, whether it was a teaching or rural hospital, number of beds, and services offered. Financial information on revenue, expenses and

investment, aggregated patient information on revenue, outpatient visits and inpatient days and discharges by pay-type is included as well. The data set also includes information on the type of hospital, such as general acute, psychiatric or specialty. I drop all specialized hospitals and focus my analysis on general acute facilities because I will be limiting my analysis to heart disease patients. While long term care facilities might treat patients for heart disease, the treatment is likely to consist of rehabilitative care after the patient has been discharged from a general acute facility.

The annual inpatient discharge data set includes every inpatient discharge from a California hospital for each year in the data. This data set includes patient level data on demographic characteristics, payment, diagnosis and procedures performed. Demographic information includes age categories, sex, race, ethnicity, county of residence for California residents and 5-digit zip code of residence. Payment information includes expected primary source of payment split into categories such as Medicare, Medicaid and private payers as well as total charges. It is important to note that total charges do not reflect the actual amount paid by the patient or the insurance provider because insurance providers often negotiate rates for each procedure and service. However, changes in average charges could provide evidence to support an observed increase or decrease in treatment intensity because charges are likely to be correlated with the number and intensity of procedures. Diagnostic and procedural information includes principal diagnoses and procedures listed as ICD-9-CM codes as well as up to 24 additional diagnoses and 20 additional procedures. There are also variables listing whether each diagnosis was present at admission and the number of days between each procedure and the patient's

admission date. In addition, each discharge record includes the facility number of the hospital where the individual was treated as well as the year and quarter of admission and length of stay (in days).

Table 2.1 provides hospital-level financial and discharge statistics for selected years. Most demographic characteristics appear fairly stable over time, though the percent of Hispanic patients and the percent of patients who are at least 65 years of age rise throughout the time period. In general, length of stay and inpatient mortality decline over this time period, while average charges increase. This could represent an increase in list prices as well as an increase in the number or intensity of procedures performed. Both the average number of discharges per hospital and available beds per hospital increase over this time period. Consolidation in the hospital market in the form of mergers is at least partly responsible for this trend. The closure of hospitals also contributes to this trend because smaller hospitals are more likely to close. The financial position of hospitals declines for at least the first two-thirds of this time period, as evidenced by falling revenue-to-expenditure margins. These figures improve between 2000 and 2005. It is also interesting to note that nursing hours per bed increase across this time period. This could represent an increase in quality of care or an increase in bed utilization.

One concern with the inpatient discharge data is that, beginning in 2000, OSHPD began masking certain variables to prevent the identification of individuals within the data set. For reporting years 2000 to 2006, 3.4 percent of observations must be dropped because quarter of admission and/or zip code of residence are missing. A potentially larger problem is that gender, race, and ethnicity are

sometimes masked as well. The prevalence of masking in these variables is much higher, particularly for race and ethnicity. Approximately 15 percent of observations from these reporting years have masked values for sex, race and ethnicity. An additional 8 percent have masked values for race and ethnicity, and another 4 percent have a masked value for ethnicity only. As evidenced in Table 2.1, percentages for the gender, race and ethnicity categories do not seem to change much upon the year 2000. The largest drop is in the percent Black variable, which drops 1.06 percentage points from 1999 to 2000, though part of this is likely to come from a slight downward trend in this variable beginning in 1995. It is unlikely that this presents a significant problem for the purpose of this analysis.

Like many of the papers referenced above, I also restrict my analysis sample to a specific diagnosis category: patients with heart disease. This type of restriction allows me to construct a reasonable set of outcome variables beyond mortality as well as a relevant set of co-morbidity factors to use as control variables. Limiting my analysis to patients with a heart attack (AMI) would be ideal because AMI is an acute health event that requires immediate hospitalization, minimalizing chances of selection bias in admission decisions. Unfortunately, there are not enough zip codes with a sufficient number of AMI discharges in all quarters. To remedy this situation, I expand the data set to include all individuals with a diagnosis of ischemic heart disease (IHD), which includes both acute and chronic forms of heart disease¹². This expansion enables me to create a balanced panel of 697 zip codes with at least 15

¹² I further restrict the analysis sample to only those IHD discharges with a cardiac major diagnosis code; I assume that a non-cardiac admission with IHD includes those for which heart disease is merely a complicating factor and those who develop an AMI in response to complications from another disease or procedure. Both groups are likely to be distinctly different from those included in this sample.

IHD discharges in all 68 quarters. Because IHD varies in severity, hospitals may have varying decision rules for when to admit a patient with heart disease. This selection issue should not come into play unless a hospital merger induces a change in admission criteria. I demonstrate below that mergers do not appear to impact the admission criteria for heart disease patients.

Table 2.2 includes descriptive statistics for IHD patients and their hospital experiences. Unsurprisingly, this subset of the discharges has a greater percentage of men and skews older, resulting in a much higher Medicare coverage percentage. It is interesting to note that the age distribution among the elderly shifts older across the time period as well. These trends suggest that there is less potential for gains from merger-related increases in bargaining power with insurance firms for cardiac care, because the percent of IHD discharges covered by private insurance falls from roughly 30 percent in the early 1990s to just under 23 percent by 2005. Thus we might expect changes on the quality margin to be particularly strong among IHD patients. However, as cited above, Dafny (2005) finds evidence that hospitals optimize quality decisions across all services, rather than envisioning each diagnostic category as a separate market in which to compete.

It also appears that while heart disease patients have more complicating diagnoses in later years, treatment intensity has increased as well, yielding potential improvements in outcomes. The prevalence of diabetes among heart disease patients nearly doubles from 1990 to 2005, as does the incidence of hypertension. The incidence of heart failure increases by over 40 percent as well. However, the percentage of patients receiving bypass surgery or angioplasty increases by more than

20 percent from 1990 to 2005. In addition, inpatient mortality falls by over 30 percent during this time period. However, considering that length of stay falls dramatically during this time period as well, this drop in inpatient mortality could be confounded by a change in discharge policies if hospitals are more likely over time to discharge someone to die at home. It is also interesting to note that the number of hospitals treating heart disease patients falls by over 25 percent while the number of heart disease patients treated at a hospital rises by nearly 60 percent. These trends are likely caused by a combination of hospital closures and mergers over the time period.

2.4 Background on California Hospital Mergers

I use two methods to identify hospital mergers. Through 2002, the documentation for the inpatient discharge data included a list of consolidated hospitals. I checked hospital websites to confirm each of the relevant mergers and to determine the fate of each satellite facility, whether it became a specialty facility, remained the same, or split specialized services with the parent hospital. In a few instances, the satellite hospital was promptly closed. I do not count these as mergers because the satellite did not continue to operate in any capacity after the merger.

Because the hospital consolidation lists were not continued beyond 2002, another method was necessary to identify mergers in 2003 to 2005. This was possible because a merger looks like a closure in the annual financial data set. Both involve the disappearance of a facility number: a satellite hospital in a merger surrenders its facility license in the merge and a closing hospital surrenders its facility number in the closure. I researched each apparent closure and checked each against a report of

hospital closures published by the California Hospital Association¹³. From this process, I identified several mergers that transpired after the consolidation lists were discontinued. Table 2.3 provides details on the timing of all California hospital mergers during this time period.

A total of 40 hospital mergers occurred between the years 1990 and 2005. Twenty-seven of these mergers involved non-profit hospitals, 10 involved for-profit hospitals, and 3 involved county or district hospitals. In general, especially during the 1990s, mergers were more likely to take place in greater metropolitan areas, such as Los Angeles or San Francisco. Mergers also tended to involve hospitals that were geographically close to one another: 25 mergers were between hospitals that were no more than five miles apart. All but one of the remaining mergers were between hospitals no more than 15 miles apart. The one exception is a merger between two district hospitals in northeastern California, where the satellite is a small rural hospital that needed to find a merging partner in order to survive. Nearly one-third of mergers involved one of the hospitals being acquired and merged into the other simultaneously. Ten mergers transpired one to five years after one of the two merging hospitals was acquired or after a local system was created; six out of these were completed three years after the ownership change. In the remaining mergers, both hospitals were owned by the same firm or government entity for more than five years or the entire data period¹⁴.

¹³ This report can be found here:
www.calhealth.org/public/press/Article%5C107%5CHospitalclosures.pdf

¹⁴ There are eight hospital mergers that take place in the first five years of the time period and do not change ownership during the time period. Because I do not have data for years before 1990, I do not know if the facilities involved in these mergers experienced ownership changes before 1990.

Parent hospitals are, on average, larger than satellite hospitals when measured by bed size, as evidenced by Table 2.4. Parent hospitals are also, on average, larger than non-merging hospitals, even before the merger. The average bed size of the merged hospital is smaller than the sum of the average satellite and parent hospital bed sizes because some hospitals do not keep all beds after the merger. These hospitals are likely converting some of the bed space to other uses or closing off some of the patient rooms. As one would expect from the size comparisons, parent hospitals are also more likely than non-merging hospitals to have specialty services such as NICUs and open heart surgery services as well as diagnostic tools such as MRI machines. Satellite hospitals are less likely to have these services and tools than non-merging hospitals. It is interesting to note that in many cases, the percent of merged hospitals with a particular specialty service does not increase much from the percent of parent hospitals, even when a reasonably large percentage of satellite facilities have the service. For example, 78.4 percent of parent hospitals and 51.4 percent of satellite hospitals house neurological surgical services in the year before a merger. However, only 83.8 percent of merged hospitals house this specialty. This pattern suggests that hospitals are not merging for the purpose of expanding service capabilities, but instead to potentially cut costs by reducing overlapped service provision.

The importance of a merger in terms of the number or share of people impacted varies across mergers. For example, the Eden Medical Center/San Leandro Hospital merger in 2004 had little impact on market concentration for heart disease discharges because there is a small degree of overlap in market shares of the two

facilities. Both hospitals had market shares above 10 percent in a few zip codes, but in most cases the market share of one was far less than half the market share of the other. The greatest change in HHI was from 0.12 to 0.20. The St. Joseph Hospital-Eureka/General Hospital of Eureka merger in 2000, however, had a much larger impact on market concentration in the surrounding area. Several zip codes experience an increase in HHI of over 0.15, resulting in HHIs as high as 0.88 and market shares as high as 94 percent. Other mergers fall in between these two examples in their impacts on merged market shares and increases in market concentration.

Table 2.5 reports pre-post statistics on the characteristics of heart disease admissions in merged facilities. There are three noteworthy observations. Average charges increase more rapidly after a merger as compared to before. Considering that length of stay remains fairly constant and number of procedures steadily increases, this increase in charges likely comes from a combination of increase in treatment intensity as well as an increase in list prices for cardiac hospital services¹⁵. Similarly, the percentage of patients receiving intensive procedures rises considerably after a merger. This trend is particularly notable because this percentage is fairly stable in the year before the merger. However, there is an overall upward trend in usage of intensive procedures across all patients and time periods, and the relative impact of mergers and trends cannot be teased out without an adequate comparison group. In addition, the percent of patients with hypertension and diabetes appears to increase after a merger. While the other co-morbidity rates are too noisy to discern whether

¹⁵ Recall that list prices are not a proxy for actual transactions prices; even if this increase in charges stems solely from an increase in list prices, it is not necessarily true that hospitals received higher prices from insurance firms.

there is a break in trend, there is suggestive evidence of a change in the average sickness of patients in merged hospitals which could also contribute to apparent changes in treatment intensity.

2.5 Empirical Framework and Results

It is difficult to identify the causal impact of a merger at the hospital level because patients may reallocate themselves across hospitals in response to a merger. This would lead to shifts in patient composition that could confound the identification of outcome or treatment changes. Reallocation could stem from two potential sources. One source is fairly innocuous and originates from people preferring hospitals that are close by. If a satellite hospital is transformed into a specialty facility, then some patients may choose a non-merging hospital after a merger if it is closer than the parent facility. The other motivation for reallocation presents a larger concern. If a hospital merger causes a change in public perception of the merged facility's quality, then reallocation could result in a differential change in the health distribution of patients admitted to the merged facility. Sicker patients are thought to be more likely to travel further for higher quality care, so if a merged facility is perceived to improve quality, the average patient after the merger might be sicker. Likewise, if common perception is that quality declined due to the merger, the average patient might be healthier because local sicker patients choose to travel further to an alternate hospital. While the first scenario may be less likely to bias hospital level analysis, the latter two possibilities have a strong likelihood to bias quality change estimates down and up, respectively.

Figures 2.2 and 2.3 show the variation in response to a hospital merger. Lighter gray zip codes represent drops in merged facility market share for heart disease patients one year after the merger. Darker gray zip codes represent increases in share, with black representing larger increases. Some mergers result in widespread decline in merged market share, as in Figure 2.2, which represents the Eden Medical Center and San Leandro Hospital merger from 2004. This drop in market share is especially visible in zip codes nearby Eden Medical Center, the parent facility. Other mergers result in a more varied response. Figure 2.3 depicts the impact of the Fresno Community Hospital and Valley Medical Center merger in 1999. In this case, merged market share falls in some zip codes, yet rises a great deal in others. It is unlikely that this variation is entirely random, which suggests that a hospital level analysis could be biased by differential changes in hospital choices by patients.

For this reason, it is necessary to analyze outcomes within population groups that are unlikely to change in response to a merger, such as geographic areas. Previous hospital merger research has either used geographic areas such as counties or health service areas (HSAs) or incorporated overlapping fixed radius market definitions. Both of these methods have drawbacks. HSAs and even counties are large enough that individuals on opposite ends of the market face a different set of likely hospital choices. In addition, individuals near the border of a market may be closer to a hospital outside of their county or HSA. Fixed radius definitions are difficult to establish because hospitals of different sizes and with different service offerings have different geographic reaches. In addition, picking a radius requires a

somewhat arbitrary decision of how far an individual is likely to travel to use a particular hospital.

I avoid these issues by utilizing a zip code-level analysis. Zip codes are fairly small, and thus all individuals within a single zip code are likely to face a similar set of relevant hospital choices. Utilizing zip codes also does not require any assumptions to be made over how far a particular hospital's reach is. Instead, I let patient choices dictate where merged hospitals are relevant. The share of patients within a zip code discharged from a merged facility is used to measure exposure to a merger. This methodology enables me to exploit the heterogeneity of exposure to a merger to measure its effect. If a merged facility changes its quality level or treatment practices, we would expect these changes to have a larger impact on a zip code with 40 percent of its discharges from the merged facility than on a zip code with 2 percent of discharges from the merged facility. In addition, individuals are unlikely to relocate their residence in response to a hospital merger, especially as compared to the likelihood of utilizing a different hospital. This allows me to compare pre- and post-merger outcomes for plausibly similar populations.

Another benefit of utilizing a zip code based analysis is that it includes all patients, whether or not they were discharged from a merged facility. Estimating the effect across an entire zip code includes not only the direct effect on patients using the merged facility but also the ripple effects of the merger. These include any changes in hospital choice decisions by patients as well as any changes made by hospitals in response to changes in the competitive landscape. While it will not be possible to disentangle the direct effect of the merger on quality at the merged facility

from the indirect effects of patient reallocation and non-merging hospital changes, this empirical methodology enables an analysis of how all patients were affected by the merger.

Relying entirely on merged market shares is insufficient for measuring the impact of a merger. This approach would be subject to the same potential selection bias as the hospital-level approach. I use a solution that is similar to the one used by Town, et al (2006). They bypass the issue of hospital choice by assigning market shares from 1990 to hospitals in the following years so that the only changes in market shares come from hospital mergers. As a result, any hospitals that open after 1990 are left out of their analysis. In addition, some information may be lost by relying on shares from 1990. This may be less of an issue for their analysis because they are analyzing market share in terms of staffed beds rather than number of discharges, and staffed bed levels may change less over time. I modify their approach by using shares of discharges from one year before the merger to predict shares of discharges in quarters following the merger¹⁶. Pre-merge parent and satellite market shares are used to create a predicted merged facility share. Thus, the IV estimations will only use the variation in post-merge shares that comes from the expected impact of the merger ex-ante, alleviating the selection bias concern.

As described above, the baseline model in OLS is:

$$y_{zt} = \alpha_1 + \theta_{1z} + \phi_{1t} + \gamma_1 X_{zt} + \beta_1 share * I(post)_{zt} + \varepsilon_{1zt} \quad (1)$$

The relationship between the explanatory variable and its instrument can be modeled as:

¹⁶ Shares from one year before the merger are used in case facility changes are made in anticipation of the merger. The earliest possible quarter is used for the four mergers that transpire less than one year into the study period.

$$share * I(post)_{zt} = \alpha_2 + \theta_{2z} + \phi_{2t} + \gamma_2 X_{zt} + \beta_2 pre_share * I(post)_{zt} + \varepsilon_{2zt} \quad (2)$$

In the above models, $share * I(post)_{zt}$ is the zip code-level share of patients discharged from merged hospital in all zip codes and quarters of admission. It equals zero before a merger takes place and in any zip codes unaffected by a merger. In a zip code affected by more than one merger, $share * I(post)_{zt}$ equals the sum of the shares of patients in each of the merged facilities. The instrument, $pre_share * I(post)_{zt}$, is the predicted merged share of discharges described above. It is calculated by adding together the parent and satellite shares of discharges from one year before the merger. In zip codes affected by one merger only, this variable equals zero in all quarters leading up to the merger, and equals the pre-merger share in all quarters following the merger. In zip codes affected by more than one merger, $pre_share * I(post)_{zt}$, increases by the pre-share values of subsequent mergers after they are completed¹⁷.

The outcome variables, represented by y_{zt} , are percent of patients receiving intensive procedures (defined as bypass surgery or angioplasty), percent of patients receiving their primary treatment within one day, average number of procedures, log of average charges, inpatient mortality, and average length of stay. Hospital mergers have the potential to increase the usage of intensive heart surgeries because only two mergers combined facilities that had both offered these procedures before the merger. Sixteen of the parent hospitals and four of the satellites had this capability. For the sixteen merged facilities in which only one of the facilities offered intensive heart surgery capabilities, individuals who would have used the other hospital automatically have increased access to these procedures. One might expect that the

¹⁷ These share variables are based on heart disease diagnoses. Using shares of all discharges yields similar results as the ones reported below.

incidence of their usage would increase with their availability. While it is reasonable to expect that a patient would be more likely to choose a hospital with these capabilities when she expects or plans to use them, it is conceivable that distance to the hospital may be a greater priority. There is also the possibility of induced demand: that increased availability increases the likelihood of utilization because it is now more convenient.

Number of procedures can be interpreted as another measure of treatment intensity. The percent of patients receiving treatment within one day is a potential measure of quality of care. Delays in treatment could reflect overcrowding or physician understaffing. Delays could also come from treatment by less experienced or less qualified physicians.¹⁸ The control variables, represented by X_{zt} , include demographic variables for characteristics including average age and the percentage of zip code discharges in categories for race and gender, insurance-status, and comorbidity diagnoses including diabetes, heart failure, and pneumonia. θ_z and ϕ_t represent zip code and year-quarter fixed effects, respectively.

The above model is identified on four main assumptions. First, while mergers are endogenous in terms of which hospitals merge and the merging partners chosen, their timing is not. Completing a merger involves a complex negotiation process as it requires developing a singular hospital board and unifying two medical staffs. As such, it is difficult to predict when a merger will be finalized, regardless of when the negotiations begin. Second, the composition of patients residing within a zip code

¹⁸ Doyle, Ewer, and Wagner (2008) find that teams of residents from a lower ranked medical school take more time to diagnose patients in a VA hospital than teams from a highly ranked medical school in same facility.

does not change because of the merger. While individuals may base moving decisions on available hospitals and their quality, this is unlikely to happen in response to a merger and is unlikely to represent a significant percent of any zip code's population in the future. The third identifying assumption is that pre-merge shares of discharges are not impacted by the impending merger.

