Essays on the Determinants of Healthcare Utilization

by

Kathleen Fehring Easterbrook


Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of
Doctor of Philosophy

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

June 2015

© 2015 Kathleen Fehring Easterbrook. All rights reserved.

The author hereby grants to MIT permission to reproduce and to distribute publicly paper and electronic copies of this thesis document in whole or in part in any medium now known or hereafter created.

___________________________
Author

___________________________
Department of Economics
May 15, 2015

___________________________
Certified by

___________________________
Jonathan Gruber
Ford Professor of Economics
Thesis Supervisor

___________________________
Certified by

___________________________
Heidi Williams
Class of 1957 Career Development Assistant Professor of Economics
Thesis Supervisor

___________________________
Accepted by

___________________________
Ricardo Caballero
Ford International Professor of Economics
Chairman, Departmental Committee on Graduate Studies
Abstract

The first chapter investigates how hospital mergers affect technology adoption and utilization. I analyze the mergers of five for-profit hospital chains in a difference-in-difference framework, comparing markets in which two or more of the merging chains operated prior to the mergers to markets in which zero or one chain operated. The estimates suggest that treated markets gained 5.1 to 7.1 technologies as a result of the mergers. An increase of 5.1 technologies represents 39% of the increase in technology levels in these markets during the post-merger period. I find little evidence that utilization increased post-merger, but there is some evidence that utilization of certain technologies increased when they were more widely available.

In the second chapter, I investigate whether the hospitals that were part of the merging chains achieved cost savings through local consolidation and economies of scale resulting from the formation of a large hospital system. Exploiting variation in how the hospitals were affected by the mergers, I find no evidence that local consolidation led to lower expenditures. However, I document that expenditures at hospitals that were part of the merging chains declined by 14% after the mergers, suggesting there may be economies of scale for a large, national chain. Taken together, the results imply limited cost efficiencies from local consolidation for hospitals that are part of a larger system.

The final chapter, co-authored with Jonathan Gruber, examines how individuals respond to changes in copays for physician office visits using data from over 1500 employer-sponsored insurance plans with 1.5 million enrollees. Within our sample, there are 171 separate changes in copays for physician office visits, and we use a difference-in-difference methodology to evaluate their effect. We estimate the elasticity of office visit spending with respect to patient contribution is -0.13. Individuals with chronic conditions appear to be less sensitive to the price of office visits than individuals without chronic conditions. We find suggestive evidence that these effects compound, as copay increases correspond with declines in spending on labs and radiology and a larger decrease in total spending than office visit spending alone.
Acknowledgments

First and foremost, I want to thank Jon Gruber and Heidi Williams, without whose guidance, perspective, and encouragement this dissertation would not have been possible. They taught me what makes a good research topic, how to turn an idea into a project, and how to overcome the inevitable challenges of research. They were extraordinarily generous with their time and gave me advice on everything from my research strategy and framing down to technical details and writing suggestions. Even when things I tried didn’t work, they deepened my interest in health economics, and I always left our meetings feeling more optimistic. They also helped me navigate the PhD program and the next step in my career. I am incredibly fortunate to have had them as mentors.

In addition, I am grateful to Nancy Rose for pushing me to think about how my research related to wider issues in industrial organization. Jim Poterba provided valuable suggestions on my work in addition to invaluable career advice. It highly likely I would never have had this opportunity if it weren’t for my undergraduate advisers, Bob Hall and Greg Rosston, who instilled in me a love of economics and encouraged me to pursue a PhD.

Throughout my years at MIT, I met a group of amazingly talented individuals, and I am honored to be able to call them colleagues and friends. I thank Manasi Deshpande, Sarah Moshary, Arianna Ornaghi, Manisha Padi, Brendan Price, Adrienne Sabety, Melanie Wasserman, and Yufei Wu for the many conversations, coffee breaks, dinners, drinks, and suggestions over the years. One year ahead of me in the program, I looked to Maria Polyakova and Adam Sacarny for advice on research and life in the following year. I also had the pleasure of navigating the world of teaching with Richard McDowell and Sara Hernandez.

I am grateful that I had the opportunity to be part of the community at the National Bureau of Economic Research. I benefitted from the Aging and Health Fellowship, which gave me the opportunity to draw on a network of peers with deep knowledge in healthcare. Also, I thank Jean Roth and Mohan Ramanujan for administering much of the data I used. Jean was tremendously helpful when I was trying to learn a new dataset, and Mohan kindly responded to emails anytime I had problems, mostly self-induced, with the servers.

I owe a great debt of gratitude to my family and David Houska for the incredible amounts of moral support they provided through this process. My parents’ kind words, love, and unfailing belief in me, though constants in my life, never meant more than as I faced the challenges of the PhD. And many thanks to my sister Allison and cousin Cristina for diverting my attentions away from my studies now and then. David, my calmer, wiser half, patiently listened to every small problem and victory in the research process, and his humor and conviction that I could resolve the latest challenge were a source of strength for me. I am grateful for the conversations, weekend visits, and vacations—the much-needed breaks which let me return to my research with a clearer perspective.

Lastly, I’d like to thank the friends and family, too numerous to list here, who offered support and encouragement over the last five years.
Contents

1 The Effects of Hospital Mergers on Technology Adoption and Utilization 15
   1.2 The Columbia/HCA Mergers and Methodology .......................... 18
   1.3 Data and Sample Restrictions ........................................... 21
      1.3.1 Measuring Technology Adoption .................................. 22
      1.3.2 Measuring Utilization ............................................. 24
      1.3.3 Additional Data .................................................... 24
   1.4 Empirical Analysis ...................................................... 25
      1.4.1 Descriptive Statistics ............................................. 25
      1.4.2 Effect of Mergers on Technological Adoption ................... 26
      1.4.3 Relationship between Technology Adoption and Utilization .... 29
      1.4.4 Effect of Mergers on Technology Utilization ................... 31
   1.5 Robustness Checks ...................................................... 33
      1.5.1 Alternative Specifications and Definitions of the Technology Index 33
      1.5.2 Alternative Samples .............................................. 35
   1.6 Possible Mechanisms ................................................... 36
      1.6.1 Competing with HCA .............................................. 36
      1.6.2 Temporary Increase in Revenues and Admissions ................. 37
   1.7 Conclusion ............................................................. 39
   Figures ............................................................................. 41
   Tables ............................................................................. 48

2 Market Consolidation, System Formation, and Hospital Expenditures 55
   2.2 The Columbia/HCA Mergers and Methodology ......................... 58
   2.3 Data ............................................................................ 61
Appendix to Chapter 3

C.1 Technical Appendix ................................................................. 137
C.2 Figures and Tables ................................................................. 140
List of Figures

1-1 Markets with For-Profit Hospitals ......................................... 41
1-2 Technology Levels before and after the Mergers .......................... 42
1-3 Relationship between Technology Availability and Use .................. 43
1-4 Technology Utilization before and after the Mergers ..................... 44
1-5 Revenues and Admissions at Rival Hospitals before and after the Mergers 45
1-6 Market Consolidation and Changes in Revenues, Admissions, and Technology Immediately Following the Mergers ........................................... 46
1-7 Relationship between Admissions, Revenues, and Technology at Rival Hospitals Immediately Following the Mergers ........................................... 47

2-1 Volume-Adjusted Expenditures for the Columbia/HCA System Pre and Post-Merger 72
2-2 Treatment and Control Markets to Evaluate Effects of Local Consolidation .... 73
2-3 Change in Expenditures at Columbia/HCA Hospitals that Experienced Local Consolidation ................................................................. 74
2-4 Output-Adjusted Expenditures for Columbia/HCA and Other Hospitals .... 75
2-5 Change in Expenditures at Columbia/HCA Hospitals Compared to Other Hospitals 76

3-1 Distribution of Office Visit Copays in Baseline Year ...................... 98
3-2 Distribution of Copay Changes among Plans that Changed Copays .......... 99
A-1 Technology Utilization before and after the Mergers ..................... 115
A-2 CT Adoption and Use before and after the Mergers ...................... 116
A-3 MRI Adoption and Use before and after the Mergers ...................... 117
A-4 SPECT Adoption and Use before and after the Mergers .................. 118
A-5 Open-Heart Surgery Adoption and Use before and after the Mergers .... 119
A-6 Cardiac Catheterization Adoption and Use before and after the Mergers 120
A-7  Angioplasty Adoption and Use before and after the Mergers          121
A-8  Adoption of Diagnostic Radioisotopes, ESWL, and Advanced OB Units before and
      after the Mergers                                           122
A-9  Technology Adoption before and after the Mergers, Robustness Checks   123
B-1  Robustness Checks for Change in Columbia/HCA’s Expenditures in Markets with
      Local Consolidation                                      133
B-2  Robustness Checks for Change in Columbia/HCA’s Expenditures Relative to Other
      Hospitals                                               134
C-1  Change in Utilization before and after Copay Increases             140
List of Tables

1.1 Market Summary Statistics for Treatment and Control Markets, 1992 .......................... 48
1.2 Market Technology Levels, 1992 .................................................................................. 49
1.3 Market Utilization Levels, 1992 .................................................................................. 49
1.4 Technology Adoption in Markets with Mergers ............................................................ 50
1.5 Technology Adoption in Markets with Mergers, Controlling for Number of Hospitals in Market .................................................................................................................. 51
1.6 Relationship between Technology Levels and Use ....................................................... 52
1.7 Utilization in Markets with Mergers .............................................................................. 53

2.1 Summary Statistics for Columbia/HCA and non-Columbia/HCA Hospitals, 1992 .... 77
2.2 Change in Expenditures at Columbia/HCA Hospitals in Markets with Mergers .... 78
2.3 Post-System Formation Change in Expenditures at Columbia/HCA Hospitals .... 79

3.1 Summary Statistics for Plans in Baseline Year .............................................................. 100
3.2 Change in Office Visit Expenditures Following Change in Doctor Visit Copay ........ 101
3.3 Change in Office Visit Use Following Change in Doctor Visit Copay .......................... 102
3.4 Change in Office Visit Expenditures for Patients with and without Chronic Conditions 103
3.5 Change in Office Visit Use for Patients with and without Chronic Conditions ........ 104
3.6 Change in Other Types of Medical Expenditures Following Change in Doctor Visit Copay ................................................................................................................................. 105
3.7 Change in Other Types of Medical Expenditures for Patients with and without Chronic Conditions ............................................................................................................................. 106

A.1 Relationship between Imaging Technology Levels and Use ....................................... 124
A.2 Relationship between Cardiac Technology Levels and Use ......................................... 125
A.3 Technology Adoption in Markets with Mergers, Sensitivity Analysis ......................... 126
A.4 Utilization in Markets with Mergers, Sensitivity Analysis .......................... 127
A.5 Technology Adoption among a Balanced Panel of Hospitals ......................... 128
A.6 Technology Adoption in Markets with Mergers, Excluding HCA-Healthtrust Markets 129
A.7 Utilization in Markets with Mergers, Excluding HCA-Healthtrust Markets ....... 130
A.8 Technology Adoption in Markets where HCA Owned Hospitals .................... 131
B.1 Change in Expenditures at Columbia/HCA Hospitals in Markets with Mergers, Sensitivity Analysis ................................................................. 135
B.2 Post-System Formation Change in Expenditures at Columbia/HCA Hospitals, Sensitivity Analysis .............................................................................. 136
C.1 Changes in Utilization for Plans with Known Inpatient and Outpatient Prices .... 141
C.2 Sensitivity of Changes in Utilization to Alternative Cutoff for Outliers ............ 142
C.3 Sensitivity of Changes in Utilization to Controls Using Full Sample ............... 143
C.4 Sensitivity of Changes in Utilization to Controls among Patients without Chronic Conditions .......................................................... 144
C.5 Sensitivity of Changes in Utilization to Controls among Patients with Chronic Conditions .......................................................... 145
Chapter 1

The Effects of Hospital Mergers on Technology Adoption and Utilization*

The diffusion and utilization of medical technologies has attracted much attention. Economists point to technology as one of the main drivers of increasing health expenditures, which have risen from 5% of GDP in 1960 to approximately 18% in 2010. Many studies have investigated the causes of technological innovation, adoption, and utilization in medical markets, including the role of competition (Dranove, Shanley and Simon 1992, Hamilton and McManus 2005, Carey, Burgess and Young 2009), strategic incentives (Schmidt-Dengler 2006, Lenzo 2007, Lenzo 2010), insurance markets (Baker 2001, Baker and Phibbs 2002), and market size (Acemoglu and Linn 2004).

This paper investigates how hospital mergers affect the adoption and utilization of medical technologies, complementing the literature on the role of hospital competition. Mergers change the nature of competition in the market. They reduce the number of competitors, yet the merged entity may be a stronger rival than the two separate hospitals. How competition affects technological adoption depends on the extent to which hospitals compete on price or quality. If hospitals tend to compete on price, hospitals with less (more) competition will have higher (lower) prices. This in turn could affect the financial resources available to invest in new technologies. Alternatively, hospitals

*1 I am extremely grateful to Jon Gruber, Heidi Williams, and Nancy Rose for their advice and guidance on this project. I thank Sarah Moshary, Manisha Padi, Daria Pelech, Maria Polyakova, Jim Poterba, Brendan Price, Arianna Ornaghi, Adrienne Sabety, Adam Sacarny, Mark Shepard, Zirui Song, Melanie Wasserman, Yufei Wu as well as seminar participants at the MIT Public Finance and Industrial Organization lunches and the NBER Health and Aging Seminar for their comments and suggestions. I also want to thank Jean Roth and Mohan Ramanujan for their invaluable support with systems and data. Leemore Dafny and Laurence Baker generously shared data and code. I gratefully acknowledge funding from the Bradley Fellowship Program and the National Institute on Aging grant T32-AG000186. All errors are my own.

1 Newhouse (1992) is the seminal study on the contribution of technology to the increase in health expenditures.
could primarily compete on quality. If patients and doctors perceive technology as providing “higher quality” care, less (stronger) competition would lead hospitals to adopt less (more) technology.

In addition to shedding light on competition, mergers merit attention in their own right. Recent years have seen an increase in the number of hospital mergers and system acquisitions compared to much of the 2000s (Saxena, Sharma and Wong 2013, The Hospital Acquisition Report 2014). Depending on how mergers impact technological adoption and utilization, this market restructuring could lead to changes in technological adoption rates and cost growth. The effects of mergers on technology adoption and utilization are also relevant for antitrust policy, as they could affect costs and patient outcomes. However, incorporating these effects into antitrust policy analysis would require knowledge not only of whether mergers increase or decrease technological adoption, but also whether a change in adoption is beneficial to patients. Examining how a change in technology affects patients outcomes is beyond the scope of this paper.

To understand how mergers affect hospitals’ decisions, I analyze a series of for-profit hospital chain mergers in a difference-in-difference framework. Between 1993 and 1995, five for-profit hospital chains merged into one company, Columbia/HCA. Markets that had hospitals owned by two or more of the merging chains became more consolidated whereas markets in which zero or one chain operated hospitals did not. I compare technology adoption and utilization between these two types of markets to analyze the effect of the mergers.

Two aspects of my study design suggest the key assumption underlying the difference-in-difference model—that treatment and control markets would have similar technology adoption and utilization in absence of the merger—is reasonable for my analysis. First, my study has the advantage of focusing on the mergers of national chains, which mitigates this concern. Mergers of large chains are arguably less likely to be driven by unobservable characteristics in a treatment market than the merger of two independent hospitals within that market. The second aspect is that I restrict my analysis to markets with two or more for-profit hospitals since every treatment market, by definition, had at least two for-profit hospitals. Hence, two for-profit hospitals deemed every market in my study attractive enough to enter.

I document technology adoption increased in markets that became more consolidated. The rivals of the merging hospitals adopted more technologies after the mergers, leading to an increase in technological capabilities at the market level. I estimate that, in the average treatment market, 5.1 to 7.1 new technologies were adopted as a result of the mergers. The lower end of these estimates represents 39% of the increase in these markets’ technological capabilities in the years
following the mergers. Investigating heterogeneity across nine technologies in the sample, I find that statistically significant increases in adoption of diagnostic radioisotopes, SPECT scanners, and advanced obstetrics (OB) units.² Any statistically significant increases among the other six technologies tend to be sensitive to specification.

I uncover little evidence that utilization of technologies changed systematically as a result of the mergers. Technological use could change either due to increased availability of technology or due to the mergers altering financial incentives to perform certain procedures. While mergers do not appear to impact overall utilization, I do find some evidence that when a particular technology became more widely available in the market, its utilization increased, suggesting the relationship between adoption and use may be heterogeneous across technologies.

The increase in technology adoption is correlated with temporary, statistically insignificant increases in admissions and revenues at the rival hospitals. While far from conclusive, these correlations are consistent with several theories on the effects of mergers. One explanation is that the mergers caused temporary disruptions in the markets, during which patients sought care at the rival hospitals. Higher patient volumes increased revenues, and some combination of greater scale and liquidity prompted these hospitals to adopt new technologies. Alternatively, if hospitals compete on quality, rival hospitals may have adopted technologies to attract patients. If the Columbia/HCA hospitals were making merger-specific investments, the rival hospitals may have felt pressure to become more competitive. The rival hospitals may have been only temporarily successful as Columbia/HCA attracted patients upon completing their merger-specific investments.

In related work, Dranove and Shanley (1995) examine whether hospitals that are part of local systems have less technology; the cross-sectional analysis indicates they do not. My study addresses a similar question, but improves upon their methodology, analyzes the behavior of rivals and market-level changes, as well as examines the use of technologies.

Other studies of hospital mergers have focused on how they affect prices (Dafny 2009, Krishnan 2001, Tenn 2011, Haas-Wilson and Garmon 2011, Gaynor and Vogt 2003, Capps, Dranove and Satterthwaite 2003, Gowrisankaran, Nevo and Town 2013), costs (Dranove and Lindrooth 2003), and quality (Ho and Hamilton 2000, Capps 2005, Mutter, Romano and Wong 2011, Romano and Balan 2011, Hayford 2012). Additionally, Cuellar and Gertler (2005) examine how hospitals' prices, costs, and quality change after being acquired by a system. Generally, the literature has documented

---

²SPECT scanners and diagnostic radioisotopes are both diagnostic tools. Advanced OB units are equipped to handle relatively complicated deliveries.
that hospitals increase prices post-merger, and Dafny (2009) shows that rivals of the merging entities also increase prices. The evidence on how mergers affect quality is much more mixed, and many quality measures are unaffected. For a more extensive review of the literature on hospital mergers, see Gaynor and Town (2012).

The next section provides institutional background on the mergers analyzed in my study and describes my methodology. Section 1.3 discusses the data and sample selection. Section 1.4 presents my main empirical analysis. Section 1.5 documents a series of robustness checks, and Section 1.6 describes potential explanations for my findings. The final section concludes.

1.2 The Columbia/HCA Mergers and Methodology

That mergers are not random is a challenge for every empirical study of mergers. I focus on the mergers of five for-profit hospital chains between 1993 and 1995 into the company Columbia/HCA, a strategy that mitigates concerns about the comparability of treatment and control groups. In this section, I provide background on these mergers and discuss the advantage of focusing on mergers of large chains.

The Hospital Corporation of America (HCA) was founded in the 1960s in Nashville, Tennessee (Lutz and Gee 1998, p. 14-15). By the second half of the 1980s, HCA owned over 250 hospitals, though it and other for-profit chains faced financial difficulties (Kuttner 1996). Medicare had moved from a fee-for-service payment model to a prospective payment system (PPS) in 1983. Under PPS, Medicare reimbursed hospitals fixed amounts based on patients' diagnoses rather than for paying for every service provided. Increasing numbers of privately-insured patients were enrolling in managed care insurance plans, which usually contracted with fewer hospitals and negotiated lower reimbursement rates in exchange for offering relatively higher volumes of patients (Barro and Cutler 1997). Further, managed care plans offered incentives to reduce inpatient days, which decreased demand at hospitals. As a result, Medicare's payment change and the rise in managed care put pressure on hospital revenues.

Additionally, HCA and Humana, the largest hospital chains at that time, each tried unsuccessfully to operate hospitals and HMO insurance plans. Both companies eventually split their insurance and hospital businesses. Humana spun off its hospitals in 1993, creating the for-profit hospital chain Galen Health Care Corporation. HCA also spun off 104 of its hospitals in 1987, which became the company Healthtrust (Kuttner 1996, Lutz and Gee 1998).
Columbia Hospital Corporation entered the market in 1988 with two hospitals in El Paso, Texas (Lutz and Gee 1998, p. 52). By 1992, Columbia had grown to 24 hospitals. Its strategy was to acquire existing hospitals “cheaply, upgrading some acquisitions and closing or consolidating others” (Kuttner 1996, p. 364). In 1993, Columbia acquired Galen, growing to 95 hospitals (Kuttner 1996).

It has just completed that merger in September when it announced on October 2, 1993 that it would merge with HCA. The new company, called Columbia/HCA, would be based in Louisville, KY and Nashville, TN and would own about 200 hospitals (Freudenheim 1993). Early in 1994, Healthtrust acquired Epic, another for-profit chain (HealthTrust Completes Acquisition of EPIC Holdings 1994). Then on October 4, 1994, Columbia/HCA announced that it would acquire Healthtrust (Amos 1994). The merger was completed in early 1995, resulting in the company owning over 300 hospitals (Amos 1994, Lutz 1995). During the acquisition of Healthtrust, the FTC required Columbia/HCA to divest seven hospitals (Lutz 1995). Following the mergers, Columbia/HCA owned over 40% of for-profit general medical and surgical hospitals and about 6% of all general hospitals in the US.

My empirical strategy makes use of variation in how markets were affected by the Columbia/HCA mergers. Markets in which two or more of the five merging chains operated hospitals in 1992 became more consolidated by 1995 as a result of the mergers. I define these markets as the “treatment” markets. Markets that did not have hospitals owned by two or more of these chains did not become more consolidated because of the mergers and hence function as the “control markets.” To facilitate comparisons between the two groups, I limit the control markets to those with two or more for-profit hospitals; by definition, all treatment markets have two for-profit hospitals. I use a difference-in-difference strategy, comparing how technology adoption and utilization changed over time in these two types of markets. This empirical strategy is very similar to the one that Dafny, Duggan and Ramanarayanan (2012) employ. They exploit local variation in insurer market concentration due to the merger of two national health insurers to study how market concentration affects insurance premiums.

The identifying assumption is that had the mergers not occurred, technology adoption and utilization would have had similar trends in treatment and control markets. As I will show in sections 1.4 and 1.5, the data provide empirical support for this assumption.

My empirical strategy also mitigates the concern that the timing of the mergers or their occurrence is correlated with changes in unobserved market factors. Many previous merger studies compare the outcomes of two hospitals that merge (or many pairs of merged hospitals) with hospitals that did not merge. The concern is that some unobservable characteristic—such as a change
in patient preferences—explains both why and when the hospitals merged and why technological adoption changed. Differences in unobservable characteristics are arguably less likely to be driving the results in my setting since I study the mergers of national chains.

In particular, the mergers are unlikely to have occurred solely because the company wanted to gain market power in the exact markets that became more consolidated. Of course, the markets that became more consolidated are not random. Favorable conditions in the treatment markets likely made these mergers attractive for Columbia, and in its 1993 Annual Report to Shareholders, Columbia/HCA acknowledges the company will benefit from “synergies” and “efficiencies” in overlapping markets in the Galen and HCA mergers. The concern this poses is partially alleviated because with each merger, Columbia not only acquired market power in treatment markets, but entered many of the control markets. This happened when the target chain was present in a market, but it was the only chain of the five merging chains present. In acquiring Galen, Columbia increased its market power in 7 markets, but also expanded into 34 markets in my study. When merging with HCA, Columbia consolidated market power in 17 markets and entered an additional 24 markets. Favorable conditions in the markets in which Columbia entered because of these mergers would also have made these mergers attractive for Columbia. Additionally, the number of markets entered with each merger strongly suggests the goals of these mergers included much more than acquiring market power in overlapping markets. To that point, Columbia/HCA also says there is growth opportunity from bringing Columbia’s strategy for efficient care to markets in its 1993 Annual Report.

I investigate how mergers affect technology adoption and utilization at a market level. The market level results have more direct implications for welfare than hospital level results. Additionally, I examine how mergers affect technology adoption for two groups of hospitals within the market: the Columbia/HCA hospitals and non-Columbia/HCA hospitals (“rival” hospitals). I am able to decompose the estimates in this manner since the merging chains operate hospitals in both treatment and control markets. When a merging chain is present in a control market, it is the only one of the five merging chains that is in 1992. Hence, I can compare the technological capabilities among Columbia/HCA hospitals between markets that became more consolidated and markets that did not. Similarly, I compare outcomes at non-Columbia/HCA hospitals in treatment and control markets to see how rival hospitals respond to the mergers.
1.3 Data and Sample Restrictions

To study how hospital mergers affect technology adoption and utilization, I need data on the composition of hospital systems, the location of hospitals, the technologies available at hospitals, and the utilization of those technologies. Data on market characteristics help me select my sample of markets and evaluate the comparability of the treatment and control markets. I supplement this data with information on hospital revenues and admissions when investigating the mechanisms that cause technological adoption to change post-merger.

The American Hospital Association (AHA) Survey contains much of the information needed. The survey collects data for every hospital in the US and includes data on whether a hospital is part of a system, and if so, what system, allowing me to identify which hospitals were part of chains that became Columbia/HCA. The hospitals’ addresses allowed me to identify which markets became more consolidated and which did not. I define markets as hospital referral regions (HRRs). The Dartmouth Atlas constructed HRRs such that patients living within the HRR receive the majority of major cardiovascular procedures within that HRR. There are 306 HRRs in the United States. 45 of these had two or more of the merging chains present in 1992.3

With information on hospital ownership in the AHA survey, I restrict the markets in my study to those with at least two for-profit general medical and surgical hospitals in 1992. This reduces the number of control markets in my study to 65 and results in a sample of 110 markets. The restriction helps ensure that the treatment and control markets are comparable. Markets in which for-profit hospitals enter may have different underlying characteristics, such as patient preferences for medical care, that make them attractive to for-profit hospitals. Every treatment market, by definition, must have at least two for-profit hospitals, and the restriction ensures that the control markets do as well. Additionally, for-profit hospitals have a much larger presence in the south and west of the United States. By restricting to markets with two for-profit hospitals, both my treatment and control markets are drawn from the same regions of the US.

Figure 1-1a shows the HRRs where the merging chains operated in 1992. The dark blue shows markets where two or more of the merging chains owned hospitals. The light blue shows where one of the merging chains owned a hospital or hospitals. Figure 1-1b shows the treatment and control markets for this study. The dark blue markets are the treatment markets and are the same as the dark blue markets in Figure 1-1a. The light blue markets are the control markets. Comparing the

---

3When defining treatment markets, I do not consider the hospitals Columbia/HCA had to divest when it acquired Healthtrust as documented in the prior section.
two maps, 48 of the markets with one of the merging chains present are also control markets in my study. Differences between the maps occur because of restrictions. The control markets include any market with two or more for-profit hospital, regardless of whether those hospitals were affiliated with Columbia/HCA, and any market with just one for-profit hospital was discarded, even if that hospital belonged to Columbia/HCA.

The maps also show that the markets in my study are largely in the south and west of the US. Since both treatment and control markets are from the south and the west, regional differences should not be pose a threat to identification in my study.

Using the AHA data, I restrict the hospitals in my study to non-federal general medical and surgical hospitals since these hospitals compete with each other most directly. My restriction primarily excludes military hospitals and those run by the Department of Veterans Affairs, psychiatric hospitals, rehabilitation hospitals, and hospitals specializing in substance abuse. It also excludes specialty hospitals such as children’s hospitals and eye, ear, and throat hospitals. These hospitals offer more differentiated products than a general medical and surgical hospital or do not compete for the same set of patients and thus compete less directly.

1.3.1 Measuring Technology Adoption

I use the AHA data to measure technologies available at hospitals. I focus on nine technologies that are covered by the AHA survey: magnetic resonance imagers (MRIs), cardiac catheterization labs, open-heart surgery, computed tomography (CT) scanners, extracorporeal shock wave lithotripsy (ESWL), diagnostic radioisotopes, angioplasty, advanced OB units⁴, and single-photon emission computerized tomography (SPECT) scanners. A requirement of the technologies in my study is that the AHA survey had to cover them from 1990 through 2000.

I selected these technologies because their adoption and use has been studied in previous literature (Cutler and Meara 2000, Baker 2001, Baker and Phibbs 2002, Cutler and Huckman 2003, Schmidt-Dengler 2006, Lenzo 2007, Acemoglu and Finkelstein 2008, Cutler, Huckman and Kolstad 2010).⁵ The only notable differences from past technology measures are the following:

---

⁴I define advanced OB units as those that are level 2 or 3 of three possible units. Classification of OB units primarily depends on gestational age and weight of the baby, with higher levels being able to handle riskier cases, ie, smaller babies born earlier.