The fourth assumption is that merging hospitals do not change their admission criteria for heart disease patients. I test this assumption by regressing the percent of AMI admissions on pre-merge shares, including zip code specific time trends to control for how zip code populations may be changing over time. The coefficient of 0.002 with a standard error of 0.007 suggests that heart attack admissions as a percent of heart disease admissions did not change. This in turn suggests that the average sickness of patients admitted is not correlated with exposure to a merger and that admission practices at merged facilities did not change post-merger¹⁹.

Table 2.6 reports the results from estimation of the first stage relationship described by equation (2) above. As reported in columns (1) and (2), the coefficient on pre_share_{zt} is precisely estimated as 0.84. The F statistics are well above 10, suggesting a strong first stage relationship. Columns (3) and (4) report the results of a similar estimation in which the pre-merger shares are interacted with 5 timing variables. Each of these timing variables is turned on for a different period following the merger: one each for the first four years after the merger transpires, and one that is turned on during and after the fifth post-merger year. The coefficients on the timing-interacted pre-merger share variables suggest that the relationship between pre- and

¹⁹ Conducting the same test with other demographic characteristics and measures of illness severity such as co-morbidity diagnoses and percentage of admissions through the emergency room yields similar results.

post-merger shares is not unique to the initial quarters or years after a merger. Merged facilities retain most of their market share even five years after the merger transpires, though there is a small decline in later years. Columns (5) - (8) report results of estimations similar to columns (1) – (4) with the addition of zip code specific time trends. Both the magnitude and statistical significance of the coefficients are similar between the two sets of columns.

Table 2.7 reports the OLS regression results for each of the six dependent variables listed above. The top panel reports the results from specifications without zip code time trends, and the bottom panel reports the results from specifications that include zip code time trends. The results listed in columns (1) – (2) for both panels suggest that merged hospitals increase usage of intensive heart surgeries. The coefficient of 0.043 in column (2) of the top panel suggests that a zip code with 20 percent of its heart disease discharges coming from a merged hospital will experience an increase in intensive surgeries of 0.86 percentage points. Considering that in 2000, 23.7 percent of heart disease patients received intensive heart surgeries, this point estimate represents a 3.6 percent increase. Zip codes with larger percentages of discharges from a merged facility will be impacted proportionally to their share. Given that the usage of heart surgeries is increasing over this time period, we may be concerned that this estimate is driven by this upward trend. The coefficient of 0.030 in the second panel of Table 2.7 suggests that merged hospitals increase the utilization of intensive surgeries in addition to the upward trend. This coefficient translates into a 2.5 percent increase in intensive surgery utilization for the benchmark zip code described above.

The next four columns in Table 2.7 list the coefficients for the percent of patients receiving their primary treatment within one day and the average number of procedures performed on an individual. In the first panel, both of these outcomes appear to be significantly impacted by hospital mergers. The coefficients of 0.075 for treatment within one day and 0.444 for number of procedures each translate to a 3.2 percent increase above 2000 averages for the benchmark zip code. However, when time trends are added, both coefficients become economically and statistically insignificant. Log of average charges, which is listed in the columns (7) – (8), does not appear to be impacted by hospital mergers, regardless of whether time trends are included.

The final two outcomes, inpatient mortality and average length of stay, are listed in the last four columns of the table. These outcomes are discussed together because it is possible for hospital discharge practices to impact inpatient mortality rates. If we see a decrease in average length of stay coupled with a decrease in inpatient mortality, then we may be skeptical of the mortality result. In this case, however, it appears that inpatient mortality may increase after a merger, whereas average length of stay appears to fall once time trends are included. The implied impact on inpatient mortality rates is also larger than the impact on average length of stay. For the benchmark zip code, inpatient mortality may increase by 1.1 to 1.6 percent from its 2000 average, though the estimates are not precise. Average length of stay, however, changes by less than 1 percent from its 2000 average for either coefficient.

As described above, OLS results may be biased if individuals change their hospital choices in response to a perceived quality change from a merger. Table 2.8 reports the same results as Table 2.7 for the IV specification. In general, the IV specification suggests a larger impact on treatment intensity. For example, the estimated impact of mergers on utilization of intensive treatment rises to 2.9 percent for the benchmark zip code even after time trends are included. The coefficients for treatment within one day and average number of procedures are larger as well, though they still shrink to economic and statistical insignificance when time trends are included. The impact on log of average charges remains small and statistically insignificant regardless of time trend inclusion.

The most striking change between the OLS and IV specification is the estimated impact on inpatient mortality. The coefficient on merged market share increases to 0.003 without time trends and to 0.005 with time trends. Both coefficients are statistically significant at the 5 percent significance level. For the benchmark zip code, these point estimates suggest an increase in inpatient mortality of 1.6 percent and 2.7 percent, respectively. The small and statistically insignificant coefficients for the average length of stay outcome suggest that the impact on inpatient mortality is not driven by changes in discharge practices. It is perhaps surprising that inpatient mortality rises despite the increase in treatment intensity described above. I provide a potential explanation for this result below.

Given the average post-merger results described above, we may question whether these effects are driven by transitory changes after a merger or long-term changes. Table 2.9 provides evidence of how the impact of a merger changes with

time. As demonstrated earlier, OLS estimates are subject to some selection bias. This bias was particularly apparent in the inpatient mortality specifications. To bypass this potential source of bias, I use the instrument, pre-merge share, for this set of specifications instead of actual market share. Pre-merge share is interacted with the same set of timing dummy variables as in the first stage specifications, with the addition of an indicator for quarters within one year before a merger for a particular zip code. This addition is included to confirm that mergers did not have an impact on patient experiences before the merger transpired.

The first observation to note from this table is that, with two exceptions, mergers do not impact patients before the merger has transpired. The first exception is the treatment within one day specifications in the first panel. As was already noted, the results for this measure of treatment intensity are part of an increasing trend. The inclusion of time trends results in a large reduction in magnitude for all of the timing-interacted pre-merge share variables. In addition, these coefficients are no longer statistically significant at any level once time trends are included. The second exception is the average length of stay outcome once time trends are included. The pre-merge interaction variable has a coefficient that is both more negative and more precisely estimated than any of the post-merger interaction terms. This negative coefficient could come from adjustments just before the merger considering that length of stay is determined by the endpoint of the admission. In addition, while this coefficient is statistically significant, it only represents a 1 percent decrease in length of stay for the benchmark zip code from the 2000 average. Considering that most mergers transpire before 2000 and length of stay falls a great deal during the 1990s,

this represents an even smaller change in most cases, plausibly coming from discharges at the end of the quarter. Thus we shouldn't be concerned about changes happening in anticipation of a merger.

The second observation is that the results from Tables 2.7 and 2.8 are not driven by transitory changes. Instead, most effects grow with time following a merger. For example, when time trends are included, the increase in percentage of patients receiving intensive treatments rises from 2.7 percent in the first year after a merger to 3.5 percent in the fourth year after a merger for the zip code described above. The increases in inpatient mortality follow a similar trend. In addition, the number of procedures appears to increase with time after a merger, even when time trends are included. In later years, these coefficients are statistically significant and translate to increases of 2.4 percent or more from a base of 2.8 procedures for the benchmark zip code.

2.6 Alternate Specifications

The main specification described in section five does not differentiate between zip codes primarily exposed to a merger from the parent from those primarily exposed through the satellite. As demonstrated in Table 2.4, parent and satellite facilities differ in several ways. Thus, their patients may be differentially impacted. The main specification also does not analyze whether mergers that have a large effect on market concentration differ from those that do not. Given the findings of the competition and quality literature, we may expect mergers with more market overlap

to have different effects than those with less. This section will explore these two questions.

2.6.1 The Differential Effect of Satellite Facilities

The changes experienced by parent and satellite hospitals are usually different. As described above, there is often some degree of service consolidation that comes with a merger. In these cases, the parent facility is more likely to retain the cardiac service specialty. Thus, we may expect patients who would have used the satellite in the absence of the merger to be differentially impacted compared to those who would have used the parent facility. The specification reported below explores this possibility.

Because all discharges for a merged facility are attributed to the merged facility license, Tables 2.7 and 2.8 cannot be replicated with market shares split into parent and satellite shares. I use a reduced form specification with the addition of pre-merge satellite shares to investigate how patients are differentially impacted by exposure through the satellite. Table 2.10 reports these results. Three observations stand out. First, the impact of a merger on the utilization of intensive treatments is not driven solely by exposure through the satellite facility. We might expect this increase to be particularly prominent for satellite patients because these individuals are more likely to experience an increase in access to intensive procedures. Even with the inclusion of time trends, the coefficient on pre-merger share is positive and statistically significant, while the coefficient on satellite share is insignificant. Thus, exposure through the satellite does not appear to drive the increase in treatment intensity. This result suggests that the increase in utilization of intensive procedures

is not merely a mechanical effect from an increase in access for satellite patients. Instead, hospital mergers appear to increase the propensity for prescribing these procedures for all patients.

The second notable observation is that the average number of procedures appears to increase for satellite patients, even with the inclusion of time trends. The coefficient of 0.372 translates into a 1.4 percent increase in number of procedures from the 2000 average of 2.8 procedures for a zip code with a 10 percent pre-merge share in the satellite facility. The third, and possibly most important, observation is that the increase in inpatient mortality is potentially driven by exposure through the satellite facility. Without time trends, the coefficient on pre-merge share is roughly zero while the coefficient on satellite share is a precisely estimated 0.012. When time trends are included, neither coefficient is statistically significant, though the satellite share coefficient is over twice that on overall pre-merge shares. This effect could be driven by an increase in the average travel time and distance to care for satellite patients. Exposure through the satellite may be larger when the two facilities are further apart, increasing the impact on inpatient mortality. This interpretation is consistent with Buchmueller, et al (2006), which found that urban hospital closures in Los Angeles were associated with increases in distance to care that resulted in increased mortality from heart attacks and unintentional injuries.

2.6.2 The Effect of Increased Competition

Much of the concern with hospital mergers stems from their impact on market concentration. Thus far my analysis has not differentiated between mergers that have a large impact on local market concentration and those that have a smaller impact.

The effect on market concentration varies across mergers, as evidenced by a comparison of Figures 2.4 and 2.5. Figure 2.4 is a map of the zip codes surrounding Fresno Community Hospital and Valley Medical Center in Fresno County. Zip codes are shaded based on the predicted change in HHI from the merger of the two facilities. Predicted change in HHI is calculated as follows:

$$\text{predicted_dHHI} = (\text{parent_share} + \text{satellite_share})^2 - \text{parent_share}^2 - \text{satellite_share}^2$$

As above, parent and satellite shares are measured one year before the merger transpires. Thus, *predicted_dHHI* predicts the impact of a merger on market concentration absent any changes in market shares beyond those that stem directly from the merger.

Figure 2.4 shows that there were several zip codes with large potential changes in market concentration from the merger of Fresno Community Hospital and Valley Medical Center. This is unsurprising because the two facilities are only 2 miles apart. Other mergers had a minimal impact on local market concentration. One example is shown in Figure 2.5. Saddleback Memorial Center and San Clemente Hospital are located 14 miles apart, and thus have much lower overlap in shares of patients. In this case, there is a much smaller potential impact on market concentration. It is plausible that the impact of a merger would vary with its impact on concentration, particularly given the findings from the literature on the correlation between concentration and quality of care.

To investigate how the impact of a merger changes with its effect on market concentration, I estimate a regression similar to (1) above, with the addition of HHI. Like market share, HHI is subject to selection bias from patients' perceived quality

differences across hospitals. To bypass this issue, I use the predicted change in HHI measure described above to instrument for HHI. Table 2.11 reports the first stage results for post-merger market share and HHI and their instruments. The relationship between parent and satellite shares and post-merger market share and the relationship between predicted change in HHI and HHI are robust to both the inclusion of control variables and of time trends. F-statistics are above 10 in all cases. The time interacted results are not included here, though they follow a similar trend to the time-interacted first stage results described above.

The IV specification results are reported in Table 2.12. In general, these results suggest that mergers may have a stronger effect on treatment intensity when they have a larger impact on market concentration. For example, the coefficient on HHI for utilization of intensive treatments is 0.128 when time trends are excluded and 0.141 when they are included. The percentage of patients who receive their primary treatment within one day is also affected by a merger's impact on HHI, though only the coefficient in the second panel is statistically significant, and then only at the 10 percent significance level. While not precisely estimated, the coefficient on HHI for inpatient mortality is fairly large and negative both with and without time trends. Considering that the increase in inpatient mortality may be driven by an increase in average travel time to a hospital, it is perhaps unsurprising that this coefficient would be negative. Mergers that have a large effect on HHI are likely to involve facilities that are geographically close to each other, thus alleviating the travel time issue. It is possible that the concentration-induced increases in treatment intensity described above partially stem from geographical distance rather than reduced competition. If a

merging pair of hospitals is closer to each other, they may be more likely to retain patients from both facilities regardless of the extent of service consolidation. Thus the merged hospital may have more of an impact mechanically through treating more individuals.

2.7 Discussion

The results detailed above suggest that hospital mergers both increase utilization of intensive treatments and increase the incidence of inpatient mortality. Increases in treatment intensity through other measures, such as receiving treatment within one day or number of procedures are possible, though these results are not robust to the inclusion of zip code specific time trends. Patients who are exposed through the satellite facility appear to be most affected by a merger, both in their utilization of intensive heart surgeries and in their incidence of inpatient mortality. These patients also experience an increase in the number of procedures received during a hospital stay that is robust to the inclusion of time trends. It should be noted, though, that the increase in usage of intensive heart surgeries is experienced by patients exposed to a merger through the parent as well. This suggests that increases in heart surgeries are not solely originating from an increase in access; merged facilities are increasing their provision of these surgeries across the board. These results differ from those found in both Ho and Hamilton (2000) and Capps (2005) in that I find statistically significant and robust increases in inpatient mortality for heart disease patients. This difference may be driven by the fact that I study a larger number of mergers across a longer time period, increasing the statistical power

of my study. However, the difference may also be driven by capturing the effects across all patients, including those who changed hospitals in response to the merger and those treated at hospitals whose behavior responded to the merger. The difference in mortality results between the OLS and IV specifications suggests that not accounting for patient composition may have biased the results in previous literature.

It is difficult to discern from the results listed above whether the increases in treatment intensity are welfare enhancing or reducing. An increase in the provision of bypass surgery and angioplasty may improve the length or quality of life for the individuals who would not have otherwise received these treatments. The volume-outcome literature suggests that increases in the number of these surgeries performed yields improved outcomes as well. However, any improvements must be weighed against the additional cost of providing the treatments. In addition, previous research has found that regions prone to more intensive treatment do not achieve improved outcomes. In fact, outcomes among heart attack patients were found to be worse in these regions (Fisher, et al 2003). Access to readmission rates and 30-day mortality rates may shed light on this question, though it would remain difficult to accurately assess improvements in quality of life against their costs. This question requires further investigation.

Table 2.1: Financial and Discharge Statistics for Selected Years

	1990	1995	2000	2005
Patient Characteristics:				
% Male	39.8%	39.9%	39.0%	39.5%
% Black	8.6%	8.8%	7.5%	7.1%
% Hispanic	22.5%	25.7%	27.0%	29.8%
% Aged 65+	25.9%	29.5%	31.2%	30.2%
% Medicare	24.8%	28.2%	31.5%	31.9%
% Medicaid	22.3%	27.1%	23.1%	25.8%
% Die	2.5%	2.4%	2.3%	2.2%
Average Length of Stay	5.43	4.86	4.55	4.55
Average Charges	9134	13706	22568	36684
Hospital Characteristics:				
Average # Discharges	7559	7761	8560	9597
Operating Revenue/Expenditures	1.23	1.03	1.00	1.02
Revenue/Expenditures	1.28	1.07	1.04	1.06
Available Beds	190	188	197	212
Registered Nurse Hrs/Beds	1440	1533	1810	2205
Lic. Vocational Nurse Hrs/Beds	243	224	230	238
# For-Profit	125	115	109	91
# Government	96	87	69	68
# Non-Profit	234	238	228	208
Total # Hospitals	455	440	406	367

* Financial statistics are weighted by available beds; discharge statistics are weighted by number of discharges.

Table 2.2: Descriptive Statistics for All Ischemic Heart Disease Diagnoses for Selected Years

Year	1990	1995	2000	2005
% With the Following Demographic Characteristics:				
Male	58.2%	58.4%	57.2%	58.4%
Black	6.6%	6.7%	6.5%	7.2%
Hisp	9.0%	11.4%	11.5%	14.5%
Aged 60-74	43.6%	41.7%	36.9%	35.1%
Aged 75+	31.1%	34.7%	40.8%	41.6%
% With the Following Expected Payer:				
Medicare	57.2%	58.3%	61.7%	62.9%
Private	30.5%	28.9%	25.8%	22.9%
# procedures	2.9	2.8	2.8	3.0
Length of Stay (days)	6.2	5.1	4.6	4.4
Charges (1990\$)	17395	21810	32301	44430
Received Bypass Surgery or Angioplasty	22.4%	25.8%	27.3%	27.9%
% Treat Within 1 Day	53.8%	52.7%	47.3%	47.9%
% With the Following Co-morbidity Diagnoses:				
Heart Attack	25.5%	25.9%	26.4%	24.2%
Diabetes	21.2%	27.0%	32.8%	38.4%
Heart Failure	25.8%	31.2%	32.6%	36.8%
Hypertension	34.2%	49.4%	60.3%	72.0%
% With the Following Admission & Discharge Characteristics:				
Admitted from (own) ER	55.2%	53.6%	59.3%	61.6%
Discharged Home	73.0%	68.5%	67.1%	67.1%
Die	4.6%	4.0%	3.7%	3.2%
Average # Ischemic Discharges (unweighted)	508	588	695	713
# Hospitals	461	448	408	382

* All averages are weighted by number of discharges unless otherwise specified

** The data sample includes all discharges with a major diagnostic category of "circulatory system, diseases & disorders" (5) and any diagnostic code of "Ischemic Heart Disease" (ICD-9 codes 410-414) in relevant hospitals.

*** Note that the number of hospitals may from the discharge data set may be larger than the number of hospitals from the financial data set for two reasons. Some satellite hospitals continue to have observations after a merger. In addition, discharge data is based on calendar year rather than fiscal year, so that if a hospital closes in fiscal year X, it may still appear in the discharge data for calendar year X+1.

Table 2.3: Timing of Mergers

Year	# Merging Hospitals	# Mergers Completed	Total # Mergers	# Other Hospitals
1990	74	4	4	381
1991	73	1	5	380
1992	71	2	7	381
1993	69	2	9	381
1994	66	3	12	379
1995	64	2	14	376
1996	59	5	19	380
1997	55	4	23	375
1998	53	2	25	368
1999	50	3	28	361
2000	47	3	31	359
2001	45	2	33	355
2002	44	1	34	354
2003	40	2	36	352
2004	37	3	39	346
2005	35	1	40	332

* Numbers of hospitals and mergers are as of the end of the calendar year.

** 2 merged hospitals closed during 2003 and one closed at the end of 2004; two parent hospitals were involved in two mergers each.

Table 2.4: Comparing Merging and Non-Merging Hospitals - An Overview

	Parent Hospital Year Before Merge	Satellite Hospital Year Before Merge	Merged Hospital Year Afer Merge	Non-Merging Hospitals (1995)
Beds:				
Licensed	264	168	397	207
Available	232	145	339	185
Delivery Room	78.4%	48.6%	91.9%	54.6%
NICU	51.4%	27.0%	64.9%	29.1%
Surgery Services:				
Neurological	78.4%	54.1%	83.8%	44.8%
Heart	33.3%	13.9%	41.7%	28.1%
Open Heart	32.4%	8.1%	37.8%	22.6%
Organ Transplant	13.5%	10.8%	21.6%	7.9%
MRI	54.1%	43.2%	67.6%	37.7%
Emergency Room	89.2%	81.1%	91.9%	65.6%

* Note that 3 mergers are excluded because pre-merger data was unavailable for the satellite facility.

Table 2.5: Pre/Post Merger Statistics for Ischemic Heart Disease Diagnoses

Quarter With Respect to Merge	-4	-3	-2	-1	0	1	2	3	4	5	6
# procedures	2.6	2.6	2.6	2.7	2.7	2.7	2.7	2.8	2.8	2.9	3.0
Length of Stay (days)	4.6	4.7	4.7	4.5	4.5	4.6	4.6	4.4	4.4	4.5	4.6
Charges (\$)	30965	32890	32801	32756	34623	35661	36757	37174	37704	39246	41449
Received Bypass Surgery or Angioplasty	23.2%	23.1%	23.4%	22.9%	24.6%	24.5%	26.4%	26.9%	26.7%	27.8%	28.0%
% Treat Within 1 Day	46.9%	47.4%	48.4%	48.8%	47.5%	47.9%	48.7%	50.2%	50.5%	49.6%	50.0%
% With the Following Co-morbidity Diagnoses:											
Heart Attack	24.4%	25.1%	26.3%	24.6%	25.0%	25.4%	25.7%	24.7%	25.1%	25.3%	24.4%
Diabetes	29.7%	31.3%	31.3%	30.9%	32.5%	31.6%	32.0%	32.1%	32.0%	32.5%	33.0%
Heart Failure	33.6%	34.2%	34.4%	33.2%	33.6%	34.0%	35.7%	32.8%	33.6%	34.9%	34.1%
Hypertension	56.0%	56.2%	56.9%	57.1%	57.5%	57.2%	59.3%	58.7%	59.8%	60.4%	59.7%
% With the Following Admission & Discharge Characteristics:											
Admitted from (own) ER	57.6%	58.8%	58.0%	57.9%	58.4%	58.8%	59.1%	59.4%	60.2%	58.3%	59.1%
Discharged Home	68.3%	67.5%	67.1%	68.0%	68.3%	68.2%	68.5%	68.5%	69.5%	69.4%	69.8%
Die	3.8%	4.3%	4.2%	3.9%	3.8%	4.0%	3.9%	3.4%	3.7%	3.7%	3.7%
Average # Ischemic Discharges (unweighted)	152	155	158	155	146	274	277	278	276	279	280
# Hospitals	68	67	67	67	67	36	36	36	36	35	36

* All averages are weighted by number of discharges unless otherwise specified

** The data sample includes all discharges with a major diagnostic category of "circulatory system, diseases & disorders" (5) and any diagnostic code of "Ischemic Heart Disease" (ICD-9 codes 410-414) in relevant hospitals.

*** Only mergers/hospitals with all 13 quarters of data available are included. One satellite hospital stops serving heart disease patients three quarters before its merger. Another merged facility has no discharges in the fifth quarter after its merger, then begins operating again.

Table 2.6: First Stage Relationship between Share*(Post) and Pre-Merge Share

	without zip trends				with zip trends			
Pre-Merge Share	0.839 (0.020)***	0.838 (0.020)***			0.887 (0.018)***	0.886 (0.018)***		
Pre-Merge Share * Year 1 (post)			0.823 (0.022)***	0.822 (0.021)***			0.828 (0.024)***	0.828 (0.024)***
Pre-Merge Share * Year 2 (post)			0.807 (0.023)***	0.805 (0.022)***			0.813 (0.026)***	0.813 (0.025)***
Pre-Merge Share * Year 3 (post)			0.817 (0.022)***	0.815 (0.022)***			0.827 (0.026)***	0.827 (0.026)***
Pre-Merge Share * Year 4 (post)			0.807 (0.021)***	0.806 (0.021)***			0.823 (0.027)***	0.823 (0.027)***
Pre-Merge Share * Year 5+ (post)			0.794 (0.023)***	0.793 (0.023)***			0.809 (0.029)***	0.809 (0.029)***
Constant	0 (0.002)	0.02 (0.036)	0 (0.002)	0.022 (0.038)	-0.002 (0.001)**	0.011 (0.021)	-0.002 (0.001)**	0.012 (0.022)
Observations	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.92	0.92	0.92	0.92	0.95	0.95	0.94	0.94
F statistic for instrument(s)	1663.04	1711.85	335.38	340.41	1796.74	1838.27	261.93	340.41
Control Variables	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.7: OLS Regression Results

	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.048	0.043	0.084	0.075	0.522	0.444	-0.004	-0.030	0.005	0.002	0.230	0.150
	(0.010)***	(0.009)***	(0.018)***	(0.018)***	(0.122)***	(0.122)***	(0.029)	(0.029)	(0.001)***	(0.001)*	(0.145)	(0.138)
Constant	0.216	0.270	0.524	0.628	2.893	2.987	9.696	9.680	0.049	0.000	6.323	4.319
	(0.002)***	(0.029)***	(0.004)***	(0.056)***	(0.025)***	(0.307)***	(0.008)***	(0.112)***	(0.001)***	(0.012)	(0.081)***	(0.620)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.6	0.68	0.48	0.52	0.48	0.52	0.86	0.87	0.11	0.16	0.69	0.69

With Zip Code-Specific Time Trends:

	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.035	0.030	0.005	0.003	0.027	0.018	-0.022	-0.021	0.003	0.003	-0.223	-0.183
	(0.006)***	(0.005)***	(0.015)	(0.014)	(0.104)	(0.101)	(0.021)	(0.020)	(0.002)	(0.002)	(0.107)**	(0.103)*
Constant	0.217	0.272	0.523	0.663	2.889	3.025	9.699	9.717	0.050	-0.001	6.313	4.188
	(0.002)***	(0.027)***	(0.003)***	(0.045)***	(0.020)***	(0.262)***	(0.008)***	(0.107)***	(0.001)***	(0.012)	(0.081)***	(0.605)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.65	0.72	0.61	0.64	0.61	0.64	0.88	0.89	0.13	0.18	0.72	0.73
Control Variables	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.8: IV Regression Results

	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.043	0.042	0.109	0.104	0.593	0.524	0.01	-0.022	0.007	0.003	0.33	0.171
	(0.009)***	(0.008)***	(0.019)***	(0.018)***	(0.122)***	(0.119)***	(0.035)	(0.034)	(0.002)***	(0.001)**	(0.155)**	(0.144)
Constant	0.087	0.19	0.426	0.605	2.437	2.933	9.57	9.588	0.045	0.002	6.904	3.933
	(0.002)***	(0.029)***	(0.005)***	(0.056)***	(0.025)***	(0.307)***	(0.008)***	(0.115)***	(0.001)***	(0.012)	(0.080)***	(0.679)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.6	0.68	0.48	0.52	0.48	0.52	0.86	0.87	0.11	0.16	0.69	0.69
With Zip Code-Specific Time Trends:												
	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.039	0.034	0.011	0.009	0.067	0.054	0.005	0.004	0.005	0.005	-0.107	-0.029
	(0.006)***	(0.006)***	(0.020)	(0.019)	(0.136)	(0.133)	(0.026)	(0.025)	(0.002)**	(0.002)**	(0.129)	(0.118)
Constant	0.073	0.184	0.442	0.608	2.426	2.744	9.536	9.591	0.046	0.002	7.199	4.31
	(0.002)***	(0.027)***	(0.004)***	(0.046)***	(0.021)***	(0.266)***	(0.008)***	(0.108)***	(0.001)***	(0.012)	(0.079)***	(0.631)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.65	0.72	0.61	0.64	0.61	0.64	0.88	0.89	0.13	0.18	0.72	0.73
Control Variables	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.9: OLS Regressions on Timing Interacted Pre-Merge Share

	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Pre-Merge Share * Year 1 (pre)	-0.002	0.003	0.046	0.044	0.166	0.142	-0.048	-0.048	0.004	0.002	-0.166	-0.156
	(0.007)	(0.006)	(0.014)***	(0.012)***	(0.111)	(0.100)	(0.036)	(0.033)	(0.002)*	(0.002)	(0.106)	(0.107)
Pre-Merge Share * Year 1 (post)	0.03	0.033	0.047	0.046	0.19	0.164	-0.043	-0.047	0.006	0.003	-0.055	-0.04
	(0.007)***	(0.007)***	(0.016)***	(0.014)***	(0.095)**	(0.090)*	(0.035)	(0.034)	(0.002)**	(0.002)	(0.113)	(0.107)
Pre-Merge Share * Year 2 (post)	0.032	0.031	0.058	0.054	0.305	0.273	0.002	-0.003	0.003	0.002	-0.028	-0.019
	(0.008)***	(0.006)***	(0.018)***	(0.017)***	(0.118)***	(0.112)**	(0.035)	(0.035)	(0.002)	(0.002)	(0.127)	(0.114)
Pre-Merge Share * Year 3 (post)	0.029	0.033	0.069	0.068	0.364	0.345	-0.009	-0.019	0.006	0.004	0.059	0.016
	(0.008)***	(0.007)***	(0.017)***	(0.017)***	(0.124)***	(0.118)***	(0.037)	(0.037)	(0.002)***	(0.002)	(0.123)	(0.115)
Pre-Merge Share * Year 4 (post)	0.039	0.038	0.083	0.081	0.551	0.49	-0.007	-0.033	0.007	0.003	0.253	0.125
	(0.009)***	(0.008)***	(0.018)***	(0.017)***	(0.113)***	(0.108)***	(0.036)	(0.035)	(0.002)***	(0.002)	(0.141)*	(0.137)
Pre-Merge Share * Year 5+ (post)	0.041	0.039	0.139	0.131	0.78	0.681	0.035	-0.012	0.006	0.002	0.501	0.263
	(0.011)***	(0.009)***	(0.018)***	(0.017)***	(0.119)***	(0.118)***	(0.033)	(0.034)	(0.002)***	(0.001)	(0.188)***	(0.178)
With Zip Code-Specific Time Trends:												
	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Pre-Merge Share * Year 1 (pre)	0.001	0.003	0.016	0.019	0.052	0.06	-0.019	-0.016	0.004	0.003	-0.264	-0.265
	(0.007)	(0.006)	(0.016)	(0.015)	(0.144)	(0.137)	(0.027)	(0.026)	(0.002)*	(0.002)	(0.108)**	(0.121)**
Pre-Merge Share * Year 1 (post)	0.034	0.032	0.015	0.015	0.05	0.039	-0.016	-0.018	0.005	0.004	-0.211	-0.17
	(0.007)***	(0.006)***	(0.018)	(0.017)	(0.140)	(0.134)	(0.029)	(0.028)	(0.003)**	(0.003)	(0.134)	(0.135)
Pre-Merge Share * Year 2 (post)	0.039	0.031	0.02	0.014	0.161	0.137	0.028	0.025	0.003	0.004	-0.198	-0.122
	(0.007)***	(0.007)***	(0.023)	(0.022)	(0.175)	(0.169)	(0.031)	(0.031)	(0.003)	(0.003)*	(0.137)	(0.134)
Pre-Merge Share * Year 3 (post)	0.041	0.035	0.022	0.016	0.24	0.218	0.014	0.007	0.006	0.006	-0.066	-0.023
	(0.007)***	(0.007)***	(0.024)	(0.022)	(0.191)	(0.186)	(0.035)	(0.036)	(0.003)**	(0.003)**	(0.152)	(0.148)
Pre-Merge Share * Year 4 (post)	0.054	0.041	0.025	0.015	0.409	0.33	0.003	-0.024	0.007	0.006	0.127	0.124
	(0.008)***	(0.008)***	(0.029)	(0.027)	(0.198)**	(0.192)*	(0.042)	(0.041)	(0.003)**	(0.003)*	(0.180)	(0.178)
Pre-Merge Share * Year 5+ (post)	0.07	0.047	0.058	0.037	0.658	0.515	0.017	-0.033	0.008	0.007	0.284	0.242
	(0.009)***	(0.008)***	(0.032)*	(0.029)	(0.238)***	(0.233)**	(0.047)	(0.048)	(0.003)**	(0.003)**	(0.188)	(0.181)
Control Variables	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with comorbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.10: OLS Regressions on Pre-Merge Merged and Satellite Shares

	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Pre-Merge Share*(Post)	0.051 (0.013)***	0.049 (0.012)***	0.064 (0.024)***	0.061 (0.023)***	0.479 (0.152)***	0.440 (0.149)***	0.028 (0.036)	0.018 (0.036)	0.000 (0.002)	-0.001 (0.002)	0.433 (0.171)**	0.451 (0.173)***
Satellite Share*(Post)	-0.032 (0.024)	-0.030 (0.021)	0.081 (0.047)*	0.078 (0.043)*	0.047 (0.260)	-0.011 (0.254)	-0.044 (0.068)	-0.092 (0.069)	0.018 (0.005)***	0.012 (0.004)***	-0.419 (0.325)	-0.852 (0.300)***
Constant	0.216 (0.002)***	0.27 (0.029)***	0.524 (0.004)***	0.63 (0.055)***	2.893 (0.025)***	2.991 (0.308)***	9.696 (0.008)***	9.679 (0.111)***	0.049 (0.001)***	0.001 -0.012	6.324 (0.081)***	4.312 (0.620)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.6	0.68	0.48	0.52	0.48	0.52	0.86	0.87	0.11	0.16	0.69	0.69
With Zip Code-Specific Time Trends:												
	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Pre-Merge Share*(Post)	0.022 (0.009)**	0.025 (0.008)***	0.009 (0.019)	0.015 (0.018)	-0.049 (0.109)	-0.023 (0.102)	-0.017 (0.032)	-0.005 (0.029)	0.003 (0.003)	0.003 (0.003)	-0.126 (0.152)	-0.035 (0.139)
Satellite Share*(Post)	0.055 (0.022)**	0.027 (0.020)	0.016 (0.037)	-0.010 (0.038)	0.471 (0.220)**	0.372 (0.220)*	0.032 (0.080)	0.006 (0.075)	0.005 (0.006)	0.007 (0.006)	-0.214 (0.349)	-0.206 (0.308)
Constant	0.217 (0.002)***	0.272 (0.027)***	0.523 (0.003)***	0.663 (0.045)***	2.89 (0.020)***	3.018 (0.262)***	9.699 (0.008)***	9.716 (0.107)***	0.05 (0.001)***	-0.001 -0.012	6.314 (0.081)***	4.187 (0.605)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.65	0.72	0.61	0.64	0.61	0.64	0.88	0.89	0.13	0.18	0.72	0.73
Control Variables	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.11: First Stage Regressions of Share*(Post) and HHI on Parent & Satellite Shares and Predicted Change in HHI

	without zip trends				with zip trends			
	HHI		Share * (Post)		HHI		Share * (Post)	
Parent Share	0.024 (0.027)	0.016 (0.024)	0.905 (0.030)***	0.902 (0.030)***	0.024 (0.025)	0.019 (0.022)	0.917 (0.027)***	0.916 (0.027)***
Satellite Share	-0.024 (0.039)	-0.009 (0.037)	0.659 (0.041)***	0.657 (0.041)***	-0.03 (0.032)	-0.004 (0.031)	0.739 (0.037)***	0.739 (0.037)***
Predicted DHHI	0.752 (0.166)***	0.706 (0.161)***	0.126 (0.154)	0.14 (0.154)	0.826 (0.143)***	0.767 (0.133)***	0.159 (0.145)	0.162 (0.144)
Constant	0.261 (0.003)***	0.156 (0.045)***	0 (0.001)	0.021 (0.036)	0.261 (0.002)***	0.158 (0.032)***	-0.002 (0.001)**	0.016 (0.022)
Observations	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.83	0.84	0.92	0.92	0.88	0.89	0.95	0.95
F statistic for instrument(s)	16.09	15.41	655.77	673.26	20.63	24.23	720.43	735.00
Control Variables	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Table 2.12: IV Regressions Including HHI

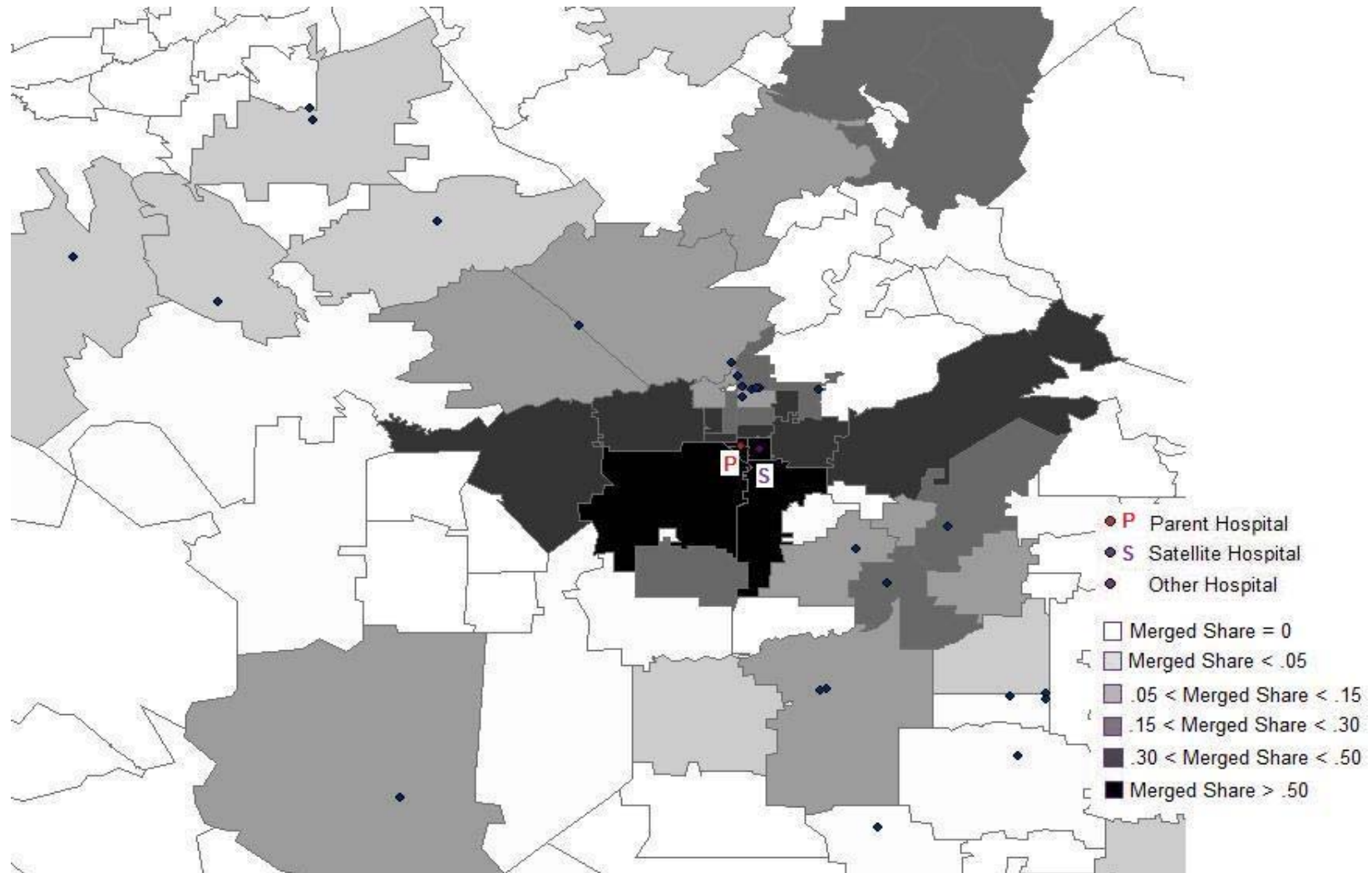
	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.037	0.026	0.085	0.059	0.33	0.155	0.045	-0.01	0.008	0.005	0.324	0.186
	(0.012)***	(0.010)**	(0.028)***	(0.028)**	(0.171)*	(0.171)	(0.056)	(0.056)	(0.003)***	(0.002)**	(0.224)	(0.200)
HHI	0.051	0.128	0.151	0.324	1.834	2.764	-0.247	-0.072	-0.016	-0.018	0.077	0.034
	(0.077)	(0.069)*	(0.180)	(0.200)	(0.929)**	(1.058)***	(0.277)	(0.296)	(0.015)	(0.015)	(1.031)	(0.942)
Constant	0.083	0.194	0.413	0.614	2.289	3.014	9.59	9.586	0.047	0.002	6.898	3.934
	(0.007)***	(0.029)***	(0.016)***	(0.057)***	(0.080)***	(0.314)***	(0.024)***	(0.116)***	(0.002)***	(0.012)	(0.113)***	(0.684)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.6	0.68	0.47	0.49	0.46	0.49	0.86	0.87	0.11	0.16	0.69	0.69
With Zip Code-Specific Time Trends:												
	% Intensive Treatment		% Treated Within One Day		Average # Procedures		Log Average Charges		% Die		Average Length of Stay	
Share*(Post)	0.035	0.014	-0.011	-0.025	-0.017	-0.106	0.008	-0.006	0.006	0.007	0.021	0.155
	(0.011)***	(0.011)	(0.025)	(0.025)	(0.159)	(0.159)	(0.043)	(0.043)	(0.003)*	(0.003)**	(0.196)	(0.185)
HHI	0.022	0.141	0.155	0.251	0.526	1.128	-0.033	0.064	-0.006	-0.015	-0.888	-1.351
	(0.061)	(0.070)**	(0.139)	(0.146)*	(0.859)	(0.897)	(0.207)	(0.215)	(0.018)	(0.019)	(1.026)	(0.943)
Constant	0.071	0.186	0.427	0.612	2.376	2.765	9.539	9.592	0.047	0.002	7.284	4.286
	(0.006)***	(0.027)***	(0.014)***	(0.046)***	(0.088)***	(0.270)***	(0.021)***	(0.108)***	(0.002)***	(0.012)	(0.133)***	(0.633)***
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396
R-squared	0.65	0.71	0.6	0.63	0.61	0.64	0.88	0.89	0.13	0.18	0.72	0.73
Control Variables	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Quarter Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Zip Code Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