⁵Of the commonly studied technologies available in the AHA survey in the 1990s, I exclude transplant services, radiation treatments, and PET scanners. I exclude the first two because a change in the survey format in 1994 prevents me from having continuous measures. I exclude PET scanners since they were not widely adopted in the 1990s; fewer than 200 hospitals had a PET scanner nationwide before 1999. The technology with the lowest adoption in my sample was adopted by more than three times as many hospitals.
first, I use “advanced OB unit,” which is closely related but slightly different than the measures of neonatal intensive care units (NICUs) used by Baker and Phibbs (2002) and Cutler and Meara (2000). Advanced OB units handle more complicated deliveries, based on newborn’s weight and gestation age, and NICUs care for newborns with health complications, which often result from premature births. I use the OB unit measure because the AHA survey directly asks hospitals about it, whereas the presence of a NICU would need to be inferred from beds, as explained in (Baker and Phibbs 2002). The OB unit measure corresponds reasonably well with Baker and Phibbs’s definition of NICUs.6 Second, I add, ESWL which is a non-invasive treatment for kidney stones developed in 1980s and since has become the most common treatment for kidney stones. The number of hospitals using ESWL more than doubled during the 1990s.

The survey asks whether these technologies were offered at the hospital or subsidiary, within the system, within a network, or as part of a joint venture. I use the indicator whether the technology was offered at the hospital or subsidiary since it will be the best available measure of the number of technologies in the market. Unfortunately, I only know whether a hospital has at least one of a given technology, but not how many or when it was last upgraded. The AHA survey changed forms in 1994, but the variables I use do not seem affected by this revision.

When examining technology adoption, my dependent variable will either be the number of hospitals in a market with a particular technology or a technology index. Precisely, the number of hospitals in market $i$ and year $t$ with technology $s$ is:

$$Tech_{sit} = \sum_{h \in i} tech_{sth}$$

$tech_{sth}$ is a equal to one if hospital $h$ has reported having technology $s$ sometime before year $t$. I define technology adoption as an absorptive state. By and large, this is true in the data as the technologies I examine are expensive to adopt and not easily un-adopted.7 $h$ indexes hospitals in the relevant sample in market $i$, where the relevant sample is all hospitals, Columbia/HCA hospitals, or hospitals that aren’t part of Columbia/HCA depending on the analysis.

The technology index captures how much technology a market has. It is the sum of $Tech_{sit}$

---

6 Between 1990 and 2000, 95% of hospitals that report having an OB unit of level 1 (the lowest level) do not have a mid-level or high-level NICU. Of the hospitals that report having a level 3 OB unit in a given year, an average of 73% have a high-level NICU. The greatest difference occurs in level 2 OB units. Roughly 52% would not have a NICU by Baker and Phibbs’s definition, 38% have a mid-level NICU, and 10% have a high-level NICU.

7 The base year when constructing for defining technology as an absorptive state is 1990. That is, technology adoption in 1990 is taken directly from the survey, and data from years following 1990 may be modified as a result of how I define technology. The results from using as base year of 1989 are nearly identical.
variable over the nine technologies. Precisely:

\[ TechIndex_{it} = \sum_s \sum_{h \in t} tech_{sth} \] (1.2)

By construction, the resulting technology index will be larger for markets with more hospitals.

1.3.2 Measuring Utilization

I use Medicare claims data from the Centers for Medicare and Medicaid Services to measure utilization of technologies. The Medicare claims data allows me to count the number of people and claims for CT scans, MRIs, SPECT scans, coronary artery bypass graft surgeries (CABG), cardiac catheterization, and percutaneous coronary intervention (PCI) in each market and year. I identify the procedures in the carrier files, which contains claims that physicians have submitted to Medicare in both inpatient and outpatient settings. My dependent variables are the number of procedures and the number of people receiving a procedure.

There are several limitations with the Medicare data. The physician claims only allow me to examine utilization at a market level. I am unable to decompose the results into changes at the merging and rival hospitals. The data only tell me how utilization changes for Medicare beneficiaries. The Medicare population is important, accounting for a large share of hospital and medical expenditures. However, Medicare prices are set administratively, whereas most other patients are privately insured. Many previous studies have found that mergers allow hospitals to increase the price they charge insurers. Such price changes could affect utilization differently than in a setting where prices don’t change. Finally due to data limitations, I only have claims data for years from 1992 through 2000, so I am not able to examine trends in utilization prior to the mergers given that the mergers occur in 1993 and 1994. See the Data Appendix for more details on the Medicare claims data.

1.3.3 Additional Data

In investigating potential mechanisms for my results I use Medicare Cost Reports to measure hospital revenues. All facilities that are certified by Medicare must submit annual reports with their costs, charges, charges by cost center, and select utilization data. Total hospital revenue is the sum of net patient revenue and other additional revenues. It is representative of a facility’s

---

8 The data do not include people enrolled in Medicare managed care plans.
revenue, including skilled nursing facilities. As a supplement, I use AHA data on total admissions.

Finally, I also use population data from the Census and data on the percentage of people enrolled in health maintenance organizations (HMOs) that Laurence Baker constructed from Interstudy records and generously shared.

1.4 Empirical Analysis

1.4.1 Descriptive Statistics

Tables 1.1, 1.2, and 1.3 show characteristics for treatment and control markets in 1992. Table 1.1 shows that treatment markets have larger populations and more hospitals, such that their average Herfindahl-Hirschman Index (HHI) is smaller. The treatment markets have populations that average 1.5 million, almost 29 hospitals per market, and an HHI of 1347. The control markets have 1.2 million people, 19 hospitals per market, and an HHI of 1702. It isn’t surprising that larger markets would have more hospitals and higher total utilization.

To compare these markets in a way that accounts for differences in population, the second panel of Table 1.1 scales general hospital and utilization measures by population. The number of government-owned hospitals, non-profit hospitals, and for-profit hospitals per 100,000 people is very similar across treatment and control markets. Similarly, there aren’t statistically significant differences in general utilization across treatment and control markets. Of the 31 hospital and market characteristics I examine, only two—HHI and total outpatient surgeries—differ between treatment and control markets once I control for population differences.

Table 1.2 shows that the number of hospitals with particular technologies per 100,000 people is quite similar across treatment and control markets in 1992. Additionally, neither the treatment nor control markets seems to have slightly more technology adoption overall. The technology indexes are nearly identical between the two types of markets.

Table 1.3 shows utilization of technologies in treatment and control markets in 1992. Since these measures reflect utilization among the Medicare population, I scale these measures by the number of Medicare enrollees. The table indicates that overall utilization rates are higher in the treatment markets. The sum of the number of people receiving each of the six advanced procedures

---

9 HHI is the sum of the squared shares of each hospital system within the market. The HHI can range between 0 for a perfectly competitive market and 10,000 for a monopoly. I calculate HHI based on the number of beds in each hospital system in 1992.

10 I don’t control for population with measures such as HHI, percentage of people enrolled in an HMO, and population density, which make more sense as is than scaled by population.
is higher in treatment markets, and the total number of claims for the advanced procedures is higher. Examining variation across technologies, though, reveals that the difference in the total utilization rates is primarily driven by differences in the numbers of CT scans and MRIs. Accounting for over 50% of the total utilization measures, CT scans in particular cause treatment and control markets to have different overall utilization rates. These statistics cast some doubt on the comparability of the treatment and control markets, particularly when examining any changes in the use of CT scanners and MRIs. Unfortunately, data limitations prevent me from examining the trends in utilization prior to the mergers, which could otherwise address this concern.

1.4.2 Effect of Mergers on Technological Adoption

My main specification is an event study, which allows me to flexibly compare technology adoption rates prior to the mergers and see changes to adoption after the merger. I use the year of the merger announcement as the relevant event in all my analyses since rival hospitals could start adjusting their strategies then. Further in the 1990s, the FTC only challenged a “handful” of hospital mergers, so rival hospitals had reason to believe that an announced merger would occur (Capps 2005, p. 5, Capps and Dranove, p. 175). The distinction between dating the merger from the announcement and the time at which the merger was complete also does not make much practical difference; all of the mergers I study were completed within five months of being announced.

The event studies specification is:

$$E\{tech_{it} | X_{it}\} = \exp\{ -2 \sum_{k=-5}^{5} \beta_k [YAM_{it} = k] + \sum_{k=0}^{5} \beta_k [YAM_{it} = k] + \gamma_t + \alpha_i + \epsilon_{it}\}$$

When studying technology adoption, $tech_{it}$ is the count of hospitals in market $i$ and year $t$ with a particular technology or $tech_{it}$ is the market technology index. These measures are discrete counts, have skewed distributions, and can be no less than 0. This motivates my use of a pseudo-maximum likelihood Poisson model for the estimation.

$YAM_{it}$ indicates how many years have passed since the merger. If the market is a treatment market, $YAM_{it}$ will equal 1 one year after the merger, 2 two years after the merger, etc. It is 0 the year(s) of the merger announcement and takes on negative values for years preceding the merger. $\mathbb{I}[YAM_{it} = k]$ is an indicator for whether the observation occurs $k$ years after the merger. I normalize $\beta_{-1}$ to be equal to 0 such that all years are compared to the year prior to the merger. $\gamma_t$ are year fixed effects, $\alpha_i$ are market fixed effects, and $\epsilon_{it}$ is the standard error, clustered at the
market level. Observations from the control group are included the regressions and help identify the year fixed effects. The market fixed effects account for overall time-invariant differences across market in technology adoption or use. The technology adoption regressions include data from 1990 through 2000.

The \( \beta_k \)'s are of interest, and I plot them graphically in Figures 1-2. They indicate how the technology index changed in treatment markets. The values on the x axis represent the years after the mergers, or the value of \( YAM_t \). The y axis represents the values of \( \beta_k \). The first graph of Figure 1-2 shows changes in technology adoption for all hospitals in the market. Prior the mergers, hospitals in treatment and control markets did not have statistically different levels of technology adoption, though the coefficients suggest technology adoption was increasing in treatment markets relative to control markets. The pre-trend in this graph is concerning. However, other graphs in this figure and in the robustness checks provide support for the assumption that technology adoption would have been similar in the two groups of markets in absence of the merger. Following the mergers, the treatment markets had higher levels of technology adoption.

The second graph of Figure 1-2 shows technology adoption changes for the sample of HCA hospitals. This graph would appear to suggest that HCA hospitals in treatment markets are far less likely to adopt technology after the mergers. In fact, relative changes in the number of Columbia/HCA hospitals between treatment and control markets is mechanically affecting the technology index. This will become apparent when I control for the number of hospitals in regressions and in one of the robustness checks.

The change in the number of Columbia/HCA hospitals after the mergers is quite small. Starting in 1998, Columbia/HCA sold or spun off a third of hospitals in the aftermath of the allegations the company committed Medicare fraud (Hundley 1999, Eichenwald 1997). In 1998, about 30% the HCA hospitals that were in the sample in 1994 were no longer part of the HCA system, and about 12% had left the data altogether.

The final graph of Figure 1-2 shows technology adoption among the rival hospitals. There almost is no pre-trend in this graph, and rival hospitals in treatment markets significantly increase their technological capabilities after the mergers.

To provide a point estimate summarizing the magnitude of the effect of mergers on technology adoption, I estimate the following pseudo-maximum likelihood Poisson model:

\[
E\{tech_{it}|X_{it}\} = \exp\{\beta_1\text{treat}_i + \beta_2\text{treat}_x \text{ during}_{it} + \beta_3\text{treat}_x \text{ post}_{it} + \gamma_t + \alpha_i + \epsilon_{it}\} \quad (1.4)
\]
As in the event studies, techit is the technology measure of interest, the number of hospitals in the market that have certain technologies or the technology index. $\beta_3$ is the coefficient of interest and the coefficient on the interaction of dummy variables indicating the observation is from a treatment market and a year after the mergers. $\beta_2$ is the coefficient on the interaction of dummies indicating the observation is from a treatment market and from a year in which a merger was announced. When estimating the effect of mergers on technology adoption, I will also estimate a Poisson model with an exposure variable, which accounts for the fact that the upper bound for the value of the dependent variable differs across observations because the markets have different numbers of hospitals.\textsuperscript{11}

Tables 1.4 and 1.5 summarize the results from the difference-in-difference regressions in Equation 1.4, showing the coefficient on the interaction between the treatment group and post-merger period, $\beta_3$. The first column shows the coefficients when all hospitals are included in the sample, the second column includes only the Columbia/HCA hospitals, and the third column contains the results from the sample of rival hospitals. They indicate that the rivals of the merging hospitals adopted more technologies post-merger, leading to an increase in technology at a market level. The coefficient of 0.044 can be interpreted in the following way. Because of the merger, treatment markets have $e^{0.044} = 1.045$ times the technology they would have otherwise. More concretely, the average treatment market had a technology index of 118.4 in 1998. Had the mergers not occurred, I estimate average treatment market would have had a technology index of 113.3. Hence, 5.1 new technologies were adopted in each market as a result of the mergers. This represents about 39% of the increase in these markets' technological capabilities between 1994 and 1998. The results also indicate that the market level increase in technologies is due to a statistically significant increase in the number of hospitals that have diagnostic radioisotopes, ESWL, advanced OB units, and SPECT scanners after the mergers.

The market level results are quite similar when I account for differences in the number of hospitals, as shown in Table 1.5. In the first row of Table 1.5, I also show how the number of hospitals changes. There doesn't appear to be statistically or economically significant change at the market level, although many Columbia/HCA hospitals exit that sample. The rest of Table 1.5 shows the $\beta_3$ estimates from Equation 1.4, where I control for the number of hospitals through

\textsuperscript{11}A market with 10 hospitals cannot possibly have more than 10 hospitals with a certain technology whereas a market with 20 hospitals can. Failing to control for the different upper bounds could produce inaccurate estimates. Consider market A with 5 MRIs and market B with 7 MRIs. Market B would seem to have more technology. Yet if market A has 10 hospitals and market B has 20, market A in fact has a higher rate of MRI adoption. However, since the number of hospitals in endogenous to the market, I also present results without the exposure variable.
the exposure variable. The coefficient of 0.062 on the technology index is similar to the coefficient estimated without the exposure variable. It implies that about 7.1 new technologies were adopted in each treatment market as a result of the mergers.

Moving to the decomposition of results between Columbia/HCA and their rivals, Table 1.4 suggests that there is a sharp decline in the technological capabilities of Columbia/HCA hospitals. However, comparing the second column of Table 1.4 to that in Table 1.5 shows that this decline is due to changes in the number of hospitals in the sample. The results in Table 1.5 suggest that the merging hospitals didn’t meaningfully change their technological capabilities in treatment markets.

Increases in the technological capabilities of the rival hospitals are responsible for the increase in technology at the market level. The final column of Tables 1.4 and 1.5 shows the technology changes among the rival hospitals. The positive coefficients on all the technologies suggest that hospitals competing with Columbia/HCA adopt more technologies after the mergers. This increase is statistically significant for catheterization labs (after controlling for the number of hospitals), CT scanners, diagnostic radioisotopes, advanced OB units, and SPECT scanners, leading to a statistically significant increase in the technology index.

1.4.3 Relationship between Technology Adoption and Utilization

Before presenting the results on the effect of mergers on technology utilization, it is useful to examine the relationship between technology levels and utilization at a market level. The analysis presented in this section doesn’t establish the causal relationship between technology adoption and utilization, but rather provides context for comparing the main technology and utilization results. Higher technology levels, such as those post-merger, could be associated with greater use on the margin if ease of access to a technology affects doctors’ incentives to recommend a procedure or patients’ incentives to agree to a procedure. Alternatively, less-than-complete adoption may not limit utilization of technologies that have been widely adopted. For instance, if most physicians who recommend and perform a certain procedure already have access to the relevant technology at one hospital or medical center at which they work, having that technology available at multiple centers may not be associated with higher utilization levels. In that case, hospitals might adopt more technology in response to competitive changes, but on the margin, more technology might not be associated with greater use.

To examine the relationship between technology levels and utilization, I compare the adoption of six technologies and and use of corresponding procedures—CT scans, MRIs, SPECT scans, open-
heart surgery facilities and CABG, catheterization labs and cardiac catheterization, angioplasty and PCI. Figure 1-3 contains two plots of technology levels versus utilization across markets and years. The first graph contains the residuals from the following pooled regressions:

\[
\log(y_{its}) = \gamma_{ts} + \psi_s + \epsilon_{its}
\]  

(1.5)

I use pooled regressions to normalize technology and procedure levels and use across time and technology. \(\log(y_{its})\) is the log of the number of people in market \(i\) and year \(t\) receiving procedure \(s\) when normalizing utilization and the log of the number of hospitals with the corresponding technology \(s\) when normalizing the technology levels. \(\gamma_{ts}\) are year fixed effects, which can vary across technologies or procedures. \(\psi_s\) are technology or procedure fixed effects. \(\epsilon_{its}\) is the residual and is clustered at a market level. The residual indicates the amount of utilization and technology that is unexplained by year-to-year variation or average differences across technologies or procedures. The first plot of Figure 1-3 shows a strong positive correlation between technology levels and utilization.

The second graph of Figure 1-3 shows the relationship between technology levels and utilization controlling for population differences across markets. Population controls are important since larger markets will logically have higher technology levels and use. Specifically, the graph shows the residuals from Equation 1.5 with two additional controls: market population and Medicare population. Like the year fixed effects, the effect of these controls can vary by technology.\(^{12}\) The graph shows that after accounting for differences in population, there is almost no correlation between relative technology levels and use.

The point estimates from regressions, shown in Table 1.6, support these findings. Panel A in Table 1.6 shows the results from regressions where the procedures and technologies are summed:

\[
\log(\text{util}_{it}) = \beta \log(\text{tech}_{it}) + X_{it} + \epsilon_{it}
\]  

(1.6)

\(\log(\text{util}_{it})\) is the log of the sum of people receiving each advanced procedure or of all claims, and \(\log(\text{tech}_{it})\) is the log of a technology index that only includes the six relevant technologies. \(X_{it}\) represents controls, which include year and market fixed effects and population controls (log population and log Medicare population) depending on the specification. \(\epsilon_{it}\) is clustered at the

\(^{12}\) I control for both market population and Medicare levels since both are likely to be important for technology levels. The utilization measures only reflect utilization among Medicare enrollees, so I also want to account for variation in the Medicare population. Both controls are in logs. Medicare population varies across years; market population, because it comes from the Census, does not.
market level.

The results show a strong correlation between technology and utilization when only year fixed effects are included among controls. Market fixed effects, which would capture time-invariant differences in markets greatly diminishes the correlation. When accounting for differences in population—with or without market fixed effects—the relationship between technology levels and utilization is small and statistically insignificant.

By using the sum of utilization across procedure types, these regressions put relatively more weight on the utilization of highly-used procedures such as CT scans. Panel B of Table 1.6 shows the results from pooled regressions, where the dependent variable is the utilization of a certain procedure and the independent variable is the availability of the relevant technology in the market. The set of controls can vary by technology type and now also include technology type fixed effects. Observations are weighted by the inverse of the average number of procedures for that technology. This is my preferred specification.

The results in Panel B are quite similar to those in Panel A. The correlation between technology levels and use is greatly diminished once controlling for time-invariant differences across markets and/or population. The first four columns of Panel B suggests there is a statistically significant relationship between the number of people receiving procedures and the corresponding levels of technology within a market, even after controlling for population, but that population differences explain most cross-market variation in use. The slight difference in the two panels suggest that the relationship between utilization and technology levels varies for different types of technologies. Tables A.1 and A.2 show the results from regressions where each technology is separately evaluated using Equation 1.6. They show more people receive SPECT scans and more claims for CABG procedures in markets with more of those technologies, even after controlling for population differences and time-invariant market differences.

1.4.4 Effect of Mergers on Technology Utilization

Given the evidence above that higher technology levels don’t correspond with greater use after controlling for differences in population, it wouldn’t be surprising if the marginal increase in technological levels post-merger did not correspond with a post-merger increase in utilization. However, mergers could affect utilization independent of changes in technology levels. For instance, if hospitals are liquidity constrained, they might perform more procedures that are reimbursed at high levels. If prices increase following a merger, the hospital may have less incentive to perform highly-
reimbursed procedures on the margin and utilization could fall.

To determine how mergers affect the utilization of technologies, I conduct event studies, as above. My main results show two graphs, shown in Figure 1-4: 1) where the dependent variable is the sum of people receiving the six advanced procedures and the regression is the same as in Equation 1.3, and 2) a pooled regression where the dependent variable is the number of people receiving that particular procedure. The pooled regression includes procedure dummies and allows the year and market fixed effects to vary by technology. Unfortunately, due to data limitations, I cannot access pre-trends in these graphs, nor can I decompose the results in the effects at Columbia/HCA hospitals and rivals.

The first graph in Figure 1-4 suggests relatively higher utilization levels in the treatment markets after the mergers. However, since the dependent variable is the sum of people receiving procedures, this specification gives more weight to highly-used procedures, such as CT scans. The second figure shows the results from the pooled regression. It suggests utilization remained relatively constant post-merger. Figure A-1 shows the equivalent graphs when the number of claims, rather than people, is the dependent variable. In these figures, as in Figure 1-4, utilization appears to increase some when evaluating the total number of claims, but the pooled regressions suggest utilization did not change.

Figures A-2 through A-7 plot the event studies for the adoption and use of individual technologies. More people seem to receive CT scans after the merger, although without a pre-period, the result isn’t certain. This would have greatly contributed to the apparent market-level increase in sum of people receiving advanced procedures. It is also interesting to note that more hospitals have SPECT scanners after the mergers, and the number of people receiving SPECT scans increases. The pre-trend on the adoption of SPECT scanners is worrying, and I will address this when discussing the robustness of my results. For completeness, I show the event studies of the adoption of diagnostic radioisotopes, extracorporeal shock wave lithotripsy, and advanced OB units in Figure A-8. The pre-trend on diagnostic radioisotopes suggests that hospitals in treatment markets were increasingly adopting them prior to the mergers. Again, I will address this pre-trend in the following section.

To summarize the effect of mergers on technology utilization, I estimate the pseudo-maximum likelihood Poisson model in Equation 1.4, but with a utilization measure as the dependent variable. Table 1.7 shows the results. The first two columns present the results when the outcome variable is

\[ \text{As with the main event studies, utilization is the number of people receiving that procedure in these figures.} \]
the number of people receiving the certain procedures. The second two columns display the results when the dependent variable is the total number of claims. The results when the total number of people receiving procedures or total number of claims for all procedures is the dependent variable are in the first row. As discussed above, this measure puts relatively more weight on changes in highly-used procedures. My preferred estimate, from pooled regressions, is in the second row.\textsuperscript{14}

The regressions in the second and fourth columns include demographic controls for the Medicare population—the log of the number of Medicare beneficiaries and the percentage of people in different age groups or of different genders and races—could affect the types of care patients receive. Since treatment markets are larger markets, it’s possible that demographics differ across markets and would be important to control for in regressions.

The summary measures in Table 1.7 suggest that utilization does not change at a market level after the mergers. Examining the individual procedures, there is some evidence that more people receive CT and SPECT scans after the merger, but this does not translate into higher utilization at a market level.

The results involving SPECT scanners suggest the relationship between adoption and use may be heterogenous across technologies. With certain technologies, higher adoption rates may be associated with more use on the margin. Markets with more hospitals that had SPECT scanners performed SPECT scans on more people, even when controlling for population differences across markets. Additionally, the results indicate that after the mergers, hospitals adopted more SPECT scanners and more people received SPECT scans.

1.5 Robustness Checks

I test the robustness of the results to a variety of alternative analyses.

1.5.1 Alternative Specifications and Definitions of the Technology Index

Given the pre-trend in the adoption of diagnostic radioisotopes and SPECT scanners in Figure A-4 and A-8, I examine whether the results hold if I eliminate those two technologies from the technology index. The concern is that the increases in diagnostic radioisotopes and SPECT scanners drive

\textsuperscript{14}The controls for the pooled specification include procedure fixed effects. Unlike the event study, the market fixed effects, year fixed effects, and demographic controls have constant effects across procedure types. These particular regressions would not converge when the controls were allowed to vary. In robustness checks, the utilization results are very similar whether or not the controls are allowed to vary by procedure type, suggesting the additional flexibility does not alter the results here.
the substantial increase in technology, but that unobservable market characteristics were causing treatment markets to adopt these technologies at higher rates irrespective of the mergers. To test this, I construct a “short technology index” without those technologies and estimate the event studies model and a point estimate using Equations 1.3 and 1.4.

Simultaneously, I test whether my results to alternative specifications of the technology index. In one analysis, I define the dependent variable as the average technology index per hospital, which allows me to control for the number of hospitals in the market in the dependent variable. This is preferable to including the number of hospitals as an independent variable since it is endogenous. I also test whether my results are robust to a specification where the dependent variable is the log of the technology index since log and poisson models are both sensible for variables with skewed distributions.

Figure A-9 shows the event studies when I use the shortened technology index in the left column. The right column shows the event studies when the average technology index per hospital is the dependent variable. Both columns indicate a similar pattern as shown in Figure 1-2. The technological capabilities of hospitals increase at a market level after the merger, and the rivals of Columbia/HCA are the hospitals driving this result. Additionally, these figures mitigate concerns that hospitals in treatment markets were increasingly adopting technologies prior to mergers. As in Figure 1-2, the event studies with short technology index would seem to indicate that Columbia/HCA’s technology levels decline post-merger. The average technology index event studies shows that Columbia/HCA hospitals have similar technological capabilities in treatment and control markets once I’ve accounted for the number of hospitals.

Table A.3 shows the regression results from these alternative specifications. The result that technology increases at a market level post-merger is not statistically significant once diagnostic radioisotopes and SPECT scanners are excluded from the technology index, but is if I also control for the number of hospitals in the market. The increase in technology at the rival hospitals is still statistically significant. When the dependent variable is the average technology index per hospitals, the market level results become statistically insignificant, and the results for the rival hospitals remain statistically significant. Neither result is significantly different from 0 if the dependent variable is the log of the technology index. Hence the initial results are reasonably robust to excluding diagnostic radioisotopes and SPECT scanners from the technology index and alleviate concerns about pre-trends. However, the results are more sensitive to the specification.

I also evaluate whether the utilization results were robust to specifying the dependent variables
in logs. Table A.4 presents the results. The summary measures—the results from adding the utilization measures across procedures or from pooled regressions—remain unchanged. The mergers do not seem to affect utilization. The results that more people receive CT and SPECT scans after the merger is also robust to this change in specification.

1.5.2 Alternative Samples

I also check whether my results hold when I restrict the hospitals in my sample to those that were general acute hospitals from 1990 through 2000 to address the concern that the increase in technology is driven by technologically-advanced hospitals entering the market and/or less technologically capable hospitals exiting. Table A.5 shows the results.\textsuperscript{15} In this sample, I allow hospitals to move between being Columbia/HCA hospitals and rival hospitals, provided they remain in the market for all years. At a market level, technology increases, and the coefficients are similar to those estimated when the sample isn’t balanced. However, the market-level results are more reliant on the increase in diagnostic radioisotopes and SPECT scanners. The increase in technological capabilities among the rivals after the merger remains statistically significant.

Finally, I eliminate treatment markets that were defined as such because HCA and Healthtrust operated in them in 1992. When HCA was struggling in the mid-1980s, it spun off about 100 hospitals into a new company, Healthtrust (Lutz and Gee 1998, p. 27). Healthtrust had been its own company for seven years when Columbia/HCA acquired it. Nonetheless, how consolidation affected markets with just HCA and Healthtrust may have differed from the effects in other markets. HCA essentially picked which hospitals would be part of Healthtrust. It may have spun-off hospitals that wouldn’t compete with the hospitals that remained part of HCA. The future acquisition then may not have affected competition much in these markets. Hence it would be troubling if the results were driven by technology changes in these markets.

I reestimate Equation 1.4, eliminating the six markets where HCA and Healthtrust were the only two merging chains operating hospitals. If a third chain also operated a hospital in that market, the market became more consolidated regardless of HCA and Healthtrust’s relationship and thus remains in the sample. The results for technology adoption are in Table A.6, and the utilization results are in Table A.7. The results are virtually the same as the original results; technology adoption increases, but utilization does not. The only notable difference is that the magnitude

\textsuperscript{15}I only show the technology adoption results for this robustness check because the utilization measure are all at a market level, preventing me from constructing a balanced panel.
of the effect on utilization of SPECT scans is slightly lower than in the original results, and the increase in the number of people receiving SPECT scans is no longer statistically significant.

1.6 Possible Mechanisms

The finding that hospitals increase their technological capabilities when competitors merge raises the question of why this happens. While I am not able to offer a complete explanation, I investigate several potential mechanisms in this section.