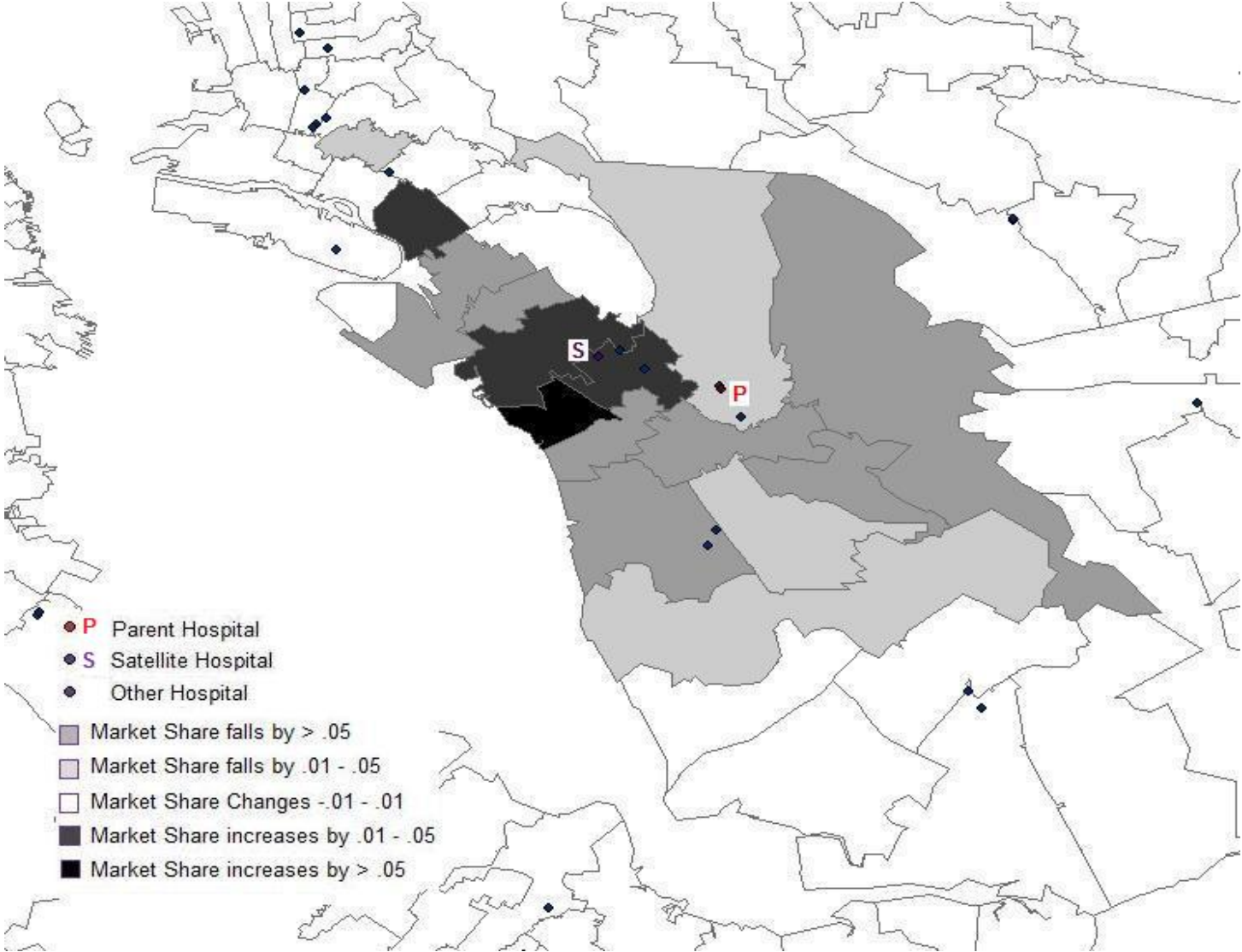
Zip codes are only included if they have 15 or more discharges in every quarter. All regressions are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percent of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; and admission through the ER.

Figure 2.1: Fresno Community Hospital/Valley Medical Center Merged Market Shares at Time of Merger



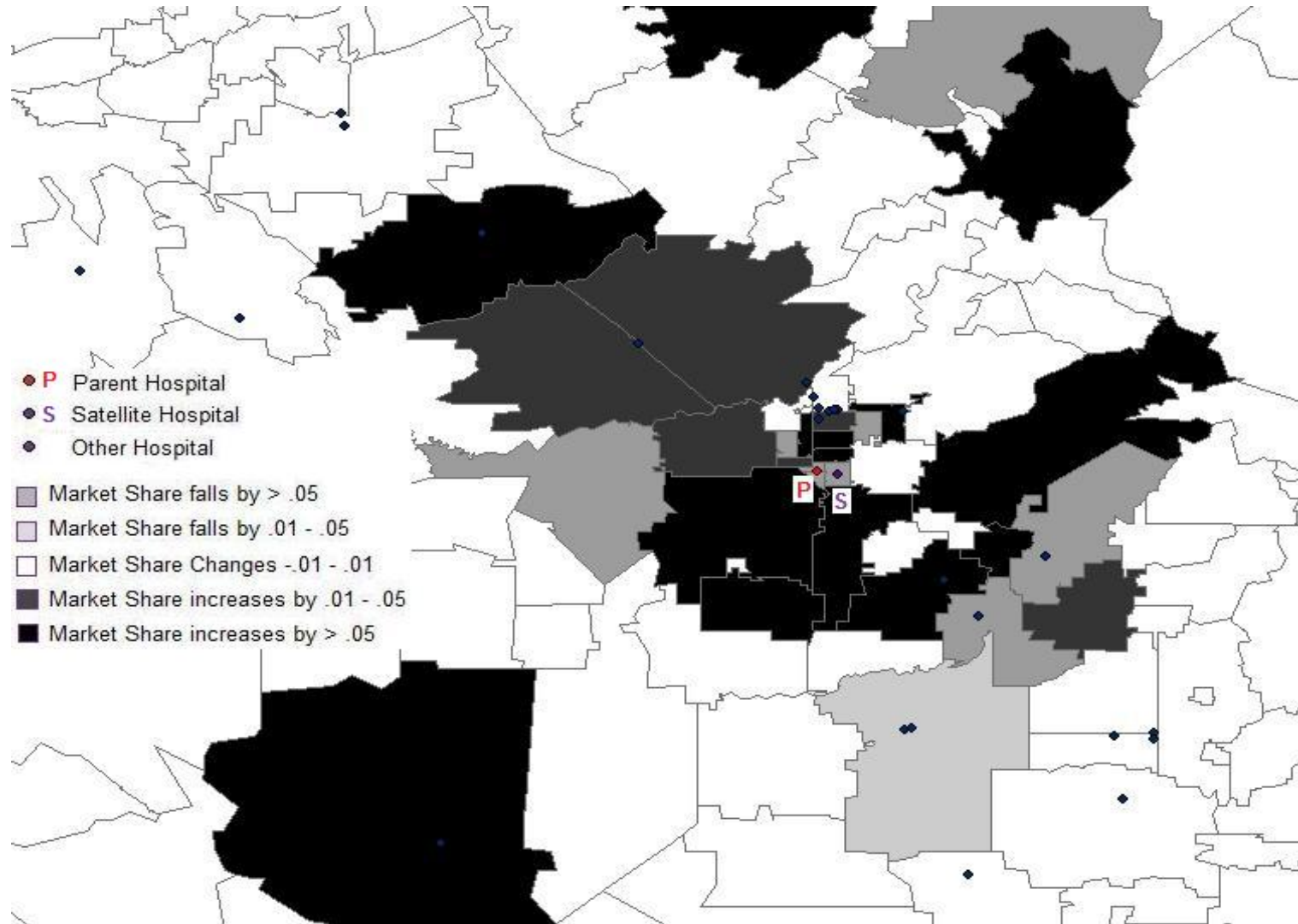
* Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Figure 2.2: Eden Medical Center/San Leandro Hospital One-Year Changes in Merged Market Shares



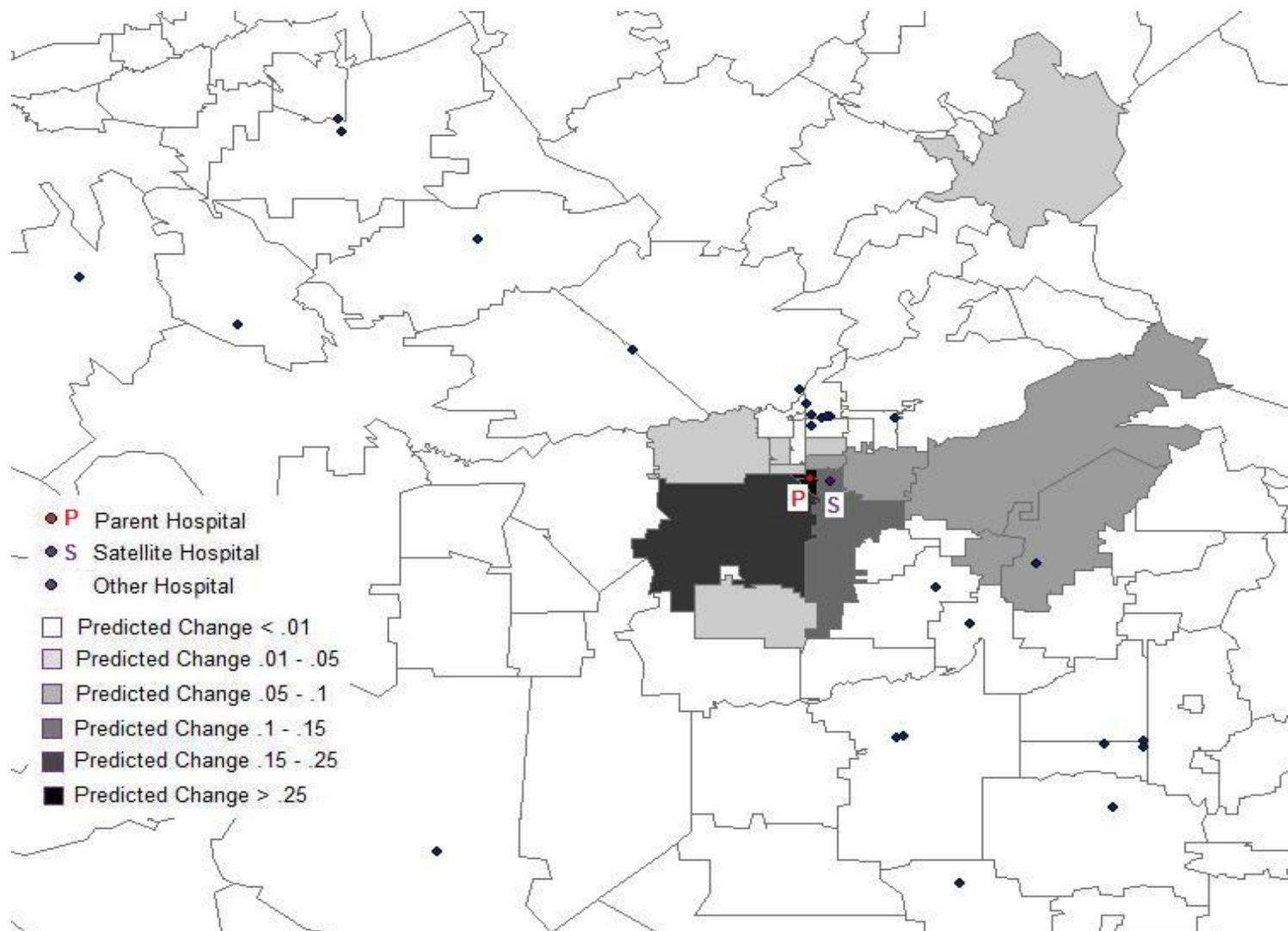
* Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Figure 2.3: Fresno Community Hospital/Valley Medical Center One-Year Changes in Merged Market Shares



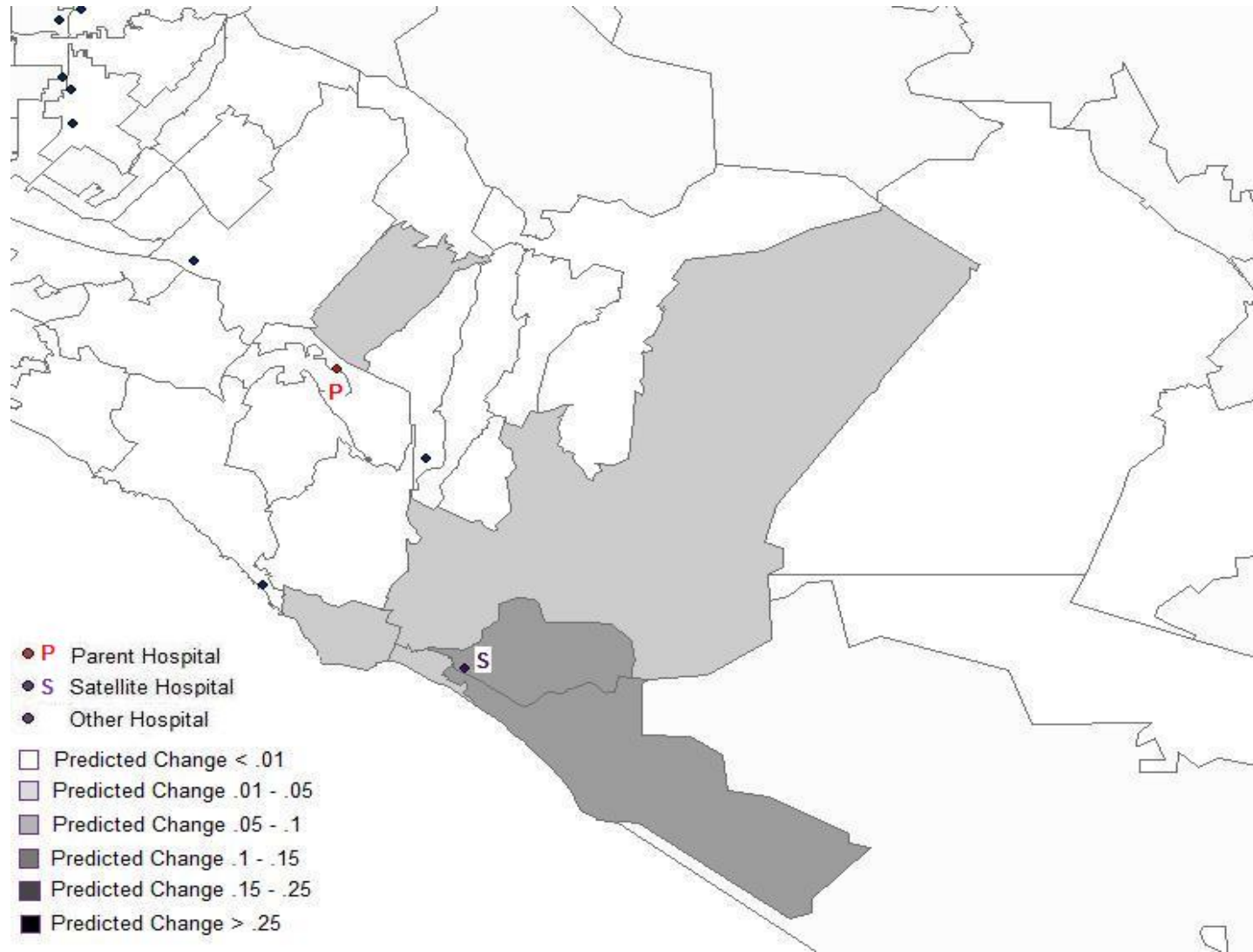
* Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Figure 2.4: Fresno Community Hospital/Valley Medical Center Predicted Change in HHI



* Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Figure 2.5: Saddleback Memorial Medical Center/San Clemente Hospital Predicted Change in HHI



* Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Chapter 3: The Impact of Managed Care on State Medicaid Programs: Evidence from State and Local-Level Mandates

Abstract: State governments began shifting large numbers of Medicaid enrollees from traditional fee-for-service into managed care during the 1990s, in part as a response to the rapid growth in Medicaid expenditures during this time period. In theory, managed care organizations may be able to reduce costs through shifting utilization towards lower cost environments, such as physician offices instead of emergency rooms, and through negotiating lower prices with providers. However, Medicaid reimbursement rates are already quite low, and splintering Medicaid enrollees into multiple insurance plans could reduce the efficiency of the program. Thus the impact on expenditures of shifting millions of Medicaid recipients into managed care programs is ultimately an empirical question. We use state-level aggregate administrative data for the years 1991-2003 in conjunction with an instrument created from a unique data set on mandatory Medicaid managed care enrollment policies to estimate the average impact of Medicaid managed care on a national level. Results suggest that this policy increased the expense of the Medicaid program, particularly for HMO-style insurance plans. We also extend our analysis to investigate the impact of these policies on enrollment decisions. Using CPS data, we find mixed responses to mandatory managed care policies, though all changes in take-up were small and did not appear to increase uninsurance rates.

3.1 Introduction

The Medicaid program provided health insurance to more than 57 million U.S. residents during the 2005 calendar year, with expenditures totaling \$312 billion (CMS 2009). This expenditure figure is nearly three times the program's expenditures in 1990 in real dollars. Part of this increase was driven by an increase in program enrollment, which doubled during this 15-year period. At the same time, many states expanded the scope of the Medicaid program to create a safety net for their most vulnerable residents (Coughlin, et al 1999).

The expanding role of Medicaid has contributed to a fifty percent increase in per enrollee expenditures in real dollars. While Medicaid is a state-run program, it is

jointly financed by state and federal governments. Thus, its expenditure expansion poses a financing issue for both levels of government. For example, Medicaid comprises over one fifth of state government spending (NASBO 2008)²⁰. And the share of federal outlays spent on the Medicaid program has increased from 3.3 percent in 1990 to 7.4 percent in 2005 (CBO 2009).

Because Medicaid is administered by states, state governments introduce the majority of program reforms and innovations. During the late 1980s and early 1990s, ballooning private health expenditures led to a closer examination of how traditional fee-for-service (FFS) insurance plans incentivize physicians and patients to prescribe and consume medical treatments without consideration for their cost. This, in turn, led to the development of managed care style insurance plans. Perhaps partly in response to the rapid increase in program expenditures, states followed this innovation and began shifting large numbers of Medicaid recipients from traditional fee-for-service into managed care during the 1990s. This led to a fivefold increase in the fraction of Medicaid recipients in managed care plans, from 11 percent in 1991 to 55 percent by 2000. Figure 3.1 displays the pattern of Medicaid managed care (MMC) penetration rates from 1990 to 2005. Penetration rates begin ramping up rapidly in 1993, and began to level off in 1999. It is interesting to note that the percent growth in total payments (in 2000 \$) fell during this period of rapid expansion in MMC penetration rates²¹.

²⁰ Medicaid expenditures tie with elementary and secondary education as the largest share of state expenditures (NASB 2008).

²¹ The subsequent rise in payments in 1998 is partly attributable to the creation of the SCHIP expansion program.

Despite the anecdotal evidence presented in Figure 3.1, it is unclear whether contracting out to private managed care insurers reduced program expenditures. On one hand, managed care firms have an incentive to keep patients healthy because their revenue does not depend on payments to providers. This incentive could influence managed care insurers to negotiate lower rates with providers and to keep enrollees healthy through increasing access to and utilization of preventive care. In fact, research shows that managed care firms successfully negotiated lower reimbursement rates in the private market (Cutler, McClellan, and Newhouse 2000; Dor, Grossman, and Koroukian 2004; Shen and Melnick 2006).

More specifically for Medicaid enrollees, managed care programs may also increase access to primary care through creating networks of providers that are willing to treat Medicaid patients. Increasing access to primary care is important because of the possibility that Medicaid enrollees may substitute expensive emergency room services for much less expensive care in a doctor's office. This substitution may stem from difficulty in finding a physician who accepts Medicaid insurance (Zuckerman, et al 1997). Lack of access to primary care may result in putting off care for a minor health issue until it becomes severe or inappropriate management of a chronic disease such as diabetes or asthma.

However, there are significant factors that challenge the goal of reducing program expenditures. First, one of the primary methods through which managed care firms have reduced health care expenditures is through negotiating lower rates with providers (Cutler, McClellan, and Newhouse 2000 and Dor, Grossman, and Koroukian 2004). But according to previous research, Medicaid reimbursement rates

to hospitals, physicians, and other health care providers are already very low to begin with (Gruber 2003). Thus, managed care firms may be unable to reduce costs in this fashion. The extent to which enrollment in a managed care plan is voluntary could also increase expenditures, particularly if healthier recipients are more likely to enroll in managed care and if premiums are set using average per person Medicaid expenditures as a benchmark.

One of the benefits of state administration of health insurance benefits is that administrative costs are spread among a large number of enrollees. Contracting out insuring services to several firms may reduce the efficiency of the program because these administrative costs are now split among smaller groups of individuals. In addition, the extent to which MMC programs are designed to maintain the expanded role of Medicaid described above will also counteract the likelihood of reducing Medicaid expenditures.

When one considers all of these facts, it is clear that both the sign and magnitude of the effect of MMC on Medicaid expenditures are theoretically ambiguous. Duggan (2004) investigated this topic in California and found that contracting out to managed care organizations increased Medicaid program expenditures²². While California is a large and diverse state, these results may not generalize nationally. For example, their Medicaid reimbursement rates are especially low, dampening the ability of managed care plans to reduce expenditures

²² Two other papers have analyzed a MMC demonstration program in Florida (Buchanan, Leibowitz, and Keesey 1996; Goldman, Leibowitz, and Buchanan). Both found lower expenditures for managed care enrollees as compared to those enrolled in traditional fee-for-service. However, as these study a single pilot program, they are likely to be less representative of a national effect.

through this channel (Duggan 2004). Thus, it would be useful to assess the effects of contracting out to managed care firms on a national level.

In this study, we use aggregate state-level administrative data along with information on MMC enrollment to estimate the causal effect of MMC on Medicaid expenditures. This data includes all expenditures for the Medicaid program, such as administrative expenditures and payments to medical providers and managed care insurers. Because managed care enrollment may be endogenous, we use a unique data set with information on mandatory MMC policies to instrument for managed care enrollment in determining its impact on expenditures. We find evidence suggesting that it is unlikely that contracting out to managed care firms has reduced program expenditures. Instead, it appears that the MMC program has actually increased the burden of Medicaid on state budgets. Contracts with health maintenance organizations (HMOs) as opposed to other types of managed care organizations seem to be driving this increase.

A second possible effect of MMC programs is that they may have reduced the desirability of Medicaid enrollment. Reduced Medicaid take-up could result in an increase in uninsurance among the state's most vulnerable population. To investigate this possibility, we use data from the March supplement of the CPS to study how the pattern of insurance enrollment responds to mandatory MMC policies. We find small increases in Medicaid enrollment among some populations, and small decreases in others, but conclude that enrollment is not particularly responsive to mandatory MMC policies.

The remainder of this paper is organized as follows: Section 3.2 provides background information on MMC and how states implemented it in addition to a literature review. Section 3.3 describes the data and estimation strategy for the impact of MMC on program expenditures. Section 3.4 discusses the first stage results, while section 3.5 discusses the expenditure results. Section 3.6 contains the analysis on enrollment, and section 3.7 concludes.

3.2 Medicaid Managed Care Background

3.2.1 How States Implemented Medicaid Managed Care

States adopted MMC policies slowly in the early 1990s, as evidenced by Table 3.1. In 1991, only 10.8 percent of Medicaid beneficiaries were enrolled in an MMC plan, a large fraction of whom were voluntarily enrolled. Enrollment in managed care accelerated beginning in 1994, when penetration increased to 24.6 percent. Penetration doubled again by 1998, increasing to 52.5 percent. Increases in managed care enrollment came from additional states adopting managed care programs and from within-state program expansions. For example, Tennessee, Rhode Island, and Hawaii began statewide managed care programs in the mid-1990s, while California rolled out its MMC program gradually across several years.

MMC programs vary in several ways. For example, they vary from least comprehensive, primary care case management plans (PCCMs), to most comprehensive, HMOs. PCCMs incorporate a combination of gatekeeping by a primary care provider (PCP) with Fee-For-Service (FFS) style payments, while HMOs fully manage the care of their enrollees and utilize capitated payments.

Capitated payments are set fees per enrollee that are fixed regardless of how much or how little care an enrollee utilizes. Because states still pay providers directly at FFS rates for PCCM enrollees, these plans are unlikely to affect service prices. However, plans that are paid capitated rates pay providers directly and thus have an incentive to lower prices. Another category of managed care plan is the prepaid health plan (PHP), which is like an HMO but may incorporate partially capitated instead of fully capitated payments.

Table 3.1 shows how enrollment in these three plan types varied over time. Enrollment in HMOs increased the most from 1993-1998, growing from 6.6 percent of Medicaid enrollees in 1993 to 35.9 percent in 1998, and creeping up for the remainder of the time period. Enrollment in PCCMs leveled off in 1997 at approximately 14 percent. PHP enrollment didn't pick up until the late 1990s, though by 2005, 25 percent of Medicaid beneficiaries were enrolled in a PHP²³.

Medicaid managed care policies also vary by whether managed care enrollment is mandatory. Mandatory enrollment is defined as whether an enrollee is automatically assigned to a managed care plan if she does not enroll voluntarily. In some states, mandatory enrollment was limited to those eligible through federal welfare programs. In others, mandatory enrollment includes most or all eligibility categories. Some managed care programs were voluntary for all beneficiaries. Table 3.2 shows how usage of voluntary versus mandatory policies has changed over time²⁴.

²³ It is worth noting that enrollment in these different plan types exceeded overall MMC enrollment because several HMOs and a number of PHPs are for specific, carved-out services such as mental health or substance abuse treatment.

²⁴ The data in this table is constructed using annual county-level population estimates in conjunction with county-level MMC policy data that was generously provided to us by the Urban Institute.

The data in this table represents the percentage of state population that was subject to the various MMC policies²⁵.

Early MMC programs often utilized voluntary enrollment policies, particularly favoring voluntary HMO enrollment. However, by 1996, mandatory enrollment policies were more prevalent. This preference for mandatory enrollment increased throughout the next five years. Like Table 3.1, Table 3.2 also shows that usage of both PCCMs and HMOs increased over time, though HMO usage was more widespread. In several counties, both PCCM and HMO policies existed and enrollment in one was mandatory. In these cases, an individual was automatically enrolled in one of the two plan types if she did not enroll voluntarily.

Some populations, such as those who qualify through the Supplemental Security Income (SSI) program, are less likely to be subject to mandatory enrollment policies. These individuals consume a disproportionate share of Medicaid expenditures: in 2005, disabled beneficiaries comprised 14.2 percent of Medicaid enrollment, and accounted for a much greater share of Medicaid expenditures at 43.4 percent (SSA 2008). While the potential for cost cutting may be greatest among these individuals, they are also the most vulnerable members of the Medicaid population. According to the 2000 CMS National Summary Report, 20 of 36 programs with mandatory HMO policies and 10 of 13 with mandatory PCCM policies included disabled individuals within the mandatory requirement²⁶. Unlike total Medicaid expenditures, however, disabled individuals comprise only 26.6 percent of capitated

²⁵ For the purposes of this table, a county is considered to have a mandatory MMC policy if the MMC plan is mandatory for federal welfare beneficiaries.

²⁶ The CMS National Summary Report data is current to June 30, 2000. Four states have both a PCCM program and an HMO program listed for overall care.

payments in 2005 while still accounting for 12.0 percent of enrollees with capitated claims (CMS 2009b).

States set per-enrollee MMC payments in a variety of ways. Payments to PCCM providers were fairly consistent. Providers are paid a monthly nominal amount for each enrollee in exchange for ensuring access to primary care services and for providing gatekeeping services. This payment generally ranges from \$3 to \$6. Payments to HMOs and other capitated plans vary across states. The Urban Institute (UI) conducted surveys in 1998 and 2001 to determine how these capitated rates were set. States set payment rates to HMOs using one of three broad methodologies: individual negotiation with plans, administrative rate setting, and competitive bidding. While both administrative rate setting and competitive bidding incorporated negotiation with plans, this was a secondary technique.

Each method has its strengths and weaknesses. Administrative rate setting is fairly simple from an administrative perspective, though it requires states to pick the 'correct' payment rate. In general, these rates are a discounted estimate of FFS expenditures. Competitive bidding has the attractive feature of allowing market competition to set capitated rates, but it is a complex process from an administrative perspective. One-third of the 14 states that utilized competitive bidding in 1998 switched to another method by 2001. Only one state started using this method between the two surveys. Some states also incorporated reinsurance rates into their contracts and assumed some of the risk for expensive enrollees. Overall, there was a great deal of variation in capitated payments across states, even outside of differences

in rate adjustment policies. This variation does not appear to be driven by geographic differences in health care costs (Holahan and Suzuki 2001, Holahan, et al 1999).

3.2.2 Literature Review/The Impact of MMC on Costs

The implementation of MMC policies has the potential to either decrease or increase Medicaid expenditures. One mechanism through which managed care programs are likely to decrease costs is through effectively managing care. Previous research suggests that MMC programs are often effective in shifting utilization from hospitals to doctors' offices, though some of this evidence is mixed²⁷. Managed care programs also have a track record of reducing unit costs for the same services as compared to traditional FFS insurance plans. For example, Cutler, McClellan, and Newhouse (2000) found that managed care insurance plans in Massachusetts succeeded in cutting costs in the private insurance market by up to 30-40 percent. Focusing on heart disease treatment allowed them to determine that the cost savings stemmed from negotiating lower rates with providers rather than a reduction in services. Dor, Grossman, and Koroukian (2004) find similar results for a broader geographic area.

While both of these studies use data from the early and mid 1990s, Shen and Melnick (2006) assess whether the impact of managed care organizations falls with the backlash against their strict structure in the following decade. They find that the impact on hospital operating expenditures and patient revenue grew in the late 1990s, and consequently fell to early-1990s levels in the early 2000s. Thus it is likely that managed care organizations continue to have a real downward pressure on the cost of

²⁷ See, for example, Garrett, Davidoff, and Yemane 2003, Garrett and Zuckerman 2005, Bindman, et al 2005, Basu, et al 2004, and Baker and Afendulis 2005.

medical care. However, there are several mechanisms through which MMC programs may actually increase costs without improving quality of care.

One concern only applies to voluntary HMO policies. There is evidence that voluntary HMO policies lend themselves to favorable selection by insurers so that healthier Medicaid enrollees are more likely to choose to enroll in an HMO (Glied, et al 1997 and Mitchell and Gaskin 2005). Thus capitated payments that are based on total FFS expenditures will overestimate average payments for voluntary enrollees. Another concern stems from the ability of HMOs to reduce the cost of care. Medicaid FFS rates are traditionally low already, making it difficult to negotiate a lower rate with health care providers (Garrett and Zuckerman 2005). In addition, even if HMOs are more efficient than Medicaid FFS, some states allow numerous plans to participate so long as they each accept the set premium. For example, nine plans signed up to participate in the Cleveland MMC market (Mayer 1997). There is a natural tension between competition and efficiency because plans that face more competition are more likely to increase plan quality to attract enrollees, but the addition of more insurance plans covering the same population means that administrative costs and risk pooling are spread among fewer individuals in each plan.

An additional issue is that Medicaid programs now have a vested interest in keeping state health care safety nets intact. In some cases, Medicaid payments have partially subsidized health care for the uninsured, and in others, Medicaid patients' use has allowed safety net providers to continue operating. Because states would like to maintain these safety nets, a variety of policies have been incorporated into MMC

programs to continue to protect these providers. These policies range from financial incentives for including safety net providers in MMC networks to encouraging these providers to develop Medicaid-only managed care plans. Each of these policies involves additional expenditures to maintain the safety net (Coughlin, et al 1999 and Maloy 2000). While this is a laudable goal, it potentially clashes with the goal of cutting Medicaid expenditures.

Overall, enrolling Medicaid recipients in managed care plans has the potential to both improve health outcomes and reduce costs by increasing access to regular primary care and shifting utilization from hospital emergency rooms to physician offices. However, as described above, there are a number of potential roadblocks that states need to overcome in order for these policies to succeed in reducing expenditures. The impact of MMC policies on Medicaid expenditures remains an empirical question.

Thus far, there has been one state-level study on the impact of MMC on Medicaid expenditures. Duggan (2004) investigated the impact of the gradual roll-out of a mandatory MMC program in California. This research studied the introductions of 19 separate county-level programs to identify changes in expenditures. These 19 programs followed one of three basic formats, which determined the number of plans operating in the county as well as the extent to which competitive bidding was necessary to operate. Ultimately, this study found that MMC increased Medicaid expenditures. Additional specifications found that counties with single, county-operated managed care plans have larger expenditures than those with multiple plans operating. This finding is consistent with the concerns

that Medicaid-only HMOs may be less efficient and that less competition for contracts results in larger premiums and thus higher expenditures.

While this analysis incorporated several different managed care programs in varied demographic settings, it may not be nationally representative. As stated in its conclusion, California Medicaid reimbursement rates are lower than average, giving MMC plans less ability to cut costs. In addition, each of these programs follows an HMO-style managed care plan. Several states utilize PCCM plans as well. These may have a different effect on expenditures than HMOs, especially since they have the potential to reign in excess utilization but only directly involve nominal additional payments. However, PCCMs may also involve higher expenditures if the incentive to limit excess utilization is insufficient, resulting in additional utilization when combined with an increased access to primary care. For these reasons, a national analysis is necessary to determine the effect of MMC on Medicaid expenditures.

3.3 Data and Estimation Strategy

3.3.1 Medicaid Expenditure and Managed Care Enrollment Data

The data for this analysis comes from several sources. Expenditure data comes from the Center for Medicare and Medicaid Services (CMS) Financial Management Reports, which are compiled from quarterly reports submitted by each state. These reports include spending in various categories, such as administrative services, inpatient hospital care, pharmaceuticals, and outpatient care. Beginning in 1998, Medicaid expenditures for the SCHIP program were reported as well.

Managed care expenditures for programs besides targeted case management were also reported separately beginning in 1998.

Expenditures in several categories as well as total net expenditures for each major category are reported in Table 3.3. One of the more striking rows is prescription drugs, for which expenditures more than quadrupled from 1990-2005. Other major expenditure categories, including inpatient and outpatient expenditures, grow a great deal during this time period as well. Likewise, real managed care expenditures increased by nearly 70 percent from 2000 to 2005. This increase transpired after the majority of MMC policy expansion, so it was likely driven by an increase in per-enrollee payments rather than increases in enrollments.

Medicaid managed care enrollment data comes from the Medicaid Managed Care Enrollment Reports, which are available for the years 1991-2006²⁸. These reports list the number of individuals in each MMC plan across the country as of June 30 of each year as well as the type of plan, such as HMO or PCCM. In order to calculate an accurate measure of MMC enrollment as a percentage of total enrollment, overall Medicaid enrollment as of June 30 of each year is necessary²⁹. This figure is included in the Medicaid Managed Care Enrollment Reports beginning in 1996. For 1991 through 1995, Medicaid enrollment is available by fiscal year in the Annual Statistical Supplement to the Social Security Bulletin. Enrollment as of June 30 is estimated for these years by deflating the fiscal year enrollment numbers by the ratio of fiscal year enrollment to June 30 enrollment in 1996.

²⁸ In our analysis below, we only use data through 2003 because of data limitations with our instrument.

²⁹ The fiscal year enrollment figure includes individuals who dropped out of the program by the end of the fiscal year, and is thus necessarily larger than a point-in-time figure.

Enrollment patterns in MMC vary widely across states. Some states, such as South Carolina, have very low managed care penetration rates throughout the time period (see Figure 3.2). Other states follow a pattern similar to that of New Jersey. This second group of states experienced a sharp increase in managed care penetration in the mid-1990s, followed by a plateau in enrollment for the remainder of the time period. And some states, such as Pennsylvania, encouraged managed care policies more gradually, beginning with a few counties and slowly expanding across the state. This last group of states experienced a gradual increase in managed care penetration rates throughout the decade.