1.6.1 Competing with HCA

One might reasonably think that rivals increased their technological capabilities after the merger because they now had to compete a large, aggressive chain. Following the mergers, Columbia/HCA had over 300 hospitals. It represented over 40% of all for-profit hospitals in the markets I study, where for-profit hospitals comprised about 25% of general hospitals. The company was listed on the New York Stock Exchange, and as of 1996, “[had] targeted and achieved a formidable corporate goal of 20 percent gross return on revenues” (Kuttner 1996, p 364). Columbia/HCA’s annual reports discuss how the company strived to provide all aspects of care. As Kuttner (1996) notes, if it had a large enough market share, insurers would have no choice but to contract with them. The company also waged an advertising campaign to “discredit” not-for-profit hospitals (Kuttner 1996, p 365). In short, Columbia/HCA was an aggressive competitor and had deep pockets. Hospitals competing with Columbia/HCA might have invested in technology either to make themselves stronger competitors or attempt to deter Columbia/HCA from further investing in their particular markets.

I am able to investigate this hypothesis because many of my control markets contain hospitals that are part of the merging chains. The presence of a large, aggressive chain should have affected rivals in the control markets similarly. To test this, I estimate the following Poisson regression using pseudo-maximum likelihood:

\[
E\{tech_{it}|X_{it}\} = \exp\{\beta_1 M_{2i} + \beta_2 M_{2i} \times \text{during}_{it} + \beta_3 M_{2i} \times \text{post}_{it} ... + \beta_4 M_{1i} + \beta_5 M_{1i} \times \text{during}_{it} + \beta_6 M_{1i} \times \text{post}_{it} + \gamma_t + \alpha_i + \epsilon_{it}\} \tag{1.7}
\]

M2 is a dummy that indicates that two or more of the merging chains operated hospitals in market
in 1992. These are the original treatment markets. M1 indicates that one of the merging chains operated hospitals in market $i$ in 1992. For the M1 markets, the “during” and “post” dummies are defined as when Columbia/HCA became a large chain or entered the market. This was 1993 for markets in which HCA, Columbia, and Galen operated hospitals and 1994 for markets in which Healthtrust and Epic operated hospitals. \( \beta_3 \) and \( \beta_6 \) are the coefficients of interest. If the theory about hospitals investing to compete with Columbia/HCA is accurate, both \( \beta_3 \) and \( \beta_6 \) should be positive and statistically significant.

Table A.8 shows the results of the regression when all hospitals in the market are included in the sample. The first column shows the original market level results for comparison. Column 2 shows \( \beta_3 \) for the various technologies, and column 3 shows \( \beta_6 \). The values of \( \beta_3 \) are very similar to the original results. When the technology index is the outcome variable, \( \beta_6 \) is much smaller than \( \beta_3 \) and statistically insignificant, indicating the entry or emergence of the large chain induced very little technology adoption in markets without consolidation. \( \beta_6 \) is statistically significant for four individual technologies, but adoption of two of these technologies declines after the emergence of Columbia/HCA. Hence, mergers, not the presence of a large chain, spurred rival hospitals to adopt technologies.

### 1.6.2 Temporary Increase in Revenues and Admissions

I examine the relationship between revenues, admissions, and technology adoption because it could shed light on why hospitals adopt more technologies after the mergers. The evidence in this section is merely suggestive that temporary increases in revenues and admissions contributed to greater technology adoption. One mechanism through which these could affect technology adoption is that after the merger, patients may have been more likely to seek care at the rival hospitals. With higher admissions, hospitals had the scale to render adoption sensible. Admissions might have increased at rival hospitals for a number of reasons. Insurers may have temporarily dropped the new system from the network if it charged higher prices. Merging hospitals might have been making merger-specific investments, which temporarily disrupted care. For instance, they may have transferred services or doctors across hospitals now that they could coordinate care. Disruption or construction from this process could temporarily lead patients to seek care at other hospitals.

An alternative mechanism is that the mergers increased the liquidity of the rival hospitals, enabling investment. Higher admissions will increase revenues, \textit{ceteris paribus}, and hospitals may use higher revenues to invest in technologies. Since most hospitals are not-for-profit or community
hospitals, higher revenues would not be returned to shareholders.¹⁶

Finally, the hospitals competing with Columbia/HCA may have adopted more technology with the precise aim of attracting more patients and increasing revenues. The investments may have been in anticipation of Columbia/HCA hospitals making merger-specific improvements. The competitors’ efforts may have been only temporarily successful as patients returned to Columbia/HCA hospitals after they completed merger-specific improvements.

I am unable to disentangle these possible mechanisms because I cannot separately identify changes in admissions, revenue, and technology. To illustrate the relationship among admissions, revenues, and technology, though, I first show that there seems to be a temporary increase in revenues and admissions among rival hospitals following the mergers. Precisely, I estimate the event studies model, Equation 1.3, using the log of average net patient revenues, log of average hospital revenues (which I’ll also refer to as average revenues), and log of average total admissions as the dependent variables. As seen in Figure 1-5, there is a one-year spike in the average net patient revenue and total revenue. Average admissions per hospital also increase slightly for the two years following the merger, particularly in comparison to the years preceding the mergers.¹⁷

These temporary increases in revenues and admissions are notable, but the graphs are far from conclusive that the mergers caused these increases. Data anomalies could also be responsible for the one-year increases. However, the fact that the temporary increase in admissions and revenues is correlated with the predicted effect of the merger provides support for the theory that the mergers caused these temporary changes. Figure 1-6 shows that the increases in revenues and admissions are positively correlated with the predicted increase in HHI due to the merger. The predicted change in HHI is the difference between the HHI in 1992 when I include all hospitals belonging to the merging systems as one entity in the market and when I calculate the market’s HHI as is. Each observation in the graph represents the average change hospitals in a treatment market experienced. The change in revenues is the difference between the log revenues one year after the merger and the average log revenues for the three years preceding the merger. Change in admissions is calculated the same way. For completeness, I show the change in technology among the rival hospitals is also correlated with the predicted change in HHI. The change in the technology per

³⁶If hospitals were able to negotiate higher prices from insurers after the merger, increased prices could have also contributed to greater liquidity.

³⁷In point estimates, the increases in revenues, patient revenues, and admissions is statistically insignificant. The lack of statistical significance is unfortunate, but perhaps unsurprising given that I only have 45 treatment markets and 65 control markets and any increase seems temporary. I may have too few observations to obtain statistical significance for a short-lived change. Technology adoption, on the other hand, is a more permanent change to the hospital. The years of data after the merger may provide the observations needed for statistical significance.
hospital is the difference between the average of the first two years after the merger and the average for the three years preceding the merger. I use the two years after the merger for the technology index because several event studies suggest the rival hospitals’ technological capabilities steadily increase over these two years. Figure 1-6 shows that markets where the mergers were likely to have greater effects on competitive dynamics also had larger temporary increases in revenue and admissions among rival hospitals as well as larger changes in technological capabilities.

Finally, I show in Figure 1-7 that the increase in admissions is positively correlated with changes in revenues, and both are positively correlated with the average technology increase in treatment markets. In particular, markets with greater increases in admissions at rival hospitals closely correspond to markets with greater technological adoption among the rival hospitals. Taken together the correlations suggest that the mergers precipitated changes in admissions, revenues, and technology adoption, such that technological capabilities of rival hospitals increased. Unfortunately, without more information, it is unclear why revenues and admissions only increased temporarily, and which mechanism best explains why rival hospitals adopted more technology.

1.7 Conclusion

This study investigates how mergers affect the adoption of technology and their use. The chains that merged to form Columbia/HCA provide a useful empirical setting in which to study this question. These mergers involved the consolidation of national chains, mitigating concerns about the comparability of markets that experience mergers and those that did not. I find that hospitals that competed with Columbia/HCA hospitals adopted technologies after their markets became more concentrated, increasing the technological capabilities of the market. Use technologically-advanced procedures did not increase at a market level after the mergers.

Despite the robustness of these findings, the evidence on the relationship between technology adoption and utilization is mixed. Of the technologies that increase at a market level—diagnostic radioisotopes, advanced OB units, and SPECT scanners—I am only able to investigate the utilization of SPECT scanners. Following the mergers and more people received SPECT scans, suggesting that once SPECT scanners were more widely available, they were used more frequently. However, higher levels of CT scanners, MRIs, catheterization labs, open-heart surgery facilities, and angioplasty at a market level are not associated with greater use of corresponding procedures among Medicare beneficiaries. It is reasonable that the relationship between adoption and use could vary
across different technologies or technologies in different phases of adoption.

The increase in technology is correlated with temporary increases in admissions and revenues at rival hospitals, though the exact mechanism by which these affected technology adoption is uncertain. While future research could shed light on the most plausible reason, the contribution of this paper is to provide evidence that competitive changes affect hospitals’ decisions to adopt technology and that mergers can result in changes in technology that persist for years.
Markets are HRRs, as defined by the Dartmouth Atlas. Markets with merging chains and two or more for-profit hospitals are determined using AHA Survey data.
These figures show how the technology index changes in treatment markets before and after the merger. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The specification includes year and market fixed effects, and standard errors are clustered at the market level. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1990 through 2000. The three subgraphs show the results from three samples: all hospitals, just Columbia/HCA hospitals, and just rival hospitals.
These figures show the correlation between technology levels and utilization. Both technology levels and utilization are in logs. Utilization is the number of people receiving a certain procedure. The estimates in the first graph control for year-to-year differences in technology levels and use, which can vary by type of technology, and average differences across technology/procedure type. The estimates in the second graph also control for market population and Medicare population. Both are in logs, and their effect can vary by technology. Market population doesn’t vary over years since it is taken from the Census. Medicare population varies over time. Data is from 1992 through 2000.
These figures show how the utilization changes in treatment markets before and after the merger. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The first graph shows how the sum of people receiving the six procedures changes, and the second graph shows the results from a pooled regression. Both specifications includes year and market fixed effects, and standard errors are clustered at the market level. The pooled regression includes procedure fixed effects, and the effect of the controls varies by type of procedure. Observations are weighted by the inverse of the average number of procedures for that technology. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1992 through 2000.
These figures show how revenues and admissions at rival hospitals change in treatment markets before and after the merger. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The dependent variables are in logs. The specification includes year and market fixed effects, and standard errors are clustered at the market level. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1990 through 2000.
These graphs show the relationship between the predicted change in HHI and the increase in admissions, revenue, and technology at rival hospitals. The predicted change in HHI is the difference between the HHIs in 1992 when all hospitals belonging to the merging systems are considered one entity in the market and the market's actual HHI in 1992. The change in revenues and admissions is the difference between the log one year after the merger and the average log revenues or admissions for the three years preceding the merger. Change in technology represents the average change among rivals between the three years prior to the mergers and two years following the merger.
Figure 1-7: Relationship between Admissions, Revenues, and Technology at Rival Hospitals Immediately Following the Mergers

These graphs show the relationships between the temporary increases in admissions and revenues as well as the change in technology, all at rival hospitals. See note for Figure 1-6 for admissions, revenue, and technology calculations. Outliers excluded include observations where the log change in revenue exceed 0.7 or was less than 0 when revenues are shown in the plots. In the Technology Change vs. Admissions Change graph, outliers include observations with change in the technology index greater than 2 and observations with changes in log admissions less than -2.
### Table 1.1: Market Summary Statistics for Treatment and Control Markets, 1992

<table>
<thead>
<tr>
<th></th>
<th>Non-Merger Markets</th>
<th>Merger Markets</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Populations (100,000s)</td>
<td>12.06</td>
<td>15.39</td>
<td>3.33</td>
<td>0.22</td>
</tr>
<tr>
<td>Pop. Density</td>
<td>358.02</td>
<td>148.79</td>
<td>-209.24</td>
<td>0.35</td>
</tr>
<tr>
<td>HHI</td>
<td>1702.28</td>
<td>1346.77</td>
<td>-355.51</td>
<td>0.05</td>
</tr>
<tr>
<td># Neighboring Hospitals</td>
<td>18.91</td>
<td>28.69</td>
<td>9.78</td>
<td>0.01</td>
</tr>
</tbody>
</table>

**Panel B: Scaled By Population**

<table>
<thead>
<tr>
<th></th>
<th>Non-Merger Markets</th>
<th>Merger Markets</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td># govt owned hosp</td>
<td>0.81</td>
<td>0.81</td>
<td>0.00</td>
<td>1.00</td>
</tr>
<tr>
<td># not-for-profit hosp</td>
<td>0.87</td>
<td>0.84</td>
<td>-0.03</td>
<td>0.76</td>
</tr>
<tr>
<td># for-profit hosp in sample</td>
<td>0.55</td>
<td>0.66</td>
<td>0.10</td>
<td>0.17</td>
</tr>
<tr>
<td># w/ Residency Prog</td>
<td>0.22</td>
<td>0.19</td>
<td>-0.03</td>
<td>0.32</td>
</tr>
<tr>
<td>Hospital Admissions</td>
<td>10593</td>
<td>11073</td>
<td>480</td>
<td>0.33</td>
</tr>
<tr>
<td>Inpatient Days</td>
<td>65049</td>
<td>68087</td>
<td>3037</td>
<td>0.46</td>
</tr>
<tr>
<td>Hospital Beds</td>
<td>299</td>
<td>323</td>
<td>24</td>
<td>0.21</td>
</tr>
<tr>
<td>Total Surgeries</td>
<td>7545</td>
<td>8079</td>
<td>535</td>
<td>0.14</td>
</tr>
<tr>
<td>Total Outpatient Visits</td>
<td>106374</td>
<td>103323</td>
<td>-3051</td>
<td>0.68</td>
</tr>
<tr>
<td>Births</td>
<td>1357</td>
<td>1276</td>
<td>-81</td>
<td>0.12</td>
</tr>
<tr>
<td>Medicare Discharges</td>
<td>3914</td>
<td>4245</td>
<td>331</td>
<td>0.21</td>
</tr>
<tr>
<td>Medicaid Discharges</td>
<td>1833</td>
<td>1772</td>
<td>-61</td>
<td>0.66</td>
</tr>
<tr>
<td>ED Outpatient Visits</td>
<td>33149</td>
<td>34095</td>
<td>946</td>
<td>0.63</td>
</tr>
<tr>
<td>Total Expenditures</td>
<td>75732864</td>
<td>80296208</td>
<td>4563341</td>
<td>0.28</td>
</tr>
</tbody>
</table>

**Panel C: Additional Characteristics**

<table>
<thead>
<tr>
<th></th>
<th>Non-Merger Markets</th>
<th>Merger Markets</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>% People in HMOs</td>
<td>0.11</td>
<td>0.10</td>
<td>-0.01</td>
<td>0.51</td>
</tr>
<tr>
<td>N</td>
<td>65</td>
<td>45</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This table compares general characteristics of treatment (merger) and control (non-merger) hospital markets. The third column reports the difference between the merger and non-merger markets, and the fourth column shows the p-value on the difference based on a t statistic. HHI is calculated based on the number of beds in the hospitals. Variables scaled by population represent the variable per 100,000 people, averaged across markets. See text for more detail on the data.
Table 1.2: Market Technology Levels, 1992

<table>
<thead>
<tr>
<th></th>
<th>Control</th>
<th>Treatment</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Scaled By Population</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average Hosp Tech Index</td>
<td>6.68</td>
<td>6.65</td>
<td>-0.02</td>
<td>0.96</td>
</tr>
<tr>
<td># Hosp with MRI</td>
<td>0.56</td>
<td>0.59</td>
<td>0.03</td>
<td>0.58</td>
</tr>
<tr>
<td># Hosp with Cath Lab</td>
<td>0.69</td>
<td>0.68</td>
<td>-0.01</td>
<td>0.85</td>
</tr>
<tr>
<td># Hosp with OH Surg</td>
<td>0.38</td>
<td>0.38</td>
<td>0.00</td>
<td>0.92</td>
</tr>
<tr>
<td># Hosp with CT scan</td>
<td>1.69</td>
<td>1.73</td>
<td>0.04</td>
<td>0.79</td>
</tr>
<tr>
<td># Hosp with Diag. Radioisotopes</td>
<td>1.44</td>
<td>1.40</td>
<td>-0.04</td>
<td>0.74</td>
</tr>
<tr>
<td># Hosp with ESWL</td>
<td>0.25</td>
<td>0.23</td>
<td>-0.03</td>
<td>0.44</td>
</tr>
<tr>
<td># Hosp with Angioplasty</td>
<td>0.48</td>
<td>0.47</td>
<td>-0.01</td>
<td>0.81</td>
</tr>
<tr>
<td># Hosp with advanced OB unit</td>
<td>0.64</td>
<td>0.62</td>
<td>-0.02</td>
<td>0.73</td>
</tr>
<tr>
<td># Hosp with SPECT</td>
<td>0.55</td>
<td>0.56</td>
<td>0.01</td>
<td>0.82</td>
</tr>
</tbody>
</table>

This table compares technology levels, per 100,000 people, between treatment and control markets in 1992. The third column reports the difference between the merger and non-merger markets, and the fourth column shows the p-value on the difference based on a t statistic. See text for more detail on the data.

Table 1.3: Market Utilization Levels, 1992

<table>
<thead>
<tr>
<th>Scaled By Medicare Enrollees</th>
<th>Control</th>
<th>Treatment</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sum of People Receiving Advanced Procedures</td>
<td>183.66</td>
<td>194.07</td>
<td>10.40</td>
<td>0.05</td>
</tr>
<tr>
<td>People Receiving CT Scans</td>
<td>108.70</td>
<td>115.93</td>
<td>7.23</td>
<td>0.02</td>
</tr>
<tr>
<td>People Receiving MRIs</td>
<td>28.98</td>
<td>31.64</td>
<td>2.66</td>
<td>0.12</td>
</tr>
<tr>
<td>People Receiving SPECT Scans</td>
<td>16.52</td>
<td>17.14</td>
<td>0.62</td>
<td>0.69</td>
</tr>
<tr>
<td>People Receiving CABG</td>
<td>5.02</td>
<td>5.14</td>
<td>0.12</td>
<td>0.72</td>
</tr>
<tr>
<td>People Receiving Cardiac Catheterization</td>
<td>19.97</td>
<td>20.04</td>
<td>0.07</td>
<td>0.95</td>
</tr>
<tr>
<td>People Receiving PCI</td>
<td>4.47</td>
<td>4.18</td>
<td>-0.29</td>
<td>0.37</td>
</tr>
<tr>
<td>Claims for Advanced Procedures</td>
<td>285.75</td>
<td>327.28</td>
<td>41.53</td>
<td>0.00</td>
</tr>
<tr>
<td>CT Scan Claims</td>
<td>195.14</td>
<td>226.97</td>
<td>31.83</td>
<td>0.00</td>
</tr>
<tr>
<td>MRI Claims</td>
<td>36.26</td>
<td>43.15</td>
<td>6.88</td>
<td>0.01</td>
</tr>
<tr>
<td>SPECT Scan Claims</td>
<td>19.17</td>
<td>20.60</td>
<td>1.43</td>
<td>0.49</td>
</tr>
<tr>
<td>CABG Claims</td>
<td>5.87</td>
<td>6.42</td>
<td>0.55</td>
<td>0.26</td>
</tr>
<tr>
<td>Cardiac Catheterization Claims</td>
<td>22.98</td>
<td>24.01</td>
<td>1.03</td>
<td>0.41</td>
</tr>
<tr>
<td>PCI Claims</td>
<td>6.33</td>
<td>6.15</td>
<td>-0.19</td>
<td>0.77</td>
</tr>
</tbody>
</table>

This table compares utilization levels, per 1000 Medicare enrollees, between treatment and control markets in 1992. Data is scaled by Medicare enrollees rather than population since markets with relatively more Medicare enrollees for their size would appear to have higher utilization rates if scaled by population. The third column reports the difference between the merger and non-merger markets, and the fourth column shows the p-value on the difference based on a t statistic. See text for more detail on the data.
Table 1.4: Technology Adoption in Markets with Mergers

<table>
<thead>
<tr>
<th></th>
<th>Full Market</th>
<th>Just Merging Chains</th>
<th>Just Rivals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Market Tech Index</td>
<td>0.044</td>
<td>-0.263</td>
<td>0.070</td>
</tr>
<tr>
<td></td>
<td>(0.018)**</td>
<td>(0.108)**</td>
<td>(0.021)***</td>
</tr>
<tr>
<td># of Hosp with MRI</td>
<td>-0.020</td>
<td>-0.313</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.178)*</td>
<td>(0.059)</td>
</tr>
<tr>
<td># of Hosp with Cath Lab</td>
<td>0.016</td>
<td>-0.283</td>
<td>0.042</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.146)*</td>
<td>(0.033)</td>
</tr>
<tr>
<td># of Hosp with Open-Heart Surg</td>
<td>0.019</td>
<td>-0.175</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.189)</td>
<td>(0.038)</td>
</tr>
<tr>
<td># of Hosp with Diag Radioisotopes</td>
<td>0.066</td>
<td>-0.196</td>
<td>0.097</td>
</tr>
<tr>
<td></td>
<td>(0.022)***</td>
<td>(0.085)**</td>
<td>(0.026)***</td>
</tr>
<tr>
<td># of Hosp with ESWL</td>
<td>0.124</td>
<td>-0.108</td>
<td>0.108</td>
</tr>
<tr>
<td></td>
<td>(0.068)*</td>
<td>(0.250)</td>
<td>(0.078)</td>
</tr>
<tr>
<td># of Hosp with Angioplasty</td>
<td>0.004</td>
<td>-0.404</td>
<td>0.038</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.173)**</td>
<td>(0.041)</td>
</tr>
<tr>
<td># of Hosp with CT scan</td>
<td>0.012</td>
<td>-0.331</td>
<td>0.053</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.094)***</td>
<td>(0.028)*</td>
</tr>
<tr>
<td># of Hosp with Adv. OB unit</td>
<td>0.063</td>
<td>-0.326</td>
<td>0.101</td>
</tr>
<tr>
<td></td>
<td>(0.029)**</td>
<td>(0.159)**</td>
<td>(0.031)***</td>
</tr>
<tr>
<td># of Hosp with SPECT</td>
<td>0.154</td>
<td>0.018</td>
<td>0.162</td>
</tr>
<tr>
<td></td>
<td>(0.053)***</td>
<td>(0.211)</td>
<td>(0.054)***</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement.

* Significant at 10%. ** significant at 5%, *** significant at 1%. 
<table>
<thead>
<tr>
<th></th>
<th>Full Market</th>
<th>Just Merging Chains</th>
<th>Just Rivals</th>
</tr>
</thead>
<tbody>
<tr>
<td># of Hospitals</td>
<td>-0.015</td>
<td>-0.235</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.080)***</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Market Tech Index</td>
<td>0.062</td>
<td>-0.016</td>
<td>0.077</td>
</tr>
<tr>
<td></td>
<td>(0.015)***</td>
<td>(0.081)</td>
<td>(0.016)***</td>
</tr>
<tr>
<td># of Hosp with MRI</td>
<td>0.002</td>
<td>-0.105</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.049)</td>
<td>(0.166)</td>
<td>(0.054)</td>
</tr>
<tr>
<td># of Hosp with Cath Lab</td>
<td>0.036</td>
<td>-0.050</td>
<td>0.050</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.130)</td>
<td>(0.030)*</td>
</tr>
<tr>
<td># of Hosp with Open-Heart Surg</td>
<td>0.041</td>
<td>0.180</td>
<td>0.031</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.168)</td>
<td>(0.037)</td>
</tr>
<tr>
<td># of Hosp with Diag Radioisotopes</td>
<td>0.084</td>
<td>0.031</td>
<td>0.103</td>
</tr>
<tr>
<td></td>
<td>(0.020)***</td>
<td>(0.066)</td>
<td>(0.022)***</td>
</tr>
<tr>
<td># of Hosp with ESWL</td>
<td>0.139</td>
<td>0.043</td>
<td>0.116</td>
</tr>
<tr>
<td></td>
<td>(0.068)**</td>
<td>(0.236)</td>
<td>(0.079)</td>
</tr>
<tr>
<td># of Hosp with Angioplasty</td>
<td>0.026</td>
<td>-0.104</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.171)</td>
<td>(0.038)</td>
</tr>
<tr>
<td># of Hosp with CT scan</td>
<td>0.028</td>
<td>-0.100</td>
<td>0.059</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.064)</td>
<td>(0.024)**</td>
</tr>
<tr>
<td># of Hosp with Adv. OB unit</td>
<td>0.080</td>
<td>0.001</td>
<td>0.104</td>
</tr>
<tr>
<td></td>
<td>(0.028)***</td>
<td>(0.155)</td>
<td>(0.029)***</td>
</tr>
<tr>
<td># of Hosp with SPECT</td>
<td>0.169</td>
<td>0.263</td>
<td>0.171</td>
</tr>
<tr>
<td></td>
<td>(0.051)***</td>
<td>(0.186)</td>
<td>(0.052)***</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, market fixed effects, and an exposure variable to control for the number of hospitals. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
### Table 1.6: Relationship between Technology Levels and Use

#### Panel A: Utilization and Technology Variables Summed Across Types of Technology/Procedures

<table>
<thead>
<tr>
<th>People Receiving Procedures</th>
<th>People Receiving Procedures</th>
<th>People Receiving Procedures</th>
<th>People Receiving Procedures</th>
<th>Log Total Claims</th>
<th>Log Total Claims</th>
<th>Log TotalClaims</th>
<th>Log Total Claims</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Technology, Summed</td>
<td>0.956</td>
<td>0.185</td>
<td>-0.061</td>
<td>0.044</td>
<td>0.998</td>
<td>0.126</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.057)**</td>
<td>(0.086)**</td>
<td>(0.045)</td>
<td>(0.044)</td>
<td>(0.056)**</td>
<td>(0.120)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td>X</td>
<td></td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

#### Panel B: Pooled Regressions, Weighted

<table>
<thead>
<tr>
<th>Log People Receiving Procedures</th>
<th>Log People Receiving Procedures</th>
<th>Log People Receiving Procedures</th>
<th>Log People Receiving Procedures</th>
<th>Log Claims</th>
<th>Log Claims</th>
<th>Log Claims</th>
<th>Log Claims</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Technology</td>
<td>0.994</td>
<td>0.130</td>
<td>0.043</td>
<td>0.049</td>
<td>1.047</td>
<td>0.099</td>
<td>0.044</td>
</tr>
<tr>
<td></td>
<td>(0.058)**</td>
<td>(0.047)**</td>
<td>(0.032)</td>
<td>(0.023)**</td>
<td>(0.059)**</td>
<td>(0.069)</td>
<td>(0.055)</td>
</tr>
<tr>
<td>Year Fixed Effects, Interacted</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>with Tech Type</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Market Fixed Effects,</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interacted with Tech Type</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population Controls,</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interacted with Tech Type</td>
<td></td>
<td></td>
<td></td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Technology Type Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the log of technologies when the dependent variable is the log of a utilization measure: people receiving selected procedures or the number of claims. Panel A presents results from regressions where the dependent variable represents the sum of all people receiving or claims for the selected procedures. The technology measure is the sum of the hospitals with each of the six technologies that correspond with the relevant procedures. The regressions include some combination of year fixed effects, market fixed effects, and population controls, as indicated. Standard errors are clustered at the market level. The population controls are log of the market population and log of the Medicare population. (Population doesn’t vary across years; Medicare population does.) Panel B presents results from pooled regressions where observations are at the market-year-technology type level. They include combinations of year fixed effects and population controls, all of which the effect of which can vary by technology type. The regressions also have technology fixed effects, and standard errors are clustered at the market level. Data is from 1992 through 2000.

* Significant at 10%, ** significant at 5%, *** significant at 1%. 
Table 1.7: Utilization in Markets with Mergers

<table>
<thead>
<tr>
<th>Panel A: Regression Coefficients</th>
<th>People Receiving Procedure</th>
<th>Unique Claims for Procedure</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>All Procedures (Sum)</strong></td>
<td>0.038</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td>(0.023)*</td>
<td>(0.013)</td>
</tr>
<tr>
<td><strong>All Procedures (Pooled, Weighted Regression)</strong></td>
<td>0.026</td>
<td>0.007</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.020)</td>
</tr>
<tr>
<td><strong>CT Scans</strong></td>
<td>0.040</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td>(0.020)*</td>
<td>(0.009)*</td>
</tr>
<tr>
<td><strong>MRIs</strong></td>
<td>0.008</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.030)</td>
</tr>
<tr>
<td><strong>SPECT scans</strong></td>
<td>0.095</td>
<td>0.096</td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.048)**</td>
</tr>
<tr>
<td><strong>CABG</strong></td>
<td>-0.026</td>
<td>-0.038</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.040)</td>
</tr>
<tr>
<td><strong>Cardiac Catheterizations</strong></td>
<td>0.008</td>
<td>-0.010</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.020)</td>
</tr>
<tr>
<td><strong>PCI</strong></td>
<td>0.025</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.044)</td>
<td>(0.039)</td>
</tr>
</tbody>
</table>

**Panel B: Controls for All Regressions in Column**

Demographic Controls: \( X \) \( X \)

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. All Procedures (Sum) is the sum of all six procedures; All Procedures (Pooled, Weighted Regression) treat each procedure separately and weights each observation by the inverse of the average number of that procedure to account for the fact that certain procedures are performed more frequently. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1992-1998, and post-merger period are years after the merger announcement. Demographic controls include 5-year age bins, log of the Medicare population, and indicators for female and race.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Chapter 2

Market Consolidation, System Formation, and Hospital Expenditures*

Whether mergers reduce costs through economies of scale has long been of interest to economists and policymakers. Theoretically, merged firms should benefit from economies of scale if fixed costs can be shared over greater volume. Firms often argue that mergers will allow them to achieve efficiencies, offsetting effects from their incentive to raise prices after the merger is complete. Antitrust officials weigh the potential efficiency gains from firms’ proposed mergers, as well as any other benefit, against the merged firms’ potential to increase prices. Understanding whether and how merged firms achieve such efficiencies could inform policymakers’ analyses of future mergers.