3.3.2 Identification Strategy

As discussed previously, enrollment in voluntary managed care plans has the potential to suffer from selection bias. Healthier Medicaid enrollees are more likely to choose to enroll in MMC plans voluntarily. If these enrollees are easier and cheaper for managed care plans to insure, then OLS estimates that use average Medicaid expenditures per recipient and managed care penetration rates could be biased downward. To address this possible endogeneity, we construct an instrument from mandatory MMC policies. UI conducted surveys in 1998 and 2001, in which they compiled county-level MMC policies for the years 1990 through 2001. The results of these surveys allowed UI to identify if and what type of managed care policy was in effect for each county in each year³⁰.

³⁰ The survey was constructed to identify managed care policies for the federal welfare population. Policies for SSI enrollees may differ depending on the state.

The raw data identified whether a PCCM and/or HMO plan was available for Medicaid enrollees³¹. Availability of each of these plans was broken down into whether enrollment was voluntary or mandatory. From this raw data, UI constructed variables that characterized the nature of the MMC environment for the county-year observation. A county could be characterized as having one of the following policies: no MMC, voluntary MMC only, mandatory PCCM, mandatory HMO, and mixed mandatory. The last category, mixed mandatory, represents a county in which both HMO and PCCM plans are available and enrollment in one of the plans is mandatory.

Because county-level Medicaid enrollment is not readily available, our instrument for mandatory enrollment in a managed care plan is constructed using annual county population totals. The UI county characterizations allow us to construct a dummy variable for whether Medicaid enrollees in a county are subject to a mandatory managed care policy. We use this dummy variable and estimates of the number of poor people living in a county to construct the percent of a state's Medicaid population that would be subject to a mandatory managed care policy³².

This instrument is similar to the one used by Currie and Fahr (2005). They used the percentage of children in a state that would be subject to a mandatory managed care policy as an instrument for the percentage of Medicaid children enrolled in a managed care plan to estimate the impact of managed care enrollment on

³¹ PHP plans are included in the survey data and are classified as PCCM or HMO according to how individual states reported them in the survey data.

³² This assumes that the distribution of Medicaid enrollees is similar to the distribution of individuals living in poverty. Our first stage will be weakened to the extent that this is not true. Replacing the number of poor people with the number of poor children aged 0-17, however, yields similar results to the ones that follow.

utilization and program participation of children³³. Table 3.4 shows the distribution of the population living in poverty subject to mandatory MMC policies by state for selected years. Many states rolled out mandatory policies rapidly statewide. Other states rolled these policies out more gradually across the time period. A handful of states subject few, if any, of their population to mandatory policies. This table, and the analysis that follows below, carries the 2001 policy data through 2003, thus assuming that there are no new mandates in these years. There is very little change in county-level policies beyond 1999, suggesting that the 2001 policies are a reasonable approximation for the two subsequent years. We extend our study through 2003 instead of limiting it to 2001 in order to include additional post-policy years in our analysis.

In addition to variation in policy timing, there is also variation in type of mandatory policies. Table 3.5 shows the distribution of mandatory managed care across the three categories of mandatory policies: HMO only, mixed mandatory, and PCCM only. Utilization of each increases slowly in the first half of the 1990s. Mandatory PCCM shares drifted up slightly for the remainder of the analysis period. Mixed mandatory policy shares increased for a few years, and then level off, while mandatory HMO policy shares increased even further. Particularly as time passes, mandatory HMO policies are much more prevalent than the other two policy types.

One possible concern with this instrument is with the potential for legislative endogeneity. For example, IV estimates would be biased if states and counties adopted mandatory managed care policies in response to an expected change in

³³ Currie and Fahr use the National Summary of State Medicaid Managed Care Program reports to identify counties with mandatory programs. Percent mandatory descriptive statistics are similar for the years that overlap between the two studies.

expenditures or change in enrollee characteristics. However, managed care programs required approval from CMS to be implemented, and it is likely that this process was unpredictable in its timing. Thus it is unlikely that the timing of implementation was related to changes in Medicaid enrollment or expenditures.

As described above, our main first stage and structural equations can be written in the following format:

$$PercentMC_{st} = \alpha_1 + \gamma_1 PercentMand_{st} + \beta_1 X_{st} + \phi_{1s} + \theta_{1t} + \varepsilon_{1st} \quad (1)$$

$$\ln(ExpPerEnroll_{st}) = \alpha_2 + \gamma_2 PercentMC_{st} + \beta_2 X_{st} + \phi_{2s} + \theta_{2t} + \varepsilon_{2st} \quad (2)$$

with s and t indexing states and years, respectively. $PercentMC_{st}$ represents the percentage of state s 's Medicaid beneficiaries that was enrolled in a managed care program in year t , and $PercentMand_{st}$ represents the percentage of state s 's poor population that lived in a county with a mandatory managed care enrollment policy in year t . $ExpPerEnroll_{st}$ represents the per enrollee expenditures for the state-year observation, so the dependent variable for the reduced form equation is the log of per enrollee expenditures.

Since the two primary forms of managed care, HMOs and PCCMs, are vastly different from each other in both practice and payments, it is possible that they have different effects on expenditures. To explore this possibility, we divide managed care percentage into percent of Medicaid recipients enrolled in HMO and PCCM plans separately. Likewise, we separate out the percentage of the population subject to mandatory MMC policies into the three mandatory policies described above. These changes lead to the following set of estimation equations:

$$PercentHMO_{st} = \alpha_1 + \gamma_1 PctMandHMO_{st} + \varphi_1 PctMixedMand_{st} + \delta_1 PctMandPCCM_{st} + \beta_1 X_{st} + \phi_{1s} + \theta_{1t} + \varepsilon_{1st}$$

(3)

$$PercentPCCM_{st} = \alpha_2 + \gamma_2 PctMandHMO_{st} + \varphi_2 PctMixedMand_{st} + \delta_2 PctMandPCCM_{st} + \beta_2 X_{st} + \phi_{2s} + \theta_{2t} + \varepsilon_{2st}$$

(4)

$$\ln(ExpPerEnrdl_{st}) = \alpha_3 + \rho_3 PercentHMO_{st} + \lambda_3 PercentPCCM_{st} + \beta_3 X_{st} + \phi_{3s} + \theta_{3t} + \varepsilon_{3st} \quad (5)$$

It should be noted that PercentHMO and PercentPCCM do not cover all managed care enrollees. PHPs, the other most utilized form of MMC, are not included because many of these plans are for targeted types of care. Including PHP enrollment would vastly increase the likelihood of double-counting enrollees.

3.4 First-Stage Results for MMC Enrollment

The first stage results are reported in Table 3.6. The first triple of columns comes from equation (1) above. The second and third triples come from equations (4) and (5) above. Within each triple, the first column includes state and year fixed effects. The second column adds control variables, and the third column adds state-specific trends. The control variables include the percentage of Medicaid recipients enrolled in AFDC, the percentage of Medicaid recipients receiving Social Supplementary Income (SSI) benefits, the unemployment rate in June of each year, and the year's average poverty rate. The first two control variables are constructed using enrollment figures for AFDC and SSI programs from the Social Security

Administration's Supplemental Statistical Bulletin, assuming that all of these individuals are enrolled in Medicaid as well.

The results listed suggest a very strong relationship between mandatory MMC policies and actual managed care enrollment. The coefficient in column 3 suggests that a 10 percentage point increase in the population subject to mandatory MMC leads to a 4 percentage point increase in enrollment in managed care. The magnitude of the first stage is very similar to the first stage relationship found in Duggan (2004). It is unsurprising that this relationship is well below one-to-one, especially because some individuals enrolled in managed care voluntarily before participation became mandatory. In addition, more vulnerable Medicaid recipients, such as those who are blind or disabled, are not always subject to mandatory MMC policies. There also may have been a lag in implementation for new enrollees.

Columns 6 and 9 similarly suggest strong relationships between the various mandatory MMC policies and enrollment in HMOs and PCCMs. In particular, differences in both magnitude and statistical significance indicate that mixed mandatory policies are more strongly predictive of PCCM enrollment than of HMO enrollment. This suggests that enrollees in mixed mandatory counties are much more likely to enroll in PCCM plans, whether by choice or automatic enrollment. Mandatory HMO policies and mandatory PCCM policies are solid predictors of HMO enrollment and PCCM enrollment, respectively.

3.5 Results for Medicaid Expenditures

According to tables available in the Medicaid Statistical Information System Data Mart (CMS 2009b), capitated payments account for only 16.7 percent of total Medicaid expenditures in 2005. As discussed above, beneficiaries that are disabled or otherwise likely to consume intensive quantities of medical services are less likely to enroll in managed care plans. This contributes to the relatively small percentage comprised by capitated payments. Recall, for example, that the 14.2 percent of enrollees who were disabled in 2005 accounted for nearly one-half of that year's expenditures. And while PCCM plans have the potential to affect FFS-reimbursed utilization, a smaller share of the population is enrolled in these plans. As such, contracting out to managed care organizations would need to have a large effect on expenditures in order to be detectable in the aggregated totals.

The first panel of Table 3.7 shows the IV results for both specifications of the expenditure regressions. Like Table 3.6, each triple of columns represents regressions including state and year fixed effects, followed by adding controls, and finishing with adding state-specific trends. The coefficient of 0.124 in the first column suggests that for every 10 percentage point increase in MMC enrollment, expenditures increase by 1.24 percent. When controls are added, the magnitude falls to 1.12 percent, though the statistical precision increases. Column 3 shows that when state-specific trends are included, the magnitude falls even further, to 0.76 percent, and statistical precision falls as well. Combined, these results suggest that the increase in expenditures may be driven by trends instead of managed care enrollment.

The last three columns represent specifications from equations (3) through (5). The coefficients in column 4 suggest that an increase in HMO enrollment of 10 percentage points induces a 2.43 percent increase in Medicaid expenditures, while PCCM enrollment does not appear to have an effect. Adding controls reduces the coefficient on the HMO percentage variable, though increases the magnitude and statistical significance of the coefficient on the PCCM percentage variable. However, including state-specific trends shows that the increases from PCCM enrollment were driven by trends in expenditures. This is not surprising given that there is little incentive for PCCM plans to reduce utilization. The effect of HMO enrollment is robust to the inclusion of state trends, which is consistent with the findings in Duggan (2004). The coefficient of 0.169 in column 6 translates to a 1.69 percent increase in Medicaid expenditures for every 10 percentage point increase in HMO enrollment.

In 2000, nominal Medicaid expenditures per enrollee totaled \$6383. Thus, a 10 percentage point increase in HMO enrollment would correspond to a \$107 increase in expenditures on average. Referring back to Table 3.1, HMO enrollment increased from 25.0 percent in 1997 to 35.9 percent in 1998. Per enrollee expenditures increased from \$5385 in 1997 to \$5913 in 1998. Our regression results suggest that approximately one-fifth of this increase could have been driven by the increase in HMO enrollment. Considering the low percentage of expenditures that cover capitated payments, the magnitude of the increase in expenditures induced by HMO enrollment may be much larger³⁴.

³⁴ For example, if per capita expenditures are equal to \$5385, HMO penetration is 25 percent, and capitated payments are 15 percent of overall enrollment, per capita capitated payments are approximately equal to $5385 \times .15 / .25 = 3231$ for HMO enrollees. Similarly, \$5913 translates to \$3548

In the other two panels of Table 3.7, we investigate the impact of MMC enrollment on administrative expenditures and on Medicaid enrollment. Given that contracting out to managed care firms requires state Medicaid programs to coordinate with a number of actors and that setting premium rates is documented as challenging, it is plausible that this procedure would increase administrative costs. We use a similar set of estimating equations as above, replacing the log of per enrollee Medicaid expenditures with the log of per enrollee administrative expenditures. The second panel of Table 3.7 presents these results. The magnitude of the coefficients on the various managed care percentage variables is somewhat similar to the coefficients for log of total Medicaid expenditures, though the estimates are not nearly as precise.

These results suggest that MMC programs may increase administrative expenses in addition to overall expenditures. This implication would be consistent with the previously documented challenge in setting up the HMO contracts. Setting up competitive bidding for managed care contracts was particularly difficult as evidenced by the number of states that substituted another method for setting premiums between 1998 and 2001. However, these estimates are very imprecise, and thus we cannot draw any substantive conclusions.

In the next set of specifications, the left hand side of the structural equation is replaced with June 30 Medicaid enrollment as a percentage of state population. These specifications investigate the effect of managed care enrollment on Medicaid enrollment to determine whether MMC alters the attractiveness of the Medicaid program to enrollees. Enrollment falling could suggest that a vulnerable population is

in capitated payments per HMO enrollee. Thus, per HMO enrollee capitated payments increase by \$317 and HMO enrollment drove 1/3 of this increase.

made more vulnerable by the introduction of MMC policies. Turning to the third panel of Table 3.7, it appears that MMC policies may, in fact, have reduced take-up of the Medicaid benefit among eligible individuals.

The coefficient of -0.017 in column 3 suggests that a 10 percentage point increase in managed care enrollment would induce a 0.17 percentage point drop in Medicaid enrollment as a share of state population. Likewise, the coefficient of -0.029 in column 6 would induce a 0.29 percentage point drop for the same magnitude increase in HMO enrollment. Considering again the increase in managed care and HMO enrollment from 1997 to 1998, these coefficients imply a reduction of approximately 0.02 percentage points from the 11.5 share of the population enrolled in Medicaid in 1997. In 1998, enrollment drops to 11.1 percent of the population. The increase in managed care enrollment could account for approximately 4.6 percent of this decrease in enrollment.

If MMC programs reduce enrollment, then this effect would dampen the increase in Medicaid expenditures slightly. However, there might be a concern for who is reducing their take-up of Medicaid benefits and whether they substitute into private insurance or uninsurance. This aggregate-level data does not enable us to determine who is altering their take-up decision. Nor does it enable us to identify the category individuals are switching into. We explore these questions further in the next section.

3.6 Enrollment Responses to MMC

To further explore the impact of mandatory MMC policies on enrollment, we turn to individual-level data. This type of data, unlike the administrative data used above, enables us to control for other components of a person's life that may impact their likelihood of Medicaid enrollment. For example, children are more likely to be enrolled in Medicaid than adults, as are children of poor or single-mother households compared to children of wealthier or two-parent households. The data set that has primarily been used for examining the likelihood of Medicaid enrollment in addition to investigating health outcomes and utilization is the National Health Interview Survey (NHIS). The drawback with this survey, however, is that state identifiers are not included in the publicly accessible version of the data set beyond 1994.

In order to study a similar time period as the one we analyzed above, we take advantage of the CPS. Compared to the NHIS, the main drawback of the CPS is that it does not include information on health status or healthcare utilization. However, it is a large individual-level data set that includes state identifiers throughout the data set, enabling us to extend our analysis through 2003. The CPS also lists MSA codes for many observations as well as FIPS county codes beginning in 1996. While we will be unable to investigate the health and utilization effects of MMC programs, we can more thoroughly investigate their effect on Medicaid enrollment. Several papers have investigated the effect of MMC on health and utilization, while few have investigated the response of enrollment to MMC mandates.

Medicaid managed care mandates may have an effect on enrollment if the availability of managed care makes Medicaid more or less attractive to those who are

eligible for the program. Given that managed care insurance plans are more restrictive than traditional FFS plans, managed care mandates may reduce the attractiveness of the Medicaid program. However, if managed care plans are successful in increasing the accessibility of primary and preventive care for enrollees, then managed care may actually increase the attractiveness of the Medicaid program.

Few papers have studied the effect of MMC policies on Medicaid enrollment decisions. One analysis, performed by Garrett, Davidoff, and Yemane (2003), used NHIS data to investigate whether women enrolled in welfare programs or children changed their Medicaid take-up decisions in response to mandatory MMC programs of various types. They found that women did not alter their enrollment likelihood, nor did they find an effect on children's enrollment overall. The one exception was Black children, who were less likely to enroll in response to mandatory PCCM policies.

Currie and Fahr (2005) also used NHIS data to analyze the effect of MMC policies on enrollment for a similar time period (1991-1994), though they focused on the effect of MMC penetration rates. This variable was instrumented for similarly to our expenditure analysis above. Like Garrett, Davidoff, and Yemane, Currie and Fahr found MMC penetration rates to have no effect on the Medicaid enrollment rates of children overall. They did, however, find that a 20 percentage point increase in MMC enrollment induced a 2.5 percentage point decline in Medicaid enrollment among Black children and a 1.7 percentage point decline in Medicaid enrollment among toddlers. Medicaid enrollment for poor school-age children, however, increased by 3 percentage points. Put together, these analyses suggest that some

children may have been differentially affected by MMC policies in the early 1990s, but the effects are not large. In addition, both of these studies only looked at the likelihood of enrollment in Medicaid, without differentiating between other insurance and uninsurance as alternatives. There is a substantial difference between turning down Medicaid in favor of other insurance as opposed to becoming uninsured.

To investigate this question, we use a similar sample of individuals as used by Currie and Fahr in their 2005 paper. We restrict our sample to children aged 0-14 in families earning up to 300 percent of poverty level income because these children are more likely to be eligible for Medicaid benefits. This leaves us with a sample of 323,586 children, or an average of 23,000 children per year for the years 1990-2003. Table 3.8 presents descriptive characteristics of the children in our sample. Average family size fell very slightly across the study period, as did average number of siblings in both the under-6 and under-18 categories. Similarly, the average age increased slightly and the percent of children under age 6 decreased, suggesting that fertility rates dropped slightly in this demographic. The percentage of children living in female-headed households increased slightly across the analysis period as well.

Turning to statistics on program participation, the percentage of children in families receiving welfare benefits fell a great deal between 1995 and 2003. This is consistent with welfare reform efforts that were enacted in this time period. The percentage of children in families receiving SSI increased from 1990 to 1995, but then fell gradually for the remainder of the study period. Medicaid enrollment increased a great deal in the early 1990s, then fell slightly and stabilized for the

remainder of the study period³⁵. This trend is consistent with initial increases in welfare enrollment, followed by implementation of the SCHIP program which expanded the group of children eligible for Medicaid in many states. Uninsured rates initially fell slightly, then rose again in the last few years of the study period.

We use sets of linear probability models to assess how substitution of Medicaid enrollment for other insurance or uninsurance is affected by mandatory MMC policies. The instruments described above are now utilized as explanatory variables. The mixed mandatory and mandatory PCCM variables are combined into a single mandatory PCCM variable to simplify the analysis. Recall that mixed mandatory policies were much more closely connected to PCCM enrollment than to HMO enrollment. This suggests that mixed mandatory policies can be viewed as analogous to mandatory PCCM policies, enabling their consolidation. This structure enables us to estimate how take-up of Medicaid benefits was affected by mandatory MMC policies³⁶. A variety of demographic controls are included in these specifications, including gender, race, living in a female-headed household, and categories for a variety of income levels with respect to the poverty line. Program participation variables for welfare and SSI are included, as are state unemployment rates and state poverty rates. We also include fixed effects for states and years as well as interactions between year fixed effects and dummy variables for poverty categories. State-specific trend variables are included in all specifications.

³⁵ For the years 1990-1994 and 2001-2003, there are inconsistencies between the Medicaid enrollment variable and the child Medicaid enrollment recode variable. The latter variable is more consistent with Medicaid enrollment for the years 1995-2000 when the two variables are internally consistent, so the child Medicaid recode variable is used throughout to determine enrollment in Medicaid.