This paper focuses on the effects of consolidation among hospitals. The implications of mergers in this industry are particularly relevant as the number of mergers and acquisitions increased following the passage of the Patient Protection and Affordable Care Act (Saxena et al. 2013, The Hospital Acquisition Report 2014). With healthcare approximately 17% of the US economy, understanding the potential of these mergers to affect costs is of great importance.

To understand the effects of hospital consolidation, I study the formation of the Columbia/HCA hospital system. Between 1993 and 1995, five for-profit hospital chains—Columbia, HCA, Galen,

---

*I thank Jon Gruber, Heidi Williams, and Nancy Rose for their guidance on this project as well as Jean Roth and Mohan Ramanujan for their invaluable support with the data and systems. Laurence Baker generously shared data. I gratefully acknowledge funding from the Bradley Fellowship Program and the National Institute on Aging grant T32-AG000186. All errors are my own.
Healthtrust, and Epic—merged into or were acquired by Columbia/HCA. Figure 2-1 shows the average costs per hospital for the five systems preceding the mergers, Columbia/HCA after the mergers, and non-Columbia/HCA hospitals. Prior to the formation of Columbia/HCA, four of the five chains had similar expenditures as the unaffiliated hospitals, and Columbia had higher costs. Post-merger, the system had lower expenditures than unaffiliated hospitals, and its expenditures didn’t rise over time as those of unaffiliated hospitals did. Did the mergers generate cost efficiencies, lowering Columbia/HCA’s expenditures?

I investigate two ways in which the mergers could have contributed to lower expenditures: 1) local consolidation and 2) economies of scale from the overall size of the system, which had over 300 hospitals after the mergers. First, local consolidation could lead to lower costs as hospitals eliminate duplicate facilities, reorganize divisions, combine administrative services, and pool staff (Dranove and Lindrooth 2003, Connor, Feldmand and Dowd 1998, Dranove and Shanley 1995). Forming a local system may also increase the hospitals’ bargaining power when negotiating with physicians and staff. In theory, these cost efficiencies could be achieved by any local consolidation.

Second, a large hospital system, by virtue of its size, may have additional means to lower its costs. It could have greater bargaining power with medical suppliers, many of which are companies with a national presence themselves. Suppliers, such as General Electric and Johnson & Johnson, may not be willing to negotiate with a two or three-hospital system, but willing to negotiate with a system of several hundred hospitals. Additionally, systems with many hospitals may be able to develop a large network across which they share information about best practices and ways to reduce costs. Through better access to capital markets, a large system may also have a lower cost of capital.

To examine whether local consolidation led to lower expenditures, I exploit variation in how hospitals were affected by the series of mergers. Many hospitals that belonged to the merging chains operated in markets where one or more of the other merging chains also owned a hospital. After the mergers, these hospitals had become part of the same system as one or more of their former rivals. The local market structure of other Columbia/HCA hospitals, though, did not change as a result of the mergers. These hospitals operated in markets in which the other merging chains weren’t present.

I estimate the effects of the mergers in a difference-in-difference framework. Often the concern when comparing hospitals that merged to hospitals that did not is that that unobservable char-

---

1The expenditures measure has been adjusted for differences in volume across hospitals.
acteristics precipitate the merger and are correlated with costs, such that estimates capture both the effect of the merger and the unobservable characteristics. National mergers, however, mitigate this concern as they are less likely to be related to conditions in a particular market or hospital. Additionally to facilitate the comparisons between the treatment and control hospitals, I limit the analysis to hospitals that were in markets containing at least two for-profit hospitals. All treatment hospitals, by definition, operated in markets with two for-profit hospitals.

To investigate whether a large, national system can achieve cost efficiencies through economies of scale, I document the post-merger change in expenditures at Columbia/HCA in a difference-in-difference framework. The analysis provides a possible estimate for the cost efficiencies of a large hospital system. However, the estimate is speculative. I am unable to establish whether the expenditure difference between Columbia/HCA and unaffiliated hospitals is due to efficiencies gained from operating in a large system or practices that Columbia/HCA had that any hospital could adopt.

The results suggest that local consolidation did not contribute Columbia/HCA’s lower expenditures. In contrast, I document that Columbia/HCA’s expenditures were approximately 14% lower after the mergers. Although far from conclusive, it suggests there may be substantial efficiencies for large systems. Taken together, the two results are striking; a hospital system that lowered its average per hospital expenditures 14% was unable to lower expenditures more in markets where it consolidated. This suggests that economies of scale from local system formation are minimal if the hospitals are already part of a system.

Prior studies have examined how hospital consolidation affects costs. Here it is important to distinguish between hospital mergers, in which two hospitals operate under one license post-merger, and system formation, in which hospitals have common ownership but continue to operate under separate licenses post-consolidation. I use the terms “mergers,” “system formation,” and “consolidation” to refer to the mergers of the hospital systems that are the focus of this paper. After these mergers, the hospitals involved continued to operate under separate licenses, making this study more directly comparable to prior studies on whether systems can reduce costs.

The distinction of whether hospitals operate under one license may not be important when studying the effects of increased market power, but crucial when studying how consolidation affects costs. Hospitals operating under one license may bypass state regulations that affect whether hospitals can eliminate or reorganize duplicate services (Dranove and Lindrooth 2003). Dranove and Lindrooth (2003) argue that because of such regulatory differences, mergers after which the
hospitals operate under one license have greater potential cost savings. Their results bear this out; they find that when two hospitals merged under one license, they experienced 14% lower costs. When two independent hospitals formed a system, on the other hand, there were no significant cost savings.

Additional research has examined the impact of mergers, after which hospitals operate under one license, and find that hospital mergers lower costs 2%-10% (Connor et al. 1998, Spang, Bazzoli and Arnould 2001, Harrison 2011). Prior work on hospital systems is more mixed. Dranove and Shanley (1995) and Dranove, Durkac and Shanley (1996) find that local systems do not experience cost reductions, but Menke (1997) finds that multihospital systems were less costly than independent hospitals. The difference in results may be due to the fact that Menke (1997) examines hospitals in all systems, local, regional, and national.

My study contributes to this literature in several ways. First, my study addresses a methodological limitation of prior studies. By studying local consolidation that resulted from national chain mergers, my empirical strategy reduces concerns that unobservable characteristics affect both the decision to merge and costs. The result that local system formation does not lead to cost reductions bolsters previous findings. Second, in contrast to much previous work on systems, I present some evidence that these large hospitals systems may be able to reduce cost growth even if local systems cannot.

In the following section, I discuss the Columbia/HCA mergers and the methodology for this study. Section 2.3 describes the data, and Section 2.4 presents the empirical analysis. Section 2.5 shows the results are robust to a number of changes. Section 2.6 concludes.

### 2.2 The Columbia/HCA Mergers and Methodology

A challenge with studying the effects of mergers is that entities do not randomly merge. Previous research on how hospital mergers and system acquisitions affect costs compares the costs of the merged entity to those of hospitals that did not merge. Although the studies control for observable differences between the merging and non-merging hospitals, unobservable characteristics that led the hospitals to merge, such as anticipated changes in demand, could also affect the hospitals’ costs. My empirical strategy mitigates such concerns. I employ the same strategy as in Easterbrook (2015), focusing on the mergers of five for-profit hospital chains.

Between 1993 and 1995, five national hospital chains—Columbia, Galen, the Hospital Corpo-
ration of America (HCA), Healthtrust, and Epic—consolidated into one chain, Columbia/HCA. The series of mergers began when Columbia acquired Galen, which was comprised of the hospitals previously owned by Humana (Kuttner 1996). Columbia and HCA merged in 1994, forming Columbia/HCA (Kuttner 1996). In October 1994, Columbia/HCA announced it would acquire Healthtrust, which had recently acquired Epic (Amos 1994, *HealthTrust Completes Acquisition of EPIC Holdings 1994*). The merger was completed in early 1995. During the acquisition of Healthtrust, the FTC required Columbia/HCA divest seven hospitals (Lutz 1995). Following these mergers, Columbia/HCA owned over 40% of for-profit general medical and surgical hospitals and about 6% of all general hospitals in the US. More context on the mergers is provided in Easterbrook (2015).

To study how local hospital consolidation affects costs, I leverage differences in how these mergers affected the hospitals involved. Hospitals that belonged to one of the merging chains and located in markets where two or more of the merging chains operated in 1992 became part of the same system as other local hospitals by 1995. These hospitals are the “treatment” group or in “treatment” markets. The “control” hospitals are those that belonged to the merging chains, but were located in markets where only one of the five merging chains operated. These hospitals did not experience local consolidation as a result of the mergers. This strategy is similar to the one Dafny et al. (2012) employ. They exploit local variation in insurer market concentration due to the merger of two national health insurers to study how market concentration affects insurance premiums.

The identifying assumption is that had the mergers not occurred, the costs of treatment and control hospitals would have trended similarly after 1995. In section 2.4, I provide empirical support for this assumption. The methodology also mitigates concerns that the timing of the mergers is correlated with changes in costs among the treatment hospitals. Since these were mergers of national chains, the timing and even the decision to merge was unlikely to have been affected by the particular characteristics of any one hospital or market.

Yet, the hospitals that experienced local consolidation are not random. These were hospitals in markets that had favorable enough conditions that at least two for-profit hospitals had entered by 1992. Additionally, the decision to merge would have accounted for the fact that the acquisition would lead to increased market power in some markets. In its 1993 Annual Report to Shareholders, Columbia/HCA acknowledges the company will benefit from “synergies” and “efficiencies” in overlapping markets in the Galen and HCA mergers. If costs were changing more favorably in many of
the overlapping markets, that could have made the mergers more attractive to Columbia.

I address the first concern by limiting my study to hospitals in markets where two or more for-profit hospitals operated. I am unable to fully address the second concern, though the fact that Columbia/HCA expanded into new markets with each merger reduces it. Galen had hospitals in 7 markets in which Columbia also operated hospitals and 34 hospital in markets where Columbia wasn’t present. Upon merging, Columbia expanded into those 34 markets. When merging with HCA, Columbia consolidated market power in 17 markets and entered an additional 24 markets. Favorable conditions in the markets in which Columbia expanded would have made the mergers attractive, mitigating concerns Columbia chose its acquisition targets largely based on favorable conditions in the particular markets that became more consolidated.

To study how system acquisition may have impacted costs, I compare changes in costs at all Columbia/HCA hospitals to costs in hospitals unaffiliated with Columbia/HCA in a difference-in-difference framework. This is merely suggestive of the effect of systems; I have no way to distinguish between the effects of being part of a hospital system with hundreds of hospitals and the effect of being part of Columbia/HCA. Of particular concern is that Columbia/HCA may have employed strategies to control costs that were available to any hospital, regardless of whether it was part of a system. For instance, Columbia/HCA could have substituted supplies toward cheaper alternatives, refinanced debt, or developed more efficient ways of handling paperwork. In contrast, benefits from a stronger negotiating position with suppliers or larger information network do depend on the hospital being part of a large system.

There is reason to think that both mechanisms may have contributed to lower costs. Columbia/HCA’s 1993 Annual Report says,

“[A] benefit of the HCA merger is the significant synergies the combined company expects to realize as a result of its size. In 1994, Columbia/HCA will spend almost $2 billion in medical supplies. With this magnitude of combined purchasing power, coupled with our hospitals’ commitment to standardize national contracts, we expect to obtain significant pricing reductions from our vendors. In addition, we expect savings in overhead, insurance costs, information systems and interest expense.”

Additionally, Columbia/HCA’s 1995 Annual Report discusses how the company is sharing information across the system. It

“initiated efforts for determining best practices for specific types of programs and clin-
ical procedures—information that again can be shared among the Columbia system
providers. ...The teams are establishing clinical pathways for specific procedures which
may lead to new designs for surgical supplies and methods for improving patient out-
comes and reducing the overall cost of a given procedure.”

Yet, Columbia/HCA Annual Reports also discuss measures to reduce costs that are not directly
related to the size of its system. The 1993 report details how handheld computers have saved nurses
time and electronic filing of claims has reduced costs.

2.3 Data

To study how hospital consolidation affects expenditures, I need information to identify the merging
hospitals as well as data on hospital expenditures and factors affecting costs, such as number of
admissions. The American Hospital Association’s (AHA) annual survey provides most of this
information. Each year, it surveys all hospitals in the United States. It collects data on whether a
hospital belongs to a system and if so, which system, allowing me to identify the hospitals that were
part of Columbia/HCA. The hospitals’ addresses allow me to identify which hospitals experienced
local consolidation. As in Easterbrook (2015), I define markets as hospital referral regions (HRRs).
The Dartmouth Atlas constructed HRRs such that patients living within the HRR receive the
majority of major cardiovascular procedures within that HRR. There are 306 HRRs in the United
States. 45 of these had two or more of the merging chains present in 1992, and 64 HRRs had only
one of the merging chains.2

The AHA Survey also contains information on hospital ownership, enabling me to restrict to
hospitals in markets in which two or more for-profit hospitals operated. This restriction helps
ensure that the treatment and control hospitals are comparable; both operate in markets that at
least two for-profit hospitals deemed attractive to enter. The restriction also indirectly helps reduce
any regional differences between treatment and control hospitals that could threaten identification.
In the 1990s, for-profit hospitals had the largest presence in the southern and western states of the
US. The restriction reduces the number of markets that have hospitals in the control group from
64 to 48.

Using the AHA data, I restrict the hospitals in my study to non-federal general medical and
surgical hospitals since the factors affecting costs are likely to be similar across these hospitals. My

2 When defining treatment markets, I do not consider the hospitals Columbia/HCA had to divest when it acquired
Healthtrust as documented in the prior section.
restriction primarily excludes military hospitals, those under the Department of Veterans Affairs, psychiatric hospitals, rehabilitation hospitals, and hospitals specializing in substance abuse. It also excludes specialty hospitals such as children’s hospitals and eye, ear, and throat hospitals. These hospitals offer more differentiated products than a general medical and surgical hospital or cater to a particular type of patient, which may affect their costs.

To measure hospital expenditures, I use the AHA’s variable on total facility expenditures. Since I’m studying hospital consolidation, hospital expenditures, excluding long-term care facility expenditures, would have been ideal, but a 1994 change in the AHA survey eliminated this variable. To the extent that certain costs, such as overhead or building maintenance, are difficult to divide between a hospital and long-term care facility, total facility expenditures may be more accurate in a survey.

In many specifications, I control for hospital and market characteristics that could affect the expenditures. For instance, hospitals with higher volumes, ceterus paribus, have higher expenditures. Similarly patient mix at a given hospital could affect costs. Patients that have more severe conditions and or who are enrolled in Medicaid tend to be more costly to treat. Additionally, hospitals in areas with higher wages are likely to have higher costs, and hospitals in markets with more competition or higher HMO penetration may be incentivized to lower expenditures to be more efficient.³

The AHA Survey provides information on such factors that affect facility expenditures. Most importantly, it contains total admissions and outpatient visits, allowing me to control for output differences across hospitals. Additionally, it contains information on the number of beds at hospitals, which I use to calculate the Herfindahl-Hirschman Index (HHI) of a market. I use Medicare and Medicaid inpatient days and number of births to control for differences in patient mix.

I supplement the AHA data with data from the Medicare Impact Files, which contain information on the case-mix of Medicare patients in a hospital and the hospital’s wage index. The wage index will allow me to control for labor cost differences across hospitals. Finally, I also use population data from the Census and data on the percentage of people enrolled in health maintenance organizations (HMOs) that Laurence Baker constructed from Interstudy records and has generously shared.⁴

³ The relationship between competition and costs has been subject to much research. The “medical arms race” literature argues that competition induces hospitals to make costly investments to attract physicians and patients, raising expenditures. Regardless of the direction of the relationship, controlling for competition is important.

⁴ Baker and Phibbs (2002) describes the construction of this data.
2.4 Empirical Analysis

Table 2.1 displays summary statistics for the hospitals in systems that would become Columbia/HCA. The table shows that while the market characteristics differed between the treatment and control hospitals, the average hospital characteristics are similar. Compared to control hospitals, the average treatment hospital was located in a market with a greater population, more hospitals, and a less concentrated hospital market. It had more for-profit hospitals and fewer hospitals with residency programs in its market. Yet hospital characteristics, such as total expenditures, admissions, outpatient visits, and beds were similar between the treatment and control hospitals. Among these variables, only births as a percentage of admissions differed significantly. While the comparison in market characteristics raises some concerns about the comparability of treatment and control hospitals, I will show that prior to the mergers, trends in expenditures were similar between the two groups.

The fifth column in Table 2.1 shows the characteristics of the average unaffiliated hospital across all markets. These are the hospitals against which I compare Columbia/HCA hospitals when evaluating the possible effects of forming a system. Their expenditures, volumes, and patient mix characteristics are in the same general range as the those at the Columbia/HCA hospitals, though they have higher expenditures and volume and a larger Medicaid share. This suggests it will be important to control for volume and patient mix when including unaffiliated hospitals in the analysis.

Figure 2-2 shows the HRRs that had two or more for-profit hospitals and where chains that merged into Columbia/HCA operated. The dark blue in the map represents markets in which two or more of the merging chains owned hospitals. The Columbia/HCA hospitals in these markets are the treatment hospitals when studying local consolidation. The light blue markets are the markets in which the control hospitals operated. In these markets, only one of the merging chains is present in 1992. The map shows that treatment and control hospitals operate in the same regions of the US, namely the southern and western states.

2.4.1 The Effect of Local Consolidation

My main specification to evaluate whether local consolidation resulted in lower costs is an event study. The event study allows to me flexibly estimate whether expenditures changed post-merger as well as evaluate the trends in expenditures prior to the mergers. The existence of pre-trends
would cast doubt on the identifying assumption of this analysis. Specifically, I assume in absence of the mergers, treatment and control hospitals would have had similar trends in expenditures. The presence of similar trends before the mergers provides support for this assumption.

The event study specification is:

\[
\log(\expit) = \sum_{k=-5}^{-2} \beta_k [YAM_{it} = k] + \sum_{k=0}^{5} \beta_k [YAM_{it} = k] + \delta V_{it} + \gamma_i + \alpha_i + \epsilon_{it} \tag{2.1}
\]

\(\log(\expit)\) is the natural log of hospital \(i\)'s expenditures in year \(t\). \(YAM_{it}\) indicates how many years have passed since the merger. If the hospital experienced local consolidation, \(YAM_{it}\) will equal 1 one year after the merger, 2 two years after the merger, etc. It is 0 the year(s) of the merger announcement and takes on negative values for years preceding the merger. \([YAM_{it} = k]\) is an indicator for whether the observation occurs \(k\) years after the merger. I normalize \(\beta_{-1}\) to be equal to 0 such that all years are compared to the year prior to the merger. \(V_{it}\) are volume controls since costs will naturally vary depending on hospitals’ annual volume. Following Dranove and Lindrooth (2003), the volume controls are:

- \(\ln(a_{it})\) - log admissions
- \((\ln(a_{it}))^2\) - square of log admissions
- \(\ln(o_{it})\) - log outpatient visits
- \((\ln(o_{it}))^2\) - square of log outpatient visits
- \(\ln(a_{it}) \ln(o_{it})\) - interaction of log admissions and log outpatient visits

\(\gamma_i\) are year fixed effects, \(\alpha_i\) are hospital fixed effects, and \(\epsilon_{it}\) is the standard error, clustered at the hospital level.

While the volume controls absorb variation in costs because of year-to-year changes in hospital volume, they are endogenous. The number of admissions and outpatient visits may be affected if the hospital became part of the same system as a former rival in its market. Therefore, I also estimate the event studies specification without volume controls.

Figure 2-3 shows the \(\beta_k\)'s from the event specification plotted with their 95% confidence intervals. The first graph shows the estimates without controlling for volume, and the estimates in the second graph include volume controls. Reassuringly, they look very similar. Prior the
mergers, treatment hospitals had very similar trends in expenditures as the control hospitals, providing empirical support for the identifying assumption. After the mergers, expenditures at the Columbia/HCA hospitals that did and did not experience local consolidation continued to trend similarly. It does not appear that local consolidation enabled Columbia/HCA to reduce costs.

To obtain a point estimate of the effect of local consolidation on hospital costs and determine the bounds of the possible effect, I estimate the following specification:

\[
\ln(\exp_{it}) = \beta_1 \text{treat}_i + \beta_2 \text{treat} \times \text{during}_{it} + \beta_3 \text{treat} \times \text{post}_{it} + \delta V_{it} + \psi X_{it} + \gamma t + \alpha_i + \epsilon_{it} \quad (2.2)
\]

treat\_i is a dummy indicating whether the hospital was in a market that became more consolidated. treat \times during\_it is an interaction between the treatment dummy and a dummy indicating the observation is from a year in which a merger affecting hospital \(i\)'s market was announced. treat \times post\_it is an interaction between the treatment dummy and a post-merger dummy. \(X_{it}\) are additional controls that account for differences in the market and patient mix that can affect a hospital's costs. I add these here for robustness. The market characteristics include the log of the number of hospitals in the market, the market’s HHI, log of the percentage of people in the market in HMO plans, and the wage index. Patient mix controls include the log of the number of Medicare inpatient days, log of the number of Medicaid inpatient days, log of the number of births, and the Medicare case mix index. Since the wage index and Medicare case mix index are missing for some observations, I estimate Equation 2.2 with and without those controls. As above, the controls may be affected by whether the hospital experiences local consolidation, and thus I also estimate the impact of mergers without them.

Table 2.2 shows the results. Neither during nor after the mergers were complete is the estimate of the effect of local consolidation statistically different from 0. The results are consistent across the different sets of controls and confirm the results of the event studies. When including all the controls, the confidence intervals imply expenditures did not decline by more than 6.6% nor increase by more than 4.3%. This interval is very similar to the results in Dranove and Lindrooth (2003), who rule out expenditure declines greater than 5.9% and increases greater than 5.4% three years after system formation, as well as declines greater than 8.3% and increases greater than 2.7% four years after system formation.

While consistent with prior research, these results are not aligned with Columbia/HCA’s stated expectations that it should realize “efficiencies in markets where both Columbia and HCA operate”
and “synergies...in overlapping markets [with Galen].” Further, Columbia anticipated benefits from local consolidation. Columbia/HCA’s expansion in these markets wasn’t limited to hospitals; it bought outpatient, rehabilitation, psychiatric, and skilled nursing care facilities. In doing so, it suggested there would be efficiencies from “sharing support services, monitoring quality assurance activities as a unit, and marketing the network as a single entity, all of which reduce overhead for the individual facilities and local network as a whole.”

While the described local efficiencies would not have resulted solely from the affiliation of hospitals, it’s notable that hospital consolidation did not lead to a significant change in costs.

2.4.2 Cost Efficiencies within Columbia/HCA

The results on local consolidation are also striking because Columbia/HCA hospitals were able to maintain much lower expenditure growth than other hospitals. Figure 2-4 shows the average output-adjusted expenditures at Columbia/HCA hospitals and non-Columbia/HCA hospitals. To illustrate both the difference between Columbia/HCA and unaffiliated hospitals as well as the lack of an effect from local consolidation, each group of hospitals is separated into the average at hospitals in markets where Columbia/HCA experienced local consolidation and hospitals in markets that didn’t experience consolidation. The graph strongly suggests that prior to the system mergers, the expenditures at Columbia/HCA hospitals were similar to those at unaffiliated hospitals, but after the mergers, Columbia/HCA’s expenditures were markedly lower. As in the prior analysis, the Columbia/HCA hospitals that experienced local consolidation do not appear to have economically significant cost efficiencies over their counterparts in other markets.

To document the cost savings within Columbia/HCA, I conduct an event study and estimate the magnitude of the decrease. The identifying assumption of this difference-in-difference analysis that had the Columbia/HCA system not formed, the hospitals that were part of chains that became Columbia/HCA would have had similar trends in expenditures as the unaffiliated hospitals. Examining the pre-trends in the event studies allows me to gauge the plausibility of this assumption.

The event study specification is:

$$
log(expit) = \sum_{k=-5}^{2} \beta_k [YAS_{it} = k] + \sum_{k=0}^{5} \delta_k [YAS_{it} = k] + \delta V_{it} + \gamma_t + \alpha_i + \epsilon_{it} \quad (2.3)
$$

$YAS_{it}$ indicates how many years have passed since the hospital joined the Columbia/HCA system.

5 The quotes are from Columbia/HCA’s 1993 Annual Report.
The timing of this event is based on when the system that owned the hospital in 1992 joined or merged into Columbia/HCA. This was 1993 for hospitals originally belonging to Columbia, HCA, or Galen and 1994 for hospitals that were initially part of Healthtrust or Epic. YAS<sub>it</sub> equal to 0 the year in which the hospital joined the Columbia/HCA system, 1 one year after the hospital joined Columbia/HCA, etc. The control hospitals are all unaffiliated hospitals in markets that had two or more for-profit hospitals in 1992, and they help identify year fixed-effects and the effects of the volume controls.

Figure 2-5 shows the results of the event study. The Columbia/HCA hospitals had higher expenditures than non-Columbia/HCA hospitals prior to system formation. This is consistent with Figure 2-4. It doesn’t appear to be a decline in expenditures prior to the mergers, although it’s not clear what its costs were in the year prior to the system formation. This provides empirical support for the identifying assumption.

After the Columbia/HCA system formed, expenditures for hospitals within the system fell, starting in the year of the merger announcement. This is true whether or not I control for volume in the hospitals. The decline in this year could indicate Columbia/HCA started to implement cost saving measures unrelated to the system formation. It could also indicate that there was greater information sharing across hospitals; it seems unlikely that the hospitals could have reduced costs through a stronger negotiating position with suppliers just as the mergers were announced.

To determine the magnitude of the decline, I estimate:

\[
\ln(\text{exp}_{it}) = \beta_1 \text{HCA}_i + \beta_2 \text{HCA} \times \text{during}_{it} + \beta_3 \text{HCA} \times \text{post}_{it} + \delta V_{it} + \psi X_{it} + \gamma_i + \alpha_i + \epsilon_{it} \quad (2.4)
\]

HCA<sub>i</sub> is a indicator for whether the hospital belonged to Columbia/HCA, regardless of whether it was located in a market that experienced consolidation. The during and post periods in these regressions depend on the years in which the hospitals became part of Columbia/HCA, rather than refer the timing of local consolidation.

To confirm that Columbia/HCA’s expenditure decline is similar between markets with and without consolidation, I separate the effect by market type and estimate:

---

<sup>6</sup>For hospitals that joined Columbia/HCA, but were not part of one of the original systems, the “years-after-system formation” would equal 1 in 1994, 2 in 1995, 3 in 1996, etc, provided the hospital was part of the Columbia/HCA system in that year.
\[
\ln(\text{exp}_{it}) = \beta_{1a}M_{1i} + \beta_{1b}M_{2i} + \beta_{2a}M_{1i} \times \text{treat}_{i} \times \text{during}_{it} + \beta_{2b}M_{1i} \times \text{treat}_{i} \times \text{during}_{it} + \beta_{3a}M_{1i} \times \text{treat}_{i} \times \text{post}_{it} + \beta_{3b}M_{2i} \times \text{treat}_{i} \times \text{post}_{it} + \delta V_{it} + \psi X_{it} + \gamma_{t} + \alpha_{i} + \epsilon_{it} \tag{2.5}
\]

M1 is a dummy that indicates the Columbia/HCA hospital was in a market that did not become more consolidated as a result of the mergers, and M2 is a dummy indicating that the Columbia/HCA hospital was in a market that became more consolidated. If Columbia/HCA hospitals had significantly lower expenditures, but didn’t benefit from local consolidation, \(\beta_{3a}\) and \(\beta_{3b}\) should both be negative and of similar magnitudes.