³⁶ This structure is more similar to the Garrett, Davidoff, and Yemane analysis than the Currie and Fahr analysis in that we study the effect of the policy in place rather than the effect of MMC penetration.

The policy variables are constructed slightly differently in this section. Three sets of policy variables are utilized. One set is the same as those used above with the mixed mandatory and PCCM variables combined as described above. These represent the percent of poor people subject to a particular Medicaid policy environment within a state. The second set is constructed similarly to the state-level variables, but at the MSA-level instead. The third set is the county-level variables constructed by UI. These variables equal zero or one and can be interpreted as zero percent or 100 percent of poor people in the county being subject to a particular policy environment. Observations from the CPS are linked to the most local level policy data possible. When county is reported, then county-level data is connected to it. If not and MSA is reported, then the MSA-level data is connected to it. If neither county nor MSA is reported, then state-level data is used. This strategy enables observations to be connected with the most specific information available.

Table 3.9 reports results for these regressions. The first panel reports specifications including the explanatory variables without any interaction terms, while the second and third panels include interactions between the explanatory variables and race dummies or poverty categories, respectively. The first panel reveals that control variables affect Medicaid insurance rates as expected. For example, children of single moms are more likely to be insured by Medicaid and less likely to have private insurance or be uninsured. Similarly, children in families receiving SSI or welfare benefits are much more likely to receive Medicaid. Due to space constraints, poverty category coefficients are not reported, but, as expected, Medicaid likelihood decreases with additional income.

The statistically insignificant coefficient of -0.005 on PercentMand in column one in the first panel indicates that the percentage of poor individuals subject to a particular policy environment does not impact Medicaid enrollment. In column 4, the coefficient of -0.009 on PctMandHMO is statistically significant, suggesting a very small decline in take-up in response to mandatory HMO enrollment. For example, living in a county with a mandatory HMO policy reduces the likelihood of Medicaid take-up by 0.9 percentage points. Living in an MSA in which 50 percent of the poor population lives in a mandatory HMO county reduces take-up by only 0.45 percentage points. Columns 5 and 6 suggest that those who leave Medicaid have alternate insurance options and substitute into another insurance program instead of being uninsured. Mandatory PCCM policies do not appear to affect Medicaid take-up.

The second panel presents results including separate effects for Black and Hispanic children. Coefficients on control variables are not reported but are similar in magnitude and significance to the first panel. The first specification suggests that expansions in mandatory MMC policies induced greater take-up of Medicaid benefits among Black children, increasing the percentage of Black children that were insured at all. Hispanic children respond in the opposite direction. The magnitude of these effects is not much larger than those reported in the first panel and represents 1.2 percentage point increases and 1.5 percentage point falls in Medicaid take-up, respectively. Columns 4-6 report similar responses to mandatory HMO policies as the first panel, and minority children do not respond differentially. Black and Hispanic children do, however, differentially respond to mandatory PCCM policies.

The magnitude of these responses remains small; take-up among Black children is increased by 1.9 percentage points in a mandatory PCCM county and take-up among Hispanic children fell by 2.2 percentage points. The substitution effect of the fall in Medicaid among Hispanic children is split between other insurance and uninsurance.

The third panel of Table 3.9 investigates how individuals in varying income categories are differentially affected by mandatory MMC policies. While those living in families earning under 300 percent of the poverty line are certainly more likely to be eligible, and therefore enrolled, in Medicaid than those living in families earning more than that cut-off, there may also be variation within the under-300 percent group. For example, those in the highest income category are likely to have greater access to private insurance than those in the lowest income category. For the poverty interaction terms, we simplified the analysis by creating three categories out of the original nine: less than 100 percent of the poverty line, 100 to 200 percent of the poverty line, and 200 to 300 percent of the poverty line. The first of the three, less than 100 percent of the poverty line, is the omitted category.

The results reported in column 1 suggest that children in families earning less than 200 percent of poverty-level income may be slightly less likely to be enrolled in Medicaid, while those in families earning 200 to 300 percent of poverty are slightly more likely to be enrolled in Medicaid. When HMO and PCCM policies are included separately, the results suggest that children in families earning less than 200 percent of the poverty line substitute from Medicaid into other insurance. As before, the magnitude of the response is small: the likelihood of take-up falls by 1.7 percentage points in a mandatory HMO county. Those in the third income category do not alter

their take-up in response to mandatory HMO policies: the coefficient of 0.016 on their interaction term nearly cancels out the coefficient of -0.017 on the non-interacted PctMandHMO variable. This group is, however, slightly more likely to take-up Medicaid benefits in response to mandatory PCCM policies.

Overall, these results suggest very small-magnitude enrollment responses to mandatory MMC policies. In general, Medicaid take-up falls in response to mandatory HMO policies, particularly among the poorest two income categories. The response to mandatory PCCM policies is mixed. Black children and those in the highest income category increase Medicaid take-up, while Hispanic children reduce their take-up. Considering the magnitude of our results, they are not vastly different from those found in previous literature. The main difference is the direction of the impact on Black children. This difference could stem from the fact that our analysis covers a much longer time period, including nearly all of the mandatory MMC policy expansions. It is interesting to note that most of the reduced Medicaid take-up substitutes into other insurance, suggesting that mandatory enrollment policies do not result in increases in uninsurance among poor children. It is also interesting to note that increases in Medicaid take-up often come at the expense of the uninsured category, suggesting that mandatory PCCM policies may slightly reduce the rate of uninsurance among poor children.

3.7 Conclusion

On net, it does not appear that Medicaid managed care programs benefit state budgets. To the contrary, if anything, it appears that managed care programs are more expensive per enrollee than traditional FFS Medicaid programs. This is particularly true for HMO-style managed care, while PCCM programs do not appear to affect per enrollee expenditures. Our analysis exploits the variation in managed care enrollment within and across states. It includes state and year fixed effects in addition to state-specific trend variables in order to address concerns of legislative endogeneity. For example, if states enact or expand a Medicaid managed care policy in response to growing program expenditures, the inclusion of state trend variables should effectively control for this possibility.

There are several possible reasons why Medicaid HMO plans may be more expensive than FFS plans. First, many states contract out to several HMO insurers simultaneously, which potentially reduces the efficiency of the Medicaid program, particularly compared to situations where all Medicaid beneficiaries are under the same administrative umbrella. This stems from the fixed costs of running an insurance plan spreading among smaller groups of individuals. In addition, there is an added administrative cost within the Medicaid program to setting up and maintaining the managed care contracts. It has also been well documented that states tried to maintain safety net care for the uninsured when designing managed care programs. This practice has tended to favor less efficient Medicaid-only plans and plans that are willing to include safety net providers in their networks.

It is also worth noting that many individuals are on Medicaid for a relatively short period of time. Thus even if the benefits of managed care in the form of accessibility and utilization of primary and preventive care are large enough to offset the increase in expenditures, it may be unlikely that the Medicaid program would reap the benefits of these improvements. For example, if a family is enrolled in Medicaid for six months, that family may take advantage of access to primary care to schedule physicals and treat smaller health problems before they become emergent. However, the family may no longer be enrolled in Medicaid by the time the family's improved health would benefit the program financially. Thus it could be true that the larger expenditures reflect greater usage of primary care for individuals enrolled in the program for a short duration.

Several papers have been written on the impact of MMC on utilization and health outcomes. Many studies find little relationship, and those that find a relationship have mixed findings. For example, Duggan (2004) finds that Medicaid managed care programs in California have no effect on infant health, while Aizer, Currie, and Moretti (2007) find MMC in California to be associated with reduced usage of prenatal care and worse birth outcomes. However, Howell et al (2004) find a positive association between MMC and prenatal care in Ohio, though they find no effect of MMC on birth outcomes. Kaestner, Dubay, and Kenney (2002) perform a national analysis, and find no association between MMC and infant health outcomes, nor do they find a significant effect on utilization of prenatal care.

There is mixed evidence within the literature that focuses solely on the impact on primary care utilization as well. For example, Garrett and Zuckerman (2005) find

no impact on primary care utilization for adults and a reduction for children, while Garrett, Davidoff, and Yemane (2003) find some reduction in utilization of primary care for women and increases in primary care for children. Currie and Fahr (2005) find mixed results for primary care usage among children, and Long and Coughlin (2001) find no difference in reported access, utilization, or satisfaction with care between FFS and MMC enrollees in rural Minnesota. Baker and Afendulis (2005) find increases in outpatient care utilization and reductions in emergency room usage, though also find increases in reports of putting off care. Overall, it does not appear that increases in Medicaid expenditures are driven by improvements in primary care utilization that would translate into health and financial benefits that aren't realized until after exiting the Medicaid program. Nor do these increases appear to be justified by improvements in health outcomes.

Our enrollment results suggest that mandatory HMO policies may have reduced expenditures slightly through reducing the number of people enrolled in Medicaid. If those who exit the program are healthier, part of the increase in per enrollee expenditures could be driven by this reduction in take-up. However, given that the size of the effect on enrollment is very small, the size of this potential bias is small as well. The increase in Medicaid enrollment driven by mandatory PCCM policies could, likewise, increase the size of the Medicaid budget through increasing the size of the program. It is important to recall, however, that the size of these enrollment responses is small, and thus is likely to have only a slight effect on program expenditures.

Table 3.1: Enrollment in Medicaid Managed Care 1991-2005

Year	Percent of Medicaid Enrollees in:			
	Percent in Managed Care	Health Maintenance Organization	Primary Care Case Management	Prepaid Health Plan
1991	10.8%	5.2%	3.2%	1.7%
1992	13.1%	5.7%	4.2%	2.7%
1993	16.0%	6.6%	5.0%	3.9%
1994	24.6%	12.2%	8.8%	2.7%
1995	34.4%	15.2%	11.3%	7.2%
1996	38.9%	22.4%	12.5%	7.3%
1997	47.5%	25.0%	14.1%	10.7%
1998	52.5%	35.9%	13.3%	15.3%
1999	55.0%	36.0%	13.8%	24.3%
2000	54.9%	35.1%	14.2%	23.5%
2001	56.0%	35.4%	14.8%	21.7%
2002	56.9%	37.3%	14.4%	21.9%
2003	58.4%	38.1%	14.7%	21.5%
2004	60.0%	38.4%	13.6%	23.5%
2005	62.2%	39.4%	14.7%	25.2%

*Note: columns 2-4 add up to greater than column 1 because an individual may be enrolled in a dental or behavioral health managed care plan in addition to a medical managed care plan.

Table 3.2: Percent of Impoverished Population Subject to the Following Medicaid Managed Care Policies

Year	Mandatory HMO	Voluntary HMO	Mandatory PCCM	Voluntary PCCM
1990	3.5%	21.0%	3.1%	3.3%
1991	3.6%	22.4%	3.2%	5.2%
1992	3.7%	27.2%	7.8%	11.6%
1993	3.9%	28.6%	9.9%	12.9%
1994	7.6%	29.3%	15.8%	11.8%
1995	11.4%	38.6%	23.2%	12.6%
1996	21.5%	31.5%	26.4%	12.2%
1997	32.9%	25.9%	31.6%	9.8%
1998	48.8%	17.6%	31.7%	8.2%
1999	55.0%	13.6%	33.3%	8.6%
2000	59.1%	11.8%	33.3%	8.1%
2001	58.4%	12.0%	34.6%	7.8%
2002	59.1%	11.9%	34.6%	7.9%
2003	60.0%	11.8%	34.6%	7.8%

Table 3.3: Medicaid Expenditures by Category, Selected Years (in Millions of 2000 \$)

year	1990	1995	2000	2005
Medicaid:				
Inpatient	23317.5	48275.6	36654.8	48949.8
Outpatient	12470.6	21196.8	19353.4	27839.3
Mental Health	2761.0	8501.5	7331.9	7244.6
Prescription Drugs	6073.6	9639.1	16574.8	27046.7
Managed Care	259.4	2089.4	27341.6	46160.2
Medicare Payments	1468.9	3802.8	4163.7	6855.6
Dental	905.1	1771.6	1795.3	2985.2
Other	42857.6	75637.8	81849.0	97471.5
Total Net Expenditures	90547.4	171541.6	195506.2	265156.7
Administrative Total Net Expenditures	4560.2	8672.5	10577.1	13354.1
Medicaid/SCHIP:				
Inpatient			112.9	204.3
Outpatient			201.4	436.4
Mental Health			47.0	58.2
Prescription Drugs			77.9	183.1
Case Management			13.3	16.6
Medicare Payments			0.0	0.0
Dental			48.2	122.9
Premiums			467.3	582.1
Other			129.9	182.9
Total Net Expenditures			1097.9	1786.5
SCHIP:				
Inpatient			22.7	301.5
Outpatient			119.6	445.9
Mental Health			6.3	31.6
Prescription Drugs			29.9	268.1
Case Management			4.5	16.8
Dental			24.4	98.8
Premiums			1296.1	3051.1
Other			179.8	436.4
Total Net Expenditures			1683.5	4650.2

*Expenditures are deflated by the CPI-U Index.

Table 3.4: Percent of State Impoverished Population Subject to Mandatory Medicaid Managed Care Policies, Selected Years

State	1990	1995	2000	2003
AK	0.0%	0.0%	0.0%	0.0%
AL	0.0%	0.0%	89.4%	100.0%
AR	0.0%	100.0%	100.0%	100.0%
AZ	100.0%	100.0%	100.0%	100.0%
CA	1.8%	5.8%	86.3%	87.5%
CO	0.0%	100.0%	100.0%	100.0%
CT	0.0%	0.0%	100.0%	100.0%
DC	0.0%	0.0%	100.0%	100.0%
DE	0.0%	0.0%	100.0%	100.0%
FL	0.0%	59.6%	100.0%	100.0%
GA	0.0%	15.6%	100.0%	100.0%
HI	0.0%	100.0%	100.0%	100.0%
IA	5.9%	82.2%	97.7%	97.8%
ID	0.0%	3.1%	3.8%	3.4%
IL	0.0%	0.0%	0.0%	0.0%
IN	0.0%	31.2%	100.0%	100.0%
KS	48.0%	49.2%	100.0%	100.0%
KY	97.6%	97.6%	99.6%	99.6%
LA	0.0%	12.0%	11.9%	11.1%
MA	0.0%	100.0%	100.0%	100.0%
MD	0.0%	100.0%	100.0%	100.0%
ME	0.0%	0.0%	63.4%	100.0%
MI	0.0%	99.8%	100.0%	100.0%
MN	28.1%	48.0%	79.7%	85.3%
MO	13.3%	13.5%	55.2%	61.1%
MS	0.0%	9.0%	100.0%	100.0%
MT	0.0%	70.6%	96.7%	97.1%
NC	0.6%	36.2%	100.0%	100.0%
ND	0.0%	100.0%	100.0%	100.0%
NE	0.0%	0.0%	43.6%	48.4%
NH	0.0%	0.0%	0.0%	0.0%
NJ	0.0%	0.0%	100.0%	100.0%
NM	0.0%	89.2%	100.0%	100.0%
NV	0.0%	0.0%	71.4%	73.3%
NY	0.0%	0.0%	49.5%	50.0%
OH	5.2%	5.4%	51.6%	53.3%
OK	0.0%	0.0%	100.0%	100.0%
OR	0.0%	100.0%	100.0%	100.0%
PA	0.0%	43.1%	100.0%	100.0%
RI	0.0%	100.0%	100.0%	100.0%
SC	0.0%	0.0%	0.0%	0.0%
SD	0.0%	42.4%	100.0%	100.0%
TN	0.0%	100.0%	100.0%	100.0%
TX	0.0%	5.0%	59.2%	63.8%
UT	73.3%	71.2%	70.4%	73.5%
VA	0.0%	79.9%	85.2%	85.1%
VT	0.0%	0.0%	100.0%	100.0%
WA	4.4%	97.2%	93.0%	79.2%
WI	35.8%	40.3%	80.5%	81.3%
WV	0.0%	56.4%	98.0%	100.0%
WY	0.0%	0.0%	0.0%	0.0%
USA	6.3%	33.2%	77.2%	78.4%

Table 3.5: Percent of the Impoverished Population Subject to the Following Mandatory Policies

Year	HMO Only	Mixed Mandatory	PCCM Only
1990	3.2%	0.3%	2.8%
1991	3.3%	0.3%	3.0%
1992	3.3%	2.8%	5.0%
1993	3.5%	3.6%	6.4%
1994	6.8%	6.8%	9.1%
1995	9.2%	12.1%	11.9%
1996	14.7%	13.7%	13.6%
1997	20.6%	18.9%	13.6%
1998	29.8%	24.0%	13.7%
1999	33.5%	24.1%	15.5%
2000	37.9%	22.9%	16.5%
2001	37.1%	22.8%	17.5%
2002	37.5%	23.2%	17.3%
2003	38.0%	23.6%	16.9%

Table 3.6: First Stage Relationship Between % of Impoverished Population in Mandatory Counties and Actual Medicaid Managed Care Penetration Rates

	MMC Penetration			% Medicaid in HMO			% Medicaid in PCCM		
% Pop in Mandatory MMC County	0.459 (0.030)***	0.449 (0.031)***	0.403 (0.043)***						
% Pop in Mandatory HMO County				0.508 (0.032)***	0.493 (0.033)***	0.480 (0.057)***	-0.070 (0.021)***	-0.069 (0.021)***	-0.035 (0.029)
% Pop in Mixed Mandatory County				0.120 (0.026)***	0.102 (0.026)***	0.066 (0.030)**	0.228 (0.033)***	0.232 (0.034)***	0.278 (0.040)***
% Pop in Mandatory PCCM County				-0.066 (0.020)***	-0.076 (0.022)***	-0.026 (0.031)	0.480 (0.024)***	0.472 (0.025)***	0.360 (0.040)***
% Medicaid in SSI		-0.015 (0.254)	0.369 (0.310)		0.111 (0.209)	0.112 (0.274)		-0.326 (0.174)*	0.340 (0.236)
% Medicaid in AFDC/TANF		0.394 (0.080)***	0.153 (0.101)		0.271 (0.065)***	0.148 (0.105)		0.045 (0.055)	-0.123 (0.063)*
Unemployment Rate		0.020 (0.009)**	0.008 (0.009)		-0.008 (0.007)	-0.007 (0.008)		0.000 (0.006)	0.001 (0.006)
Poverty Rate		0.003 (0.003)	0.003 (0.003)		0.002 (0.003)	0.003 (0.003)		0.004 (0.003)*	0.004 (0.002)*
Constant	-0.097 (0.028)***	-0.522 (0.088)***	-0.213 (0.089)**	-0.071 (0.023)***	-0.196 (0.065)***	-0.050 (0.070)	-0.028 (0.016)*	-0.061 (0.050)	-0.010 (0.054)
F-statistic for Instrument	239.21	216.28	86.30	185.05	165.08	33.81	188.34	168.68	41.96
Observations	663	663	663	663	663	663	663	663	663
R-squared	0.85	0.86	0.90	0.86	0.86	0.92	0.85	0.85	0.90
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Clustered SE	N	N	N	N	N	N	N	N	N
State Trends	N	N	Y	N	N	Y	N	N	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.7a: Instrumental Variables Regression Results
Ln of Per Enrollee Medicaid Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
MMC Penetration	0.124 (0.070)*	0.112 (0.049)**	0.076 (0.065)			
% Medicaid in HMO				0.243 (0.102)**	0.154 (0.063)**	0.169 (0.084)**
% Medicaid in PCCM				0.046 (0.065)	0.119 (0.054)**	-0.006 (0.064)
% Medicaid in SSI		3.408 (0.171)***	3.178 (0.189)***		3.403 (0.172)***	3.265 (0.196)***
% Medicaid in AFDC/TANF		0.553 (0.085)***	0.555 (0.111)***		0.553 (0.084)***	0.511 (0.104)***
Unemployment Rate		0.046 (0.009)***	0.030 (0.009)***		0.049 (0.010)***	0.032 (0.009)***
Poverty Rate		0.004 (0.003)	0.005 (0.002)**		0.004 (0.003)	0.005 (0.002)**
Constant	8.182 (0.072)***	7.092 (0.108)***	7.088 (0.124)***	8.193 (0.075)***	7.076 (0.104)***	7.085 (0.121)***
Observations	663	663	663	663	663	663
R-squared	0.84	0.93	0.96	0.85	0.93	0.96
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Clustered SE	N	N	N	N	N	N
State Trends	N	N	Y	N	N	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.7b: Instrumental Variables Regression Results
Ln of Per Enrollee Medicaid Administrative Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
MMC Penetration	0.168 (0.092)*	0.126 (0.072)*	0.106 (0.108)			
% Medicaid in HMO				0.272 (0.138)**	0.133 (0.098)	0.111 (0.136)
% Medicaid in PCCM				0.072 (0.092)	0.135 (0.083)	0.114 (0.124)
% Medicaid in SSI		4.425 (0.305)***	4.886 (0.502)***		4.447 (0.310)***	4.884 (0.500)***
% Medicaid in AFDC/TANF		0.573 (0.153)***	0.626 (0.208)***		0.584 (0.151)***	0.635 (0.210)***
Unemployment Rate		0.004 (0.012)	0.010 (0.015)		0.008 (0.012)	0.012 (0.015)
Poverty Rate		0.006 (0.004)	0.003 (0.004)		0.006 (0.004)	0.003 (0.004)
Constant	5.534 (0.094)***	4.654 (0.157)***	4.570 (0.196)***	5.540 (0.096)***	4.622 (0.151)***	4.555 (0.193)***
Observations	661	661	661	661	661	661
R-squared	0.83	0.9	0.93	0.83	0.9	0.93
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Clustered SE	N	N	N	N	N	N
State Trends	N	N	Y	N	N	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.7c: Instrumental Variables Regression Results
 Medicaid Enrollment as a Percent of State Population

	(1)	(2)	(3)	(4)	(5)	(6)
MMC Penetration	-0.014 (0.011)	-0.014 (0.007)**	-0.017 (0.009)*			
% Medicaid in HMO				-0.032 (0.017)*	-0.020 (0.011)*	-0.029 (0.013)**
% Medicaid in PCCM				0.000 (0.009)	-0.013 (0.006)**	-0.007 (0.007)
% Medicaid in SSI		-0.552 (0.031)***	-0.515 (0.030)***		-0.548 (0.030)***	-0.526 (0.032)***
% Medicaid in AFDC/TANF		-0.049 (0.019)***	-0.079 (0.019)***		-0.049 (0.018)***	-0.074 (0.018)***
Unemployment Rate		-0.005 (0.001)***	-0.002 (0.001)*		-0.006 (0.001)***	-0.003 (0.001)**
Poverty Rate		0.000 0.000	0.000 0.000		0.000 0.000	0.000 0.000
Constant	0.112 (0.006)***	0.244 (0.022)***	0.211 (0.017)***	0.111 (0.007)***	0.246 (0.021)***	0.212 (0.017)***
Observations	663	663	663	663	663	663
R-squared	0.79	0.91	0.96	0.79	0.91	0.96
State FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Clustered SE	N	N	N	N	N	N
State Trends	N	N	Y	N	N	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.8: Descriptive Statistics for CPS Sample

	1990	1995	2000	2003
Demographics:				
Average Age	6.66	6.75	6.95	7.04
% Black (incl. mixed race Black)	20.6%	20.9%	21.4%	22.1%
% Hispanic	15.3%	19.2%	23.3%	26.3%
% Male	51.3%	51.0%	51.4%	51.2%
% with Mom only	30.6%	32.8%	32.7%	33.8%
Average # People per Family	4.44	4.35	4.34	4.29
% Aged < 6	42.9%	42.8%	39.9%	39.8%
Average Number of Siblings Aged <6	0.58	0.54	0.49	0.49
Average Number of Siblings Aged <18	1.56	1.49	1.49	1.46
SSI/AFDC/TANF Participation:				
% in Families with SSI	3.0%	4.8%	4.2%	3.9%
Average Family SSI income	121	241	256	246
% in Families with AFDC/TANF	18.4%	20.2%	10.7%	7.5%
Average Family AFDC/TANF income	779	918	410	289
Income:				
Average Family Income (Total)	19757	22159	26483	28016
Average Family Earnings	17301	19089	23668	24942
Average Family Income (Other)	2456	3070	2815	3074
Insurance Characteristics:				
% Enrolled in Medicaid	21.7%	35.6%	33.2%	33.3%
% Enrolled in Other Insurance	55.9%	45.8%	48.2%	45.3%
% Uninsured	22.4%	18.6%	18.6%	21.4%
Total Number of Kids	23,482	22,980	18,870	32,042

* The second version of each insurance rate includes kids as being on Medicaid only if the child-Medicaid recode equals 1; the first includes kids in Medicaid if either the Medicaid indicator variable or the child-Medicaid recode equals 1. This is relevant for years 1990-1993 and 2001-2003.

Table 3.9a: Impact of Mandatory Managed Care Policies on Medicaid Enrollment vs. Private Enrollment and Uninsurance

	Other			Other		
	Medicaid	Insurance	Uninsured	Medicaid	Insurance	Uninsured
% Pop in Mandatory MMC County	-0.005 (0.003)	0.000 (0.003)	0.005 (0.003)			
% Pop in Mandatory HMO County				-0.009 (0.004)**	0.012 (0.004)***	-0.002 (0.004)
% Pop in Mandatory PCCM County				-0.001 (0.003)	-0.008 (0.004)**	0.010 (0.004)***
Male	0.002 (0.001)	-0.002 (0.002)	0.001 (0.002)	0.002 (0.001)	-0.002 (0.002)	0.001 (0.002)
With Mom Only	0.057 (0.002)***	-0.016 (0.002)***	-0.040 (0.002)***	0.057 (0.002)***	-0.016 (0.002)***	-0.040 (0.002)***
Black	0.031 (0.002)***	-0.060 (0.002)***	0.029 (0.002)***	0.031 (0.002)***	-0.060 (0.002)***	0.029 (0.002)***
Hispanic	0.029 (0.002)***	-0.133 (0.002)***	0.103 (0.002)***	0.029 (0.002)***	-0.132 (0.002)***	0.103 (0.002)***
Aged under 6	0.066 (0.002)***	-0.035 (0.002)***	-0.032 (0.002)***	0.066 (0.002)***	-0.035 (0.002)***	-0.031 (0.002)***
# Siblings under 6	0.019 (0.001)***	-0.006 (0.001)***	-0.013 (0.001)***	0.019 (0.001)***	-0.006 (0.001)***	-0.013 (0.001)***
# Siblings under 18	-0.008 (0.001)***	0.027 (0.001)***	-0.019 (0.001)***	-0.008 (0.001)***	0.027 (0.001)***	-0.019 (0.001)***
Family Receives SSI Benefits	0.291 (0.004)***	-0.213 (0.003)***	-0.077 (0.003)***	0.291 (0.004)***	-0.214 (0.003)***	-0.077 (0.003)***
Family Receives AFDC/TANF Benefits	0.543 (0.002)***	-0.290 (0.002)***	-0.253 (0.002)***	0.543 (0.002)***	-0.290 (0.002)***	-0.253 (0.002)***
State Unemployment Rate	-0.003 (0.002)*	0.003 (0.002)*	0.000 (0.002)	-0.003 (0.002)*	0.002 (0.002)	0.000 (0.002)
State Poverty Rate	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)	0.000 (0.001)
Constant	0.226 (0.017)***	0.275 (0.019)***	0.499 (0.018)***	0.223 (0.018)***	0.282 (0.019)***	0.495 (0.018)***
Observations	323586	323586	323586	323586	323586	323586
R-squared	0.44	0.37	0.11	0.44	0.37	0.11
State, Year FE	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y
Poverty Category*Year FE	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.9b: Impact of Mandatory Managed Care Policies on Medicaid Enrollment vs. Private Enrollment and Uninsurance, Total Sample & Race Interactions

	Other			Other		
	Medicaid	Insurance	Uninsured	Medicaid	Insurance	Uninsured
% Pop in Mandatory MMC County	-0.004 (0.003)	-0.002 (0.004)	0.005 (0.004)			
% Pop in Mandatory HMO County				-0.009 (0.004)**	0.013 (0.005)***	-0.004 (0.004)
% Pop in Mandatory PCCM County				0.001 (0.004)	-0.013 (0.004)***	0.012 (0.004)***
Black*% in Mandatory MMC County	0.012 (0.004)***	0.003 (0.005)	-0.016 (0.005)***			
Hispanic*% in Mandatory MMC County	-0.015 (0.004)***	0.004 (0.004)	0.011 (0.005)**			
Black*% Pop in Mandatory HMO County				0.004 (0.006)	-0.002 (0.006)	-0.002 (0.006)
Black*% Pop in Mandatory PCCM County				0.019 (0.005)***	0.008 (0.006)	-0.026 (0.006)***
Hispanic*% Pop in Mandatory HMO County				-0.008 (0.005)	-0.001 (0.006)	0.009 (0.006)*
Hispanic*% Pop in Mandatory PCCM County				-0.022 (0.005)***	0.010 (0.005)*	0.011 (0.006)**
Male	0.002	-0.002	0.001	0.002	-0.002	0.001
Constant	0.230 (0.017)***	0.275 (0.019)***	0.494 (0.018)***	0.227 (0.018)***	0.283 (0.019)***	0.491 (0.018)***
Observations	323586	323586	323586	323586	323586	323586
R-squared	0.44	0.37	0.11	0.44	0.37	0.11
State, Year FE	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y
Poverty Category*Year FE	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.9c: Impact of Mandatory Managed Care Policies on Medicaid Enrollment vs. Private Enrollment and Uninsurance, Total Sample & Poverty Level Interactions

	Other			Other		
	Medicaid	Insurance	Uninsured	Medicaid	Insurance	Uninsured
% Pop in Mandatory MMC County	-0.008 (0.004)*	-0.004 (0.004)	0.012 (0.004)***			
% Pop in Mandatory HMO County				-0.017 (0.006)***	0.012 (0.005)**	0.004 (0.005)
% Pop in Mandatory PCCM County				-0.001 (0.005)	-0.016 (0.004)***	0.017 (0.005)***
100-199% Pov.*% Pop in Mandatory MMC County	-0.002 (0.005)	0.014 (0.005)**	-0.011 (0.005)**			
200-299% Pov.*% Pop in Mandatory MMC County	0.013 (0.005)***	-0.002 (0.005)	-0.011 (0.005)**			
100-199% Pov.*% Pop in Mandatory HMO County				0.007 (0.007)	0.010 (0.007)	-0.017 (0.007)***
100-199% Pov.*% Pop in Mandatory PCCM County				-0.009 (0.006)	0.016 (0.006)***	-0.007 (0.006)
200-299% Pov.*% Pop in Mandatory HMO County				0.016 (0.006)***	-0.014 (0.006)**	-0.002 (0.006)
200-299% Pov.*% Pop in Mandatory PCCM County				0.011 (0.005)**	0.005 (0.006)	-0.016 (0.005)***
Constant	0.227 (0.017)***	0.276 (0.019)***	0.497 (0.018)***	0.224 (0.018)***	0.282 (0.019)***	0.494 (0.018)***
Observations	323586	323586	323586	323586	323586	323586
R-squared	0.44	0.37	0.11	0.44	0.37	0.11
State, Year FE	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y
Poverty Category*Year FE	Y	Y	Y	Y	Y	Y

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 3.1: Medicaid Managed Care Penetration and Expenditure Growth

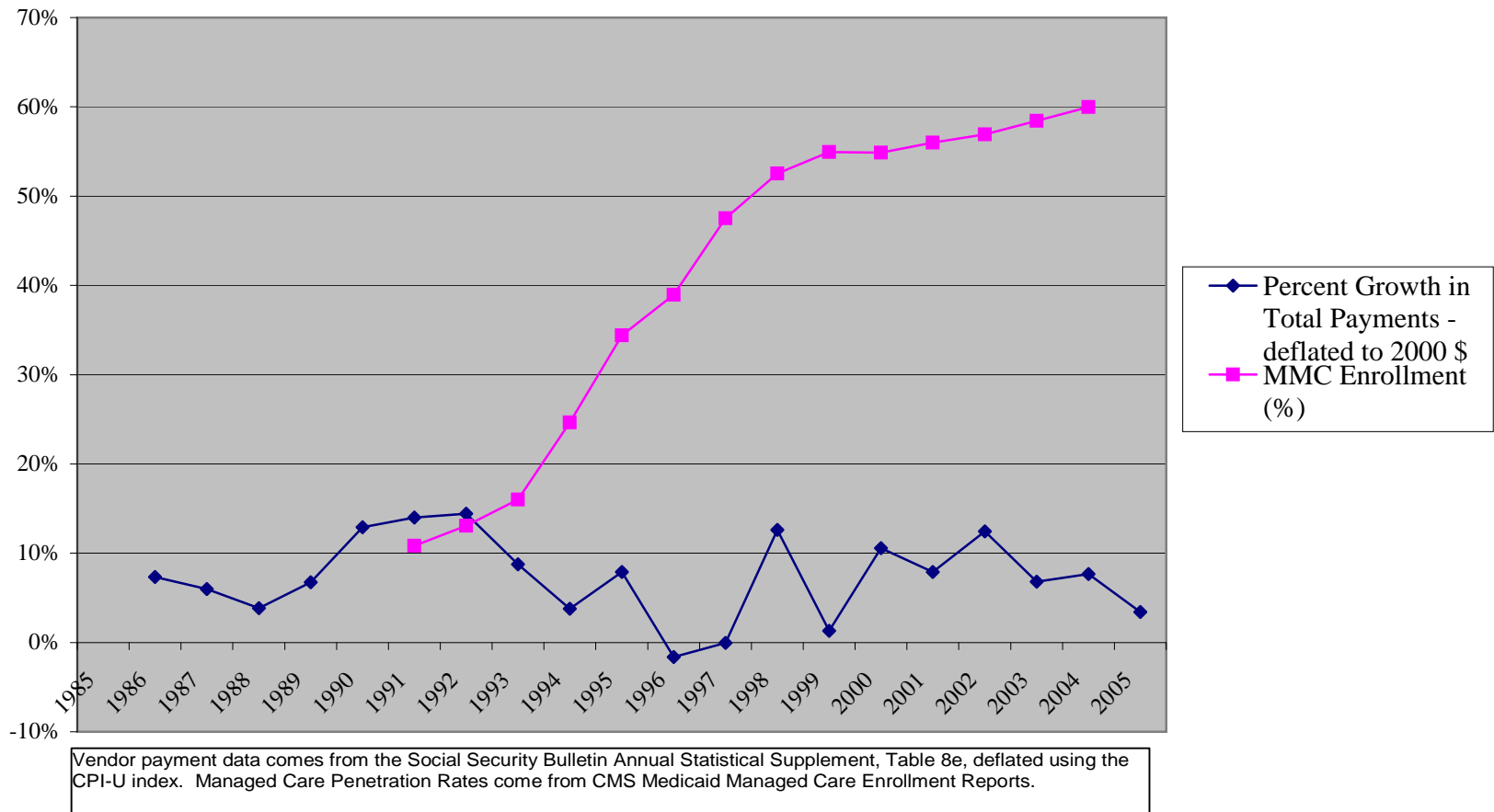
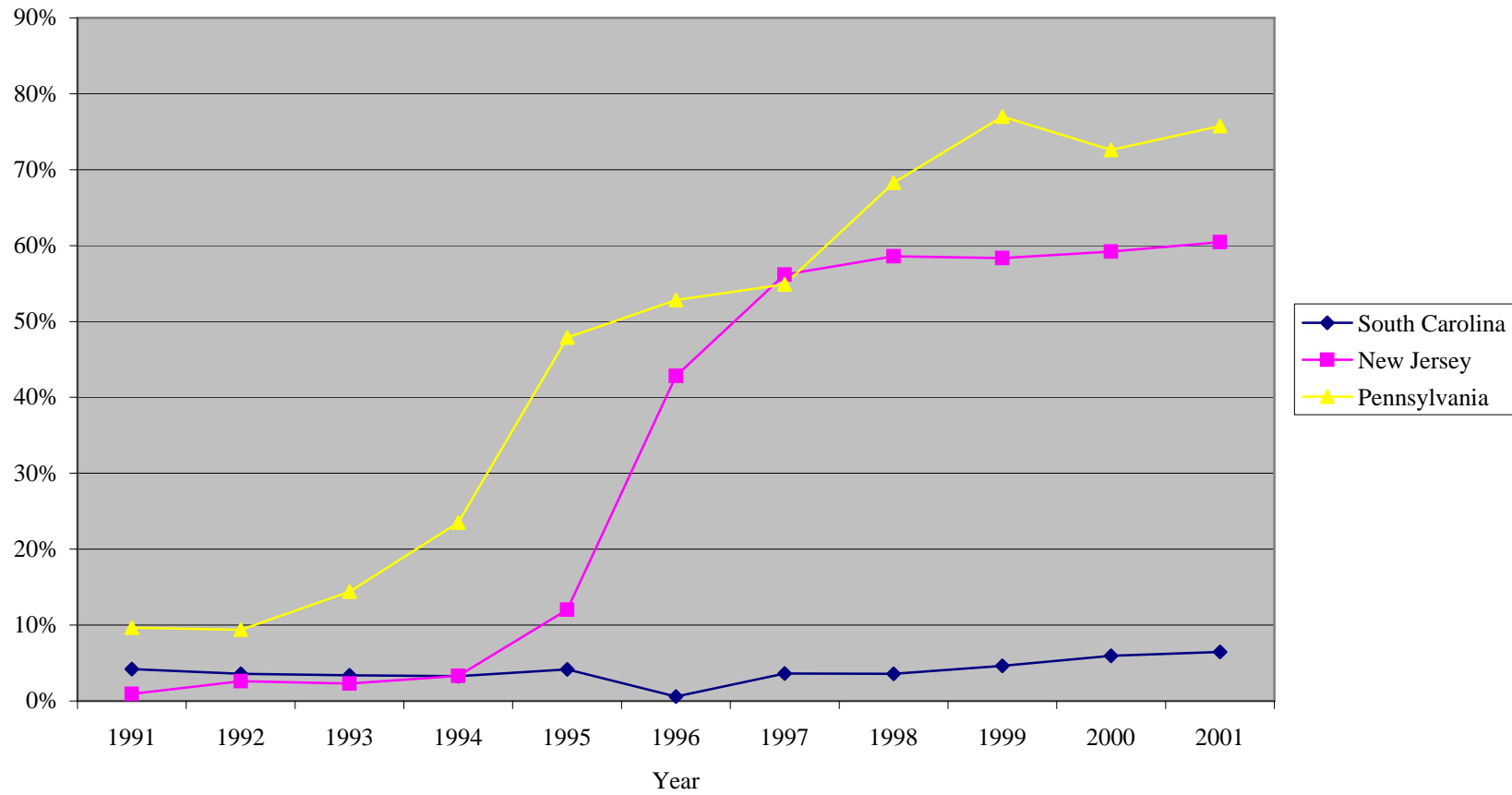


Figure 3.2: Medicaid Enrollment Patterns, Selected States



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