Table 2.3 displays the results. The coefficients indicate that expenditures at Columbia/HCA hospitals declined approximately 14% after the system formed. Consistent with the prior results, the estimates for Equation 2.5 reveal that the relative decline in expenditures is similar in markets where Columbia/HCA experienced local consolidation and those where it did not. Wald tests do not allow me to reject the hypothesis the coefficients \(\beta_{3a}\) and \(\beta_{3b}\) are the same.

2.5 Robustness Tests

The results are robust to a number of adjustments to the sample and specification.

As previously discussed, post-merger admissions and outpatient visits are endogenous to the merger, so including them as controls may cause me to misestimate the effects of the local consolidation or the difference between Columbia/HCA and its competitors. To address this issue, I redefine the dependent variable as the log of expenditures per admission and re-estimate the models. This is a cruder way to account for volume differences, as it fails to account for differences in outpatient visits and imposes that log expenditures and log admissions have a one-to-one relationship.

The results on the effects of local consolidation are shown in Panel A of Figure B-1 and Table B.1, and Panel A of Figure B-2 and Table B.2 compare Columbia/HCA to unaffiliated hospitals. The estimates indicate that local consolidation did not lead to a statistically significant change in expenditures per admission and that Columbia/HCA’s expenditures per admission declined by approximately 17% after the mergers. For completeness, I also show the regression results when controlling for output, market characteristics, and patient mix characteristics, following Connor et al. (1998).\(^7\) Their motivation for using expenditures per admission for the dependent variable is

---

\(^7\)Spang et al. (2001) also use expenditures per admission as their key measure, following Connor et al. (1998).
that it reduces heteroscedasticity in the error term. The cluster-robust errors I use should account for any heteroscedasticity in my setting.

Another concern is that some markets in the control group experienced local consolidation after the series of system mergers. If local consolidation led to lower expenditures and some control markets also became more consolidated, their inclusion among the control hospitals would attenuate my estimate of the effects of local consolidation. As a robustness check, I exclude all hospitals in control markets where Columbia/HCA acquired additional hospitals post-merger. These acquisitions were not part of large system mergers and thus, are more likely to be related to local market conditions. Because these hospitals aren’t random, I also exclude all hospitals in treatment markets where Columbia/HCA acquired additional hospitals.

Panel B in Figure B-1 and Table B.1 show the results on the effects of local consolidation. Although expenditures in treatment markets are on average lower than in control markets, the difference is not statistically significant. Although this issue isn’t a concern when evaluating the cost savings of the Columbia/HCA system, Panel B of Figure B-2 and Table B.2 show the expenditures at Columbia/HCA hospitals relative to other hospitals for completeness. As before, Columbia/HCA has lower expenditures after the mergers. While Wald tests would allow the rejection of the hypothesis that $\beta_{3a}$ and $\beta_{3b}$ from Equation 2.5 are the same, the way the during and post periods are defined in the specification are based on the formation of Columbia/HCA and not the timing of local consolidation. This makes the estimates in Equation 2.5 inferior to the estimate from Equation 2.2.

An additional concern with the definition of treatment and control markets is that HCA spun off about 100 hospitals to form Healthtrust in 1987 (Kuttner 1996, Lutz and Gee 1998). Since HCA chose which hospitals to retain and which to divest, it is unlikely that HCA spun off hospitals that would be strong, direct competitors to hospitals it retained. Important for my setting, it’s unlikely that HCA spun off some hospitals and retained others where the hospitals in question benefitted greatly from local coordination. Hence the ability to coordinate care and share overhead likely had less potential impact in the markets that became more consolidated because HCA and Healthtrust owned hospitals there in 1992.

The inclusion of the markets in the treatment group could attenuate the regression results. To test this, I exclude hospitals in the treatment markets that were defined as such because HCA and Healthtrust operated there. If a third merging chain also owned hospitals in that market in 1992, the market remains in the analysis. The results, shown in Panel C of Figures B-1 and B-2 and
Tables B.1 and B.2, are quite similar to the original results.

Finally, I test whether the results are sensitive to the use of a balanced panel to check whether entry and exit into the sample explains the results. Panel D of Figures B-1 and B-2 and Tables B.1 and B.2 present the results. The results are robust to this change in the sample.

2.6 Conclusions

This study provides further evidence that hospitals that become part of the same system as their former rivals in the same market do not have lower expenditures as a result of consolidation. This contribution is important for two reasons. First, the empirical strategy in this study addresses a shortcoming of methodologies used in past studies, which have compared expenditures at hospitals that join systems to those that do not. By focusing on the mergers of national hospital chains, local consolidation in systems is arguably less likely to be related local market conditions.

Second, the system at the center of my study—Columbia/HCA—dramatically reduced cost growth in its hospitals. After the system formed, its hospitals had approximately 14% lower expenditures than other hospitals. That a hospital system as focused on lowering expenditure growth as Columbia/HCA did not find ways to reduce costs in markets with consolidation is striking. It suggests that antitrust authorities should place particular scrutiny on claims that local systems will reduce costs.

Additionally, this study provides suggestive evidence that large hospital systems can in fact lower expenditure growth. Across all markets, Columbia/HCA’s hospitals had lower expenditures after the system grew to over 300 hospitals. I am unable to determine how much of the lower expenditure growth was due to cost advantages of a system versus cost-saving measures that Columbia/HCA implemented an other hospitals could copy. If Columbia/HCA’s lower expenditures were in part due to the system, it’s not clear how large a system must be for cost savings to accrue. Several previous studies have focused on hospitals systems comprised of two or three hospitals and found no cost savings. More interestingly, prior to the formation of Columbia/HCA, Healthtrust and HCA each had approximately 100 hospitals, and their costs were similar to other hospitals’ expenditures (see Figure 2-1). This suggests one of the following: Columbia/HCA lowered costs through means other than leveraging its system, the scale needed to reduce expenditures exceeds 100 hospitals, or not all systems realize the potential cost advantages of their scale. Regardless of the mechanism, the results of this paper suggests changes in hospital ownership can facilitate lower expenditures.
though local consolidation does not generate cost savings.
Figure 2-1: Volume-Adjusted Expenditures for the Columbia/HCA System Pre and Post-Merger

This graph plots the average volume-adjusted expenditures per hospital for chains that would become the Columbia/HCA system and the system post-merger. The volume-adjusted expenditures are the residuals from the following regression:

\[
\ln(\text{exp}_it) = \beta_1 \ln(\text{a}_it) + \beta_2 (\ln(\text{a}_it))^2 + \beta_3 \ln(\text{a}_it) + \beta_4 (\ln(\text{a}_it))^2 + \beta_5 \ln(\text{a}_it) \ln(\text{o}_it) + \epsilon_{it}.
\]

\(\text{exp}_it\) is hospital \(i\)'s expenditures in year \(t\), \(\text{a}_it\) is its admissions, and \(\text{o}_it\) is its outpatient visits. The non-Columbia/HCA hospitals are all non-federal general medical and surgical hospitals in markets with two or more for-profit hospitals in 1992 that aren’t affiliated with Columbia/HCA.
Markets are HRRs, as defined by the Dartmouth Atlas. Treatment markets are those in which two or more of the chains operated hospitals in 1992. Control markets are those where one of the chains operated hospitals and that had at least two for-profit hospitals in 1992. Markets with merging chains and two or more for-profit hospitals are determined using AHA Survey data.
Figure 2-3: Change in Expenditures at Columbia/HCA Hospitals that Experienced Local Consolidation

These graphs show how expenditures changed post-merger at Columbia/HCA hospitals that experienced local consolidation relative to Columbia/HCA hospitals that did not. The x axis represents the years prior to and after the merger, with 0 representing the year(s) of the merger announcement(s). The y axis represents the magnitude of the coefficients on the “years after merger” dummies in the event studies specification. The bars on each point represent the 95% confidence interval on the coefficient. The first graph shows the results without controlling for differences in output. The results in the second graph include output controls. See the text for more detail.
Figure 2-4: Output-Adjusted Expenditures for Columbia/HCA and Other Hospitals

This graph plots the average volume-adjusted expenditures per hospital for Columbia/HCA hospitals and non-Columbia/HCA hospitals. The output-adjusted expenditures are the residuals from the following regression:

\[
\ln(\text{exp}_{it}) = \beta_1 \ln(a_{it}) + \beta_2 (\ln(a_{it}))^2 + \beta_3 \ln(o_{it}) + \beta_4 (\ln(o_{it}))^2 + \beta_5 \ln(a_{it}) \ln(o_{it}) + \epsilon_{it}.
\]

\(\text{exp}_{it}\) is hospital i's expenditures in year t, \(a_{it}\) is its admissions, and \(o_{it}\) is its outpatient visits. The non-Columbia/HCA hospitals are all non-federal general medical and surgical hospitals in markets with two or more for-profit hospitals in 1992 that aren't affiliated with Columbia/HCA. For comparison, the Columbia/HCA and non-Columbia/HCA hospitals are split into hospitals in markets that experienced consolidation and those that did not.
Figure 2-5: Change in Expenditures at Columbia/HCA Hospitals Compared to Other Hospitals

These graphs show how expenditures at Columbia/HCA hospitals changed post-merger relative to non-Columbia/HCA hospitals. The x axis represents the years prior to and after the Columbia/HCA system formed, with 0 representing the year in which it was announced that the system would join Columbia/HCA. This was 1993 for hospitals that were part of Columbia, HCA, and Galen and 1994 for hospitals originally part of Healthtrust or Epic. The y axis represents the magnitude of the coefficients on the "years after system formation" dummies in the event studies specification. The bars on each point represent the 95% confidence interval on the coefficient. The first graph shows the results without controlling for differences in output. The results in the second graph include output controls. See the text for more detail.
Table 2.1: Summary Statistics for Columbia/HCA and non-Columbia/HCA Hospitals, 1992

<table>
<thead>
<tr>
<th>Panel A: Market Characteristics</th>
<th>Non-Merger Markets</th>
<th>Merger Markets</th>
<th>Difference</th>
<th>P Value</th>
<th>All Markets</th>
</tr>
</thead>
<tbody>
<tr>
<td>Populations (100,000s)</td>
<td>9.80</td>
<td>21.71</td>
<td>11.91</td>
<td>0.00</td>
<td>23.12</td>
</tr>
<tr>
<td>Pop. Density</td>
<td>184</td>
<td>137</td>
<td>-47</td>
<td>0.41</td>
<td>406</td>
</tr>
<tr>
<td>HHI</td>
<td>1985</td>
<td>1128</td>
<td>-857</td>
<td>0.00</td>
<td>1031</td>
</tr>
<tr>
<td># Hospitals in Market</td>
<td>16.10</td>
<td>36.97</td>
<td>20.87</td>
<td>0.00</td>
<td>39.39</td>
</tr>
<tr>
<td>% For-Profit Hosp. in Market</td>
<td>0.30</td>
<td>0.35</td>
<td>0.05</td>
<td>0.03</td>
<td>0.23</td>
</tr>
<tr>
<td>% Not-For-Profit Hospitals in Market</td>
<td>0.41</td>
<td>0.37</td>
<td>-0.04</td>
<td>0.14</td>
<td>0.47</td>
</tr>
<tr>
<td>% Gov't Hospitals in Market</td>
<td>0.29</td>
<td>0.28</td>
<td>-0.01</td>
<td>0.56</td>
<td>0.30</td>
</tr>
<tr>
<td>% Hospitals with Residency Prog.</td>
<td>0.12</td>
<td>0.09</td>
<td>-0.02</td>
<td>0.02</td>
<td>0.14</td>
</tr>
<tr>
<td>% People in HMOs</td>
<td>0.11</td>
<td>0.10</td>
<td>-0.01</td>
<td>0.36</td>
<td>0.13</td>
</tr>
</tbody>
</table>

| Panel B: Hospital Characteristics | | |
|-----------------------------------| | |
| Total Expenditures                | 44346892            | 38252936       | -6093957   | 0.21    | 48859772    |
| Expenditures Per Admission        | 7540                | 7300           | -240       | 0.46    | 7188        |
| Total Admissions                  | 5884                | 5285           | -600       | 0.28    | 6138        |
| Total Outpatient Visits           | 46566               | 40011          | -6556      | 0.12    | 62845       |
| Total Beds                        | 181                 | 170            | -11        | 0.48    | 179         |
| Total Surgeries                   | 4953                | 4138           | -815       | 0.14    | 4237        |
| Medicare % of Inpatient Days      | 0.52                | 0.54           | 0.01       | 0.58    | 0.47        |
| Medicaid % of Inpatient Days      | 0.10                | 0.09           | -0.01      | 0.53    | 0.18        |
| Births as % of Admissions         | 0.13                | 0.10           | -0.03      | 0.05    | 0.10        |
| Medicare Casemix Index            | 1.38                | 1.35           | -0.03      | 0.22    | 1.23        |
| Medicare Wage Index               | 1.09                | 1.08           | -0.01      | 0.66    | 1.08        |
| N                                 | 67                  | 193            |            |         | 2260        |

This table compares general characteristics of hospitals that were part of chains that became Columbia/HCA in treatment (merger) and control (non-merger) markets in 1992. The third column reports the difference between the treatment and control hospitals, and the fourth column shows the p-value on the difference based on a t statistic. The fifth column shows the summary statistics for non-Columbia/HCA hospitals. HHI is calculated based on the number of beds in the hospitals. See text for more detail on the data.
Table 2.2: Change in Expenditures at Columbia/HCA Hospitals in Markets with Mergers

<table>
<thead>
<tr>
<th></th>
<th>Log Total Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Interaction on Treatment and Post Dummy</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
</tr>
<tr>
<td>Interaction on Treatment and During Dummy</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
</tr>
<tr>
<td>Hospital Fixed Effects</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
</tr>
<tr>
<td>Volume Controls</td>
<td>X</td>
</tr>
<tr>
<td>Market Characteristic Controls</td>
<td>X</td>
</tr>
<tr>
<td>Patient Mix Controls</td>
<td>X</td>
</tr>
<tr>
<td>Case Mix Index</td>
<td>X</td>
</tr>
<tr>
<td>Wage Index</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>2294</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the interaction between treatment hospitals and a post-merger dummy in difference-in-difference regressions as well as the coefficient on the interaction between the treatment hospitals and dummy indicating the year of the merger. The controls are specified in the table. Volume controls include log of admissions, log of outpatient visits, their squares, and their interaction. Market characteristic controls are the log of the number of hospitals in the market, log of HMO penetration, and HHI. Patient mix controls include the number of Medicare and Medicaid inpatient days and number of births. Standard errors are clustered at the hospital level. Data is from 1990-1998, and post-merger period includes years after the merger announcement(s).

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Table 2.3: Post-System Formation Change in Expenditures at Columbia/HCA Hospitals

<table>
<thead>
<tr>
<th>Interaction on HCA and Post Dummy</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.146</td>
<td>-0.149</td>
<td>-0.152</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.019)**</td>
<td>(0.016)**</td>
<td>(0.016)**</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Interaction on HCA, Post, and Merger Market (2)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.160</td>
<td>-0.154</td>
<td>-0.157</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)**</td>
<td>(0.019)**</td>
<td>(0.018)**</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Interaction on HCA, Post, and non-Merger Market (3)</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-0.112</td>
<td>-0.137</td>
<td>-0.136</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.034)**</td>
<td>(0.027)**</td>
<td>(0.030)**</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Hospital Fixed Effects</th>
<th>X</th>
<th>X</th>
<th>X</th>
<th>X</th>
<th>X</th>
<th>X</th>
</tr>
</thead>
<tbody>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Volume Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Characteristic Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Patient Mix Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Case Mix Index</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Wage Index</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>N</th>
<th>22332</th>
<th>22332</th>
<th>22332</th>
<th>22332</th>
<th>20083</th>
<th>20083</th>
</tr>
</thead>
</table>

P-value on F statistic for Equality of (2) and (3)

|                             | 0.231 | 0.606 | 0.539 |

Specifications 1, 3, and 5 show the coefficient on the interaction between the treatment hospitals and post-merger dummy in difference-in-difference regressions. Specifications 2, 4, and 6 show the coefficients on interactions among HCA hospital dummies, post-merger period, and a dummy indicating whether the market became more consolidated as a result of the mergers. The regressions also include analogous interactions for the "during" period. The controls are specified in the table. Volume controls include log of admissions, log of outpatient visits, their squares, and their interaction. Market characteristic controls are the log of the number of hospitals in the market, log of HMO penetration, and HHI. Patient mix controls include the number of Medicare and Medicaid inpatient days and number of births. Standard errors are clustered at the hospital level. Data is from 1990-1998, and post-merger period includes years after the merger announcement based on the system to which the hospital previously belonged. Hospitals joining Columbia/HCA in the post-period are also included as part of the Columbia/HCA hospitals. The p value on the F statistic is from a Wald test of the equality of the coefficients in rows 2 and 3. See text for more detail.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Chapter 3

Patient Cost-Sharing for Physician Visits and Utilization in Employer-Sponsored Insurance Plans*

Increasing patient cost-sharing is often touted as a way to reduce healthcare expenditures, as individuals tend to use less of a service the more it costs. Prior research has shown that medical spending is sensitive to price, most notably in the RAND Health Insurance Experiment (Manning, Newhouse, Duan, Keeler, Leibowitz and Marquis 1987, Newhouse 1993, Aron-Dine, Einav and Finkelstein 2013). In this experiment, which ran from 1974 to 1981, two thousand families were assigned to insurance plans with different levels of coinsurance. Evaluating differences in utilization across families, researchers estimated the price elasticity of healthcare expenditures with respect to patient contribution to be -0.2.1 Because this estimate was determined experimentally, it has become the benchmark for price elasticities of various types of medical care.

Yet healthcare has changed in the nearly 35 years since the experiment was completed. Managed care plans emerged and now dominate the private insurance market (Baicker and Goldman 2011). These plans encourage patients to get their care from physicians within the plan’s network by setting lower copayments or coinsurance rates for use of in-network physicians. Additionally, some

---

*This section is co-authored with Jonathan Gruber. The authors acknowledge the assistance of the Health Care Cost Institute and its data contributors, Aetna, Humana, Kaiser Permanente, and UnitedHealthcare in providing the claims data analyzed in this study. We thank Sean Sall for excellent research assistance, Carolina Herrera for assistance with the data, and Robin McKnight for sharing code. Easterbrook gratefully acknowledges funding from the National Institute on Aging grant T32-AG000186.

1Aron-Dine et al. (2013) discuss the assumptions of this calculation and provide a range of estimates, from -0.04 to -.6, for the elasticity of spending implied by the RAND HIE.
plans require patients to see a primary care physician (PCP) to obtain a referral to a specialist, such that PCPs act as gatekeepers to the more expensive specialists. Such constraints could alter patients' sensitivity to the price they pay to see a physician.

Medical care itself has changed as well since 1981. For instance, prescription medications have become increasingly important, growing from 5-6% of health expenditures in the 1980s to 11-12% by the mid-2000s (Scott Morton and Kyle 2011). Since 1980, the efficacy of many prescriptions has improved, and they are now used to treat conditions that previously couldn’t be treated with prescriptions (Cutler 2004). Additionally, technology has altered the care for conditions in ways that often are less invasive for the patient. This has reduced inpatient days. Diagnostics have changed dramatically, with the introduction of the MRI and other imaging technologies. Treatments for heart attacks have also changed; aspirin wasn’t used to treat heart attacks until the 1980s, and stents were only developed in the 1990s (Cutler 2004). Cutler and McClellan (2001) argue the improved treatments in medical care have been worth the costs for many conditions. Such improvements could affect the structure of demand for healthcare. Particularly relevant for our setting, they might alter whether patients even see a physician to treat or manage a condition.

Hence it is worth assessing how sensitive medical utilization is to patient cost-sharing in a more recent setting. We study how the price of physician office visits impacts utilization. We use a difference-in-difference methodology, comparing changes in utilization in plans that change the price of an office visit for patients to changes in utilization in plans that didn’t change the price of an office visit. The price of an office visit to the patient is a copayment or coinsurance, and in our setting, most plans have patients contribute a copayment for the office visit. Throughout, we will refer to the copay as the “price” of the office visit since we focus on how the price for the patient affects utilization.

We use data from 1556 employer-sponsored insurance plans with 1.5 million enrollees from 2007-2011. Our sample contains 171 changes in copays for office visits that occurred in 166 different plans and we use to estimate the effect of patient cost-sharing. Individuals with employer-sponsored insurance plans comprise roughly 55% of the US population. These are generally working-age

---

2 Cutler (2004) discusses the example of SSRIs, medications currently used to treat depression, which only became available in the late 1980s. They have fewer side effects than earlier anti-depressants. Additionally, the first statin, a prescription to help patients manage high cholesterol, was approved in 1987, and as of 2004, statins were the most commonly prescribed medication in the US.

3 For instance, in the early 1980s, cataract surgery was an inpatient procedure, requiring several days in the hospital; by the end of the decade, it was an outpatient procedure (Barro and Cutler 1997).

adults and their dependents, and they earn enough not to qualify for state-subsidized insurance.\footnote{During this time, state-subsidized insurance—Medicaid—mainly covered low-income children and pregnant women.} This is the same demographic that participated in the RAND health insurance experiment (RAND HIE), though the RAND HIE also included low-income individuals.

We find that the number of office visits declines when copays increase, and our estimates imply that the elasticity of office visit spending is approximately -0.13 with respect to patient price. Patients with chronic conditions appear to be less sensitive to price than patients without chronic conditions; the estimated elasticities for patients with and without chronic conditions are -0.11 and -0.15. However, we can't reject that office visit spending decreases by the same percentage for these two groups of patients. We also find evidence that individuals are more sensitive to the price of a PCP office visit than the price to see a specialist.

We investigate how copays for office visits affect other categories of medical spending to see if a reduction in office spending compounds in the medical system or is offset. During office visits, physicians may order diagnostic tests or prescribe medications. Office visits could also lead to additional treatments. A reduction in office visits could hence lead to lower spending in other medical categories. Additionally, if patients reduce office visits for medical conditions requiring care, patients may increase spending on other types of care, such as ER visits and inpatient spending. It’s possible that reduced physician visits leads to offsets in some categories of spending and compounding in others; the effect on total spending indicates whether offsets or compounding dominates.

The results on compounding and offsets are speculative since we are unable to control for any price changes in other spending categories. Yet they suggest that the lower spending on office visits compounds. Our estimates imply a copay increase leads to a larger reduction in total spending than office visit spending. Expenditures on laboratory tests and radiology decline following an increase in the office visit copay, and there is some evidence that outpatient and prescription drug expenditures also decline. We find no evidence of offsets. In particular, we don’t observe statistically significant increases in ER expenditures and inpatient expenditures after office visits decrease.

This research contributes to the literature on the elasticity of medical services. The RAND HIE is the seminal work in this field and, as mentioned, estimates an elasticity of -0.2. Since then several studies have investigated how individuals respond to changes in office visit prices. Cherkin, Grothaus and Wagner (1989) study how a $5 increase in copay in one HMO plan in 1985 affected
office visits. They find a 10.9% decrease in primary care visits and a 3.3% decrease in specialist visits. Our study is similar in spirit and design to theirs. We are able to provide updated results, and our sample, with 1556 plans and 171 price changes has greater breadth, enhancing the external validity of our estimates.

Other studies have used international settings when studying how office visits or outpatient care responds to price changes (Van De Voorde, DoorSlaer and Schokkeart 2001, Farbmacher and Winter 2013, Shigeoka 2013). Within the US, Chandra, Gruber and McKnight (2010) examine how copays affect office visits among the elderly. Chandra, Gruber and McKnight (2014) study the utilization of the low-income population in Massachusetts and find a simultaneous increase in copays for multiple services led to reduced office visit spending. These studies focus on different populations than the current study.

Several studies have found evidence for offsets in the medical system. Chandra et al. (2010), Trivedi, Mosloo and Mor (2010), and Tamblyn, Laprise, Hanley, Abrahamowicz, Scott, Mayo, Hurley, Grad, Latimer, Perreault, McLeod, Larochelle and Mallet (2001) find offsets among the elderly population. Tamblyn et al. (2001) also finds evidence for offsets among low-income individuals, and Chandra et al. (2014) report suggestive evidence for offsets among low-income individuals with chronic conditions. However, the evidence on offsets among the working-age population who aren’t low-income is more mixed. The RAND HIE did not find evidence for any offsets among the non-elderly. Rather, reduced primary care seemed to lower hospital spending (Newhouse 1993). Gaynor, Li and Vogt (2007) find that when copays for prescriptions increased in employer-sponsored insurance plans, prescription use declined, but outpatient care increased. In this study, we can examine the reverse scenario—whether individuals increase prescription use when a particular type of outpatient care, office visits, became more expensive.

The following section discusses the data for this study, followed by the methodology in Section 3.3. Section 3.4 presents the main results for both office visits as well as offsets and compounding. Section 3.5 describes robustness tests, and Section 3.6 concludes.

### 3.2 Data

To understand how cost-sharing affects healthcare utilization, we need to know individuals’ utilization and the copays or coinsurance rates they faced. For identification, we need to observe changes in copays or coinsurance rates and utilization for multiple years. All our data are from the
Health Care Cost Institute (HCCI). HCCI maintains a database of insurance claims and enrollment data from three major insurers for many of their employer-sponsored insurance plans. These data represent the healthcare expenditures of working-age adults and their dependents from across the US. We use data from 2007 through 2011, and each year contains information for over 40 million individuals. From these data, we are able to measure utilization and are able to determine annual copays and coinsurances in many plans. First, we'll discuss our measures of utilization, and second, we'll explain how we determined copays for office visits.

3.2.1 Utilization Measures

We estimate how physician copays affect a variety of utilization measures, including office visit spending and use as well as their effect on other categories of care. In the claims data, we identify office visits in two ways. First, HCCI has categorized a set of current procedural terminology (CPT) codes that appear in the claims as office visits. We can use these claims to determine office visit spending and use. Each claim contains a patient identifier and the total amount paid to the provider, enabling us to measure individuals' total office visit spending over a year. The claims also include the type of provider, allowing us to separately measure office visit spending for PCPs and specialists. The claims provide the dates of service and encrypted provider ids that enable us to count office visits. Since it's possible for a single office visit to generate multiple claims, we consider all claims for a single patient with the same date of service and unique provider id to originate from one office visit.

Our second measure of office visit spending is broader. It includes all claims that HCCI has labeled as office visits, as above, as well as any claims with the place of service as an office that weren't classified as “Emergency Care,” “Labs/Pathology,” or “Radiology.” This measure is closer to the one used in Chandra et al. (2014), and we'll use this as our primary measure for calculating elasticities. In the results, we refer to this measure as “all office spending” or “all physician office spending” whereas the narrower measure is “office visit spending.”

To study offsets and compounding, we examine changes in total expenditures and expenditures for other types of care, including inpatient and outpatient procedures, emergency room (ER) care, labs, and prescription medications. Since we have complete sets of claims for individuals while they are enrolled in a participating insurance plan, we can calculate total spending.

---

6CPT codes are used to document procedures in insurance claims. HCCI's 2013 Health Care Cost and Utilization Report, Analytic Methodology V3.3 from October 28, 2014 describes the CPT codes used for the classification. It can be downloaded from http://www.healthcostinstitute.org/methodology.
We calculate inpatient spending based on the inpatient facility fees. We measure outpatient visit spending as the facility fees associated with outpatient surgery and observation, following HCCI's definition of outpatient visits. Inpatient and outpatient fees can be distinguished from one another since inpatient facility fees contain an admission code, indicating the patient was admitted to the hospital. These measures are only indicative of changes in inpatient and outpatient visit spending since they do not include physician charges. However, they provide conservative estimates of how inpatient and outpatient care is affected.

We calculate ER spending as all facility charges for ER visits, all care HCCI classifies as “Emergency Room” based on CPT codes, and all care provided in an emergency room. Lab spending includes all spending classified as laboratory, pathology, or radiology, and all charges submitted by independent laboratories, following Chandra et al. (2014). HCCI separately identifies claims for prescriptions, and we use their classification.

3.2.2 Determining Plan Copays

To investigate how cost sharing affects utilization, we also need information on the copay or coinsurance rate for office visits. The data doesn’t directly include plans’ cost-sharing structures nor do we know in exactly what plan an individual is enrolled. We back out both plan enrollment and cost-sharing from the claims data in a process described in more detail in the Technical Appendix.

To briefly summarize the algorithm we use, the data tell us the firms through which individuals received insurance and broad characteristics of the selected insurance plan. We know the insurance product type (health maintenance organization or HMO, preferred provider organization or PPO, etc.) and whether the plan is a high-deductible plan and covers prescriptions and mental health. It’s possible that a firm could offer multiple plans that all have the same broad plan characteristics, so the algorithm determines firms and sets of broad plan characteristics that are highly likely to contain only one plan. We restrict our analysis these firms and sets of broad characteristics.

Within each firm and set of broad plan characteristics, we determine the most common amount people have paid as part of a deductible as well as the most common copay or coinsurance for

---

7 Facility fees are charges a medical center submits to an insurance plan and are distinct from claims that physicians may submit for care provided to a patient while admitted to a hospital. For instance, if a patient stays overnight at a hospital following a surgery, the hospital will submit a facility claim to the patient’s insurer, and the surgeon may submit a separate claim.

8 HCCI also considers facility fees for ER visits as outpatient visits; we examine this separately. They classify different types of outpatient facility fees based on CPT codes.

9 The difference among types of insurance plans (HMOs, PPOs, etc.) is often how they structure their networks of physicians and medical centers and how much coverage they provide for use of out-of-network physicians and facilities.
primary care and specialist office visits. Each claim separately shows the deductible, copay, and coinsurance for the service. We impose the following restrictions: 90% of individuals, who are in the same firm and enrolled in plan(s) with the same broad characteristics, must have $0 deductible spending. Additionally, 90% of copays or coinsurances for PCP and specialist office visits within a firm and set of broad plan characteristics must be the same within that category. When these criteria are met, we are comfortable concluding that all the people in that firm and enrolled in a plan with the same broad characteristics are actually part of the same health insurance plan. We refer to this set of firms and broad plan characteristics as “plans.” We set the most common copay or coinsurance rate for physician visits as the plan's copay or coinsurance for an office visit. In practice, most plans have copays for PCP and specialist office visits.

3.2.3 Sample Restrictions

Our analysis examines year-to-year changes in average plan spending and use per enrollee. Before we compute average plan utilization, we restrict to individuals who are enrolled in the plans for the two consecutive years. This reduces the number of individuals in our sample by approximately 30%. People may not be enrolled in the same plan for two full years for many reasons, including being hired or fired or switching plans or jobs.\textsuperscript{10}

Additionally, we discard individuals when their year-to-year change in total spending is below the 2.5th percentile or above the 97.5th percentile of all year-to-year changes. Such large fluctuations are most likely due to changes in health conditions, but the magnitude of them can skew average measures. In practice, this restricts the analysis to individuals whose year-to-year change in total spending is between a $12,511 decrease and a $15,056 increase. After computing the plan averages, we discard plans whose change in average spending or log average spending is in the 1st or 99th percentiles. This leaves us with 1556 insurance plans with approximately 1.5 million enrollees, many of which appear in multiple pairs of consecutive years.

3.3 Empirical Strategy

We employ a difference-in-difference strategy to estimate how medical utilization responds to changes in physician visit copays. In our final sample, there are 171 instances of plans changing their PCP or specialist copay from one year to the next. We compare year-to-year changes in

\textsuperscript{10}With employer-sponsored insurance, often people can only switch plans once per year unless they have a qualifying event, such as marriage or the birth of a child.
utilization in plans that changed their copays to the changes in utilization among plans in which the price of office visits remained constant. We pool all pairs of consecutive years and estimate:

\[ \Delta y_{pt} = \beta \Delta c_{pt} + X_{pt} + \gamma_{t} + \varepsilon_{pt} \]  

(3.1)

\( \Delta y_{pt} \) is the average change in outcome measure \( y \) per enrollee in plan \( p \) between years \( t \) and \( t+1 \). \( \Delta c_{pt} \) is the change in physician copay over that same period. \( \beta \) is the coefficient of interest, measuring how the price change affects the utilization measure. 101 of the 171 plans change their PCP and specialist copays by the same amount, so we combine the two price changes in one measure of physician copay. \( \Delta c_{pt} \) is the weighted average of the change in the PCP copay and specialist copay, where the weights are the average number of annual visits to PCPs and specialists in the baseline year (year 1) across the entire sample.

In Equation 3.1, \( X_{pt} \) control for differences among plans that may be correlated with the plan's decision to change its office visit copay. \( X_{pt} \) includes dummies for the type of insurance product and whether the plan has mental health and prescription drug coverage. It includes the log of average member spending in the baseline year, log of average member spending on office visits, log of the number of people in the plan, the percentage of enrollees that are female, and the percentage of enrollees who fall in 5-year age bins. \( \gamma_{t} \) are year dummies for year 1 since we pool all year-to-year changes. \( \varepsilon_{pt} \) is the error, clustered at the plan level since many plans appear in more than two consecutive years and utilization within a plan is likely autocorrelated. Observations are weighted by the number of enrollees that appear in the two consecutive years.

The key assumption in this model is that absent the change in physician visit copays, the year-to-year change in utilization would have been the same among the plans. In a robustness check, we will evaluate the plausibility of this assumption with a subset of plans.

Since we limit our analysis to individuals who remain in the same plan for two consecutive years, \( \beta \) captures the change in utilization among individuals who don’t switch plans. If people who are more sensitive to the price of an office visit switch plans upon learning of a price increase, we may underestimate the effect. However, in practice this is not too concerning. Handel (2013) documents substantial inertia in plan choice among individuals with employer-sponsored health insurance. Although we lose a sizable part of our sample when we restrict to individuals who are in the same plan for two consecutive years, over 80% of individuals who are enrolled in one of the plans for the full twelve months in the baseline year are enrolled in the same plan for twelve months in the
following year. In our sample, plans that change office visit changes, the vast majority of which are increases, do not have lower retention the following year than plans without price changes. 83.8% of individuals in plans that change the price of office visits remain in the plan for twelve months in the following year; 81.0% of individuals in plans that don’t change the price of office visits do. The difference is not statistically significant.

A limitation of the methodology is that we are uncertain whether the changes in physician copays correspond with similar changes in copays or coinsurances for other types of care. If plans often change cost-sharing arrangements for multiple services at once, our estimates will reflect the change in utilization due to both the change in office visit copay and other cost-sharing adjustments. This is particularly problematic when we examine utilization of ER visits, prescriptions, labs, and inpatient and outpatient care. We will partially address this limitation in a robustness check.

3.4 Empirical Analysis

3.4.1 Plan Characteristics

Figure 3-1 shows the distributions of copays for PCP and specialist office visits in the baseline year. Since we pool all years of data and calculate changes in utilization, year 1 represents 2007-2010. The median and modal price for a PCP office visit is $15, and the median and modal price for an office visit to a specialist is $20. The final graph in Figure 3-1 shows a combined price for office visits, which we construct as the weighted average of the PCP and specialist prices. The weights are the average number of PCP and specialist office visits in year 1 in the entire sample. The combined price falls between $15 and $20 for 33% of plans.11

Figure 3-2 shows the distributions of price changes for PCP and specialist visits as well as the price change of the combined price. To better illustrate the distribution of the price changes, these graphs exclude observations from plans that do not change prices. The first two graphs illustrate that plans tend increase both their PCP and specialist copays by $5. Hence, the analysis focuses on the combined price change for office visits.

Table 3.1 displays summary statistics for the plans, divided into the “treatment” plans which experience copay changes and “control” plans that do not. The statistics in the table represent the weighted averages among the plans in year 1 of the analysis, where the weights are the number of

---

11 Figure 3-1 excludes 39 plans that use coinsurances for PCP and specialist office visits. The median coinsurance rate is 20% for both PCP and specialist office visits. None of these plans change their coinsurance rate for doctor office visits.
enrollees for two consecutive years.\textsuperscript{12}

Table 3.1 illustrates that treatment plans are more likely to be EPOs and less likely to be PPOs. Most other plan and enrollee characteristics are similar between treatment and control plans prior to the change in copay. Additionally, average enrollee utilization is similar between treatment and control plans. With the exceptions of inpatient and outpatient spending, expenditures in various categories are not statistically different nor are the number of office visits to PCPs or specialists. This table provides some support for the assumption that utilization in the plans would have trended similarly absent the copay changes. While the assumption relates to the trends in utilization, the fact that plans have similar utilization levels prior to the copay changes lends credence to the assumption.

3.4.2 Effect on Office Visits

Tables 3.2 and 3.3 presents the main results on office visits. Each column contains the $\beta$ estimate from Equation 3.1 for a different measure of utilization. Table 3.2 shows that spending on office visits decreased in response to copay increases. The estimate in the first column indicates that a $1 increase in physician copays caused a $4.24 decrease in all physician office spending, the broader of the two office spending measures. It implies an elasticity of -0.13 with respect to the spot price of office visits. The second column shows the change in office visit spending based on our narrower definition. Its implied elasticity of -0.12 is nearly identical. The elasticities are slightly smaller than the elasticity found in the RAND HIE, suggesting the elasticity of office visits may have declined slightly since the 1970s. Our estimate is similar to the elasticity of -0.15 for low income individuals found in Chandra et al. (2014).

Table 3.3 displays the results on office visit use. Columns 1-3 show the effect of the copay change on the average number of office visits. Given the average copay changed by $5.20, the coefficient of -0.014 on office visits to PCPs or specialists implies a change of 0.073 in the number of office visits. Since most copay changes were increases, this represents a 3% decline in office visits. This is smaller than the response in Cherkin et al. (1989), which estimates a reduction of 10.9% for PCP visits and 3.3% for specialist visits following an increase in copay from $0 to $5. However, the larger response in their setting isn’t necessarily surprising given the much greater percentage increase in copay. Our implied elasticity of office visits is -0.10, which is the same as the arc elasticity for office visits among the elderly in Chandra et al. (2010).

\textsuperscript{12}The statistics, including the p value, are identical those computed for across all individuals in the sample.
Columns 4-6 show the decline in use is at least in part due to adjustments on the external margin. The percentage of individuals who see a PCP or specialist during the year declines by 0.11 percentage points for every $1 increase in copay. Given approximately 66.3% of enrollees saw a physician in the baseline year, this represents less than a 1% change.

The results suggest that individuals are more price sensitive for office visits to a PCP than a specialists. The results in column 3 of Table 3.2 imply that the average $5.20 change in copay led to a $5.29 change in PCP spending, representing a 4.1% change. Column 4 indicates that specialist spending decreases by $0.32 for every dollar increase in copay. The average copay change of $5.20 caused a 2.9% change in specialist spending. We cannot compute separate elasticities because the combined price represents a weighted average price for PCP and specialist office visits, but among plans that changed copays, the average PCP copay changed by 28.9% and the average specialist copay changed by 32.6%, suggesting a smaller elasticity for specialist office visits.

The results in Panel B of Table 3.2, where the dependent variable is specified in logs, are even more telling. Log PCP office visit expenditures fell by more than log specialist office visit expenditures; a Wald test allows us to reject the coefficients are the same with 99% confidence. A Wald test also allows us to reject that log PCP office visits and log specialist office visits, shown in columns 2 and 3 of Table 3.3, fell by the same amount with 95% confidence.

We separately investigate how individuals with and without chronic conditions respond to the change in copays. Following Chandra et al. (2014), we classify individuals as having a chronic condition if they receive a diagnosis of hypertension, high cholesterol, asthma, diabetes, arthritis, affective disorders, or gastritis during an office visit in the baseline year. We compute average utilization within a plan separately for individuals with and without chronic conditions. Tables 3.4 and 3.5 show how office visit utilization changed for patients with and without chronic conditions. Panel A in each table shows a $1 increase in copay corresponds with a larger decline in office visit utilization among patients with chronic conditions for every measure of utilization. The estimated elasticities for office spending are -0.15 for patients without chronic conditions and -0.11 for patients with chronic conditions, suggesting chronically ill individuals are less sensitive to price. However, Panel B in each table, which displays results when change in log utilization is the outcome measure, suggests the percentage change in utilization is not statistically different.

---

13 The Wald tests are based on regressions that do not include the percentage of people in five-year age bins. These controls are highly collinear. As will be seen in the robustness tests, the estimates are not very sensitive to the sets of controls used.
between the patients with and without chronic conditions.\textsuperscript{14} Since patients with and without chronic conditions experience similar average price changes, the evidence isn’t conclusive as to whether chronically ill patients are less sensitive to price.

3.4.3 Offsets and Compounding Effects

We investigate whether changes in office spending have offsets or compounding effects in other medical spending categories. Since office visits are often an entry point into the medical system, other types of spending, such as that on diagnostic tests or prescriptions, could decline. Spending in other categories of care might increase, though. In particular, if individuals reduce care for conditions that require physician treatment (as opposed reducing care related to conditions that simply require rest), emergency room and inpatient spending could increase.

The results in this section, however, are speculative since we are uncertain whether patient cost-sharing changes for other types of spending. If plans tend to change multiple prices at once and other types of medical care are sensitive to price, we overestimate the effect of a change in physician visit copay.

Table 3.6 shows $\beta$ from Equation 3.1 when $y$ represents various types of medical spending. The estimate in column 1 implies that total spending deceased by $8.93$ for every $1 increase in copay, which translates to an elasticity of $-0.076$. This is comparable to the elasticity for office visit spending, though it is smaller.

That the effect on total spending has a greater magnitude than the effect on office spending suggests that an increase in office visit copays led to declines in spending elsewhere. The results in columns 2-6 of Table 3.6 indicate that copay increases led to declines in spending on lab tests and may also have affected spending on prescriptions and outpatient visits. To the extent these decreases were the result of fewer doctor office visits, the increases in physician copays had compounding effects in the medical system.

There is little evidence of any offsets. The estimates imply increases in physician visit copays did not lead to statistically significant increases in spending in any category.

Table 3.7 shows the results when we examine offsets and compounding for patients with and without chronic conditions. Panel A shows how expenditure levels change with a change in physician visit copay, and Panel B show the change in log utilization. As with the results on office visits, the

\textsuperscript{14}The one exception here is the percentage of people who see PCPs. As above, the Wald statistics are computed based on regressions without the percentage of people in five-year age bins.
effect is larger for patients with chronic conditions, but the percentage change isn’t significantly different between the two groups of patients. We find evidence that the effects of an increase in physician copay compound for both patients with and without chronic conditions and no evidence for any offsets.

Perhaps most striking in these figures is how much patients with chronic conditions reduced prescription drug expenditures when copays increased. Two of the conditions that classify patients as having a chronic condition are hypertension and high cholesterol. Since prescriptions are often used to manage these conditions, a decline in prescriptions might worsen health outcomes.

### 3.5 Robustness Checks

In this section, we address two potential threats to our identification strategy, specifically whether the identifying assumption is reasonable for our analysis and whether price changes of other medical services bias our results. Our analyses, which only include a subset of the plans, don’t suggest either is problematic in our setting. The more limited sample, though, prevents us from drawing conclusions. Additionally, we check that our results are robust to adjustments in our sample and different sets of control variables.

#### 3.5.1 Evaluating the Identifying Assumption

The identifying assumption of the difference-in-difference analysis is that all plans in the sample would have had similar changes in utilization had none of the plans changed their physician visit copay. To check whether this assumption is reasonable, we examine pre-trends in an event study using a subset of the plans that appear in four or more consecutive years. This reduces the number of plans in our analysis to 223, 47 of which increase copays at some point during the sample. Additionally, this analysis only includes plans that increased copays once or not at all. The average utilization of each plan is computed using a balanced panel of individuals.

The event study specification is:

\[
y_{pt} = \sum_{k=-2}^{-1} \beta_k [YAC_{pt} = k] + \sum_{k=1}^{3} \beta_k [YAC_{pt} = k] + \gamma_t + \alpha_p + \varepsilon_{pt}
\]  

(3.2)

\(YAC_{pt}\) denotes how many years have passed since the copay changed. If the plan increased its copay at some point in the sample, \(YAC_{pt}\) is equal to 1 the year the copay changed, 0 the year
before the increase, -1 two years before increase, etc. \( \mathbb{I}[YAC_{pt} = k] \) is an indicator for each year before or after the copay change. The \( \beta_k \)'s capture the average effect on \( y_{pt} \) years after the copay changed.\(^{15}\) We normalize \( \beta_0 \) to be 0. \( \alpha_p \) are planned fixed effects. As before, \( \varepsilon_{pt} \) is clustered at the plan level, and plans are weighted by the number of enrollees within this sample.

We plot the \( \beta_k \)'s in graphs to evaluate the pre-trends in Figure C-1. The figures show very little evidence of pre-trends in number of office visits, all office spending, and total spending prior to the copay increases. Among this subset of plans, this provides empirical support for the identifying assumption.

The decreases in utilization after the copay increases are consistent with our main results, but the continued decline raises the question as to what caused the decline two and three years after the copay increase. There are several potential explanations. It is possible there could be continued adjustment to the copay increase, particularly for individuals who only see a physician one or two times per year. The continued decline could also be due to contemporaneous or further adjustments in the insurance plans. The composition of the sample three years after the price change could affect the results; we only observe 14 plans three years after a copay increase. Of these, only contemporaneous changes in the insurance plans would be problematic for our estimates. Finally, given the large confidence intervals three and four years after the copay increases, it is not certain utilization continued to decline.

3.5.2 Evaluating Bias from Other Price Changes

Another potential threat to our identification strategy is that we cannot control for changes in other copays and coinsurance rates because we do not know the relevant prices. If plans tend to change the cost-sharing arrangements of multiple services at once, our estimated effect of the copay change captures both the effect of the change in the office visit copay and changes in other patient prices. This is most concerning for our results on offsets and compounding effects.

To explore whether this might impact our estimates, we reestimate Equation 3.1, limiting the sample to plans in which we can determine the inpatient price and the price of outpatient surgery.\(^{16}\) The inpatient and outpatient prices do not change among this sample. This reduces our sample to 207 plans and 19 office visit copay changes. 92 of remaining plans appear in multiple pairs of

\(^{15}\)YAC_{pt} only indicates that the copay changes at some point, but doesn’t depend on the magnitude of the copay change. Therefore, including plans that both increase and decrease copays in the estimation would bias the \( \beta_k \)'s toward 0. To resolve this issue, we exclude 2 plans that decrease their copays.

\(^{16}\)In both cases, we only know the patient’s contribution toward facility fees.
consecutive years. We also reestimate the effect of the copay changes separately for patients with and without chronic conditions.

Table C.1 shows the results for level changes in office utilization and other types of medical utilization. For brevity, it just shows level measures and among the office visit utilization measures, just the change in office spending (the broader of our two spending measures) and number of office visits. In this estimation, the standard errors increase such that it is difficult to draw conclusions, but we cannot reject that the estimated coefficients with the full sample are not the same as from the ones in Tables 3.2, 3.3 and 3.6. Additionally, the magnitudes of the coefficients are similar as with the full sample. Panels B and C of Table C.1 show the results for the subsamples of patients without and with chronic conditions. Once again, we cannot reject that the coefficients on change in copay aren’t the same as the coefficients in Tables 3.4, 3.5 and 3.7.17 This provides some evidence that changes in inpatient and outpatient prices are not driving the results.

Among the sample of patients with chronic conditions, the positive coefficients on prescription spending and ER spending suggest there could be offsets within some plans. However, this evidence is quite weak. Not only are the coefficients not statistically different from 0, but we still are unable to control for prescription drug prices and ER prices.

3.5.3 Checks on the Sample and Control Variables

We also test whether our results are sensitive to the cutoffs chosen for our sample. Discarding individuals and plans with extreme changes in total spending helps ensure that outliers aren’t driving the results. However, the cutoffs in the main results were not too restrictive as we want the estimates to apply to a large population. To check whether and how individuals and plans with large changes in total spending affect the results, we reestimate Equation 3.1 using stricter cutoffs. Specifically, we discard individuals whose change in total spending was in the 5 highest or lowest percentiles, and plans whose average change in total spending or log total spending was in the 5 highest or lowest percentiles.

The results are shown in Table C.2. The results are very similar to the main results, suggesting that magnitudes of our estimates are not due to outliers in our initial sample. Interestingly, with this sample, we find evidence of offsets. Inpatient spending increases for the full sample when physician copays increase, driven by an increase among patients with chronic conditions.

Finally, we also test the sensitivity of our results to different set of controls. Table C.3 shows the

---

17The statistical tests don’t use age-bin controls since those controls are highly collinear.
results for the full sample, Table C.4 contains the results when examining patients without chronic conditions, and Table C.5 presents the results for the sample of patients with chronic conditions. The results are robust to different sets of controls.

### 3.6 Conclusion

As health insurance plans seek to reduce the growth in their expenditures, many may increase the costs to patients for given services. Proponents suggest that this limits “moral hazard” in the medical system; since the marginal cost of care is much lower for the insured patient than the whole system, the patient has incentive to overuse medical services. However, it is important to understand whether higher cost-sharing causes patients to reduce necessary care, leading to increased spending or worse health outcomes.

This paper estimates the price elasticity of office visits and examines whether a price increase for office visits leads to offsets in the medical system. The contribution of this paper is to provide updated estimates for the population with employer-sponsored insurance as the settings for prior estimates in the RAND HIE and Cherkin et al. (1989) occurred over thirty years ago. To estimate the effects of office visit copays, we use an extensive dataset from HCCI. Our sample has over 1500 insurance plans with 1.5 million enrollees, and we leverage variation from 171 changes in office visit copays.

We find that office visit spending has an elasticity of -0.13, which is slightly lower than, but comparable to the famous estimate of -0.2 from the RAND HIE. We also find some evidence that patients with chronic conditions are less sensitive to price than patients without chronic conditions, but we cannot reject their spending declines by the same percentage. Additionally, we find individuals are more sensitive to the price of seeing a PCP than a specialist.

We find limited evidence for offsets in this population. Following a price increase for office visits, the decline in total expenditures exceeds the decline in office visit spending. Reduced office visits coincides with less spending on labs, and possibly prescriptions and outpatient visits. The results on offsets and compounding are speculative as we are unable to control for prices of other services, but they are sensible. Physicians often prescribe medications or order tests during office visits.

A remaining question is whether the reduced care leads to worse health outcomes. The limited evidence on offsets in inpatient or ER spending suggests that reduced care may not be immediately harmful for the population with employer-sponsored insurance, though the more mixed results for
chronically ill patients suggests this subset may be harmed. Further research is needed to establish whether there are health consequences from reduced care. Combined with information on the effects of cost-sharing on utilization, it would enable us to assess the costs and benefits of increased cost-sharing.
These graphs show the distribution of office visit copays in the final sample of plans. Plans can appear more than once if they appear in more than one pair of consecutive years. The graphs exclude 39 plans in the final sample that had coinsurances for PCP and specialist office visits. The final graph shows the distribution of the weighted average of PCP and specialist prices, where the weights are the average number of PCP and specialist visits in year 1 of the sample. See the text for more detail on how plan copays were determined and the outliers that were excluded.
Figure 3-2: Distribution of Copay Changes among Plans that Changed Copays

These graphs show the distribution of year-to-year copay changes among plans that changed copays. The final graph shows the distribution of the weighted average of PCP and specialist copay changes, where the weights are the average number of PCP and specialist visits in year 1 of in the sample. See the text for more detail on how plan copays were determined and outliers that were excluded.
Table 3.1: Summary Statistics for Plans in Baseline Year

<table>
<thead>
<tr>
<th>Panel A: Plan Characteristics and Enrollee Demographics</th>
<th>Weighted Average Control</th>
<th>Treatment</th>
<th>Difference</th>
<th>P Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>EPO Plans</td>
<td>42.34%</td>
<td>60.41%</td>
<td>0.181</td>
<td>0.067</td>
</tr>
<tr>
<td>HMO Plans</td>
<td>40.30%</td>
<td>33.00%</td>
<td>-0.073</td>
<td>0.433</td>
</tr>
<tr>
<td>Indemnity Plans</td>
<td>0.69%</td>
<td>0.00%</td>
<td>-0.073</td>
<td>0.138</td>
</tr>
<tr>
<td>POS Plans</td>
<td>9.69%</td>
<td>6.30%</td>
<td>-0.073</td>
<td>0.330</td>
</tr>
<tr>
<td>PPO Plans</td>
<td>6.97%</td>
<td>0.29%</td>
<td>-0.067</td>
<td>0.002</td>
</tr>
<tr>
<td>Plans with Rx Coverage</td>
<td>56.04%</td>
<td>58.68%</td>
<td>0.026</td>
<td>0.825</td>
</tr>
<tr>
<td>Plans with Mental Health Coverage</td>
<td>90.74%</td>
<td>94.70%</td>
<td>0.040</td>
<td>0.253</td>
</tr>
<tr>
<td>Copay for Physician Visit*</td>
<td>16.79</td>
<td>17.37</td>
<td>0.572</td>
<td>0.627</td>
</tr>
<tr>
<td>Price Change</td>
<td>0.00</td>
<td>5.12</td>
<td>5.119</td>
<td>0.000</td>
</tr>
<tr>
<td>% Female</td>
<td>51.12%</td>
<td>51.32%</td>
<td>0.002</td>
<td>0.795</td>
</tr>
<tr>
<td>Avg Age</td>
<td>31.71</td>
<td>31.33</td>
<td>-0.380</td>
<td>0.661</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Utilization Measures</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Expenditures</td>
<td>2028.70</td>
<td>2029.42</td>
<td>0.727</td>
<td>0.995</td>
</tr>
<tr>
<td>Outpatient Visit Facility Expenditures</td>
<td>213.06</td>
<td>195.56</td>
<td>-17.494</td>
<td>0.097</td>
</tr>
<tr>
<td>Inpatient Facility Expenditures</td>
<td>149.24</td>
<td>133.76</td>
<td>-15.477</td>
<td>0.026</td>
</tr>
<tr>
<td>ER Expenditures</td>
<td>179.33</td>
<td>184.04</td>
<td>4.706</td>
<td>0.698</td>
</tr>
<tr>
<td>Rx Expenditures**</td>
<td>638.58</td>
<td>643.62</td>
<td>5.041</td>
<td>0.905</td>
</tr>
<tr>
<td>Lab Expenditures</td>
<td>291.63</td>
<td>297.20</td>
<td>5.567</td>
<td>0.777</td>
</tr>
<tr>
<td>All Physician Office Expenditures</td>
<td>548.02</td>
<td>566.02</td>
<td>17.998</td>
<td>0.476</td>
</tr>
<tr>
<td>Office Visit Expenditures</td>
<td>177.94</td>
<td>186.69</td>
<td>8.749</td>
<td>0.341</td>
</tr>
<tr>
<td>PCP Office Visit Expenditures</td>
<td>119.62</td>
<td>128.03</td>
<td>8.409</td>
<td>0.245</td>
</tr>
<tr>
<td>Specialist Office Visit Expenditures</td>
<td>58.32</td>
<td>58.66</td>
<td>0.340</td>
<td>0.911</td>
</tr>
<tr>
<td>Office Visits to PCP or Specialist</td>
<td>2.41</td>
<td>2.41</td>
<td>-0.005</td>
<td>0.955</td>
</tr>
<tr>
<td>PCP Office Visits</td>
<td>1.67</td>
<td>1.68</td>
<td>0.013</td>
<td>0.851</td>
</tr>
<tr>
<td>Specialist Office Visits</td>
<td>0.74</td>
<td>0.72</td>
<td>-0.019</td>
<td>0.616</td>
</tr>
</tbody>
</table>

N = 2328  171

The summary statistics represent the weighted average among plans in year 1, where the weights are the number of enrollees. The p value is calculated based off a t statistic when the variable of interest is regressed on a treatment indicator. In the regressions, standard errors are clustered at the plan level, and observations are weighted by enrollees. See text for more detail.

* Excludes the 39 plans that have coinsurances for doctor visits
** Only includes plans that have prescription drug coverage
Table 3.2: Change in Office Visit Expenditures Following Change in Doctor Visit Copay

<table>
<thead>
<tr>
<th>All Physician Office Expenditures</th>
<th>Office Visit Expenditures</th>
<th>PCP Office Visit Expenditures</th>
<th>Specialist Office Visit Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-4.240</td>
<td>-1.340</td>
<td>-1.017</td>
</tr>
<tr>
<td></td>
<td>(0.662)***</td>
<td>(0.231)***</td>
<td>(0.177)***</td>
</tr>
<tr>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
</tr>
</tbody>
</table>

Panel A: Dependent Variable Specified in Levels

Panel B: Dependent Variable Specified in Logs

This table shows the coefficient on the change in copay from regressions of change in the plan's average office visit expenditures per enrollee on change in copay. Expenditures reflect both patient and insurer contributions. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, the percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees, and standard errors are clustered by plan. "All physician office expenditures" is a broader measure of office visit spending than "office visit expenditures." Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

*10% significance; ** 5% significance; *** 1% significance
Table 3.3: Change in Office Visit Use Following Change in Doctor Visit Copay

<table>
<thead>
<tr>
<th></th>
<th>Office Visits to PCP or Specialist</th>
<th>PCP Office Visits</th>
<th>Specialist Office Visits</th>
<th>% Seeing a PCP or Specialist</th>
<th>% Seeing a Specialist</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.014 (0.002)***</td>
<td>-0.010 (0.002)***</td>
<td>-0.004 (0.001)***</td>
<td>-0.00108 (0.00026)***</td>
<td>-0.00128 (0.00029)***</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
</tr>
</tbody>
</table>

Panel A: Dependent Variable Specified in Levels

This table shows the coefficient on the change in copay from regressions of change in the plan's average office visit expenditures per enrollee on change in copay.

The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, the percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees, and standard errors are clustered by plan. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

*10% significance; ** 5% significance; *** 1% significance

Panel B: Dependent Variable Specified in Logs

<table>
<thead>
<tr>
<th>Change in Copay</th>
<th>-0.00556 (0.00084)***</th>
<th>-0.00633 (0.00089)***</th>
<th>-0.00282 (0.00181)</th>
<th>-0.00159 (0.00041)***</th>
<th>-0.00254 (0.00063)***</th>
<th>-0.00101 (0.00122)</th>
</tr>
</thead>
<tbody>
<tr>
<td>N</td>
<td>2497</td>
<td>2492</td>
<td>2496</td>
<td>2497</td>
<td>2492</td>
<td>2496</td>
</tr>
</tbody>
</table>
### Table 3.4: Change in Office Visit Expenditures for Patients with and without Chronic Conditions

<table>
<thead>
<tr>
<th>Panel A: LEVEL REGRESSIONS</th>
<th>All Physician Office Expenditures</th>
<th>Office Visit Expenditures</th>
<th>PCP Office Visit Expenditures</th>
<th>Specialist Office Visit Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td></td>
</tr>
<tr>
<td><strong>Panel A1: Patients without Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-3.872</td>
<td>-0.947</td>
<td>-0.708</td>
<td>-0.239</td>
</tr>
<tr>
<td></td>
<td>(0.622)***</td>
<td>(0.192)***</td>
<td>(0.155)***</td>
<td>(0.088)***</td>
</tr>
<tr>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
</tr>
<tr>
<td><strong>Panel A2: Patients with Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-6.056</td>
<td>-2.921</td>
<td>-2.300</td>
<td>-0.620</td>
</tr>
<tr>
<td></td>
<td>(1.275)***</td>
<td>(0.497)***</td>
<td>(0.382)***</td>
<td>(0.214)***</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
</tr>
</tbody>
</table>

**Panel B: LOG REGRESSIONS**

| Panel B1: Patients without Chronic Conditions |                                   |                           |                               |
| Change in Copay             | -0.00752                          | -0.00595                  | -0.00785                      | -0.00164                            |
|                             | (0.00123)***                      | (0.00124)***              | (0.00146)***                  | (0.00217)                           |
| N                           | 2498                              | 2491                      | 2477                          | 2470                                |
| **Panel B2: Patients with Chronic Conditions** |                                   |                           |                               |
| Change in Copay             | -0.00581                          | -0.00796                  | -0.00848                      | -0.00412                            |
|                             | (0.00128)***                      | (0.00141)***              | (0.00158)***                  | (0.00190)***                        |
| N                           | 2478                              | 2471                      | 2466                          | 2407                                |

This table shows the coefficient on the change in copay from regressions of change in a plan’s average utilization per enrollee on change in copay. Plan averages are computed separately for patients who have chronic conditions and those who don’t. Expenditures reflect both patient and insurer contributions. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees with or without chronic conditions, and standard errors are clustered by plan. "All physician office expenditures" is a broader measure of office visit spending than "office visit expenditures." Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

*10% significance; ** 5% significance; *** 1% significance
<table>
<thead>
<tr>
<th></th>
<th>Office Visits to PCP or Specialist</th>
<th>PCP Office Visits</th>
<th>Specialist Office Visits</th>
<th>% Seeing a PCP or Specialist</th>
<th>% Seeing a PCP</th>
<th>% Seeing a Specialist</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>PANEL A: LEVEL REGRESSIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel A1: Patients without Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.011</td>
<td>-0.008</td>
<td>-0.003</td>
<td>-0.00133</td>
<td>-0.00161</td>
<td>-0.00068</td>
</tr>
<tr>
<td></td>
<td>(0.002)**</td>
<td>(0.001)**</td>
<td>(0.001)**</td>
<td>(0.00031)**</td>
<td>(0.00035)**</td>
<td>(0.00029)**</td>
</tr>
<tr>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
</tr>
<tr>
<td><strong>Panel A2: Patients with Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.028</td>
<td>-0.020</td>
<td>-0.008</td>
<td>-0.00096</td>
<td>-0.00104</td>
<td>-0.00111</td>
</tr>
<tr>
<td></td>
<td>(0.003)**</td>
<td>(0.003)**</td>
<td>(0.002)**</td>
<td>(0.00039)**</td>
<td>(0.00051)**</td>
<td>(0.00048)**</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
</tr>
<tr>
<td><strong>PANEL B: LOG REGRESSIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel B1: Patients without Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.00535</td>
<td>-0.00684</td>
<td>-0.00222</td>
<td>-0.00176</td>
<td>-0.00354</td>
<td>-0.00048</td>
</tr>
<tr>
<td></td>
<td>(0.00107)**</td>
<td>(0.00113)**</td>
<td>(0.00239)**</td>
<td>(0.00058)**</td>
<td>(0.00086)**</td>
<td>(0.00159)**</td>
</tr>
<tr>
<td>N</td>
<td>2491</td>
<td>2477</td>
<td>2471</td>
<td>2491</td>
<td>2477</td>
<td>2471</td>
</tr>
<tr>
<td><strong>Panel B2: Patients with Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.00645</td>
<td>-0.00626</td>
<td>-0.00485</td>
<td>-0.00107</td>
<td>-0.00112</td>
<td>-0.00219</td>
</tr>
<tr>
<td></td>
<td>(0.00085)**</td>
<td>(0.00106)**</td>
<td>(0.00164)**</td>
<td>(0.00044)**</td>
<td>(0.00064)*</td>
<td>(0.00115)*</td>
</tr>
<tr>
<td>N</td>
<td>2471</td>
<td>2466</td>
<td>2407</td>
<td>2471</td>
<td>2466</td>
<td>2407</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in a plan's average utilization per enrollee on change in copay. Plan averages are computed separately for patients who have chronic conditions and those who don’t. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees with or without chronic conditions, and standard errors are clustered by plan. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

* 10% significance; ** 5% significance; *** 1% significance
Table 3.6: Change in Other Types of Medical Expenditures Following Change in Doctor Visit Copay

<table>
<thead>
<tr>
<th>Total Expenditures</th>
<th>Outpatient Visit Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td><strong>Panel A: Dependent Variable in Levels</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-8.933</td>
<td>-1.247</td>
<td>0.529</td>
<td>-0.338</td>
<td>-2.717</td>
</tr>
<tr>
<td></td>
<td>(1.577)***</td>
<td>(0.629)**</td>
<td>(0.552)</td>
<td>(0.496)</td>
<td>(0.922)**</td>
</tr>
<tr>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
</tr>
<tr>
<td><strong>Panel B: Dependent Variable in Logs</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.00332</td>
<td>-0.00149</td>
<td>0.00458</td>
<td>-0.00039</td>
<td>-0.00250</td>
</tr>
<tr>
<td></td>
<td>(0.00067)***</td>
<td>(0.00299)</td>
<td>(0.00474)</td>
<td>(0.00231)</td>
<td>(0.00181)</td>
</tr>
<tr>
<td>N</td>
<td>2499</td>
<td>2391</td>
<td>1971</td>
<td>2460</td>
<td>2118</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan's average utilization per enrollee on change in copay. Expenditures reflect both patient and insurer contributions in that category. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc.), fixed effects for prescription and mental health coverage, share of enrollees who are female, the percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees, and standard errors are clustered by plans. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

* 10% significance; ** 5% significance; *** 1% significance
**Table 3.7: Change in Other Types of Medical Expenditures for Patients with and without Chronic Conditions**

<table>
<thead>
<tr>
<th></th>
<th>Outpatient Visit Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Total Expenditures</strong></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
</tr>
<tr>
<td><strong>PANEL A: LEVEL REGRESSIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel A1: Patients without Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-7.338</td>
<td>-1.735</td>
<td>0.635</td>
<td>-0.179</td>
<td>-1.160</td>
</tr>
<tr>
<td></td>
<td>(1.473)***</td>
<td>(0.620)***</td>
<td>(0.630)</td>
<td>(0.464)</td>
<td>(0.620)*</td>
</tr>
<tr>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2119</td>
</tr>
<tr>
<td><strong>Panel A2: Patients with Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-15.509</td>
<td>0.011</td>
<td>0.461</td>
<td>-0.899</td>
<td>-6.988</td>
</tr>
<tr>
<td></td>
<td>(3.902)***</td>
<td>(1.549)</td>
<td>(1.393)</td>
<td>(1.191)</td>
<td>(2.498)***</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2108</td>
</tr>
<tr>
<td><strong>PANEL B: LOG REGRESSIONS</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel B1: Patients without Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.00390</td>
<td>-0.00464</td>
<td>0.00449</td>
<td>0.00038</td>
<td>-0.00162</td>
</tr>
<tr>
<td></td>
<td>(0.00092)***</td>
<td>(0.00398)</td>
<td>(0.00604)</td>
<td>(0.00261)</td>
<td>(0.00211)</td>
</tr>
<tr>
<td>N</td>
<td>2498</td>
<td>2238</td>
<td>1742</td>
<td>2401</td>
<td>2114</td>
</tr>
<tr>
<td><strong>Panel B2: Patients with Chronic Conditions</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-0.00353</td>
<td>0.0024</td>
<td>0.00378</td>
<td>-0.00278</td>
<td>-0.00349</td>
</tr>
<tr>
<td></td>
<td>(0.00090)***</td>
<td>(0.00385)</td>
<td>(0.00615)</td>
<td>(0.00385)</td>
<td>(0.00192)*</td>
</tr>
<tr>
<td>N</td>
<td>2478</td>
<td>2078</td>
<td>1346</td>
<td>2150</td>
<td>2097</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in a plan's average utilization per enrollee on change in copay. Plan averages are computed separately for patients who have chronic conditions and those who don't. Expenditures reflect both patient and insurer expenditures in that category. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees with or without chronic conditions, and standard errors are clustered at the plan level. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

*10% significance; ** 5% significance; *** 1% significance


___ and Mark McClellan, “Is Technological Change in Medicine Worth It?,” Health Affairs, 2001, 20 (5).


108


HealthTrust Completes Acquisition of EPIC Holdings, Business Wire, 5 May 1994.


Appendix to Chapter 1

A.1 Data Appendix

The procedures I measure in the Medicare claims data are closely tied to technologies that I study. CT, MRI, and SPECT scans are performed using the corresponding imaging equipment. CABG is the most common type of open-heart surgery performed in the US. Open heart surgery refers to any surgery in which the chest is opened to perform surgery on the heart. It also include valve repairs and replacements, heart transplants, and the insertion of medical devices. I restrict to CABG since it is the most common procedure, has been studied extensively in the health economics literature, and is most readily identifiable in the claims data. Cardiac catheterizations are performed in cardiac catheterization laboratories. These procedures are diagnostic; cardiologists guide a catheter through a patient’s arteries to locate blockages. Angioplasty is a therapeutic procedure for blocked arteries. A tiny balloon on the end of a catheter is inflated, which causes the plaque spread out and widens the artery. Often the cardiologist will insert a stent in conjunction with angioplasty. The terms PCI and angioplasty are often used interchangeably, but PCI also includes atherectomy. In atherectomy, tiny blades at the end of the catheter scrape plaque from the arteries, and it can be performed in conjunction with angioplasty. Atherectomies, with or without angioplasty, comprise less than 10% of PCI claims in the years in my study.

I use the CPT codes, which document the procedures performed, and add-on procedure codes to identify the number of unique claims for each procedure as well as beneficiary ids to count the number of Medicare enrollees who receive a given procedure. I determine the relevant CPT codes for MRIs and CT scans by examining which CPT codes were associated with the BETOS codes for MRIs and CT scans. Song, Skinner, Bynum, Sutherland, Wennberg and Fisher (2010) also identify imaging procedures from BETOS codes.\(^1\) The CPT codes for CT scans are: 70450, 70460,

\(^1\)The BETOS codes are only available starting in 1997; the associated CPT codes are relatively consistent between 1992 and 2000 although many new codes are added for MRI procedures over the years.
The CPT codes for MRIs are: 70336, 70540, 70541, 70544, 70551, 70552, 70553, 71550, 71555, 72141, 72142, 72146, 72147, 72148, 72149, 72156, 72157, 72158, 72159, 72196, 72198, 73220, 73221, 73225, 73720, 73721, 73725, 74181, 74185, 75552, 75553, 75554, 75555, 75556, 76093, 76094, 76390, 76400, 78492, and 95923. Not all codes are present in all years. The SPECT scans are well-identified in the appendices of the books documenting CPT codes and in the individual descriptions of the CPT codes (CPT codes 78205, 78320, 78464, 78465, 78469, 78647, 78494, 78710, 78803, and 78807). I follow Lucas, DeLorenxo, Siewers and Wenneberg (2006) when selecting the CPT codes to identify CABG (CPT codes 33510-3536), cardiac catheterization (CPT codes 93508, 93510-93529, 93539-93540, 93543, and 93545-93552), and PCI (CPT codes 92980-92984 and 92995-92996).
A.2 Figures and Tables

Figure A-1: Technology Utilization before and after the Mergers

These figures show how the utilization changes in treatment markets before and after the merger. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The first graph shows how the total number of claims for the six procedures changes, and the second graph shows the results from a pooled regression. Both specifications includes year and market fixed effects, and standard errors are clustered at the market level. The pooled regression includes procedure fixed effects, and the effect of the controls can vary by type of procedure. Observations are weighted by the inverse of the average number of procedures for that technology. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1992 through 2000.
These figures show how the adoption and use of CT scans changes in treatment markets before and after the merger. Utilization is the number of people receiving CT scans. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The specification includes year and market fixed effects, and standard errors are clustered at the market level. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1990 through 2000 when examining adoption and 1992 through 2000 when examining utilization.
Figure A-3: MRI Adoption and Use before and after the Mergers

Hospitals with MRIs in Market

People Receiving MRIs

See Figure A-2 for more details.
Figure A-4: SPECT Adoption and Use before and after the Mergers

See Figure A-2 for more details.
Figure A-5: Open-Heart Surgery Adoption and Use before and after the Mergers

See Figure A-2 for more details.
Figure A-6: Cardiac Catheterization Adoption and Use before and after the Mergers

See Figure A-2 for more details.
Figure A-7: Angioplasty Adoption and Use before and after the Mergers

Hospitals with Angioplasty in Market

People Receiving PCIs

See Figure A-2 for more details.
Figure A-8: Adoption of Diagnostic Radioisotopes, ESWL, and Advanced OB Units before and after the Mergers

See Figure A-2 for more details.
Figure A-9: Technology Adoption before and after the Mergers, Robustness Checks

These figures show how the technology index changes in treatment markets before and after the merger. The left column shows the results from the “short technology index,” and the right column contains the event studies using the average technology index. The three rows show the results from three samples: all hospitals, just Columbia/HCA hospitals, and just rival hospitals. The x axis represents the years to the merger, with the year(s) of the merger announcement being standardized to 0. The y axis represents the coefficients on the year-to-merger dummies in the event studies specification. The specification includes year and market fixed effects, and standard errors are clustered at the market level. The bars from each point represent the 95% confidence interval on that coefficient. The figures contain data from 1990 through 2000.
This table shows the coefficients on the log of technology counts when the dependent variable is the log of a utilization measure: the people receiving procedures or the number of claims. The different panels represent the results for individual procedures and technologies. The regressions include some combination of year fixed effects, market fixed effects, and population controls, as indicated. Standard errors are clustered at the market level. The population controls are log of the market population and log of the Medicare population. Population doesn't vary across years; Medicare population does. Data is from 1992 through 2000.

* Significant at 10%, ** significant at 5%, *** significant at 1%.

### Table A.1: Relationship between Imaging Technology Levels and Use

#### Panel A: CT Scans

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving CT Scans</th>
<th>Log People Receiving CT Scans</th>
<th>Log People Receiving CT Scans</th>
<th>Log People Receiving CT Scans</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log CT Scanners</td>
<td>1.019</td>
<td>0.174</td>
<td>0.030</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.060)**</td>
<td>(0.079)**</td>
<td>(0.038)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

#### Panel B: MRI Scans

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving MRIs</th>
<th>Log People Receiving MRIs</th>
<th>Log People Receiving MRIs</th>
<th>Log People Receiving MRIs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log MRIs</td>
<td>0.934</td>
<td>0.119</td>
<td>0.025</td>
<td>0.064</td>
</tr>
<tr>
<td></td>
<td>(0.080)**</td>
<td>(0.062)*</td>
<td>(0.054)</td>
<td>(0.037)*</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

#### Panel C: SPECT Scans

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving SPECT Scans</th>
<th>Log People Receiving SPECT Scans</th>
<th>Log People Receiving SPECT Scans</th>
<th>Log People Receiving SPECT Scans</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log SPECT Scanners</td>
<td>0.999</td>
<td>0.160</td>
<td>0.063</td>
<td>0.118</td>
</tr>
<tr>
<td></td>
<td>(0.076)**</td>
<td>(0.068)**</td>
<td>(0.087)</td>
<td>(0.059)*</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>
Table A.2: Relationship between Cardiac Technology Levels and Use

**Panel A: Open-Heart Surgery/CABG**

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving CABG</th>
<th>Log People Receiving CABG</th>
<th>Log People Receiving CABG</th>
<th>Log People Receiving CABG</th>
<th>Log CABG Claims</th>
<th>Log CABG Claims</th>
<th>Log CABG Claims</th>
<th>Log CABG Claims</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Open-Heart Surgery Facilities</td>
<td>0.928</td>
<td>0.112</td>
<td>0.089</td>
<td>0.131</td>
<td>1.001</td>
<td>0.409</td>
<td>0.181</td>
<td>0.430</td>
</tr>
<tr>
<td></td>
<td>(0.079)*****</td>
<td>(0.111)</td>
<td>(0.050)**</td>
<td>(0.093)</td>
<td>(0.085)*****</td>
<td>(0.212)**</td>
<td>(0.095)**</td>
<td>(0.205)**</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Panel B: Cardiac Catheterization**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Cath Labs</td>
<td>0.986</td>
<td>0.067</td>
<td>0.154</td>
<td>0.015</td>
<td>1.010</td>
<td>0.083</td>
<td>0.156</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.049)*****</td>
<td>(0.092)</td>
<td>(0.075)**</td>
<td>(0.071)</td>
<td>(0.051)*****</td>
<td>(0.103)</td>
<td>(0.098)</td>
<td>(0.081)</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Panel C: Angioplasty/PCI**

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving PCI</th>
<th>Log People Receiving PCI</th>
<th>Log People Receiving PCI</th>
<th>Log People Receiving PCI</th>
<th>Log PCI Claims</th>
<th>Log PCI Claims</th>
<th>Log PCI Claims</th>
<th>Log PCI Claims</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Facilities that Can Perform Angioplasty</td>
<td>0.936</td>
<td>-0.149</td>
<td>0.054</td>
<td>-0.144</td>
<td>1.014</td>
<td>-0.123</td>
<td>0.102</td>
<td>-0.119</td>
</tr>
<tr>
<td></td>
<td>(0.064)*****</td>
<td>(0.124)</td>
<td>(0.084)</td>
<td>(0.119)</td>
<td>(0.071)*****</td>
<td>(0.192)</td>
<td>(0.124)</td>
<td>(0.185)</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Population Controls</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

This table shows the coefficients on the log of technology counts when the dependent variable is the log of a utilization measure: the people receiving procedures or the number of claims. The different panels represent the results for individual procedures and technologies. The regressions include some combination of year fixed effects, market fixed effects, and population controls, as indicated. Standard errors are clustered at the market level. The population controls are log of the market population and log of the Medicare population. Population doesn’t vary across years; Medicare population does. Data is from 1992 through 2000.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
**Table A.3: Technology Adoption in Markets with Mergers, Sensitivity Analysis**

<table>
<thead>
<tr>
<th>Model Description</th>
<th>Full Market</th>
<th>Chains</th>
<th>Just Rivals</th>
</tr>
</thead>
<tbody>
<tr>
<td>Poisson, Short Tech Index</td>
<td>0.026</td>
<td>-0.302</td>
<td>0.053</td>
</tr>
<tr>
<td></td>
<td>(0.019)</td>
<td>(0.111)**</td>
<td>(0.023)**</td>
</tr>
<tr>
<td>Poisson, Short Tech Index (with exposure variable)</td>
<td>0.045</td>
<td>-0.051</td>
<td>0.059</td>
</tr>
<tr>
<td></td>
<td>(0.016)**</td>
<td>(0.088)</td>
<td>(0.018)**</td>
</tr>
<tr>
<td>Avg Tech Per Hospital</td>
<td>0.109</td>
<td>-0.022</td>
<td>0.175</td>
</tr>
<tr>
<td></td>
<td>(0.070)</td>
<td>(0.418)</td>
<td>(0.080)**</td>
</tr>
<tr>
<td>Avg Tech Per Hospital (Short Index)</td>
<td>0.046</td>
<td>-0.161</td>
<td>0.110</td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.342)</td>
<td>(0.064)*</td>
</tr>
<tr>
<td>Log-linear, Tech Index</td>
<td>0.015</td>
<td>-0.291</td>
<td>0.040</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.113)**</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Log-linear, Short Tech Index</td>
<td>0.001</td>
<td>-0.314</td>
<td>0.029</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.113)**</td>
<td>(0.030)</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference regressions. The first two specifications are Poisson models, but use the short technology index. The next two are linear models, and the last two are log linear models. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Table A.4: Utilization in Markets with Mergers, Sensitivity Analysis

<table>
<thead>
<tr>
<th></th>
<th>Log People Receiving Procedure</th>
<th>Log Unique Claims for Procedure</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A: Regression Coefficients</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>All Procedures (Sum)</td>
<td>0.017</td>
<td>-0.034</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.034)</td>
</tr>
<tr>
<td>All Procedures (Pooled, Weighted Regression)³</td>
<td>-0.005</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.053)</td>
</tr>
<tr>
<td>All Procedures (Pooled, Weighted Regression)⁴</td>
<td>-0.006</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.055)</td>
<td>(0.057)</td>
</tr>
<tr>
<td>CT Scans</td>
<td>0.010</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>MRIs</td>
<td>0.005</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
<td>(0.052)</td>
</tr>
<tr>
<td>SPECT scans</td>
<td>0.097</td>
<td>0.107</td>
</tr>
<tr>
<td></td>
<td>(0.060)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>CABG</td>
<td>-0.079</td>
<td>-0.068</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.059)</td>
</tr>
<tr>
<td>Cardiac Catheterizations</td>
<td>-0.009</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>PCI</td>
<td>0.047</td>
<td>0.053</td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.072)</td>
</tr>
</tbody>
</table>

**Panel B: Controls for All Regressions in Column**

Demographic Controls X X

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference regressions. The dependent variables are all in logs. All Procedures (Sum) is the sum of all six procedures; All Procedures (Pooled, Weighted Regression) treat each procedure separately and weights each observation by the inverse of the average number of that procedure. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1992-1998, and post-merger period are years after the merger announcement. Demographic controls include 5-year age bins, the Medicare population, and indicators for female and race.

³ Controls have a constant effect across procedure types. Controls in procedure fixed effects.

⁴ Controls can vary across procedure types. Controls in procedure fixed effects.
### Table A.5: Technology Adoption among a Balanced Panel of Hospitals

<table>
<thead>
<tr>
<th></th>
<th>Full Market</th>
<th>Just Merging Chains</th>
<th>Just Rivals</th>
</tr>
</thead>
<tbody>
<tr>
<td># of Hospitals</td>
<td>0</td>
<td>-0.228</td>
<td>0.017</td>
</tr>
<tr>
<td></td>
<td>(0)</td>
<td>(0.0850)**</td>
<td>(0.008)**</td>
</tr>
<tr>
<td>Market Tech Index</td>
<td>0.043</td>
<td>-0.232</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td>(0.016)**</td>
<td>(0.117)**</td>
<td>(0.020)**</td>
</tr>
<tr>
<td># of Hosp with MRI</td>
<td>-0.007</td>
<td>-0.237</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.050)</td>
<td>(0.214)</td>
<td>(0.055)</td>
</tr>
<tr>
<td># of Hosp with Cath Lab</td>
<td>0.004</td>
<td>-0.278</td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.159)*</td>
<td>(0.033)</td>
</tr>
<tr>
<td># of Hosp with Open-Heart Surg</td>
<td>0.022</td>
<td>-0.142</td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.208)</td>
<td>(0.035)</td>
</tr>
<tr>
<td># of Hosp with Diag Radioisotopes</td>
<td>0.081</td>
<td>-0.192</td>
<td>0.113</td>
</tr>
<tr>
<td></td>
<td>(0.020)**</td>
<td>(0.089)**</td>
<td>(0.024)**</td>
</tr>
<tr>
<td># of Hosp with ESWL</td>
<td>0.064</td>
<td>0.024</td>
<td>0.026</td>
</tr>
<tr>
<td></td>
<td>(0.083)</td>
<td>(0.266)</td>
<td>(0.096)</td>
</tr>
<tr>
<td># of Hosp with Angioplasty</td>
<td>-0.003</td>
<td>-0.372</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.218)*</td>
<td>(0.039)</td>
</tr>
<tr>
<td># of Hosp with CT scan</td>
<td>0.022</td>
<td>-0.344</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.100)**</td>
<td>(0.026)**</td>
</tr>
<tr>
<td># of Hosp with Adv. OB unit</td>
<td>0.041</td>
<td>-0.302</td>
<td>0.065</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
<td>(0.181)*</td>
<td>(0.031)**</td>
</tr>
<tr>
<td># of Hosp with SPECT</td>
<td>0.150</td>
<td>0.094</td>
<td>0.139</td>
</tr>
<tr>
<td></td>
<td>(0.055)**</td>
<td>(0.228)</td>
<td>(0.058)**</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement. In these regressions, hospitals that enter the market-level observations are general medical and surgical hospitals from 1990 through 2000.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
<table>
<thead>
<tr>
<th>Market Tech Index</th>
<th>Full Market</th>
<th>Just Merging Chains</th>
<th>Just Rivals</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.039</td>
<td>-0.254</td>
<td>0.066</td>
</tr>
<tr>
<td></td>
<td>(0.019)**</td>
<td>(0.117)**</td>
<td>(0.022)**</td>
</tr>
<tr>
<td># of Hosp with MRI</td>
<td>-0.009</td>
<td>-0.199</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.214)</td>
<td>(0.066)</td>
</tr>
<tr>
<td># of Hosp with Cath Lab</td>
<td>0.018</td>
<td>-0.223</td>
<td>0.035</td>
</tr>
<tr>
<td></td>
<td>(0.031)</td>
<td>(0.164)</td>
<td>(0.035)</td>
</tr>
<tr>
<td># of Hosp with Open-Heart Surg</td>
<td>0.016</td>
<td>-0.191</td>
<td>0.032</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.214)</td>
<td>(0.039)</td>
</tr>
<tr>
<td># of Hosp with Diag Radioisotopes</td>
<td>0.061</td>
<td>-0.220</td>
<td>0.096</td>
</tr>
<tr>
<td></td>
<td>(0.023)**</td>
<td>(0.089)**</td>
<td>(0.027)**</td>
</tr>
<tr>
<td># of Hosp with ESWL</td>
<td>0.098</td>
<td>-0.108</td>
<td>0.077</td>
</tr>
<tr>
<td></td>
<td>(0.071)</td>
<td>(0.267)</td>
<td>(0.082)</td>
</tr>
<tr>
<td># of Hosp with Angioplasty</td>
<td>0.010</td>
<td>-0.341</td>
<td>0.037</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.186)*</td>
<td>(0.044)</td>
</tr>
<tr>
<td># of Hosp with CT scan</td>
<td>0.004</td>
<td>-0.354</td>
<td>0.047</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.098)**</td>
<td>(0.030)</td>
</tr>
<tr>
<td># of Hosp with Adv. OB unit</td>
<td>0.055</td>
<td>-0.309</td>
<td>0.089</td>
</tr>
<tr>
<td></td>
<td>(0.030)*</td>
<td>(0.189)</td>
<td>(0.031)**</td>
</tr>
<tr>
<td># of Hosp with SPECT</td>
<td>0.145</td>
<td>-0.050</td>
<td>0.169</td>
</tr>
<tr>
<td></td>
<td>(0.058)**</td>
<td>(0.223)</td>
<td>(0.060)**</td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement. These regressions exclude markets that were defined as treatment market because HCA and Healthtrust operated in them.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Table A.7: Utilization in Markets with Mergers, Excluding HCA-Healthtrust Markets

<table>
<thead>
<tr>
<th>People Receiving Procedure</th>
<th>Unique Claims for Procedure</th>
</tr>
</thead>
<tbody>
<tr>
<td>All Procedures (Sum)</td>
<td>0.023</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
</tr>
<tr>
<td>All Procedures (Weighted)</td>
<td>0.014</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
</tr>
<tr>
<td>CT Scans</td>
<td>0.024</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
</tr>
<tr>
<td>MRIs</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(0.042)</td>
</tr>
<tr>
<td>SPECT scans</td>
<td>0.075</td>
</tr>
<tr>
<td></td>
<td>(0.063)</td>
</tr>
<tr>
<td>CABG</td>
<td>-0.051</td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
</tr>
<tr>
<td>Cardiac Catheterizations</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.028)</td>
</tr>
<tr>
<td>PCI</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.047)</td>
</tr>
</tbody>
</table>

Panel B: Controls for All Regressions in Column

Demographic Controls | X | X

This table shows the coefficients on the interaction between the treatment market and post-period in difference-in-difference Poisson regressions. All Procedures (Sum) is the sum of all six procedures; All Procedures (Pooled, Weighted Regression) treat each procedure separately and weights each observation by the inverse of the average number of that procedure. The regressions also include an interaction between the year(s) of the merger announcements and treatment markets, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1992-1998, and post-merger period are years after the merger announcement. Demographic controls include 5-year age bins, the Medicare population, and indicators for female and race. These regressions exclude markets that were defined as treatment market because HCA and Healthtrust operated in them.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Table A.8: Technology Adoption in Markets where HCA Owned Hospitals

<table>
<thead>
<tr>
<th></th>
<th>Original Full Market Results</th>
<th>Markets with HCA Presence and Merger</th>
<th>Just HCA Presence</th>
</tr>
</thead>
<tbody>
<tr>
<td>Market Tech Index</td>
<td>0.044</td>
<td>0.050</td>
<td>0.013</td>
</tr>
<tr>
<td></td>
<td>(0.018)**</td>
<td>(0.021)**</td>
<td>(0.021)</td>
</tr>
<tr>
<td># of Hosp with MRI</td>
<td>-0.020</td>
<td>-0.005</td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td>(0.053)</td>
<td>(0.054)</td>
<td>(0.059)</td>
</tr>
<tr>
<td># of Hosp with Cath Lab</td>
<td>0.016</td>
<td>0.017</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.030)</td>
<td>(0.031)</td>
<td>(0.036)</td>
</tr>
<tr>
<td># of Hosp with Open-Heart Surg</td>
<td>0.019</td>
<td>0.020</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td>(0.029)</td>
<td>(0.033)</td>
<td>(0.030)</td>
</tr>
<tr>
<td># of Hosp with Diag Radioisotopes</td>
<td>0.066</td>
<td>0.089</td>
<td>0.042</td>
</tr>
<tr>
<td></td>
<td>(0.022)***</td>
<td>(0.025)***</td>
<td>(0.025)*</td>
</tr>
<tr>
<td># of Hosp with ESWL</td>
<td>0.124</td>
<td>-0.017</td>
<td>-0.226</td>
</tr>
<tr>
<td></td>
<td>(0.068)*</td>
<td>(0.102)</td>
<td>(0.101)**</td>
</tr>
<tr>
<td># of Hosp with Angioplasty</td>
<td>0.004</td>
<td>-0.034</td>
<td>-0.066</td>
</tr>
<tr>
<td></td>
<td>(0.034)</td>
<td>(0.035)</td>
<td>(0.036)*</td>
</tr>
<tr>
<td># of Hosp with CT scan</td>
<td>0.012</td>
<td>0.043</td>
<td>0.056</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.026)</td>
<td>(0.027)**</td>
</tr>
<tr>
<td># of Hosp with Adv. OB unit</td>
<td>0.063</td>
<td>0.075</td>
<td>0.020</td>
</tr>
<tr>
<td></td>
<td>(0.029)**</td>
<td>(0.030)**</td>
<td>(0.036)</td>
</tr>
<tr>
<td># of Hosp with SPECT</td>
<td>0.154</td>
<td>0.151</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.053)***</td>
<td>(0.056)***</td>
<td>(0.059)</td>
</tr>
</tbody>
</table>

The first column shows the original market level results. See Table 1.4 for details. The last two columns show the coefficients on the interaction between the post-period and market type in difference-in-difference Poisson regressions. Market type is shown in the column headers; markets with HCA presence and merger are the original treatment markets. The regressions also include an interaction between the year(s) of the merger announcements and market type, year fixed effects, and market fixed effects. Standard errors are clustered at the market level. Data is from 1990-1998, and post-merger period are years after the merger announcement.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Appendix to Chapter 2

Figure B-1: Robustness Checks for Change in Columbia/HCA’s Expenditures in Markets with Local Consolidation

(a) Expenditures per Admission

Expenditures per Admission at HCA Hospitals

(b) Exclude Hospitals in Markets Where Columbia/HCA Added Hospitals Post-Merger

Volume-Adjusted Expenditures at HCA Hospitals

(c) Exclude Hospitals in Markets with Healthtrust and HCA

(d) Balanced Panel

Volume-Adjusted Expenditures at HCA Hospitals

These graphs show how expenditures changed at Columbia/HCA hospitals in markets that became more consolidated post-merger relative to Columbia/HCA hospitals in markets that did not. See the note on Figure 2-3 for more detail. The regressions for the estimates in panels (b), (c), and (d) control for hospitals’ volume.
Figure B-2: Robustness Checks for Change in Columbia/HCA's Expenditures Relative to Other Hospitals

(a) Expenditures per Admission

(b) Exclude Hospitals in Markets Where Columbia/HCA Added Hospitals Post-Merger

(c) Exclude Hospitals in Markets with Healthtrust and HCA

(d) Balanced Panel

These graphs show how expenditures at Columbia/HCA hospitals changed post-merger relative to unaffiliated hospitals. See the note on Figure 2-5 for more detail. The regressions for the estimates in panels (b), (c), and (d) control for hospitals' volume.
### Table B.1: Change in Expenditures at Columbia/HCA Hospitals in Markets with Mergers, Sensitivity Analysis

<table>
<thead>
<tr>
<th>Panel</th>
<th>Dependent Variable</th>
<th>Log Expenditures per Admission</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A:</strong></td>
<td>Interaction on Treatment and Post Dummy</td>
<td>0.015</td>
<td>-0.010</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.029)</td>
<td>(0.027)</td>
<td></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td></td>
<td>2294</td>
<td>2294</td>
<td></td>
</tr>
<tr>
<td><strong>Panel B:</strong></td>
<td>Exclude Hospitals in Markets where Columbia/HCA Acquired Hospitals Post-System Formation</td>
<td>Interaction on Treatment and Post Dummy</td>
<td>-0.018</td>
<td>-0.037</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.041)</td>
<td>(0.029)</td>
<td></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td></td>
<td>1115</td>
<td>1115</td>
<td></td>
</tr>
<tr>
<td><strong>Panel C:</strong></td>
<td>Exclude Hospitals in Treatment Markets Where Only HCA and Healthtrust were Present</td>
<td>Interaction on Treatment and Post Dummy</td>
<td>-0.027</td>
<td>-0.025</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.035)</td>
<td>(0.028)</td>
<td></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td></td>
<td>2014</td>
<td>2014</td>
<td></td>
</tr>
<tr>
<td><strong>Panel D:</strong></td>
<td>Balanced Panel of Hospitals</td>
<td>Interaction on Treatment and Post Dummy</td>
<td>-0.049</td>
<td>-0.035</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.049)</td>
<td>(0.042)</td>
<td></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td></td>
<td>1071</td>
<td>1071</td>
<td></td>
</tr>
<tr>
<td><strong>Panel E:</strong></td>
<td>Controls for All Regressions in this Column</td>
<td>Hospital Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Output Controls</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Market Characteristic Controls</td>
<td>X</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Patient Mix Controls</td>
<td>X</td>
<td></td>
</tr>
</tbody>
</table>

This table shows the coefficients on the interaction between the treatment hospitals and post-merger dummy in difference-in-difference regressions. In Panels B, C, and D, the dependent variable is log total expenditures. The regressions also include the interaction between the hospitals in treatment markets and year of the merger announcement(s). The controls are specified below. See Table 2.2 for description of controls. Standard errors are clustered at the hospital level. Data is from 1990-1998, and post-merger period includes years after the merger announcement(s). See text for more detail.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Table B.2: Post-System Formation Change in Expenditures at Columbia/HCA Hospitals, Sensitivity Analysis

<table>
<thead>
<tr>
<th>Panel A: Dependent Variable Log Expenditures per Admission</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction on HCA and Post Dummy</td>
<td>-0.184</td>
<td>-0.149</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.017)**</td>
<td>(0.016)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and Merger Market</td>
<td>-0.184</td>
<td>-0.154</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.022)**</td>
<td>(0.019)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and non-Merger Market</td>
<td>-0.184</td>
<td>-0.137</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.027)**</td>
<td>(0.027)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>22332</td>
<td>22332</td>
<td>22332</td>
<td>22332</td>
</tr>
<tr>
<td>P-value on F Statistic</td>
<td>0.991</td>
<td>0.606</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B: Exclude Hospitals in Markets where Columbia/HCA Acquired Hospitals Post-System Formation</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction on HCA and Post Dummy</td>
<td>-0.108</td>
<td>-0.146</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.023)**</td>
<td>(0.017)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and Merger Market</td>
<td>-0.142</td>
<td>-0.170</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.024)**</td>
<td>(0.018)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and non-Merger Market</td>
<td>-0.046</td>
<td>-0.103</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.043)</td>
<td>(0.031)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>13060</td>
<td>13060</td>
<td>13060</td>
<td>13060</td>
</tr>
<tr>
<td>P-value on F Statistic</td>
<td>0.049</td>
<td>0.061</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel C: Exclude Hospitals in Treatment Markets where Only HCA and Healthtrust were Present</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction on HCA and Post Dummy</td>
<td>-0.145</td>
<td>-0.149</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.021)**</td>
<td>(0.017)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and Merger Market</td>
<td>-0.162</td>
<td>-0.156</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.025)**</td>
<td>(0.021)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and non-Merger Market</td>
<td>-0.111</td>
<td>-0.135</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.034)**</td>
<td>(0.027)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>20490</td>
<td>20490</td>
<td>20490</td>
<td>20490</td>
</tr>
<tr>
<td>P-value on F Statistic</td>
<td>0.227</td>
<td>0.523</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel D: Balanced Panel of Hospitals</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Interaction on HCA and Post Dummy</td>
<td>-0.120</td>
<td>-0.140</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.026)**</td>
<td>(0.022)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and Merger Market</td>
<td>-0.143</td>
<td>-0.155</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.027)**</td>
<td>(0.024)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Interaction on HCA, Post, and non-Merger Market</td>
<td>-0.064</td>
<td>-0.105</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.058)</td>
<td>(0.048)**</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>18522</td>
<td>18522</td>
<td>18522</td>
<td>18522</td>
</tr>
<tr>
<td>P-value on F Statistic</td>
<td>0.213</td>
<td>0.355</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel E: Controls for All Regressions in this Column</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hospital Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Year Fixed Effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Output Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Market Characteristic Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Patient Mix Controls</td>
<td>X</td>
<td>X</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

In specifications 1 and 3, the coefficient on the interaction between the treatment hospitals and post-merger dummy in difference-in-difference regressions is shown. In specifications 2 and 4, the coefficients on the interaction among a HCA hospital dummy, the post-merger period, and dummies indicating whether the market became more consolidated are shown. The regressions also include analogous interactions for the "during" period. The controls are specified below. See Table 2.3 for description of controls. Data is from 1990-1998, and the post-merger period includes years after the merger announcement based on the system to which the hospital previously belonged. Hospitals joining Columbia/HCA in the post-period are also included as part of the Columbia/HCA hospitals. The p-values on the F statistics are from Wald tests of the equality between the coefficients of the decline in expenditures in merger and non-merger markets. See text for more detail.

* Significant at 10%, ** significant at 5%, *** significant at 1%.
Appendix to Chapter 3

C.1 Technical Appendix

The HCCI data doesn’t contain information on the exact plans in which people were enrolled or the cost-sharing within each plan. The empirical challenge is to back out this information from the claims data. Every claim in the HCCI data has fields to describe the amount the patient contributed in copays, coinsurance, or deductible. We also know the firm through which a patient obtained insurance and broad characteristics of that plan, which include the type of insurance product she has and indicators for whether the plan is a high deductible plan and covers mental health conditions and prescriptions. Together this allows us to identify firms and sets of broad plan characteristics that likely contain only one plan and the cost-sharing arrangements for PCP and specialist office visits.

We limit our analysis to the approximately 43,000-50,000 firms in each year that have at least 50 people enrolled in their insurance products in that year. Since we rely on individual claims to determine prices, we need enough people within the plan to file claims.

To back out patient cost-sharing in each year, we first determine an individual’s total deductible spending by summing all her deductible spending from inpatient claims, outpatient claims, and physician claims. We identify firms in which 90% of people who are enrolled in plans with the same broad characteristics have $0 deductible spending. We did not consider plans with positive deductibles because deductibles most often apply to the employee and their dependents’ combined spending, and we are unable to link families in the data. However, we do know an individual’s relationship to the policy-holder, that is, if they are the employee, spouse, or child. If 90% of all individuals do not have $0 deductible spending, we consider whether 90% of dependents, or spouses and children, meet this criteria, or whether restricting to only employees allows us to meet this criteria. Since single individuals, couples, and families often have different insurance plan options,
it’s possible firms might offer only single individuals or a family plan with a $0 deductible.

Most plans that we identify are for individuals. If we’ve determined the deductible only applies for families (employees), when considering copays and coinsurances, we only use the copays of spouses and children (employees) to determine the most common copay within that plan.

After restricting to plans that meet the deductible criteria, we proceed to calculate copays and coinsurances for office visits to primary care physicians and specialists. We identify office visits using a variable HCCI created to categorize the type of service within the claim. Their variable is based on CPT codes. Primary care physicians are identified as pediatricians, family practices, and internal medicine. A provider type variable allows us to separate office visit to PCPs and specialists. We defined office visits for specialists as office visits to a provider specializing in: allergy and immunology, cardiology, dermatology, endocrinology, gastroenterology, general surgery, hematology and oncology, neurology, obstetrics and gynecology, ophthalmology, orthopedics, otolaryngology, rheumatology, and urology. We would expect the copay to be the same across specialties in most plans.

Within a firm and set of broad plan characteristics, we determined the most common copay or coinsurance rate for PCP and specialist office visits. The most common copay or coinsurance rate was based off individuals’ first office visit claims for PCP or specialists. We restricted to the first office visit to reduce the possibility that the copay or coinsurance rate in the claim only applies after a patient has exceeded an annual maximum spending limit. Additionally, this gives equal weight to the copays of all individuals who have office visits rather than weighting individuals with higher number of office visits more heavily.

We retain firms and sets of broad plan characteristics in which 90% of the individuals who have PCP (specialist) claims have the same copay or coinsurance rate for that service. We also set the most common copay or coinsurance rate as the price for an office visit within that plan.

We also impose the restriction that 5 individuals must have PCP and specialist claims. Since some plans with smaller numbers of enrollees have few claims, without this restriction, a plan’s PCP or specialist copay could be determined by one or two individuals’ claims. In practice, this implies that everyone must have the same copay or coinsurance rate in plans with fewer than 10 individuals with PCP (specialist) claims.

We define the firms and sets of broad plan characteristics left as plans. In each plan 90% of the individuals have $0 deductible spending, at least 90% of individuals who had PCP and specialist office visits faced the same cost-sharing arrangement for each type of provider.
Having determined plans and prices for each year, we restrict our analysis to plans for which we determined prices in two consecutive years. This information also allows us to determine changes in the copays for physician visits and calculate changes in utilization.
C.2 Figures and Tables

Figure C-1: Change in Utilization before and after Copay Increases

These graphs show how average utilization within a plan changed after plans increased their copays for physician visits. The x axis represents the years after the copay increase, with 0 representing the year prior to the copay increase. The y axis represents the magnitude of the coefficients on the "years after copay increase" dummies in the event studies specification. The bars on each point represent the 95% confidence interval on the coefficient. Each plan's average is computed based on a balanced panel of enrollees, and in the estimation, plans are weighted the number of enrollees in the sample. See the text for more detail.
<table>
<thead>
<tr>
<th>Panel A: All Individuals</th>
<th>Change in Copay</th>
<th>All Physician Office Expenditures</th>
<th>Office Visits to PCP or Specialist Expenditures</th>
<th>Total Expenditures</th>
<th>Outpatient Visit Facility Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-1.128</td>
<td>-0.014</td>
<td>-9.153</td>
<td>-2.302</td>
<td>-0.830</td>
<td>0.575</td>
<td>0.682</td>
<td>-3.780</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.634)</td>
<td>(0.007)*</td>
<td>(5.314)*</td>
<td>(2.078)</td>
<td>(2.078)</td>
<td>(1.211)</td>
<td>(1.476)</td>
<td>(1.120)</td>
<td>(1.563)**</td>
</tr>
<tr>
<td>N</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>291</td>
<td>362</td>
</tr>
<tr>
<td>Panel B: Patients without Chronic Conditions</td>
<td>Change in Copay</td>
<td>-1.264</td>
<td>-0.012</td>
<td>-10.497</td>
<td>-2.356</td>
<td>-1.163</td>
<td>-0.312</td>
<td>-1.019</td>
<td>-1.044</td>
</tr>
<tr>
<td></td>
<td>(1.632)</td>
<td>(0.007)*</td>
<td>(4.609)**</td>
<td>(1.902)</td>
<td>(1.652)</td>
<td>(1.599)</td>
<td>(2.011)</td>
<td>(1.136)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>362</td>
<td>291</td>
<td>362</td>
</tr>
<tr>
<td>Panel B: Patients with Chronic Conditions</td>
<td>Change in Copay</td>
<td>0.574</td>
<td>-0.016</td>
<td>1.950</td>
<td>-0.581</td>
<td>1.244</td>
<td>3.550</td>
<td>8.635</td>
<td>-10.955</td>
</tr>
<tr>
<td></td>
<td>(3.246)</td>
<td>(0.013)</td>
<td>(16.278)</td>
<td>(7.036)</td>
<td>(2.809)</td>
<td>(4.818)</td>
<td>(8.494)</td>
<td>(4.231)**</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>359</td>
<td>359</td>
<td>359</td>
<td>359</td>
<td>359</td>
<td>359</td>
<td>290</td>
<td>359</td>
<td>359</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan’s average utilization per enrollee on change in copay. Inpatient and outpatient prices are known and do not change among these plans. When the dependent variables are expenditure measures, they reflect both patient and insurer contributions. In panels B and C, the plan’s average only includes patients without or with chronic conditions. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, the percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees (Panel a) or number of enrollees without or with chronic conditions (panels b and c). Standard errors are clustered by plan. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded before limiting to plans with known inpatient and outpatient prices.

*10% significance; ** 5% significance; *** 1% significance
<table>
<thead>
<tr>
<th>Panel</th>
<th>All Physician Office Expenditures</th>
<th>Office Visits to PCP or Specialist</th>
<th>Total Expenditures</th>
<th>Outpatient Visit Facility Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td>Panel A: All Individuals</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-3.536</td>
<td>-0.013</td>
<td>-7.121</td>
<td>-1.043</td>
<td>0.527</td>
<td>-0.208</td>
<td>-2.696</td>
<td>-0.841</td>
</tr>
<tr>
<td></td>
<td>(0.599)**</td>
<td>(0.002)***</td>
<td>(1.365)***</td>
<td>(0.509)**</td>
<td>(0.287)*</td>
<td>(0.497)</td>
<td>(0.779)***</td>
<td>(0.387)***</td>
</tr>
<tr>
<td>N</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>1887</td>
<td>2242</td>
</tr>
<tr>
<td>Panel B: Patients without Chronic Conditions</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-3.098</td>
<td>-0.011</td>
<td>-5.877</td>
<td>-1.195</td>
<td>0.287</td>
<td>-0.333</td>
<td>-1.332</td>
<td>-0.446</td>
</tr>
<tr>
<td></td>
<td>(0.583)**</td>
<td>(0.002)***</td>
<td>(1.209)***</td>
<td>(0.482)**</td>
<td>(0.276)</td>
<td>(0.423)</td>
<td>(0.507)***</td>
<td>(0.337)</td>
</tr>
<tr>
<td>N</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>2242</td>
<td>1887</td>
<td>2242</td>
</tr>
<tr>
<td>Panel C: Patients with Chronic Conditions</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-5.590</td>
<td>-0.026</td>
<td>-2.185</td>
<td>-0.846</td>
<td>1.639</td>
<td>0.301</td>
<td>-6.478</td>
<td>-2.334</td>
</tr>
<tr>
<td></td>
<td>(1.157)***</td>
<td>(0.004)***</td>
<td>(2.975)***</td>
<td>(1.266)</td>
<td>(0.852)*</td>
<td>(1.124)</td>
<td>(2.146)***</td>
<td>(1.025)**</td>
</tr>
<tr>
<td>N</td>
<td>2223</td>
<td>2223</td>
<td>2223</td>
<td>2223</td>
<td>2223</td>
<td>2223</td>
<td>1874</td>
<td>2223</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan's average utilization per enrollee on change in copay. When the dependent variables are expenditure measures, they reflect both patient and insurer contributions. In panels B and C, the plan's average only includes patients without or with chronic conditions. The controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), fixed effects for prescription and mental health coverage, share of enrollees who are female, the percentage of people in 5 year age bins, and year fixed effects for year 1. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees (panel a) or number of enrollees without or with chronic conditions (panels b and c). Standard errors are clustered by plan. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the 5 highest or lowest percentiles also are excluded.

*10% significance; ** 5% significance; *** 1% significance
Table C.3: Sensitivity of Changes in Utilization to Controls Using Full Sample

<table>
<thead>
<tr>
<th></th>
<th>All Physician Expenditures</th>
<th>Office Visits to PCP or Specialist Expenditures</th>
<th>Outpatient Visit Facility Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td>Panel A: No Controls</td>
<td>Change in Copay</td>
<td>-4.555</td>
<td>-0.016</td>
<td>-9.628</td>
<td>-1.637</td>
<td>0.626</td>
<td>-0.325</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.642)**</td>
<td>(0.002)**</td>
<td>(1.992)**</td>
<td>(0.658)**</td>
<td>(0.555)</td>
<td>(0.510)</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2120</td>
</tr>
<tr>
<td>Panel B: Plan Characteristic Controls Only</td>
<td>Change in Copay</td>
<td>-4.358</td>
<td>-0.015</td>
<td>-9.761</td>
<td>-1.374</td>
<td>0.471</td>
<td>-0.367</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.658)**</td>
<td>(0.002)**</td>
<td>(1.528)**</td>
<td>(0.589)**</td>
<td>(0.579)</td>
<td>(0.492)</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2120</td>
</tr>
<tr>
<td>Panel C: Individual Demographic Controls Only</td>
<td>Change in Copay</td>
<td>-4.125</td>
<td>-0.014</td>
<td>-7.639</td>
<td>-1.34</td>
<td>0.590</td>
<td>-0.239</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.633)**</td>
<td>(0.002)**</td>
<td>(1.857)**</td>
<td>(0.766)*</td>
<td>(0.545)</td>
<td>(0.490)</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2499</td>
<td>2120</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan’s average utilization per enrollee on change in copay. When the dependent variables are expenditure measures, they reflect both patient and insurer contributions. All regressions include year fixed effects for year 1. The plan characteristic controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), and fixed effects for prescription and mental health coverage. The individual demographic controls include the percentage of enrollees who are female and the percentage who are in 5 year age bins. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees, and standard errors are clustered by plan. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile also are excluded.

*10% significance; ** 5% significance; *** 1% significance
Table C.4: Sensitivity of Changes in Utilization to Controls among Patients without Chronic Conditions

<table>
<thead>
<tr>
<th>Panel</th>
<th>All Physician Office Expenditures</th>
<th>Office Visits to PCP or Specialist</th>
<th>Total Expenditures</th>
<th>Outpatient Visit Facility Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td>A: No Controls</td>
<td>Change in Copay</td>
<td>-4.021</td>
<td>(0.622)***</td>
<td>-0.012</td>
<td>(0.002)***</td>
<td>-7.660</td>
<td>(2.071)***</td>
<td>-1.968</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2119</td>
</tr>
<tr>
<td>B: Plan Characteristic Controls Only</td>
<td>Change in Copay</td>
<td>-3.893</td>
<td>(0.599)***</td>
<td>-0.012</td>
<td>(0.002)***</td>
<td>-7.928</td>
<td>(1.431)***</td>
<td>-1.775</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2119</td>
</tr>
<tr>
<td>C: Individual Demographic Controls Only</td>
<td>Change in Copay</td>
<td>-3.477</td>
<td>(0.645)***</td>
<td>-0.009</td>
<td>(0.002)***</td>
<td>-5.588</td>
<td>(1.965)***</td>
<td>-1.803</td>
</tr>
<tr>
<td></td>
<td>N</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2498</td>
<td>2119</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan's average utilization per enrollee on change in copay. Only individuals without chronic conditions are included. When the dependent variables are expenditure measures, they reflect both patient and insurer contributions. All regressions include year fixed effects for year 1. The plan characteristic controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), and fixed effects for prescription and mental health coverage. The individual demographic controls include the percentage of enrollees who are female and the percentage who are in 5 year age bins. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees without chronic conditions, and standard errors are clustered by plan. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles of all individuals are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile based on all individuals' spending also are excluded.

*10% significance; ** 5% significance; *** 1% significance
Table C.5: Sensitivity of Changes in Utilization to Controls among Patients with Chronic Conditions

<table>
<thead>
<tr>
<th>Panel</th>
<th>All Physician Office Expenditures</th>
<th>Office Visits to PCP or Specialist</th>
<th>Total Expenditures</th>
<th>Outpatient Visit Facility Expenditures</th>
<th>Inpatient Facility Expenditures</th>
<th>ER Expenditures</th>
<th>Rx Expenditures</th>
<th>Lab Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
</tr>
<tr>
<td>Panel A: No Controls</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-6.691</td>
<td>-0.031</td>
<td>-15.839</td>
<td>-0.378</td>
<td>0.489</td>
<td>-0.995</td>
<td>-8.142</td>
<td>-3.097</td>
</tr>
<tr>
<td></td>
<td>(1.438)***</td>
<td>(0.004)***</td>
<td>(4.659)***</td>
<td>(1.608)</td>
<td>(1.339)</td>
<td>(1.314)</td>
<td>(2.494)***</td>
<td>(1.305)**</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2108</td>
<td>2480</td>
</tr>
<tr>
<td>Panel B: Plan Characteristic Controls Only</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-6.347</td>
<td>-0.029</td>
<td>-16.526</td>
<td>-0.149</td>
<td>0.415</td>
<td>-0.782</td>
<td>-8.003</td>
<td>-3.046</td>
</tr>
<tr>
<td></td>
<td>(1.343)***</td>
<td>(0.003)***</td>
<td>(3.999)***</td>
<td>(1.487)</td>
<td>(1.405)</td>
<td>(1.233)</td>
<td>(2.494)***</td>
<td>(1.315)**</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2108</td>
<td>2480</td>
</tr>
<tr>
<td>Panel C: Individual Demographic Controls Only</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in Copay</td>
<td>-6.028</td>
<td>-0.028</td>
<td>-13.229</td>
<td>-0.014</td>
<td>0.396</td>
<td>-1.061</td>
<td>-6.982</td>
<td>-2.858</td>
</tr>
<tr>
<td></td>
<td>(1.212)***</td>
<td>(0.004)***</td>
<td>(3.891)***</td>
<td>(1.666)</td>
<td>(1.328)</td>
<td>(1.203)</td>
<td>(2.361)***</td>
<td>(1.286)**</td>
</tr>
<tr>
<td>N</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2480</td>
<td>2108</td>
<td>2480</td>
</tr>
</tbody>
</table>

This table shows the coefficient on the change in copay from regressions of change in the plan’s average utilization per enrollee on change in copay. Only individuals with chronic conditions are included. When the dependent variables are expenditure measures, they reflect both patient and insurer contributions. All regressions include year fixed effects for year 1. The plan characteristic controls include the log number of people in the plan in year 1, log of average total spending in the plan in year 1, log of average office visit expenditures in the plan in year 1, plan type fixed effects (HMO, PPO, etc), and fixed effects for prescription and mental health coverage. The individual demographic controls include the percentage of enrollees who are female and the percentage who are in 5 year age bins. The average spending controls include all people enrolled in the plan for 12 months; the dependent variable averages are computed for all people who are in the plan for 12 months in two consecutive years. In regressions, plans are weighted by number of enrollees with chronic conditions, and standard errors are clustered by plan. The regression with Rx expenditures only includes plans that cover prescriptions. Individuals whose change in total spending is in the 2.5 highest or lowest percentiles of all individuals are excluded before computing the plan average, and plans whose change in total expenditures or log total expenditures is in the highest or lowest percentile based on all individuals’ spending also are excluded.

*10% significance; ** 5% significance; *** 1% significance